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Quantitative Parsimony

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ABSTRACT

In this paper, I motivate the view that quantitative parsimony is a theoretical virtue: that is, we should be concerned not only to minimize the number of kinds of entities postulated by our theories (i.e. maximize qualitative parsimony), but we should also minimize the number of entities postulated which fall under those kinds. In order to motivate this view, I consider two cases from the history of science: the postulation of the neutrino and the proposal of Avogadro's hypothesis. I also consider two issues concerning how a principle of quantitative parsimony should be framed.

1 Introduction

There are many types of simplicity which are held to be desirable for theories. They include, but are probably not limited to, such varieties as descriptive simplicity (how easy it is to state the theory), simplicity of hypotheses, inductive simplicity (e.g. using 'straight rules', or some such), simplicity of postulated laws, formal simplicity, and so on, and there are interesting questions about whether any of these principles reduce to any of the others. The kind of simplicity I am interested in dealing with in this paper, however, is simplicity of ontological postulates—a variety of simplicity which sometimes has the label 'parsimony' reserved for it[1]—though sometimes of course the words 'simplicity' and 'parsimony' are used interchangeably. A famous formulation of this sort of simplicity is 'Occam's Razor': Entities Should Not Be Multiplied Beyond Necessity.[2] A caveat is usually added to the effect that this is an injunction to do with the *postulation* of entities, rather than its being a suggestion that theorists are guilty of filling the world with entities through their work.

Occam's Razor has intuitive plausibility for a great many people. However, many theorists have wished to add a further qualification, to the effect that we

[1] See McLaughlin ([1982a], p. 92) for an example.
[2] Though the extent to which this razor should be attributed to Occam is unclear—see Smart [1984], or for more detail see Thorburn [1918].

are not to care about how many entities that have been postulated by our theories *per se*, but rather that we should only be concerned with not postulating any *kinds* of entities more than necessary. Parsimony with respect to kinds of entities is known as *qualitative* parsimony, whereas parsimony about the numbers of the entities themselves is known as *quantitative* parsimony.[3] For instance, David Lewis, in a well-known passage, says: 'I subscribe to the general view that qualitative parsimony is good in a philosophical or empirical hypothesis; but I recognise no presumption whatever in favour of quantitative parsimony' (Lewis [1973], p. 87). 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. As the title suggests, therefore, I will be concerned in this paper to mount a defence of quantitative parsimony as a theoretical virtue.

Before I attempt any kind of defence, however, I'll mention some things that I am *not* intending to defend. I'm certainly not claiming that quantitative parsimony is the only theoretical virtue, or the only theoretical virtue besides empirical adequacy, or even one of the most important theoretical virtues. That would be absurd. Obviously, even if it is better to be quantitatively parsimonious all other things being equal, quantitative parsimony may be (and indeed often is) outweighed by other considerations—the most parsimonious theory may not be as empirically adequate, it may be less simple in other respects than its rivals, it may be less comprehensive ... or it may be all of these together. In arguing that quantitative parsimony is of some importance, I'm not arguing necessarily that it ever outweighs other considerations. On the other hand, of course, I am not arguing that it can never outweigh other considerations either—the question of its relative weighting is something that I am simply not concerned to address here.

The second claim that I do not want to be thought to be making is that quantitative parsimony is a primitive theoretical virtue—that is, I am not claiming that it cannot be explained as flowing from other, more fundamental virtues. Some people argue that parsimony is to be justified on inductive grounds, or aesthetic grounds, or that parsimony is to be explained by some more general notion of simplicity. I do not intend anything I say to be inconsistent with the view that there can be some further explanation of the value of parsimony, nor with the opposite view. As with the issue of relative weighting, I mention this issue only to set it aside.

With the question of what I am not going to be talking about out of the way, then, let us move on to what I am intending to discuss. Ideally, I would like to be able to give you a general account of how we are to tell when a procedure is cognitively virtuous, and then provide conclusive arguments based on that to

[3] This terminology is derived from Lewis ([1973], p. 87).

justify quantitative parsimony. Unfortunately, things are much less than ideal and I can at present do no such thing. I will therefore proceed in a much more *a posteriori* and less conclusive manner: by looking at some well-known cases from the history of science, and arguing that they make the virtue of quantitative parsimony plausible.

The general strategy for using cases to argue for the plausibility of taking quantitative parsimony to be a virtue is to look at cases where some postulation of new ontology is apparently justified and where the only relevant difference between a range of rival theories about what new ontology to accept is how quantitatively parsimonious the theories are. The cases where quantitative parsimony seems to be the only relevant difference that I will be discussing are cases where historically the most quantitatively parsimonious alternative was adopted, and where it seems clearly more intuitive and attractive than its less parsimonious rivals. It turns out that the cases I discuss also cast some light on some issues connected with formulating a principle of quantitative parsimony. Once I have examined the cases, then, I will conclude with a discussion of some of these issues connected to formulating a principle of quantitative parsimony.

2 The cases

There are many examples in the history of science where considerations of quantitative parsimony seem to have been relevant, at least implicitly—many times when scientists, faced with a range of alternative explanations which had as the only salient difference between them how quantitatively extravagant they were, unhesitatingly plumped for the most quantitatively parsimonious. This evidence can only be of limited value—after all, arguing from the practice of scientists to what norms they should follow (or what the cognitive virtues are) is by no means straightforward. Nevertheless, the two cases I will discuss will, I hope, provide support for quantitative parsimony in several ways. The first is that an examination of the methods in fact employed by successful scientists is one way to discover which methods are worth employing (we are given prima facie evidence that such methods *work*). The second is that these cases serve as intuition pumps—once the cases are described and the use of quantitative parsimony pointed out, it should become intuitively obvious (just as, it seems to me, it was intuitively obvious to the scientific communities of the day) that it would have been wrong to accept one of the less quantitatively parsimonious rivals, and even perhaps (though this is presumably more controversial) that the scientists involved would not even have been justified in being agnostic about which of the theories differing only in point of quantitative extravagance to accept, given that they had made up their minds that one of the theories that differed only in this respect were true. Finally, the third method by which such cases can make quantitative parsimony more plausible is through providing a meta-induction—the fact that employing quantitative parsimony has led to more accurate theories in the past is evidence that it will in the future also. This consideration is connected to the point about examining scientists' practices—though they may come apart in various ways. I do not want to make too much of this meta-induction, however, since a few cases of simple enumeration is hardly a strong inductive base, and also because it raises issues like the status of induction and the role which induction ought to play in supporting other theoretical virtues. Let us then get down to cases. The two I will be discussing will be (1) the postulation of the neutrino by Wolfgang Pauli and Enrico Fermi, and (2) some of the early developments in atomic chemistry.

The case of the neutrino is the simpler of the two, so I will discuss it first.[4] In the 1920s and 1930s, physicists were puzzled by an aspect of Beta decay—the emission of electrons (these electrons being known as Beta particles) from the nuclei of some radioactive atoms. The problem was that the drop in energy of nuclei during Beta decay was more than the combined mass-energy and kinetic energy of the emitted electron. Mass-energy seemed to have been annihilated.[5] Niels Bohr considered that this should be dealt with by denying the conservation of mass-energy in atomic reactions, but there was understandably reluctance to jettison such a central conservation law. Pauli, on the other hand, suggested that a new, neutrally charged, never before detected particle was emitted along with the electron in Beta decay, and the sum of its mass-energy and kinetic energy was to make up the missing energy.[6] Fermi refined the theory substantially (and is credited with coining the name 'neutrino') and provided a plausible account of the generation of neutrinos, and published his results in Italian and German in 1934 (Fermi [1968] is an English translation of this seminal paper). As well as explaining the missing mass-energy, the neutrino theory was also able to explain why emitted electrons were observed with many different energy levels—the slower electrons could be presumed to be produced in reactions that produced faster neutrinos, and vice versa. Yet another advantage of the postulation of the neutrino was that it could be used to account for a missing 1/2 spin in Beta decay which violated conservation of spin laws. The theory was useful and helpful, and that neutrinos are given off as part of the process of Beta decay has been more than adequately confirmed by subsequent observations and experiments (though the particle emitted with the electron in the process of Beta-decay

[4] My account of the postulation of the neutrino may sound simplistic (and it has been simplified a little), and so one may think that it merits Lakatos' charge that 'Most conventional accounts [of the discovery of the neutrino] are confused ... and prefer to falsify history' (Lakatos [1970]). The complications discussed by Lakatos seem to be irrelevant to discussion of the aspect of neutrino postulation that I am interested in here, however.

[5] Sutton ([1992], pp. 17–18). Sutton's book contains an interesting and lively account of the development of the theory of neutrinos from its beginning to the present day.

[6] A translation of the letter in which Pauli suggested this can be found in Brown ([1985], p. 344).

is now called an anti-neutrino, and it is considered an open scientific question whether anti-neutrinos are identical to neutrinos. This has more to do with the developments of the meaning of the word 'neutrino' than any disconfirmation of Fermi's general idea, however).

Let us suppose that postulating an at-the-time unobserved particle was justified in the explanation of Beta-particle emission. If this is so, then there are a plethora of very similar neutrino theories which would also explain the 'missing energy', the variation of the energies of the emitted electrons and the missing spin. A theory which stated that there were two 'neutrinos' emitted every time Beta-decay occurred would also explain the missing mass-energy—each neutrino's mass-energy could amount to half the missing amount. A theory which postulated three 'neutrinos' would also work, as would a theory postulating any finite number of neutrinos. The missing spin would also have to be divided among the postulated neutrinos, and while this would result in the postulated neutrino's having spin values which we now believe nothing possesses, there was no reason to think nothing could have those fractional spin values at the time.[7] Pauli and Fermi did not consider in their papers outlining the new theory the possibility that there may be more than one tiny neutral particle produced in each case of Beta decay. I predict, however, that if someone had seriously suggested a theory which held that there were, say, exactly seventeen million 'neutrinos' produced whenever there was Beta decay (or even suggested that there were only two, without further reason), their theory would have received short shrift from the two scientists (and presumably because it would have been needlessly extravagant). I take the fact that they saw fit only to postulate one additional particle in each Beta-decay, and either did not consider postulating more or at least did not give it serious consideration,[8] as evidence that there was some reason for their quantitatively sparse postulation—and since the 'one neutrino' explanation and the 'seventeen million neutrino' explanations would have appeared at the time to be equally virtuous in other respects (they explain the same phenomena

[7] Even if there had been, a theory of multiple particles could be saved if the spin values of all but one of them cancelled out. A theory on which two 'neutrinos' had a spin of +1/2 and one had spin of −1/2, and each had one-third of the missing mass-energy would save the phenomena adequately. One problem with such rival theories is that it might be thought that they are less qualitatively parsimonious because they postulate 'neutrinos' with more than one spin-value, and so might be taken to be postulating more than one sort of entity. In any case, the hypothetical example of scientists who were in Pauli and Fermi's position with regard to needing to explain missing mass-energy and the range of emission values but who were unaware of any issues relating to spin would do to make the point that a 'neutrino theory' could face a range of rivals which seem obviously inferior but which differ relevantly from the preferred theory only in their quantitative extravagance.

[8] As far as I can discover, Fermi never discussed the possibility that there might be more than one, and while Pauli did make a passing reference to the possibility that there might be more than one (see the extract of the translation of Pauli's 1933 conference discussion on p. 345 of Brown [1985]), he usually talks as if there is only one neutrino.

equally comprehensively, there were no known further experiments to test one hypothesis but not the other, and so on).

An objection might be raised to the reasoning used to draw my conclusion (besides the points I have already conceded about this example's being of only limited value in showing that quantitative parsimony is to be valued). It is that those who would postulate two particles, or three, or indeed seventeen million, would be postulating a different kind of particle from those that postulate only one such particle, and so there is a difference in the kinds of things being postulated by the rival theories, and so the question is one of qualitative parsimony (parsimony in respect of what kinds of entities are postulated, rather than the quantity of any members of a postulated kind). This objection would be misguided. Even if it was conceded that the postulator of two particles in each reaction is postulating entities of a different kind than the postulator of only one, neither is postulating more kinds of things than the other. Pauli and Fermi have one kind—their neutrino—but the 'two-particle' theorist has only one new kind of entity as well—the kind of entity which the two-particle theorist may call a 'neutrino', but let us call that kind of object a neutrino*, to distinguish it. Now it is true that Pauli and Fermi were not committed to neutrinos* (assuming for the sake of argument that neutrinos* would be a different kind of thing from neutrinos), but the situation is symmetrical—the two-particle theorist is not committed to Pauli and Fermi's neutrinos, but only to neutrinos*. What this does serve to point out is that the neutrino case may not be thought to be a case where two theories agree on the kinds that there are and disagree about the numbers of things in one or more of those kinds—quantitative parsimony is rather relevant because each theory is, it seems, equally cognitively virtuous in most respects, and each of the rival theories is committed only to one extra kind of thing, and each differs on the question of how many of that thing there are. So even if each of the rival theories is committed to a different sort of thing, they are still on a par in terms of cognitive virtue (including qualitative parsimony, since each is committed to only one extra kind of thing), except for the question of quantity of the new things postulated.

It seems intuitive to me, and I hope to the reader, that postulating seventeen million neutrinos in each case of Beta decay produces a theory that is less plausible and attractive than the theory proposed by Pauli and Fermi. This example also shows, I think, that Pauli and Fermi at least implicitly subscribed to a principle of quantitative parsimony when they were faced with the need to postulate new entities. One thing that makes this especially plausible is that there seems to have been no temptation to being officially agnostic as to the number of small neutral particles produced by Beta decay. Even if quantitative parsimony is no cognitive virtue, then it could still be argued that Pauli and Fermi made no mistake in choosing one particle over two, or three, or seventeen million. After all, one number is as good as any other if there is no question of being quantitatively parsimonious, so their choice was still as good as the choice of any other specific number of particles. But surely, if qualitative parsimony is not a virtue, they did make a mistake to decide on a specific number rather than suspend judgement, or be agnostic about exactly how many 'neutrinos' there were. For presumably one should suspend judgement when faced with alternative explanations that are equally cognitively virtuous, especially when the theories do not deliver differing recommendations for action.[9] By not being agnostic, they seemed disinclined to consider the more quantitatively extravagant rival theories even as being on a par with their own. They employed (if only implicitly) a principle of quantitative parsimony, and it seems counterintuitive to suppose that they were at fault for doing so.[10]

Another example of a theory whose only advantage over similar but unintuitive theories was its quantitative parsimony was Avogadro's hypothesis, developed in the early nineteenth century to help explain the behaviour of gases[11] in chemical reactions.[12] In order to explain Avogadro's hypothesis, it will be important first to mention three (at the time controversial) theses which Avogadro accepted. The first was that explanations of the reactions of gases should be atomistic—that is, it should be supposed that there were tiny particles which made up gases, and it was the joining or separating of these particles that the chemist was studying when the chemist studied chemical reactions. Some chemists of the day resisted this picture, and preferred to attempt to discover only laws framed in terms of the ratios of gases which reacted to produce other gases (whether these ratios were of weight or volume), rather than engage in speculation about the ultimate makeup of these gases

[9] The 'how many neutrinos' case is unlike a case where vital practical decisions need to be made. It is different, for example, from a 'subjectivist Buridan's ass' case like the following: suppose the ass stands in front of two bales of hay, is completely rational, and has two theories, A and B. According to A, the ass should eat first from the bale on the left; according to B, the ass should eat first from the bale on the right. Let us suppose that A and B are equally subjectively probable for the ass, and furthermore are in all other ways equally cognitively virtuous (let us suppose the ass has been warned about the relative quality of the bales by two informants, each of whom is equally trustworthy, and each contradicted the other about which was the good bale to eat). If the ass does not choose either A or B, (and let us suppose the prospect that each bale is equally good has been well and truly ruled out), the ass will not eat at all, which may be a worse outcome than eating the wrong bale first. In such a case, it seems clear that the ass should decide between A and B rather than suspend judgement.

[10] Of course, the postulation of the neutrino was not exactly correct: current physics tells us that it is an intermediary particle which is emitted from the nucleus initially, and it is it which decays into a Beta particle and an antineutrino (though calling the small neutral particle emitted in this process an antineutrino is just as much a change in nomenclature as a change in theory).

[11] In fact, the hypothesis was of much more general significance, but as it was originally developed to explain the actions of gases, I will only be discussing this.

[12] Useful accounts of Avogadro's 1811 paper, 'Essai d'une manière de déterminer les masses relatives des molecules élémentaires des corps et les proportions selon lesquelles elles entrent dans ces combinaisons' (originally published in the *Journal de Physique*, **lxxiii**, pp. 58–76) can be found in Partington [1964] and Idhe ([1964], pp. 120–2).

(Leopold Gmelin is a good example).[13] The second thesis accepted by Avogadro was the Gay–Laussac law of combining volumes, which stated that volumes of gases at equivalent temperatures and pressures combined in fixed ratios, and furthermore that these ratios were in low whole numbers (so that e.g. one volume of oxygen combined with two of hydrogen to produce water, one of nitrogen combines with three of hydrogen to produce ammonia, and so on). This law was also controversial, as it had some experimental evidence against it (though this evidence later turned out to be incorrect). Nevertheless, for convenience let us suppose that both assumptions were theoretically justified at the time (I will later explain why this assumption is strictly unnecessary). Finally, Avogadro assumed that a given volume at a given temperature and pressure of gas would contain the same number of molecules: apparently because he thought that this was the best way to reconcile the Daltonian picture to the law of combining volumes.[14] Let us suppose that he was justified in assuming this also. Given these theses, Avogadro thought it reasonable to suppose that, for example, since two volumes of hydrogen combine with one volume of oxygen to produce water, there is twice as much hydrogen as oxygen in water. Furthermore, if one volume of oxygen was reacted with two of hydrogen, the natural thing to expect would be that one volume of water be produced (since there are twice as many hydrogen as oxygen in water, there would be as many water molecules as there were oxygen, and half as many as there were hydrogen). However, the experimental result was different: combining two volumes of hydrogen and one of oxygen produced two volumes of steam. Similarly with ammonia. Since three volumes of hydrogen were needed to react with all the nitrogen in one volume of nitrogen, we would think that ammonia was made of one molecule of nitrogen and three of hydrogen. But the reaction of three volumes of hydrogen and one of nitrogen produced two volumes of ammonia, not one as one would expect (for one would expect that there be only as many ammonia molecules as nitrogen molecules).

To solve this problem, Avogadro proposed that a distinction be drawn between *molecules élémentaires* and *molecules intégrantes*, a distinction which basically coincides with the contemporary one between 'atoms' and 'molecules' respectively. To explain the fact that there were twice as many water molecules as oxygen molecules, he proposed that we suppose that a molecule of oxygen be made up of two 'half molecules' of oxygen—that is, that each molecule of oxygen is composed of two oxygen atoms, and that each

[13] See Ihde ([1964], p. 153).
[14] See the discussion in Morselli ([1984], p. 89). While the theory would be more complicated if this was not assumed, it does not seem that this assumption is strictly necessary to reconcile Dalton's atomism and the Gay–Laussac law, provided that the number of molecules in a given volume of gas at a given temperature and pressure was a low whole number multiple (or inverse multiple) of the number of molecules of any other gas at that volume, temperature, and pressure.

molecule of steam be composed of two hydrogen and one oxygen atom. Similarly, to solve the problem of their being twice as many ammonia molecules as nitrogen molecules, he proposed that each nitrogen molecule is composed of two nitrogen atoms, whereas ammonia only contains one. Avogadro's hypothesis, that molecules of elements might contain more than one atom of those elements (and the specific claims that molecules such as oxygen, nitrogen, and hydrogen all contain two atoms) was almost ignored at the time, and even when it was resuscitated by Ampère and his followers it took a long time to become orthodoxy. Nevertheless, it became orthodoxy, and is still believed today.

Avogadro assumed the minimum number of atoms in each element to explain the new volumes discovered, but many other ratios of atoms to elements were consistent with the evidence. For instance, if instead of assuming that there were two atoms in each hydrogen molecule, oxygen molecule and nitrogen molecule, if one were to assume that there were four, six, eight, or indeed any even number the same phenomena could be explained. If the formula for hydrogen was H_{22}, for example, and the formula for nitrogen was N_{48}, then ammonia would be $N_{24}H_{33}$, and the experimental evidence possessed by chemists of the early nineteenth century need not have been any different (assuming that the atoms were each smaller than the atoms of these elements actually are). If instead of assuming the minimum number of atoms per molecule needed Avogadro had chosen some other arbitrary number (that was a multiple of the minimum needed), his theory would have explained the same reactions, would not have any additional kinds of entities, and would differ from his actual theory only in being more quantitatively extravagant. I find it intuitive that in selecting the minimum number of atoms per molecule needed Avogadro was proposing a theory simpler than any of the 'multiple rivals' are, and that if he had, in fact, said that elemental gases all contained eight thousand atoms of the element, or sixteen million, he would have been producing an arbitrary and bad theory. (This is not to say that such a theory would always be bad or unjustified. In fact historically Ampere proposed that these molecules contained four atoms of the relevant element—he was motivated by some 'geometrical considerations'[15] which later chemists ignored or discarded as mistaken. My point is not that there could not have been eight thousand hydrogen atoms in each hydrogen molecule,[16] but rather that in the absence of further considerations it would have been wrong to believe this, or to accept this theory over the 'simple', actual rival. Of course, if there were

[15] Partington ([1964], p. 117).

[16] Or more precisely, for those that hold that the number of hydrogen atoms per molecule is a matter of 'natural necessity', I am not claiming that it was impossible that a person in the same epistemic state as Avogadro and Ampere could not have had the molecule they referred to as 'hydrogen' consist of eight thousand elemental atoms.

further considerations available (such as Ampère believed himself to have), that would have been a different matter.

I said earlier that I would assume that Avogadro had been justified in accepting his starting hypotheses (an atomistic theory of gases, the Gay–Laussac law, and equal number of molecules at a given temperature and pressure), but that this assumption was not strictly necessary to make my point. This is because Avogadro's hypothesis and the other 'multiple rivals' rely on his starting hypotheses for their justification to the same extent, and so their virtue and plausibility are affected by the acceptability of the starting hypotheses to the same extent. My point only requires that Avogadro's actual hypothesis is more virtuous and attractive than the 'multiple rivals', rather than relying on any of them being absolutely virtuous or attractive enough to merit acceptance. Personally I am inclined to think that Avogadro was justified in at least provisionally accepting his view, despite the contested nature of his starting assumptions. In any case, Avogadro's hypothesis turned out to be correct in the long run, and so was a successful piece of science in at least one important sense.

So it seems that, as a matter of practice, theorists (at least implicitly) take quantitative parsimony into account when constructing theories, even when the parsimony is only a matter of halving the number of entities or dividing the number by ten (since even the postulation of ten neutrinos per reaction, or ten atoms per molecule was not countenanced). Of course, even if it were true that scientists actually employ a principle of quantitative parsimony in some areas, it would still be a further step to the claim that employing quantitative parsimony was genuinely virtuous. Successful use of quantitative parsimony provides at least some reason to think such a step is justified, in the obvious ways I have mentioned. Suppose, then, that some principle of quantitative parsimony should be adopted. What should we take such a principle to be?

3 Towards formulating a principle of quantitative parsimony

The two cases I have discussed may be able to shed some light on the question of how exactly we are to formulate a satisfactory principle of quantitative parsimony. The first thing which they show us, I will argue, is that an initially tempting formulation (and justification) of a principle of quantitative parsimony will not do to capture its intuitive conception.

There is a plausible principle of parsimony which states that one should not admit any entities into one's theory that lack explanatory power.[17] I, for one,

[17] This principle may already need some modification, for it does not allow that one might have entities in one's theory not only to provide explanatory power but that there may be entities present which are to be explained but do not necessarily explain anything in turn. These entities may be the stimulus for the whole explanatory enterprise in the first place, after all. Nevertheless, for many domains of postulated entities, the principle stated appears plausible.

think that something like this principle (with perhaps some modifications and exceptions) is correct and almost self-evident—provided that 'explanation' is not construed too narrowly (i.e. if one thought that explanation was a causal matter it would become much less self-evident that all entities had to be 'explanatory'). Some might think that an adequate principle of quantitative parsimony is a consequence of this more general principle (something like 'do not admit any more entities of the same kind as entities already postulated beyond those that have explanatory power').[18] However, the previously mentioned examples show that this formulation is not strong enough. Consider, for example, the 'seventeen million neutrino' theory. In this theory, each of the seventeen million neutrinos makes some contribution to the explanation—each is required for the total mass-energy to be conserved, in the sense that if only sixteen million, nine hundred and ninety-nine thousand nine hundred and ninety nine were given off each with the predicted mass-energy, then there would still be a small amount of mass-energy still to be accounted for. Similarly, if it were thought that the number of atoms of nitrogen in each nitrogen molecule was twenty-four, then all twenty-four would be needed to explain the formation of ammonia. Suppose, for example, that we took it that the number of hydrogen atoms per molecule of hydrogen was only two. Then our formula for ammonia would be $N_{12}H_3$. Our explanation of the fact that one molecule of nitrogen plus three molecules of hydrogen produces two molecules of ammonia would involve all twenty-four atoms in each nitrogen molecule—none of them would be explanatorily inert. Contrast this with cases where it is obvious that an object is explanatorily inert in a given explanation—for instance, if it was assumed that nitrogen molecules were surrounded by luminiferous ether this ether would not have to be part of the explanation of the formation of ammonia—and it seems that the seventeen million neutrinos or the twenty-four nitrogen atoms are not ruled out by the injunction cast in terms of explanatorialness any more than a single neutrino or pair of nitrogen atoms would be. Of course, it might be possible to define 'explanatory power' in such a way that only the most quantitatively parsimonious postulates were allowed into 'explanation', so defined, but I take it that this would be a verbal victory achieved only by producing an *ad hoc* redefinition of 'explanatory power'. Better, rather, to have quantitative parsimony expressed as a different principle to the independently plausible principle about 'explanatory parsimony', rather than tying them together in this way. This separation of these two principles is the first thing we can learn about the nature of quantitative parsimony from these examples.

The second issue that I wish to deal with is perhaps even more important

18 As an example, Sober ([1981], p. 145) describes the injunction not to postulate entities beyond those that have explanatory power as *the* principle of parsimony (my emphasis).

from the point of view of a defence of quantitative parsimony, for I must argue against a sort of formalization of quantitative parsimony which would to a large extent trivialise it. It might be claimed that it is almost never a significant cost, for the following sort of reason: mathematics is committed to an incredibly large number of entities. ZF, for example, is committed to well more than continuum-many pure sets, and if sets with inaccessible cardinality are admitted, then the number of sets committed to becomes staggering. If one accepts some sort of Platonism (i.e. that these sets apparently referred to in fact all exist), then one is committed to more entities than one will be committed to in virtue of any other part of one's theory. If Platonism about mathematical entities should be part of one's best theory (a big if, but one that many people accept), then whatever one is committed to in other parts of one's theory will be practically irrelevant to the question of how quantitatively extravagant one's overall theory is (so this argument goes): after all, no matter how many entities we may be tempted to postulate, chances are that our Platonistic set and number theories will have postulated many, many more.

What this shows us (were we not already aware of it) is that it is not merely the total number of entities postulated by a theory that is important—it is something more fine-grained. Not just total numbers of things, but how many things *of each type* there are is relevant.[19] So, for example, a mathematical Platonist who also believes that one neutrino is given off in each case of Beta decay is more quantitatively parsimonious than one who believes in all of the mathematical entities plus seventeen million little neutral particles being produced in every case of Beta decay. Or at least it seems clear to me that this is what it shows us. How to argue against someone who denies that it shows us this is a difficult question, however (especially in the absence of a general account of theoretical virtues). And it is a challenge that does not arise just for the mathematical Platonist. It faces anyone who wishes to countenance an infinite ontology—those that think space or time is continuous, those that believe the universe is infinite, Platonists about some varieties of *abstracta* besides mathematical *abstracta*, and so on.[20] One obvious argument is just the *modus tollens*: if it is only overall quantity of entities that counts, and not how many entities there are of each type, then a principle of quantitative parsimony

[19] An example of someone who seems to assume that it is only the total number of entities postulated which matters is to be found in Oliver ([1996], p. 7).

[20] Even those whose ontology is only finite may find an insistence on counting only the total number of entities worrying. While increasing the total number of entities in one area of their theory will be reflected in the overall number of entities in these ontologies, the increase may not be as significant as we would intuitively think it should be. This is because, intuitively, small changes in the number of entities matter more when the total number of entities being considered is smaller. An additional thirty entities is not very significant when we are already committed to billions of entities (or more), but the difference between, say, one and thirty-one is significant indeed.

is of very little use. But (as I have argued) the principle of quantitative parsimony is of use—so something more than the total number of entities postulated must be important in deciding the quantitative parsimony of theories. (This is just the objection of the previous paragraph run in the other direction.)

There may be other considerations which help motivate the intuition that quantitative parsimony is something that should be conserved for each kind of entity, and not merely for the total number of entities. One is that it seems possible to evaluate and compare theories dealing with a certain subject matter for their quantitative extravagance without having to worry very much about theories about quite different subject matters. Avogadro's hypothesis was (intuitively) quantitatively simpler than the possible rivals I suggested—and this did not seem to depend on one's view of set theory, or the structure of space-time, or whether the universe is finite in time. Intuitively, the strengths and weaknesses of theories about one subject matter (such as the question of the existence of neutrinos or elemental atoms) has little to do with theories about some other subject matters. Thoroughgoing holists would reject this of course—but a holism that does not even allow that relevance of one subject matter to another is at least a matter of degree seems to me to be an absurd position. Such a holist, it seems to me, would have great difficulties accounting for such quotidian facts as the fact that one can have a good grasp of one area without being an expert on every area. If facts about penguins were as relevant as the facts about teeth to the issue of what dental procedures should be employed, it would be hard to understand why dentists are not expert zoologists, and vice versa. This is not to say that a holism needs to be this extreme to object to the argument that the quantitative parsimony of a theory about one subject matter (such as theories of Beta decay) should not depend on the state of theories about quite different subject matters—theories about pure sets or the continuity of space, for example. Nevertheless, I think the argument that theories about the structure of space or set-theory are not terribly relevant to, for instance, explanations of Beta-decay, has some intuitive force, even if holism of some sort is admitted (provided it is not of the extreme sort that has no means of assessing or allowing for higher and lower degrees of relevance of theories of one subject matter to theories of another).

4 Conclusion

The thesis that we should minimize the number of entities of each kind that we postulate has intuitive plausibility. When we think that the 'seventeen million neutrino' theory or the '$N_{24}H_{33}$' theory are not simple, and absurd to suppose without some reason to rule out the 'one neutrino' or 'NH_{30}' theories, we are appealing to an at least tacitly held principle of quantitative parsimony. Furthermore, such appeals have helped physicists and chemists move closer to the truth at important junctures in the development of their theories. I find these cases reasonably convincing in support of the view that quantitative parsimony is sometimes a consideration in theory formation, and that in general one ought to be more quantitatively parsimonious when all other things are equal. Challenges of course remain: for instance, we may want further convincing that this principle can be extended to other areas (the life sciences or the social sciences, for example, and one of my particular interests is in extending it to evaluation of metaphysical theories). We could do this simply by enumerating cases from different disciplines that are as intuitive as the cases I have described—I suspect this will be possible, but I have not attempted to do so. Or a better method would be 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. This is beyond me at the moment (and I know of no successful attempt to do this by anyone else), but hopefully the recognition that there is such a virtue as quantitative parsimony is a first step in that direction.

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