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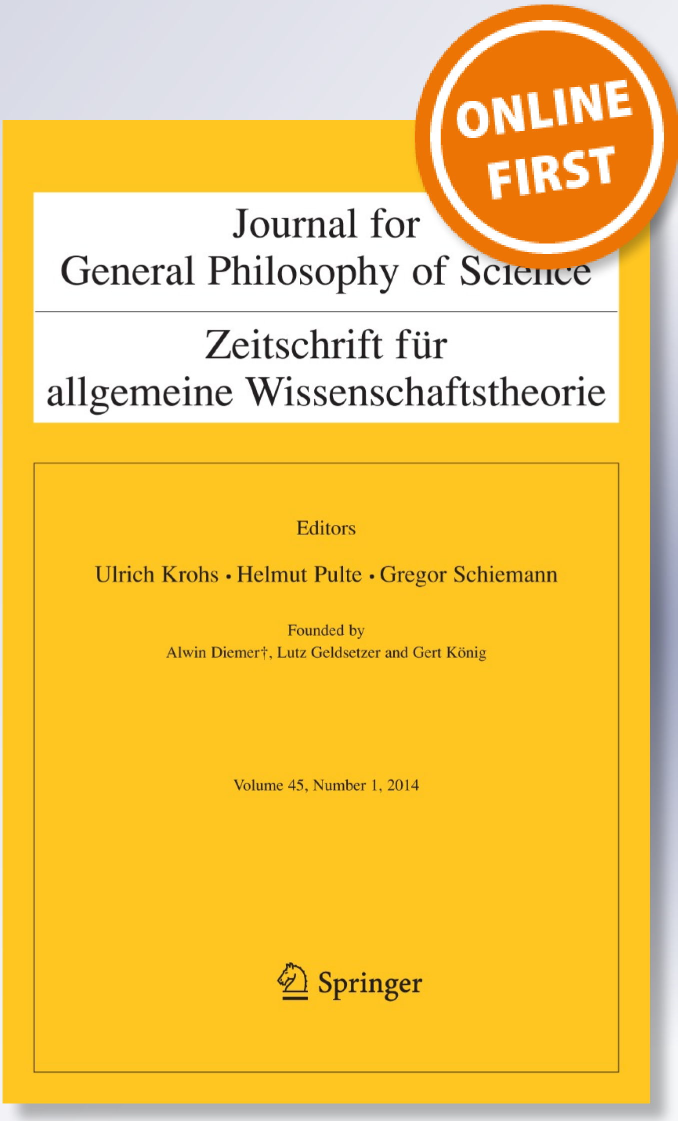

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ARTICLE

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Abstract Baker (Br J Philos Sci 54:245–259, 2003) argues that quantitative parsimony—the principle that hypotheses requiring fewer entities are to be preferred over their empirically equivalent rivals—is a rational methodological criterion because it maximizes explanatory power. Baker lends plausibility to his account by confronting it with the example of postulating of the neutrino in order to resolve a discrepancy in Beta decay experiments. Baker's account is initially attractive, but I argue that its details are problematic and that it yields undesirable consequences when applied to the case of astrophysical dark matter. Baker has not succeeded in showing why quantitative parsimony is a theoretical virtue.

Keywords Simplicity · Quantitative parsimony · Scientific methodology · Theory choice

1 Introduction

Simplicity is widely thought to be a theoretical virtue. Where rival scientific theories are predictively equivalent, the simplest one is supposed to be most choice-worthy. There is much less agreement about *why* simpler theories should be preferred. Is simplicity somehow linked to truth, or is it merely a pragmatic virtue dependent upon human capacities and inclinations? Deciding what makes simplicity valuable as a methodological principle of theory choice, and in what way, is difficult in part because simplicity has many different facets and it is invoked for many different purposes. It will be useful for the present discussion to distinguish three kinds of simplicity (without prejudice as to whether or not there may be others):

1. *Elegance or syntactic simplicity* is a property of theories as linguistic structures: the number and complexity of the hypotheses comprising the theory determines its degree of syntactic simplicity.

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2. *Parsimony or ontological simplicity* has to do with the number and complexity of the entities a theory postulates. This category may be subdivided as follows:
   (a) The degree of *qualitative parsimony* of a theory is determined by the number of *types* of entities that the theory postulates.
   (b) The degree of *quantitative parsimony* of a theory is determined by the number of *individual entities* (of a given type) that the theory postulates (see Baker 2003, 247).

David Lewis writes: "I subscribe to the general view that qualitative parsimony is good in a philosophical or empirical hypothesis; but I recognize no presumption whatever in favour of quantitative parsimony" (Lewis 1973, 87). Daniel Nolan responds: "I wish to take issue with this view. I claim that not only ought we not multiply types of entities beyond necessity, but that we should also be concerned not to multiply the entities *of each type* more than is necessary" (Nolan 1997, 330). To motivate this view Nolan discusses two examples from the history of science—the postulation of the neutrino and the proposal of Avogadro's hypothesis—in which he sees quantitative parsimony actually being used as a principle of theory choice. However, Nolan is unable to come up with a theory of quantitative parsimony, and so he ends his paper with a challenge to others to "work out why in general quantitative parsimony might be thought to be a good thing, and then see from there how wide its applicability is" (Nolan 1997, 342).

Baker (2003) attempts a partial answer to Nolan's challenge. Baker defends the thesis that "there is a wide class of cases for which the preference for quantitatively parsimonious hypotheses is demonstrably rational" (Baker 2003, 245). Baker limits his discussion to "additive" cases, those cases that

> involve the postulation of a collection of individual objects, qualitatively identical in the relevant respects, which collectively explain some particular observed phenomenon. The explanation is 'additive' in the sense that the overall phenomenon is explained by totaling the individual positive contributions of each object. (Baker 2003, 248)

In these cases, Baker argues, "the preference for quantitatively parsimonious hypotheses emerges as one facet of a more general preference for hypotheses with greater explanatory power" (Baker 2003, 258). That is, it is rational to prefer hypotheses that are more quantitatively parsimonious precisely because they have greater explanatory power.

While I am sympathetic to the idea that quantitative parsimony is a theoretical virtue, I find problematic certain aspects of Baker's particular analysis of what makes it a virtue, and I also doubt that Baker's account can survive confrontation with real examples that it ought to fit. Below I consider the case of astrophysical dark matter and argue that Baker's account has some undesirable consequences for that case; this may undermine Baker's account. I suggest, moreover, that in the detailed analysis of actual scientific episodes it is rarely possible to separate quantitative, qualitative and semantic simplicity in the way Baker and many others attempt.

2 Baker on Quantitative Parsimony: Beta Decay

Baker develops his account of quantitative parsimony in the context of a case from 1930s quantum physics, namely the case of radioactive Beta decay. Beta decay occurs when an atomic nucleus decays, emitting an electron.[1] For Baker's purposes the key aspect of this episode is an observed discrepancy in spin. The quantum mechanical property, *spin*, does not literally refer to the rotation of fundamental particles, but spin can be reliably measured, and it turns out that different kinds of particles have different spin numbers (but always integer multiples of the "natural unit" ½ h/2π). Spin, like mass-energy, is a conserved quantity: the total spin in a closed system should be a constant. It was therefore doubly surprising when Beta decay experiments appeared to violate both the conservation of mass-energy and the conservation of spin. The experiments in question indicated that "the total mass-energy of the system of particles before Beta decay is greater than the total mass-energy of the observed particles…following the decay, and the total spin of the particles in the system before the decay exceeds by (the unit) 1/2 the total spin of the observed particles…following the decay" (Baker 2003, 246).

In resolving this empirical discrepancy physicists had a choice between giving up the conservation principles or postulating some previously unknown mechanism to explain away the discrepancy. In 1930 Wolfgang Pauli proposed the existence of an unobserved new particle in order to resolve the discrepancy. By 1934 Enrico Fermi had developed a detailed theory of Beta decay that included a massless, chargeless *neutrino* (the "little neutral one"), with spin-1/2.

The existence of spin-1/2 neutrinos was not confirmed experimentally until 1956. Today we know that the quantum theory of angular momentum rules out spin <1/2. In the 1930s, however, many mutually incompatible but empirically equivalent hypotheses would in principle have been able to account for the available evidence. Why was the hypothesis of a single neutrino with spin-1/2 the best choice at that time, rather than a hypothesis involving *n* particles with spin-1/2*n* (2 particles with spin-1/4, 10 particles with spin-1/20, etc.)? The spectrum of rival theories that would have been available to Fermi can be summarised as follows.

$\mathbf{H_1}$ One neutrino with spin 1/2 is emitted in each Beta decay.

$\mathbf{H_2}$ Two neutrinos with spin 1/4 is emitted in each Beta decay.

$\mathbf{H_3}$ Three neutrinos with spin 1/6 is emitted in each Beta decay.

…

$\mathbf{H_n}$ *n* neutrinos with spin 1/2*n* is emitted in each Beta decay.

According to Baker,

> There is nothing to choose between the various neutrino hypotheses on grounds of syntactic elegance. Nor do the hypotheses differ in their qualitative parsimony; each postulates exactly one new *kind* of particle. Several other potentially relevant features are also on a par: for example, the predicates used in each hypothesis are equally 'natural', and no one hypothesis is logically stronger than any other. (Baker 2003, 249)

The principle of quantitative parsimony, however, does provide a plausible reason for choosing the hypothesis of a single neutrino over its rivals. So why is quantitative

[1] For a modern description of Beta decay, see http://hyperphysics.phy-astr.gsu.edu/hbase/quantum/fermi2.html#c1. In Beta decay, a proton (with quarks up, down, up) decays via the weak interaction to a neutron (with quarks up, down, down), releasing an electron plus an electron anti-neutrino.

parsimony a good principle to apply in this case? What justifies choosing theories on the basis of quantitative parsimony? Baker writes:

> The key, in my view, to explicating the role of quantitative parsimony in additive cases such as the neutrino example is to focus on the relative explanatory power of the alternative hypotheses, H_1, H_2, H_3, … (T)here are significant and relevant phenomena which are easier to explain when a more quantitatively parsimonious neutrino hypothesis is assumed. (Baker 2003, 251)

Baker argues that in general the greater explanatory power of quantitatively parsimonious hypotheses is what provides the rational justification for choosing those hypotheses over their rivals. The value of quantitative parsimony is thus derivative rather than intrinsic, but that is a feature it seems to have in common with other sorts of simplicity. (Syntactic simplicity, for example, seems to be valuable because theories that have it are easier to understand and use.)

In a significant sense, as Baker notes (2003, 251), the competing neutrino hypotheses are explanatorily equivalent: they all explain the missing 1/2 spin. But the quantitatively parsimonious hypothesis is better able to explain *other* features of our experimental knowledge. For example, there were no known cases of "missing" 1/3-, or 1/4-, or 1/100-spin: "The absence of these smaller fractional spins was a phenomenon which competing neutrino hypotheses might have helped to explain" (Baker 2003, 252). Again,

> Given that H_{10} postulates particles with spin 1/20, we might ask ourselves: why has no experimental set-up yielded a 'missing' spin-value of 1/20? My claim is that H_1 allows a better answer to this question than H_{10} does, for H_{1-} is consistent with a simple and parsimonious hypothesis which explains why no cases of missing 1/20-spin have been observed … [namely,] that there exist no particles with spin 1/20. (Baker 2003, p. 252)

Similarly, H_1 can explain, and H_{10} cannot explain, why no experiments yield missing spins of 9/20, and analogous points can be made for each H_n with $n > 1$. Note that Baker is not claiming that H_1 provides a *complete* explanation of why no missing spin values other than integer multiples of the "natural unit" ½ h/2π have been observed. Nor is he denying that H_{10} could, if combined with appropriate auxiliary hypotheses, turn out to be explanatorily better than H_1 overall (although in the latter instance the resulting theory would be syntactically less simple, too). Nor is he ruling out better explanations from some other hypothesis. But the neutrino case does seem to show that quantitative parsimony can be rationally justified as a principle of theory choice in terms of the relative explanatory power of rival hypotheses.

3 The Dark Matter Problem

With Baker's account of quantitative parsimony in mind, let me now outline a different scientific case, the astrophysical dark matter problem. Like the Beta decay case, the dark matter case involves a serious empirical discrepancy, a promising line of solution to which is to postulate the existence of previously unknown entities. In the next section I examine what theory choices Baker's account of quantitative parsimony would tell us to make in the dark matter case, and use this as a basis to critique Baker's account.

The dynamical discrepancy in astrophysics was also discovered in the 1930s, although the astronomical community did not take the problem seriously until the 1970s; despite intensive efforts since then, the discrepancy remains unresolved. There is no way to explain the internal motions of galaxies and larger dynamical systems given just the matter observed in those systems plus General Relativity, the current best theory of gravity. A complete account of the several lines of evidence that confirm this result is beyond the scope of this paper, but a brief description will give the flavor of the situation. (For surveys of the evidence and the history of the problem, see Trimble 1987; Vanderburgh 2001, 2014.)

A "rotation curve" is a plot of the rotational velocities of the bodies in a dynamical system against their radial distances from the center of rotation. In the rotation curve for the planets in our solar system, for example, the rotation velocity drops off asymptotically to zero as radius increases. This pattern is to be expected in any system whose motions are governed by an inverse square gravitational law and in which the mass of the system can be treated as concentrated at the centre. According to Newton (1999, 451), any such system must obey Kepler's third law ($k = T^2/r^3$), which says that for a given central mass the square of the period of any satellite divided by the cube of its orbital radius is a constant. Kepler's third law implies that the farther an orbiting body is from its dynamical center, the slower its orbital velocity will be. Rotation patterns like those of our solar system are therefore sometimes called "Keplerian".

The rotation curve for a spiral galaxy is obtained by plotting the spectrographic redshift of light emitted by stars and gas within that galaxy as a function of radius. The redshift gives the component of rotation along the line of sight; the rotation curve shows how rotation velocity varies with distance from the center of the galaxy. It turns out that the observed rotation curves for spiral galaxies are decidedly non-Keplerian: Even taking account of the fact that the mass is not concentrated at the center of the galaxy, a major discrepancy remains. On the initially plausible hypothesis that mass density decreases more or less linearly with increasing radius, as does the intensity of the light emitted, the rotation velocity should still decrease with increasing radius. At the very least the rotation velocity should drop off once the edge of the mass distribution is reached; it is natural to assume that this edge is near where the visible galaxy stops. Instead, the velocity stays constant or even increases at extreme radii: Gas clouds as far out as ten times the radius of the luminous disk orbit the galaxy at the same velocity as the stars in the disk.

The dynamical discrepancy in spirals is paralleled by similar discrepancies in all types of large scale astrophysical systems, including elliptical and dwarf galaxies, clusters of galaxies, superclusters of clusters, etc. These other types of systems do not have a unique plan and sense of rotation, so different measurement techniques must be employed. One is the virial theorem, from which the mass of an elliptical galaxy or the mass of a cluster of galaxies can be estimated when a "velocity dispersion" of the components of the system (stars or galaxies, respectively) can be obtained spectroscopically. Also, many galaxies and clusters emit a flux of X-ray radiation from a cloud of hot gas that enshrouds them, and on the reasonable assumption that it is the gravitational potential of the system that heats the gas, the "X-ray temperature" can be converted into an estimate of the system's overall mass. Similarly, gravitational lensing can be used to estimate the masses of galaxies and clusters whose gravitational fields deflect and distort the images of background objects. In short, multiple independent lines of evidence confirm the existence of the dynamical discrepancy in every type of system at least as large as a galaxy.

There are two classes of possible solutions to the astrophysical dynamical discrepancy. Either (1) there is *much* more mass present than we can directly detect, and its distribution is very different from that of the luminous matter in astrophysical systems, or (2) Einstein's General Theory of Relativity does not describe the gravitational interactions in question.

The most popular approach to solving the dynamical discrepancy is to postulate some new kind of matter that is 10–100 times more abundant by mass than the ordinary baryonic matter observed in these systems; it is called *dark* matter because, if it exists, it emits no detectable electromagnetic signature. A plethora of dark matter candidates have been proposed, ranging from black holes to otherwise unknown fundamental particles. So far, despite determined efforts to detect dark matter over the last 30 years, scientists have succeeded mainly in ruling out many of the candidates.

The persistent failure to detect dark matter has led several theorists to attempt to resolve the dynamical discrepancy by proposing new laws of gravity for very large scale interactions, laws which are predictively equivalent to General Relativity over solar-system scales but which make quite different predictions over galactic scales. The alternative gravitation theories proposed and the very idea of a gravity solution remain unpopular in the scientific community. Evidentially and methodologically, however, it is possible that the astrophysical dynamical discrepancy will be solved by a new relativistic theory of gravity that has General Relativity as its limit for interactions taking place over distances corresponding to roughly the size of our solar system (see Vanderburgh 2003, 2005).

4 Dark Matter and Quantitative Parsimony

Under the first class of solutions to the dynamical discrepancy there are many candidate dark matter hypotheses, each of which is consistent with the available dynamical evidence and other parts of our background knowledge. Quantitative parsimony would tell us that we should choose from within this collection of rival hypotheses the very one that minimizes the overall number of dark matter particles. This in turn implies that we should prefer a hypothesis that makes the individual dark matter particles as massive as possible, within other empirical constraints.[2]

I have three sorts of complaints about this implication: one from intuition and two from the fact that the case does not seem to fit important features of Baker's account of quantitative parsimony. On the level of intuition, it seems to me that we should not be committed to the idea that the dark matter particles are individually as massive as possible. There are some empirical upper limits on the mass of the "unit" of dark matter—studies of gravitational micro-lensing, for example, show that there are too few objects of the mass of Jupiter or greater for such objects to be the dark matter. Below these limits, however, there is no reason to prefer individual dark matter particles to be more rather than less massive.

Beyond this intuition there are other, more solid, complaints that can be raised against Baker's account. One is that, contrary to Baker's explication of the value of quantitative parsimony, there does not seem to be any explanatory benefit from preferring the most quantitatively parsimonious dark matter hypothesis. In the Beta decay case, the most quantitatively parsimonious hypothesis, in addition to explaining the missing spin, also explains why there are no observations of fractional spins such as 9/20. The most quantitatively parsimonious dark matter hypothesis explains the dynamics of galaxies and

[2] This assumes that quantitative parsimony never advises us to postulate *zero* entities of a kind that qualitative parsimony permits. If quantitative parsimony does tell us to postulate zero dark matter particles—that would be equivalent, perhaps, to a general injunction against unobservable entities—then quantitative parsimony would be telling us to prefer a gravitational solution to the astrophysical dynamical discrepancy. But then Baker's explanation of the value of quantitative parsimony in terms of explanatory power cannot work: having zero dark matter particles does not explain anything. I therefore ignore this possibility in the discussion of Baker that follows.

clusters—as do all the less quantitatively parsimonious hypotheses. But I do not see that there is anything that the most quantitatively parsimonious dark matter hypothesis explains that its rivals do not. In fact, it may be easier to explain the non-observation of dark matter if the individual dark matter units are individually *less* massive.[3] Since Baker's justification of quantitative parsimony turns on explanatory power and that explanatory power is missing from the most quantitatively parsimonious dark matter hypothesis, either Baker has not analyzed quantitative parsimony correctly or it is a principle that does not apply to the dark matter case.

Another consideration is that there should be a reasonable "creation story" for the dark matter; this is in part to say that our theory of dark matter should be consistent with our other theories. On this score the supersymmetric particles have an advantage over other potential hypotheses that have greater quantitative parsimony. Take, for example, a dark matter hypothesis that makes individual dark matter particles 100 times more massive than the most massive supersymmetric dark matter candidate. If we are unable to fit such a particle into our overall scheme of particle physics, then the fact that that theory is 100 times more quantitatively parsimonious would seem to be inconsequential—or at least, that consideration is swamped by other methodological considerations such as unification, consistency with established theory, or something else. (A related point could be made about Baker's Beta decay case, too: the H_{10} hypothesis makes no sense because we cannot fit spin-1/20 particles into our contemporary understanding of particle physics.) Baker could well respond that his account was not meant to be complete, and certainly was not meant to claim that quantitative parsimony was the only or the most important methodological consideration. I will suggest below, however, that simultaneously weighing many methodological criteria is in fact something that is *required*: the strategy of dividing the problem into small parts each of which gets treated in its own terms and without reference to other parts leads to error and confusion.

A further complication that would need to be addressed in a full account of quantitative parsimony is what specific version of quantitative parsimony is in play and how it negotiates choices between, for example, two rival dark matter theories that turn out to be such that the first postulates M entities of Type A, and the second M + N entities of Type B.[4] In this comparison, both theories satisfy qualitative parsimony with regard to the number of *types* of new matter, but differ in their quantitative parsimony in that one proposes a larger number of *individual entities*. It is not clear that the Baker's definitions of qualitative and quantitative parsimony, quoted near the beginning of this paper, provide any advice at all about making such a choice since the definition of quantitative parsimony is relativized to the specific type of entity under consideration. (Perhaps the syntactic simplicity of the relative types of entities in the competing theories would come into play.) One can imagine even more complicated cases, where a dark matter theory proposes M entities of Type A plus N entities of Type B, etc. Qualitative parsimony would say to minimize the types of entities involved, but if multiple types of entities seem to be required (as in the real dark matter case, where we have ordinary but dim matter, cosmic neutrinos adding something to

[3] The non-observation of dark matter is easier to explain with less massive dark matter units because more massive particles have a larger cross section of interaction and thus should be more easily detectable in particle detection schemes; they would be expected to have shorter decay times and hence would be more readily inferable from decay products such as radiation; if we are dealing with approximately Jupiter-mass objects, improved micro-lensing sensitivity will detect them or rule them out; and so on. I suppose we could say that hypotheses postulating more massive dark matter units are more testable or more falsifiable, but that in itself does not show that dark matter is in fact more likely to come in more massive units.

[4] Thanks to a referee for this journal for pointing this out.

galaxy cluster masses, plus the need for other exotic particles) it becomes very difficult to imagine a rule or guideline for balancing the number of types of entities against the number of entities of each type.

5 Is Dark Matter "Non-additive"?

One way to defend Baker's account from the criticisms raised above would be to show that the dark matter case does not meet Baker's restriction to "additive" cases: then what we say about dark matter would not redound to the credit or discredit of Baker's account. At the end of Sect. 1, I noted that for Baker an explanation is additive when a collection of qualitatively identical objects collectively explains a phenomenon, that is, where the explanation derives simply from totaling up the effects of the individual, qualitatively identical objects. Now, Baker in fact argues that the case of inferring a hitherto unobserved planet from unexplained perturbations on a known planet is non-additive (Baker 2003, 257–258), while Vanderburgh (2005) argues that this type of case in celestial mechanics is precisely analogous to the astrophysical dynamical discrepancy. If the celestial mechanics case really is non-additive, as Baker claims, it would seem to follow that the dark matter case is not relevant to Baker's account of quantitative parsimony. I see three sorts of possible responses to this line of defense.

First, perhaps the celestial mechanics case actually is additive, in which case Baker's account of quantitative parsimony ought to apply to it but does not. This is the least promising of the possible responses; the celestial mechanics case does seem to be non-additive since, as Baker remarks, there is no reason to expect planets to be qualitatively similar to one another, and besides, "the gravitational forces of several planets may partially or completely cancel one another out" (Baker 2003, 257). In short, there is no unique way to add up the individual contributions of unknown entities to reach an explanation of the planetary perturbation.

A second possible response is more promising. Whether or not the celestial mechanics case is non-additive, perhaps the dark matter case is additive. The celestial mechanics case is non-additive in Baker's lights because it is possible to produce the same perturbation with many different arrangements of matter (one small planet nearby, or a larger one farther away, or two planets, or a cloud of planetoids, etc.).[5] But in the dark matter case we do not need a particular direction of force, just a total amount of dynamical mass. The total dynamical mass can be distributed in any number of individual particles, so long as their masses add up to the correct overall value. If the dark matter case is in fact additive, as this suggests, then the failure to give the right advice in the dark matter case discredits Baker's account.

A third possible response, if dark matter turns out to be non-additive after all, begins from the fact that Baker's account would then not cover it. This means, I suggest, that Baker's account is fine as far as it goes, but that it does not go nearly far enough: the restriction to additive cases makes Baker's account unable to deal with important real cases in which quantitative parsimony does seem to be a relevant factor. On this point, let me mention that the Beta decay case itself may not be additive—Baker's analysis perhaps does

[5] For these purposes I skip over secondary effects that would be produced by some of these matter distributions, on the basis of which they might be distinguished empirically. For example, some of the candidate matter solutions to Mercury's anomalous perihelial precession would have produced effects on Venus's orbit that were not observed.

not even cover his own example. One reason to think the Beta decay case is non-additive is that another thing that required explanation in later developments of Beta decay theory, in addition to the missing spin, was the fact that the nuclear recoil was not in the direction opposite that of the momentum of the emitted electron. The nuclear recoil direction indicates (by the principle of action and reaction) the direction of the total momentum of the particles emitted in the Beta decay. But, as in the planetary perturbation case, there are an indefinite number of ways to add particles with various combinations of direction, mass, and velocity so as to produce the observed recoil direction. Baker needs to counterfactually restrict the discussion to spin alone in order to get his account off the ground, but he thereby makes the example unrealistic. The upshot is that there are no obvious real cases which *are* additive in Baker's sense. If this is correct, Baker's is a theory without any application.

6 Conclusion

I have argued that there are reasons to doubt the adequacy of Baker's account of quantitative simplicity. There may be some way to save Baker's account; in any case I certainly have not shown that there is no possible justification of quantitative parsimony as a principle of theory choice. I would like to end with some programmatic remarks prompted by the preceding discussion.

In many actual theory choice situations, *several kinds* of simplicity must be compared: often we cannot focus just on individual aspects or principles of simplicity. For example, one reason to reject neutrinos with spin-1/4 or 1/20 is that having a theory that includes only spin values that are integer multiples of ½ h/2π is syntactically simpler than a theory that includes both integer and non-integer multiples of the spin unit. This will be an especially powerful consideration when there are no non-integer multiples of the spin unit known; thus the principle of theoretical conservatism comes into play as well.

In fact, I think that artificially separating out the different methodological aspects of actual scientific cases, while sometimes heuristically useful, can lead to confusion and error. In comparing dark matter candidates, it can seem as if the relative syntactic simplicity or the relative quantitative parsimony of the rival matter hypotheses is all that needs to be considered. But if we step back and realize that a choice also needs to be made between a matter solution and a gravity solution to the dynamical discrepancy, we quickly see that other things need to be taken into account at the same time. Is a theory that postulates an entirely new type of matter while retaining the old law of gravity *more simple* or *less simple* than a competing theory that attempts to explain the same dynamical phenomena with only the known matter but a new theory of gravity? The choice is between theoretical groups each of which includes a gravitational law and a matter distribution.[6] Syntactic, qualitative and quantitative simplicity are all in play, not to mention other methodological considerations.

[6] The analogy between the Beta decay discrepancy and the astrophysical dynamical discrepancy is tighter than may at first appear. It would have been possible, before the experimental confirmation of the existence of neutrinos in 1956, to have explained Beta decay by saying that spin and mass-energy are *not* conserved—Niels Bohr actually proposed denying conservation of mass-energy in the quantum realm. But would such a theory be more simple or less simple than one that postulates an otherwise unknown particle to explain the same phenomenon?

If I am right about this, then although Nolan (1997) tries to pick his examples so that the competing hypotheses within each differ only with regard to quantitative parsimony, this in fact cannot be the only relevant methodological consideration. It will usually (if not always) be possible to resolve an empirical discrepancy either through alternative hypotheses that revise fundamental laws and/or background assumptions, or through hypotheses that leave those laws and assumptions untouched while postulating new entities or mechanisms. In a given evidential context there may be good reasons to retain the old laws and background assumptions and reject the revised ones, but my point is that those reasons, and the methodological principles governing them, need to be explicitly considered. The all-too-common retreat to *ceteris paribus* conditions will be inevitably uninformative—not to mention potentially misleading—about actual theory choice situations, where things are never equal.

I suggest, therefore, that Baker's analysis fails in part because it offers no way to compare the relative weights of the several methodological principles that must be balanced in actual theory choice situations. I have no specific proposal for how to do balance that either, and it would no doubt be extremely difficult to devise one,[7] but I think it is important to acknowledge the need for it. One way of doing this balancing would be to cash out the value of methodological considerations, including all kinds of simplicity, in terms of explanatory power. Such an approach is suggested by Baker's explication of quantitative parsimony, and it would perhaps be possible to carry it out, but I am not confident that it would capture philosophers' intuitions about the role and value of simplicity in theory choice. Other things besides explanatory power matter, and for things such as syntactic simplicity it is hard to see what the advantage in explanatory power might be. (The explanatory advantage cannot merely be that the theory is doing more (or the same) with less—whether or not the *less* is important is exactly what is at issue.) However, if it is not the case that everything turns on explanatory power (or some other single theoretical virtue), there may be an incommensurability problem: without something in common between different methodological criteria, it will be more difficult to figure out how to weigh them against each other in a principled way.

To conclude: Baker attempts to fill a gap in our understanding of methodological reasoning in science by giving an account of quantitative parsimony in terms of the surplus explanatory power of quantitatively parsimonious hypotheses. I have argued that Baker's account is not satisfactory. We need to be able not only to weigh many different methodological principles at once (including several kinds of simplicity), but also to be able to specify the conditions under which principles such as quantitative parsimony are to be applied. Baker's account does not say anything about the first point, and does go far enough on the second. Moreover, as suggested above, there will be some cases, such as the dark matter case, where the most quantitatively parsimonious hypothesis will *not* have an explanatory advantage over its otherwise empirically equivalent competitors. Nolan's challenge to explain the value of quantitative parsimony thus has not yet been answered.

[7] Perhaps out of recognition of the difficulty of giving an explicit account of how to balance the many competing factors in theory choice situations, Duhem (1982, 216–218) declined to do so, giving authority instead to the "good sense" of experienced scientists in deciding which of the competing hypotheses to pursue or adopt.

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