# Strict Finitism and the Happy Sorites

**Ofra Magidor**

[Penultimate draft of paper forthcoming in *Journal of Philosophical Logic*

Please cite the published version]

**Abstract:** Call an argument a 'happy sorites' if it is a sorites argument with true premises and a false conclusion. It is a striking fact that although most philosophers working on the sorites paradox find it at prima facie highly compelling that the premises of the sorites paradox are true and its conclusion false, few (if any) of the standard theories on the issue ultimately allow for happy sorites arguments. There is one philosophical view, however, that appears to allow for at least some happy sorites arguments: strict finitism in the philosophy of mathematics.

My aim in this paper is to explore to what extent this appearance is accurate. As we shall see, this question is far from trivial. In particular, I will discuss two arguments that threaten to show that strict finitism cannot consistently accept happy sorites arguments, but I will argue that (given reasonable assumptions on strict finitistic logic) these arguments can ultimately be avoided, and the view can indeed allow for happy sorites arguments.

## §1. Introduction

Consider a well known version of the sorites paradox:

> One grain does not make a heap.
>
> For all n, if n grains do not make a heap, then n+1 grains do not make a heap.
>
> Therefore, $2^{100}$ grains do not make a heap.

As is well familiar, the premises of the argument seem to be true and its conclusion seems to be false. But is there any way to maintain both of these compelling intuitions without running into contradiction?

Call an argument a 'happy sorites' if it is a sorites argument with true premises and a false conclusion. More precisely, given natural numbers k, m>k, let a happy sorites be an argument that is (i) of the form: k is F. For all n, if n is F, then n+1 is F. Therefore, m is F; (ii) has true premises and a false conclusion.

It is a striking fact that although most philosophers working on the sorites paradox find the intuition that the premises of the sorites paradox are true and its conclusion is false highly compelling, few (if any) of the standard solutions to the paradox ultimately accept these intuitions and allow for happy sorites arguments. Consider some of the main views in the market: epistemicism about vagueness maintains that the major premise of the paradox is false (though we don't know which instance of it is false); contextualism about vagueness also maintains the major premise is false (though we never utter an instance of it falsely); And so does supervaluationism (though no instance of it is (super)false); Fuzzy logic claims that while the major premise is not fully false, it is also not fully true (true to degree 1); And nihilism about vagueness claims that vague terms like 'heap' are meaningless, and hence – I take it – the premises phrased in terms of it cannot be true.

However, there is a view in the philosophy of mathematics which at least appears to allow for happy sorites arguments: strict finitism. My aim in this paper is to explore to what extent this appearance is accurate. That is to say, to what extent does strict finitism in the philosophy of mathematics really allow for happy sorites arguments.

It should be noted at the outset that even if strict finitism does allow for happy sorites arguments, it is doubtful that many will see strict finitism as providing the ultimate solution to the sorites paradox. One reason is that even if it turns out that strict finitism does allow for *some* happy sorites arguments, this does not mean that according to strict finitism *all* sorites arguments are happy, and indeed many of the unhappy arguments may still make for troubling versions of the sorites paradox (more on this in §6.1).[1] More importantly, strict finitism is an extreme position that many will find unappealing, and thus it is unlikely that those who are unsympathetic to the view in the first place will adopt it merely for the sake of allowing for (some) happy sorites arguments.

[1] Though of course one might allow that different version of the paradox require different solutions. See for example Fara (2001) for such a view.

Still, the question of whether strict finitism really does allow for happy sorites arguments is nevertheless interesting, and as we shall see, far from trivial. Exploring this question can, I think, deepen our understanding both of strict finitism as a position in the philosophy of mathematics and of the sorites paradox and the full range of solutions that are available in addressing it – whether or not one ultimately chooses to opt for this particular solution.

Before I turn to the main line of discussion, a few preliminaries are needed. First, what do I mean by 'strict finitism'? As I will understand it, strict finitism is a species of constructivism in philosophy of mathematics. According to Michael Dummett, constructivism is  the which maintains that "the meaning of all terms, including logic constants, appearing in mathematical statements must be given in relation to constructions which we are capable of effecting, and of our capacity to recognise such constructions as providing proofs of those statements".[2] Strict finitism is a version of constructivism that interprets the phrase 'we are capable of effecting' above quite literally: we are capable of effecting a construction or surveying a proof if and only if it is *in practice* within our capacity to do so. So for example, according to strict finitism, a so-called proof of $2^{100}$ steps cannot truly count as a proof because, although finite, we are in practice unable to survey its details or recognise it as a legitimate proof.[3]

[2] Dummett (1975), p. 301.

[3] This broad characterization is no doubt underspecified and vague, and in particular leaves open various technical issues concerning the exact nature of strict finitistic logic and mathematics. Rather than settle these issues in advance, I will make (and where appropriate, justify) certain technical assumptions as the paper progresses. The purpose of this paper is not to argue that every version of strict finitism allows for happy sorites arguments. Rather, the paper attempts to explore whether there is a reasonably well-motivated, interesting, and coherent version of the view that allows for such arguments. Moreover, if such a version of the view can be found, then this might in itself provide a good reason for preferring this rather than other versions of strict finitism.

I also note that it is entirely beyond the scope and aims of this paper to argue for, or even motivate, strict finitism more generally. It is worth pointing out, though, that there are reasons to take many of the grounds that lead philosophers to be intuitionists – especially those involving Dummett's manifestation arguments – to ultimately support the more extreme, strict finitistic view. See Dummett (1975) and Wright (1982) for a defence of this claim, and Tennant (1997), ch. 5 for an argument against it.

Now it has been claimed - most notably by Dummett - that strict finitism is committed to the existence of non-empty sets of natural numbers that are closed under the successor operation, but which nevertheless have an upper bound.[4] To simplify things, let us call a set S of natural numbers a '*happy set*' if and only if it satisfies the following two constraints:[5]

Tolerance: $\forall n(n \in S \rightarrow n+1 \in S)$

Boundedness: $\exists m(m \notin S \wedge \exists n < m(n \in S))$

Thus according to Dummett, strict finitism is committed to the existence of happy sets. But given a happy set S, one can construct a happy sorites argument: let $n_0$ and $m_0$ be such that $m_0 \notin S$, $n_0 \in S$, and $m_0 > n_0$ (these exist by Boundedness). By Tolerance, for all n, if n is a member of S then n+1 is a member of S. So the sorites argument: '$n_0$ is a member of S; for all n, if n is a member of S then n+1 is a member of S; Therefore, $m_0$ is a member of S;', is a happy sorites argument. So if strict finitism is committed to the existence of happy sets it is committed to the existence of happy sorites arguments (and if strict finitism is committed to happy sorites arguments then it is committed to happy sets, at least assuming the view accepts set comprehension over natural numbers). But is Dummett right that strict finitism is committed to the existence of happy sets? The remainder of the paper shall be devoted to a discussion of this question.

In **§2**, I argue that the claim that happy sets exist is formally consistent in strict finitistic logic and arithmetic. This, however, will still leave open the question of whether strict finitism would accept the existence of happy sets. In **§3**, I will introduce a promising proposal, borrowed from Dummett, for a particular set that strict finitism may plausibly be thought to accept as a happy a set: the set of apodictic numbers. According to Dummett, though, the

[4] See Dummett (1975).

[5] In Magidor (2007), I used the term 'IF-set' for what I here call 'happy set'. Note that as I have defined happy sets, the claim that S is a happy set is slightly weaker than the claim that it is a non-empty sets of natural numbers that is closed under successor but has an upper bound. This weaker definition suffices for our purposes, and will serve to simplify the discussion.

finitist cannot accept that apodictic numbers form a happy set without running into an internal contradiction. If this is so, then if strict finitism is at all coherent, it should not after all accept this proposal. In **§4,** I introduce Dummett's argument, and argue that it fails. At this stage, it might be thought that we can be content with the claim that strict finitism is committed to the existence of happy sets. However, in **§5** I present a different argument ('the zooming argument'), one inspired by an idea of George Boolos, which seems to show that Dummett's proposal does after all lead the finitist to a contradiction. Moreover, this argument is more damaging than Dummett's original, because it can be generalised to show that not only apodictic numbers, but many other proposals for happy sets that strict finitism might be thought to allow, would ultimately lead to contradiction. In **§6**, I discuss a range of strategies that a finitist who nevertheless wishes to commit to happy sets might use in addressing the zooming argument and assess their merit in turn. And In **§7,** I briefly discuss what conclusions we can draw about strict finitism and the happy sorites. The two appendices provide proofs of two technical results that I appeal to in the course of the paper.

## §2 The consistency of happy sets

It is clear that in classical mathematics the claim that happy sets exist is inconsistent. Suppose by contradiction that there are happy sets. Let S be such a set. By Boundedness, S is not empty. Let $n_0$ be the minimal number of S. By Tolerance, $\forall n(n\in S\rightarrow n+1\in S)$. So by induction, for any natural number $k\geqslant n_0$, $k\in S$. But this is in contradiction with Boundedness: for suppose there is $n\in S$, and $m\notin S$, such that $m>n$. Since $n_0$ was the minimal member of S, $m>n\geqslant n_0$, in contradiction with the conclusion of the above induction.

Would this inconsistency argument be acceptable to a strict finitist? A crucial question is whether or not finitists would accept the principle of induction.[6] While it is not completely

[6] Although it is worth noting that induction is not the only assumption in the inconsistency argument that the finitist might not accept. Another issue is that the argument relies on the principle that every non-empty set of natural numbers has a minimal member – a principle that due to their constructivists commitments strict finitists are unlikely to accept.

obvious they would not accept the principle (at least in intuitionistic arithmetic, induction is considered a valid principle[7]), I nevertheless think that there are good reasons to assume that the finitist, as opposed to the intuitionist, would not accept the principle.

I take the following kind of argument to be the most intuitive and straightforward justification of induction[8]: Suppose that we accept F(0) and $\forall n(F(n)\rightarrow F(n+1))$ . Let m be an arbitrary natural number. Then by universal instantiation of the induction step and modus ponens we can show that F(1). And by another universal instantiation of the induction step and modus ponens we can show that F(2). And if we continue by m repeated applications of universal instantiation of the induction step and modus ponens, we can prove that F(m). But since m was arbitrary, it follows that $\forall m F(m)$.

This argument, however, will not persuade the strict finitist for two related reasons. First, for some large numbers m (e.g. $m=2^{100}$), the m-step proof suggested above will be too long to carry out in practice, and hence will not be finitistically acceptable.[9] Second, for some even larger numbers m (e.g. $m=10^{10^{100}}$) not all (classically acceptable) numbers smaller than m exist according to the finitist. The set of finitistically acceptable number is in a sense 'gappy': 1 is a finitistically acceptable number and $10^{10^{100}}$ is a finitistically acceptable number but there are (classically acceptable) numbers in between which cannot be constructed in practice, and hence not fintisitically acceptable. Not every number can therefore be 'reached' by starting from 0, and reapplying the successor operation and so the suggested argument in favour of induction would fail to convince the finitist.[10] It is plausible then to assume that strict finitist

[7] See Dummett (1977), p. 14, 34.

[8] Note that this kind of argument is also what Dummett takes to be the intuitionistic justification of induction (see Dummett (1977), p. 14).

[9] This is of course where the intuitionist differs from the finitist: for the intuitionist it suffices that we can *in principle* produce a proof of F(m). It is not required that we are able to carry the proof out in practice.

[10] The suggested argument is, of course, not the only available justification of induction. Other justifications appear, for example, in Frege (1974) and Dedekind (1963). However, accounts such as Frege and Dedekind's rely on their respective definitions of natural numbers, definitions that those who are genuinely sceptical about induction are unlikely to accept.

will not accept mathematical induction as a valid form of reasoning (and I will rely on this assumption for the remainder of the paper).[11]

But once we acknowledge that strict finitistic arithmetic does not include the principle of induction, we can prove that strict finitism is formally consistent with the existence of happy sets. For let T be a theory containing all first-order axioms of (classical) arithmetic, omitting the induction schema (note that this theory contains no second-order axioms or talk of sets). It follows from the compactness theorem for first-order logic that this set of axioms has a non-standard model M.[12] Now let $T^+$ be a theory which adds to T the following second-order sentence:

(H) $\exists S \forall n(n \in S \rightarrow n+1 \in S) \wedge \exists m(m \notin S \wedge \exists n<m(n \in S))$

Let $M^+$ be a model for the extended object-language which is just like M as far as the first-order vocabulary is concerned, and assume the standard semantics for second-order logic (i.e. the second-order variables of the object language range over sets of the domain of $M^+$).

I claim that M+ is a model for $T^+$. Since T contains no second-order vocabulary, M+ is clearly also model for T. And let X be the set of 'standard numbers' in M+ (i.e. the minimal set containing the interpretation of '0' and closed under the interpretation of the successor function). It is clear that X will be witness for H. (By definition X is non-empty and is closed under the interpretation of the successor function. Also, M+ contains a non-standard number, which is 'greater than' all standard numbers). Thus the claim that there exists a happy set is consistent with classical arithmetic, not including induction.[13] And so assuming that strict finitism is weaker than classical arithmetic[14], the claim that there exists a happy set conjoined

[11] Note that this assumption is also granted by Dummett in his 1975 (see pages 304-305).

[12] For the reader unfamiliar with non-standard models: roughly put, M is a model containing a number d such that d>0, d>1, d>2, and so forth. More precisely, there is an object d in the domain of M, such that if *d* is a constant of the object language which is interpreted as referring to d, then the object-language sentences '*d*>0', '*d*>1', '*d*>2' and so forth, will all be true relative to M.

[13] Note that the reason we have considered only first-order classical arithmetic is simply that the only second-order axiom included in standard second-order arithmetic is the (second-order) axiom of induction, which we are anyhow omitting.

[14] It is worth noting that it is not entirely clear that finitistic arithmetic is weaker than classical one. For example, finitistic arithmetic might maintain that there is a number with no successor (the largest number). However, this

with any (first-order) axioms we wish to include in finitistic arithmetic, have a model and are thus formally consistent.

**§3 Apodictic numbers**

In §2, I argued that the claim that there are happy sets is formally consistent in strict finitistic arithmetic. But this still leaves open the question of whether strict finitism would accept the existence of happy sets. A philosophical theory need not include every claim which it is otherwise logically consistent with. Moreover, in our current context it is worth keeping in mind that given their constructivist interpretation for the quantifiers, strict finitists would only accept that happy sets exist if there is some particular example of a happy set to which they are committed.

This naturally leads to the question of whether there are any particular examples of sets that strict finitists are likely to accept as happy sets. Any specific proposal for such a set must, I think, satisfy the following two constraints. First, we should have at least some prima facie reasons to think that the finitist would consider the set in question to be a happy set. That is to say, we must think there is some reason for the finitist to accept that the set is closed under the successor operation on the one hand, but that it has an upper bound (or at least that it satisfies the weaker Boundedness). Second, even if the finitist has prima facie reasons to accept that a certain set satisfies both Tolerance and Boundedness, if it turns out upon reflection that accepting these claims would lead the finitist to a contradiction by their own light, then I take it that the finitist would not after all want to accept to the proposal. Thus the second constraint on a feasible proposal is that accepting it would not lead to a contradiction by the finitist's own light. Even by the lights of the conventional classical theorist, many sets satisfy the first constraint: given the sorites paradox, for many sets one has prima facie reasons to accept both Tolerance and Boundedness. However, for the classical theorist no set satisfies the analogue of the second constraint: according to classical logic and mathematics

leads to subtle questions regarding how to interpret claims such as $\exists x \forall y(\neg y=x+1)$. Presumably, we would need some sort of free logic to handle terms such as 'm+1', where m is the largest number, and depending on which logic one appeals to, the claim $\exists x \forall y(\neg y=x+1)$ might after all fail to be true.

saying of any set that it satisfies both Tolerance and Boundedness does lead to a contradiction, and thus upon reflection the classical theorist would not want to allow for the existence of happy sets, despite their initial appeal. The question then, is whether strict finitism is in a similar predicament, or whether there are any cases of putative happy sets that the strict finitist can accept without contradiction.

A promising proposal for a set that might be thought to satisfy both of these constraints is suggested by Dummett in his seminal paper 'Wang's Paradox'.[15] Call a number n 'apodictic' if and only if n steps is an acceptable length for a proof according the finitist.[16] Now consider the set of apodictic numbers. This set seems like a promising candidate for satisfying the first constraint I suggested above: 1 is clearly apodictic and $2^{100}$ is clearly not apodictic, so Boundedness is satisfied. Moreover, the vagueness associated with the notion of being 'apodictic' (recall that this notion is defined in terms of 'a finitistically acceptable proof', which in turn is defined using vague terms such as 'proofs we are capable of surveying'), might lead one to think that 'apodictic' cannot have a perfectly sharp boundary, and hence that if n is apodictic then n+1 apodictic, i.e. Tolerance holds as well.[17]

How about the second constraint, namely that adopting the proposal not lead the finitist to a contradiction? One way to try and derive a contradiction would be to appeal to an argument by induction, but we have already seen in §2 that this argument would hold little force for the finitist. Another attempt might be to appeal to a step-by-step sorites argument of the following sort. Suppose that the set of apodictic numbers really is a happy set. So

(1) 1 is apodictic

[15] Dummett (1975).

[16] More precisely: Dummett defines 'apodictic' to mean that there is a finitistically acceptable proof of length n. But his argument which I discuss in §4 below, actually requires that *any* proof (with individually acceptable steps) of length n is finitistically acceptable. I will thus assume the latter definition.

[17] In Magidor (2007) pp. 404-406, I argued that the temptation to accept Tolerance in this case is not exactly due to vagueness and at any rate ought to be resisted. I will leave those worries aside here, especially since my main concern here is merely to show that there is a prima facie reason for the finitist to accept that apodictic numbers form a happy set, not that they are committed to this claim even if it ultimately leads to contradiction.

(2) If 1 is apodictic then 2 is apodictic (by universal instantiation of Tolerance). So 2 is apodictic (by 1 and Modus Ponens)

…

($2^{100}$) If $2^{100}$-1 is apodictic then $2^{100}$ is apodictic, so $2^{100}$ is apodictic.

But we know that $2^{100}$ is clearly not apodictic – hence a contradiction.

The problem is that if the details of the proof were to be filled out, it would consist of $2^{100}$ steps, and hence would clearly not form a fintisitically acceptable proof. So this attempt to show that adopting the proposal would lead strict finitists to a contradiction (by their own lights) fails as well.

At a first pass it seems, then, that the set of apodictic numbers are a promising candidate for satisfying my two constraints, and hence provide an example of a happy set that strict finitism might be committed to. However, it bears emphasis that I have not proved that the second constraint is satisfied: all I have shown is that two obvious attempts for deriving a contradiction based on the proposal fail, but there may well be other, more sophisticated ways to show that the accepting the proposal apodictic numbers form a happy set would the finitist to a contradiction. Indeed, Dummett has argued that the proposal does after all lead the finitist to a contradiction. In the next section, I will describe Dummett's argument, and explain briefly why it ultimately fails.

## §4 Dummett's argument[18]

Michael Dummett has provided a very interesting argument which purports to show that the claim that apodictic numbers form a happy set leads the finitist to a contradiction by their

[18] The material in this section is an abbreviated version of the more detailed argument appearing in Magidor (2007), pp. 407-410. It should be noted, however, that I was there concerned with Dummett's attempt to show that strict finitism is inconsistent. Here I assume that if accepting the claim apodictic numbers form a happy set leads the finitist to a contradiction, the finitist would simply reject the proposal. My focus here is simply with whether the finitist can feasibly accept that apodictic numbers form a happy set.

own lights. Dummett recognises that neither an argument by induction nor the simple step-by-step argument noted above will do. Instead he offers a more sophisticated version of the step-by-step argument.

Suppose that apodictic numbers formed a happy set. Now call a number n 'small' if and only if n+100 is apodictic. The first thing to note is that if the set of apodictic numbers is a happy set then so is the set of the small numbers: 101 is clearly apodictic, so 1 is small. And $2^{100}$+100 is clearly not apodictic, so $2^{100}$ is not small, so Boundedness is satisfied. Moreover, if apodictic numbers satisfy Tolerance then so do small numbers: if n is small, then n+100 is apodictic, so by Tolerance (as applied to apodictic numbers) n+101 is apodictic, so by definition, n+1 is small.

Dummett then proceeds to argue as follows (1975, p. 306): "Now it seems reasonable to suppose that we can find an upper bound M for the totality of apodictic numbers such that M-100 is apodictic. (If this does not seem reasonable to you, substitute some larger number k for 100 such that it does seem reasonable…and understand k whenever I speak of 100)". We can now construct a new step-by-step argument concerning small:

(1) 1 is small

(2) If 1 is small then 2 is small. So 2 is small

…

(M-100) If M-100-1 is small then M-100 is small. So M-100 is small.

But by definition of 'small', the claim that M-100 is small entails that M is apodictic. But this contradicts the stipulation of M.

What's interesting about this argument is that as opposed to the original one, here the finitist cannot argue that the proof of a contradiction is too long to be finitistically acceptable. After all, the proof is M-100 steps long and by stipulation M-100 *is* apodictic, which means precisely that the proof is short enough to be finitistically acceptable.

The problem, however, is that Dummett's argument contains a crucial hole. The difficulty lies with the supposedly naïve side-comment in parentheses which I quoted from Dummett above: "(If this does not seem reasonable to you, substitute some larger number k for 100 such that it does seem reasonable…and understand k whenever I speak of 100)". I agree that there must be some number k which satisfies Dummett's constraints (namely: there is a number M such that M is not apodictic and M-k is apodictic). But suppose k is a number which itself is not apodictic. Recall that 'n is small' would now be interpreted to mean that n+k is apodictic. But then if k is not apodictic, 'n is small' would be false even for n=1. So already the first premise of Dummett's argument above would be false, and the argument would not go through.

In order to ensure that the argument works, Dummett needs to show that finitist accepts the following claim:

(*) There is a number k and a number M, such that M is not apodictic, k is apodictic, and M-k is apodictic.

But why should the finitist accept (*)? Try to think of examples. $10^{100}$ is not apodictic, and it is easy to come up with examples for a number k such that $10^{100}$-k is apodictic. But any example that immediately springs to mind is something like k=$10^{100}$-100, which is not an apodictic number. It is thus far from obvious that we can come up with example that would vindicate (*).[19]

[19] In Magidor (2007), p. 409, I consider a rejoinder that attempts to defend (*). The idea is to start with an apodictic number k; If 2k is not apodictic, then (*) is proved (letting k and 2k stand for k and M in (*)). Otherwise, take 4k. If it is not apodictic, then (*) is proved (with respect to 2k, 4k). And so forth, for at most 100 iterations (since $2^{100}$k is not apodictic). But I argue (Magidor (2007), pp. 409-10) that this rejoinder ultimately fails.

I conclude that Dummett's argument does not succeed in showing that accepting apodictic numbers as a happy set leads the finitist to a contradiction. So far, then, things look good for the proposal that strict finitism can accept apodictic numbers as a happy set.

## §5 The zooming argument

But the story is not over yet. I would now like to propose a different argument (call it 'the zooming argument') which suggests that strict finitism cannot after all consistently accept the claim that the apodictic numbers form a happy set. The argument is based on an idea of George Boolos (though Boolos suggested this idea in a different context, one that is not concerned with strict finitism).[20]

Here is how the argument goes. Let 'A(n)' stand for 'n is apodictic', and assume as above that apodictic numbers form a happy set. We first go through the following mini-argument:

(1) $\forall n(A(n)\rightarrow A(n+1))$ [by Tolerance].

(2) $A(n)\rightarrow A(n+1)$ [from (1), by universal instantiation]

(3) $A(n+1)\rightarrow A(n+2)$ [from (1), by universal instantiation]

(4) $A(n)\rightarrow A(n+2)$ [from (2) and (3)]

(5) $\forall n(A(n)\rightarrow A(n+2))$ [from (4), by universal generalisation]

It should be noted that each of the steps in this mini-argument is intuitionistically acceptable, and so it is reasonable to suppose that it is also finitistically acceptable. We can now construct an additional mini-argument of the same length, starting from step (5):

(5) $\forall n(A(n)\rightarrow A(n+2))$

(6) $A(n)\rightarrow A(n+2)$ [from (5), by universal instantiation]

[20] Boolos (1991)

(7) $A(n+2)\rightarrow A(n+4)$ [from (5), by universal instantiation]

(8) $A(n)\rightarrow A(n+4)$ [from (6) and (7)]

(9) $\forall n(A(n)\rightarrow A(n+4))$ [from (8), by universal generalisation]

Now it is easy to see that in a similar fashion, given the assumption that $\forall n(A(n)\rightarrow A(n+2^k)$, one can provide a 4-step mini-argument to the conclusion that $\forall n(A(n)\rightarrow A(n+2^{k+1}))$, each step of which is fintistically acceptable. So using 400 steps, we can construct an argument that starts with the assumption that $\forall n(A(n)\rightarrow A(n+1))$ and concludes with $\forall n(A(n)\rightarrow A(n+2^{100}))$. Each step of this argument seems finitistically acceptable, and the whole argument is only 400 steps long, which means the proof as a whole is finitistically acceptable. The finitist must therefore accept that for all n, if n is apodictic, then $n+2^{100}$ is apodictic. But since 1 is apodictic, this entails that $2^{100}+1$ is apodictic, which is a contradiction.

One thing to note about this argument is that, as opposed to Dummett's argument, it does not rely on the particular meaning of 'apodictic'. In fact, it is easy to see that the argument would be equally damaging against any happy set proposal with $2^{100}$ as an upper bound. Also as opposed to Dummett's argument, I don't think this argument does contain a fallacy.[21] Nevertheless, I do not think that we should yet conclude that strict finitism is not committed to the existence of happy sets (or at least something very much like them). In the next section I will discuss various ways in which the finitist might nevertheless accept the existence of happy sets, despite the zooming argument.

## §6 Strategies for dealing with the zooming argument

### §6.1 Happy sets with large upper bounds

The first strategy I would like to discuss in response to the zooming argument follows directly from the observation that the argument only explicitly shows that strict finism cannot

[21] Although it does make an assumption about the finitist's proof system that I shall go on to question in §6.3 below.

consistently accept happy sets with the upper bound of $2^{100}$. If successful, the argument does show, for example, that the strict finitist cannot accept the claim that apodictic numbers form a happy set. But how about other proposals for happy sets, ones for which we can only find much larger upper bounds?

Call a number 'not so large' if it is significantly smaller than $2^{2^{100}}$. Consider the set of not so large numbers. This set is at least a good prima facie candidate for being a happy set: 1 is clearly a member of the set, and $2^{2^{100}}$ is clearly not a member of the set. Moreover, the vagueness involved in the notion of a 'not so large' might well lead one to think that if n is not so large, then n+1 is not so large either.

Can the finitist, then, consistently accept that the set of not so large numbers is a happy set? By applying the original zooming argument we can provide a finitistically acceptable proof that $2^{100}$ is not so large. But this is no contradiction: plausibly $2^{100}$ is indeed significantly smaller than $2^{2^{100}}$. On the other hand, if we were try to generalise the zooming argument so as to produce a proof that $2^{2^{100}}$ is not so large, the proof would require more than $2^{100}$ steps, and hence would clearly not be finitistically acceptable.

Boolos's proof trick is thus insufficient to derive a contradiction in this case. Still, having seen one clever proof trick that allows one to exponentially shorten the length of the original step-by-step argument, one might wonder whether there aren't other tricks that would allow us to provide even shorter proofs. The issue is not merely whether the specific upper bound of $2^{2^{100}}$ is a sufficiently large one to render any 'proof tricks' unhelpful in driving a finitistically acceptable proof of contradiction. After all, if it isn't sufficiently large, we could always suggest new putative examples for happy sets, ones with even larger upper bounds (for any n we can consider the predicate 'significantly smaller than n'). But the issue is whether there even exists an upper bound that is too large to allow for a finitistically acceptable proof of contradiction. And it's important to note that it is not trivial that there does exist such an upper bound. Let us grant that the length of the shortest proof from A(1) and $\forall n(A(n) \rightarrow A(n+1)$ to A(k) increases as k increases. Still, we need to keep in mind that in order to find a suitable happy set, k has to be small enough that it is nevertheless a finitistically acceptable number – i.e. it is constructible in practice.[22] That is to say, we need to show that there is a suitable upper bound k such that it is on the one hand small enough to be a finitistically acceptable number, but on the other hand is large enough so that the shortest proof from A(1) and $\forall n(A(n) \rightarrow A(n+1)$ to A(k) is too long to be finitistically acceptable.

In Appendix I of this paper, I provide a proof that there indeed exists such a number k (moreover, the proof will show how to construct a specific number k satisfying these constraints). Thus although the matter is not trivial, it turns out that there are specific proposals with sufficiently large upper bounds that the finitist can consistently accept as happy sets.

Still, even with the technical worries put the rest, there are some philosophical worries concerning this strategy. Call a number 'not large' if it significantly smaller that $2^{100}$ and 'not super large' if it is significantly smaller than the suitable upper bound k, suggested above. As we have seen, the finitist can consistently accept that the set of not super large numbers is a happy set, but (due to the zooming argument) cannot consistently accept that the set of not large number is a happy set. But the motivation for accepting Tolerance in the latter case (namely the vagueness of 'significantly smaller than $2^{100}$') seems precisely the same as our motivation for accepting Tolerance in the former case (namely the vagueness of 'significantly smaller than k', for the relevant bound k). Thus the challenge for one who chooses to adopt this strategy is to explain why one might be motivated to ultimately accept that the set of not super large numbers is a happy set, while nevertheless rejecting the equally compelling claim that the set of not large numbers forms a happy set. Or to put matters otherwise, is there any

[22] Of course this depends on exactly which kinds of constructions are allowed. Throughout this paper, I assume that finitism at least allows constructions which involve addition, multiplication, and exponentiation. One appealing way to generalise this assumption is to allow for any construction that can be defined in primitive recursive arithmetic using a definition that is constructible in practice. Note that even though on this generalised approach the finitist will be able to construct some extremely large numbers, since the finitist cannot appeal to induction, there will be various basic theorems concerning these extremely large numbers that it will be impossible for the finitist to prove.

motivation for a solution to the sorites paradox that addresses some versions of the paradox, but has little to say about other compelling versions?

There are various points that a finitist wishing to draw on this strategy might raise in response to this challenge. First, the finitist might point out that the difference between the case of 'not super large' and that of 'not large' is precisely that we can consistently accept the former but not the latter as happy sets. (Compare this to response to Russell's paradox: one might think that the only motivation for accepting the axiom of separation while rejecting unrestricted comprehension is simply that the former but not the latter is consistent). Second, it is worth noting that it was clear from the outset that the strict finitistic strategy for dealing with the sorites paradox only ever had a hope in addressing versions of the paradox with relatively many steps. After all, it is clear that no strict finitistic solution would work for versions of the paradox that require only seven steps. All that the zooming argument has shown is that the view is only suitable in addressing versions of the paradox with even more steps than we may have originally assumed. Third, it may be noted that it is not completely implausible to give a somewhat different treatment to 'short' and 'long' versions of the paradox. Consider the set of (natural) numbers that are significantly smaller than 5. 1 is clearly a member of the set, and 5 is clearly not a member of the set. Moreover, the set is defined using a vague description, and there certainly seem to be borderline cases of membership in this set (is 3 a member of the set or not?). Still, many might nevertheless feel reluctant to accept that if n is a member of the set, then so is n+1 (perhaps this is because one can immediately see how accepting this claim will lead one to a contradiction, or perhaps simply that when working with such small numbers, it is not so surprising that one unit can make a difference).[23] Thus it seems that even amongst those that are tempted by the thought that vagueness often provides a good motivation for accepting Tolerance, the motivation seems much more compelling in the case of sorites arguments with large upper bounds, than those with smaller ones. Finally, one may point out that the most compelling versions of the paradox are ones that involve dense domains, where the steps of the paradox can be made arbitrary small. Consider a sorites

[23] It is worth noting in this context that the motivation Boolos provides for introducing the zooming argument is that he takes the major premise of the paradox to be significantly more compelling if the shortest derivation of a contradiction is extremely long, and thus he suggests that by offering a shorter derivation of a contradiction he can make the rejection of the major premise more intuitively palatable (see Boolos (1991), p. 699).

paradox for ‘tall’. One might start with a major premise claiming that if a person who is n cm tall is not tall, then a person who is n+1 cm tall is not tall. However, if one fails to find the claim that 1 cm cannot make a difference to whether someone is tall or not especially compelling, we can move to a version of the paradox where the major premise involves steps of 1mm. Don’t find this compelling enough? Consider a version with steps of 1 nanometre. And so forth. The upshot is that sorites arguments with very small steps seem to be the most compelling, and the smaller the increments involved in each steps, the larger the *number* of steps one needs to reach a false conclusion.

Nevertheless, these lines of response may not be sufficiently convincing. After all, even if one accepts that versions of the paradox with relatively small increments and relatively many steps seem more compelling, it may seem that $2^{100}$ steps should be sufficient. Other strategies for dealing with the zooming argument might thus be preferred.

### §6.2 Replacing Tolerance with its instances

The next strategy I would like to suggest for dealing with the zooming argument is to replace the commitment to happy sets with a commitment to something very much like them. Call a set S of natural numbers a ‘happy* set’ if:

(i) The set satisfies Boundedness

(ii) If $1\in S$ then $2\in S$

(iii) If $2\in S$ then $3\in S$

… and so forth.

In short, a happy* set is defined by replacing the universal Tolerance with all of its instances.

Now it is worth noting that the general claim that there exist happy* sets is consistent even by classical standards (although it is not ω-consistent). To see this, take all the classical axioms of arithmetic together with a new predicate P, which stands for membership in S. The claim that S is a happy* set can thus be specified by adding to our theories the sentences: $\exists m(\neg P(m) \wedge \exists n<m\ P(n))$ (ii) $P(1)\rightarrow P(2)$ (iii) $P(2)\rightarrow P(3)$, and so forth. It is easy to see that any finite subset of these sentences has a model, and thus it follows from the compactness theorem that the theory as a whole has a model.[24] (Moreover, it would have a model even if we add to our theory every instance of the induction schema, including one that applies to the new predicate P).

Of course, it is not classically consistent to claim of any particular set for which we can specify a standard number as its upper bound that it is a happy* set. But as long as we pick a set with a upper bound that is sufficiently large so that one cannot reach its upper bound using a number of steps that is allowed in a finitistically acceptable proof, I see no reason to think that the claim that that the set in question is a happy* set will be inconsistent by strict finitistic standards. Thus, for example, consider the proposal that apodictic numbers form a happy* set. This proposal seems unproblematic by finitistic standards: we can still construct that analogue of Dummett's argument, but it would fail for precisely the same reason that Dummett's original argument failed. And without the universal premise Tolerance, we can no longer reconstruct the zooming argument.

The current strategy for responding to the zooming argument is one that makes a certain concession to the argument, because it replaces project of finding a happy set that the finitist is committed to with a different project – one of finding a happy* set. Still, the concession seems relatively small: it is an interesting enough result if strict finitism accepts the existence of happy* sets.

However, the challenge to those who opt for this strategy is clear: are there any philosophical motivations for accepting that claim that, say, apodictic numbers form a happy* set, while rejecting the claim that they from a happy set? That is to say, is there any motivation for accepting all of the instances of Tolerance while rejecting the universal claim? Again, various issues can be raised in considering this challenge: Is it plausible to reject the universal claim while accepting its instances merely because a commitment to the universal claim leads to

[24] Recall that the compactness theorem states just this: given an infinite set of first-order sentences, if every finite sub-set has a model, than the set has a model.

inconsistency? Is our acceptance of the universal claim prior to acceptance of each of its instances, or vice versa? Can we give a plausible story of what accepting the infinitely many instances of Tolerance involves, which does not go via accepting the universal claim?

One who is disposed towards the current strategy might well be able to address these questions in a favourable way. After all, it is not unusual in discussions of the sorites paradox to give more weight to one's intuition that each instance of the major premise is true, than to the intuition that the universal premise is true. For example, contextualism about vagueness claims that the universal premise is false as uttered in any context, but every instance of it is true whenever uttered. Similarly, supervaluationism about vagueness maintains that the universal premise is (super) false, but no instance of it is false (some instances are true, and some are neither true nor false).[25] I shall leave the question of how philosophically satisfactory the current strategy ultimately is aside for now. [26]

### §6.3 Using cut-free proof systems

The final strategy that I would like to discuss seems to me the most promising one. In the discussion so far, I have implicitly assumed that the finitist allows use of procedures or rules such as the cut-rule or its correlates. Very roughly, the cut-rule is what allows us to reapply, as a single step in the proof, previously proved results or lemmas.[27] In the zooming argument,

---

[25] See also Dummett (1975), p. 304 for a proposal that accepts each of the instances, without accepting the universal major premise.

[26] It is worth raising in this context another strategy one might suggest for dealing with the zooming argument, one that also involves replacing the concept of a happy set with a slightly different one. The proposal is to replace the universal Tolerance with the claim Tolerance*: $\neg\exists n(n\in S\wedge\neg n+1\in S)$. Tolerance* is equivalent to Tolerance in classical logic but not in intuitionistic logic. In intuitionistic logic Tolerance* does entail $\forall n(n\in S\rightarrow\neg\neg n+1\in S)$, but this claim is insufficient for use in the zooming argument, because from $A(n)\rightarrow\neg\neg A(n+1)$, and $A(n+1)\rightarrow\neg\neg A(n+2)$, one cannot, in intuitionistic logic, infer that $A(n)\rightarrow A(n+2)$. However, this proposal will not help because one can use Tolerance* in a 'reverse zooming argument': an argument that uses Boolos's strategy to prove using a mini-argument that $\forall n(\neg A(n+2)\rightarrow\neg(A(n))$, and then uses another mini-argument to show that $\forall n(\neg A(n+4)\rightarrow\neg(A(n))$, and so forth, until using 400 steps one can show that $\forall n(\neg A(n+2^{100})\rightarrow\neg(A(n))$. But then for any case where the finitist accepts that A(1) but $\neg A(2^{100}+1)$ one can still derive a contradiction, so this strategy ultimately fails. (See Read & Wright (1985) for the proposal to use a 'reverse sorites' to attack an intuitionistic solution to the sorites paradox).

[27] More precisely, in Gentzen's sequent calculus, the cut-rule is the rule that allows one to infer the sequent $\Gamma\rightarrow\Delta$ from the sequents $\Gamma\rightarrow\Delta$, A and A,$\Gamma\rightarrow\Delta$, and a cut-free proof is one that does not use the cut-rule. (Think of A as corresponding to the lemma we are applying). It is actually not entirely obvious how to define the correlate of a cut-free proof with respect to other proof systems, but a standardly accepted principle is that cut-

for example, having proved that $A(n)\rightarrow A(n+2)$, and that $A(n+2)\rightarrow A(n+4)$, we were able to infer using (roughly) one step that $A(n)\rightarrow A(n+4)$. But not all proof systems allow such moves: even in classical logic there are sound and complete proof systems that do not allow it. (Of course, in such classical systems it is still true that if $A(n)\rightarrow A(n+2)$ and $A(n+2)\rightarrow A(n+4)$ are provable from our premises, then so is $A(n)\rightarrow A(n+4)$, but the proof of the latter may require a lot more than one additional step).

The final strategy I would like to suggest in response to the zooming argument, then, is one that insists that the strict finitist appeal to a cut-free proof system. Now as with the previous strategies there are two issues to consider: first, is adopting this strategy technically sufficient to block the relevant contradiction. And second, assuming it is technically sufficient, is the strategy well motivated on philosophical grounds.

With respect to the technical issue, a crucial result is proved by Boolos in his paper 'Don't eliminate cut' (Boolos 1984). What Boolos shows is that with respect to at least one paradigmatic cut-free proof system (proofs by tableaux), the shortest (classical) proof of $A(k)$, from the claims $A(1)$ and $\forall n(A(n)\rightarrow A(n+1))$ and the (first-order) axioms of arithmetic (not including induction on A of course), is at least k steps long. This shows, for example, that in the relevant cut-free system, any proof of $A(2^{100})$ from the claims $A(1)$ and $\forall n(A(n)\rightarrow A(n+1))$ will be too long to be finitistically acceptable, and thus no analogue of the zooming argument can be found in the relevant system. Moreover, although Boolos only proves his result with respect to one cut-free system, he assumes that his claim can be generalised to cut-free systems more generally.[28] I think this claim is right, and to support it, I propose in Appendix II a proof which shows how to generalise Boolos's result to a cut-free

free proofs have to at least obey the 'sub-formula property': if one proves A from Γ, then all formulas appearing in the proof must be sub-formulae (or instances of sub-formulae) of Γ, A. (Note that auxiliary lemmas usually violate the sub-formula property). In keeping with this principle, the tableaux system is standardly taken to be cut-free, while Natural Deduction is taken to be a non cut-free system.

[28] Of course this is not to say that the result holds in *any* cut-free system. It clearly would not hold in a system that simply had $(A(1)\wedge\forall n(A(n)\rightarrow A(n+1)))\rightarrow\forall n(A(n))$ as an axiom, whether or not the system were cut-free. I take it that what Boolos means is that this claim holds in standard cut-free systems that do not include such axioms.

version of Gentzen's sequent calculus. It thus seems that by appealing to cut-free systems, the finitist can indeed avoid the zooming argument (as well as any other 'proof tricks').

How about the philosophical motivations for the current strategy? I think that it is actually quite plausible to assume that a finitistically acceptable proof system would be cut-free. For one thing note that one intuitive justification for the cut-rule is something like this: suppose that A is provable from Γ and that B is provable from A, Γ. Then it seems that B must be provable from Γ, because we can 'paste' together the two proofs, to produce a proof of B from Γ. This justification, however, need not persuade the strict finitist: while there may be a short enough proof of A from Γ, and a short enough proof of B from A, we have no guarantee that 'pasting' the two proofs together would yield a proof that is short enough to be finitistically acceptable.[29]

More importantly, cut-free proofs play a crucial rule in constructivist arithmetic more generally. Suppose one proves, from some axioms of arithmetic, a claim of the form $\forall x\exists y\varphi(x,y)$ (where φ contains no unbounded quantifiers). A constructivist interpretation of this formula requires that there be an effective procedure, such that for every number x, the procedure would produce a particular 'witness' y, such that $\varphi(x,y)$. Now it turns out, that given a *cut-free proof* of $\forall x\exists y\varphi(x,y)$ one can use this proof to construct precisely such an effective procedure.[30]

Now it's worth pointing out, that intuitionists are nevertheless willing to accept use of the cut-rule. Why? Because of the cut-elimination theorem (which can be proved in both classical

[29] However, it is worth keeping in mind here that given our discussion of (*) above, the finitist should not accept that there are two proofs that are short enough to be finitistically acceptable, but where combining them would yield a proof that is too long to be finitistically acceptable. On the other hand, this does not mean that they should accept the negation of this claim. In short, finitists ought to neither accept nor reject the claim that there exist such proofs.

[30] See Buss (1998b), §3 ('witnessing theorems') for a more precise characterisation and proof of the relevant results.

and intuitionistic logic): since any proof with cut can be constructively converted to a cut-free proof, the use of the cut-rule is deemed to be harmless. Crucially, however, this line of thought does not apply for the strict finitist. The cut-elimination theorem is *not* valid by strict-finitistic standards: after all, our discussion so far provided us precisely with an example where a claim is finitistically provable in a system with cut (the zooming argument) but is not finitistically provable in a cut-free system (see Theorem II in the appendix).

The strategy that insists that the finitist appeal to cut-free proof systems thus seems both well-motivated philosophically, and technically sufficient in order to block the zooming argument. If this strategy is accepted, we can return to our very first proposal and allow that strict finitism can accept the claim that apodictic numbers form a happy set.

## §7 Conclusion

My aim in this paper was to assess whether strict finitism might allow for an unusual solution to (at least some versions) of the sorites paradox: one that takes our intuitions concerning the sorites argument at face-value, and thus allows for happy sorites arguments.

One often gets the impression in philosophy that as long as we are willing to accept sufficiently radical assumptions (modal realism, paraconsistent logic, strict finitism, and so forth…), we would gain solutions to difficult philosophical problems very easily. As we have seen, at least in the case in question, this impression is far from accurate. Even if one accepts the radical position of strict finitism, one which at a first pass seems to allow for happy sorites arguments, much careful thought is needed to assess with whether strict finitism really is committed to the existence of such arguments. On the other hand, I have tried to show that despite various potential difficulties, strict finitists can reasonably accept the existence of happy sorites arguments. Either way, the discussion will serve, I hope, to deepen our understanding both of strict finitism and of the sorites paradox.[31]

[31] For helpful comments and discussion, I am grateful to audiences at the University of Barcelona, University of Connecticut, Hebrew University, Institut Jean Nicold, University of Oxford, and the 2006 Joint Session as well

## Appendix I[32]

**Theorem:** There exists a number k, such that k is finitistically acceptable, but so that any proof (in Gentzen's first-order sequent calculus) of the sequent $S_k$ is too long to finitistically acceptable, where $S_k$ is: $\Gamma_0$, $A(t_1)$, $\forall x(A(x)\rightarrow A(x+1)) \rightarrow A(t_k)$, $\Gamma_0$ is a collection of axioms of arithmetic (not including induction on A), and $t_1$ and $t_k$ are terms which have 1 and k as their numerical values respectively.[33]

*Informal gloss*: there is a finitistically acceptable number k, such that any proof of a sorites argument with A(1) as its premise and A(k) as its conclusion, is too long to be finitistically acceptable.

Note: Recall that it is important for our purposes that the proof be constructive in the sense that it provide a specific k which satisfies the relevant requirements, and the proof below is indeed constructive in this sense.[34]

**Proof:** There are two separate measures of the complexity of proofs. The first measures (roughly) the number of inferences or steps in each proof, and the second measures the complexity of each of the formulas appearing in the proof. Let the *length* of the proof P be the number of strong inferences occurring in P.[35] And let the *depth* of a formula be defined as follows:

---

as to Michael Dummett, Volker Halbach, Carl Posy, Josh Schechter, Bruno Whittle, and an anonymous referee. Special thanks to Timothy Williamson for extensive feedback on an earlier version of the paper, and to Menachem Magidor for very helpful discussion of the issues, and for his part in the proof of theorem II.

[32] The system I will use throughout appendix I and II is Gentzen's first-order sequent calculus, as it is defined in Buss (1998a), p. 11 and p. 32. All other definitions and results that are not explicitly defined or proved below, can also be found in Buss (1998a).

[33] The numerical value of a term is the interpretation it receives in the standard model for the language of arithmetic.

[34] Note, though, that the proofs in Appendix I and II are not intended to be finitistically acceptable (they provide a classical study of strict finitism).

[35] A strong inference is one that involves one of the propositional rules, one of the quantifier rules, or cut.

(i) if φ is atomic, then depth(φ)=0;

(ii) depth($\varphi_1 \vee \varphi_2$) = depth($\varphi_1 \wedge \varphi_2$) = depth ($\varphi_1 \rightarrow \varphi_2$) = 1+max{depth($\varphi_1$), depth($\varphi_2$)}

(iii) depth(¬φ) = depth (∃xφ)= depth(∀xφ)=1+ depth(φ).

We will also use the standard notation for super-exponentiation ($2^n{}_k$ will mean stacking k 2's on top of each other with n at the top. E.g. $2^{100}{}_2 = 2^{2^{100}}$ ).

Now one important assumption that I will make is that the way strict finitism limits the length of proofs is by defining a precise, and reasonably small limit *md* to the depth of each formula appearing in a finitistically acceptable proof, while allowing the limit on the number of inferences in a proof to be defined using the more vague notion of an apodictic number.[36] When I say that *md* is 'reasonably small', all I require by that is that $2^{100}{}_{2md+3}$ is a finitistically acceptable number. Recalling that I allow exponentiation as a finitistically acceptable form of construction (see f.n. 22), this is not a very tight restriction on *md*: all that is required is that that it is possible in practice to write down, say, 2*md*+10 numerals.[37]

[36] Note that this assumption has been already implicitly in play throughout the paper, since we only considered limitations on the number of steps in proofs, and have not mentioned any limitations on the complexity of each step. But if the steps are not themselves bounded, then it would not be true that even 2 is apodictic (presumably, some proofs containing exactly two very complex steps would not be finitistically acceptable). See also f.n. 37 below.

[37] If one does not like the assumption that the finitist artificially limits the depth of formulas in proofs in this manner, here is one alternative. Assuming we work with a syntax such that any new addition of a connective or quantifier requires use of parentheses, one can then show that any formula with depth d contains at least 3d characters. But then one can pick *md* so that 2*md*+10 is just about small enough that one can in practice write 2*md*+10 characters, but so that 3*md* is already too large a number of characters to be capable in practice to be written. This would ensure that *md* is small enough for our needs in the proof, and also that we can show (rather than stipulate) that the depth of any formula appearing in a finitistically acceptable proof is smaller than *md* (otherwise, the formula will have at least 3*md* characters, and hence would not be finitistically acceptable).

The down-side of this alternative is that I don't think it would be possible to specify a particular number *md* that knowably satisfies these constraints (cf. my discussion in Magidor (2007), pp. 409-410). But it seems that if we want our proof to result in a particular choice of *k*, we do want to be able to commit to a particular choice of *md*.

Now let k = $2^{100}_{2md+3}$. By our restriction on *md*, k is a finitistically acceptable number. Suppose, by contradiction that there existed a finitistically acceptable proof $P_0$ of a sequent $S_k$ defined above. The first result we need to appeal to is the cut-elimination theorem. As stated and proved in Buss (1998), p. 37, the cut-elimination theorem says that if P is a proof such that every cut formula in P has a depth of at most *d*, then there is a cut-free proof P* with the same end-sequent as P, with a length of at most $2^{\mathrm{length}(P)}_{2d+2}$. Since (we have assumed) the maximum depth for any formula in a finitistically acceptable proof is *md*, then we know that the maximum depth of a cut formula in $P_0$ is *md*. It follows that there is a cut-free proof of $S_k$ of length of at most $2^{\mathrm{length}(P_0)}_{2md+2}$.

But by the claim proved in Appendix II below, any cut-free proof of $S_k$ is at least of length k. So we can conclude that $2^{\mathrm{length}(P_0)}_{2md+2} \geqslant k = 2^{100}_{2md+3} = 2^{2^{100}}_{2md+2}$. So by the monotonicity of $\log_2$, $\mathrm{length}(P_0) \geqslant 2^{100}$, in contradiction to the assumption that $P_0$ was a finitistically acceptable proof. Thus k = $2^{100}_{2md+3}$ is a finitistically acceptable number for which there is no finitistically acceptable proof of $S_k$. QED.

## Appendix II

**Theorem:** Let $S_k$ be the same sequent defined in appendix I above ($\Gamma_0$, $A(t_1)$, $\forall x(A(x) \rightarrow A(x+1)) \rightarrow A(t_k)$). Then any cut-free proof of $S_k$ in Gentzen's first-order sequent calculus contains at least k strong inferences.[38]

*Informal gloss*: In a cut-free system, any proof of a sorites argument with A(1) as its premise and A(k) as its conclusion, is at least k steps long.

[38] In fact, we will prove an even stronger claim: that the proof contains at least 2(k-1) strong inferences.

**Proof[39]:** We note first that any formula appearing in a cut-free proof of a sequent must be either a sub-formula or an instance of a sub-formula, of a formula appearing in its end-sequent. (This follows by straightforward induction on the rules of the system). Let P be a cut-free proof of $S_k$. Given our end-sequent $S_k$, it follows that the only formulae containing A that can appear in P, are of the form $\forall x(A(x)\rightarrow A(x+1))$, A(t), or $A(t)\rightarrow A(t+1)$ (where t is some term). Next we prove the following lemma.

**Lemma:** Given a sequence or set of formulae Γ, call a number n an A-value of Γ, if and only if there is some term t, such that n is the numerical value of t, and where Γ contains the formula A(t). Let S be a sequent appearing in P. Let n be the *minimal* A-value appearing in the succedent (RHS) of S. Let k be the *maximal* A-value appearing in the antecedent (LHS) of S. Then for every m such that $k \leq m \leq n$, there is some sequent $S_1$ appearing in the proof tree above (or including) the occurrence of S, such that m is an A-value of one of the formulae appearing in the antecedent of $S_1$.

**Proof of Lemma:** By induction on the levels of the proof tree.

Base case: Assume S appears in a leaf of the proof tree. Note that the only sequent containing A that can appear at the leaf of a proof tree is of the form $A(t)\rightarrow A(t)$. But then if n is the numerical value of t, the only m for which we need to prove the claim is m=n, and we are done.

Induction step: Assume that the lemma is true for any sequent appearing above S in the proof tree. Note that the only inferences for which we might need to prove something new is ones where formulae of the form A(t) are removed from either the antecedent of one of the upper sequents (hence possibly decreasing the maximal A-value of the antecedent), or from the succedent of one of the upper sequents (hence possibly increasing the minimal A-value of the succedent).

39 This theorem was proved together with Menachem Magidor.

But note that none of the weak structural rules involves removing formulae from the upper sequents. Moreover, recalling that the only formulae appearing in the proof which include A may be of the form $\forall x(A(x)\to A(x+1))$, A(t), or $A(t)\to A(t+1)$, it is easy to show that the only case where a strong structural rule may be applied in a way that removes a formula of the form A(t) from one of the upper sequents is left implication. No inference which involves a structural rule of a connective other than implication or the existential quantifier can contain an auxiliary formula containing A, or otherwise the inference will result in a sequent containing a formula that has A and that connective or the existential quantifier, which is not one of the allowed forms. Right-implication can never be applied to a formula containing A either, because if it were applied, the lower sequent would have a formula containing both A and $\to$ in its succedent. But since our end-sequent $S_k$ has no such formula in it succedent, we will need to apply some rule further down the proof which has a formula $\varphi$ in the succedent of one of its upper-sequents which contains both A and $\to$, and no such formula in the succedent of its lower-sequent. But the only rule of inference that would allow this is left implication, which would result in formula that has A and at least two occurrences of $\to$, which is not one of the allowed forms. Finally, applying the left-universal or right-universal rules where the auxiliary formula is of the form A(t), will result in a sequent containing the formula $\forall x(A(x))$, which is not an allowed form.

Thus the only application of a rule of inference which allows us to remove a formula of the form A(t) from the one of the upper sequents is left-implication. Furthermore, there is only one kind of case in which the rule can be applied in the required way:

$$\frac{\Gamma\to\Delta, A(t) \quad A(t+1), \Gamma\to\Delta}{A(t)\to A(t+1), \Gamma \to \Delta}$$

(This is so because the only legitimate formula of the form $\varphi\to\psi$, where $\varphi$ or $\psi$ contain A, is of the form $A(t)\to A(t+1)$).

Thus this particular inference is the only case we need to consider for the induction step. Assume, then, that we have an inference of this form. Let n be the minimal A-value of Δ, and k the maximal A-value of Γ. Now let m be such that k≤m≤n. Note that k is also the maximal A-value for the antecedent of the left-hand upper sequent (Γ→Δ, A(t)), and that the minimal A-value for the succedent of this sequent is min{n, val(t)}. So by the induction hypothesis on the left-hand upper sequent, we get that for any m≤n such that k≤m≤val(t), m is an A-value of the antecedent of some sequent appearing above S in the proof.

Next note that the maximum A-value of the antecedent of the right-hand upper sequent (A(t+1), Γ→Δ) is max{val(t)+1,k}, and that the minimum A-value of the succedent of this sequent is n. So by the induction hypothesis on the right-hand upper sequent, we get that for any m such that k≤m and val(t+1)≤m≤n, m is the A-value of the antecedent of some formula appearing above S.

Putting these two claims together we get that for any m such that k≤m≤n, m is the A-value of the formulas appearing above our sequent, and we are done proving the Lemma◆.

Now consider the occurrence in our proof of the end-sequent $S_k$: $\Gamma_0$, $A(t_1)$, ∀x(A(x)→A(x+1)) →$A(t_k)$. The maximal A-value of the antecedent of this sequent is 1, and the minimal A-value of the succedent is k. So by Lemma, for any 1≤m≤k, a formula $A(t_m)$ (where $t_m$ has m as its numerical value) appears in the antecedent of some sequent in our proof. Now since for any m>1, $A(t_m)$ does not appear in the antecedent of our end-sequent $S_k$, there must be some inference where $A(t_m)$ appears in the antecedent of one of the upper sequents, but not in the antecedent of the lower sequent. But as we have already seen, the only case where this can occur is in an application of left-implication, of the form:

$$\frac{\Gamma\to\Delta, A(t_{m-1}) \quad A(t_m), \Gamma\to\Delta}{A(t_{m-1})\to A(t_m), \Gamma \to \Delta}$$

Thus for each $1<m\leq k$, there is a strong inference (left-implication) corresponding to it, and the proof contains at least k-1 strong inferences. Moreover, since for every m, our end-sequent does not contain $A(t_{m-1})\rightarrow A(t_m)$ in its antecedent, and since (given our allowed forms) the only rule that will allow us to remove this formula from the antecedent is left universal, then we must apply at least one more strong inference (left universal), and thus P contains at least k strong inferences. (In fact, for each distinct occurrence of $A(t_{m-1})\rightarrow A(t_m)$, we will need a separate application of the left universal, so P contains at least 2(k-1) strong inferences – but this is a stronger lower bound than we need for our current purposes). QED.

## References


Boolos, G. (1984), ‘Don’t eliminate cut’, *Journal of Philosophical Logic* 13, pp. 373-378.

Boolos, G. (1991), ‘Zooming down the slippery slope’, *Nous* 25, pp. 695-706.

Buss, S. (1998a), ‘An introduction to proof theory’, in Buss, S. (ed.), *Handbook of Proof Theory*, Elsevier.

Buss, S. (1998b), ‘First-order proof theory of arithmetic’, in Buss, S. (ed.), *Handbook of Proof Theory*, Elsevier.

Dedekind, R. (1963), *Essays on the Theory of Numbers*, translated by Beman, W., Dover Publications.

Dummett, M. (1975), ‘Wang’s paradox’, *Synthese* 30, pp. 301-324.

Dummett, M. (1977), *Elements of Intuitionism*, Oxford University Press.

Fara, D. (2001), ‘Phenomenal Continua and the Sorites’, *Mind* 110, pp. 905-935. (Originally published under ‘Delia Graff’).

Frege, G. (1974), *The Foundations of Arithmetic*, translated by Austin, J.L., Oxford University Press.

Magidor, O. (2007), ‘Strict finitism refuted?’, *Proceedings of the Aristotelian Society* CVII, pp. 403-411.

Read, S., and Wright, C., (1985), ‘Hairier than Putnam thought’, *Analysis* 45, pp. 56-58.

Tennant, N. (1997), *The Taming of the True*, Oxford University Press

Wright, C. (1982), ‘Strict Finitism’, *Synthese* 51, pp. 203-282.