



Pluto and the platypus: An odd ball and an odd duck - On classificatory norms



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ABSTRACT

Many astronomers seem to believe that we have *discovered* that Pluto is not a planet. I contest this assessment. 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 laden with normative commitments of a distinctive kind. I conclude with a discussion of the relevance of such “norm-ladenness” to other controversies in the metaphysics of classification, such as the monism/pluralism debate.

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1. Pluto's fall from grace

Many influential 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) 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 must now be changed: “My Very Eager Mother Just Served Us *Nothing!*?”¹ *We cannot just change our minds like this!* Of course, such appeals should not affect the IAU's resolve. The history of science is replete with mind-changes that reflect new knowledge. Indeed, it took the scientific revolution to popularize the belief that the *Earth* is a planet on a par with the other “wandering stars”. But the discussion about Pluto so far, like a brown dwarf, has given off more heat than light (even in scientific corners). In particular, some astronomers hold that there is a uniquely correct understanding of ‘planet’ dictated by recent discoveries at the edge of the solar system and so there is a straightforward sense in which we *discovered* that Pluto is not a planet — much like we discovered that whales are not fish, that the platypus is a mammal, or that there is no such thing as phlogiston. Early on in the dispute, Gibor Basri and Michael Brown, the astronomers who made some of the key discoveries that led to Pluto's “demotion” from the ranks of the planets, responded to the tradition-mongers arguing for maintaining Pluto's planetary classification in stark terms: “either tradition or logical consistency must be abandoned” (Basri & Brown, 2006, p. 210;). Of course the matter is not so simple. One can consistently classify Pluto as a planet by making compensatory adjustments to other claims about the solar system

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¹ One version (“My Very Eager Mother Just Served Us Nine Pizzas”) records the “traditional” order of the nine familiar planets: Mercury, Venus, Earth, Mars, Jupiter, Saturn, Uranus, Neptune, Pluto.

— for example, by recognizing perhaps a few thousand more planets as part of the solar system.²

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 relevant norms might be in this case and how we might identify, defend, and criticize classificatory norms in general. Philosophers of science have been growing more comfortable with the idea of epistemic and methodological norms involved in theory choice. The present norms, however, fit uneasily in this category. Rather than guiding acceptance of certain theories, the kind of norms with which I am concerned appear to influence how we carve nature up into different kinds of things. In this sense, they are more like *metaphysical* or *ontological* norms. However, since I take no stance here about the metaphysics of classification, I will simply call them *classificatory norms*. Though to some degree voluntary, I argue that they should not be thought of as mere conventions. Their activity shows us, I believe, that a deep variety of classificatory pluralism may be more widespread — and in a certain way inevitable — than previously recognized. It has gone unnoticed, in part, because it can obtain even when its outward effects (*viz.* disagreement) are hidden.

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 traits 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 retain the 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 situation? Before addressing this question, let us consider why Pluto was *initially* grouped with the planets.

One initial answer points to the fact that Clyde Tombaugh found Pluto while looking for a planet — the so-called *Planet X*, the trans-Neptunian planet Percival Lowell reckoned was causing orbital perturbations for Neptune and Uranus. This answer only goes so far, though. Granted, the search for Planet X is what got Tombaugh looking so carefully; but 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 thought of in the early 20th century), rather than, say, a comet. V.M. Slipher wrote in the *Lowell 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, p. 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 similar to Earth's mass; in fact, it's about 1/5th Earth's diameter and less than 0.2% its mass).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. 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, pp. 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

²One might wonder, given that scientists are sometimes mistaken in their self-descriptions (or evidently mistaken in their assessments of the logic of the situation, as Brown seems to be), whether the Pluto case is worthy of philosophical attention (thanks to an anonymous referee for raising this concern). Are the sentiments described here common among the astronomical community? Admittedly, I have no statistically-representative opinion surveys to share on this; but it is not difficult to find the rhetoric of discovery used by influential astronomers and science writers (see, e.g., Tyson, 2009; Brown, 2010). In any case, I am less concerned with how representative or *prima facie* plausible the view that astronomers discovered Pluto's non-planetness is as I am to use the case as a springboard to consider a general but apparently unrecognized feature of classification and classificatory disputes in science.

³There are other examples: some suggest that *Rodentia* should be contracted to exclude Guinea pigs from the folk-category, lest it include highly non-paradigmatic rodents (see LaPorte, 2004, p. 63).

⁴Though 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 for the time); 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.

planets — a grouping that Lowell’s followers were keen to maintain (some no doubt out of nationalistic pride).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 (or best) 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 are anomalous in terms of their height, but this is of course irrelevant to their classificatory status. Membership in *Homo sapiens* simply 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*. One obvious way of showing this would be to point out that such parameters were in fact 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. We’ll see this more clearly as we turn to Pluto’s companion in classificatory puzzlement: the platypus.

2.2. An odd duck

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 thus 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 a better explanation than Taylor’s for why Pluto was not disqualified as a planet even as evidence mounted that it lacked many of the properties of other recognized planets. 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 of the 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.

Enter the platypus. Early naturalists struggled classifying this furry, 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, p. 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 laid eggs rather than giving live births like all other (then) known mammals. Famously, it has an electroreceptor-laden “duckbill”. Less well-known are its poison spur and cloaca (a channel shared by both excretory and reproductive systems⁸), features common in birds and 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 in which history might have gone differently: first, by imagining that the platypus was initially thought to be a mammal (this isn’t much of a stretch, given how furry and shy it is!); or second, by 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 more instructive. Forget about Lowell, Tombaugh, American Astronomical Pride, the early gross overestimates of Pluto’s mass, classificatory inertia, and all that. Suppose Pluto was found fifty years later than it actually was and that the better instruments of the time staved off major inaccuracies. Would we still have classified it as a planet?

If the case of the platypus can serve as guide, a plausible answer is ‘yes’. Though perhaps not a *paradigm* mammal, the platypus certainly appeared to fit *better* with the mammals than with any other group. As Grant puts it in his study of the species: “When all its characteristics are considered, the platypus is much more 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 & Fanning, 1995, p. 5).⁹ While this latter assertion is compelling, in the context of early “platypus scholarship” it would have been an overstatement. As LaPorte has argued, “cases like this spur theorists to refine the use of their terms to reduce vagueness” (2004, p. 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 Pluto’s discovery as occasion to make more precise the meaning of ‘planet’ — e.g., 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 argue 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

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, Matthew, Barker, and Brigandt (2007), and Slater (2015).

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” along with the echidna.

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, pp. 118–120). I take no stance on this issue here.

not lead to exclusion from their initial categories¹¹: 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 but defeasible 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, 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, pp. 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?

Let us return to the question of whether we *discovered* that Pluto is not a planet. On what facts did this purported discovery depend? As it turns out, nothing new was discovered about *Pluto* itself (e.g., determining that it was in fact an alien megastructure); the relevant discoveries concerned rather its *company* — specifically, the fact that there were *other* Pluto-like objects in a distant zone of the solar system known as the Edgeworth-Kuiper Belt.¹³ Though Gerard Kuiper and Kenneth Edgeworth postulated this region around 30–55 astronomical units (AU) from the sun in the 1950s (as home of millions of short-period comets) 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 QB₁’ — was observed. 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 UB₃₁₃ (now officially named ‘Eris’). Two things stood out about Eris: it’s bigger than Pluto and it has its own 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) was a major factor in the lead up to Pluto’s official “expulsion” from the class of planets.¹⁵ But this doesn’t make Pluto’s purported non-planethood a discovery. 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 (including horses, seals, and so on) of the ancestor of the more paradigmatic rodents. If expansion and contraction of the category were both live options, it seems questionable to reckon either move as a discovery (LaPorte, 2004, pp. 66–67). Likewise, all the discovery of Eris (and its siblings) does is prompt a choice: either we have fewer planets than ordinarily thought or far, far more.

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 infect 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 — might be thought to incorporate an *element* of choice. For non-definitive 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, p. 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](#), p. 183; *my italics*)

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 You might wonder: *what justifies this norm?* I am concerned here only with motivating its descriptive plausibility; I address the matter of justification in §4.1.

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.

¹⁵ [Brown \(2010\)](#) clearly sees it this way; for another detailed accounting of “the Pluto Affair”, see chapter 1 of [Dick’s \(2013\)](#) book.

¹⁶ If a scientific *choice* is a conclusion underdetermined by evidence, then *every* scientific conclusion is a matter of choice. I have little sympathy for this view, though of course many influential post-Modern critics of science seem to accept something like it. At best, it is a highly misleading way of thinking about scientific discovery.

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 classificatory yarn. 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 more or less *a priori* a certain degree of acceptable — or preferable? — diversity within a category. But this presumption begs the question at hand. 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 (perhaps in the same way that the oddballs at the party seem less odd the more of them there are). On the other side, there’s an evident 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 a more manageable number of beetle species.

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” (2001, p. 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”. Assuming the right phylogenetic facts, 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 forces 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 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, 2008).

We should take issue with Brown’s reasoning as trading on an ambiguity of ‘population’ (and cognate terms) between classificatory and locational 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 little compelling reason to treat ‘KBO’ as anything more than a location specifier (like ‘Siberian megafauna’). 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 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, sometimes 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 & Musgrave, 1970, p. 178). Kuhn’s response was that the “maxims, norms, or values” that guide — rather than determine — theory choice are in fact shared (Kuhn, 1977, p. 322). That these values could be equivocal, differently emphasized, or come into conflict did not imply that their role should be rejected (330). Still, insistence that the norms guiding theory choice are largely shared

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.

¹⁸ Indeed, as I shall suggest below, there are independent reasons for wanting to dispense with Condition (c).

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 require intentional or extensional revision. How, then, do we avoid the specter of subjectivity, arbitrariness, or mob-rule in classificatory revision? The simple answer is to recognize 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. First, just how many members should a category ideally possess and what could possibly justify a univocal answer to such a question *a priori*? Second, 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 biological category. Or consider the genus *Otocyon*, which contains just one species: *Otocyon megalotic* (the bat-eared fox). On their face, such examples might suggest that the *Avoid Lonely Categories* norm isn't even descriptive of our classificatory practice.¹⁹

There are a number of things to 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, pp. 321–322), he wisely 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" (p. 322, see also Laudan, 1984, p. 25). This latter remark points the way to a response to the worry about our two sorts of lonely categories (the last individual member of *Didu ineptus* and the single species taxon within the *Otocyon* genus). 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).²⁰ And as Stephen Jay Gould has pointed out, evolution may aptly be characterized as a process of repeated diversification and

decimation (1989, p. 47). We know from fossils, for example, that the late Pliocene and early Pleistocene saw a cousin of *Otocyon megalotic*: *Otocyon recki* (Clark, 2005, p. 1); that *Otocyon megalotic* persists alone in *Otocyon* reflects only its relative good luck among its evolutionary siblings.²¹ 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" over the long haul.

Still, it might not be clear that the aim of information conveyance sufficiently justifies the avoidance of loneliness. After all, even if we had in general sought to keep our categories well-populated and few in number for aesthetic reasons, surely information conveyance would not have been unduly hampered by the introduction of a novel group of organisms somehow tucked "between" mammals and reptiles. Perhaps not. The biological case is complicated by the tension with what I regard as other normative requirements on classification (and their change across history). In the case of Pluto, perhaps another sort of classificatory conservatism intervened. Perhaps aesthetic considerations are ultimately relevant. As distasteful as Pluto's ill-fit with the rest of the planets might seem to Taylor and company, better this — apparently — than tolerate a novel category with a population of one among many other well-populated categories.

There are obvious affinities between the concept of classificatory norms as I have articulated them thus far and Laudan's methodological rules and scientific aims or "cognitive ends" (see his 1984, 1987). For these methodological (and meta-methodological) rules have essentially normative content — even if Laudan believes that they can be judged by their ability to bring about chosen cognitive ends — and, as I will discuss shortly, form evaluative hierarchies. One notable difference between Laudan's methodological rules and classificatory norms, however, is that the former pertain to the adoption of theories (or truth-apt theoretical claims) whereas the latter pertain to the categories described by the theories. While classification and theory cannot always be cleanly separated — classifications arguably embed theoretical commitments — there is a minimal sense in which classificatory norms are conceptually prior to the activity of methodological rules/norms as they influence the way in which the theoretical options are expressed in the first place. In order to flesh out this picture, let us consider a further classificatory norm relevant to the Pluto case.

4.2. Extrinsic parameters of classification

My motivation and defense of *Avoid Lonely Categories* was necessarily brief and impressionistic; but I hope to have illustrated both its descriptive plausibility and how its justification might be approached without immediately resorting to brow-beating or mob-incitements. Casting off the language of "discovery" and bringing 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 appreciate the importance of such openness by reflecting on some of the minority resistance to Pluto's reclassification. Consider the crucial condition (c) in the IAU's resolution. Many are puzzled by the thought that an object's *company* should hold decisive sway as to its kind. As Astronomer Alan Stern (principal investigator of the New Horizons mission that recently swung

¹⁹ Thanks to an anonymous referee for raising concerns along these lines.

²⁰ See, e.g., Velasco (2008) for a defense of this norm.

²¹ 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 presumably guided by further *meta-norms*.

by Pluto) put it: “We do not classify objects in astronomy by what they are near, we classify them by their properties”.²² Stern of course has in mind *intrinsic* properties — and it seems at first very plausible norm of classification: that extrinsic properties ought not bear on an object’s kind. We wouldn’t classify perfect intrinsic duplicates differently, would we?

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. It does seem initially plausible 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 might seem to violate a basic aim of classification: conveying 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 presses, 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.²³

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 (Ellis, 2001) — that classification is, ideally, a sort of systematic recording of what natural kinds of things (broadly speaking) exist. But this does little more than repeat the norm of intrinsic classification in different terms.²⁴ The skeptic simply asks again: *why suppose natural kinds must only 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. Again, 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) may strike us as inappropriately risky. Such extrinsic properties would never be serious candidates for a kind-relevant property (or 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, many of 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 — and just how? 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 change willy-nilly by plucking it from its pond.²⁵ So the claim that only intrinsic properties are stably possessed is incorrect and thus should not 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 individuatons are those like genetic profile or developmental-stage-relativized phenetic properties which are likewise stable across a great many actual and counterfactual perturbations.

Notice how the considerations in the previous several paragraphs link particular ground-level classificatory norms to broader “meta-norms” and aims of classification in a hierarchical structure. Avoiding extrinsic parameters of classification is argued to promote their information-bearing capacities. A critic points out that this is a near miss of a norm that applies more broadly to further sciences: *favor stable parameters of classification*. One might ask further whether the information-bearing aim contributes to what Laudan called the cognitive ends of particular scientists, perhaps given interactions with other norms and aims. Or one might ask whether, in a given scientific discipline studying a certain domain of entities, the ground-level norms in fact served the higher-level norms — *meta-norms*, if you like — or whether concession in these cases to other operative norms took precedence. Moving up the hierarchy, we might also consider whether there are alternative functions of classification in tension with its information-bearing function. While we can evaluate a system of norms internally — say, on grounds of prescriptive consistency or, as Laudan might put it, according to their historical or prospective tendency to promote our ends, values, aims, goals (however one wishes to parse these) — but at no point does it seem plausible to assert a particular norm as *true* (or criticize it for being *false*). Rather, their evaluation is simply constrained by the ends up the ladder about which there is no final appeal — a point to which I will return in §4.3.

But let me emphasize that given a particular broad conception of the aim(s) of classification and how these aims further serve the cognitive ends of science, it is possible to rationally evaluate and motivate candidate norms without resorting to mob-rule. Disagreement can likewise be traced upwards to the norms guiding evaluation at the ground level. Making this all explicit facilitates a more nuanced discussion of proposals for classificatory revision. Suppose, to make this concrete, that one advocated the *favor stable parameters of classification* norm. How might this enable them to engage with astronomers energetically campaigning for the “demotion” of Pluto from the planets? To answer this, we need to ask what degree of taxonomic stability the IAU’s chosen parameters exhibit. Clearly, our focus should remain with condition (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

²² quoted in *Nature* 31 August 2006, 965.

²³ We could also cite biology as a domain in which extrinsic properties — position on the “tree of life” — are commonly taken as classificatorily relevant.

²⁴ 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.

²⁵ 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 (2005).

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.²⁶ Accordingly, if some interloper — a “rogue planet” — ever comes through and crowds Earth’s orbit, condition (c) dictates that Earth would cease to be a planet (again, without befalling any intrinsic change).²⁷ Fortunately, the gravitational influence of the gas giants makes this rather unlikely (interlopers would likely be quickly 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 to be taken seriously. 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 with interloping bodies. Still, Pluto’s orbit is relatively crowded. But note that 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 and break out of the discovery vs. arbitrary convention model of classificatory revision. Moreover, advocates of the IAU definition have a number of footholds for constructive engagement with this critique. They might, for instance, agree that the stable parameter norm is in general conducive to the aims they share with the critic I imagine but disagree that it applies to this particular case (cf. Laudan, 1984, p. 25). What happens, however, when the disagreement pertains the norms or meta-norms in question?

4.3. Normative and classificatory pluralism

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 (1984) takes roughly this form (see also Mishler & Donohue, 1982). He notes the paleontologist’s plight in using Mayr’s biological species concept (which emphasizes reproductive compatibility):

²⁶ 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, pp. 61–62).

²⁷ Indeed, so-called rogue planets (aka interstellar planets, unbound, orphaned, or scattered planets), raise interesting questions for definitions of ‘planet’ with criteria involving orbital dynamics. Recent surveys and theoretical work suggests planetary-mass objects unbound or extremely distant from stars may be at least as common as star-bound planets (Sumi et al., 2011). Some of these objects (in the several Jupiter-mass range) may be failed brown dwarfs (Liu et al., 2013); others may simply be planets ejected from their solar systems during their initial chaotic formation (Bromley & Kenyon, 2014). Should we regard the IAU’s condition (c) as trivially satisfied since such objects are not orbiting anything (even if they are, in fact, crowded by other objects)? Should we simply deny that they are planets despite their intrinsic similarity to their star-bound kin? If so, it’s currently an open question whether planets represent the majority of planetary-mass objects in the universe.

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, p. 317)

Monists may of course remain unmoved. It might 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. It is something we might yet discover.

Some worry, moreover, about a slide from moderate to radical pluralism; going pluralist might entail, as the evolutionary biologist Michael Ghiselin put it, 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” (1987, p. 136; see also Hull, 1999, p. 36).²⁸ Without such a criterion, Ghiselin suggests, *any* respect of similarity can count, leading to a truly radical pluralism. Kitcher was 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, p. 187).

Notice the disconnect between Ghiselin’s worry and Kitcher’s response, however: 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 bizarre 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 what we would regard as inane or insane systems of classification.

Recognizing the role of classificatory norms helps us understand what is at stake in this debate (and perhaps enables a defense of 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 plays in informing the construction of 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 shared aims, conventions, and norms and the options the world makes compatible with these. This is essentially how Marc Ereshefsky, drawing on Laudan (1987), responds to the purported threat of “anything-goes” radical pluralism: use the aims and cognitive goals of scientific disciplines to understand the legitimacy of different approaches to taxonomy (Ereshefsky, 2001, pp. 170–173; see also Ereshefsky, 1992; §3.3). Laudan cautions historically oriented philosophers of science against evaluating meta-methodologies of science on the basis of their fit with the behavior of past scientists whose aims may have

²⁸ 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.

been very different from our own. Instead, the rationality of the methodological rules they use can be judged on their aptness to bring about those aims. But this can be evaluated only if we have some purchase on the aims and background beliefs in question. Likewise for classification: ground-level classificatory norms can only be judged according to the further meta-norms and aims they serve. Ereshefsky argues that the “ultimate aim of biological taxonomy” is indeed largely shared by diverse practitioners of systematics and that disagreement concerns “which methodological rules best achieve that aim” (2001, p. 172).²⁹

There are thus two routes to a norm-driven classificatory pluralism: first, at this ground level where different classificatory norms (or sets of norms) might equally serve the higher-level norms and aims; and second, at the level of those meta-norms and aims themselves. 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 by the lights of shared higher-level aims. Classificatory choice operates within a limited space of legitimate possibilities (cf. Brown, 2010, pp. 233–234). The question of moderation or radicalness of classificatory pluralism for a given domain turns on the size of this space — the degree to which the world and our norms constrain our classificatory activities — at any level we care to countenance.

The disconnect between moderate pluralists like Kitcher and Ereshefsky and their monist critics who contend that this moderation is unstable can thus be thought of as concerning the level at which possible normative disagreement occurs. However constraining the norms guiding our classificatory activities in a certain domain are, a non-cognitivist about norms 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. Shared aims of classification are presumably quite contingent (or perhaps even the products of socially-imbriated negotiation). Other meta-norms are possible. 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 implies a very radical form of pluralism (though not necessarily the *maximally* radical form imagined by Ghiselin).

By making plain the role that classificatory norms play in informing our classificatory practices, moderate pluralists can admit that this “possibilist” form of pluralism has a place at the table while nevertheless mitigating the threat that it poses to *their* moderation; legitimacy for them takes account of not just what *possible* aims scientists might hold for classification (or scientific investigation in general), but what those aims *actually are*. 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 (lower-) 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.³⁰ Even a single system of ground-level norms could leave room for a plurality of classificatory systems.

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?³¹ 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:

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, p. 113)

As in LaPorte's precisification cases, neither the world nor our norms decides how we ought 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 for the monist. 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. Back to the Pluto affair

I have been 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 those norms. 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

²⁹ In my reading, Laudan does not fully appreciate the significance of this parallel possibility.

³⁰ 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.

³¹ I address some of these issues in my (2015).

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, p. 184)

I hope to have made it plausible that this reasoning involves a false dichotomy — or, to put it in more positive terms, the fact 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 *should* we say about Pluto, then? If my claims about the norm-ladenness 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 could be criticized as offending from a plausible norm of classification. If members of the IAU in fact share the classificatory norms and meta-norms I have articulated above — either in explicit acceptance or implicit expression — this criticism will be a matter of internal consistency with their other commitments. If they do not share these norms, then discussion must turn to those norms themselves and their interaction with the facts as we understand them. Either way, however — and this is the crucial point — it would be best to prosecute these debates by attending to the relevant classificatory norms explicitly.

We unquestionably now know more about the structure and origin of the solar system than we did even a decade ago. And we will doubtless continue to learn more as data from the *New Horizons* mission trickles in from the Pluto system 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 (or whether we should indeed favor stable parameters of classification) 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 or if this is indeed a classificatory norm we should accept.

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