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Reply to Vann McGee's 'Whittle's Assault on Cantor's Paradise'

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I would like to start by thanking Professor McGee for his interesting and thoughtful response to my paper, 'On Infinite Size' (OIS). Naturally, however, I would also like to say a few things in reply.

In OIS I offer two arguments each aimed at establishing that we are not justified in believing that there are infinite sets of different sizes: an initial argument in terms of Russell's paradox in §1, and a more direct argument in §2. I will refer to these as the 'initial' and the 'direct' argument, respectively. I will start with a discussion of the direct argument, and then move on to the initial one.

The direct argument aims to establish that we are not justified in believing Cantor's account of infinite size, by which I mean the following pair of claims.

(C1) For any infinite sets A and B, A is the same size as B iff there is a one-to-one correspondence from A to B.

(C2) For any infinite sets A and B, A is at least as large as B iff there is a one-to-one function from B to A.

Our only justification for believing that there are infinite sets of different sizes would seem to be based on this account. Thus, if we are not justified in believing this account, then we are not justified in believing that there are infinite sets of different sizes. I will describe the version of the direct argument aimed at establishing that we are not justified in believing (C1), but a similar argument could be given for (C2).

I began by arguing that (C1) is not an analysis of the same-size relation. That is, it does not tell us *what it is* for infinite sets to be of the same size. Why? Because the size of a set—whether infinite or finite—is an intrinsic property of that set. It is a property the set has purely in virtue of what it is like; specifically, in virtue of which members it has. Thus, what it is for A and B to be the same size is for them to share a certain sort of intrinsic property. It is *not* for there to exist a certain sort of function between the sets (in typical cases, neither A nor B will contain a function between the sets).

This point can of course be made without using the word ‘intrinsic’. The essential point is simply that the same-size relation is one that holds between a pair of sets purely in virtue of which members they have; it does not hold in virtue of the existence of a function between the sets (except in the unusual case where the sets happen to contain such a function). For example, the same-size relation holds between {0, 1} and {2, 3} purely in virtue of which members these sets have; it does not hold between them in virtue of the existence of a function between them. Similarly, this relation holds between ω and ω+1 (assuming that it does so hold) purely in virtue of which members these sets have, not in virtue of the existence of a function between them. Thus, (C1) of Cantor’s account is not an analysis of the same-size relation.

This means that our reason for believing (C1) cannot come simply from reflecting on what it is for two sets to be of the same size. Why then is (C1) widely believed? In particular, why is the size-to-function direction of (C1) widely believed? (By the size-function direction of (C1) I mean the claim that for any infinite sets A and B, if they are the same size, then there is a one-to-one correspondence from A to B.) This direction is the crucial one when it comes to establishing that there are infinite sets of different sizes. But arguments for this direction seem rather thin on the ground. (There are arguments for the function-to-size direction of (C1): see §2 of OIS. And these seem sufficient to justify our belief in this direction of (C1). It is only our justification for the size-to-function claims that I aim to challenge in OIS.)

The version of (C1) for finite sets is true. However, many claims true of finite sets fail for infinite ones (e.g., the claim that a set is always larger than its proper subsets), and so this does not seem sufficient to justify belief in the size-to-function direction of (C1). But perhaps whatever justifies our belief in the finite version of this claim will extend to the infinite one? Our belief in the finite version seems justified by the fact that given two finite sets of the same size, if one successively chooses members of the two sets, then the result will be a one-to-one correspondence (for if this process does not result in a one-to-one correspondence, i.e., if one runs out of members of one of the sets before that happens, then this would show that there is a one-to-one correspondence between the 'exhausted' set and a proper subset of the other one; which would in turn show that the exhausted set is smaller). However, this justification will not extend to the infinite version of the claim: since given two infinite sets of the same size (e.g., two copies of the natural numbers), successively choosing members may *not* result in a one-to-one correspondence (e.g., if one chooses members of one of the copies in the 'wrong' order).

But why then is the infinite version of the claim widely believed? As I said, arguments seem rather thin on the ground, but my best guess is that this claim is widely believed because of something like the following line of thought: if there does *not* exist a one-to-one correspondence from some infinite set A to another B, then the only possible reason is that A and B are different sizes. This would seem to give us: if there is no one-to-one correspondence from A to B, then A and B are not the same size; which is the contrapositive of the size-to-function direction of (C1). If I am correct about this, then our justification for believing the size-to-function direction of (C1) is an inference to the best explanation: it is the thought that if there is no such function, then the only possible explanation—and hence the best!—is that the sets are of different sizes.

I argue, however, that this inference to the best explanation fails: because in the paradigm cases of infinite sets such that there is no one-to-one correspondence between them, i.e., sets and their powersets, there is a better explanation of why there is no such function. This superior explanation is very straightforward (and closely related to the proof of Cantor's theorem): it starts simply from the principle that for any function f from a set D to its powerset P(D), there will be a subset of D containing precisely those $d \in D$ such that $d \notin f(d)$. This explanation of why there is no one-to-one correspondence from D to P(D) is different from that in terms of the sizes of D and P(D): the principle about functions and subsets of D just stated is clearly not in and of itself a principle about the sizes of D and P(D). Further, it does not seem that this principle must itself be explained in terms of the sizes of D and P(D)—any more than the following principle must be explained in terms of the sizes of D and P(D): for any subset E of D, there is a subset G of D containing precisely those members of D not in E. In each case, the principle is explained simply by the fact that for any property, there is a subset of D containing precisely the members of D with that property; the principle is *not* explained by the sizes of D, P(D) or any other sets. Finally, this explanation seems clearly superior to that in terms of size: it is more economical, since it uses only principles that we are committed to anyway. Thus, it is not in fact the case that if there is no one-to-one correspondence between two sets, then the explanation must be that they are of different sizes. This means that the thought that seemed to justify our belief in the size-to-function direction of (C1) is mistaken, and we appear to be left without any justification for this belief.

There are two points that McGee makes that might be thought to bear on this argument (although it is only the second point that is *clearly* a criticism of it). The first point is that although I talk in OIS of 'the size' of a set, there are in fact multiple notions of size applicable to sets—just as there are multiple notions of size applicable to physical bodies (e.g., volume, length and mass).[1] Thus, consider the following two sets of points in $\mathbb{R}^3$: S1, the set of points less than one unit from the origin; and S2, the set of points less than two units from the origin. According to one perfectly legitimate notion of size (i.e., volume), S1 is smaller than S2. But there is a one-to-one correspondence from S1 to S2: and so according to another notion, they are the same size. It is thus an oversimplification to talk of *the* size of a set without making clear which notion one means. However, I trust that it was clear that when I spoke of the size of a set in OIS I meant the notion that one has in mind when one asks 'How many members does it have?', rather than the notion one has in mind when one asks 'How much space does it occupy?', for example; and this is what I will continue to mean by the size of a set here.

However, this might prompt a related worry, as follows. How can we be sure, even once we restrict attention to notions of size relevant to 'How many?' questions, that there is a single determinate such notion, especially when it comes to infinite sets? But if there is *not* a single determinate such notion, then might we not be justified in believing Cantor's account on at least one 'sharpening' of our indeterminate notion (i.e., on at least one determinate notion that 'sharpens' our indeterminate one)? However, while I don't think we can be *sure* that we have a single determinate such notion of size, I think that this worry can be straightforwardly answered. For it would appear that all of the claims made in these arguments would hold for any sharpening of the relevant notion. For

[1] See McGee [forthcoming: 10–11].

example, it seems that on any such sharpening, the size of a set must be an intrinsic property of it; and similarly, it would appear, for all the other claims made in these arguments. But then these arguments would seem to establish that we are not justified in believing the size-to-function direction of (C1) under *any* sharpening of the relevant notion.

The second point that McGee makes—which I take it is intended as a criticism of the above direct argument—is as follows.

> Notions of size are, it seems to me, tied inextricably to size comparisons. To ask meaningfully about the size of a thing, whether it's a body or a set or some other sort of thing, we have to have an understanding of when something else would be the same size as it, and when something else would be larger or smaller. The idea that size is an intrinsic property of a thing and that we can inquire about the size of a thing just by looking at the thing itself, in isolation from everything else, baffles me. (*Op. cit.*, p. 11.)

As I have indicated above, it seems clear to me that the size of a set is an intrinsic property of it. Here McGee seems to object to this claim on the basis that it entails that 'we can inquire about the size of a thing [in this case a set] just by looking at the thing itself, in isolation from everything else'. However, the metaphysical claim that the size of a set is an intrinsic property of it certainly does not entail the epistemic claim that we can know which size a set has without considering its relations to objects other than its members (or any similar epistemic claim). Thus, it is plausible that having a hairline fracture is an intrinsic property of a foot. But this certainly does not entail that we can know whether a foot has such a fracture without considering its relation to distinct objects, such as MRI machines. In just the same way, the claim that the size of a set is an intrinsic property of it does not entail that we can know which size a set has without considering its relations to objects other than its members. Nor does it entail that we can give an adequate mathematical theory of the sizes of sets without giving a criterion for when two sets are the same size, for example, that mentions objects other than the members of the sets. However, if this is really to be a theory of the *sizes* of sets, then this criterion must track the relevant intrinsic properties (i.e., it must obtain iff the sets in question share a size property). And we are justified in believing on the basis of this criterion that there are infinite sets of different sizes only if we are justified in believing that it does so track the relevant intrinsic properties. What I argue in the paper is that in the case of Cantor's criterion (i.e., in (C1)) we are not so justified in believing this.

As far as I can tell, then, the direct argument emerges unscathed from the points that McGee makes that might be thought to bear on it. However, most of his ire is reserved for the initial argument, so let us now move on to discussing that. This argument aims to establish that Cantor did not establish that there are infinite sets of different sizes. It is in terms of Russell's paradox: i.e., Russell's derivation of a contradiction from Frege's Basic Law V.

The heart of this argument is as follows. I start by arguing that if Cantor established with his theorem (i.e., Cantor's theorem) that there are infinite sets of different sizes, then the reason for—or the diagnosis of—Russell's paradox must be that there are more pluralities than objects. (For simplicity, I formulated Frege's Basic Law V, which I call (V), in terms of pluralities rather than concepts.) Since this was just the initial argument of the paper, I did not spell things out as fully as I might have done, but what I had in mind was the following.

By saying that the reason for, or the diagnosis of, Russell's paradox is that there are more pluralities than objects, I meant that the reason why (V) cannot be true—i.e., the reason why there is no function ext as (V) requires—is that there are more pluralities than objects. But why think that if Cantor's theorem establishes that there are infinite sets of different sizes, then the reason (V) cannot be true must be that there are more pluralities than objects?

Well, what Cantor's theorem directly establishes is that for any infinite set A, there is no one-to-one function from P(A) to A. But why think that this further establishes—i.e., puts us in a position to know—that A and P(A) are of different sizes; in particular, that P(A) is larger than A? It seems plausible that it does this only if the sole reason why there is no such function is that P(A) is larger than A (and if also we know that). For if, for all we know, there might be no such function for some other reason, then why would we be entitled to conclude from Cantor's result that P(A) is larger than A? Of course, it is in principle possible that we might have some very different route from Cantor's result to the fact that P(A) is larger than A, but I have no idea what that might look like—and I have never seen such an alternative articulated. Thus, it seems reasonable to assume that Cantor's result establishes that P(A) is larger than A only if the sole reason why there is no one-to-one function from P(A) to A is that P(A) is larger than A (and if also we know this).

(V) states that ext is a one-to-one function from pluralities to objects.[2] It would seem extremely plausible that if (for any infinite set A) the sole reason why there is no one-to-one function from P(A) to A is that P(A) is larger than A, then, similarly, the sole reason why there is no such function from pluralities to objects is that there are more pluralities than objects. Thus, it would seem that if Cantor's theorem establishes that P(A) is larger than A, then the sole reason why (V) cannot be true—i.e., the diagnosis of Russell's paradox—is that there are more pluralities to objects.

I argue, however, that this is not the sole reason why (V) is true. I do this by considering a variant—in fact a weakening—of (V), (V*). Whereas (V) attempted to assign a distinct object to each plurality, (V*) is a schema that merely attempts to assign a distinct object to each definable plurality—but where 'definable' means definable in a language that contains a term for the function that does the assigning (i.e., 'ext'). (V*) gives rise to a contradiction in essentially the same way that (V) does. Since (V*) is a weakening of (V), any reason (V*) cannot be true is a fortiori a reason why (V) cannot be. But no reason (V*) cannot be true involves size: since there are *not* more definable pluralities than there are objects. Thus, it would seem that at least one reason (V) cannot be true does not involve size (i.e., the—or every—reason (V*) cannot be true). But we saw that if Cantor's theorem establishes that there are infinite sets of different sizes, then the sole reason (V) cannot be true must be that there are more pluralities than objects (i.e., must involve size). Thus it seems that Cantor's theorem doesn't establish this, after all.

McGee's main criticism of this argument seems to be as follows:

> Terminological niceties aside, it's hard to see how the analogy between Cantor's proof and Russell's is useful as an explanation of where Frege went wrong. It's not as if Frege ever said to himself, "Postulating [(Va)[3]] [i.e., the one-to-one-function direction of (V)] is unproblematic, since there aren't more concepts than objects." Professor Whittle [in OIS] attacks Cantorian set theory on the grounds that Cantor's theorem fails to provide an

[2] Strictly speaking, (V) is a biconditional: one direction states that ext is a one-to-one function from pluralities to objects; the other states that if X and Y are pluralities of the same objects, then ext(X) = ext(Y). Since the latter is presumably a logical truth, I talk as if (V) states simply that ext is a one-to-one function from pluralities to objects. This simplifying assumption could easily be done without if desired.

[3] McGee has '(Vb)' here, but he clearly means (Va).

> adequate diagnosis of what went wrong in the *Grundgesetze*. I think he can fairly be accused of setting up a straw man. No one ever thought that an appeal to Cantor's theorem offered an adequate diagnosis of Russell's paradox. (*Op. cit.*, p. 3.)

In fact, I do not anywhere in OIS consider the view that Cantor's theorem provides a diagnosis of Russell's paradox. (I am not sure what that would mean.) What I do consider—and argue against—is the claim that the diagnosis of Russell's paradox is that there are more pluralities than objects. But I hope that I have made clear that far from being a straw man, this seems to be a consequence of the extremely widely held claim that Cantor's theorem establishes that the powerset of an infinite set is always larger than that set.

Indeed—concerning the 'straw man' question—on p. 7 (*op. cit.*) McGee writes:

> Because concepts are more numerous than objects, postulating (V) leads to inconsistency.

Is this not pretty close to the claim that the diagnosis of Russell's paradox is that there are more pluralities (or in this version: concepts) than objects—at least as articulated above, and as argued against in OIS?

McGee also argues that set-theoretic and semantic paradoxes need not have similar solutions (*op. cit.*, p. 9). Does my use of (V*) require that these must in fact have similar solutions? Absolutely not. All I rely on concerning (V) and (V*) is essentially that any reason why (V*) cannot be true is a fortiori a reason why (V) cannot be. Since (V*) is a weakening of (V) this seems hard to deny. And it is of course entirely consistent with this that our theories of definability, truth, etc. should be very different from our theories of sets.

It seems to me, then, that the initial argument also emerges pretty much unscathed from McGee's criticisms.

McGee ends his response by declaring that he accepts Cantor's 'theory of infinite numbers [i.e., ordinals and cardinals] as the great advance it appears to be' (p. 14). I should thus end mine by making clear that I too accept Cantor's theory of infinite ordinals and cardinals—and I certainly also regard it as a great advance! It is just that I do not think we are justified in believing that the theory of infinite cardinals is a theory of infinite size. Rather, I think we would do better to regard it simply as a theory of which sets have which sorts of functions between them. So viewed, it is still an important theory—just not of size.[4]

Reference

McGee, Vann. Forthcoming. Whittle's Assault on Cantor's Paradise. *Oxford Studies in Metaphysics*.

[4] Thanks to Zoltán Gendler Szabó.