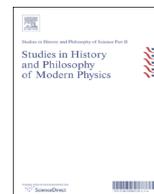




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Planets, pluralism, and conceptual lineage



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ABSTRACT

Conceptual change can occur for a variety of reasons; some more scientifically significant than others. The 2006 definition of ‘planet’, which saw Pluto reclassified as a dwarf planet, is an example toward the more mundane end of the scale. I argue however that this case serves as a useful example of a related phenomenon, whereby what appears to be a single kind term conceals two or more distinct concepts with independent scientific utility. I examine the historical background to this case, as a template for developing additional evidence for pluralist approaches to conceptual disputes within science and elsewhere.

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1. Introduction

In *Theory Change and the Indeterminacy of Reference*, Hartry Field famously challenged the continuity of reference for scientific terms in the face of revolutionary theory change. His key example was the term ‘mass’ as referred to from within Newtonian physics and Einsteinian physics. Field’s contention was that “many scientific terms are referentially indeterminate – there is no fact of the matter as to what they denote (if they are singular terms) or as to what their extension is (if they are general terms)” (Field, 1973).

In this paper, I propose an alternative approach to conceptual change in science which is compatible with Field’s, but which would ground at least some indeterminacy of reference in a qualified form of ambiguity. My contention is that many scientific terms are ambiguous because of terminological and conceptual lineages where different disciplines or specialisations develop in parallel. What I am concerned with is not in-series, mismatched meanings across revolutionary theory change, but rather mismatch of reference as can develop gradually between different branches of the same broad endeavour. My case study involves the history behind the 2006 definition of planethood (and demotion of

Pluto from ‘planet’ status), and the difficulty amongst the Astronomical community in determining that term’s reference.

The discussion will complement recent work on the nature of natural kinds. Indeed, the case of Pluto’s demotion is a high profile example which has been used in recent work on natural kinds, such as (Bokulich, 2014) and (Magnus, 2012), who both contrast it against the more philosophically well-trodden examples. While I will discuss these views (with similar comparisons), the natural kinds debate is not my chief concern. I will outline what I take the implications to be for the debate, but my main conclusions will be compatible with a wide range of positions within that debate.

My position will be that the *lineages* of kind concepts matter when considering whether those concepts are best seen as unified or divided, useful or dispensable; and these lineages can be evidenced by changes in kind term usage within identifiable, historically extended areas of study. Many conceptual histories (including across scientific revolutions) track conceptual change linearly as unbranching chains¹. Synchronic comparisons of allegedly pluralistic scientific concepts on the other hand (such as in the ‘species’ debate) can miss deep conceptual connections and

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¹ For an excellent example of a conceptual history with respect to electrons, see (Arabatzis, 2005). Similar conclusions are expressed in (Kitcher, 1995).

common origins. The aim here is to argue that evidence of conceptual fission provides an underappreciated, pro-tanto reason to take synchronic pluralism more seriously.

I will argue that this can help resist metaphysical kind-monism, but also resists Ereshefsky's eliminativist pluralism (Ereshefsky, 1998, 1992) and Dupré's promiscuous realism (Dupré, 1993). In this regard, I will be broadly in agreement with the approaches to scientific kinds argued for by (Magnus, 2012) and (Brigandt, 2009, 2003). However, while my sympathies are with these authors with respect practice-driven approaches to natural kinds, embracing specific metaphysical positions is not necessary for my own approach.

Rather, what I outline here is more of a philosophical heuristic which may be valuable in a narrow class of cases of apparent scientific pluralism, more or less independent of specific metaphysical commitments. There is a general form of analysis to be elaborated here which may help to explain how terminological disputes arise, and also explain why they can be so fiercely contested and resistant to resolution. In this sense at least, my intent is broadly in the spirit of (Hájek, 2014; 2016) and other work that fleshes out philosophical heuristics. I conclude that the identified analysis and approach may have utility for certain disputes and philosophical debates, in bolstering the case for 'no fact of the matter' positions.

2. Polysemy

Key here is polysemy, a form of ambiguity in which a single lexical form is capable of being deployed in different but closely related way. Like regular ambiguity, polysemy is different from semantic vagueness in that it is not a matter of deciding on the extension of something that 'tails off'. But neither is it just regular ambiguity. For example, the word 'man' is polysemous because it can be variously used to refer to humankind as a whole, to male humans, or to adult male humans. In contrast, the different meanings of the word 'bank' (a river bank, a financial institution) exhibit simple ambiguity – there is no significant conceptual similarity or extensional overlap between the two uses of that term.

Speaking figuratively, if the problem of vagueness is about precisifying semantic categories (such as 'bald') along single dimensions of incremental change (like the number of hairs on a head), and ambiguity is about deciding which of several orthogonal dimensions we might be talking about when using a term which could refer to any of them, then polysemy is ambiguity without strict orthogonality – distinguishing between senses which are conceptually related and extensions which might partially overlap².

For linguists, there are many species of polysemy with different features and associated problems (see for example (Blank, 2003)). For my purposes though there are three simple features of interest:

1. Polysemy is the result of symbolic vocabulary being outstripped by imaginative capacity (there are fewer words than there are concepts),
2. Polysemy tends to arise in an uncoordinated way, when a new linguistic usage arises without the extinction of the old usage, or when two specialised usages diverge, and,
3. Polysemy is contextually sensitive, i.e. the concept which the lexical item stands for is typically made clear by the context of utterance, frame of enquiry, or descriptive utility.

² Cheerfully enough, the boundary between polysemous and non-polysemous cases of ambiguity is vague.

Polysemy becomes philosophically interesting when the different, polysemous senses of a term each arise to have heft and weight as part of larger conceptual modes, models, or systems. Consider the following thought experiment:

Imagine you've been abducted by geographically inept time-travellers, who are keen to acquire Polish Vodka from a very specific era and have abducted you, a renowned expert on Poland, and taken you back to the year 1901 (assume this was a very good year for Polish Vodka). They are hovering their time machine over Europe asking you to identify Poland for them. In their eagerness however they haven't explained their motives; so all you know is that it's 1901, you're looking down on Europe, and they have asked you: "Where is Poland?"

Being an expert on all things Polish, you would know that Poland ceased to exist as an independent state in 1840, and will not be resurrected as a sovereign state until after World War One. So if your own interests were primarily in political history it might be obviously correct to tell them that their question is mistaken – as there is no such country as Poland in 1901. Alternatively, it also might be reasonable to indicate the lands contained within the pre-1840 Polish borders, or the inter-war borders, or the present day borders (they are all distinct, though partially overlapping) and use any of those as a basis for an answer. Or you might assume the Grand Duchy of Warsaw to be the 'true' Poland of 1901 (part of the Russian Empire).

But there are also entirely non-statist grounds on which to make a response; for example to indicate the area where the Polish language is spoken, or Polish culture the strongest (in 1901 that is). There seem to be a number of reasonable ways to frame a response, because without context the reference of 'Poland' is indeterminate.

This is the hallmark of a polysemous term. In this case there are many politically defined candidate referents with sharp boundaries, and many other more nebulous ones based on ethnicity, cultural practices and so-forth. Each of these is based on established practices or models which (by themselves) are perfectly reasonable, conceptually rich contexts within which the proper name 'Poland' might properly occur.

But in this thought experiment no suitable context had been set, so resorting to any answer straight off the bat would have been premature. From an entirely disinterested viewpoint, the best response would be a request for clarification: why is it *Poland* that they are asking for? Once you discover that their sole interest is locating Polish vodka, you have a better context for a precise answer. But in the absence of any precisifying context the referent is not straight-forward, even if you know everything there is to know about Poland – and misalignment of interests (you: politics, them: vodka) can naturally lead to talking past one another.

So this artificial case illustrates:

- a) a significant failure of coincidence between polysemous extensions,
- b) a lack of prior precisifying context, and
- c) an understandable conflict of reference due to differing interests.

And the polysemy here also stems from a polysemous kind term with its own conceptual lineage: the kind term 'country'. Plausibly, the multiple senses of 'country' likely developed benignly, in parallel, as descriptive and explanatory needs become more sophisticated over time. For example, prior to the rise of the Westphalian system of modern nation states, there was arguably less polysemy around 'country', because state power cleaved more closely to dynasties and empires than to ethno-linguistic nations. It was developments in politics and international law over the last few hundred years which produced the situation where 'Poland',

'Hungary', and 'Italy' refer to nation states as well as culturally or geographically defined regions. If so, then so the fact that the word 'country' is now as polysemous as it is comes down to contingent, interest-driven conceptual developments – gradual alterations in the mappings of entities useful to refer to (legally, socially, politically, etc.) as countries. It would take an empirical, historical enquiry to confirm such a story.

Such reflections are scientifically relevant because the push and pull of scientific progress – conceptual change and diversifying explanatory projects in the face of a 'gappy' evidence base – is also exactly the sort of environment where we might expect polysemous kind terms to develop, as disciplines and sub-disciplines branch and conceptual utilities multiply. I argue that if the *indispensability* of kinds invoked in successful scientific theories are seen as any sort of evidential guide to natural kinds in the world, then polysemy (diagnosed by historical examination of kind-term development) can pose a special evidential problem for non-pluralist views of natural kinds, as well as for efforts to impose unified uses of kind terms in science.

I will focus on the kind term 'planet' as a case study for this purpose. I argue that all the above ingredients for polysemy-based reference conflict were at play, with the interests of different scientific sub-disciplines and explanatory projects having divergent uses for a planet-concept. Rival conceptions of planethood seemed obvious from within their relevant scientific context, but bizarre and misguided from the standpoint of rival contexts. The conceptual history behind this conflict is useful for understanding conflicts of this nature, and how they might best be resolved – kind-pluralism and indeterminacy of reference is more intractable than otherwise thought in such cases.

3. Case study

In August 2006, the International Astronomical Union (IAU) met in Prague. Besides being a professional academic conference, IAU meetings serve as gatherings to pass resolutions on the official nomenclature in astronomical science – a rare example of the meanings of scientific terms being legislated by experts. On the agenda that year was an item which drew a great deal of media attention: the first official definition of 'planet' as a formal scientific term. The well-known outcome is that Pluto, formerly known as the ninth planet, was stripped of planetary status and reclassified as a Dwarf planet (a newly defined kind-term). I will argue that the details of this case illustrate how the polysemic nature of even a well-known kind term: a) can be concealed by the specialised utilisations of the concepts it stands for, b) might only be brought to light by novel considerations or evidence, and c) can remain a source of disagreement even in the light of those considerations.

Astronomy, like many sciences, relies heavily on systematic naming and cataloguing conventions. Astronomers study phenomena via the observation of a vast array of discrete astronomical objects; and these all have to be recorded and catalogued according to agreed systems of nomenclature for re-identification and meaningful classification. For example, at time of writing the SIMBAD database lists 8 million objects (mostly stars of our galaxy)³, which are the subject of 22 million database references and more than 300 thousand bibliographical references (SIMBAD, 2015). Historically, it was the categorisation of large populations of stars according to observable properties (luminosity, distance

inferred from parallax/interferometry, and elemental composition inferred from emission spectra) that revealed the clustering of these properties due to deeper natural processes of astrophysics, with causal hypotheses empirically distinguished from one another by different predictions about numbers and distributions. There is no humanly-feasible laboratory for Astronomy, so categorisation-driven, explanatory ontologies for observables are crucially important; and how you count and classify really matters.

However when the IAU debated their official definition of 'planet' in 2006, this was the first time that any official scientific body had ever done so. Categorisation systems for the *lists* of planets, minor planets, exo-planets had all been assessed and revised by the IAU and its delegated bodies, and advances in informatics and observational techniques dramatically added to those lists. For example, in the ten years prior to August 2006 the ranks of so-called 'minor planets' had swollen from 31,113 to 338,097 (and 696,114 at time of writing) categorised as 'aten', 'amor', or 'centaur' based on orbital properties (Minor Planet Center, 2015). These data are the raw material for much of the science concerned with understanding the origins and evolution of the solar system. However the necessary and sufficient conditions for an individual body counting as a *planet* (as opposed to the more numerous categories of solar system objects, defined for various purposes) had never been agreed upon.

The proximate cause for this issue coming to a head when it did is accidental: the contingency in the size and distribution of solar system objects, and their resulting order of discovery. For the past eighty years there were nine known 'bigish things' orbiting the sun as a primary (planets), with a large gap in detectability between those nine and the smaller, more distant or otherwise dimmer asteroids, planetoids, and Kuiper-belt objects. Astronomical classification is not done for its own sake, and up until recently there was no interest or pressing need for an official lower mass/size limit for planets as a class of objects.

However, after the relatively tiny size of Pluto was established in the 1970s and with the accelerating influx of new discoveries, there was a looming possibility that a new solar-system body would be discovered that rivalled Pluto in size. Distant, trans-Neptunian objects such as Sedna and Varuna were discovered which approached Pluto in observable size, and were indeed touted by some as 'the tenth planet' (BBC, 2004), but these were officially categorised with an extremely non-committal term 'Kuiper belt object', or KBO. But in 2003 a KBO was discovered that possessed a tiny orbiting moon, which allowed its mass to be calculated at 27% more than that of Pluto. With the systematic name of 2003 UB313 (informally named 'Xena' by discoverer Mike Brown – its moon was 'Gabrielle'), this object broke the quietism and prompted a decision on the official use of the term 'planet'. If Pluto was a planet and 'planet' meant something, then 2003_UB313 should probably be one as well. So what did 'planet' actually mean?

The upper limit of planetary mass, the boundary between planets and stars, is a natural 'joint' in the mass scale. At about thirteen times the mass of Jupiter the internal pressure of a large body will be sufficient to initiate the nuclear fusion processes at its core. At this point the body is no longer a planet and is classified instead as a brown dwarf: a commonly occurring (though very dim) class of object intermediate between planets and stars. The problem with setting a *lower* mass limit is the lack of a similarly dramatic joint at which to cut. Prior to the August 2006 meeting, one suggested cut-off was the point at which self-gravitation would overcome rigid body forces and enforce 'hydrostatic equilibrium' – i.e. compressing the body into the same overall spheroid shape that it would have had if it had been liquid instead of solid (Stern and Levison, 2002).

³ The European Space Agency's GAIA spacecraft, launch 20 December 2013, aims to up that figure considerably by measuring approximately 1 billion stars of our galaxy (~1% of the total) and mapping their positions in three dimensions.

Specifying the lower limit criteria was a job given to a 7-person IAU sub-committee consisting of five professional astronomers and science writer Dava Sobel; chaired by astronomer and science historian Owen Gingerich. In April 2006 this group was tasked with composing a draft resolution to present to the full session of the IAU at the August meeting. The committee settled on a proposal based around the equilibrium cut-off: hydrostatic equilibrium would be the natural joint on the mass scale at which planets began. This criterion would result in at least twelve known planets, with the addition of Ceres (the largest of the asteroids), Charon (formerly known as Pluto's largest moon)⁴ and 2003_UB313. Depending on further investigation of other solar-system bodies, dozens more could potentially have met the new criteria.

However the proposal did not go down well with the main body of the IAU, which would usually be expected to approve whichever categorisation system its sub-committees formulated. Instead, revisions and counterproposals were made ahead of the vote, scheduled for 22 August. This session was inconclusive and two days later a heavily modified version of the proposal was passed with different physical criteria. This restricted the proposed expansion of the planet-class but also so as to exclude Pluto, Ceres and 2003_UB313. There were now officially eight planets: Mercury, Venus, Earth, Mars, Jupiter, Saturn, Uranus and Neptune and a new category of 'dwarf planets' was created, of which Pluto, Ceres, and 2003_UB313 were now members (dwarf planets were explicitly stated to not be planets). Many scientists were publicly critical of the new definition and the process that produced it. A few months later, the dwarf planet 2003_UB313 was given an appropriate name: Eris, after the Greek goddess of discord⁵.

3.1. *What was being disputed*

And a close reading of the final resolution reveals something of the fractious process that produced it. The key passage specifies three necessary and jointly sufficient conditions for a solar system object to qualify as a planet:

1. The object is in orbit around the sun.
2. The object is under hydrostatic equilibrium, i.e. has assumed a (nearly) spherical shape because of self-gravitation.
3. The object 'has cleared the neighbourhood around its orbit', or otherwise dominates that local environment.

This set of conditions is curious. Most obviously (and pedantically), this does not serve as a literal, general-purpose definition of 'planet', as the first condition rules out planets in orbit around other stars (there are almost 2000 known extra-solar planets at time of writing). It is a testament to the convoluted nature of the debate that the scope of the above language (*solar system object*) was deliberately restricted in order to minimise disagreement: it remains quiet about extra-solar planets and (though obviously generalizable to other stars) is not literally a full definition of the word 'planet' at all⁶.

More interesting though is the relationship between conditions 2 and 3. Condition 3 (the 'dominance' clause) was the major amendment to the resolution, between submission by the sub-

committee and the final resolution. The original clause 2 (the 'equilibrium' clause) invokes the previously mentioned 'geological' notion of hydrostatic equilibrium: being a lump of matter of sufficient mass such that self-gravitation overwhelms the structural resistance of that matter and squashing it into a more or less spherical shape. What the dominance clause inserts is a distinction entirely orthogonal to the equilibrium clause, and which evaluates not just on the intrinsic gravitational efficacy of the object, but also with respect to other solar system objects in relation to it.

Firstly, the capacity for dominance relies not just on mass, but also orbital characteristics. The orbital path that a planet-candidate must clear or dominate is proportional to its orbital distance from the sun. Therefore, if the Earth (or an intrinsically identical copy) were to instead orbit the sun at many times the actual distance, its status as a planet could be in peril because the greatly expanded region of space it must dominate would correspondingly dilute its gravitational influence. It also depends on what else there is to dominate or be dominated by. The dominance condition depends on a fairly complex understanding of long term capacity to either eject much smaller bodies from that orbital space, capture them as moons, or corral them into orbits in harmonic resonance with its own (Pluto's domination by Neptune in this last way is what fails it as a planet)⁷. A planet could therefore cease to be a planet by merely migrating into a more distant orbit. Likewise a massive non-planet (for example the Jovian moon Ganymede, which is larger than the planet Mercury) could meet this condition if it were relocated to an appropriate solar orbit close enough to the sun.

Under the dominance definition of planethood, being a planet is therefore a much less intrinsic property of an object, and (intuitively) less essential. An individual persisting entity like the Earth can cease to be a planet without ceasing to exist, or even undergoing intrinsic change. 'Planet' as defined under the dominance clause cannot be considered a substance concept in the traditional sense; but is instead a phase sortal – a status that an object may acquire or lose, depending on relative location/role within a solar system. Indeed, relocation of the thing itself is unnecessary: the insertion or removal of other objects (massive enough that they cannot be gravitationally dominated) into or out of the local orbital environment would be sufficient. Planethood can therefore be contingent on states of affairs with realisers external to the planet itself.

Does this make the kind 'planet' less sound? Scientifically at least the answer should be 'no': "peak predator" is an extrinsically defined, biologically important kind, and there are numerous other examples.

In any case, concerns such as these are not central concerns of this paper. The feature of the final, three-clause definition which is most significant for my purposes is that, while the typical mass-limit required to satisfy the dominance clause is dependent on orbital position, it is generally much higher than that of the equilibrium clause and will greatly exceed it in most cases. That is to say, while there are many bodies that meet the equilibrium criterion but fail the dominance one, we won't find something that meets the dominance criterion *without* satisfying the equilibrium clause (at least in this solar system). For practical purposes, the insertion of this as a third clause superseded the equilibrium clause and rendered it redundant. Procedurally speaking, this seems to have been the point, and a new class of objects was

⁴ Charon's inclusion was on the basis of a clause (later discarded) that recognised double-planet systems: where two bodies orbit one another with the common centre of rotation being outside/above the planetary surfaces. The Pluto-Charon system meets this criterion, which was intended to be analogous to double-star systems.

⁵ The story here is largely taken from (Boyle, 2010) and (Brown, 2012).

⁶ The intent of course was to exclude moons, which do not orbit the sun as a primary.

⁷ The language of the original text referring to 'clearing the neighbourhood' has been criticised for example in (Weintraub, 2008), however the current understanding of the clause is similar to another, more technically precise criterion also formulated by Stern and Levison, which does not rely on this exact language. Weintraub's objections will not be considered here.

introduced for objects that satisfied the first two clauses but not the dominance clause: ‘dwarf planets’.

What is interesting about this messy definition is its conceptual and historical background. I argue that the dissonant criteria in the equilibrium and dominance clauses are likely the result of two distinct polysemous planet-concepts (two of several), each of which is autonomously respectable as a scientific concept – useful and well-justified with its own scientific domain and intellectual lineage. The fact that it took so long for these parallel conceptions to come into conflict, and the circumstances under which they did, provide a useful illustration of how polysemous parallelism is inherently non-obvious while the relevant concepts do not come into contact, how disputes are to be expected when they do, and how (not) to resolve such disputes.

3.2. *Discovering the Solar System*

The word ‘planet’ has a long lineage, originating as a Classical Greek term that generally referred to five solar-system bodies visible to the naked eye: Mercury, Venus, Mars, Jupiter and Saturn. These five were the ‘wandering stars’ that moved throughout the year against the background of fixed stars, it was this motion that set them apart as a separate category of heavenly objects. However some Ancient sources (such as Cicero) refer to seven planets, and some Medieval writers clearly classify the sun and the moon under the term as well: often in the context of the seven ‘celestial spheres’ they were each theorised to be embedded in.

Arguably, the first new planet to be discovered was Earth. The famously revolutionary shift from a geocentric model to a solar system one (whatever else its significance) set the planet count at six.

The realisation that Earth was a planet of course involved a large amount of observational and theoretical development. The development of telescope technology allowed the direct observation of the planets as globes: with the inferior planets Mercury and Venus clearly displaying crescent phases like those of the moon. With the establishment that the Earth was also a globe, and the empirical superiority of Kepler’s model of Heliocentric elliptical orbits around the sun, the modern understanding of the solar system took shape.

But it was the discovery of a causal mechanism – Newton’s law of Gravitation – that created what might be called a genuine planetary science: with the power not just to describe regularities but to also explain and predict them. Moons were moons rather than planets not because of size alone but because they were gravitationally dominated by their more massive primaries and thereby bound to them; in exactly the same way that the planets were gravitationally bound to the Sun. Orbital mechanics was simply Newtonian mechanics; gravitation and the laws of motion were all that were needed to explain and predict the motion of the bodies of the solar system, with no extra cosmic machinery required.

However there is no reason to think that the final nomenclature eventually passed down to us (with a strong planet-moon distinction) was pre-determined. The process of scientific discovery and development from Copernicus to Newton took approximately 250 years, and during that time the exact extension of the term ‘planet’ (as observable from scientific usage at least) was demonstrably uncertain. For example, for some time after their discovery the Galilean moons of Jupiter (Ganymede, Callisto, Europa and Io) were referred to as ‘the Medician Planets’. Also dubbed a planet was Saturn’s largest moon, Titan. We can expect the outcome to have been extremely sensitive to order of discovery and the human contingencies of who was responsible for which observation or publication, their nationalities, politics and intellectual traditions, the rise, fall and interplay of different

‘lingua francas’, and so forth. The contingent make-up of the particular solar system we inhabit also mattered. For example, if the Earth had more than one moon (or no moon at all), then there would not have been the semantic precedent to transform the name of that singular object ‘the Moon’ into a class of objects: ‘moons’, plural. The Medician planets might very well have remained in the astronomical nomenclature, with no planet-moon distinction being made.

In any case though, and for whatever reason, the heliocentric understanding of the solar system with its familiar received description finally took shape. Titan, Phobos and The Moon became moons rather than planets, and The Earth became a planet, rather than Mars and Venus (etc.) becoming ‘earths’. However the hierarchical Sun:planet:moon taxonomy did not comfortably accommodate all the solar system objects.

3.2.1. *Comets*

One of the most celebrated early discoveries in orbital mechanics was by Newton’s friend and collaborator Edmund Halley in 1705, 18 years after Newton published the *Principia*. Halley’s discovered that the comet phenomena of 1682, 1607, 1531 were all appearances of that same solar system object. Its orbital parameters were calculated via Newton’s equations, thereby bringing comets (insofar as their motions were concerned) under the same explanatory umbrella as the planets and moons.

Once it was discovered that the planets orbited the sun, and the comets orbited the sun, it was perhaps possible that the scientific notables of the time would class comets as planets. That did not happen. There were the planets, there were moons, and there were comets. Whatever the term ‘planet’ meant by this time, simply orbiting the sun as its primary was not sufficient for a thing to count as a planet. The list of planets remained the same, with the sole addition of the Earth.

3.2.2. *Uranus*

The discovery of Uranus in 1781 by William Herschel was therefore unexpected. Herschel originally announced his discovery as that of a new comet. Further observation of its orbital path however showed the new body to be more planet-like than comet-like: with a near-circular orbit (unlike the highly elongated, sun-grazing orbits that account for a comet’s dramatic appearance near perihelion) and able to be resolved as a globe despite the great distance.

Naming controversy aside⁸, the discovery of Uranus accelerated the development of astronomy as an observational science: it demonstrated that there were new things to discover in the solar system, lurking below the threshold of easy detection – and some of them were planets.

3.2.3. *Ceres, Pallas, Vesta, Juno*

The next novelties to be discovered were Ceres, Pallas, Vesta, Juno between 1804 and 1807, and it is at this point that the term ‘planet’ undergoes its first overt dispute. Uranus had been found at an orbital distance that matched the Titius-Bode law (a mathematical formula that relates the orbital radius of the planets to a whole-number sequence) with a margin of error of five percent.

⁸ Herschel proposed to name the new planet George, after his patron George III of England. The French scientific community was somewhat against having an entire planet named after this particular monarch, and the compromise “Herschel” was used instead for many years (perhaps because it clearly wasn’t an English name). The name eventually settled on extended the existing sequence of superior planets by classical mythological paternity: Mars is the son of Jupiter, who is the son of Saturn, who is the son of Uranus. Uranus has no parent in the standard mythological narratives (unless it is Erebus, the darkness of pre-creation), so perhaps this also demonstrated a certain lack of imagination (or wishful thinking) when it came to the prospect of further undiscovered planets.

However there was a gap in that sequence between Mars and Jupiter. Ceres, the first discovered of the four new objects, fitted into that gap to within a two percent margin. However all four new discoveries were found in that one orbital zone, and all four were extremely faint and too small to resolve as disks through a telescope.

Herschel saw these four objects as being importantly different from the ‘proper’ planets, and proposed the term ‘Asteroid’ on the basis of their merely star-like (i.e. point-like) appearance in the telescopes of the day. However the language used in texts in this age is intriguing. For example, (Riddle, 1824) uses the terms planet and asteroid in a manner that is not mutually exclusive, by stating: “The planets, in the order of their distances from the sun, are Mercury, Venus, the Earth, Mars, Vesta, Juno, Ceres, Pallas, Jupiter, Saturn, and Herschel”, but also that: “Vesta, Juno, Ceres, and Pallas, which are all nearly at the same distance from the sun, are so small that they are generally termed *Asteroids*.” While (Picquot, 1828) informs us that: “In the Copernican, or true, system of the world, the sun is placed in the centre, whilst around him, at various distances, and with different velocities, circulate eleven primary planets, placed in the following order beginning by that which is nearest the sun: Mercury, Venus, the Earth, Mars, Vesta, Juno, Ceres, Pallas, Jupiter, Saturn, and Herschel. There are besides eighteen secondary planets, or moons, that turn round their primaries, and accompany them in their revolution round the sun”.

The language being used at this time is therefore somewhat unresolved, with a mix of different terms being used to describe and categorise the same small sample of objects, and different uses of the word ‘planet’. However we can roughly resolve two broad extremes: one which excludes the asteroids – call this the Herschellian use – and one which includes them.

What eventually broke the semantic stalemate toward the Herschellian usage was the discovery of *many* more asteroids. Between 1845 and 1851 fifteen were discovered, and the pace of discovery accelerated in following decades, filling out the ranks of ‘the asteroid belt’ (see (Bokulich, 2014) and (Weintraub, 2008) for a more detailed account of this).

The first concession this brought was in the name of pragmatism: planets had previously been assigned a symbol to be used as a shorthand notation in almanacs, publications and navigation tables. This is only helpful if the number of planet symbols to recognise is few (and not increasing every few months) so a numbering system was adopted instead for the asteroids, which is still used. Interestingly, the term ‘minor planets’ was gradually adopted, as a category separate from ‘planets’. The observational nail in the eleven planets coffin came in 1855 when more accurate measurements of Ceres, Pallas and Juno showed that the sizes of those three asteroids had been massively over-estimated. Nature it seemed had two populations of things – asteroids and planets – with a large gap in the mass scale between them.

3.2.4. *Neptune, Pluto and beyond*

Just prior to this time, the discovery of Neptune had marked another important difference in the story thus far. Up until now, the new discoveries (Uranus, the asteroids and the comets) were the result of “look around” observational astronomy. The only regularity used to guide those observations was that solar system bodies tended to be found somewhere close to the ecliptic (i.e. along the plane of the solar system).

Neptune was first definitively observed and verified by the Berlin Observatory on 24 September 1846, after just a one hour search on the basis of a prediction by French mathematician Urbain Le Verrier. Le Verrier’s calculations had sought to account for irregularities in the orbit of Uranus by means of gravitational

perturbation by another planet orbiting further out from it. The planet was found in more or less the predicted location (within one degree of arc), and was immediately and uncontroversially described as a new planet.

At this point we can make a few tentative conclusions about how the use of the word ‘planet’ changed and why. Due to convention and convenience, what we now call asteroids and moons have been relegated to a secondary status with the term ‘planet’ being reserved for objects that more closely resemble the five naked-eye ‘wandering stars’; with the addition of Earth, Uranus and Neptune to that small, neatly ordered list. The rough and ready idea of planets was as ‘big players’ in the solar system, in line with both Herschell’s observational distinction and the successful search for planet Neptune as a paradigm gravitational perturber.

And after the scientific and mathematical triumph of Neptune’s discovery, further calculations based on the observed orbits of Uranus and Neptune appeared to indicate the presence of another planet. The search for this new planet became an ongoing quest in ‘celebrity science’ for entrepreneurial astronomer Percival Lowell (also famous for claiming to be able to observe canals on Mars). In any case, the search for this mysterious planet was again the search for a gravitational perturber – something hefty enough to play a role in how the solar system behaves.

Pluto was finally discovered in 1930 by Lowell Observatory employee Clyde Tombaugh using photographic comparisons of sections of the sky taken weeks apart. As it was found six degrees from one of Lowell’s predicted locations, the initial assumption was that the search had been vindicated. However estimates of the mass of the planet varied widely.

Lowell had been looking for a planet of approximately 7 times the mass of the Earth. But Pluto was too small to resolve as a disc, and far too dim to be so large. Optimistic assumptions (that it had a very dark surface, reflecting only 7% of light that fell on it) gave an estimate of 0.6 of Earth mass, with other initial measurements suggesting upper mass limits of 0.3 of Earth and a best guess of 0.1 (Bower, 1931). However estimates based on the orbital residuals of Neptune and Uranus gave figures of between 0.5 and 1.0 of Earth up until the 1950s (Duncombe and Seidelmann, 1980). Interferometry techniques to estimate the angular size of Pluto as a light source (despite being below imaging resolution) were also consistent with a planet the size of Mars (0.1 of Earth) or larger (Arnold et al., 1979).

Discoveries in the 1970s however delivered two deadly blows to the big-Pluto hypothesis. Spectral measurements of that distant twinkle of light indicated a surface of methane ice (rather than the dark dust which had been previously thought), which meant it would be far more reflective than assumed and therefore far smaller given the quantity of light reflected. More importantly though, Pluto’s relatively large moon Charon was resolved from their joint light-source, and observing its gravitational dance with Pluto finally allowed an accurate mass estimate of just 0.0024 that of Earth. The residuals on which gravitational predictions of a much larger Pluto had been based turned out to be simply the result of earlier observational error. As noted by (Messeri, 2009), (Magnus, 2012) and (Bokulich, 2014), plummeting mass estimates for Pluto were extrapolated by two astronomers to jokingly predict that it would reach mass zero by 1984 (Dessler and Russell, 1980).

However, because Pluto’s actual mass was still more than an order of magnitude greater than the next most massive solar system object then known (Ceres, at 0.00016 of Earth), the lower limit of what counted as a planet simply plummeted down with it. Pluto had been a planet for generations, and those generations of people had passed through the respective education systems

learning that there were nine planets. As long as there was a healthy gap on either side of the planetary mass scale, there was no need to rush to any decisions on where the exact dividing line should be.

With the discovery of QB₂ in 1992 and subsequent large Kuiper belt objects such as Sedna and Varuna, the healthy gap began to fill up. It became increasingly clear that Pluto was merely somewhere near the apex of a much larger statistical cluster of small, icy worlds out beyond Neptune; part of a second belt of small solar-system bodies.

3.3. What the history of 'planet' illustrates

The backstory here illustrates how contingent scientific nomenclature can be, especially when there are limited (and sometimes misleading) data points on which to base scientific analysis.

One of the more striking coincidences is the seeming repetition of the 'demotion' phenomena: with the four large asteroids in the mid-19th century, and with Pluto more recently. In both cases there was a choice between there being twelve planets on one hand (counting Neptune by 1848) and only eight on the other. And in both cases the build-up to the demotion was driven by a combination of observational error (overestimating the masses of small objects), conceptual fixedness (the only things we know in this location are planets, so that must be what this is), and ignoring the basic systemic bias of scientific discovery (the most obvious members of a population will be the ones discovered first).

The part of the story that I would like to emphasise is that (despite its eventual triumph at the IAU) the rise of the gravitational approach to planetary astronomy was just another contingent, merely convenient way of latching on to the planet concept. Solar system bodies can also be categorised in a number of different ways, and the two planet definitions considered by the IAU both carve them up along joints that are scientifically interesting: one at a breaking point in geophysical behaviour (hydrostatic collapse under self-gravitation) and the other at a point of dynamical, inter-planetary behaviour (regional gravitational dominance).

It should also be stated that while the story as I've told it lends an intuitive momentum to the dynamicist position, parallel developments such as robotic planetary science missions (like Voyager and Viking, along with Earth observation satellites) have generated a strong sense of what planets *look* like in themselves: with weather and complex geological features generated by fractionation. Picture a planet as a thing itself (i.e. not just a gravitational data point) and the intuitive heft of the geophysical conception becomes apparent.

These two conceptions of 'planet' have distinct scientific utilities for different purposes. It would have been natural for the sub-disciplines of planetary geophysics and planetary dynamics to latch on to these two different defining features as the 'true' core of the concept. The geophysical conception has the advantage of keeping planethood intrinsic to the planet's own composition, and also latches on to some powerful intuitive imagery: the planet as a globe floating in space, which we might perhaps travel to and stand upon as we would the Earth (just don't think of the moon). The dynamical conception however taps into a lineage of physics in astronomical discovery which began with Newton and passed through the discovery of Neptune and the search for Pluto itself. In modern scientific contexts, the geological conception is the stuff of Mars rovers, and the other is the go-to concept concerning how planetary systems originate, develop, and behave over time.

So there are different scientific or epistemic contexts at play, with different explanatory projects, different uses for a planet-concept, and different ways of latching onto the objects that fall under the concepts

used. The two conceptions under consideration in 2006 each corresponded (very roughly) to a different set of scientific sub-disciplines and explanatory projects; with each conception being obviously-what-'planet'-means within its relevant scientific context, but bizarre and misguided from within rival contexts. This would explain how a small committee of like-minded planetary science experts could come up with a definition in which they were so confident, which yet failed so spectacularly with their erstwhile colleagues. The 2006 meeting was the culmination of a decade or more dispute over the term, but it wasn't until the formalisations were presented for collective choice that the lack of conceptual congruence became so stark and salient. According to science journalist Alan Boyle (who also dubs the 2006 meeting 'the battle of Prague'): "you have to think in terms of planetary conservatives versus liberals – or, more accurately, dynamacists versus geophysicists. The skirmishes over the definition of planethood that took place in Prague weren't so much about poor little Pluto, but about two different ways of seeing the solar system." (Boyle, 2010, p. 115)

Indeed, Boyle's account of what was said by various officials on August 22 provides ample evidence that these two sub-disciplinary 'ways' of seeing things were at cross purposes, and driving the dispute. After a pragmatic appeal from the outgoing IAU president to find a sensible compromise, the head of the panel which formulated the 'roundness' proposal, defended it on that basis that it was "a physical definition without an arbitrary cut-off ... [that] would be letting nature make the decision about what was a planet or not" (Boyle, 2010, p. 124). This was followed by fellow panel member Richard Binzel, professor of planetary sciences at MIT (whose research interests are listed on his MIT page as "Collisional evolution of asteroids; physical parameters and surface features of the Pluto-Charon system"). Binzel seems to have put the cat amongst the pigeons with the following statement: "you can vote on physical principle, that physics is a good way to define a planet. Or maybe you have some preconceived notion of what a planet should be... our recommendation is that you decide based on physics" (Boyle, 2010, p. 125). The first comment when the floor was opened for discussion was from an Italian dynamicist:

Milani complained that the initial resolution didn't give any consideration to planetary dynamics, which was "the historically most important contribution of astronomy to modern science." He said the resolution would wrongly bring Ceres back into the planetary fold, more than a century after astronomers took it out of the solar system's top lineup. Most of all, he took offense at the way the IAU's resolution was being presented as the most scientifically sensible option.

"I would like to note that the two speakers who have spoken so far have both done the same extremely insulting gaffe," he said. "They have used the expression 'a physical definition of a planet' – by implication, suggesting that a dynamical definition is not physics!" He said he felt he had to teach the panel "something you should know": that dynamics was indeed physics, and in fact was addressed before solid-state physics in every textbook ever written." (Boyle, 2010, p. 126)

By the time of the final vote two days later, on the second last day of the conference (once many delegates had already left), the dynamicists had successfully taken control of the debate and had successfully added the third, 'dominance' condition. This was the version of the resolution which finally passed⁹.

Office politics aside, this episode looks like case of revealed, entrenched polysemy: the end-point of a process when distinct

⁹ For first hand reflections and regrets by two senior presiding officials, see (Williams and Bell Burnell, 2006).

senses of a term – developed in parallel within distinct contexts of use and utility – came into stark relief. The concept of planet used by a planetary geologist prior to 2006 and that of a dynamicist are two autonomous conceptions of two distinct descriptive classes for (mostly) the same objects: roughly speaking ‘gravitational eggs’, and ‘gravitational bull-dozer’. Considered independently, both concepts seem perfectly reasonable qua physical scientific concept: both of them are suitable for the purposes they have in the contexts in which they are used. And for as long as the (known) extension of ‘planet’ was the same for both the geophysicists and dynamicist sense of the word (i.e. before the discovery of Sedna, Varuna, and so forth), there was little reason to differentiate the two senses. But then the extensions became contested (with the shrinking of Pluto and the discovery of Kuiper belt objects) and astronomers were asked to decide what ‘planet’ really meant in and of itself. And, as a polysemous term with generations of practice and lineage entrenching the different uses, there was no single, fully-acceptable answer to that question.

4. Discussion

In what way does this story matter, philosophically, and what does consideration of polysemy and parallel conceptual lineages add, philosophically? In 4.1 I argue that the scientific kind defined in the 2006 IAU planet resolution is best understood as an unstable and unparsimonious mashup of two stubbornly distinct de facto scientific kinds, i.e. a refusal to acknowledge that ‘planet’ names two scientific kinds. In 4.2 I argue that the origins and histories of cases like this (with polysemous kind terms which indicate conceptual pluralism and cross-cutting scientific kinds) offers modest support for pluralism about natural kinds. I conclude by outlining the scope and potential applications of this approach.

4.1. Resolving polysemy

Recall that polysemy-based confusion surrounding country-talk could be resolved by being explicit about the frame of enquiry, conversational context, or descriptive utility. Such disambiguation can ‘complete’ the term and help pick out the most appropriate sense and extension. In the case of country-talk, the polysemous uses are familiar enough to be navigated more or less automatically, and competent users of the term are generally amphibious enough to switch between them automatically. The ‘planet’ case is an interesting, specifically scientific case where this was not true – because the known extensions of the differing conceptions entirely overlapped during the time that they (and the scientific specialisations in which they were embedded) were developing. So when the polysemy was revealed it was also novel and more contentious.

In general, there are several moves that can be made when polysemy is revealed. Firstly, we could privilege one particular use of the polysemous term, and retire (or assign other terms to) all other uses. This was (for the most part) the option taken by all factions at the IAU – it was the preferred standardisation which differed.

A second option would be to take a step back from the old term, and define all new, specialised and unambiguous referring terms for the distinct uses of it. For example, the IAU could have defined technical neologisms for geophysical and dynamical planet conceptions (‘planets_g’ and ‘planets_d’, hereafter) to be used in professional astronomy, and leave ‘planet’ to the general public. And this does seem to have been part of Stern and Levison’s initial proposal. The planet (roundness and dominance; both egg/planet_g and bulldozer/planet_d) vs dwarf planet (egg/planet_g only) distinction from 2006 corresponds

to what Stern and Levison dubbed ‘uberplanets’ and ‘unterplanets’ (Stern and Levison, 2002)¹⁰.

A similar move seems to have been attempted with the introduction of the term ‘planemo’ – a neologism proposed about three years prior to the 2006 debates which has seen limited uptake (Basi and Brown, 2006). ‘Planemo’ stands for ‘planetary-mass object’, and describes sub-stellar bodies which meet the Stern-Levison roundness criterion, irrespective of whether they orbit a star or not. This of course is a more ‘pure’ term for that conception, as it also drops the “must orbit a star” requirement, implicit in the dwarf planet/unterplanet notion. The irony of the name of course is that Stern-Levison roundness is now not what it takes to have ‘planetary mass’, given the 2006 resolution.

The 2006 formulation (call it ‘planet₂₀₀₆’), is a messy hybrid of these strategies. One use of the term ‘planet’ (the dynamicist use) was privileged by adding the dominance condition, effectively overriding the equilibrium condition (at least for this solar system, see below). The term ‘dwarf planet’ was also coined to capture Stern-Levison-satisfying objects which failed to satisfy the dynamicist’s criterion. Combining this terminology, dwarf planets are therefore planemos which are orbitally bound to a star but are not (locally) orbitally dominant, while planets₂₀₀₆ are planemos which are both orbitally bound and dominant. Free-floating planemos (which appear to be more common than stars) are neither planets_{IAU2006} nor dwarf planets, though they might have originated as planets_{IAU2006} (and subsequently ejected from their home solar systems).

4.1.1. Complications

Magnus argues that the new planet₂₀₀₆ is as good a natural kind as any. He states that the messiness of the drafting process should be ignored, as the two criteria are not independent:

The criterion (b), roundness, is mentioned separately for political reasons; namely, that it was a separate criterion that had been discussed in the debate leading up to the IAU decision. With that in mind, we might more compactly define a planet in this way: A planet is an object which is not itself a star but which is massive enough to dominate its orbit around a star. This definition is equivalent to the IAU definition, but it makes the explanatory power of thinking about planets more perspicuous. (Magnus, 2012, p. 79)

The dynamicist criterion (and by proxy the planet definition) is then given an extended defence against (Weintraub, 2008) and others who attack the original language of the dominance/dynamicist criterion as not being able to account for Trojan asteroids and other objects which remain in the orbital neighbourhood of planets.

To a certain extent this is fair enough; Magnus is entirely correct to defend the dynamicist criterion as a substantive, non-gerrymandered physical boundary of scientific interest, and there is a certain neatness in fixing on that as the key criterion given that all gravitational bulldozers are gravitational eggs. Buried by the more demanding dynamicist criterion there is little hope that the roundness criterion will come into play for discriminating planets from non-planets in this solar system.

But it is simply false to say that the two criteria *can’t* come apart: the orbit-dominating ‘oomph’ required for a planet candidate to satisfy the dynamical criterion increases with mass but also decreases with orbital distance from the parent star¹¹. It is therefore at least

¹⁰ In their nomenclature, these would be clarifying terms for two subsets within a general class ‘planet’.

¹¹ As a rough visual metaphor, consider a planet’s constituent matter stretched out into a thin continuous hoola-hoop along its orbital circumference; the mass-per-unit-length of this hypothetical hoop of matter gives an idea of a planet’s capacity to dominate that orbital zone. However the dynamical dominance

conceptually possible that a small star might host bodies which orbit so close to it as to be able to dominate their tiny orbital environment while not being intrinsically massive enough to satisfy the roundness condition – i.e. to be planets_d but not planets_g, and therefore not planets₂₀₀₆.

Now determining whether this is actually nomologically possible would involve the minimum masses/sizes for stars, minimum masses for planets_g of various compositions, and allowable scenarios for orbital dynamics. For example, perhaps exotic systems with amorphous carbon/diamond bodies rigidly resisting hydrostatic collapse and tightly orbiting compact neutron stars might manage to invert the usual order of criterion satisfaction: bulldozers yes, eggs no. A less speculative (but entirely counterfactual) case can be made by considering the amorphously shaped icy moons which orbit within and around Saturn's rings, and which corral the ring particles into the characteristic band pattern via close passes and orbital resonance effects. Were Saturn a (impossibly) low-mass star, and with enough other counterfactual tweaking these moons might also pose a counter-example; depending on exact criteria for orbital dominance.

In any case, and strong defences of the dynamicist criterion as a putative natural kind boundary-setter are not (strictly speaking) arguments in favour of the planets₂₀₀₆ as a natural kind. It is an empirical question whether there are any such miniature dynamical planets out there to be discovered (they would likely be beyond the reach of any foreseeable detection techniques). However this would be the actual test case for planets₂₀₀₆. Intuitions regarding currently known bodies in this solar system do not discriminate between planets₂₀₀₆ and planets_d.

And on the empirical question of uptake, it is not clear that scientific language is actually converging toward the new definition, even in peer reviewed publications. While Ceres and Pluto have been dutifully referred to as 'dwarf planets' in recent coverage of the Dawn and New Horizons missions, other evidence is not so clear (Yeager, 2015). For example, gravitationally unbound (i.e. free-floating) planets should be an oxymoron, yet titles like "Astronomy: Bound and unbound planets abound" still appear in *Nature* (Wambsganss, 2011). And while some discovery papers hedge with language like "free-floating planetary mass analog to directly-imaged young gas giant planets" (Liu et al., 2013), others happily refer to them as planets (Beichman et al., 2013; Bennett et al., 2014; Delorme et al., 2012), as do investigations into possible composition and origins (Abbot and Switzer, 2011; Badescu, 2011; Veras and Raymond, 2012). While the evidence here is far from conclusive, it is certainly compatible with the idea that a 'roundness' or observationally-mediated conception of planets is still in play, and may not simply disappear from scientific discourse. For the moment at least the term still seems to be covertly polysemous when applied outside our own solar system, and this is interesting in itself.

4.1.2. *Semantic pluralism and monism: explaining the resistance*

This all illustrates a further possible response to polysemy: to not resolve it at all (neither introduce new terms nor standardise use of the old one), but instead manage the ambiguity and make efforts to disambiguate when context does not do this automatically. This would be a form of quiet pluralism about the terms in question – making no changes to the language and instead

recognising distinct, contextually separated (and useful) concepts. 'Planet' was polysemous; let it remain so.

However, two key features of polysemy can be predicted to work against this, and may have done so in this case. The first is that polysemy can be easy to ignore, and can be surprising or a source of acrimony once revealed. The other is that polysemous terms typically diverge or branch off from earlier, more unified usages. Together, these features mean that a polysemy analysis (such as I attempted in Section 3) can be predictive of observed behaviour.

While a more in-depth investigation would be necessary here, we can at least begin to speculate on the basis of the differences in the cases considered. For example, unlike country-talk where each competent language-user will take part in a full variety of conversational contexts easily distinguishable from one another, there was never much reason to tease apart the different senses of 'planet' until recently. According to the story I told then, internalising both uses was generally not necessary, and astronomers would likely gravitate to the usage most pertinent to their professional focus; canalising and sustaining two traditions of use more or less in parallel. And each tradition would have reason to see their work as continuous with the intellectual heritage of planetary astronomy (as each saw it). In other words, the conceptual lineage of the term has branched, but from inside the specialised, canalised utilities of each branch there need be little concern about this – little motivation to be conceptually amphibious in the sense of cultivating plural internalisations. We talk normally, it's those other folks who have an accent.

So the story would be that polysemy manifested at an interpersonal level, between usage-bound experts rather than merely between uses – there were scientists from different (sub)disciplines talking past one another, rather than individuals employing the different uses in different situations. What both sides of the argument had in common though was a presupposition of monism, so it would have been an *in vivo* case of conceptual pluralism but without any pluralists to argue that case. Understanding the process in a historical context as (partially) the result of concealed polysemy therefore both makes a case for pluralism, and explains why no one was interested in it.

Again, this is a speculative reading of the case – a hypothesis about historical polysemy. In particular, my supposition of robust, parallel conceptual and intellectual lineages which fissioned from a more unified original usage is open to question. So while I have inferred dual uses in the 19th century and in the 21st based on how the term was deployed and argued about, to properly follow through on this would involve establishing some sort of continuity from the Newtonian-inspired Herschellian usage to the modern dynamicist conception on one hand, and to the geophysicist's conception from the observational 'looking at things through telescopes' usage (whatever that was) on the other. This would be a much larger project.

But even if it is not entirely accurate, the hypothesis at least provides an interesting prototype for a class of disputes we might want to recognise in the abstract: technical disputes over a concept where pluralism may well be a parsimonious option, yet where this option is ignored, underrepresented and/or resisted outright because of canalised conceptual entrenchment. This then is the first kind philosophical work that a polysemy analysis can perform. In the case of monism-on-monism disputes where the camps tend to divide along sub-disciplinary lines, there is potential for a natural history of the conceptual lineages in question to help explain the dispute. If the historical polysemy hypothesis fits the facts of the case, then this can motivate stepping back from the first order dispute to consider whether or not a pluralist approach might instead be a better option.

(footnote continued)

quantity usually appealed to is the probabilistic Λ (lambda) quantity, spelled out by Stern and Levison to distinguish uberplanets from unterplanets and which relates mass and orbital elements to the likelihood (over Hubble time, or about 13.8 billion years) of interacting with theoretical point particles, thereby deflecting them (Stern and Levison, 2002).

There may be other disputes for which the hypothesis might be apt, but for the moment this conclusion should not be overstated. It is a sociological point, and considers only arguments from observed usage. It addresses debates but not their content: giving us a pro tanto reason to consider pluralism (where it might not otherwise be) without considering what a pluralist treatment of any particular dispute might look like and how or if it might work. The question is then: why should polysemous scientific language be any reason to be a *realist* about natural kind pluralism? I turn to that question now.

4.2. Polysemy and Natural Kinds

Thus far I have avoided discussing the metaphysics and semantics of natural kind. Kinds in language are cheap: some kind distinctions are artefactual ('gable roof' vs 'hip roof'), or otherwise based on human interests or with arbitrary divisions (like 'millionaire', or the distinction between hills and mountains based on an arbitrary altitude threshold). Scientific kinds on the other hand are supposed to be more natural and objective than the cheap kinds of ordinary language and convenience; and this is not just a philosopher's ideal. Scientists often see themselves as drawing distinctions where science tells them there are distinctions to be drawn, irrespective of where our own interests or habits of language might incline us to look. In the context of the planets debate this was probably best summed up by one of the participating astronomers:

"ad hoc definitions devised to retain Pluto as a planet tend to conceal from the public the paradigm shift that has occurred since the 1990s in our understanding of the architecture of the solar system. [...] To be useful, a scientific definition should be derived from, and draw attention to, the basic principles [of nature]" (Soter, 2006), also cited in (Bokulich, 2014).

These ideas (that the universe has a definite 'architecture', with distinctions to be drawn from 'basic principles of nature') dovetail with the familiar philosophical conception of natural kinds in the Kripke-Putnam theory of reference, which extends accounts of reference for proper names to natural kind terms (Bird and Tobin, 2012). Considerable philosophical progress and success hinges on (at least some) scientific kinds and Kripke-Putnam natural kinds (or something similar) coinciding.

So, usual injunctions against simply reading metaphysics off of language notwithstanding, a historical/linguistic analysis of scientific kinds (such as with polysemy cases) has a clear justification. Technical scientific concepts which play important roles in good scientific theories are in a different league from kind terms the result of mere human convenience; in that a) scientific method includes a commitment to naturalness when forming scientific terms, and b) successful scientific theory is our best guide to what natural kinds there really are. Metaphysics describes the format of natural kinds and the rules they must follow, but this format must be apt for usage and content from the sciences. What a historical polysemy analysis does is not read natural kinds off of language, it reads scientific concepts off of scientific language – giving us a guide as to how they evolved and diverged over time, how the successful ones came to be successful, and how they are related to one another. Some sort of link between successful scientific concepts and natural kinds is already in play, and a polysemy analysis provides evidence regarding the former.

Figuring out what historical polysemy in science might mean for natural kinds therefore leads through some well-worn territory. A crude characterisation (sufficient for current purposes) is that there is a tension between the ideal of metaphysically 'clean'

natural kinds on one hand, and the (sometimes) more messy and pragmatic kinds which can be inferred from best science. While stock cases of chemical kinds (like gold and H₂O) are usually taken to line up suitably as Kripke-Putnam natural kinds¹², not all scientific kinds fit the template quite as well¹³. This means that there is a tension between metaphysical/essentialist and scientific/pragmatic readings of natural kinds; the various proposed compromises are contested.

Magnus for example reads off the literature a list of what he sees as eleven 'adages' about natural kinds; common (though not universally held) assumptions or desiderata for natural kinds, not all of which (he argues) can be or should be admitted into a coherent account. These include 'metaphysical' desiderata such as intrinsically (kind membership should be intrinsic to the member, not extrinsic or relative to contingency of function), essentiality (kinds have essences), sharpness of boundaries, and there being a small number of natural kinds with hierarchical structure (as opposed to overlap), and also appropriateness for scientific enquiry, indicative inference, natural law-making, and direct reference (Magnus, 2012, p. 8). Magnus, as a pragmatist, argues that many scientific kinds violate many items on this list (mostly from the metaphysical camp), and that these desiderata will need to be sacrificed if science is to remain a guide to natural kinds.

This is the general form of argument which forces a choice between a clean, metaphysically austere conception of natural kinds, and a strong link between natural kinds and scientific enquiry. If science is deferred to with regard to *scientific* merits of this or that kind schema, then there are only two obvious moves to make when the kinds supplied by science lack metaphysical parsimony: restrict natural kinds to a certain subset of scientific kinds, or relax our desiderata for what it takes for a kind to be a natural kind, so that all/most natural kinds and scientific kinds remain in lock step. Making the first move requires a principled way of privileging some scientifically derived kinds over others, and undermines the relevance of 'natural kinds' to science. However making the second move typically risks admitting an unwieldy and perhaps incoherent plethora of natural kinds.

4.2.1. Monism vs Pluralism

The planets debate is rich pickings for several of these issues, discussed in more detail in (Magnus, 2012) and (Bokulich, 2014). For example, as already mentioned, the first and third criteria of the IAU planet definition are explicitly extrinsic – orbiting a star, and domination of the local orbital environment. Like 'peak predators', 'planets' are as planets do, and being a planet is neither intrinsic to the particular planet (Mars is only a planet because Jupiter is far enough away from it) nor essential to it (were Mars to be ejected from the solar system or become dominated by Jupiter it would cease to be a planet, without ceasing to be¹⁴).

¹² For dissenting views though see for example (LaPorte, 2003, pp. 92–111), (Slater, 2005) and (Weisberg, 2006).

¹³ Of particular interest here are examples of cross-cutting systems of kinds, some of which can be traced to particular human interests beyond the austere ontological. For example, the earlier cited division of asteroids into Atens, Apollos, and Armors is a specifically anthropocentric division: as these kinds of asteroids are distinguished by how closely (and the manner in which) their orbits approach the orbit of the Earth. These can be said to be non-scientifically interest-driven scientific kinds – driven by our interest in knowing how likely they are to hit us. And divergent scientific interests are also at work, with asteroids also divided into systems of kinds based on composition (such as C-type/carbonaceous as opposed to M-type/metallic) which cross-cut those of orbital classification. Similar cross-cutting systems of classification exist for stars (Ruphy, 2010), dividing populations of stars according to spectral properties, luminosity, size, magnetic activity, origin, association and so-forth. Each of these systems arose to serve distinct sets of explanatory purposes.

¹⁴ To clarify, what this violates is not the desiderata that natural kinds have essences, but rather that such essences are also the essences of members of natural

The interest for current purposes though is competing, discipline-specific natural kind schemas and how to reconcile them; i.e. the choice between monism on one hand (allow at most one discipline's demarcation of natural kind boundaries as correct) and pluralism on the other (allow multiple, incommensurate demarcations).

One key motivation for pluralism is that, in the face of entrenched, incommensurate scientific kinds, monists can be caught in what looks like an epistemic dilemma. If best science (*in toto*) invokes multiple, independently indispensable kind distinctions which are incommensurate with one another, then the monist must hope this will turn out to be an error which final science resolves, or can otherwise be second-guessed and reinterpreted (in some principled manner) with a single (perhaps nested) system of natural kinds. The alternative for monism would be an unattractive form of mysterianism: to insist that there is one true natural kind schema but that science is an insufficient guide to that reality. Some form of pluralism might look like a parsimonious option in comparison.

4.2.2. *Species*

The costs and benefits of the various positions regarding natural kind monism and pluralism have been most thoroughly explored in the context of biological kinds, particularly in the extensive literature on the so-called species problem. Indeed, Magnus, Bokulich and Ruphy (Ruphy, 2010) all introduce their respective cases of astronomical kinds (planets for Magnus and Bokulich, stars for Ruphy) as problem cases from the physical sciences to supplement the evidence base from biological kind examples (and other cases from the special sciences). As a well-explored example, it is useful to focus on biological species for a moment, to be more precise about what a polysemy-style analysis can add to the pluralism debate.

In the species problem, the driver toward pluralism is the differing conception of species in biology. Starting with Ernst Mayr (Mayr, 1942), there has been a long literature devoted to defending or contrasting competing ways to delineate species. More recently, John Wilkins extracts over twenty species concepts from the literature, which he groups into “‘Reproductive Isolation Concepts,’ ‘Evolutionary Concepts,’ ‘Phylogenetic Concepts,’ ‘Ecological Concepts,’ and a trash can category of ‘Other Concepts’” (Wilkins, 2009, p. 197). These conceptions constitute different methods of specifying biological taxa, which might agree in most cases as to whether a particular lineage or population constitutes their own species, however there are also divergences – the list of species generated by one method (applied to all the same lineages and populations) will differ from the lists generated by other methods. Given the plurality of species concepts in science, various pluralist positions about biological natural kinds have been articulated, for example by Philip Kitcher (Kitcher, 1989, 1984), Marc Ereshefsky (Ereshefsky, 2010a, 2010b, 1998, 1992), Ingo Brigandt (Brigandt, 2009, 2003) and most radically John Dupré (Dupré, 1993).

These authors agree that some form of species pluralism is required, at least at the pragmatic level of scientific kinds, because

a single conception of species can't do all the scientific work that 'species' is actually used for. There are multiple concepts at play, each justified by some sort of explanatory utility, and/or entrenched within a perfectly respectable faction of the overall scientific project. As discussed in the abstract, a species monist is therefore stuck with arguing the metaphysical inferiority of all the alternatives to their preferred precisification, despite localised scientific commitments to those alternative kinds (undermining the general inferential connection between scientific kinds and natural kinds).

The theoretical costs on the pluralist side are also significant however. One simple worry, in David Hull's words, is: “the great danger of pluralism is ‘anything goes’”. In order to avoid this end of the slippery slope, criteria must be provided for distinguishing between legitimate and illegitimate species concepts” (Hull, 1999). In contrast, Dupré's view, promiscuous realism, is distinguished exactly by letting almost anything go: the kinds of any useful kind-schema (no matter how interest-relative or cross-cutting of others) are just as legitimate candidates to be counted as natural kinds as any other¹⁵. Any attempt to limit pluralism to a more moderate version will face a similar requirement that monism does: a principled way to pick out the ‘best’ kind schemas as natural. And it is open for the dedicated monist or antirealist about species (or natural kinds in general) to see promiscuity like this as a reason to doubt the sincerity of the pluralist strategy, and perhaps even the soundness of the science as a guide to deep metaphysical questions.

A second challenge for pluralism is the very disunity of the species concepts. For example, Ereshefsky argues that having different conceptions (with rival classification schemes) within a pluralist model itself promises to fatally undermine taking a unified, realist stance toward the plurality: “If species taxa lack a common unifying feature, then we have reason to doubt the existence of the species category. What, then, is common to the entities referred to by the term ‘species?’” (Ereshefsky, 1998). If these concepts have different content then they are different concept, and should be recognised as such. Ereshefsky therefore calls his view eliminative pluralism, in that he denies that ‘species’ is a natural kind at all and instead recognises three kinds he calls ‘biospecies’, ‘phylospecies’ and ‘ecospecies’, which are the kinds being referred to in reproductive science, the science of phylogenetic lineages, and ecological science (respectively).

4.2.3. *Limited support for limited pluralism*

This quick survey of the literature is of course less than comprehensive. However we can now say something about what a polysemy/lineage analysis can add to the debate.

There are two motivating reasons for pluralism (to some degree of ontological commitment). The first is the assumption that the concepts invoked by best science are a guide to what objects and kinds exist. The second is the observation that best science sometimes leaves two or more incommensurate incarnations of a concept as scientifically indispensable. There are also two broad questions about it: when can we justify pluralism over monism (beyond the initial motivation), and, assuming that competing concept-incarnations are really indispensable to scientific endeavours, why we should not instead treat them as entirely independent concepts? Why pluralism, and why pluralism rather than just fragmentation?

Firstly, *historical* scientific polysemy – branching lineages of entrenched concepts within autonomous areas of science – offers

(footnote continued)

kinds, and that natural kinds must be metaphysically fundamental substance kinds for individuation and persistence as in the sense of (Wiggins, 2001). These two essentialist theses are distinct and species membership essentialism is acknowledged to be the far less plausible thesis in the current natural kind literature (see for example (LaPorte, 2003, pp. 52–62)). The substance kind interpretation of this example – that Mars would cease to exist and be replaced by a numerically distinct (non-planet) entity that was intrinsically, qualitatively identical to the former planet – is one I am simply assuming to be implausible (though stranger bullets have been bitten, and a related issue occurs in the personal identity literature with the so-called ‘corpse problem’, see (Hershenov, 2005, 2009; LaPorte, 2009; Olson, 2004)).

¹⁵ It is worth noting that Dupré's view is a global one about concepts and natural kinds in science, in general, and not restricted to the concept(s) of biological species.

an additional, qualitatively different evidential standard for plural indispensability.

We can imagine numerous, arbitrarily narrow precisifications of concepts that might be useful or indispensable within numerous, arbitrarily narrow explanatory projects. But the point of historical polysemy is that there is a fact of the matter to be discovered, not just proposed or observed from current science. Dynamicists have good reason to think that, via Newton, their conception of ‘planet’ has some sort of continuity with that of the earliest astronomers. But this is also true of the non-dynamical conception of planet (let us assume), and so we have a fission case: the persistence of a valuable scientific concept, except twice over. Evidence of conceptual *fission* among respectable scientific lineages is a stronger evidential basis for the need to recognise plurality in our stocktake of indispensable scientific kinds; over and above indispensability as currently and synchronically evidenced.

So if we take scientific indispensability as evidence for natural kinds in reality, then the observation of branching lineages adds to that evidence by embedding the conceptual plurality in a phylogeny of scientific kinds and their ontological commitments. Where it applies, this approach can add weight to pluralism, over and above synchronic evidence of overt scientific claims or rational reconstructions with respect to the current commitments of best science.

However the evidential standards of this approach are both high and conservative. Not just any usage can be appealed to, it must be both conceptually continuous and indispensable in actual, successful scientific practice over an extended period of time. Thus there is no danger of supporting ‘anything-goes’ promiscuous pluralism, because to be evidential there actually has to be a history of robust polysemy to be discovered, which ‘anything goes’ pluralism is simply not capable of taking advantage of. Flags of conceptual convenience don’t count until there is an established history of them being saluted by scientific practitioners. Establishing whether this holds in any given case will require a specific empirical investigation of the lineages and usage(s) of the terms in question, looking for branchings of autonomous, indispensable and robust uses; for example via the branching of genuine scientific enterprises and lines of inquiry (planetary dynamics and planetary geology, in my example).

Finally, being able to describe at least some cases of pluralism as the result of historical fissionings (evidenced by historical polysemous usages and their development) can also provide additional reason to resist or at least reframe Ereshfsky’s challenge of eliminative pluralism. Assume for sake of argument that there is a polysemy-by-branching story to be told about the species concepts. If so, we can say that the species concepts are united not by content, but by their common origin in the earlier, undifferentiated species concept. The different species concepts might have no single privileged umbrella concept to which they belong by virtue of parthood or similarity, but they are all part of the same *conceptual clade*. To push this ironic metaphor a little further, with conceptual cladistics we could re-describe the different candidate species concepts as the evolved products of selective pressures within particular scientific environments and niches – the different sub-disciplines and explanatory projects to which they adapted. If enough adaptive radiation has indeed occurred, then such daughter concepts might truly be seen as distinct under any reading of what concepts are and what they do (akin to speciation), yet are still recognisable and properly recognised to be species concepts, plural.

Generalising this may not entirely satisfy the eliminativist charge that there is no single inheritor of the mantle of ‘species’, or ‘planet’, and instead there are several concepts with equal claim. I have said that they would all be species or planet concepts, in some sense. What is the difference between this position and

saying that there was never any simple species concept in the first place, only a period of time when the biospecies, phylopecies and ecospecies concepts were being conflated?

In this context, the eliminativist position would be analogous to Lewis’s on fission cases in personal identity (Lewis, 1983). Lewis holds that the existence of two individuals (B & C) who result from the fission of an earlier individual (A), where either of them would otherwise have been considered as being identical to the original (were the other not to have existed), means that in spite of appearances, there was no one A originally, but only an ‘overlapping’ of B and C. So, while it is difficult to answer the hypothetical question from the eliminativist without delving into the actual conceptual history, the answer will also hinge on what concepts are and how they are individuated over time, and so this is where the fission metaphor may become much more complicated and perhaps break down. For present purposes, I will simply mention Derek Parfit’s preferred non-solution to fission cases as another analogy: Parfit holds that A, B and C are each singularly individuated, but that it is *indeterminate* which of B or C is rightfully A. Flipped back to considering branching conceptual lineages rather than branching individuals, this is the picture which most happily compliments Field’s intuition of the indeterminacy of reference across revolutionary change, and leaves pluralism sitting more comfortably with a common-sense way of individuating concepts.

This analogy has been resorted to because up to now I have been talking in a loose manner – using a rough and ready notion of conceptual change whereby scientific concepts themselves adapt to their scientific environment, which implies that they persist through such changes. Taken literally this is a controversial claim, though it also has its defenders (see for example (LaPorte, 2003)). Taken less literally, much of the story would still be possible as metaphor; or perhaps explicable by regarding conceptual lineages as lineages of discrete, fixed-content concepts – adopted and discarded in the process of scientific progression within a given branch of scientific enquiry (like generations of a population with fixed traits). But precisely how to individuate concepts or make sense of conceptual change is not necessarily crucial for current purposes, and will not be delved further into here. In any case, while more might be expected by way of a coherent formal story in order to support my general picture on this last point, the earlier points do not hang on this.

Time then to down tools and sum up what has and has not been delivered.

5. Conclusion, applications, limitations

I have argued toward several conclusions. The most immediate is that the planets debate has a deeper and more interesting conceptual history than it would seem on the surface, and that it is best understood as a case of conceptual branching due to the emergence of specific scientific projects and specialisations. I argued this by looking at the polysemy of the term ‘planet’ in the 19th century, and suggested that this had persisted to underpin the conceptual dispute which came to a head in the 2006 IAU meeting. This analysis also has the virtue of making a sociological prediction borne out by accounts of the meeting: that in cases like this – cases of historical, canalised polysemy and conceptual pluralism – we can predict the sort of conflict between experts which in fact occurred; a dispute of intensity and duration which seemed to take all involved by surprise. Part and parcel of this is the prediction that pluralist alternatives to such disputes will satisfy no one directly involved, so investigating them may well be work largely left for philosophers and historians of science to take the initiative on.

I also identified three ways in which an historical study of the emergence of entrenched polysemy might add to the general philosophical debate over pluralism about scientific concepts and natural kinds. I argued that it provides evidence not just of the indispensability of plural concepts, but also of their in-principle propriety: for each end-node concept produced by an historically branching conceptual tree (of the right kind, robustly embedded specialising scientific practices), we would have had no problem identifying that node as the proper successor concept – as long as it were the only one. If there is more than one then that makes pluralist approaches more desirable. However this backing for pluralism has a built-in constraint: determined by facts about historical usage, it can't run to 'anything goes' because not everything did. Finally, I suggested that with the notion of 'conceptual cladistics' that in some cases the move from pluralism to eliminativism can be resisted, though whether this move would satisfy an eliminativist is open to question.

Taken as a whole, this provides something of a philosophical recipe for generating or supporting pluralist positions with respect to conceptual disputes (in specific, limited cases), and a template for potentially debunking 'natural histories' of why monist-on-monist disagreements tend to dominate. It is in this sense that I referred to it as a 'philosophical heuristic' in the introduction, as it can be applied in any apparent disagreements which fit the historical template. I don't claim that this is an original recipe, in fact I am sure that it (or parts of it at least) are used all the time. For example, though their analysis and terminology differs, a similar story for the 'gene' concept is described by Paul Griffiths and Carola Stotz (Griffiths and Stotz, 2013, 2007; Stotz et al., 2004), with multiple, distinct concepts of 'gene' with their own distinct contexts and intellectual lineages of use, though with more 'amphibious' users:

"The gene today has several identities which have accumulated as the molecular biosciences have developed and diversified... [e]ach of these identities plays a productive role in some form of biological research. Scientists are adept at thinking about genes in whichever way best suits their work, and at switching between these different representations of the gene as the nature of their work changes." (Griffiths and Stotz, 2013)

Likewise, this should not be seen as a rival to other attempts in the literature to develop and make sense of (non-eliminative) pluralism about natural kinds. For example in the species case it would be broadly compatible with and perhaps supportive of (Brigandt, 2003) who argues that 'species' is/was an 'investigative kind'; a kind concept that groups entities by similarities or patterns in nature which may or may not be unified by a common underlying mechanism or structure. My approach is a form of evidence with respect to the diachronic structure of scientific thinking and practice, which can supplement explicit scientific findings and declarations for purposes of philosophical interpretation. How it might plug in to more sophisticated interpretive projects is left undiscussed.

In my story, what sets the planets case apart from this was the paucity for many decades of actual data points to make the different concepts/representations salient: until the downward refinement of Pluto's mass the different conceptions of 'planet' were extensionally equivalent. This is the sort of historical environment in which conceptual disputes for the future can gestate unobserved. In cases where it can be made, an historical polysemy analysis just adds a further *pro tanto* reason toward pluralism of some form, and to a level of ontological commitment dependant on external considerations. For *realist* pluralism about natural kinds we would still need all the usual reasons for natural kind realism while holding such reasons *more than once*, and have these reasons collectively outweigh any reasons (philosophical of otherwise) for resisting pluralism. In such cases it would be

reasonable to split the term, and admit a messier ontology than what we might otherwise.

As a philosophical heuristic then, it will be most useful in disputes where pluralism is *covertly* parsimonious: in that it has been ignored resisted because of conceptual entrenchment by the specialities which trade in the different conceptual precisifications.

If intellectual lineages in philosophy also admit of progress, diversification and conceptual entrenchment, then perhaps we should also be looking there for underexplored pluralist possibilities involving concepts that are known to serve many purposes. Some seemingly intractable disputes might in part be driven by the competing conceptual familiarities within distinct philosophical areas, and some interesting correlations between areas of speciality and philosophical views suggest that this might be the case.

Return to the personal identity debate for example. Professional philosophers who self-identify as having an area of speciality in philosophy of science are much more likely to endorse the biological view of personal identity, while AOS in moral and political theory correlates strongly with the psychological view.¹⁶ Anecdotaly, metaphysicians are the main proponents of bodily identity theories with narrative identity championed by ethicists. This is consistent with a conceptual entrenchment/polysemy hypothesis. Some philosophers are already sympathetic to pluralist or 'no fact of the matter' views about personal identity (Sider, 2001), and divergent uses of 'selfhood' are already recognised in moral philosophy, action theory and personal identity (Velleman, 2006, pp. 1–16). Pluralism about *numerical* personal identity might look like a batty view (and have its own special metaphysical and epistemic issues), but perhaps a close reading of the philosophical usage of 'person' and associated terms (taking Locke as a common ancestor perhaps) might make it seem less so, and also make resistance to it seem more like par for the course.

Looking at this and other possible applications are obviously tasks for another day. Likewise, I have not said anything about standards for *scientific* robustness: for example the fact that 'planet' has a robust lineage of use in astrology has to be excluded from the pool of natural kind evidence-gathering somehow, so the demarcation problem remains important. The relevance of all this for conceptual change and ontology will similarly be left dangling: the outcome of this approach with respect to those debates will depend very much on other moves made within them.

In conclusion then, I have argued that the history of 'planet' shows it to be a case of the revealed polysemy of distinct, entrenched scientific kind concepts, with a common lineage. I also argued that pluralism may have been the more appropriate outcome of that dispute, despite understandable appearances to the contrary. The case illustrates how histories of conceptual lineages in science can (in some cases) underpin a 'conservative pluralist' position toward the relevant concept(s), and how some debates which involve deep conceptual lineages might also systematically bias against pluralist or no-fact-of-the-matter positions. This suggests a heuristic that might be useful both in identifying disputes of this kind and potentially in resolving them.

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¹⁶ This is based on linear correlations from a 2009 survey of professional philosophers, available here: http://philpapers.org/surveys/linear_most.pl. A summary of the results of this survey is published in Bourget & Chalmers (2014).

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