# STRICT FINITISM AS A FOUNDATION FOR MATHEMATICS

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## ABSTRACT

The principal focus of this research is a comprehensive defence of the theory of strict finitism as a foundation for mathematics. I have three broad aims in the thesis; firstly, to offer as complete and developed account of the theory of strict finitism as it has been described and discussed in the literature. I detail the commitments and claims of the theory, and discuss the best ways in which to present the theory. Secondly, I consider the main objections to strict finitism, in particular a number of claims that have been made to the effect that strict finitism is, as it stands, incoherent. Many of these claims I reject, but one, which focuses on the problematic notion of vagueness to which the strict finitist seems committed, I suggest, calls for some revision or further development of the strict finitist's position. The third part of this thesis is therefore concerned with such development, and I discuss various options for strict finitism, ranging from the development of a trivalent semantic, to a rejection of the commitment to vagueness in the first instance.

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# Contents

# INTRODUCTION

The aim of this research is to provide a proper account of strict finitism as a foundation for mathematics, and to provide a robust defence of the theory against numerous objections which have been thought to deflate the theory as a serious or tenable position. I shall also aim to present a model of strict finitism which, while it differs from the traditional account, will, I suggest, solve many of the problems associated with such an account.

## *An outline of the project*

Strict finitism has been discussed variously in the literature surrounding the philosophy of mathematics, but proponents of the theory are rare. For this reason, perhaps, there is no reliable, general account of the theory, and I hope to go some way towards offering such in the opening chapters of this thesis. In particular, the notion of surveyability, which seems central to the strict finitist's account, is used in a variety of contexts to mean anything from 'is possible to physically reproduce' to 'is recognised and understood in the mind of the surveyor'. I shall try to offer an account of surveyability which clearly marks the bounds of the criterion.

There are also many objections to the theory to be found in the literature, and a large focus of this thesis will be to address these issues. Not least, there have been several attempts to show that the theory is internally inconsistent, and I shall argue that no such thing is established.

Lastly, after a thorough consideration of the various options for the formulation of a strict finitary model, including a detailed examination of the plausibility of a trivalent semantics, I shall offer a form of finitism that I have labelled 'Fanatical Finitism', because, if anything, it takes the fundamental ideas of strict finitism *more* seriously than a traditional account, which I contend proves robust to the various remaining obstacles for the position as a whole. Fanatical finitism is, I suggest, the preferred model for the strict finitist – and one worthy of further study both in itself and for the potential consequences its failure might have on the finitist programme as a whole.

## *On the subject of numbers*

Although I shall offer some qualification in the opening chapter as to why I shall find it convenient to focus on the objects of mathematics – in particular on *numbers* themselves – rather than on the statements or proofs which must also ultimately be the concern of the philosopher advancing a coherent theory for the foundation of mathematics, I would also like to be clear from the start about the central concern of this work. Firstly, I should reiterate that numbers are of paramount importance and interest to me – my suspicion of the infinite, which will become apparent, is a suspicion that it is not a genuine *number* at all. Furthermore, in general the objects of study will be the natural numbers; again, this is largely for the sake of simplicity, and to ensure that the examples are intuitively accessible in what is, after all, for many people a very counter-intuitive model for the foundation of mathematics. However, where appropriate, I shall also include some discussion of the other species of number (in relation to finitism), and will try to offer enough remarks to indicate how what I suggest may be successfully applied in a wider context.

I would also like to make some preliminary remarks about what I take the term number to apply to, in an ordinary sense. (I do not mean here how far I think the term 'number' may be usefully applied 'up the number line', as it were; this will be the subject of a good deal of the work that follows. By 'ordinary' sense, I mean I should like to explain what I understand by the term '2 is a number', where 2 is perfectly acceptable as a number, finitistically speaking).

There is an apparent confusion between numbers and their physical representations, the symbols (or string of symbols) used to represent the numbers; and I have been surprised to learn that many philosophers think the distinction is not an important one. It seems to me that numbers are distinct from numerals in a very important sense. A numeral may be canvassed and *not understood*, and indeed this is precisely the case for all children before they come to understand that a particular representation *stands for* a particular number. A number, on the other hand, is a concept that one has a possession of, irrespective of the presence of a representation of it. Indeed, it is plausible that a child might know some of the very early numbers in a relational context (e.g. 'there are two apples over there') without knowing or recognising which particular numeral represents that number. Moreover, we needn't think that understanding of a particular notation (including its component symbols) is an understanding of every number in that notation – in fact, I think quite the opposite. When children learn to count, they may at first learn simply the sequence of words 'one', 'two', 'three', and so on. But soon, if not before, they will understand that these words refer to numbers, which they *understand*, and understand in the sense that they recognise that the number 'three' (for example) corresponds to a collection of three things. We do not teach numbers purely by the introduction of symbols, but by explaining that the number is exemplified in this or that particular case, and *then* that such a number is *represented* by this or that symbol. Two very young children might both mechanically be able to count to ten, but while one will recognise a collection of ten objects as exemplifying the number, the other may have no understanding of the concept and hence no ability to use the number appropriately. And this seems to hold true if they are both taught to write the symbol '10' for 'ten'.

I maintain then that understanding of a number is more than simple recognition of the numeral. Children usually learn the symbol for one hundred before they have sorted out all the numbers below it, and indeed when a child who has only really grasped the concepts of the first few numbers is asked how big numbers get, one hundred is a good candidate, or one thousand perhaps, or even a million. But no genuine understanding accompanies the assertion, even though, as I assert, a genuine understanding does accompany the numbers with which the child can already operate, and apply, like 'two', or 'five'. And this is not because they will recognise or can write down the symbols '2' and '5' but not '100' since in many cases they will be able to do both.

On the subject of numerals, I should also qualify my use of this term throughout my thesis. I have tried to be careful to say number when I mean a number in the sense outlined above, and numeral when I mean a representation of a number, a physical inscription; but I have not been so careful to distinguish between single-digit numerals (the symbols 0-9 in Arabic notation) and complex numerals (representations made up of a string or arrangement of single-digit numerals). The reason for this is that I think it is an important part of our recognition of numerals that we connect them to number concepts – and hence it is an important part of the way in which we understand complex numerals that they are comprised of 'simple' (single-digit) numerals. So while I might speak of the numeral '100', I might also suggest that the representation contain three numerals, since I take the meaning of numeral to mean simply a representation of a number.

*The Motivation behind a Strict Finitary account*

Strict finitism, as I shall describe in Chapter two, is fundamentally committed to an anti-realist position with respect to mathematics. That is to say that the numbers, statements and proofs of mathematics are mind-dependent. Indeed, it is a constructivist theory – it stems from the idea that mathematics is constructed by the mathematician, and hence numbers (for example) are only 'real' if they are constructible. This is a key motivation behind strict finitism – those committed to a realism about mathematics, a Platonist ontology regarding number, are unlikely to find the theory attractive. I personally find the constructivist account intuitively plausible – from the moment we begin to learn about numbers, we begin to construct them; the very first mathematical procedures that we learn are constructivist in nature: simple addition, for example – the notion that 2+2 *makes* 4.

Strict finitism is perhaps a natural extension of constructivist ideas. It takes its motivation in part from an objection that intuitionism, which is also a constructivist philosophy, does not take the tenets of constructivism seriously enough. It is, as I shall show in Chapter eleven, possible to insist that even Strict Finitism is not the natural 'rest-point' for constructivist constraints, but for the time being let us just point out that Strict Finitism involves a strengthening of the constructivist constraint in what is, to the strict finitist, a perfectly natural direction: it requires that the relevant construction is *actually* possible, given the actual constraints of human minds. If one takes the intuitionistic requirement that numbers must be potentially constructible in order to be admissible, it is hard to resist the thought that potential construction is only barely constructivist. Since the constructivist requirement is essentially that numbers must be constructible in minds, it seems odd to allow that nonetheless minds may (potentially at least) be as powerful as required in order to effect virtually any construction, short of the unconstructible (such as the infinite). Strict finitism maintains that if constructivism is going to be a plausible constraint, it must be based on the actual powers of construction of actual minds.

This, then, I take to be the starting point for this work. I shall look at various forms of finitism, including intuitionism, on the understanding that all a commitment to finitism involves is commitment to a rejection of the infinite in mathematics, but my principle interest in such investigation is in distinguishing strict finitism properly from these other forms, and I shall assume from the outset that there are good (constructivist) reasons for pursuing finitism 'further' than in the case of intuitionism.

I would like to close this introduction by simply mentioning that I think there is scope for a finitism of a more general kind, to which a finitist (particularly of the strict or fanatical variety) understanding of mathematics will be invaluable, and to which I hope this research will also be indirectly useful. Modern physics supports the notions that both space and time are finite, at least in extension, and recent work suggests that it is not implausible to assert that they are finitely divisible also. There are a great many paradoxes of the infinite which I think themselves warrant an investigation into the plausibility of a 'global' finitism, and a rejection of the problematic notion of infinity at both the physical and mathematical levels. Towards that endeavour, a vindication of a strict finitary foundation for mathematics, a task I hope is at least begun in this volume, would be a good place to start.

# PART ONE:
# A FISTFUL OF NUMBERS

## A definition and explanation of the philosophy of Strict Finitism

# CHAPTER I: AN INTRODUCTION TO FINITISM IN MATHEMATICS

The finitist program, conceived as broadly as possible, can be described as having a single goal – the rejection of our ordinary concept of infinity, and the successful interpretation of a coherent conceptual schema that operates without it. Whatever the prospects may be for a successful attempt at the 'physical level' – in terms of the extension and division of matter, space and time – it is clear that a complete rejection of the infinite will involve extensive revision of the way in which we commonly think about mathematics. Mathematicians (and physicists) certainly operate with a notion of infinity, and in a seemingly unproblematic manner. What are we to say about such practice? Are we to reject mathematics altogether because of its adherence to such notions? Surely such a conclusion would be unnecessarily drastic. In fact, some philosophers have tried to support a *finitary* foundation for mathematics – that is, a basis for mathematical practice that legitimizes that practice *up to a point*; and it is upon these theories that I wish to focus my attention. Clearly mathematical practice that involves use of the infinite – be it infinite quantities, limits, or operations – will have to be viewed with the utmost suspicion during such analysis; even if such theories may still be pressed to provide some account of precisely what is going on with practices involving recourse to infinite quantities, limits, etc. Equally, the claim that the infinite remains *useful* in scientific and mathematical endeavour perhaps deserves further examination.

I shall not here attempt to provide extensive responses on either of these counts however, as it is the intention of this study to focus upon a single kind of finitism, namely *strict finitism*, and explore the limits of this theory – often in contrast to other concepts of finitism, or even to broader theories about numbers as mind-dependent entities. The worries expressed immediately above are not, I suggest, problems peculiar to strict finitism, but to most theories of this sort, as I shall describe in more detail shortly. As to the precise distinction between strict finitism and the more general finitism I will also discuss, the principle aim of this chapter will be to describe the distinction in detail. But let us begin with a clear statement of difference, so that the intent may be followed in the upcoming discussion. Finitism, in the broadest sense, amounts to a rejection of *actual* or *completed* infinite. That is to say, a finitist will not allow talk of completed infinite sets, or of transfinite mathematics (operations involving the actual infinite as a relative or comparable quantity). Finitism may perhaps be best characterised by a denial of the actual, but not necessarily the potential infinite, following the Aristotelian distinction. Strict finitism, on the other hand, denies that there are *any* (potential *or* actual) infinite collections/sequences.

In this opening chapter, then, I shall begin with a proper account of the distinction between strict and 'classical' finitism. I shall offer a preliminary account of what is known as constructivism, and describe how both the strict and classical finitist interpret the notion of *constructibility*. Lastly, I shall offer some initial definitions, not least of which is a description of the extent to which the term 'strict finitism' should be properly applied to existing theories.

## *On the 'objects' of discussion*

Before I develop a proper account of these positions, it is prudent to establish precisely the issues of debate, and the focus of my own discussions. Consideration of mathematical practice can focus on different aspects of that practice, for example on the *objects* of study, on the *practices, rules* or *conventions* in use, and so on. I shall proceed with an investigation largely focused on mathematical objects – that is to say the numbers, sets, points, etc. – with which a mathematician operates. Moreover, what I am most interested in is an account of the *ontology* of number – what it is for a number to count *as* a number. The corresponding claim of any kind of finitism is of course that infinity can have no such status – there can be no infinite magnitudes, no *legitimate* numbers of infinite (or indeed multiply infinite) size.

Although number is certainly a central concern of finitism, the notion of proof has also been traditionally discussed. Finitists have suggested that only proofs of finite lengths, and involving only finite operations, should be accepted as legitimate. While I shall devote some time to discussion of proofs, I shall stay largely focused upon the debate concerning numbers. My justification for doing so is, I suggest, that in general, what goes for numbers, i.e. the requirements imposed upon that which is to count as a legitimate number, will go *a fortiori* for (what is to count as) legitimate proofs – since in the latter case, we are talking of either proofs with a certain number of steps, or else with a certain complexity, which in most cases will itself depend upon the size of numbers used in the proof.

As a result, much of my discussion will focus on questions about number – and the central claim of any kind of finitism with regard to numbers is that there are only finite numbers. Infinity is not a genuine (legitimate) mathematical object, and should not be given 'number-status', nor talked about as if it has such status. Now, what precisely is meant by 'there are only finitely many numbers' depends upon the species of finitism under consideration. There seems to be a broad usage of the term in some of the literature, not all of which will be useful here. Another way to describe the distinction I have already offered (between finitism and strict finitism), which emphasises the focus upon numbers which I wish to adopt, is that strict finitism asserts not only that there are only finite numbers, but also that there are only finitely many of them. The distinction may seem an odd one at first glance, but 'less-strict' finitists, such as the school of intuitionism, are happy with *potentially* infinite collections – that is, there is no *finite* end, as such, to the numbers on such accounts, but the numbers, such as they are, still never exceed finite values. Before I get too deeply into such questions, however, let me first establish what I take the philosophy of finitism to be, and furthermore how we are to precisely distinguish strict finitism.

### *Clearing ambiguity – the uses of the term 'finitism'*

Let us begin, then, with an examination of what has been broadly termed 'Finitism'. As I have suggested, it is important to differentiate between finitism in a broad sense, and the theory of strict finitism, but it is not immediately obvious what theories are to fall underneath the umbrella of finitism. Mary Tiles, for example, in *The Philosophy of*

*Set Theory*, describes only two distinct forms of finitism. The first of these is strict finitism, and the theory of strict finitism as it is traditionally advanced is outlined (if not exactly advocated) in the literature by (at least) Michael Dummett, Crispin Wright, and Paul Bernays, and defended perhaps in part by Wittgenstein and certainly by Aleksander Yesenin-Volpin. Strict finitism occasionally bears other names – ultra-finitism, for example, and sometimes even ultra-intuitionism.

The second form of finitism Tiles has called 'classical' finitism, and although she identifies neither philosophers nor specific schools of thought in her description, by her accompanying discussion it is clear that she has in mind at least the school of intuitionism, and perhaps also Hilbert's finitism.

The distinction between strict and classical is assuredly that I outlined in the previous section – where classical finitism asserts that there are only finite numbers, strict finitism insists that there are only finitely-many finite numbers. Strict finitism then is generally regarded as a more thorough-going version of 'classical' finitism. The view that classical finitism encompasses intuitionism and that strict finitism involves a strengthening of that thesis is supported by Robin Gandy, who writes:

> "I prefer this term [ultra-finitism] to Esenin Volpin's 'ultra-intuitionism' and Dummett's 'strict finitism'."[1]

From this it is clear that Gandy assumes the terms are synonymous with one another, and hence intuitionism is equivalent to, or at least encompassed by, what Tiles describes as (classical) finitism. In fact the term 'finitism' is often used in the literature to describe a commitment to there being only finite numbers; and as such the distinction that Tiles has in mind is sometimes reduced to a distinction between 'finitism' and 'strict finitism'. This is presumably due to Hilbert's finitist program, which he simply calls finitism, but which has more in common (ontologically) with the intuitionists than with

[1] Gandy, *Logic Colloquium '80*, in a footnote on p.145

the strict finitists. Such a distinction is often confused, however; Joan Moschovakis writes:

> "intuitionism differs . . . from finitism by allowing (constructive) reasoning about infinite collections"[2]

But here Moschovakis clearly has in mind some form of strict finitism when she employs the simpler term 'finitism', since there is nothing in Tiles' discussion of 'classical' finitism that precludes reasoning about infinite collections, at least to the extent that such collections are only potentially infinite; a constraint which at any rate is certainly adhered to by the intuitionists.

As a final note on this point, the idea that Hilbert's finitism should also be included under the broad heading of 'classical' finitism is supported by Jean-Paul Van Bendegem:

> "The additional qualification [of the label *strict* finitism] serves to make the distinction with Hilbert's finitism which, roughly speaking, can be seen as a form of finitism on the meta-level"[3]

Here, Van Bendegem suggests that the distinction between strict finitism and classical finitism is between strict finitism (which, to further complicate matters, he refers to often - as in this quote - simply as finitism; the distinction here is trivial) and David Hilbert's finitism. Although Van Bendegem does not mention the intuitionists, it is clear that the two philosophies (intuitionism and Hilbert's finitism) share at least the pertinent features ascribed to 'classical' finitism As a result, I shall find it convenient in the discussions that follow to distinguish between strict finitism and intiutionism, or between strict finitism and classical finitism whenever the debate has wider application[4]; and the

[2] Moschovakis, 'Intuitionistic Logic', *Stanford Online encyclopaedia*
[3] Van Bendegem, 'Finitism in Geometry', *Stanford Online encyclopaedia*
[4] But in much of what follows I shall really consider only intuitionism as the 'classical' counterpart to strict

broader term 'finitism' I shall take to apply to any theory which takes as a central theme the rejection of the infinite as legitimate practice.

So, now that the terminology of the debate is clearer, I would like to turn my attention to describing precisely the pertinent features I mention above, and indeed to the features in general belonging to both the strict and 'classical' finitist theses. Moreover, I shall begin to illustrate the resulting commitments of each.

*Strict finitism vs. intutionism: the differences and similarities*

The central feature of such theories, which is shared by all (past and) present finitist positions, is commitment to a view of mathematics called constructivism. Van Bendegem describes this:

> "Finitism is one of the foundational views of mathematics that is listed under the broader heading of constructivism. It shares with the many forms of constructivism the fundamental view that mathematical objects and concepts have to be accessible to the mathematician in terms of constructions that can be executed and performed. The various forms are distinguished from one another as to how 'execution' or 'performance' is to be understood."[5]

finitism, as the parallels and points of difference are perhaps easier to distinguish. Since the thesis is intended as a thorough investigation of *strict* finitism, the subtler divisions between forms of classical finitism, that is those competing theories that take as fundamental only that the numbers will never exceed finite values, need not concern us greatly. Intuitionism is, at any rate, the more contemporary theory, and the existing literature, when considering the distinctions, makes much more use of the intuitionist position.

[5] *Ibid.* In fact, here, Van Bendegem is again speaking of strict finitism when he says simply 'finitism' – this is due to the aforementioned tendency in the literature to distinguish between strict finitism and intuitionism, rather than strict and 'classical' finitism as Tiles does. Intuitionism is one of the better-known constructivist theories, and I am sure it is precisely the difference in approach between strict finitism and intuitionism that Van Bendegem has in mind when he describes a difference between the 'various forms' of constructivism.

The important requirement imposed by constructivism is then that any operation or object in mathematics is constructible by (and in) the mind. Expressed in such terms, it is not hard to understand why a constructivist is committed to the idea that mathematical objects are mind-dependent entities. Douglas Bridges describes this generally constructivist position in relation to intuitionism:

> "In Brouwer's philosophy, known as intuitionism, mathematics is a free creation of the human mind, and a [mathematical] object exists if and only if it can be (mentally) constructed."[6]

In the next chapter I shall return to the idea of mind-dependent objects in mathematics, as the idea is central to the traditional strict finitist thesis.

I would like to turn now to what I shall call the finitist internal debate, and, following Van Bendegem above, distinguish strict and 'classical' finitism from one another by looking at the different ways in which each interprets the constructivist constraint. As we have seen, both positions are committed to the idea that mathematical objects are mind-dependent mental constructions. Furthermore, both place limits on the scale over which these mathematical objects range – a scale that, (in accordance with the original binding premise of finitism that I have acknowledged), does not extend over infinite quantities/totalities. Where they differ is over the definition, and size, of this scale.

The limit is imposed in the following way. Both theories make the intuitively-appealing (and constructivism-friendly) claim that there is some limit to what can be constructed by a mind; as a consequence of this, and the accompanying assumption that mathematical objects are mind-dependent entities in the first place, it follows that there are limits to the mathematical objects that can be constructed, and hence limits to the

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[6] Bridges, 'Constructive Mathematics', *Stanford Online encyclopaedia*

mathematical objects. For classical finitism, the limit is a potential one, or a limit of possibility, whereas for strict finitism, the limit is an actual one.

Classical finitism, such as the intuitionism first espoused by Brouwer, holds that only those mathematical objects and proofs that can *in principle* be constructed, (or in principle *recognised*, perhaps), by a mind, are to be counted among the 'real' matter of mathematics. The notion of 'in principle' is not an unproblematic one, but it does seem, intuitively at least, to capture much of mathematical practice within its compass. The intent of the thesis is relatively clear. If a machine can construct or deal with a proof or a number, then so, in principle at least, could a sufficiently advanced human mind. Again, if advanced notation affords us intellectual tools that we would not otherwise possess and with which we are able to construct greater and greater numbers and proofs, we might imagine that with sufficient intellect, such operations would be possible without the 'artificial' contribution of the notation. Infinite quantities, sequences, etc., can never be constructed (even potentially), and hence, according to the intuitionists (and indeed finitists in general), do not belong to the domain of mathematics.

The idea is that as long as we can imagine a construction occurring, even if we cannot actually perform it ourselves, we can accept it. Intuitionism is not therefore limited by human mortality, or the constraints of human minds, etc. As long as we can imagine performing the construction if we lived significantly longer, or our brain power was significantly improved, the construction is admissible – it is only in the cases where the construction is in principle impossible, such as those constructions of infinite length where the construction cannot in principle be completed, that we should reject the purported number or proof altogether. The number (in exponential notation) $10^{10^{10}}$ is clearly beyond my actual powers of construction – if I tried to count to it from 1, for example, my life would end before I completed the construction. Moreover, long before that, I would probably 'lose count', and become confused by the operation. However, for the intuitionists, $10^{10^{10}}$ is a legitimate construction (and hence a legitimate number) – I can imagine that if my life were sufficiently long, my powers of attention sufficiently advanced, I just *would* eventually count to $10^{10^{10}}$. The sense of 'imagine' here is presumably similar to the example where I can imagine when I watch a child who is learning to count (to, say, 5), that when their ability and power of attention has sufficiently improved, they just *will* be able to count to 200.

Strict finitism picks up the problematic notion of 'in principle' possibility, and discards it, insisting instead that the only constructions that are to count in the relevant way are those that may be *in practice* constructed. Again, this is not an unproblematic notion, and indeed the focus of criticism for strict finitism centres around the idea of in practice possibility; but again, the idea has an intuitive attraction. The point, raised by the strict finitist, is that we can have no clear definition of what it is for something to be in principle possible, as the intuitionist requires. We do, however, have a clear idea of what it is for something to be in practice possible, and although we may doubt the sharpness of the definition, our understanding of the notion is at least apparent. Strict finitism insists that while the intuitionists are correct to assert that there are no constructions that are not in principle performable, they are wrong to 'stop' at in principle possibility. Rather, what is required is that I am *actually* able to construct the number or proof in practice, for it to count as legitimate mathematics.

Note that there are a number of ways we can interpret 'construct' in this context – we might simply require that one is able to reproduce, or write down, a number for it to count as having been constructed. Indeed, some of the literature focuses on predicates such as 'inscribable in Arabic notation' as the strict finitist requirement for number-hood. However, most serious strict finitist theories will impose a further limit, to avoid complications arising from, say, different notations. $10^{10^{10}}$ is perfectly (in practice) inscribable in exponential notation, but probably not in Arabic notation, and certainly not in stroke notation. Usually, strict finitism requires that one can *reliably* construct a number, which implies an intuitive understanding of the number. In this case, the above example of $10^{10^{10}}$ is presumably not a legitimate number, since I cannot reliably construct it. (Again, depending upon what we say about notations – I cannot reliably construct it in Arabic notation). Much more remains to be said on this matter, and I shall indeed do so in due course - the issue of different notations will receive extensive treatment in chapter four.

Strict finitism, unlike intuitionism, places a strict limit on the size of a number; moreover, it defines that limit in very human terms, according to the intellectual abilities of human minds. This requirement is often called surveyability – a number is surveyable if and only if the mathematician can reliably construct it in practice. The strength of reliability here is also a matter for some debate, and I shall return to the question shortly – but in general, as I outlined above, the strict finitist will require not only that the number may be physically constructed (i.e. written down in a human lifetime), but also somehow understood by the mathematician. A number in Arabic notation that has 658 numerals in it is presumably not surveyable, since even though somebody could relatively easily write one down, it is unlikely that anyone could have any real grasp of its magnitude (or, say, divide it by 3 and be certain they were without error).

Now, since the strict finitist requires that only the putative numbers which are to count as legitimate numbers are those which are surveyable, strict finitism is consequently committed to an upper limit on the actual size of *finite* numbers, something the intuitionist need not agree to. Furthermore, for the strict finitist, the scale of the *finite* numbers (and thus all numbers, from her point of view) will be much smaller than that adopted by the intuitionists, or moreover by traditional mathematics.

One might understand the distinction between the two theories in the way I described in the opening of this chapter. Intuitionism ('classical' finitism) holds the view that there are only finite numbers – strict finitism maintains the stronger view that there are only *finitely many* finite numbers. That is also to say that the intuitionist holds that the numbers are open ended, whereas the strict finitist thinks that they are closed. For the intuitionist, there is a potential infinity of finite numbers – but this, of course, does not imply an actual infinity. It's just that we won't ever run out. The strict finitist on the other hand, denies that there is (or can be) a potential infinite.

Constructivism, then, is a branch of anti-realism[7] that holds that not only are numbers mind-dependent, but that they are properly constructed within the mind. In this way, the finitist asserts that only those numbers (or indeed proofs) which it is *possible* to construct in the mind are to count as proper mathematical practice. Of course, construction must be construed in the relevant sense – it is not enough for you to think of the words 'one million' to have constructed the number; instead, the finitist will require that you could construct it from first principles, e.g. by counting. (Although this is by no means the only sense of construction at work. I shall also develop this theme considerably in Chapter three).

It is now quite a small step to see how the finitist rejects the infinite – since numbers are those mathematical abstract objects which may be constructed in the mind, and infinite constructions are impossible, no numbers will be of infinite magnitude. The same holds, under a constructivist account, for all the objects and statements of ordinary mathematics, and hence the constructivist finitist rejects all infinitistic elements of mathematics.

### *Strict Finitism*

This, then, is the fundamental idea of strict finitism: numbers are mind-dependent objects that must be surveyably constructible in practice if they are to count among the legitimate objects of mathematics.

The surveyability requirement may seem intuitively attractive, at least to those with anti-realist feelings about mathematics. If numbers are constructions within the mind, surely there can be no numbers that cannot, in practice, be intelligibly constructed

[7] Anti-realism about mathematics is broadly the view that mathematics (the objects, practices, or truth values of statements within mathematics) is mind-dependent. The next chapter will focus on the relationship between anti-realism and constructivism, and on a proper examination of what it is to be an anti-realist about mathematics.

(in, or by, the mind)? It would seem odd to suggest that we might construct objects in our mind which we did not fully understand – how then are we responsible for their creation, moreover for their ontology? Surveyability is a complex requirement, however, and one that deserves considerable attention. For now, let me close this chapter with a summary of Strict Finitism, and a brief look at the problems I shall be focusing on.

The tenets of strict finitism may be summarised as follows:

i) It is a constructivist theory. Numbers are mind-dependent constructs, and in this sense, strict finitism is an anti-realist thesis.
ii) Accordingly, the only 'numbers' (as traditionally conceived) we are to take as legitimate mathematical practice are those which may be constructed.
iii) Further to the intuitionist constraints (that numbers must be constructible in principle), the strict finitist requires that numbers must be constructible in practice.
iv) Therefore, while the intuitionists will allow all finite numbers (as classically understood), since any finite number is in principle constructible, the strict finitist will only admit a proportion of the finite numbers as classically understood. [8]
v) This leads to the (often problematic) notion of a 'cap', or limit to the numbers. What exactly determines this limit depends upon the precise formulation of 'possible to construct in practice' – on the simplest reading, strict finitism may be seen as asserting that a number is constructible in practice if and only if it can be written down in a suitable notation, e.g. 'inscribable in Arabic notation'. However, more sophisticated formulations will advance a 'surveyability' requirement of some kind, of which inscribability is the weakest requirement.

[8] Naturally, the strict finitist will not argue that she does *not* allow all finite numbers, or that she denies the existence of *any* finite numbers – but by the description 'all finite numbers' she will not mean the same as the platonist; and the platonist may think that she denies the existence of a great many. But for the strict finitist, the extension of the term 'finite number' is defined by the actual possibility of construction, and hence the set of finite numbers it is actually possible to construct will constitute *all* the finite numbers.

Typically, surveyability will require some measure of intuitive grasp of the number constructed.

In the following three chapters, I shall focus on a proper explanation of the commitments and claims of strict finitism, as briefly outlined here. However, for all that I shall say here, it must be admitted that strict finitism is not a popular theory in the search for a foundation of mathematics. There are a number of objections frequently raised, and in the later chapters of this thesis I shall attempt to meet the charges on behalf of strict finitism. The claim that strict finitism is committed to a *vague* totality of numbers, and that such totalities are intrinsically incoherent, is the most prominent charge laid against the theory – and I will consider the major contributors to this debate carefully. There are one or two other objections in the literature, which I shall also examine, but I shall not elsewhere address what is perhaps the most obvious initial objection; so I shall spare a few words to discuss it here.

The charge is that of simple implausibility; this is the intuitive objection that strict finitism leads to an implausible notion of a cap on the numbers – a limit, somewhere on what we think of as the ordinary number line, below which we are supposed to accept that the numbers are legitimate, and above which they are not. I do not think this is as problematic as it first appears, for two reasons. Firstly, it only seems problematic from the standpoint of a *platonistic* conception of number[9], and only if we forget that the strict finitist criterion for admissibility is based on a *mind-dependent* conception of numbers, and a constructivist one at that. It does not seem odd to us, by comparison, that emotions can only last for a finite amount of time, dependent upon the constraints of the mind. Mourners get over their grief in time, broken hearts mend. The suggestion of a cap on emotion duration is not implausible, because we already think of emotions as mind-dependent entities. Why then, if numbers were accepted as mind-dependent entities, should constraints or limits on their properties seem intuitively implausible? Simply, I

[9] I shall explain the Platonist position in more detail next chapter – briefly, it is a theory that takes an opposite stance to that of anti-realism, to the extent that numbers are mind-*in*dependent objects; objects *in the world* in some sense. It is also called realism about mathematics, and is the traditional position on ontology, at least, both in the philosophy of mathematics and in the practices of mathematicians themselves.

think, because we traditionally think about numbers as mind-independent entities, without proper qualification. It is no small coincidence that mathematical practice operates *as if* numbers are mind-independent numbers, without asking for proper justification. Hence, we learn to think about numbers as individual, mind-independent entities as we learn about mathematics – and such thinking becomes entrenched in our intuitive responses.

A second reason that the cap on the number line may seem implausible is that we tend to think about the number line in relatively small finite terms – the suggestion that there is a cap seems absurd because we intuitively place the cap somewhere, arbitrarily certainly, but where we can still conceive of legitimate numbers above. But the strict finitist will not insist that the limit lies where we may obviously conceive of numbers above and below, like 50, or 9999. Instead, the strict finitist suggests that the cap lies precisely where our ordinary intuitive grasp of numbers runs out – where we can no longer clearly think about *numbers* beyond.

## *Difficulties of the Dialectic*

A related issue arises for strict finitism, in that this traditional conception of numbers as a mind-independent, completed totality, leads to certain semantic assumptions regarding the members of that totality – assumptions which the finitist will wish to resist. It is, for example, convenient in the discussion to speak of 'numbers' below the cap, and 'numbers' above, since traditionally all of these entities are numbers. Strictly speaking (no pun intended), the finitist ought to speak only of 'putative constructions' above the cap, and numbers below. When, in the examples given earlier, I spoke of the 'number' $10^{10^{10}}$, I ought more cautiously to speak of a putative number construction, or some such, at least until it's status is assured on the relevant (in this case, strict-) finitist grounds.

As a result, the strict finitist is often faced with the unpromising task of rejecting some 'numbers' as *being* numbers, which to many may seem absurd; but all this really highlights is that the anti-realist, constructivist position has not enjoyed traditional success – and not that it is, in itself, necessarily implausible. Certainly, finitism will require not only a revision to mathematical practice[10], but also to the semantics, since what we *call* numbers will not always be numbers on a (strict) finitary account.

There remains a question as to what the strict finitist will say about those traditionally conceived 'numbers' which lie without the proper boundaries of number on a strict finitary model. There are perhaps two obvious options here – firstly, to maintain that nothing is referred to in such cases, such that any talk of these entities is meaningless; secondly, perhaps more plausibly, that reference to such entities involves the use of empty terms[11]. This latter response is to commit the strict finitist to some form of fictionalism with respect to mathematical entities that are not encompassed by the proper definition of number, so that, for example, '10 to the power of 10 to the power of 10' will be an empty term, and the entity to which it refers a fictional entity at best.

The semantic problem remains a difficult one for anyone attempting to outline the strict finitist program within the traditional paradigm. I shall, therefore, in what follows, try to be precise, and speak of 'putative constructions', or perhaps 'putative numbers', for cases which have not been admitted as legitimate numbers; but sometimes, if only heuristically, it will be convenient to speak of 'numbers' in a looser sense. I have experimented with various conventions, such as using a capital N for Numbers in the strict finitist sense, and lower-case n for numbers in the traditional sense, but the result is I fear more confusing and not less; it is I hope sufficient to remind the reader that in sometimes speaking generally of 'numbers', (above the cap, for example), I in no way intend to commit the strict finitist to there being putative constructions with genuine number status *above* the cap, since this would be simply and obviously contradictory.

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[10] Indeed, it should be observed that some forms of finitism will be very revisionist with regard to mathematical practice, while other forms will be less so. Nonetheless, it is clear that any form of finitism which is serious about the ontological commitment (to a finite set of numbers) will result in a revision of the mathematicians proper domain. Traditionally, I suspect, the less revisionist the particular finitist position, the more plausible it has been seen to be.

[11] Although this is not to say, as in the previous case, that such terms are meaningless, per se; just that there is no 'real' entity to which the term refers.

# CHAPTER II: ANTI-REALISM AND CONSTRUCTIVISM

In the previous chapter, I suggested that constructivism is a branch of anti-realism, and that the (strict) finitist will be committed to both. I would like, in the first part of this chapter, to explore the relationship between the two, and outline the extent of that commitment. I shall describe the debate between the anti-realist and defenders of the opposing position of realism about mathematics, so that the commitments of strict finitism to anti-realism may be made explicit. I will examine the difference between being an anti-realist (or realist) about the objects of mathematics, and being an anti-realist about the truth of mathematical statements. I consider the various possible combinations of these views, and demonstrate that a (constructivist) strict finitist that is committed to an anti-realism in one sense must be committed to an anti-realism in the other.

In the second part of this chapter, I shall turn my attention to the work of Wittgenstein, and discuss the idea that he attempted to develop a non-revisionary form of strict finitism; since it seems as though any thoroughgoing constructivism (as a narrower form of anti-realism) is *likely* to be drastically revisionary. Here I shall also introduce the notion of surveyability, and distinguish between what I call the 'weak' and 'strong' claims of strict finitism. The 'weak' claim at least is supported by Wittgenstein, the 'strong' claim is that more usually advanced by the strict finitist as an (assuredly revisionist) theory on the foundation of mathematics.

## Part I – The realism/anti-realism debates

As I hinted at earlier, finitism, in its various forms, seems to be committed to some form of anti-realism with respect to the operations and subject matter of mathematics. But just what does this commitment amount to? Moreover, what is it for any philosophical theory regarding the foundations of mathematics to be committed to a realist or anti-realist position? Stewart Shapiro, in *Thinking about Mathematics*, describes two approaches toward a realist/anti-realist distinction with respect to a general foundation of mathematics – one motivated by a consideration of the nature of the objects of mathematical study (numbers, points, lines, planes, etc.), and the other arising from a shift in focus from the objects to the *objectivity* of mathematical statements and their truth values; a shift attributed (by Shapiro and Dummett at least) to the influence of Georg Kreisel.

*The Ontological issue*

The first of these realist/anti-realist debates is a question of ontology. *Realism in ontology* with respect to the objects of mathematical study describes a commitment to what is often called platonism in mathematics. The idea behind realism in ontology is that mathematical objects are mind-independent; which is to say that they exist independently - over and above, as it were - the efforts of mathematicians. The alternative view, *anti-realism in ontology*, has more in common with the idealist schools of philosophy, and takes mathematical objects as solely mind-dependent entities. As Shapiro points out, there is at least one serious worry for each position; in fact, the nature of the debate sets these problems against one another, so that in adopting either realism or anti-realism one opens oneself to the challenge of the other. This situation is described by Paul Benacerraf in 'Mathematical Truth' (1973), and the problem has been subsequently described as Benacerraf's dilemma. The point of this dilemma, in the current context, may be taken to be as follows. If we are realists about mathematics, we are committed to an external 'domain' of mathematical objects, which is furthermore usually taken to be eternal, acausal and independent of space-time. If this is the case, there is an explanatory gap to be bridged regarding how we come to know anything about, or interact with, this external domain.

On the other horn, if we are anti-realists, while we may easily explain away this problem, since the subject matter of mathematics is mind-dependent and therefore mental interaction with it is unproblematic, we are left with the seemingly (similarly intractable) problem that the account of what it is for mathematical statements to be true will now not be consistent with our account of truth for statements in ordinary language. That is, on a platonist (realist) account, a statement such as "7 is bigger than 4" has the same 'logicogrammatical' form as a statement like "Jupiter is bigger than Mars", since 'Jupiter' and '7' are both mind-independent objects. But, as Benacerraf contends, *any* theory that manages to offer an account that answers the epistemological worry (including, in the present context, anti-realism), is incapable of issuing in:

"a homogeneous semantical theory in which the semantics for the statements of mathematics parallel the semantics for the rest of the language"[1]

Essentially, for Benacerraf, there is a tension between the epistemological problem of explaining how we come to know any mathematical propositions, and the semantic problem of offering an account of the truth conditions of mathematical statements which treats them in essentially the same way as other statements of ordinary language. A theory (like realism) which is able to account for the semantic issue seems incapable of solving the epistemological concern[2], while theories (like anti-realism) which address the epistemological worry are unable to offer a uniform semantics that covers both mathematical statements and ordinary (or at the very least scientific) statements.

There are no easy answers, then; but it seems as though any foundation for mathematics must face one or other of the horns of this dilemma at some point along the line. My intent here is simply to establish that any enquiry into ontology must face an explanatory gap, and that it will present no special problem for finitism; also, perhaps, to reinforce the idea that I introduced in the last chapter that we should not simply accept the traditional Platonist account because it is the more familiar – commitment to a realism in ontology is not without problems of its own.

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[1] Benacerraf, 'Mathematical Truth', p. 661

[2] Although, it is perhaps only fair to say that there has been more effort to resolve the dilemma on the realist's behalf than on the anti-realists. One approach to the dilemma from the realist's position is to suggest that numbers are mind-independent *physical* objects. Such a solution, if convincing, would avoid the dilemma, since physical objects are eminently 'knowable', and the epistemological worry is mitigated.

The second realist/anti-realist distinction addressed by Shapiro arises from a debate regarding the truth of mathematical statements, and turns on the question of whether mathematical statements are *objectively* true or false. ***Realism in truth value*** describes a commitment to the idea that all mathematical statements are independently and objectively true or false. This position allows for the possibility that there are unknowable truths in mathematics, since there may be mathematical statements which are not (even in principle) within our epistemic reach to determine conclusively, but conversely it rules out the possibility that there are indeterminately true or false statements, since even such statements will necessarily *be* either true or false. On the other hand, ***anti-realism in truth value*** is a position that maintains that there are no objective truths or falsehoods in mathematics, which in turn entails that there are no in principle unknowable truths. Anti-realism in truth value allows for the *possibility* of indeterminate statements, but note that for many anti-realists in truth value (Dummett, for example), this does not amount to a claim that there *exist* certain statements with indeterminate truth values. Instead, the countenancing of this possibility amounts to no more than a refusal to assert that there are not - that is, a refusal to assert that every statement *is* determinately either true or false. The rejection is precisely that of the law of excluded middle: that every statement is necessarily either true or false. But the *rejection* of the law of excluded middle does not entail the existence of statements which are themselves *actually* neither true nor false.

*The commitments of finitism*

Traditionally then, finitism as a school of thought - although divided on some central concerns as we have seen - is generally committed to at least an anti-realism in ontology. As Shapiro points out, a commitment to an anti-realism in ontology at least suggests, if not exactly requires, commitment to an anti-realism in truth value. Perhaps this is a little quick (and indeed Shapiro qualifies the statement with reference to positions that attempt to reconcile either form of realism with a corresponding anti-realism regarding the other), but it seems as though for finitism in general, commitment to an anti-realism in ontology goes hand in hand with anti-realism in truth value.

We shall see why this must be the case presently. In fact, many finitists take the ontological assumption (of the mind-dependence of mathematical objects) as fundamental. Brouwer states the case most clearly for the intuitionists:

> "Mathematics rigorously treated from this point of view, including deducing theorems exclusively by means of introspective construction, is called intuitionistic mathematics. In many respects it deviates from classical mathematics. In the first place because classical mathematics uses logic to generate theorems, believes in the existence of unknown truths, and in particular applies the principle of the excluded third expressing that every mathematical assertion ... either is a truth or cannot be a truth. In the second place because classical mathematics confines itself to predeterminate infinite sequences for which from the beginning the nth element is fixed for each n."[3]

We can pick out of this a commitment to both anti-realism in ontology, from the last sentence, and anti-realism in truth value, from the third. The commitment to constructivism is fundamental for the finitist – 'introspective construction' determines the limits of mathematical practice. Since such construction will never lead us to construct completed infinities, ("predetermined infinite sequences", in Brouwer's words), such sequences are inadmissible on a finitist account. Constructivism is incompatible with a realism in ontology because this realism seems inescapably to imply a completed infinity of (objective, mind-independent) numbers. That is to say, if one allows that the realm of mathematical discourse contains reference to mind-independent objects, then it is hard to escape the conclusion that, independent of our recognising each object, the entirety of objects is already and objectively available for discussion. (Rather like when one refers to

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[3] Brouwer, "Consciousness, philosophy and mathematics" p.90

the children in a school – one is not referring to the children one knows in a school, or has epistemic access to, but rather to the predetermined totality of children in the school, regardless of whether we even know how many children there are.)

Furthermore, perhaps centrally for the finitists, there is a problem regarding how the size of any *objective* collection (whether finite or infinite)[4] could be determined or even constrained by the subjective limits of the mind. Since finitism draws its motivation from a claim about what is constructible in the mind, it would seem very odd to insist that such criteria could have any influence at all on a domain of mind-*in*dependent objects. It should also be observed that a variety of similar worries accompany such an idea; finitism, particularly strict finitism, faces the problem of vagueness when delineating a limit of constructibility – and while we may be perhaps uncomfortable about this notion with regard to mind-dependent entities and their properties, we are likely to find even less plausible the idea that there exists an inherent vagueness in a collection of mind-*in*dependent objects.[5]

*Distinguishing the issues - compatibility*

Now let us return to the claim I made at the outset of the previous section, that for finitism in general, commitment to an anti-realism in ontology goes hand in hand with anti-realism in truth value. To see why, let us consider the compatibility of these varieties of realism and anti-realism. Is it really possible to hold anti-realism in ontology consistently with realism in truth value? Or realism in ontology with a corresponding anti-realism in truth value? And what are the consequences for finitist positions? To answer such questions, we must first look at the motivation behind such assertions.

[4] By 'objective collection', I simply mean a collection which exists 'in the world', and independent of mind – any collection about which we shall be realists in ontology. 'Stones on the beach', for example, or 'people in the room'.

[5] It should be acknowledged, however, that there are those who posit mind-independent worldly vagueness – that is, they maintain that vagueness is a property of things *in the world*. In fact, Michael Tye, who's position on vagueness we shall look at in detail in Chapter 10, suggests something along these lines – he asserts there are vague objects, like mountains; which are clearly mind-independent.

One might attempt to motivate, on behalf of the platonists, a realism in ontology without a corresponding commitment to a position on the truth value distinction. To be fair to the platonists, this is not their usual route to such a position; most of them are thoroughgoing realists in truth value as well. But the question as to the plausibility of such a position remains an interesting one. One such position suggests that the objects of mathematical discourse need not exist prior to our speaking about them, in that there need not be a domain of objectively true or false mathematical statements prior to our discovery or assertion of them. However, in our assertion of statements involving mathematical objects, such objects come about, or perhaps more correctly become relevant. This is then to say very little about realism or anti-realism in truth value, but asserts something approaching the platonist realism in ontology.

As a result, it seems as though - at least in principle - one can follow a consistent route through anti-realism in truth value to realism in ontology. But is this really the case? The position suggested here implies a much weaker ontology than that suggested by the platonists in (as is usual) adhering to a realism in ontology. In the sense that the objects of study are mind-independent, the suggestion is in accordance with the doctrine. But the mind-independent objects as so defined seem to lack a certain permanence and reality which the platonists would presumably like to hold onto. For the platonist, a realism in ontology usually entails a commitment to a collection of mind-independent objects which is not subject to change, with determinate properties (even perhaps pre-*determined* properties), and the corresponding realism in truth value that statements about them are going to derive meaning from. A common understanding of the commitment of the platonists is one in which the truth (and hence the meaning) of mathematical statements for the platonists consists in reference to distinct (and pre-determined) properties of the objects referred to.

So, is it really plausible to suggest that one may be a platonist with respect to ontology (that is, to hold a realism in ontology) whilst remaining agnostic with respect to truth value (or indeed committed to an anti-realism in truth value)? If we are charitable to such a position, we may say that the debate is still up in the air. Certainly, as Shapiro describes, there are philosophers working on a theory of mathematics that supports realism in ontology with anti-realism in truth value. Such philosophies however, and to bring the discussion back into the context of this research, are unlikely to be finitary. Any *finitist* position – to the extent that such a philosophy turns upon a claim of constructibility within the mind – that attempts to support a realism in ontology is quickly going to come up against the intractable problems outlined previously. Moreover, although there have been attempts to entertain the possibility, it is often more as an exercise to demonstrate the independence of the position adopted by the anti-realist in truth value from any question of ontology, and not as a serious attempt to advance a finitist mathematics on inter-compatible grounds between anti-realism in truth value and realism in ontology.

What is of greater significance to the current debate is the question over whether or not an anti-realism in ontology, (as a general and often fundamental commitment of finitism), is compatible with a realism in truth value. We have seen that the finitist will, in making a claim about the constructive powers of the mind, wish to adhere to an anti-realism in ontology; which is to say that the objects of mathematics are mind-dependent entities. From such a position then, is it possible to move to a realism in truth value? It seems to me that such a move would require that the mind-dependent objects of study contain "hidden" and objective properties. The only way that mind-dependent objects could give rise to objective truth-values would be for them to possess certain properties over and above those that the creating mind assigns to them – properties that they cannot fail to possess, or else properties that they were destined to have upon creation, such that later statements about them were always to come out determinately and objectively true or false. But then our notion of a mind-dependent object is in crisis – how may such independent properties come into being, independently (as it were) of their creator? As long as such objects are mind-dependent in the ordinary sense, their properties remain knowable and indeed assignable by the mind in which they are created. We can further see this point by revisiting an earlier extrapolation, in that realism in truth value allows for the possibility of unknowable truths. Here again, surely no unknowable truths are possible regarding mind-dependent objects? (Note that the requirement is not that the truth be unknown, but that it is unknowable. There would need to exist, under such an interpretation, something in the nature of the object, itself a mind-dependent entity, that is unknowable to the mind in which it is created. It is hard to see how such an interpretation could be coherently advanced.)

It becomes clear that for the finitist, there is no common ground to be had between a realism in truth value and an anti-realism in ontology. The two views cannot be held consistently together. There is no alternate route to the position by taking a realism in truth value as fundamental, and moving to an anti-realism in ontology; for in such a case, in virtue of what precisely could statements about these mind-dependent objects be objectively (and pre-determinately) true? Not by reference to the objects themselves, since that would require the objects to have existed prior to construction, which is contrary to the central thesis of the anti-realist in ontology.

In fact the case is stronger still, in that it begins to look as though one cannot hold realism in truth value consistently with anti-realism in ontology given any philosophical position on the foundations of mathematics, at least in accordance with the definitions laid out here. Shapiro, in his aforementioned qualification, outlines a programme by Charles Chihara and Geoffrey Hellman which aims to combine a thoroughgoing anti-realism in ontology with a realism in truth value. But here the notion of anti-realism in ontology differs importantly from the one with which we are presently operating – Chihara and Hellman are operating with a kind of nominalism, which asserts that mathematical objects do not exist at all. The notion of anti-realism in ontology that we are dealing with, (and importantly that finitism is generally committed to), is that of the objects of mathematics as mind-dependent entities. They exist, certainly, but as mind-dependent constructs, and not independent of the mind of the constructors. To this extent, the programme of Chihara and Hellman, whose interpretation is different, is largely irrelevant to the present debate.

Our interim conclusion is thus that an anti-realism in ontology, most often adopted as fundamental by finitists for reasons we have explored, must also commit the finitist to an anti-realism in truth value. While there may still be a debate for the platonists about the compatibility of anti-realism in truth value with realism in ontology, the finitist remains a thoroughgoing anti-realist with regard to mathematical practice.

## Part II - Constructivism and Anti-Realism

We have seen that anti-realism in truth value follows from anti-realism in ontology for the finitist. It will be convenient therefore to talk simply of a commitment to *anti-realism*, by which I mean to imply a commitment to both. It is now of interest to determine the relationship between Anti-realism and Constructivism. It seems, as I have suggested, that constructivism is a branch of anti-realism, and commitment to constructivism necessarily involves commitment to an anti-realism (in ontology and about truth value). However, I have also intimated that the strict finitist's constructivist requirement may be described in terms of surveyability, and hence it ought to be the case that commitment to surveyability entails a commitment to anti-realism. Moreover, all such theories are going to be revisionist about mathematics – that is, they are going to reject some amount of traditionally accepted mathematical practice (and 'numbers'). However, there is one notable exception in the literature that suggests further investigation is required: that of Wittgenstein.

### *Wittgenstein's non-revisionary strict finitism?*

Let me begin with the assertion, which I shall fully explore next chapter, that constructivism entails a limit of some kind. The limit is imprecise – indeed, it is perhaps hard to establish that any such limit exists for intuitionism; as, in one sense, we may always 'help ourselves' to more numbers; construction in principle is perhaps limited only in one sense. However, as far as strict finitism is concerned, it is easier to see that construction in practice is limited. Even seen simply as a physical limit to the size of inscriptions, say, or a temporal limit on the life and inscribing-speed of any inscriber, it should be plain that there are (still finite) 'outer-reaches'. Furthermore, it is clear that commitment to a limit of constructibility (or surveyability) when combined with an anti-realism in ontology leads (for the strict finitist at least) to a finite cap on the numbers *that there are*. Numbers are mind-dependent objects, the mind-dependent objects must be constructed, and there is therefore a limit to the numbers which may be constructed in practice. I shall return to this in detail shortly, but for now let me simply call this the 'strong' finitist claim.[6]

Wittgenstein, however, appears not to be committed to an anti-realism in ontology, and yet his programme has been called strict finitistic; indeed, I shall, in the next chapter, use Wittgenstein's formulation of 'surveyability' as a foundation for establishing precisely what is intended by the criterion. But how can this be? The answer is simply that although Wittgenstein is indeed offering surveyability requirements which will be of tremendous use to the constructivist, Wittgenstein himself is making no commitment to constructivism – instead, he is making what I shall call the 'weak' finitist claim. Let me proceed with an examination of Wittgenstein's commitments.

It seems *prima facie* strange that Wittgenstein should be supporting a strict finitist thesis, given that strict finitism is one of the most revisionary positions on the foundations of mathematics. This seems to directly contrast with Wittgenstein's explicit desire to maintain a non-revisionary approach. For Wittgenstein, it is not the role of philosophy to set out the parameters of mathematical practice, but instead to interpret and describe such practice in an informative way. However, Crispin Wright, in *Wittgenstein on the Foundations of Mathematics* describes in detail grounds upon which Wittgenstein might be interpreted as advancing the idea of surveyability without the corresponding strict finitist commitment to ontology. Obviously, if this can be shown conclusively, Wittgenstein may avoid the revisionist charge. Wright summarises this at the end of a chapter on surveyability:

[6] I am deliberately distinguishing this from the *strict* finitist claim, although I shall make some remarks in the next chapter on classical finitism and the imposition of limits, after which it should be evident why it may be more correct to formulate in these terms the 'strong' strict finitist claim, and subsequently (as will become apparent) the 'weak' strict finitist claim.

"the stress on surveyability may be viewed as issuing not from a background anti-realism of strict finitist type but from rejection of the objectivity of internal relations ... Nowhere does Wittgenstein envisage that his idea might have a bearing on what deserves acceptance as sound mathematics."[7]

Now, the issue regarding whether Wittgenstein escapes the revisionist charge, while a difficult question, need not really concern us here. Without diving into the complexities of Wright's extensive discussion on Wittgenstein's possible non-revisionary formulation[8], it is sufficient to note that if this interpretation of Wittgenstein's *surveyability requirements* is coherent, then it seems as though it is at least possible to assert the limit of mental construction with regard to mathematical objects/statements without assuming a commitment to anti-realism in ontology.[9]

The important assertion is that Wittgenstein may be construed as making a weaker claim than that of the usual strict finitist thesis, to the extent that he is not necessarily concerning himself with the ontological debate (nor with the debate about the objectivity of mathematical statements). Instead, he can be understood as making the claim that there is simply a limit to what the mind is capable of dealing with, mathematically speaking. Let us call this then the 'weak' finitist claim: notice that it is similar to the original

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[7] Wright, *Wittgenstein on the Foundations of Mathematics*, p.139

[8] This is one of the larger projects undertaken by Wright throughout the course of *Wittgenstein on the Foundations of Mathematics*. The idea turns upon Wittgenstein's rule-following criteria and his understanding of the nature of 'proof'; as may be indicated by the phrase 'rejection of the objectivity of internal relations', it is another question to ask whether Wittgenstein is committed to an anti-realism in truth value. This may have considerable bearing on the type of debate outlined above, as when Dummett asserts the possibility of an anti-realism in truth value with either position on ontology. To some extent, Wittgenstein might be read as attempting the same thing.

[9] There are other ways to interpret Wittgenstein of course. Dummett writes: "Wittgenstein's main reason for denying the objectivity of mathematical truth is his denial of the objectivity of *proof* in mathematics, his idea that a proof does not *compel* acceptance; and what fits this conception is obviously the picture of our constructing mathematics as we go along." ('Wittgenstein's Philosophy of Mathematics')This last sentence implies that Wittgenstein is committed to an anti-realism in ontology. For our purposes, it is not so important to decide what Wittgenstein actually meant, but rather if a certain way of interpreting him is coherent. It is my belief that the approach by Wright alluded to here will coherently lead us to the conclusion that a claim about constructibility need not (and does not) entail a commitment to anti-realism in ontology.

intuitive (and still broadly constructivist) claim of the finitists, who then advance the idea to tell us something about the ontology (and indeed objectivity) of mathematics. What is lacking from Wittgenstein is any commitment to anti-realism in his constructivism – he can be seen as making a claim about numbers (or proofs) such that they *can* be constructed, (and not that they must); but moreover, (and this is the intent and limit of his constructivism) the only ones which are going to be fully intelligible to us are those that *actually* can be constructed.

Wittgenstein's assertion is thus that we should only allow surveyable proofs as 'fully-intelligible' – but this is more from the perspective of our own reliability to understand than it is to say anything about the intrinsic reliability of mathematical practice. Where we cannot construct (or 'survey'), we must be cautious in our dealing with mathematics – any unconstructible proofs cannot count as genuine proofs, since it is something about a *proof* as such that requires we are not mistaken in it. Where we cannot be certain we are not mistaken, we should refrain from holding that any mathematical 'proof-pattern' is a proof – since it is not as reliable as that. But note that this is not to say that the proof is not right; just that we should be wary of asserting its correctness. He writes:

"Finitism and behaviourism are quite similar trends. Both say, but surely, all we have here is. . . . Both deny the existence of something, both with a view to escaping from a confusion.

What I am doing is, not to show that calculations are wrong, but to subject the *interest* of calculations to a test. I test e.g. the justification for still using the word . . . here. Or really, I keep on urging such an investigation. I shew that there is such an investigation and what there is to investigate there. Thus I must say, not: "We must not express ourselves like this", or "That is absurd", or "That is uninteresting", but: "Test the justification of this expression in this way". You cannot survey the justification of an expression unless you survey its employment; which you cannot do by looking at some facet of its employment, say a picture attaching to it." (Witt. App.II, 18)

As Wright concludes on the matter, Wittgenstein's notion of constructibility, described as surveyability, is not *in itself* revisionary. As Wright describes:

"... the stress on surveyability ... is not obviously *of itself* revisionary; where, after all, in classical mathematics does anyone ever have recourse to an *unsurveyable* proof? Nowhere does Wittgenstein envisage that his idea might have a bearing on what deserves acceptance as sound mathematics. He talks of an intention to alter not the practice of mathematics but our conception of the significance of certain mathematical results."[10]

Understood in these terms, the weak finitist claim seems, as I have said, somewhat intuitively appealing. Certainly, there are mathematical operations which lie beyond the scope of the human mind to perform. (Even perhaps potentially - the classical finitist will presumably want at least those calculations involving infinite steps to fall into this category). The problem, and the crux of the present debate, is that such a claim, construed in these weak terms, says nothing about the ontology of mathematical objects (or even the objectivity of mathematical statements). It is simply an epistemological claim, concerned with the limitations of the human mind. It is therefore a claim perfectly consistent with any position on the realist/anti-realist divide with regard to mathematics.

### *The 'weak' and 'strong' finitist claims*

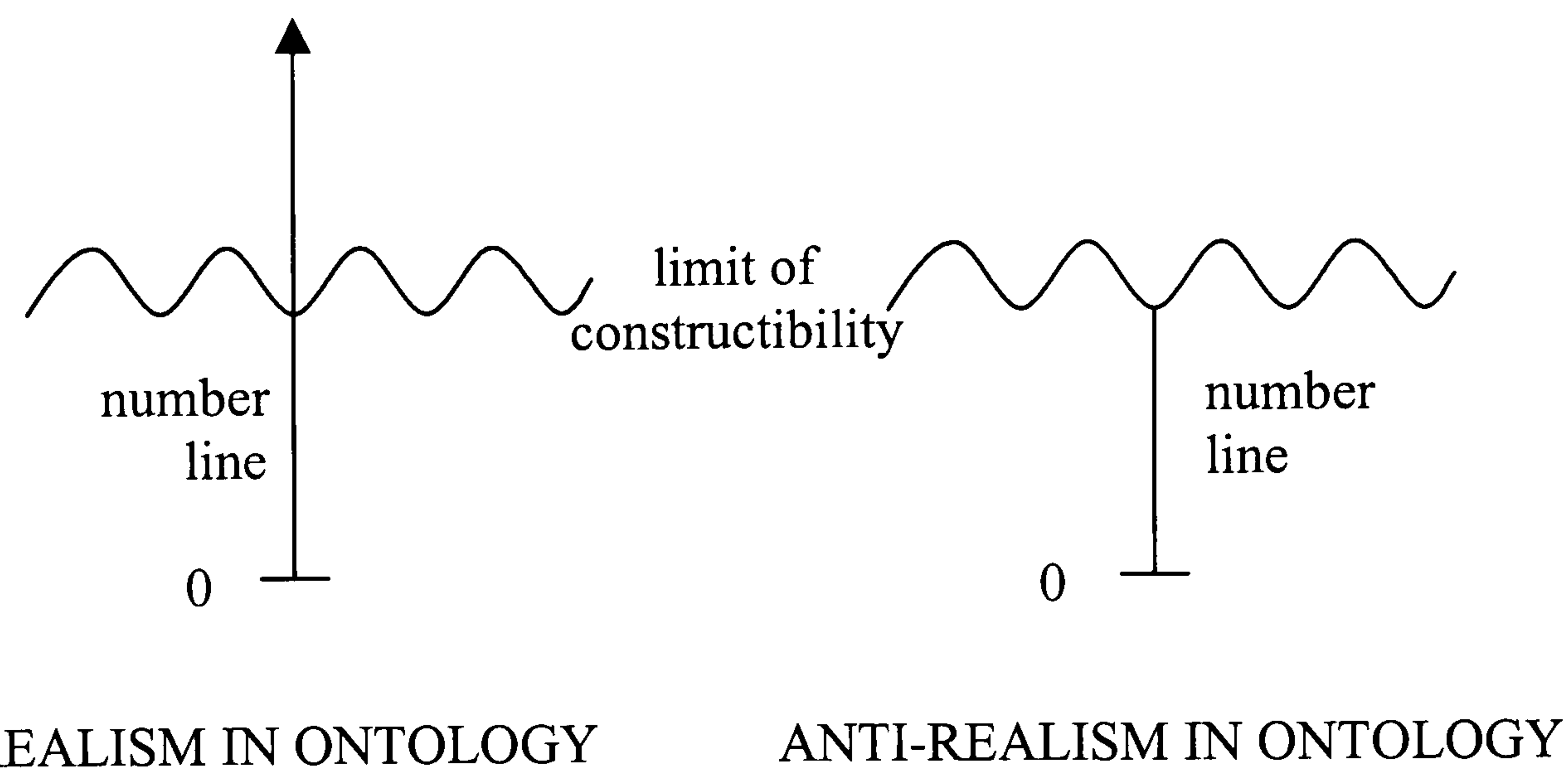
Now it can be seen that the weak (constructivist) claim of (strict) finitism can be accepted by all, and in particular, may be coherently admitted by those who wish to retain a realism in ontology with respect to the objects of mathematics; furthermore, it is a claim entirely consistent with a mind-independent domain of a collected infinity of numbers. The limit simply indicates where, in dealing with this domain, our intellectual faculties fail us. It is important to note that the additional (strict finitist) conclusion that there is a limit to the domain of numbers only follows from the additional assumption of an anti-

[10] Wright, *Wittgenstein on the Foundations of Mathematics*, pp. 139-40

realism in ontology. Hence, the strong finitist claim - that there is a cap on the numbers (or even a potential limit to constructible numbers on some 'classical' finitist account) - may be rejected simply by adhering to a realism in ontology, in which case the force of the finitist thesis is lost. It is not sufficient to advance finitism simply on the weak finitist claim, which may seem intuitively plausible – as we have seen, Wittgenstein also advances the weak claim, while resisting commitment to either side of the realist/anti-realist debate. As the finitist usually takes anti-realism in ontology as fundamental, he will have to find independent grounds upon which to persuade the platonist that the objects of mathematics are mind-dependent in order to even advance the strong claim that there is a cap or limit on the numbers. I do not mean to present this as a serious worry – given Benacerraf's dilemma, I might also suggest that the Platonist must find independent grounds to convince the anti-realist of the mind-independence of numbers; I simply wish to be clear about the importance of the constructivist element in strict finitism. The constructivist claim, construed in the way that Wittgenstein has done, is not a 'knock-down' for realist theories in the foundation of mathematics, even if it is found acceptable. The strong claim, on the other hand, assumes an anti-realism in ontology, and cannot be seen to put pressure on the realist case. What I shall be concerned with, in the chapters that follow, is whether the strong claim of (strict) finitism is a *coherent* one; in an attempt to demonstrate that strict finitism is still a viable position in the foundations of mathematics.

Before I leave the weak claim behind, however, I would like to make precise the distinction, as it may prove useful in the debates to follow. The discussion above can be summed up in the following way. The weak finitist claim[11], that there is a limit to the numbers that may be constructed by the human mind, is compatible with either a realism or an anti-realism in ontology with respect to the numbers. The precise distinction, at least with respect to number, may perhaps best be illustrated by the diagrams that follow.

[11] At any rate, the weak finitist claim with respect to number. Again, I wish to outline the point for numbers, which are, as I have said, fundamental to issues of surveyability, and anyway the model is more intuitive than that for proofs or mathematical statements. However, as before, I assume that this illustrative summary could be applied equally to proofs and statements of mathematics – where, on the diagrams that will follow, magnitude on the number line is replaced with complexity of proof (or statement).




When combined with an anti-realism in ontology, the thesis becomes the strong finitist claim, that there is a limit to the numbers, since all numbers are anyway mind-dependent objects, and hence must be constructed (or at least constructible) by the human mind if they are to exist. The internal debate of the finitists now rears up, as to what is meant by constructible in this sense – whether the numbers must be constructible 'in practice' or 'in principle', and precisely what is to be understood by those terms.

On the other hand, we have seen that the weak finitist claim is perfectly compatible with a realism in ontology; in which case, the strong finitist claim loses some of its force. Numbers below the limit of constructibility are those with which the human mind may interact sensibly, and those above are simply those which it cannot. Note again that there is still room for the internal finitist debate here – the question of actual or potential 'interaction' here will set the limit of constructibility lower or higher on the number line. The strict finitist claim, interpreted weakly, will tell us what the mind can currently, or actually, deal with, while the classical finitist claim, interpreted weakly, will tell us (if it tells us anything at all) what the mind may come to deal with. I mentioned earlier that I think intuitionism might be best understood as itself placing a cap on the numbers, albeit in a different sense to that of the strict finitists. Although I shall elaborate on this in more detail in the next chapter, I think that the weak finitist claim may highlight the issue somewhat: I suggest that it follows that if a strand of classical finitism (intuitionism, for example) does not place *some* limits on the potential capabilities of the mind, then the weak claim, for that particular strand, will say nothing at all.

As a final note in this section, I have shown the limit of constructibility on the above diagrams as a wavy line, and not as a sharp boundary; the reason behind this is that whichever side of the ontological debate you fall on (and indeed the internal finitist debate regarding constructibility), there remains the very serious problem of vagueness regarding the 'placement' of the limit itself. Vagueness is a topic worthy of extensive treatment, and will receive considerable attention in later chapters; I shall stop here simply with an acknowledgement of the problem. As with all vague predicates and properties, the fact that the sharpness of the boundary is not obvious does not of course entail the conclusion that there is no boundary at all – clearly, at least from the point of view of strict finitism, there are numbers which we can sensibly and intelligibly construct, and 'numbers' which we cannot.

# CHAPTER III: RELIABLE CONSTRUCTION AND 'SURVEYABILITY'

I would like to turn now to a discussion of constructibility, and the narrower, strict finitist requirement of surveyability. I have outlined already that finitism in general is committed to the claim that there is some limit as to what can be constructed by a mind – and I have further acknowledged that the description of this limit differs according to which particular brand of finitism is being advocated. For the strict finitist, this limit is a very real one, based on what it is *in practice* possible for the mind to construct. For the classical finitist, the intuitionist, this limit is perhaps best viewed as a potential one, since the requirement is rather what may *in principle* be constructed by the mind. Infinite quantities, or proofs requiring infinite steps, cannot be constructed even in principle, and so remain outside the scope of mental construction.

Despite the different qualification, the two requirements share certain important features. Both are required to give an account of what it is for a mind to construct an object or statement, whether that construction is an in practice or in principle one.

In this chapter I shall attempt to do precisely that: after some initial rejections, I shall turn again to Wittgenstein, who offers a useful account of surveyability as a description of the constructive requirement for *proofs*; an account which I go on to adapt for an account of the surveyability of numbers.

## *Interpreting Constructibility: 'Countability' & 'Inscribability'*

So what are we to say about such construction? Let us start at the very beginning – the numbers 1, 2, and 3 are constructible numbers. The number 1 we may take as base[1], and perhaps we are entitled to do the same for 2 and 3, on the grounds that their construction (mentally, as it were) seems no more complicated. If I wish to construct such

[1] On many conceptual schema, of course, the number zero is taken as base. There is no significant problem in taking zero as base here - however, on some strict finitary formulations, it is unclear whether zero should have number status at all. As such, and for the sake of consistency only, in my example here I shall take instead the unit singleton '1' as the base.

basic numbers, it seems as if I simply do so, with no further calculation or effort. Alternatively, we might suggest that 2 and 3 are 'constructible' from 1 – but the sense of *construct* here is slightly different from the standard interpretation. To see why, let us consider this as a 'first guess' – let us take the notion of 'countability' as our requirement for constructibility. The constraint seems to capture something of the intent of constructibility, certainly - it looks intuitively as if we may construct numbers by simply repeating the process of adding one; repeated application of the successor operation. For example, it seems I can construct the number '5' by simply adding one four times to the initial number '1'. Is this the case for all numbers? Countability in practice will certainly be capped, as it is suggested constructibility is - the finite nature of my lifetime, or my powers of concentration, will both constrain the numbers I can count to. And there is a clear sense of construction at work - just like laying bricks, I make bigger numbers the more times I add one.

Well, there are perhaps two problems with this notion as an interpretation of constructibility. Firstly, it is not clear that counting is 'reliable' construction; perhaps if I mechanically count, with say an abacus, or a calculator, there may come a point, admittedly after some considerable time, at which I have no real idea of the number that I am presently counting – that is, no idea of the magnitude besides 'very large', or has a certain number of numerals, and no ability to perform any other operations upon the number in question. If I were to write it down, it might look to me just a stream of numerals, and not a coherent number.

Secondly, and very differently, one might object that countability will not go far enough. There may be numbers, in advanced notation, say, that may not be counted, at least in practice. Such numbers might still be admissible within the constructivist framework – $10^{10^{10}}$ is presumably not countable, at least in practice. But it may still be an interesting question to ask whether it is a constructible number – to some extent, I have just constructed it.

This suggestion might lead us to an alternative interpretation of constructibility, and one not uncommon in the literature; the idea of 'inscribability'. The suggestion here is that, rather than our being able to count a number, we must instead be able to physically reproduce it (e.g. write it down). Michael Dummett, for example, suggests that the strict finitist's criteria of constructibility is such that they are committed to predicates like 'is possible in practice to write down in Arabic notation' for any acceptable number. Once again, this notion captures at least some of the fundamental principles of constructibility; a number is constructed (rather more physically, in this case) from numerals in a given notation. These numbers too, will (in practice) be limited; the constraints again of human lifetime and attentive powers, and even if we could suitably expand such constraints, presumably also the physical limitations of space-time, will all serve to restrict the extent to which such construction is possible in practice.

But again, we may begin to see similar objections rising as to the previous interpretation. Firstly, the problem of notation becomes explicit - Dummett's predicate stipulates a precise notation. But this will lead to odd answers to the question 'Is such-and-such a number constructible?'; like 'it depends in which notation it is written'. Again, the number $10^{10^{10}}$ is certainly inscribable in exponential notation; but is it inscribable in Arabic? Probably not, at least not in practice. Secondly, and perhaps even more forcefully in this case, it looks as though I could inscribe a 'number' (or at least a long string of numerals) without any idea what number I was inscribing. If I randomly scribble down numerals on a very large piece of paper for an hour or so, or even for 10 minutes, I will most likely have very little idea of even the magnitude of my inscribed number. Perhaps, if I spend sufficient time on the task, it will become practically impossible for me to recognise the magnitude or other qualities of my 'number' - does this now look like a mind-dependent construction? The problem seems to be that inscribability places too weak a constraint upon numbers, at least for the strict finitist.

It seems clear then that neither countability nor inscribability will be sufficient for strict finitism, at least; indeed neither counting nor inscribing is either necessary or sufficient for constructibility in this sense. With regard to intuitionism, the case is less clear. I think it is correct to suggest that the numbers that may potentially be counted-to, or potentially inscribed in any given notation, will amount to the same numbers which are potentially constructible on such an account; but to suggest that they are equivalent terms would be too quick. It is likely that such an account will allow for other methods of construction, to the extent that neither countability nor inscribability is a *necessary* operation for construction, within intuitionistic constructivism.

As far as we are concerned in the present debate, there are two important ideas coming out of the discussion so far – one is the notion of 'intuitive connection' with a number, or 'recognisability', and the second is the problem of different notations. The latter is a considerable problem, particularly for strict finitism, and I shall return to it in the next chapter. The first, however is an issue regarding the difference between the strict finitist and the intuitionist. The intuitionist will not require that numbers *are* intuitively recognisable, just that they could be; say, to a sufficiently advanced intellect. So the fact that I may not recognise or be able to work with a number does not mean that it is not *possible* for it to be recognised or operated with, *in principle.* For the strict finitist, however, the same fact is of paramount importance. Unless I can recognise it, a putative 'number' should not be admitted into my mathematical ontology – it is simply a collection of numerals, which *themselves* of course represent allowable numbers. Furthermore, we may also understand simpler relations of numerals, such as of the numbers (and numerals) one and two in the number twelve, which may fool us into thinking that an extremely long stream of numerals is still (a representation of) a legitimate number – but for the strict finitist, this recognisability, this intuitive connection, plays a central role. The term I shall use for this from here on is the standard term in the literature – 'surveyability' – and it is to a precise definition of surveyability that I shall turn my attention to next.

## *Wittgenstein's surveyability requirements*

The idea of surveyability is a complex one, and it has met with much criticism. I will of course devote some attention to such criticism later in this thesis, but for now, I wish only to establish precisely what the term is intended to imply. I think that some of the more basic objections to the criteria rest upon a mistaken understanding of what surveyability suggests, and so I hope, by a thorough explanation, to dispel some of these lesser worries. In fact, a useful exposition of the ideas behind surveyability is first provided by Ludwig Wittgenstein, in his *Remarks on the Foundations of Mathematics*. Wittgenstein, as I have already suggested, is not necessarily committed to a constructivism regarding the ontology of mathematics; nor, as a consequence, is Wittgenstein what has been traditionally called a strict finitist. Instead, as I demonstrated in the previous chapter, he may be read as making the 'weak' finitist claim only. Nevertheless, the criteria which he employs in establishing this claim may be picked up by the strict finitist, and combined with the constructivist strand of anti-realism in order to establish the strong claim of strict finitism. Let us start, then, with an examination of Wittgenstein's account - remember that where it differs from strict finitism is simply in scope; Wittgenstein suggests that his criteria will tell us what mathematical objects, statements, and proofs are to count as 'fully intelligible', and says little about the rest of mathematics - the strict finitist, on the other hand, suggests that all of legitimate mathematics must possess full intelligibility, since nothing can lie without on a constructivist account.

Wittgenstein offers criteria for 'surveyability'; the idea, in line with strict finitist tenets, is that more than the simple possibility of construction (as in the considered cases of counting or inscribing) is required for objects, statements, and (in particular, for Wittgenstein) proofs to count - it is also important that we can make sense of the construction in the relevant way. He writes:

> ""Proof must be surveyable": this aims at drawing our attention to the difference between the concepts of 'repeating a proof' and 'repeating an experiment'. To repeat a proof means, not to reproduce the conditions under which a particular result was once obtained, but to repeat every step *and the result.* And although this shews that proof is something that must be capable of being reproduced *in toto* automatically, still every such reproduction must contain the force of proof, which compels acceptance of the result."[2]

[2] Wittgenstein, *Remarks on the Foundations of Mathematics*, II-55

From this we may note commitment to the idea of reproducibility - a proof (and, if we are entitled at this stage to transpose these ideas onto the strict finitist claim about numbers, a number) must first be reproducible. This focus of reproduction for Wittgenstein is in writing, or re-writing, and encapsulates the idea of inscribability above - as Wittgenstein writes:

> "It must be possible to write down exactly [a] proof again."[3]

But this is not sufficient - Wittgenstein also suggests that the 'force of the proof' must be conveyed. A proof is therefore unsurveyable if we cannot see why it 'compels acceptance of the result'. So far then, we seem to have two constraints for the surveyability of proofs.

Crispin Wright suggests, however, that Wittgenstein's criteria contain a third requirement. Wright has written extensively on the topic of Wittgenstein's mathematics, and his exegesis on the subject of surveyability is instructive[4]. Wright makes a distinction between understanding each individual step of the proof (as, for example, following from a previous step) and recognising the proof as a coherent whole:

> "Arguably, then, three notions of surveyability are in play in Wittgenstein's discussion ... [which] seem to run in an ascending order of strength. If a structure of inferences is too complex or lengthy even to be physically reproduced, then naturally there can be no convincing check of every step in it; and if a physically reproducible structure is nevertheless too lengthy to be checked as a chain of inferences, then obviously it cannot serve as a paradigm of how a certain result cannot but be achieved by correctly following through a certain process."[5]

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[3] Wittgenstein, *Remarks on the Foundations of Mathematics*, II-1

[4] See Crispin Wright, *Wittgenstein on the Foundations of Mathematics.* Of particular interest to the present discussion is chapter VII entitled 'Surveyability'.

[5] Wright, *Wittgenstein on the Foundations of Mathematics,* p.122

If Wright is correct, then surveyability consists of firstly, the ability to 'reproduce' a proof (i.e. to physically write it down), secondly, the ability to follow each step of the proof, and check that it does not go wrong at some point in reasoning or mathematical calculation, and thirdly, to reach intuitive understanding of why the proof must come out as it does, and thus assert that the proof will always produce this result if correctly carried out. As Wittgenstein himself puts it:

> "The proof (the pattern of the proof) shews us the result of a procedure (the construction); and we are convinced that a procedure regulated in this way always leads to this configuration."[6]

Furthermore, the requirement that we understand, or recognise the correctness of the proof is such that we cannot be in error, or else we have not surveyed the proof :

> "That is to say, e.g.: we must be able to be certain, it must hold as certain for us, that we have not [for example] overlooked a sign in the course of the proof. That no demon can have deceived us by making a sign disappear without our noticing, or by adding one, etc.
>
> One might say: When it can be said: 'Even if a demon had deceived us, still everything would be all right', then the prank he wanted to play on us has simply failed of its purpose."[7]

It is this strongest of the three requirements (since in achieving it, we have presumably fulfilled the first two) which is of particular interest to us. The first is the simple (constructivist) finite requirement – a proof must consist of a finite number of steps, which may thus be written down. The second is (arguably) required in achieving the third – we will have performed a check of the reasoning in order to accept the outcome. What is suggested by the third requirement is that we achieve intuitive understanding of the proof. To put it plainly, and to expand upon the terminology

[6] Wittgenstein, *Remarks on the Foundations of Mathematics*, II-22
[7] Wittgenstein, *Remarks on the Foundations of Mathematics*, II-21

employed by Wittgenstein in light of Wright's analysis - we see not only *what* it is (requirement 1), but first *how* (requirement 2) and then *why* it is (requirement 3).

## *From proofs to numbers*

I should also take the opportunity to qualify, following on from the discussion here, my earlier claim about the reducibility of problems regarding the surveyability of proofs to those of the surveyability of numbers. Since I am employing Wittgenstein's criteria of surveyability to numbers, and not to proofs, as he does, I need to establish to what extent Wittgenstein's criteria can be transposed. There is a distinction, clearly, between numbers and proofs - and my earlier claim was that whatever criteria constrains the acceptance of legitimate proofs on an account of surveyability would *a fortiori* constrain the acceptance of legitimate numbers, because a proof that contained anywhere within it a number that was too big (or complex) to be surveyed, would of course make the whole proof unsurveyable. But as Wittgenstein's discussion develops, it is also clear that his criteria for the surveyability of proofs, as presented here, seems a little ill-fitting when applied to numbers. It seems odd to say, for example, that a number must convey the force of itself, and compel acceptance of its result.

It is clear, then, that we must to some extent re-interpret Wittgenstein's criteria with respect to numbers, since he does not discuss the ontology of numbers in relation to the criteria of surveyability; he is not after all, as we have seen, a constructivist of the revisionist variety. So, we must note instead the intent of the surveyability requirement, which I think is sufficiently similar in both cases. What Wittgenstein requires of proofs is that they are understandable - and understandable in the sense that once we understand them, as proofs, we know them; we cannot be mistaken about them. What the strict finitist requires, analogously, of numbers, is that they too are understandable - in the sense that we recognise a string, or arrangement[8] of numerals *as a number*, and we grasp

[8] In decimal notation, numbers are represented by a chain, or string of numerals. By the additional qualification of an 'arrangement' of numerals, I wish to include advanced notations, such as exponential notation, as well.

certain inherent mathematical properties, such as relative magnitude, perhaps, as a part of that understanding. We reach what I shall call 'intuitive intelligibility'; that is to say that we recognise not only that it is a thing upon which we can operate, (say divide by three), but we recognise what it is that we are operating on. I shall return to the notion of intuitive intelligibility presently, but for now I hope this brief explanation serves as an introduction to the way in which I believe the strongest requirement for surveyability (both for Wittgenstein and the strict finitist) may best be understood.

Another objection, before I leave the topic, to the translation of the criteria from proofs to numbers might be that it seems hard to apply the *three-stage* criteria to numbers - particular as the second requirement seems to cover the relations between steps of a proof, and it is not immediately obvious what the 'steps' of a number might be.

There are a number of responses to this query. The first is perhaps to suggest that while the second requirement plays a crucial role in the identification of proofs, it is to some extent, even for proofs, subsumed into the third requirement. For numbers then, it might be a trivial operation, entirely subsumed into the third requirement. This is to suggest that while the further distinction, drawn by Wright, is plausible in the criteria for the surveying of proofs, no such further distinction is possible in the case of numbers. The number has one step - therefore to understand and recognise each step is to understand and recognise the whole.

An alternative proposal is to suggest that we must take 'steps' in a proof to resemble 'steps' in the formation of a number - we might maintain, for example, that each numeral in the number is a step - and that we must understand, in order to ultimately recognise the whole number, each numeral in its relation to the next; as units, as tens of units, as tens of (tens of units), and so on. Perhaps this proposal is to be preferred, as it seems to preserve the most of the original criteria (for proofs).

A third possibility here of course would be simply to reject Wright's classification of the surveyability criteria into three distinct stages; we saw at the outset that on a superficial reading of Wittgenstein, at least, we may initially draw out only two. The translation into surveyability for numbers is much simpler on such an account - the first requirement is simply that the number, like the proof, is capable of being accurately reproduced; and the second is the 'intuitive intelligibility' requirement for either, which I shall return to shortly. I see this as an unpromising alternative, however, as Wright provides considerable evidence, at least, to suggest that Wittgenstein was operating with more than just the two separable notions.

So what is it for a number to be surveyable? Obviously it must be possible to write the number down. However, there are various issues to consider regarding such possibility, particularly from a strict finitist position. Firstly, there are many different notations used for number, and if we advance as a requirement the strict finitist idea that it must be possible to *actually* write (the numeral for) a number down, then it is clear that larger numbers will be possible in Arabic notation than in stroke notation. Similarly, much larger numbers will be possible in exponential notation than in Arabic, and so on. So clearly, Wittgenstein's first requirement is not sufficient to define the surveyability of numbers and place a limit of constructibility on the numbers *independent of the notation used.*

Wittgenstein's second requirement seems easily fulfilled in the case of numbers – we may check that the number desired has been correctly represented (again, with notational issues at play, this may not be simply trivial – checking that the number '73' has been correctly represented in stroke notation would presumably not simply be a matter of observation). But lastly, the third requirement is that we have an intuitive grasp of the number being represented. And here we may reach an agreeable form of isomorphism between notational models. For a number to count as intuitively surveyable, we must possess an intuitive understanding of the number and its properties. That is to say, that we must possess a certain direct connection with the number, not definable simply in terms of other smaller numbers (as in the case, presumably, of even relatively simple exponentiation). The precise boundaries of such a requirement are precisely those debated by finitists and their detractors alike, and have yet to be properly established. We can hopefully see, however, the role that Wittgenstein's intuitive intelligibility requirement plays in the description of the (strict) finitist thesis. Regardless of the notation used to represent the number, the corresponding 'number-concept' must be directly intelligible to us.

As a slightly tangential but related point, we may take this notion, as a broadly strict finitist one, and assert that the classical finitist, the intuitionist, must be committed to a similar notion of constructibility. Of course, the position will differ to the extent that it will not place a 'real' limit on the construction of finite numbers, since potentially *any* finite number may be constructed. But with respect to what it is for something to be constructible (in either sense), the finitists may perhaps reach a degree of consensus. A mathematical object is constructible if it is finitely accessible to the mind; that is, its construction consists in a finite mental operation which results in the possession of an intelligible concept – and so a mathematical object is constructible if it is possible (either in principal or in practice) to perform a mental operation which will result in the possession of the appropriate concept, where, as far as Wittgenstein (and the strict finitists in this case) are concerned, *full* possession of the concept involves a sense of the object being recognised *for what it is*, in accordance with Wittgenstein's third requirement. The important difference here is that, for Wittgenstein as for the strict finitists, the intelligibility requirement is a *further* constraint (that is, further to the first and second requirement) – the only possible constructions are those for which we are *in practice capable* of obtaining a full possession of the relevant concept *by some 'recognitional feat'*; whereas for the classical finitists, full possession of a concept requires only that we be in principle capable of carrying out the requisite constructive activity; which will of course be true of any candidates which satisfy the first two requirements.

So, it seems, the idea of constructibility contains – for either type of finitist – an idea of *intuitive intelligibility*. The classical finitist may I think concede this without abandoning any ground; the point (to re-iterate) is simply for them that anything finitely performable is in principle intuitively intelligible. Again, as I have already intimated, it is presumably the case that, as the classical finitist is committed to the view that numbers are mind-dependent, it would be odd for them not to nod assent to this suggestion; in order to arrive at the construction of a mind-dependent entity, there must come with it an accompanying (at least *in principle possibility of*) intuitive intelligibility; an understanding in the mind.

One remaining important issue[9] regarding the limit of constructibility needs to be re-examined before we continue. As I have already mentioned, the limit imposed by a strict finitist philosophy will impose a very real limit on the natural numbers. Clearly, even given just the weakest of Wittgenstein's requirements, there will be finite natural numbers that we are unable to write down, due to limitations of space, or time, or entropy[10]. In particular, humans are finite entities, and have a comparatively short lifetime, and maximum rate of operation. Hence, the largest number I may feasibly write down in my lifetime will still be a relatively small finite number, by traditional standards. Obviously, Wittgenstein's other requirements will bring this limit down much further, eliminating concerns about my lifetime, since it will presumably not take me long to 'reproduce' a number (in any notation) that out steps the boundaries of full surveyability. As a result, for the strict finitist, there is a limit on the numbers, a 'cap' beyond which meaningful construction is not possible.

For the classical finitist however, the situation is more complicated. If we are just considering the finite numbers, it seems as though, given the classical finitist requirements, no finite number will be un-constructible, or un-surveyable (unintelligible) in the *potential* sense. If we consider all numbers, the classical finitist is going to insist upon a limit, but since it is not usual to think of infinite numbers as simply extending the finite number line, it may be difficult to find an intuitive model for this. As I have said, I

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[9] For the sake of completeness, I should perhaps also note here a further concern about my discussion in this section that might arise following the analysis in the previous chapter, regarding Dummett's views on object vs. objectivity – it might be objected that my treatment here is ontological, ignoring the importance of Dummett's remarks on objectivity, and classes of disputed statements in place of mathematical objects. However, presumably we could construe the constructibility requirement as something like the limit of intelligibility for mathematical statements (of a certain length or complexity). This suggestion is, I think, implicit in my treatment here.

[10] Gandy provides a discussion of the issues involved here with respect to the finitude of time and space, and mentions the suggestion of F.J. Dyson, that in a particular model of the universe, thermodynamical considerations might be avoided; I shall not reproduce the discussion here. It is sufficient for our purposes to note that some finitude of humanity, or space-time, will impose certain limits on what is effectively achievable.

think it is best to perceive the intuitionistic (classical finitist) limit as a potential limit, but, I assert, a limit nonetheless. For my purposes, the remainder of my work, focusing largely as it does upon the claims of and issues surrounding strict finitism, shall adopt a strong (strict finitist) notion of the limit of constructibility; I shall leave it to the reader to decide with regard to classical finitism whether the limit applies analogously. I will however, reiterate the conclusion reached in the previous chapter, that if the strict finitist's limit of constructibility can be construed as making *only* a claim about what the mind can intelligibly deal with (the 'weak' claim), then unless one assumes that classical finitism places an analogous (albeit higher) limit on the numbers (perhaps in the sense that the mind is only ever going to be *potentially* so good), then it seems as though classical finitism may be construed as saying nothing at all – since in the presented case, classical finitism could be construed as making a claim about what the mind can intelligibly deal with, and yet asserting that the mind could come to know (understand, and hence intelligibly deal with) anything.

## *Intuitive Intelligibility*

I have already given a brief account of what I mean by the term 'intuitive intelligibility' for numbers. It is time now for a proper examination of the concept, and an attempt to answer some of the worries that might arise regarding such a concept. To recap: a (putative) number is surveyable, only if, (in line with Wittgenstein's strongest requirement for the surveyability of proofs), that number is intuitively intelligible. We should admit it as a legitimate object of mathematics on a strict finitist account, only if we recognise it as a number, and not merely as a string or arrangement of numerals that 'looks' like other, already allowed and recognised, numbers. Recognising it as a number is not an entirely unproblematic notion in itself, not least of all because of the inherent problems of different notations. But here let us say that recognition of a number involves (at the least) a grasp of comparative magnitude - when presented with another such number, in any notation, we would readily be able to identify the larger of the two.

The idea of intuitive intelligibility might also be seen to imply that a number be applicable without calculation - the idea here being that if I have to think about a mathematical operation involving the suggested intuitively intelligible number, then if I have to perform additional calculations, involving smaller number-concepts, then the number can hardly be genuinely *intuitively* intelligible. Well, this would be one line to take, but it appears unnecessarily restrictive - something like 'fierce finitism', perhaps. If we are to allow only those numbers which are intuitively intelligible *in this sense*, we will be forced to reject all but perhaps the basic unit numerals - and perhaps not even all of them.

In fact this line of thought is not entirely without support - it receives some empirical backing from the observation that we can only recognise groups of objects, say, in very small numbers - between five and seven for most people. With larger groups, we recognise them in terms of multiple collections of smaller groups. If one thinks about the sides of dice, one probably has a direct representation of all the numbers in terms of the arrangement of dots. If one thinks of even a slightly larger collection - say, the cards in a suit from 1 to 10, one probably recognises the collection of symbols on the eight, nine, and ten cards as two collections of five or less symbols. The eight card presents itself to me as two collections of four symbols, for example. There is a very real sense here in which we are operating with less than ten 'numbers', in order to intuitively deal with larger ones.

Well, while on the one hand a strict finitism based upon this kind of account of intuitive intelligibility might be nice and simple[11], it seems unlikely that anyone will take it seriously as a strict finitary foundation for mathematics. It is only barely constructivist in nature, and it is so drastically revisionist that it seems to call for the rejection of mathematics in its near entirety. Rather, let us attempt to move away from such a limiting definition of 'intuitive intelligibility', and try to provide an account more in keeping with the intentions of the strict finitist claim.

[11] Although it is interesting to note that even this formulation does not avoid commitment to vague totalities. Even if we may say that the limit of surveyability in this sense is for most people between five and seven, is the limit five, is it six, or is it seven? There seems no good answer to such a question - for similar reasons that there can be no universal answer in the more general case; as we shall see in a forthcoming chapter.

I have suggested that there are certain inherent mathematical qualities that are to be grasped, if a number is to count as intuitively intelligible. To require that we grasp *all* of a number's mathematical qualities at any given point would seem to be at least as restrictive as the requirement dismissed above. What then are the qualities that we must grasp?

It seems as though a natural demarcation might be that of 'intuitive operation'. By this I mean simply that by mere acquaintance with a number, we are aware of the applicability of certain mathematical operations with respect to the number in question - is it divisible by three, for example. Now it seems that for relatively small numbers, like 3 or 27, I am intuitively aware of the possibility of division by three, and indeed of the result of that operation, without calculation. (1 in the first case, 9 in the second). Whereas for unsurveyable numbers, the mere observation is impossible, and calculation is required. However, this notion is problematic on at least three counts.

Firstly, the suggestion that certain operations should allow us to determine the surveyability of numbers seems to make the selection rather arbitrary, as it will surely depend upon the precise operation involved. While division by three may not be obvious for many cases, divisibility by 10 is easy to determine, simply by examining the last digit in any decimal number. This holds for numbers that the strict finitist would ordinarily want to reject as unsurveyable, since only the last digit must be inspected. Moreover, numbers in advanced notations may even become surveyable on this account when their decimal counterparts are not. I know, for example, that $9^{10^{10}}$ is divisible by 9, even though I could not recognise the same property if presented with a decimal equivalent.[12]

Secondly, conversely, such a criterion seems too restrictive for some cases. I have no intuitive awareness of the divisibility-by-three of the number 46458, for example. Dismissing numbers in the thousands or tens of thousands is applying the finitist standard again too rigorously to be an accurate reflection of the strict finitist claim.

And thirdly, perhaps fatally, it seems impossible to give an account of when such

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[12] The success of such a constraint is also, as should be obvious here, heavily dependent upon the notation used. Division by 10 is easy to check for decimal numbers, but not for, say, hexadecimal. Although there is a good deal more to be said regarding the problems of notation for the strict finitist account, it would be preferable to find an account of surveyability that provided consistent results across (and independent of) notations.

intuitive operations themselves become non-intuitive. Although division by three may be an intuitive operation, it is presumably not the case that division by *n* will always be an intuitive operation. Since we might only provide a definition of intuitive operations in relation to the numbers involved, such a criteria for intuitive intelligibility, and hence surveyability, looks to entail vicious circularity.

Instead then, let us focus on what appears to be the primary 'recognisable' quality of intuitively intelligible numbers, and leave open the question as to whether there are attendant properties that are conveyed at the same time. The key issue is one of magnitude. I have already suggested that a grasp of a number involves a grasp of its magnitude; and indeed this seems to be the crucial difference between recognising a *number*, and recognising a string (or arrangement) of numerals.

The first and most obvious objection to such a suggestion is that, while a grasp of magnitude may be appropriate for the scale of integers, it does not seem appropriate when applied to the real numbers. Part of a strict finitist's aim is to reject a good deal of the traditionally accepted real numbers - certainly the irrationals, but also presumably a fair amount of finite but 'unsurveyable' real numbers - that is, numbers with too many decimal places to be surveyable. I shall discuss the issue of complexity versus size in the next chapter, but the objection here turns upon the plausible assertion that one may have a very good grasp of the magnitude of (for example) pi - after all, it's just a little more than 3. Furthermore, 3 is perfectly surveyable, and so is 4; hence surely pi is a surveyable size?

To make such an objection is just to miss the intent of the criterion. When I suggest that what is required by intuitive intelligibility is 'grasp of magnitude', I mean that one must have a clear grasp of magnitude, and not simply an idea of scope, or range. One must recognise the number as itself[13], and identify it by its precise magnitude. It is *that* number precisely and no other - and what sets it apart from any other, what prevents confusion once the number is recognised, is its magnitude. Once one has a clear grasp of

[13] This is not a vacuous requirement - compare this with Wittgenstein's requirement for proofs: "On the one hand we must be able to reproduce the proof . . . and on the other hand this reproduction must once more be *proof* of the result . . . every such reproduction must contain the force of proof". (II-55)

a number's magnitude, one may take *any* other number (at least, any other number of which it can be said that one has a grasp of magnitude) and immediately identify which has the greater and which the smaller magnitude.

Note that on this account, it does not matter which notation conveys the magnitude of the number to the surveyor - *that* the magnitude is conveyed is sufficient. To that extent, the limit of surveyability is not here dependent upon notation.

Notation still remains a challenging problem in a wider context, however, and so I shall turn my attention next to the special problems raised by notation for any strict finitary account.

# CHAPTER IV: COMPLEXITY AND THE PROBLEMS OF NOTATION

In order to provide a thorough account of strict finitism, to give as full a picture as possible of the shape of the theory, it becomes necessary to address some recurring worries in the discussion of the previous chapters. So far, we have identified the central tenets, and established a robust account of the notion of surveyability. One problem remains largely unanswered, however – and that is the question over how surveyability is related to the notation employed. Does more sophisticated notation lead to an increase in surveyability? Is surveyability, after all, *dependent* upon notation? The central concern here therefore, along with some related issues, will be the problem of differing notation. The problem should already be clear, and is essentially as follows; numbers that are unsurveyable in Arabic notation may seem perfectly surveyable (in some sense at least) in exponential notation. Nor is the problem especially restricted to cases involving exponential notation - the problem arises for any two notations. Equally, for example, numbers that are unsurveyable in stroke notation may seem surveyable in Arabic notation. Now, clearly, any strict finitary account – at least one that is serious about surveyability – will have to provide an adequate account of the discrepancy. I have skirted this issue more than once already in previous chapters, so I shall turn next to serious consideration of the difficulties it presents.

It seems to me that two strategies initially present themselves to the strict finitist in order to deal with the apparent problem. Firstly, one might stick rigidly to the criteria outlined in the previous chapter, and require an intuitive grasp of *numbers*, independent of the notation used to represent them. This may seem a natural proposal following the remarks I have already made regarding the criteria for surveyability, but there is at least one other interesting alternative for the strict finitist, alluded to in the literature. The strategy here is to simply embrace the distinction between differing notations, and allow various limits on the surveyability of numbers, *dependent upon notation*. This second option may not seem immediately reconcilable with the surveyability criteria as presented earlier, and on first inspection seems to require the rejection of the constraint of intuitive intelligibility. Since this second option seems the one most at odds with the current analysis, let us start by examining it in order to establish whether or not it can be coherently advanced independently of the criteria of intuitive intelligibility. If so, we might suggest an alternative formulation of the criteria of surveyability, such that complexity of notation plays the pivotal role in determining whether or not the number represented is surveyable.

*Complexity over Magnitude*

The central idea of such a proposal, then, is that, rather than the number *itself* being somehow intrinsically surveyable or unsurveyable, it is instead the expression that represents it which is open to determination. Let us suggest that when an arrangement of numerals, in any notation, is too *complex* (for example, the sheer number of numerals present makes the arrangement unwieldy, or else the complexity of the notation prevents us from seeing what is being represented for some large numbers), then the number represented by the arrangement is unsurveyable. As a result, the number 2456 is surveyable in Arabic notation, but (presumably) not in stroke notation. Equally, on such an account, the number $10^{10^{10}}$ is surveyable in Exponential notation, but not in Arabic (and certainly not in stroke-) notation.[1] Note that as I have remarked, such an account will not accommodate the third requirement for surveyability (recall that the third requirement is that the surveyor is capable of some intuitive grasp of the number *as a number*); principally because the number itself has less importance - the representation of the number is what counts. As a result, as long as we have a suitable notation (in practice) to surveyably represent a number, (i.e. one in which the notation is not too complex to be surveyed), that number is admissible as a legitimate mathematical object.

Why, then, should such an option be attractive to the strict finitist? The idea of complexity certainly seems to play a role in our capacity to survey. The suggestion that 15 is harder to survey than 4, say, and that 386 is harder than 41, seems to be true, and not just coincidentally. The more numerals involved, the longer it takes us to take in (or even just observe) the arrangement, and surely this simple fact must be a factor

[1] The question as to whether or not $10^{10^{10}}$ is a (surveyable) number is an old and much-debated one in the literature – I use it here as a paradigm example of exponential notation, which would not be surveyable in Arabic notation. To the question of whether or not it is surveyable, as a number (and hence, on a strict finitary account, admits of number-status at all), I shall offer at least a tentative response by the end of this chapter.

in any criteria of surveyability. Furthermore, it looks as though some notations are invented precisely to make larger numbers easier to deal with, intuitively - the number in exponential notation $62^6$ appears less unwieldy than the decimal equivalent 56800235584, for example.

Indeed, if we try to remember this number to reproduce it later on, we will likely remember it as $62^6$, or else, if we have only the decimal notation, as a series of smaller numbers, just as most of us remember telephone numbers, rather than remembering it as the decimal number fifty-six-thousand-eight-hundred-million, two-hundred-and-thirty-five-thousand, five-hundred, and eighty-four. Moreover, the same point holds true of different numbers like $9^{18}$ and 56563344556788, even though in this case the number represented in the first instance has far greater magnitude.

Michael Dummett offers an interesting comment on the subject of notation, which seems to entail a commitment (on behalf of strict finitism) of the kind expressed here. He is considering the case of Arabic numerals contrasted with numbers written in exponential notation, and writes:

> "On the other hand, [the totality of Arabic numerals supplemented by the symbols for exponentiation] does not have the property, which [the totality of Arabic numerals] shares with the totality of natural numbers as traditionally conceived, that, for any number n, there are n numbers less than it: for, plainly, the totality does not contain as many as $10^{10^{10}}$ numbers."[2]

Dummett's claim is that if we allow surveyability in Arabic-supplemented-by-exponentiation-notation, such that $10^{10^{10}}$ is surveyable, there are clearly not as many as $10^{10^{10}}$ surveyable numbers less than it. The idea here is that as more and more complex exponential notation is introduced, an increasing number of natural numbers (*representable* but not necessarily *surveyable* in Arabic notation) are 'missed out', or skipped over. Dummett continues:

[2] Dummett, 'Wang's Paradox', p.303

"Since a totality determined by a notation of the second [Arabic supplemented by exponentiation] kind will still not be closed under all effective arithmetical operations definable over it, it possesses no great advantage over a totality of the first [determined by a notation comprising only Arabic] kind, and, for most purposes it is better to take the natural numbers as forming some totality of this first kind."[3]

Dummett's observations suggest that since the complexity of exponential notation does not increase uniformly with the magnitude of the number represented, the discovery that a representation (in exponential notation) is surveyable will not entail that all the numbers (integers) below that number are surveyable. Therefore, unlike in decimal (or stroke notation), where there is uniform progression of complexity with respect to magnitude, the size of the set of numbers that are surveyably representable in any given notation is arguably the same (or remarkably similar) to the size of the set for any other notation.[4] This suggests a commitment to complexity, at least at the basic level - the number and arrangement of numerals and/or symbols might place a limit on the surveyability of a representation (an inscription, for example), and thus the size of the totality of surveyable numbers would be roughly equivalent across notations. That is not to say that the same *numbers* will be surveyable on any notation - such a uniformity is lost given the complexity criteria for surveyability that we are considering here - but just that there are always a (roughly equivalent) finite number of surveyable representations possible within any notation. If, for example, we were limited to taking in only arrangements of symbols in which there were no more than two symbols, the highest number we could take in in Arabic notation would be 99. The highest number we could take in in (purely) exponential notation, on the other hand, would be $9^9$. Although the second of these numbers has a much greater magnitude, still *the number of numbers*, or the size of the set of numbers, that we were capable of taking in in each notation would be equivalent, since we would be limited to the same combinations of symbols.

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[3] *ibid.*

[4] This is not quite true, since it will not hold for very simple notations such as stroke notation, in which the set of surveyably representable numbers is presumably much smaller than in other cases. There may, however, be a natural explanation for this, as I shall outline shortly.

One immediate and potentially problematic observation regarding such a claim is that if we take the numbers that are surveyably representable in *any* notation, we are inevitably left with holes in the number line. If, as Dummett suggests, the totality comprising the surveyable Arabic numerals supplemented by the symbols for addition, multiplication and exponentiation "plainly . . . does not contain as many as $10^{10^{10}}$ numbers", even though $10^{10^{10}}$ is a member of such a totality, then there are clearly some 'numbers' which are not surveyable even though their magnitude falls below others which are. Moreover, all though some of these *may* be representable in Arabic, many of them, still less than $10^{10^{10}}$, will be unsurveyable in either notation. Nor can this problem be rectified by a 'complete set' of advanced notations, for each advanced notation will presumably only expand the range of numbers to greater magnitude, and make more and bigger holes higher up the traditional number line, not less. And even though it might be true that in principle notations may be constructed to represent these 'missing' numbers surveyably, such a suggestion must be rejected by a strict finitist, who is in the first place restricted to in practice representation, and in the second, opposed to the idea that all finite numbers are representable, which such a solution would entail.

Even if we may later offer considerations to accommodate for these missing numbers, it does not look as though we have the resources for doing so here - just taking, as the present proposal suggests, complexity to be the fundamental criterion for surveyability.

Moreover, there seem to be yet more insuperable problems for such an account. Firstly, such a constraint would have the effect of dividing mathematics by notation; the criteria are supposed to provide an account of legitimate mathematical objects and practices, but on this kind of account the admissible objects and practices become merely contingent upon the *notation* used. The position will presumably entail statements like – 'such-and-such is an admissible mathematical object/statement/practice (say, a number) in exponential notation, but not in Arabic'. Something like this has been embraced by Van Dantzig, who suggests that the natural numbers which actually can be constructed (say, by a series of what he describes as 'elementary mental acts') does not include $10^{10^{10}}$, but that $10^{10^{10}}$ is nonetheless a natural number. What Van Dantzig suggests is that the meaning of the term 'natural number' has changed in this case. Indeed, he suggests that the implementation of first addition, then multiplication, then involution, etc., each introduce a new 'class' of natural numbers, such that postulates involving any of these operations will only count as (constructible) *proofs* insofar as they apply to natural numbers in the first sense. He maintains that each interpretation of natural number has a corresponding set – $S_1$ for natural numbers in the first sense, $S_2$ for those involving addition, and so on, so that numbers of the form $10^{10^{10}}$ belong to $S_4$, but not to any 'lower' sets. His conclusion then is that:

> "The difference between finite and infinite numbers is not an essential, but a gradual one. According to the successive definition of 'natural number' in the successive senses, the individual identifiability and distinguishability disappear gradually if the numbers become larger and larger and can be retained by new definitions only for a scarcer and scarcer class of numbers."[5]

While Van Dantzig is here addressing the question of whether $10^{10^{10}}$ is a *finite* number, what he says may be equally well (if not even more appropriately, given his constructivist qualification) applied to the case for surveyability – such that a natural extension of Van Dantzig would be to conclude instead that the difference between *surveyable* and *unsurveyable* numbers is not an essential, but a gradual one. Something about this may well seem appealing, and as I shall go on to discuss, the idea that the limit is absolute is one the strict finitist may well wish to resist; but in the present case, the solution suggested by Van Dantzig's analysis is to propose that surveyability has different senses; numbers surveyable in Arabic notation are surveyable$_1$, whereas those surveyable in (only) exponential (or 'higher') notation, are surveyable$_4$ (following Van Dantzig's identification of the different sets corresponding to the successive senses). This is certainly an ingenious approach to the problem, but not one which I think will prove useful in the current analysis. For one, it seems rather as though the sense which Van Dantzig attaches to surveyable$_1$ is the original *intended* sense of surveyable.[6] Moreover, it is not clear that, for the strict finitist who is concerned at least with providing an *ontological* account, Van Dantzig's solution can meet the challenge of the problem I have already raised. A

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[5] Van Dantzig, 'Is $10^{10^{10}}$ a finite number?', p. 276

[6] In this sense, it seems to me, Van Dantzig's analysis is not, after all, a genuine example of the 'surveyability as complexity' model.

(putative) number will not be an admissible, legitimate object of mathematical practice *in and of itself* – rather it will be admissible *in some senses* and not in others. Since the strict finitist is looking for a further move here, to the extent that surveyability (and constructibility in this sense) will govern what numbers there *are*, this solution, I suggest, looks an unpromising one.

A further problem for any strict finitary account that takes complexity as its requirement for surveyability is that such a criterion may well admit individual cases that the strict finitist is bound to reject. I have already acknowledged that on such a schema, $10^{10^{10}}$ is admissible, at least in exponential notation. One might go further, and suggest that the strict finitist, while following such a constraint ought, to admit $\pi$ as a legitimate (surveyable) number. The symbol is comprehensible, certainly not too complex, and by most, at least, recognisably represents at least a *candidate* for number-status. The problems of admissions of this kind for strict finitists should be obvious; since pi is an irrational number, in any notation but this symbolic form its construction is infinite in scope. One need only consider the symbol for infinity to register the force of this point. The symbol is a notation of a kind, in either case, and if complexity is the requirement for surveyability, numbers represented by symbols like these look like they should be as admissible as the numbers represented by numerals like 1, or 2. It is of course no defence to suggest that such symbols do not use 'numbers', or numerals, in representation – nor, presumably, does stroke notation, but it is clear that it nonetheless manages to serve as a notation for (some) numbers.

In fact this problem looks like a general one for the surveyability-as-complexity account, in a similar way to the objection I outlined earlier regarding the apparent holes in the number line on such an account. It looks as though the constraint will fail to adequately rule out any numbers, subject to some notation. To see this, consider the simple constraint Dummett proposes on behalf of the strict finitist – that of 'numbers that it is possible in practice to write down'.[7] Since this now becomes 'possible in practice to write down *in some notation*', it is not clear that any more is ruled out than on an intuitionistic account; indeed, to the extent that it will permit

[7] Of course this is not the preferred constraint of the proponent of surveyability, as I have already discussed – but the simple example serves well to demonstrate the point; analogous demonstrations may be made for more complex constraints.

surveyable representations of infinite numbers – perhaps even Cantor's transfinite mathematics – it may rule out a whole lot less.

As a result, this option, at least so-construed, looks unpromising for the strict finitist account of the apparent difficulty presented by alternative notations. This might seem a natural conclusion, in some respects – after all, one way to represent this construal of surveyability is as merely ignoring Wittgenstein's 'strong' surveyability constraint. In the previous chapter, I drew out what I called 'intuitive intelligibility' to be a requirement on numbers if they are to count as surveyable, following precisely from Wittgenstein's strong requirement upon proofs – a number, on this kind of account, must present itself to us as a *number*, and not merely as a numeral, or arrangement of numerals (supplemented by notational symbols where appropriate). But if, on the other hand, as is suggested by the proposal under consideration, we merely take the complexity of the representation as the criterion for surveyability, we seem to be able to do away with Wittgenstein's strong requirement – at least in the sense of intuitive intelligibility that I have attached. Such a position then, might want to construe Wittgenstein's remarks differently. One way to do this would be to read the strong requirement, when translated to numbers, as only requiring that the representation of the number must itself be 'taken in' all in one go – a notation which represents a number with few enough numerals and symbols that their entire relation is understood 'all at once', as it were, will count as sufficient notation to render the number represented surveyable. Something like this might be encouraged by the following quote, in which Wittgenstein makes an apparent commitment to the kind of proposal under discussion. He writes:

"I want to say: if you have a proof-pattern that cannot be taken in, and by a change in notation you turn it into one that can, then you are producing a proof, where there was none before."[8]

If, as I have suggested, we are entitled to translate what Wittgenstein says about proofs to numbers, the natural suggestion is that if a number is presented in a notation in such a way that it is unsurveyable in that notation, it is not to be admitted as a legitimate object, but when it is later presented in a notation in which it is

[8] Wittgenstein, II.2 (1964), III.2 (1978)

surveyable, it should be admitted. One is, in Wittgenstein's way of putting it, producing a number, where there was none before.

This is, of course, simply one way to interpret the position advanced by Wittgenstein, and even if we were to adopt it, it will not, I think, give us sufficient motivation to accept the surveyability-as-complexity account in the face of all the problems I have identified. In fact Wittgenstein's assertion here will not commit him, as I shall go on to demonstrate, to a notion of surveyability as complexity, despite the apparent similarity. The best interpretation, both for Wittgenstein and for the strict finitist, turns precisely upon what is meant by surveyable, or 'taken in', as Wittgenstein writes. Since this is precisely what is still at issue, I shall return to this point shortly. For now, let us return to the alternative option open to the strict finitist when considering the difficulties of notation, that I mentioned at the opening of this chapter.

### *Magnitude over Complexity*

Another way of dealing with the problem is simply to deny that complexity plays an important role in our criteria for surveyability. We return here to the criterion of intuitive intelligibility; and insist that for a number to count as surveyable, we must possess an intuitive grasp of magnitude, *independent* of the notation used to represent it. Thus, however I represent the number, be it " 9 ", " $3^2$ ", " | | | | | | | | | ", etc., what is important is whether or not I can possess intuitive grasp of the *magnitude*, of the *number* 9. The complexity of the representation makes no difference to the individual magnitude of the number represented, and hence, according to this kind of response, cannot determine whether or not we may reach an intuitive grasp of that magnitude.

There are a number of problems for such a response. Firstly, we ought to remember that strict finitism is an account of in practice surveyability, and hence the means by which we actually *do* survey – including the notation we use – ought to be important. When we shift from using our fingers, or stroke notation, to counting in Arabic, there seems to be a shift in our practical capability to deal with larger numbers intelligibly. Furthermore, such an account seems to deny the common-sense role of differing notations – they are often introduced precisely to make previously unwieldy numbers accessible, and in precisely the sense of 'accessible' implied by the definition I have given of 'intuitive grasp of magnitude'. It seems as though the only finitism this kind of account would suffice for would be the overly restrictive kind (which in the last chapter I called 'fierce finitism') that suggests an intuitive grasp of only six or so numbers. In this case, the intuitive grasp in question is one that is presumably independent of notation, since we appear to have intuitive access to these numbers in any notation. But it is not clear that even here the criterion is plausible – might I not construct a notation which was so complex that even the magnitude of these simple numbers was not conveyed? If that were my only notation, in what sense would I have an intuitive grasp of even these numbers?

Complexity, then, it seems must play a role in our surveying, and hence in the surveyability of numbers. If it did not, *our ability* to survey numbers ought to be independent of the notation used, and consideration of simple cases suggest that it is not. Consider the use of stroke notation alone – will stroke notation, not supplemented by any further notation, adequately convey magnitude for even relatively small numbers? Isn't it rather the case that, as I intimated above, Arabic notation simply allows us to 'grasp' higher numbers, in the relevant sense?

One response might be to suggest that familiarity with a notation, rather than complexity within that notation, is important; and that the more familiar we are with a notation, the better we are able to survey, regardless of the complexity. For example, few of us are capable of surveying hexadecimal numbers above sixteen or so – but if we (had been taught and) used only hexadecimal notation it might conceivably allow us at least as much grasp of magnitudes as our current decimal system does[9]. There seems something right about such a response, to the extent that familiarity seems to play an undeniable role in our ability to survey a given notation - consider the number (in binary) 11110110, the number (in hexadecimal) E6, and the number (in decimal) 246. For most of us, intuitive intelligibility follows only in the decimal notation; that is to say, a grasp of magnitude is only conveyed by the third of the three notations. The number represented in each case, however, is the same; but in terms of the

[9] It is, after all, entirely contingent that we use decimal notation in the first place. (Had Anne Boleyn invented counting, things might have been very different).

number of symbols employed (at least), it appears as though hexadecimal is the least complex.

But while the point about familiarity is well made, it is not clear how this would in itself dissuade us of the idea that complexity is also a relevant factor in our ability to survey. In fact, precisely this kind of example, if we consider stroke notation as well, merely serves to reinforce the point about complexity. Let us try to reconstruct the same number in stroke notation:

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Can this representation convey a grasp of magnitude? Hardly - I cannot even be sure in this case that I have reproduced it effectively - I trust that my computer's copy and paste function is adequate, and that my quick counting check is correct. Moreover, this may give us cause to doubt that familiarity is an adequate criteria – I am, in this case, relatively familiar with stroke notation, and yet the magnitude is not conveyed; I have no intuitive grasp of the number represented. Nor does it look as though any amount of training or habitual use will lead me to recognise the number represented here (represented in decimal by the numerals '246') *when presented in this notation*. It simply looks as though I can reach an intuitive grasp of magnitude in one notation (in this example in Arabic), and not in another (again, in this example, stroke notation).

Hence the complexity of the notation seems to play an undeniable role in our ability to survey representations, and hence, to the admissibility of putative numbers on a strict finitist account. We have already seen the difficulties in adhering to the principle that complexity of notation *determines* the criteria for surveyability – now, here, it seems equally unpromising to deny that it plays *any* role, and to take intuitive grasp of magnitude as our criterion *independent* of the notation used. One cannot divorce the notion of intuitive grasp of magnitude from the complexity of the representation, simply because the representation is involved in conveying (perhaps even in retaining) the intuitive grasp of magnitude.

So where does this leave us? I suggested at the outset of this chapter that there were two strategies that initially presented themselves to the strict finitist – under closer scrutiny, it seems neither option is plausible. Instead, then, I wish to advance a third option as the most promising – a hybrid of the two.

## *Complexity and Magnitude, a combinatorial approach*

What I propose then, is that our criteria for surveyability must contain both of these strands in order to be coherent. Specifically, *the notation must convey an intuitive grasp of magnitude*. Hence, two things must obtain, in order for a number to be surveyable. First, it must be possible (in practice) for the surveyor to possess an intuitive grasp of the magnitude of the number. Secondly, the notation used to represent the number must be adequate to convey this grasp of magnitude.

Let me (re-)define the (strong) criterion as follows: A number is surveyable if and only if an intuitive grasp of magnitude may be conveyed in some notation.

Now, in line with strict finitist ideas, the correct construal of 'in some notation' is presumably in some *actual* notation – i.e. the magnitude may be conveyed *in practice* in some notation. Hence, where our notation(s) are sufficient to convey an intuitive grasp of magnitude, then the numbers represented are surveyable – as long as we are ourselves capable of intuitive grasp of the magnitude in the first place.

Note then that there are two possible failures – failure of the notation, which does not necessarily entail that the number is unsurveyable in some notation (see for example the demonstration above in the case of stroke notation) and failure of our capacity to intuitively grasp the magnitude of the number.

Failure of the notation occurs when the notation is itself insufficient to convey numbers that are nonetheless perfectly intuitively intelligible. For example, consider the case above, where the number 246 is represented in stroke notation. The fact that we are unable to survey the number here is not because we cannot independently grasp the magnitude conveyed by the representation (after all, once the number is conveyed in Arabic notation, we *do* grasp the magnitude), but rather that the representation is simply too complex to convey anything useful. Hence, failure of the notation does not necessarily entail that the number is unsurveyable, just unsurveyable in that notation; so that if there exists another notation (in this case Arabic) in which the grasp of magnitude is conveyed, then the number itself is surveyable.

Failure of our capacity to intuitively grasp the magnitude of a number, on the other hand, will entail that the number represented, however simply, is unsurveyable. If the failure to survey is because the magnitude exceeds our own finite human capability to grasp magnitudes, and not in the notation used in an attempt to convey such, then no alteration (or indeed improvement) of the notation will allow us to attain that grasp. The putative number – that is to say, the arrangement of numerals and symbols intended as a candidate for numberhood – lies without our ability to survey, regardless of notation.

Note, however, that there is nothing here to suggest that the set of surveyable numbers is fixed, as it were, against the adoption of further notations (any more than it is against improvements in our *actual* capacity to survey), *once those notations are realised*. When we, as mathematicians, shifted from stroke notation to Arabic, or adopted the supplementation of exponential notation, much larger numbers became surveyable. And in this lies what I have suggested is the best interpretation of Wittgenstein on this issue. The difficulty, it will be recalled, lies in how to interpret the quote from Wittgenstein that I outlined in a previous section:

"I want to say: if you have a proof-pattern that cannot be taken in, and by a change in notation you turn it into one that can, then you are producing a proof, where there was none before."

Now, by simple substitution, I can illustrate my suggested interpretation for number.

*I* want to say: if you have a number-pattern[10] that cannot be taken in, and by a change in notation you turn it into one that can, then you are producing a number, where there was none before.

[10] Again, here 'number-pattern' means something like an arrangement of numerals and symbols intended as a candidate for numberhood.

Which is just to say, I assert, that if we adopt a notation such that we may attain a grasp of magnitude where there was not one previously, then we produce (in Wittgenstinian parlance at least; the strict finitist will presumably prefer 'construct' here) a number where there was not one before.

Of course, the hybrid model may still seem to possess some unattractive features. It looks, for example, as though even on this model, we are committed to the notion, deemed objectionable previously, that some numbers will not be surveyable in Arabic notation, *per se*, but nonetheless surveyable in *some* notation.

However, this is not so great a problem for the hybrid model as it was for the Surveyability-as-Complexity model, since we have here independent reasons for limiting the *extent* of surveyability, within perfectly finite boundaries. On this model, although the answer to the question 'is this number surveyable' may sometimes require further qualification, dependent upon the notation used, for the majority of cases, the answer will remain independent of notation – on the one hand, in the affirmative, because an intuitive grasp of magnitude is conveyed by any and all existing notations, and on the other, in the negative, because the grasp of magnitude is beyond our capacity as surveyors.

In addition, the hybrid model for surveyability suggested here possesses some attractive features for the strict finitist. On the one hand, by incorporating the third surveyability requirement outlined previously, the hybrid model also retains the desirable result that certain notations, intended only to represent enormous or irrational numbers and thus inadmissible on a strict finitary account (a paradigm example of such a notation is Cantor's notation for transfinite mathematics), will always fail to represent a (surveyable) number since they must always fail to convey an intuitive grasp of magnitude. The infinite cardinals, for example, fail to convey any intuitive sense of magnitude, simply because their magnitude is only *intended* to be relative to one another – no genuine sense of size is conveyed. With regard to the irrationals, examples like $\pi$ and $\sqrt{2}$ will be inadmissible as numbers[11] because no

[11] It is not entirely clear what the strict finitist should say about expressions like $\pi$ and $\sqrt{2}$; one solution might be to suggest that they represent finite relations, rather than numbers themselves. Certainly, when $\pi$ is used in calculation, a finite approximation is always used and not the irrational expressed by the relation 'the number of times the diameter of a circle will fit into its circumference'.

genuine magnitude is conveyed. It is not sufficient that we realise that $\pi$ lies somewhere between 3 and 4 (or even between 3.141 and 3.142) in order to obtain a grasp of its magnitude, for there are an awful lot more numbers of which this is also true.

Certainly, the strict finitist should wish to discount not only transfinite numbers, but also irrational numbers; and so it is an advantage of any account of surveyability that gives us a reason to do so.

Secondly, as promised in an earlier section, the model provides us with the proper resources to attempt an answer to the question as to whether $10^{10^{10}}$ is surveyable or not. The answer may not be simple, but it seems to me to depend upon whether intuitive grasp of the magnitude is possible (in practice).

The notation is clearly sufficiently simple in this case that if we are *capable* of grasping the magnitude, then it will be conveyed by the representation. Now, recall that intuitive grasp of magnitude requires that when the putative number is compared with any other surveyable number, one may identify (for example) the larger. The fact that for many of us, we cannot – without calculation or the aid of a computer – tell which is larger between the number-patterns $9^{9^{11}}$ and $10^{10^{10}}$, does not *rule out* that $10^{10^{10}}$ is surveyable, as it seems likely that $9^{9^{11}}$ is. My instinct is to suggest that $10^{10^{10}}$ is unsurveyable (and hence, on a strict finitary account, inadmissible as a genuine number), because we are incapable of grasping the magnitude. However, I would not like to rule out the possibility that for some mathematicians, the notation genuinely conveys an intuitive grasp, and if so, the number might be admissible for them.

As a result, it may still not be easy to give a decisive answer to the question, since the surveyability of a number is inextricably connected with the capabilities of the surveyor. In order to answer questions about specific numbers, it seems as if we must give an answer as to whether *all* surveyors can grasp a magnitude or not. But clearly, peoples' ability to survey varies – both with the abilities of others, and with their own ability on differing occasions, and under different circumstances.

But now, rather than a question of notation, the issue turns upon one aspect of perhaps the central concern for strict finitism as a whole – that of *vagueness*. It is to a proper discussion of vagueness that I shall turn in the next few chapters.

These remarks conclude the first part of my thesis. I have offered here an account of the commitments and claims of strict finitism. In summary:

Strict Finitism is a foundational theory rooted in a branch of anti-realism known as constructivism. Numbers, proofs, and statements of mathematics are mind-dependent constructions. Strict Finitism suggests that the mind-dependent nature of these constructions places a natural limit upon construction – and a more radical limit than that proposed by the intuitionists. The limit is described in terms of our ability to 'survey'; and I have provided a comprehensive analysis of this concept. Since my primary concern here is number, I have asserted that Wittgenstein's three criteria for the surveyability of a proof may be usefully and relatively straight-forwardly translated to apply to number. Of the three, only the third criterion requires extensive re-interpretation, and I suggest the best way to understand Wittgenstein's strong criterion for surveyability in terms of number is that a putative number (or number-pattern) is surveyable (and hence admissible as a *number*) if and only if its magnitude can be intuitively grasped by a surveyor, where intuitive grasp of magnitude amounts to an understanding of the exact magnitude of the number considered – a precise identification of its place on the number line. Intuitive grasp of the magnitude as a part of the requirement for surveyability will guarantee that given *any* two *surveyable* numbers, we may readily rank them in order of size. I have further discussed the effect of notation upon surveyability, and modified the strong criterion as a result, such that a putative number is surveyable if and only if an intuitive grasp of magnitude can be conveyed *in some notation* to a surveyor.

What I intend to do in the middle section of the thesis is to address some of the major objections to strict finitism. Many of these (although not all) turn on the commitment to a *vague* limit to constructibility, and I would like to end here with precursor note to that debate. In the discussions that follow I shall mostly consider, in connection with the *limit of constructibility*, only the upper limit imposed upon the size of numbers. This is purely to make the discussion easier - intuitively, the progressive sequence of the (natural) numbers makes talk of upper limits readily comprehensible; we are clear where the limit of uni-numeral (one digit) numbers lies on the natural number scale, for example. However, it should be understood that there is an accompanying assumption for (strict) finitist philosophy that the limit of constructibility (surveyability, intelligibility) will apply to other features of mathematical objects in general - the size of a plane, the 'smallness' of a fraction, the complexity of a real number, etc. It may be that the precise entailments of each case warrants further investigation, but for the present thesis, I shall consider all such cases uniformly; as I think it is reasonable to assume that where it is coherent to speak of an upper limit on the constructible size of a natural number, it is coherent to speak of analogous constructible limits on mathematical objects in general.

# PART TWO:
# THE GOOD, THE BAD, AND THE EXCLUDED MIDDLE

## The problems for Strict Finitism addressed

# CHAPTER V: THE SURVEYABILITY DILEMMA

Let us turn our attention now to some of the principle objections to strict finitism as a foundation for mathematics. Many of these objections turn upon the notion of surveyability, as problematic, or even downright inconsistent. The first of these objections that I would like to consider is regarding what Mark Addis has described as the 'psychological and epistemological' issues at play with the notion of surveyability. Addis has three objections to the strict finitist program in his article 'Surveyability and the sorites paradox', and I shall mention the first two in upcoming chapters. For now, I would like to focus on the third objection, which seems peculiar to Addis (in the literature at least), but which nevertheless is exemplary of the kind of misunderstanding common to those resistant to strict finitism. In the final section, of his paper, Addis suggests that surveyability raises a dilemma for the strict finitist, following the question over whether there is a determinate answer to questions about the unexercised capacity for surveying in potential surveyors. Addis writes:

"The question can then be raised as to whether a person can survey a proof at a certain time, even if he is not actually thinking about the proof at that time. The strict finitist is placed in a dilemma by this question. If he claims that the question has an answer, then this implies that there is a fact of the matter about whether the person can survey the proof, even though it cannot be known. This view has the undesirable consequence of entailing counterfactual realism about human beings. . . . If however the strict finitist denies that there is any fact of the matter (independently of the question of whether it is knowable or not), then he is forced to conclude that it is only possible to assert that a proof is or is not surveyable when someone is actually surveying it. Hence the range of applicability of the concept of surveyability is very restricted, and knowing whether a proof is surveyable or not is only possible at certain times." [1]

[1] Addis, p.163-4

Now, I am not convinced that either of the horns of this purported dilemma is very sharp, or indeed that there is really a dilemma here at all. I shall examine each horn in turn.

## Part I - The First horn: Counterfactual Realism

The first horn of Addis' dilemma is as follows: According to Addis, the strict finitist will run into trouble if she asserts that the question 'as to whether a person can survey a proof at a certain time, even if he is not actually thinking about the proof at that time' admits of a determinate answer, because, Addis suggests, this leads to a commitment to what he describes as 'Counterfactual Realism'. Addis offers, as a footnote definition for counterfactual realism, the following:

"Counterfactual realism is the position that there is an actual fact about something which was an unrealized possibility." [2]

### *A Dummettian account of counterfactual realism*

Although Addis dismisses counterfactual realism as 'undesirable', his reasons for doing so are not altogether transparent. Perhaps Addis' problems with counterfactual realism are drawn from an analogy with Dummett's observation that on a realist interpretation, counterfactuals about human attributes such as bravery, if true, would be true in virtue of some 'psychic mechanism'. Dummett's assertion is that a counterfactual involving a non-material conditional such as 'If Jones had been put in danger, he would have reacted bravely' when in fact Jones is dead and was never put in any danger, if true, could only be true in virtue of the fact that Jones was brave, despite Jones never having demonstrated a single brave act.[3] If Addis is following this thought, then the suggestion would be that the counterfactual 'if A had tried to survey proof X even though he did not do so, then he would/would not have

[2] Addis, footnote, p.163
[3] Dummett, 'Realism' – Dummett provides here a comprehensive discussion of the commitments of realists and anti-realists with regard to counterfactual statements of the kind Addis has in mind.

succeeded' could be true only in virtue of some such mechanism; and it is clear that Dummett considers such mechanisms rather dubious, particularly in the case of statements about character. Dummett writes:

"On the realist view, statements about character relate to something which we cannot directly observe, but to the state of which we infer indirectly from a person's behaviour. The situation may be such that, however many facts we knew of the kind which we can directly determine, we should not know whether the statement 'He was brave' was true or false: nevertheless it would necessarily *be* either true or false, since the man's character – conceived of as an inner mechanism which determines his behaviour – must either have included the quality of bravery or have lacked it.

... it is evident that only a philosophically quite naïve person would adopt a realist view of statements about character".[4]

On Dummett's account then[5], psychic mechanisms seem implausible in the case of statements about character – a person's bravery consists surely in the brave acts they have undertaken. However, to return to Addis' claim, I believe that this example is disanalogous to the case of surveyability. For bravery, if we are to accept Dummett's account, may only be defined in terms of brave acts. But surveyability may not only be defined in terms of acts of surveying; rather, our ability as surveyors *can* be described in terms of an intellectual capacity (even, allowing for a broadly physicalist interpretation of mind, in terms of certain physical characteristics of the surveyor). The counterfactual will be true or false in virtue of the mental capabilities of the subject (coupled presumably with certain environmental factors that affect the subject's powers of attention, reasoning, etc.). Dummett dismisses the notion of 'an inner mechanism which determines ... behaviour', but presumably because such an inner mechanism is neither observable nor measurable. Bravery consists in brave acts. However, our capacity to survey, it seems to me, consists not in acts of surveying at all – since we are not free to survey what we choose, and reject the rest: while our capacity for committing brave or cowardly acts is (at least as ordinarily conceived) within our control – I may act bravely on one occasion when the danger is great, with

[4] Dummett, 'Realism', pp.149-50

[5] It is not clear that Dummett is correct here even to suggest that a realist view of statements about character is indefensible – a dispositional account of bravery would be just such a view. But for the sake of exploring the analogy, I shall pass over further remarks here.

cowardice on another when it is not. But my ability to survey does not seem to be a matter of preference, determined simply by what I have chosen to survey in the past; rather, our capacity to survey consists in observable and perhaps even measurable human qualities.

Bravery is a characteristic; one which a life may properly be imagined as lacking. Ability to survey is not such a thing – all intelligent life has some capacity to survey; clearly, our ability to survey depends upon the mental capacity of the individual.[6] Now, for the analogy to proceed, intelligence takes on the role of an unobservable psychic mechanism; and this seems far less plausible than in the case of character (as an inner mechanism).

To put the contrast another way, consider the following. If I do not risk my life to save another today, that does not rule out the possibility of my doing it tomorrow. I do not consider it without my capacity, even if I do not choose to do so today. It seems to me as if I have the potential for bravery, at least until I am no longer capable of action. Perhaps it is plausibly as Dummett suggests, that bravery consists in brave acts, and nothing more mysterious. If I choose to risk my life to save another tomorrow, it is a brave act; I am therefore brave. (Of course, if I commit a good many cowardly acts in life and only a single brave one, this may temper the extent to which I can be considered brave, but for the sake of argument we may assume this is typical behaviour for me). However, if I cannot survey the difference between 17 to the power 1000 and 2 today, it does not seem as though I have the potential to do so tomorrow. Hence my ability to survey does not consist merely in acts of surveying. I am constrained, by very real (albeit vague) limits, as to what I may survey. By comparison, I cannot think of a brave act which I do not have the potential to choose to commit tomorrow. There is no act beyond my capacity for braveness, and indeed

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[6] Indeed, one might be tempted to say that our ability to survey is straight-forwardly *reducible* to the mental capacity of the surveyor. The reason I am shy of doing so is that assuredly there are factors which will affect our ability to survey that do not alter our mental capabilities, per se; if a proof is badly inscribed, for example, or if we are affected by having just performed a similar operation which influences our corresponding ability – say, one that leads us to make a mistaken assumption about the new proof that we might not have made if we had approached this proof first. However, it is clear that the relationship between surveyability and mental ability is a strong one.
I also acknowledge that 'mental capacity' is not a very precise term – for different acts of surveying, it may be more appropriate to speak of 'mathematical ability', or in other cases of 'powers of attention'. I do not use the term as definitive, nor do I intend to suggest that intellectual ability is directly quantifiable by reference to one of these narrower definitions; rather I use it merely as a 'cover-all' for such terms.

on Dummett's account this is because bravery just consists in such acts. But it seems as if we can postulate (and have here done so) putative 'numbers' beyond all our capacity to survey.[7]

What I take, then, to be undesirable about counterfactual realism, is that one needs to postulate something (otherwise) mysterious in virtue of which counterfactuals may be true or false. Dummett suggests that in the case of statements about character, this seems odd, since it involves commitment to unnecessary (in terms of explanatory role) and unobservable psychic mechanisms. Addis is perhaps suggesting that the same follows for statements regarding surveyability; to the extent that the assertion that there is something in virtue of which counterfactuals involving attempts to survey are true or false will lead to commitment to similarly unnecessary and unobservable mechanisms. I argue, however, that in the case of surveyability counterfactuals, the example is disanalogous. Firstly, it seems as if there is a genuine difference between bravery, as a *tendancy*, and surveyability as a *capacity*. The tendancy towards an action is perhaps only demonstrable by recourse to actions of the appropriate kind. A capacity for action, on the other hand, may be perfectly observable. I may have the capacity for cross-country running, without displaying the slightest tendency towards it. If I have never been cross-country running, my claims to have a tendancy to do so are unsubstantiated, but my capacity to do so need not be. Indeed, given that I swim regularly, and have two healthy legs, no-one is likely to take issue with my capacity to do so. Similarly, the capacity to survey is, I say, perfectly demonstrable, indeed *is demonstrated* by all.

There is another important distinction between the cases, which holds even if we were to regard bravery as a capacity, and that is that surveyability is not a simple irreducible capacity, but involves and is made up of a variety of other demonstrable (more general) capacities. Hence, although the task does not look promising for bravery (even as a capacity), we may however infer an individual's capacity to survey from a host of other abilities.

[7] That is to say no more than that we may imagine an operation, such as the successor operation, or some function like the exponential function, taking us well beyond our capacity to survey. The term number applies in the classical sense, but not, obviously, in the strict finitary one.

However, perhaps Dummett's account of counterfactual truth is an unjust analogy. Perhaps Addis' problem stems from a particular notion of counterfactual realism; one which, although not fully defined, may nonetheless cause trouble for the strict finitist. To try to tease out the suggested problem, I shall examine closely the analysis Addis offers us.

Addis' explanation is in terms of different kinds of counterfactuals, the latter two of which are problematic given his account of counterfactual realism. The first kind of counterfactual, Addis calls 'Accessible counterfactuals'. These are counterfactuals that it is possible to check. The second kind, Addis calls 'Inaccessible counterfactuals'. These are those that cannot be checked. The last kind Addis neglects to name, but in the spirit of his analysis I shall call 'Undecided counterfactuals'. It is worth noting at this stage that the analysis is decidedly odd, since it appears that the first two kinds are not only exclusive, but *exhaustive*; so it is not clear how there can be a third kind. Addis does not address the issue, instead offering examples of only the first two of his suggested three kinds, as follows:

"Three sorts of counterfactual can be distinguished:
1. Accessible counterfactuals: where it is possible to check the counterfactual. For example, I can work out 17 – 5.567834 to six decimal places, even though I have not done so.
2. Inaccessible counterfactuals: where it is not possible to check the counterfactual. For instance, consider the case where I attempt to survey the difference between 17 to the power 1000 and 2.
3. [Undecided] Counterfactuals: where it is not known whether they are accessible or not. (This raises the problem of sorites paradoxes about accessibility)." [8]

Addis'assertion is that, while counterfactual realism is non-problematic in the case of accessible counterfactuals, i.e. those which can be checked, it *is* problematic

[8] Addis, p.163

for those which cannot, or for those where it is not known whether they can or can't be checked.

*"Checking" as 'performing the antecedent operation'*

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A lot turns on what Addis means by 'checking' the counterfactual. Let us take it, firstly, that 'checking' means testing in the sense of *simply carrying out the operation contained in the counterfactual*, so that we may later determine whether or not the counterfactual was true/false/indeterminate. If this is the definition, we may suggest the following surveyability counterfactuals (where k is a surveyable number, 2k is not, and m<k), as embodying the ideas Addis wants to put forward:

**($A^s$)** **Accessible**: If Jones had tried to survey a proof of k steps, then he would have succeeded.

**($I^s$)** **Inaccessible**: If Jones had tried to survey a proof of 2k steps, then he would have succeeded.

**($U^s$)** **Undecided**: If Jones had tried to survey a proof of k+m steps (where m<k), then he would have succeeded.

This analysis of 'checking' is perhaps suggested by Addis' examples; for accessible counterfactuals, he reports "I can work out 17 – 5.567834 to six decimal places, even though I have not done so." Presumably, Addis 'can' in the sense that he understands the operation to be a perfectly surveyable one if he were to attempt it presently. Similarly for 'cannot' in the case of inaccessible counterfactuals – if he were to attempt to survey the operation cited in the counterfactual, he understands the operation to be well beyond the bounds of his capabilities. I am not sure why, under this interpretation, Addis would believe inaccessible counterfactuals to be problematic for the strict finitist. Surely, the finitist does not have to be able to attempt an operation to assert that he cannot do so. It is enough that any later check will fail, and that we are convinced of such, to assert that the counterfactual is inaccessible – any check of the operation will fail, to the extent that we cannot survey/perform the operation cited. If we know that a surveyability counterfactual of this type is inaccessible, then we know that the antecedent operation is not performable, and hence *a fortiori* not surveyable – and so the counterfactual has a determinate truth value. Remember that the suggested problem is that of a conflict with counterfactual realism – the idea that there is a fact of the matter about an unrealised possibility. But it seems as though in this case, there is a perfectly good fact of the matter with respect to the counterfactual e.g. 'If I had tried to survey the difference between 17 to the power 1000 and 2, I could have done so.' The counterfactual is just false – and it is false in virtue of my intellectual limits, as described previously.

Addis also believes that there is a problem regarding undecided counterfactuals – but is there? Certainly, the idea that some counterfactuals are not truth apt is incompatible with there being a fact of the matter for those counterfactuals. But remember that our current analysis is not suggesting that undecided counterfactuals are indeterminate at the 'fact of the matter' level – just that *it is not known* whether we may perform the antecedent operation. This is to say nothing about the intrinsic truth value of such counterfactuals.

*"Checking" as 'determining the truth value'*

An alternative explanation of the term 'checking' might be in terms of coming to know the truth value. The problem with counterfactual realism is then a suggested conflict between determinate truth values for counterfactuals, and our not being able - or not knowing whether we are able - to come to know the truth values of those counterfactuals.

Let us begin with some simple counterfactuals, in an attempt to understand why this might be so. I suggest these three will exemplify this analysis:

**(A)** **Accessible**: If I had dropped my pen a moment ago, it would have hit the floor.

**(I)** **Inaccessible**: If Shakespeare had not died on the 23rd April 1616, he would have died on the 30th April 1616.

**(U)** **Undecided**: If my house had been haunted, then I could have seen the ghost.

We can analyse the above counterfactuals in the following way. It is possible for me to come to know the truth of **(A)**, suggests Addis; I can come to know, for example, by dropping my pen a few times right now. As long as I have reproduced the relevant conditions, and by an analysis of a sufficiently similar open conditional ('If I drop my pen now, it will hit the floor') I can arrive at the truth or falsehood of the counterfactual by adopting the determinate truth value of the conditional. However, I cannot come to know the truth (or falsehood) of **(I)**, because there is no way to establish the truth (or falsehood) of it. For this to be a problem, however, it needs to be established that there is a problem with such statements still having determinate truth values. So in virtue of what would **(I)** be true or false? Well, if there were other known facts, such as knowledge of a murderous plot, who planned to murder Shakespeare on the 30th if he had not died in the meantime, then the counterfactual might be true in virtue of this fact. If the plot was set to kill Shakespeare on the 28th, the counterfactual might be false in virtue of the fact. But the problem comes when there is nothing in virtue of which the counterfactual is true. No plots, no terminal condition set to erupt on the 30th, no natural disaster at or around Shakespeare's house on the 30th April 1616. Well, then it might look as if the counterfactual was just false, but it is not certain. It does not seem implausible that if Shakespeare had lived a little longer, he would have lived eight days longer. Since there are no facts by which we could come to know the truth of the counterfactual (the definition of an inaccessible counterfactual), it is odd to suggest that there are nevertheless facts in virtue of which the counterfactual is determinately true or false. The problem can be exacerbated with an examination of our undecided counterfactual. I may or may not come to know the truth of **(U)**. If I manage to go somewhere I know is haunted and not see the ghost, or else if I go somewhere haunted (without necessarily knowing it) and see one, I may come to know that the counterfactual is true (or false). But in virtue of what is it true or false? If true or false, it is so because of certain properties, or lack thereof, of either myself, or ghosts. If I instead ask the question 'If my house had been haunted, could I have seen the ghost?' would it have a determinate answer? If not, then presumably on the grounds that there is no answer to whether I can see ghosts, because their properties are indeterminate; in fact, either there are ghosts, in which case the answer would be yes or no as above (in virtue of the properties of myself/ghosts), or there are not ghosts, in which case there would be nothing in virtue of which an answer could be true or false.

We are here touching on what Dummett refers to as 'simple truth' – the idea that a counterfactual may be true independently of any other facts. Dummett rejects the idea that a counterfactual conditional could be simply true in this way. But while the objection may cause problems for simple cases of the kind described above, surely, for surveyability cases, there is more to be said. I have already suggested that in all cases of surveyability counterfactuals, there is something in virtue of which all such counterfactuals may be more than simply true (or false); namely, the intellectual ability of the surveyors, coupled with local conditions.

Furthermore, it does not seem as if we are going to find simple cases, for each of Addis types of counterfactuals, where surveyability is involved. Let us see why this is the case.

It would be hard to see how one could come to know the truth value of even accessible surveyability counterfactuals – such as **($A^s$)** – *without* attempting the antecedent operation. This will then just lead us into the same considerations that followed the previous analysis of 'checking'. However, this analysis is intended to be purely in terms of coming to know the truth value, so, charitably, perhaps one might devise a method of independently establishing the truth value of accessible counterfactuals without performing the antecedent operation. But what now of **($I^s$)**? Well, it looks as though we *can* come to know the truth value of **($I^s$)**; it is just false. Nor is this simply because I have chosen a favourable example – in Addis' own example, it looks like I can come to know the truth value of the counterfactual "If A had tried to survey the difference between 17 to the power 1000 and 2, then he would have done so"; it is false. How do I come to know it? Because it is clearly without the demonstrable boundaries of anyone's intellectual ability. Accordingly, if we allow that the (perhaps repeated) success or failure of later attempts at the antecedent operation for surveyability counterfactuals is sufficient grounds for arriving at the truth value, (as is purportedly unproblematic in the case of accessible counterfactuals), then it is hard to distinguish any inaccessible surveyability counterfactuals at all. Now, admittedly, because of the boundary cases where sometimes we are successful in our later attempts and sometimes we are not, this does not slam the door on undecided counterfactuals, but it does not seem as if there is an immediate conflict here with the idea that despite the inconclusive evidence of later checks, these undecided counterfactuals just are determinately true or false in virtue of our capacities and local factors at the stipulated time.

Alternatively, it could be argued that *all* counterfactuals are of the inaccessible type[9]. Addis' may have intended to suggest that we can check a counterfactual by later attempting the antecedent operation under sufficiently similar conditions – certainly, for surveyability counterfactuals at least, it is hard to offer a different criterion for checking a counterfactual, whether or not that 'checking' is in terms of simple performance or of coming to know the truth value, as we have seen. But we surely cannot replicate all the conditions to 'test' for surveyability in this way – if we assert that we do not know whether someone is capable of surveying a proof at one particular time, presumably we mean to imply that it is a borderline case for them, and if it is a borderline case then all sorts of local factors will affect their ability, since the ability to survey is not a fixed limit, but a vague one; if the person is wide-awake, healthy, etc., we might assume that they are at optimum surveyability, but we cannot know all the factors – what if a few brain cells have expired between the time stated in the counterfactual, and the time of 'checking'? What if the person surveyed another proof in-between times – this might help, if it was a related proof, or hinder, if it has taken their train of thought away.

In this case, then, there is no division of surveyability counterfactuals. We simply cannot arrive at the truth values for any surveyability counterfactuals, since we can never sufficiently reproduce the conditions under which the counterfactual would be verified. This is perhaps a wider point about counterfactuals in general; it does not seem that we could ever replicate the temporal condition for a given counterfactual, for example. Perhaps for certain types of counterfactual this would not matter. Regardless, the issue for surveyability is much tempered – again, as above, counterfactuals may have determinate truth values despite our inability to come to know them.[10]

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[9] It also occurs to me that, given the relatively loose nature of Addis' definitions, one could probably argue in a similar fashion that all surveyability counterfactuals were simply undecided counterfactuals; but I shall not attempt that here.

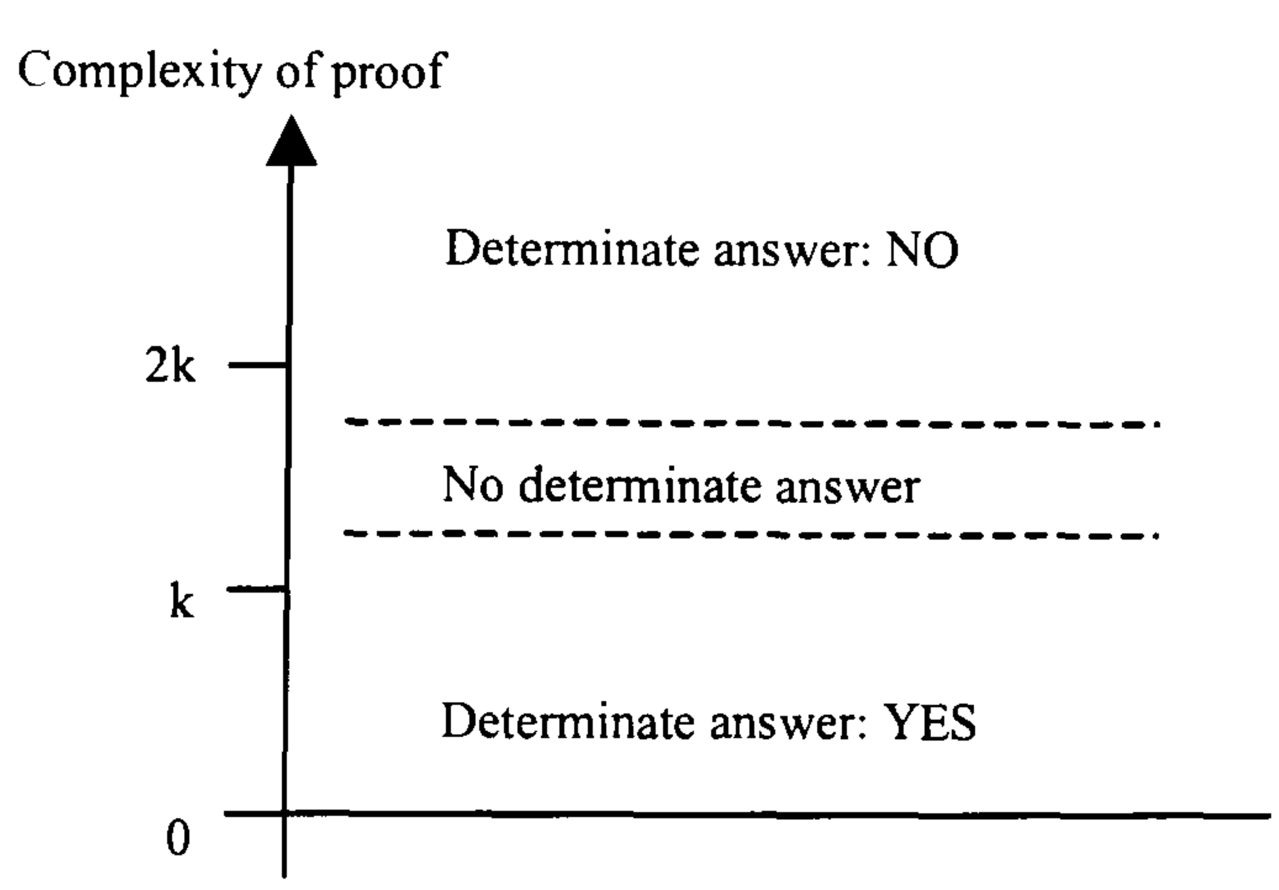
[10] Even though, in normal use, we might *approximate* to knowledge of them – I could still reasonably assert that I know I may survey a proof of three steps, even when I am not attempting to do so at that time. The point is simply that I could not be certain; nonetheless, it would presumably be true.

The solutions offered so far - to the first horn of Addis dilemma - embrace what Addis has called counterfactual realism. It does not seem as though the trappings of counterfactual realism, while potentially problematic, and 'mystical' for accounts of character and behaviour, are immediately disastrous for strict finitism. However, the discussion is not entirely without objection. For some, intelligence as a ground for surveyability may not be convincingly distinct from what Dummett describes as a 'psychic mechanism'. For others, it may seem simply uncomfortable to provide a realist account of truth value for surveyability counterfactuals, whilst on the whole maintaining an anti-realist (in this sense constructivist) position with respect to the numbers and proofs that form the objects to be surveyed. One may argue, however, to this line of thought that such an objection is not much more than a call for general continuity of approach – there is after all no inconsistency here. The realism involved regards *human capacities*, whereas the anti-realism concerns *numbers*.

It might also be objected that such a response rests upon an epistemicist description of vagueness, at least with regard to the vagueness inherent in surveyability. If the strict finitist's answer to Addis' question 'as to whether a person can survey a proof at a certain time, even if he is not actually thinking about the proof at that time' is that such questions admit of a determinate answer, then this is perhaps tantamount to the epistemicist claim that there *is* a determinate answer in all cases, though we cannot (always) know what that answer is. I shall discuss the idea of vagueness (along with the epistemicist solution) in the next chapter, and although I shall conclude there that epistemicism looks unpromising for the strict finitist, I shall offer an account of (a particular form of) strict finitism in chapter eleven which is more in line with the epistemicist view of vagueness.

## Part II – The Second horn: the limited application of surveyability

There is perhaps a more general objection to Addis' dilemma as it stands, resulting from a consideration of the second horn. At first glance, it seems as though Addis is right to suggest that a commitment to there being no determinate answer to the question will lead to a very limited application of the notion of surveyability. But this conclusion follows only if the strict finitist asserts that *all* such questions must be answered in the same way – that is, it can *never* be the case that there is an answer to the question 'can person A survey proof X even though she is not currently doing so?' Addis' point relies on the interpretation of the question as 'whether a person can survey *any* given proof at a certain time if they are not doing so', and not upon individual cases. If the response to the individual cases ('Can person A survey a particular proof 1 . . .' '. . . a particular proof 2 . . .' etc.) is only *sometimes* (or better, *occasionally*) that there is no determinate answer, then the notion of surveyability is not unduly harmed by such an admission (provided, of course, that the occasions in which it is appropriate to assert that there is no determinate answer to the individual question are relatively infrequent when compared with the occasions of questions to which there is a determinate answer). It seems to me that the only manner in which a strict finitist is *likely* to assert that there is no answer to the question is in the case of just such *individual* examples – the paradigm vague examples – and otherwise to assert that there *is* a determinate answer. To make the point as clearly as possible, consider the following model. It is plausible (to borrow from an upcoming example) to imagine a proof of k steps such that k steps is clearly surveyable, but that 2k steps is not. Now, I suggest that the question 'Are all proofs surveyable, even when someone is not attempting to surveying them?' might be best answered by something like the following:[11]




Addis' mistake is in assuming that the strict finitist must answer that there is a determinate answer for all cases, or else that there is not a determinate answer for all

[11] The dotted lines are deliberately not attached to the axis, in an attempt to dileneate only a 'vague region' of cases for which there will be no determinate answer. No attempt is made at this stage to describe precise limits for this region; it is sufficient that k lies clearly and determinately below it, and 2k similarly above it.

cases. But why can the strict finitist not answer that in some cases there is a determinate answer, in others not? The answer to the general question 'as to whether a person can survey a proof at a certain time, even if he is not actually thinking about the proof at that time' is just that it depends upon the actual proof in question. For many proofs, there is a determinate answer, either Yes or No, presumably corresponding to Addis' Accessible and Inaccessible counterfactuals. For others, there is no determinate answer, and these proofs are the 'vague cases'.

We can perhaps demonstrate the problem by analogy – if I ask the question as to whether a person can jump a high jump bar even when they are not attempting to do so, we cannot give one answer for all cases. It depends upon the height of the bar, in a perfectly sensible manner. Some heights are easily jumpable, say a foot from the ground, in which case the answer to the question 'can Jones jump this bar?' has a determinate answer (yes) and some are physically impossible, so that the question also has a determinate answer. For the heights in-between, there is no determinate answer to the question, for precisely the same reason that there is no determinate fact of the matter for the counterfactual 'if Jones had tried to jump that bar, he would have succeeded' if the counterfactual refers to a bar within Jones' vague jumpability boundary. This does not seem problematic – if it was not the case, high jumping would be a very dull event – on the one hand, if jumpability was completely determinate, everybody could jump once, and the winner be conclusively declared, at least until someone who had never jumped before had a go. On the other hand, if jumpability was completely indeterminate, then there would be no professional or world-class high jumpers – you or I might just as well enter the Olympics, since all there would be to high jumping would be 'just having jumped high'.

### *The dilemma refuted*

Addis is asserting that the question 'Could a person survey proof X when she is not actually attempting to do so?' must always have a determinate answer, or else never have a determinate answer. To answer that there is a determinate answer implies that she has an independent (and determinate) capacity for surveyability, which Addis thinks undesirable. To answer that there is no determinate answer implies, (in all cases), that there is nothing more to being a good surveyor than surveying.

I suggest the alternative formulation. The question admits of three possible answers, *dependent upon the proof X*, which is the focus of the counterfactual. The answer Yes asserts that it is within her capacity to do so. The answer No asserts that it is without her capacity to do so. And the answer that there is no determinate answer in such cases asserts that it is within her vague limit to do so.

Nor is it any objection to assert that in answering 'Yes' or 'No' to any single specific question will commit the strict finitist to counterfactual realism as Addis has described – for although this is true, counterfactual realism in this case is assuredly unproblematic for the strict finitist – the position is no longer that all such questions have a definitive answer, just that some of them do – counterfactual realism in this case just boils down to the assertion that counterfactuals are sometimes true. Surely there is nothing objectionable about that.

# CHAPTER VI: VAGUENESS – A GENERAL PROBLEM

Addis' dilemma, then, presents no serious problem for strict finitism. But one pertinent feature of the idea of surveyability that might have led to Addis' analysis, and which is generally seen as a large problem for strict finitism, is commitment to a limit (or at least some limitation) on the numbers: that is to say, as we have seen, the strict finitist is committed to the assertion that the numbers, such as there *are*, are themselves only finite in number. What I have been careful not to suggest in the discussion so far however, and what the strict finitist is usually careful to avoid, is that the numbers may be bounded *at a fixed point*. One of the most common (if not the most sophisticated) objections to the claim that there are only finitely many numbers is simply to ask: what is the biggest number? If the numbers are bounded, which precisely is the last number that lies beneath the boundary? The question is often accompanied by another, intended to demonstrate the absurdity of any response to the first question without further debate: And why can't I just add one?

In this chapter I hope to offer considerations that will ameliorate these problems. I shall introduce the sorites paradox, and discuss the actually rather general problem of vague predicates and their corresponding vague totalities. I shall begin to look at Dummett's influential paper, 'Wang's Paradox', which I shall return to in chapters seven and eight, and which is, as we shall see, both instructive and problematic for the strict finitist. I will also look at some of the standard responses to sorites paradoxes in general, and offer a preliminary assessment of the effectiveness of such responses when deployed on behalf of the strict finitist. Following these general remarks, I shall close this chapter with an examination of the rather more specific idea that sorites paradoxes need not be problematic for the strict finitist because the paradoxes are *themselves* inadmissible on strict finitary grounds.

## *Vagueness identified – the Sorites paradox*

Let us begin with the problematic questions raised above. What *is* the biggest number? Well, firstly let me say that it seems to me that any form of strict finitism that *did* hold that there was a largest number, would also hold that, contrary to intuition, one could *not* simply add one to it – if you could, it would not really be the largest possible number. Since reliable construction is necessary, the largest number is by definition the biggest *possible* construction, and therefore there can be no talk of *adding* one or anything else. Few finitists take this route, however, and are unlikely to accept that there is a largest single number in the first place. The reasons for resisting such a claim are numerous, and some are probably obvious. At the end of chapter four, I touched upon the concern that the ability to survey differs between individuals, and indeed between the same individual on different occasions. How then might we provide an answer for all people, at all times?

The disparity of ability in surveyors is a serious concern for strict finitism, but is in fact simply one aspect of a wider problem, which I have already introduced as the notion of commitment to *vagueness*. The commitment arises in that instead of making a claim that there is a determinate greatest number, the strict finitist will more standardly make the claim that the largest number – or more correctly, the upper limit of surveyable (and hence admissible, or *actual*) numbers – is *vague*: that is, it lies in, or better *constitutes*, a vague region of the traditionally-conceived number line.

Indeed, the disparity in the capacity of individuals to survey is not the only source of indeterminacy in strict finitism. Even if everyone's ability to survey was consistently equivalent, over time and individuals, the question above still looks problematic – why couldn't they simply add one, and come up with a bigger surveyable number? The suggestion here is that the act of adding one to a surveyable number cannot transform it into an unsurveyable one; presumably supported largely by the fact that the act of adding one is itself a perfectly surveyable operation, and that anyway we have a common intuition that there can be no appreciable difference in surveyability between a number and its successor.

This intuition is, I think, inspired by very similar cases, familiar by now in the literature, of occurrences of vague predicates in ordinary language. Is there an appreciable difference in baldness between two men, one of whom has three hairs on his head, while the other has four? Or indeed between another two men, one of whom has four thousand hairs on his head, while the second has four-thousand-and-one? Predicates like 'bald', 'small', 'surveyable', and even those less obvious such as 'red', 'dark', and so on are all considered vague predicates, and they are all susceptible to what is known as the Sorites paradox. The term comes from the Greek word for heap (*soros*) – and the original paradox runs like this:

If you have a heap of sand, then the removal of a single grain is not sufficient to change it into something that is not a heap. But, of course, if you repeat the process often enough, although it must still be true that with no one single grain removal do you effect the change from heap to not-a-heap, eventually you are left with no grains of sand; which of course is certainly (and pre-theoretically) not a heap.

Equally, if the removal of a grain of a single grain of sand cannot effect the change, neither can the addition of a grain to a collection less than a heap; so we might write:

0 grains of sand is not a heap.
For all $n$, if $n$ grains of sand is not a heap, then $n+1$ grains is not a heap.

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For all $n$, $n$ grains of sand is not a heap.

We thus generate the conclusion, from the intuitive premises, that no matter how many grains of sand we add, we will not make a heap, which is contrary to the obvious intuition that a great many grains of sand will easily constitute a heap. One can, of course, construct paradoxes of this type for all such predicates. In the case of surveyability, for example, the paradox runs:

0 is surveyable.
For all $n$, if $n$ is surveyable, then $n+1$ is surveyable.

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For all $n$, $n$ is surveyable.

indicating that all finite numbers (and to be sure, all finite numbers as traditionally – that is, platonistically – conceived) are surveyable. Since this conclusion is at odds with the strict finitist claim that surveyability is finitely upper-bounded, the paradox appears to generate an inconsistency in the heart of the theory.

So far, then, we have established two serious concerns for strict finitism, centring around the problem of vagueness. Firstly that our own individual subjectivity seems to prohibit the categorisation of a number *itself* as surveyable (or not), since it is entirely possible that it will be so to one surveyor and yet not to a second. Secondly, the fact that a Sorites paradox may be constructed for surveyability seems to entail a contradiction for the strict finitist.

The first of these problems, that of the differing capacities of individual surveyors, although it looks to be a source of vagueness, is actually a problem of a different sort, so it would be best to sort out the distinction here before I progress any further. This is really a problem regarding the *relativity* of surveyability to survey*ors*. If there *were* a largest number, such that there was no separate commitment to vagueness at that level, then the fact that some people could survey the largest number and others could not would not be sufficient to introduce vagueness where there was none before. Rather, we would surely say that individual's capacity to survey differed, but in a perfectly determinate sense. We might, as a consequence, generally want to resist statements like 'n is surveyable', and insist that such statements are properly quantified, such as 'n is surveyable by most people', or indeed 'n is surveyable by x'. But we are unlikely to insist that the notion of different capacities of individuals adds an extra level of vagueness when there is vagueness of another sort present, and does not when there is not.

One important question raised by such a response is that if the strict finitist relativises the notion of surveyability in this way, can he resist relativising mathematical truth in a similar fashion? Assuredly, the prospect of relativising mathematical truth looks bleak – we surely want to preserve the generalisability of mathematical statements. I believe that the answer to the question posed here is that the strict finitist can avoid a more sweeping relativisation, simply by taking the meaning of 'n is surveyable' to be something like 'n is surveyable by a surveyor of reasonable competence'. This allows us to retain a relativised notion of surveyability, which accounts for the differing capacities of individual surveyors, whilst still advancing a universal function of surveyability that can issue in generalisable mathematical statements.[1]

There is however still a potential problem with the reformulation of 'n is surveyable' as 'n is surveyable by a surveyor of reasonable competence', in that it looks to entail that particularly gifted surveyors are not doing genuine mathematics. That is, aside from removing the problem of incompetent surveyors, the reformulation looks to remove gifted surveyors as well, which is certainly an unwanted consequence. As a result of this, it might be more plausible to take the meaning of 'n is surveyable' to be 'n is surveyable *by someone*', so that at least one surveyor will suffice. I shall return to this idea seriously later, but for our present purposes it will be enough to note that here too there need be no problem in their being a fact of the matter about whether a particular number is surveyable by someone – whether or not we can actually know this.

Hence, while I take it that the fact that the capacity to survey differs between individuals might be a problem of practical significance when determining the limits of surveyability, it is not however of great theoretical significance to the proper defence of the notion.[2]

What then of the generation of sorites-type paradoxes for surveyability? This, I think, is a more serious problem for strict finitism, and indeed arises out of a more general commitment to vagueness from the strict finitists. In order to see how we might tackle the problem, let us begin by examining some of the standard ways to approach the paradox.

Michael Dummett, in an influential article entitled 'Wang's Paradox' that I shall refer to often in this and the following two chapters, outlines the difficulties of

[1] There is a slight complication here, in that it may be objected that 'reasonable surveyor' might itself be a vague term. I suspect, however, that term is indeterminate only in the sense that we may not be able to know whether an individual is a reasonable surveyor or not; there may be a perfectly determinate fact of the matter (presumably determined by their comparative ability with every other surveyor).

[2] If there is an additional worry here about the fact that we seem to differ in our own capacity to survey at different times, as I mentioned in previous chapters, I trust that the same reasoning may be applied in this case as well. The problem is still one of relativity, but now both to individuals and to conditions. We would, then, take 'n is surveyable' to mean 'n is surveyable by an individual of reasonable competence in ideal conditions'.

rejecting the apparently compelling sorites arguments, of the form presented above. Dummett initially considers the predicate 'is small' for numbers, and the corresponding paradox he attributes to Hao Wang who first presented the particular example. The paradox is generated by the following argument:

(P1) 0 is small

(P2) For all *n*, if *n* is small, then *n*+1 is small:

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(C1) Therefore, every number is small

Dummett then considers the alternatives available if one wishes to resist the paradox. He writes:

"Either premise 2 (the induction step) is not true, or else induction is not a valid method of argument in the presence of vague predicates."[3]

But, he argues, the induction step (that is, premise 2 in the above argument) certainly seems true. It seems intuitively absurd to suggest that the addition of a single unit can transform a number from a small number into one that is not – in just the same way as it would seem absurd to suggest that one hair might make the difference between bald and not bald, as above. Dummett considers the possibility that the rule of universal generalisation fails in the presence of vague predicates: in that we might not be straightforwardly entitled to pass from the truth, for some arbitrary value a, of A(a) (where A(a) is equivalent to the premise 'if a is small, then a+1 is small' in this case, and to the relevant premise in analogous sorites constructions for any vague predicate), to that of $\forall$n A(n). (Which in the current case amounts to '*For every n*, if n is small, n+1 is small'). I will look at this translation more seriously in a later chapter, but Dummett goes on to note that:

"even if we suppose this, we should still be able to derive [by iteration of a finite number of instances, say], for each particular value of n, the conclusion [that] n is small, even though we could not establish the single proposition 'For every n, n is

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[3] Dummett, 'Wang's Paradox', p. 304

small'. And this does not remove the paradox, since for each suitable interpretation of 'small' we can easily name a specific value of n for which the proposition 'n is small' is plainly false." [4]

Dummett's point here is then that in fact, we do not need to rely upon the rule of universal generalisation in order to generate the paradox. Even if we suppose that induction is not a valid method of argument in the presence of vague predicates, we can still derive the conclusion '$n_0$ is small' from the induction basis '0 is small', and a finite number of instances of the induction step, by means of a series of applications of modus ponens. ('If 0 is small, 1 is small', 'If 1 is small, 2 is small' . . .) Each application for a particular value of n (even for very large n), as long as each induction step in the iteration is valid, will demonstrate that n is *small.* That is, by following the sequence:

0 is small.
If 0 is small, then 1 is small.
1 is small.
If 1 is small, then 2 is small.
2 is small.
If 2 is small, then 3 is small.
etc.

until we reach the step involving our desired value of n, we also generate the conclusion that, for our desired value of n, n is small. Obviously, if we choose a number such that intuitively obviously n is not small, we arrive at a contradiction in the same way as with the original paradox as stated above.

*Standard responses to the paradox - epistemicism*

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Various accounts of vagueness standardly attack the induction step as a reliable premise in arguments of this kind. Epistemicists, for example, take the line

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[4] *Ibid.*

that Dummett simply dismisses (as I described above) – to the extent that the addition of a single unit can transform a number from a small number into one that is not; in short, epistemicists maintain that there *is* a precise cut off point for vague predicates, *despite our inability to know where that point lies*. Vagueness is then simply an epistemic problem, about how we come to know precisely what is governed by a predicate and what is not. An epistemicist then will assert that it is simply false that

$\forall n$ (n is small $\rightarrow$ n+1 is small)

precisely because (n is small $\rightarrow$ n+1 is small) does not hold for arbitrary n. In Dummett's example of iterated steps, one of the instances of the conditional is simply false, even if we cannot decide which it is. Indeed, the epistemicist will reject (P2) of the original argument, the induction step, on the grounds that it is just the case that, for example, the addition of a single unit will, for one particular number, transform that number from a small number into one that is not. Equally, two men who have an indiscriminable difference in the number of hairs upon their head may well nonetheless differ in respect of whether they are bald or not, although we may not be at liberty to say *which* two men. Generally, the epistemicist rejects the induction step, and asserts instead (for any vague predicate S):

$\neg\forall(x)\ (\ S(x)\rightarrow S(x')\ )$ (where x' is the successor of x)

Indeed, the epistemicist is committed to the classical equivalence of the negation of the induction step that this entails:

$\exists(x)\ (\ S(x)\ \&\ \neg S(x')\ )$

For some, epistemicism is a simple and elegant way to address the problems of vagueness. It preserves classical logic, and reduces the problem to one of human fallibility. Mark Sainsbury and Timothy Williamson, for example, arguing broadly in favour of epistemicism, conclude:

"we are not aware of any decisive refutation of [the epistemic view], and it would provide a breathtakingly simple solution to sorites paradoxes."[5]

One problem for epistemicism is of course the persistent intuition that it seems simply absurd to suggest that, say, one man may be bald while his neighbour, who has just one more hair on his head, may not be. The epistemicist rejoinder is simply that it seems absurd because *our use* of the predicate is inconsistent – sometimes we use it correctly, and sometimes we don't, because we lack the precise faculty for detecting the correct application of the predicate.[6] Certainly, the mere objection that it seems intuitively unappealing is not enough to cause serious worry for the epistemicist position, but it is one which dissuades many from adopting the position. As far as strict finitism is concerned, it seems the principle problem for adopting an epistemicist position with respect to vagueness is that of providing grounds on which there will be a determinate largest number, even if the need to provide an actual number is alleviated by the unknowability of such a number. In fact I think there is a way to define the largest number, which is not simply arbitrary, and which is in line with the suggestion I outline above regarding the understanding of 'n is surveyable' as 'n is surveyable by at least one surveyor'. I shall return to the idea in earnest in chapter eleven, when I shall consider how a strict finitism of broadly epistemicist-type might be advanced. For now, let us continue with our examination of standard methods of tackling the problem of Sorites paradoxes.

## *Standard responses to the paradox – degrees of truth*

An alternative solution to the paradox is offered by the degree theory of truth. Degree theory suggests that statements may possess differing degrees of truth. It may, for example, be *truer* to say that my elbow is close, than to say that the street outside

[5] Sainsbury & Williamson, "Sorites", in Wright & Hale Eds. *Companion to the Philosophy of Language*, p. 481

[6] I do not mean to imply here that the epistemicist necessarily thinks that, with improved faculties, we *could* determine the cut-off point; some epistemicists (Williamson, for example) hold that the cut-off point is in principle unknowable. My point is simply that the epistemicists claim that we do not, and perhaps cannot, know when the predicate is being applied correctly, except in the obvious cases.

is close, although they are both close to some degree. Hence, it may be truer to say that some number x is surveyable, than to say that 2x is surveyable.

Moreover, on this account, the logical connectives may preserve differing degrees of truth.[7] In particular, a conditional will only be entirely true (that is, possess degree of truth 1) if the consequent possesses a degree of truth at least that of the antecedent.

As a result, the induction step in the Sorites arguments need not be entirely true. If it is truer to suggest that a man with n hairs on his head is bald than to say it of a man with n+1 hairs on his head, (for some particular n) – even though, undoubtedly they will differ by only a tiny degree of truth – then the conditional ceases to be entirely true. Indeed, degree theory stipulates that a conditional has the following degree of truth:

$[p \rightarrow q] = 1$, if $[q] \geq [p]$, $= 1 - ( [p] - [q] )$ otherwise

(where degree of truth 1 represents entire truth, and 0 represents entire falsity).

Hence, if the degree of truth of the consequent is (even marginally) less than the degree of truth of the antecedent, the conditional will fall short of a degree of truth 1. As a result, the conditional

$S(x) \rightarrow S(x')$

will fall short of entire truth, as long as it is (even marginally) less true to say that the successor of a number x is small than to say that a number x is small. Thus (P2) of the original sorites argument need not be entirely true.

Indeed, the argument has more problems under a degree theoretic account of truth. Standardly, on a degree theoretic account, valid arguments must preserve degrees of truth. As Sainsbury and Williamson outline:

[7] For a more comprehensive account of degree theory see for example Forbes, 'Thisness and Vagueness', or Goguen, 'The Logic of inexact Concepts'.

"a valid argument is one such that every model assigns a degree of truth to the conclusion no lower than that assigned to the lowest value premise."

Well, now the iterated steps form of the argument looks to be in trouble too, because under such an interpretation, *modus ponens* is no longer valid. Again, Sainsbury and Williamson provide a useful demonstration of the point:

"For suppose that [p] = 0.9, and [q] = 0.8. Then [p→q] = 0.9. So an argument of the form:

p, p→q ∴q

has a lowest-valued premise of 0.9, and a conclusion valued 0.8."[8]

So what are the prospects of a degree theoretic account of vagueness for the strict finitist? Well, there is at least one good pragmatic reason for the strict finitist to resist a degree theoretic treatment as proposed along the lines presented here. To see this, we need to consider degree theory in a slightly wider context. Rosanna Keefe and Peter Smith[9] describe degree theory as a many-valued theory, grouping it with other theories that introduce one or more new truth-values and develop a multi-valued logic to deal with the problem of vagueness. They continue:

"Many-valued theories usually take one of two options. Either they assign all borderline predications the same intermediate value, to be interpreted as "indeterminate" or "indefinite": this yields a three-valued logic. Or they adopt an infinite-valued logic, with the set of values typically represented by the real numbers in the closed interval [0,1], where 0 corresponds to complete falsity and 1 to complete truth; these values are naturally interpreted as degrees of truth".[10]

Since the latter option describes degree theory, it should not be hard to see why a strict finitist might find it problematic to rely upon such an interpretation; simply because of the commitment to an infinite domain. Clearly, not all real numbers are going to be admissible on a strict finitary account, and so it seems obvious that to

[8] Sainsbury and Williamson, 'Sorites', p. 476

[9] In the introduction to *Vagueness: a reader*, Keefe and Smith 1999.

[10] Keefe and Smith, *Vagueness: a reader*, p. 36.

rely upon a solution to vagueness that requires, for some predicates at least, the closed interval of real numbers between 0 and 1 will be immediately inconsistent for the strict finitist.

This does however raise the question as to whether a multi-valued theory which takes what Keefe and Smith describe above as the first option, that of a three-valued logic,[11] may be usefully employed by the strict finitist. Moreover, there is a third option, although as Keefe and Smith suggest, it is not a common one. There remains the possibility of a finite multi-valued logic, and the corresponding question as to whether a logic with many more than three values, but nonetheless 'finitely-grained', can be adopted by strict finitism. I shall simply defer these questions for the time being. Proper answers require thorough discussion, (and I shall offer such in chapter nine), but it seems first important to establish that strict finitism is worthy of attention in this way; there still remain suggested problems for strict finitism which threaten to render it independently inconsistent, and obviate the need for such deeper analysis.

### *Further attempts to avoid the contradiction*

Although Dummett doesn't discuss either epistemicism or multi-valued theories like degree theory in the current context, he does suggest that attempts to reject classical assumptions within the restricted context of vague predicates seem to oppose the accepted meanings of the terms involved. He argues that we can either deny that the rule of universal instantiation is valid in the presence of vague predicates (insisting that we cannot, for each particular m, derive 'If m is small, then m+1 is small' from 'For every n, if n is small, then n+1 is small') - but this seems to reject the meaning of the word 'every'; or else we can regard modus ponens as invalid in the context (so that we cannot, at least for some values of m, derive 'm+1 is small' from the premises 'if m is small, then m+1 is small' and 'm is small') - but this seems to reject the meaning of the word 'if'.

The only alternative then, continues Dummett, is to deny that an argument, each step of which is valid, is necessarily itself valid in the presence of vague

[11] or indeed an n-valued logic for some quite large n.

predicates. Dummett suggests that this will fare no better than the options just mentioned, since:

"[t]his measure seems, however, in turn, to undermine the whole notion of proof (= chain of valid arguments), and, indeed, to violate the concept of valid argument itself, and hence to be no more open to us than any of the other possibilities we have so far canvassed."[12]

Nonetheless, as Dummett acknowledges, the strict finitist may be tempted to advance just such a strategy to deal with the apparent contradiction. Remember that from the strict finitist's point of view:

"a proof is valid just in case it can in practice be recognised by us as valid; and, when it exceeds a certain length and complexity, that capacity fails."[13]

It seems plausible, therefore, that the strict finitist might simply wish to reject the claim that the conclusion '$n_0$ is small' may be derived from a series of $n_0$ applications of modus ponens - the series might just be too long to effect. Thus the proof of the sorites which takes the 'longhand' form indicated by Dummett will just be unsurveyable *itself*, and hence the paradox, at least with respect to surveyability predicates, is avoided. I shall look at this suggestion in the remainder of this chapter, for it has been seriously considered by various commentators. If it were a successful strategy, it might perhaps allow us to circumvent many of the worries regarding a vague limit to surveyability, both for lengths of proofs and for size of numbers. That is to say that with such a strategy, it might be entirely plausible for the strict finitist to hold as suggested above that the rule of universal generalisation fails, whilst *resisting* the move to proof constructions of the type '0 is surveyable, if 0 is surveyable then 1 is surveyable, 1 is surveyable, etc.'. For proofs of this type will never demonstrate a paradox of the sorites type to the strict finitist, since the proof itself will be unsurveyable. However, it should perhaps also be noted that even if such a response were coherent, it does not seem to be a response that will help with all Sorites paradoxes - take Wang's paradox, for example: it might be objected that the strategy

[12] Dummett, 'Wang's Paradox' p. 306.

will not work here since it is possible to construct the paradox within acceptable limits of number for the strict finitist. The limit of 'small' is presumably lower than that of 'surveyable', and hence a proof of the paradox for small might well be achievable in a perfectly surveyable number of steps. (Given that we may be allowed a relatively 'sharpened' application of *small*, it ought to be possible with relatively few steps). Dummett himself makes a plausible case of this kind[14].

But this objection might not worry the strict finitist too much; the solution of sorites paradoxes in general is not necessarily on the agenda. If an answer could be given to the suggested paradox for *surveyability* in these terms, much of the problem for the strict finitist is ameliorated.

### *The unsurveyability objection and Wright's surveyable proof*

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A much more serious concern for the strict finitist who wishes to claim that sorites proofs are unsurveyable in this way is raised by Crispin Wright. Wright suggests that even though the strict finitist may be entitled to maintain that proofs of the kind suggested by Dummett are unsurveyable, still an argument may be constructed, that is itself perfectly surveyable, to demonstrate a sorites paradox for the strict finitist.

To see how, consider the original objection, which runs something like this: if we construct a (long-hand) proof[15] of the kind Dummett has in mind to demonstrate a sorites paradox for surveyability, it looks something like this:

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[13] *Ibid.*
[14] *Ibid.* Dummett's analysis is in terms of *apodictic* and small numbers, where a number n is apodictic if it is possible for a surveyable proof to contain as many as n steps. But his conclusion amounts to that which I have outlined here.
[15] That is, one which does not rely upon the rule of universal generalisation

A proof of length 1 step is surveyable.
If a proof of length 1 step is surveyable, then a proof of length 2 steps is surveyable.
A proof of length 2 steps is surveyable.

•

•

and so on, until we reach some point at which:
A proof of length n steps is surveyable.

(where n represents a length of proof which is *ex hypothesi* unsurveyable)

But, the strict finitist replies, *this* proof is itself of at least n steps in length[16], and hence is *itself* unsurveyable. Hence, the strict finitist is not bound to accept it as a proof of anything.

Wright's suggestion, however, is that an argument can be constructed on similar lines, (so as not to rely upon the rule of universal generalisation), but which is of a perfectly surveyable length. Wright's argument is as follows:

"It is plausible to suppose that there is some particular number $k$ such that $k$ successive pairwise steps of universal instantiation and Modus Ponens constitute a surveyable proof structure while $2k$ such steps do not. A number $m$ is *small* just in case $m+k$ such pairwise steps constitute a surveyable proof structure. Then 0 is small; and it is plausible to suppose that if $n$ is small, so is its successor; (...). Thus by $k$ pairwise steps of universal instantiation and Modus Ponens we can prove that k+k=2k is small, contrary to hypothesis." [17]

## *The extent of the proof, and Addis' contradiction*

There is however a confusing strand to the argument as presented above, and it has unfortunately been misinterpreted (at least once) as establishing more than it does.

[16] Indeed, as I present it, it is 2n steps in length. Wright, as we shall see, presents his analysis in terms of lengths of *pairwise* steps of universal instantiation and Modus Ponens, and runs the same argument. If I were to represent the current argument in the same terms, we should have a proof of length n pairwise steps.

[17] Wright, "Strict Finitism", in e.g. *Infinity*, ed. Moore, p.257. Addis (as I shall discuss in the next section) also reproduces Wright's argument; I, like he, have for brevity's sake, omitted Wright's qualification of the plausibility of the suggestion that if n is small, so is its successor.

One must be careful to draw only the conclusion that Wright intends from the argument – which is simply that it is not open to the strict finitist to claim that any proof of the sorites paradox for the predicate 'surveyable' will itself be unsurveyable. What the argument does not show is that there is a further contradiction in arguments of this type that will cause special problems for the strict finitist. However, the presence of an error in Wright's description (*not* present in his conclusion) has led at least one commentator to assume the latter. Mark Addis, in attempting to find fault with the strict finitist's construal of surveyability, exemplifies the assumption. Following an account of Wright's argument, he writes in conclusion:

> "when any two surveyable proof structures are added together they produce another surveyable proof structure to the assumption that certain proof structures, such as of length 2k, are unsurveyable." [18]

Addis' point here is not made clearer by the fact that there appears to be a word omitted from the sentence. I take it that something like the word 'contrary' has been missed out of the second line, so that the quote reads that a surveyable proof structure is produced, *contrary* to the assumption that a proof of that length is unsurveyable.

There is a more serious point here, however, and that is that the conclusion is plainly not what Wright's argument is intended to show at all. Wright's intention was to demonstrate that the strict finitist could not avoid the problem of the Sorites by simple appeal to the principle of surveyability; in other words, by asserting that any demonstration of a sorites paradox about surveyability would be a proof structure that was itself *unsurveyable*. What Wright's argument does demonstrate, in place of Addis' somewhat mistaken conclusion, is that *one can describe the sorites paradox inherent in surveyability in a* ***single*** *proof of perfectly surveyable length.*

So it is important to recognise that the argument does not show that we can get from '1 is surveyable' to '2k is surveyable' *using a surveyable proof,* for it is clear that we cannot; that is the motivation behind the original claim Wright offers on

[18] Addis, p.161.

behalf of the strict finitist – the strategy under investigation relies upon it. Instead, the proof offered is one of *just k steps*, which nevertheless demonstrates the presence of a sorites paradox for surveyability. The argument, far from "adding together two surveyable proof structures", uses just one surveyable proof structure to demonstrate the conclusion. It proceeds like this: (each line is a "pairwise step" of modus ponens and universal instantiation, as referred to by Wright):

1) If k proof steps are surveyable, then k+1 proof steps are surveyable (UI); k proof steps are surveyable (the initial premise of the argument)
2) If k+1 proof steps are surveyable, then k+2 proof steps are surveyable (UI); k+1 proof steps are surveyable (from 1 above)

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k) If k+(k-1) proof steps are surveyable, then 2k proof steps are surveyable; k+(k-1) proof steps are surveyable.

Step k generates *the conclusion that* 2k steps are surveyable, which is contrary to the original hypothesis.

Now, as I have suggested, Addis is not entirely to blame for the mistaken conclusion – there is in fact a corresponding error in Wright's argument as presented above. It is clear from the rest of Wright's presentation that the error is an oversight in the simplification of the argument – and indeed Wright is careful to outline precisely the correct conclusion elsewhere. But when he writes "we can prove that k+k=2k is *small*", it seems as though the argument requires two surveyable proof structures to demonstrate the conclusion. But what has in fact been demonstrated by Wright's argument is just that proofs of 2k steps are *surveyable*; and importantly, this has been demonstrated by a proof that is only k steps long. Wright's argument summary should end with: 'Thus by k pairwise steps of universal instantiation and Modus Ponens we can [surveyably] prove that *a proof of length 2k is surveyable*'.

As I have already described, the resultant conclusion is just that the strict finitist may not cry that sorites paradoxes are simply not a problem for surveyability, or at least not on the grounds that there are no demonstrably acceptable proofs of such paradoxes within the finitist's paradigm. However, as Wright also observes, this is not immediately to reject the Strict Finitist's program – otherwise the presence of proofs of sorites paradoxes in general language (such as the proof that all men are bald) would call for the rejection of natural language. This is an important point, and one I think worth stressing. Whatever the prospects are for obtaining a solution to the problem of vagueness for surveyability (and I shall investigate such at length in the chapters to follow), the mere fact that strict finitism is committed to vagueness in some way should not prompt us to reject the theory. As Wright suggests:

> "it is open to the strict finitist to wonder why the involvement of his philosophy with such expressions any more calls its viability into question than its involvement with certain other such expressions calls into question the viability of art criticism."[19]

There is perhaps a further thought here, worthy of mention before I proceed any further into a study of vagueness and surveyability. This is that the presence of sorites paradoxes in the strict finitist discussion of number and proofs may be *more* problematic than the presence of such in natural language because the strict finitist is attempting to mark a definitive boundary or limit, whereas in language no such limit is necessarily required. But I am not sure that this is a convincing thought when properly examined; we may recognise that in ordinary language-use we do indeed impose limits based on sorites-susceptible predicates.

If, for example, I own a nightclub, and I instruct my door staff not to admit bald people, it is not the case that bouncers will admit some people who are definitely not bald, reject some people who are definitely bald, and in some way half-admit the rest. Rather, the bouncers will simply stipulate for every given case (based perhaps, in vague cases, on mutual agreement) whether the individual is to be admitted or not. The predicate will function as a precise limit in terms of admission or non-admission, just as if I had asked the door staff to admit women and not men – that is, some people will not be admitted, others will. Hence the strict finitist limit may be roughly described as something akin to this – admit a proof (or a number) if it is surveyable, don't if it is not.

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[19] Wright, 'Strict Finitism', p.253

Alternatively, perhaps the suggested problem is an ontological one. In making a claim about numbers, the strict finitist seems to be making a claim about what there *is*; vagueness in such a case seems inherently more problematic than in the case of mere categorisation, as in the case of bald or not-bald men. But to raise this kind of objection is to quite wrongly employ a platonistic reading of the strict finitist claim about number. The strict finitist is not offering an account of what there is, objectively, *in the world*; it is important to remember that the claim is a constructivist one. Presumably, there are many plausible constructions (or plausible putative constructions) in the mind – what the strict finitist is attempting is precisely a categorisation of those constructions which are to count as numbers, or as mathematical proof. The rough guide for admittance given in the preceding paragraph with respect to 'proof/number-status' rather than nightclub entry – i.e. admit a proof (or a number) if it is surveyable, don't if it is not – is not a claim about numbers in the world; on any anti-realist account, there is no such thing. Numbers exist only to the extent that they are (or perhaps can be, in some sense) constructed in the mind. Now, if constructibility is to be construed in terms of surveyability, that is, if a number or proof must be surveyable in order for us to be certain that it has been *correctly* constructed, then our varying capacities as surveyors just will give rise to vagueness concerning the limits of construction. But there can be no independent ontological problem here – for the constructivist, numbers possess no ontology beyond that of their being valid constructions in the mind.

# CHAPTER VII: SORITES AND SURVEYABILITY

In the previous chapter, we saw that it is not open to the strict finitist to merely reject the sorites paradox on the ground that any proof of the paradox will be unsurveyable. It seems then as though we must take the problem – and Dummett's corresponding claims of inconsistency – seriously; at least, inasmuch as the problem threatens the theory as a whole: for I have also acknowledged, at the end of the last chapter, Wright's observation that it is not immediately apparent that sorites-susceptibility will be grounds for rejecting a theory, since many ordinary activities involve the use of predicates which are themselves sorites-susceptible. However, there are two avenues of inquiry that immediately present themselves following on from this thought, with perhaps obvious justification: firstly, if we cannot simply reject the claim that surveyability is sorites-susceptible, we must look at the purported entailments of inconsistency which Dummett (and others) offer us; and secondly, we may try to give a convincing account of vagueness at least with respect to surveyability, in the hope that we may ameliorate the concerns of those less willing to consider strict finitism on 'equal-footing', as it were, with practices like everyday colour-ascription. Broadly speaking, the first of these will form the basis for this chapter and the next, while the second task will be properly addressed in chapters nine and ten.

In this chapter, then, we return to Dummett's analysis, in an attempt to deal with his charge of inconsistency. Dummett's charge rests on an analogy with the case of colour-ascription, and I shall both fully outline the analogy and reject it in what follows; in the second half of the chapter I offer two principal ways in which the analogy fails.

## ***Dummett on strict finitary totalities***

Let us return, then, to Dummett's analysis. Dummett is explicit in his account of vagueness that it will entail serious consequences for strict finitism. His article 'Wang's Paradox' is intended, in part, to demonstrate that strict finitism is an untenable position in the philosophy of mathematics. Dummett suggests that the notion of a weakly finite but weakly infinite totality, such as he insists a strict finitist must be committed to, is ultimately inconsistent. We shall see precisely what Dummett intends by this notion, and examine his reasons for claiming inconsistency. I shall suggest that Dummett's remarks, although worth serious consideration for the strict finitist, do not in fact entail the repudiation of strict finitist mathematics as Dummett expects.

I should also like to note at this point that although Dummett's claim that the strict finitist is committed to weakly finite but weakly infinite totalities is plausible, it is open to question, and I shall question it in a later chapter. For now, I shall grant the claim for the sake of argument, and consider its alleged consequences for strict finitism.

Let me begin by carefully outlining Dummett's approach. He opens the article with a briefly explanation of strict finitism, in which he suggests that strict finitism is usually couched in terms of feasible operations. That is, strict finitism is committed to the idea that, as Dummett describes:

'by "natural number" must be understood a number which we are in practice capable of representing.'[1]

This is of course simply the weakest of the surveyability requirements that I have already identified, but Dummett identifies the strict finitist constraint only in this weak sense. He also explains that the natural numbers, under a strict finitist interpretation, are not closed under simple arithmetical operators such as exponentiation.

'The totality of natural numbers which we are capable in practice of representing by an Arabic numeral [for example[2]] is evidently not closed under exponentiation;'[3]

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[1] Dummett, 'Wang's Paradox', p.100

[2] Dummett discusses his reasons for taking the Arabic numerals as a paradigm case in some detail. I have discussed the problem of differing notation at length myself, and will not rehearse the discussion here. For the purposes of the present discussion, let us simply assume that what may be said about the numbers we are capable of representing in Arabic numerals may presumably be said about any other totality we may wish to formulate strict finitistically.

[3] Dummett, 'Wang's Paradox', p.101

The strict finitist is therefore at least committed to totalities that are, for example, not closed under exponentiation (or, for that matter, under the successor operation; more on this later). As Dummett suggests:

'Strict finitism is coherent only if the notion of totalities of this sort is itself coherent'[4]

There is nothing particularly controversial in the discussion so far – but Dummett makes an additional assumption about the kind of totalities to which the strict finitist must be committed; an assumption which seems at least prima facie plausible. He suggests that totalities of this sort are similar to totalities such as 'the number of heartbeats in my childhood'[5].

He describes this kind of totality as weakly-finite but weakly-infinite, as follows:

'Let us characterise a totality as "weakly infinite" if there exists a well-ordering of it with no last member. And let us characterise as "weakly finite" a totality, such that, for some finite ordinal *n*, there exists a well-ordering of it with no *n*th member."[6]

Totalities such as 'the numbers we are capable of representing by an Arabic numeral' are of this kind, argues Dummett, because they are firstly bounded above (weakly finite), to the extent that I can supply a number such that my corresponding heartbeat did not occur in my childhood, and similarly one can supply a putative 'number' (e.g. $10^{10^{10}}$) which is not in the totality of numbers we are capable of representing by an Arabic numeral; and secondly, they have no last member (are weakly infinite) since for every heartbeat that occurred in my childhood, I was still in my childhood when the next one occurred, and similarly, if I can write down a number in Arabic notation I will always be able to write down its successor.

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[4] *ibid.*
[5] Attributing the example to Yesinin-Volpin, who, as a strict finitist, apparently shares Dummett's conclusion that strict finitism must be committed to this particular kind of totality.
[6] Dummett, 'Wang's Paradox', p.109

The supposition that this last point is correct is what I describe above as a plausible assumption, and I shall discuss it further elsewhere. Again, for the purposes of the present analysis let us assume that the totalities proposed by the strict finitist are indeed weakly finite and weakly infinite totalities, in accordance with Dummett's definitions, and examine the argument by which he hopes to establish grounds for the repudiation of strict finitism.

### *Dummett's treatment of the paradox*

Dummett begins his attack on strict finitism with a consideration of the predicates used by the strict finitist, intending to show that the predicates used are vague ones, subject to the same considerations as vague predicates in ordinary language. He suggests that the inherent inconsistency within strict finitist totalities is observable in the kind of Sorites paradoxes constructible for vague predicates. His example is Wang's paradox, which, we may recall, runs as follows:

'0 is small;
For all n, if n is small, n+1 is small:
Therefore all numbers are small.'[7]

Now, it is clear that strict finitism will not couch the totality of natural numbers in terms of numbers that are 'small'; but it is also clear from Dummett's remarks that he intends the paradox as a quite general one, and would presumably endorse the following reformulation:

It is possible in practice to write down the Arabic numeral for 0;
For all n, if it is possible in practice to write down the Arabic numeral for n, then it is possible in practice to write down the Arabic numeral for n+1:
Therefore, it is possible in practice to write down the Arabic numeral for every number.

[7] Dummett, 'Wang's Paradox', p.101

It appears that Dummett believes these paradoxes to be equivalent – and certainly, there is nothing particularly implausible about the premises or conclusion of the reformulation, just as with the given example. Moreover, Dummett argues that vagueness will be a problem for all totalities of this kind; he later concludes, for example:

'The ... totalities which must underlie any strict finitist reconstruction of mathematics must be taken as seriously as the vague predicates of which they are defined to be the extensions.'[8]

Vagueness, Dummett acknowledges, is a quite general problem, not one especially attributed to strict finitist mathematics. In fact, as Dummett admits, even straight-forward acceptance of his argument will lead not only to the rejection of strict finitism, but also to the rejection of phenomenal properties (due to the inherent inconsistency of observational predicates that admit of vague application).

This last point is an important one, and not simply a 'side-effect' of Dummett's analysis. Indeed, his primary conclusion might have been the rejection of phenomenal properties, rather than strict finitism, as he develops his argument not with respect to the predicate 'possible in practice to represent by an Arabic numeral' or even 'is a heartbeat in my childhood', but rather for the predicate 'red', or 'is red'. What we shall see is that the straightforward analogy intended here will in fact simply fail for strict finitist predicates, and I shall describe the important differences between the two cases in detail. However, I shall also go on to consider the idea that Dummett may be making a slightly different claim, to the effect that all vague predicates are infected by an inconsistency that is similar in *kind*, and that the example of colour predicates is simply illustrative of the kind of inconsistency, rather than being intended as an immediately analogous case. Let us begin with Dummett's rejection of observational predicates such as 'red'.

Taking red as a paradigm example of a vague predicate, Dummett develops the conclusion that the use of observational predicates, where the source of vagueness is the non-transitivity of non-discriminable difference, is intrinsically inconsistent.

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[8] Dummett, 'Wang's Paradox', p.115

Dummett rejects observational predicates such as 'red' in two steps. Firstly, he demonstrates that the source of vagueness for such predicates lies in the non-transitivity of non-discriminable difference. That is to say, predicates such as 'red' are not "independently adjudicable"; something is red because we assign the property of 'redness' to it, and that assignment is necessarily vague because for subtle enough changes of shade we cannot tell the difference between a shade and its immediate neighbour. We can look at two different shades, one very close to another, and we may not tell the difference merely by looking. This non-discriminable difference is non-transitive because we may say there is no difference between shade 01 and shade 02, nor between 02 and 03, and yet apparently hold quite consistently that there is a difference between shade 01 and shade 03. However, (and this leads neatly to Dummett's second step), 'red' is just such a predicate, whose application is determined precisely by looking. As Dummett outlines, in the case of vague predicates such as 'red', I am bound by the principle that if shade n is red (looks red to me) then shade n+1, which I cannot discern any difference in, is also red (also looks red to me). But because non-discriminable difference is non-transitive, this principle will fail to consistently govern the use of 'red' – I can be forced to assert contrary statements, to the effect that a particular shade is both red and not red. I must admit that if shade 01 looks red, then shade 02 looks red, and equally if shade 02 looks red then shade 03 looks red, and so on. Ultimately, I must conclude (even for shade 03 in my above simplified example) that something which is (otherwise obviously) not red *is* red, given, say, a sufficiently long series of shades from red to blue, each one of which is non-discriminable from its neighbours. According to Dummett it follows that:

'the use of any predicate which is taken as being governed by such a principle is potentially inconsistent: the inconsistency fails to come to light only because the principle is never sufficiently pressed. Thus . . . the use of vague predicates – at least when the source of the vagueness is the non-transitivity of a relation of non-discriminable difference – is intrinsically incoherent.'[9]

[9] *ibid.*

I would just like to point out that Dummett's position on observational predicates is controversial – the suggestion is that the use of colour predicates (for example) is governed entirely by this 'observational' principle that Dummett has laid out. As we shall see, it is not clear that the use of all vague predicates needs to be governed by such 'observational' principles – but moreover, it may be that we do not need to accept that even colour predicates are governed in this way; a straight epistemicist response, for example, would demand that the *correct* use of the predicate was governed by properties (presumably) of objects. Dummett's conclusions regarding the inconsistency of such predicates relies upon the fact, at least for colour predicates, that to be a certain colour is just to *look* a certain colour. It seems to me that a more general defence (of the kind of phenomenal properties that Dummett is admittedly forced to reject) might start with some response of this kind; however, there are a number of considerations in favour of strict finitism that will eclipse such issues, and I need not attempt the more general (and assuredly hazardous) defence here. I want to move swiftly onto Dummett's remarks about strict finitism. On the strength of his remarks regarding observational predicates, where the source of the vagueness is the non-transitivity of non-discriminable difference, Dummett believes he is in a position to reject strict finitism.

Dummett summarises his conclusions in the following way.

'(1) Where non-discriminable difference is non-transitive, observational predicates are necessarily vague.
(2) Moreover, in this case, the use of such predicates is intrinsically incoherent.
(3) Wang's paradox merely reflects this inconsistency . . .
(4) The weakly infinite [no last member] totalities which must underlie any strict finitist reconstruction of mathematics must be taken as seriously as the vague predicates of which they are defined to be extensions . . . on the strength of conclusion 2, weakly infinite totalities may likewise be rejected as spurious; this of course entails the repudiation of strict finitism'.[10]

It is clear from this that Dummett intends us to understand an immediate connection between the kind of examples he has been discussing, and the predicates

[10] Dummett, 'Wang's Paradox', p.115

used in strict finitist analysis. But I am not convinced that such an immediate connection exists. I think that it is essential to Dummett's conclusions that he has been discussing observational predicates, where the source of vagueness is the non-transitivity of non-discriminable difference. I maintain that it is possible to reject the assumption that predicates of the kind 'possible to write down in practice' are of the same species; in fact, I suggest that they are neither observational predicates, nor is the root of their vagueness the non-transitivity of non-discriminable difference. I shall outline this precisely below. However, perhaps more seriously for Dummett, I believe that any proposed *analogy* of the argument will fail to achieve the 'repudiation of strict finitism', because of the fact that Dummett's argument involves a subtle equivocation, as I shall also aim to show in the next chapter.

### ***Rejecting the analogy***

Firstly, then, I want to consider the rejection of strict finitary predicates in line with Dummett's rejection of observational predicates like 'red'. As I have already remarked, it is not clear that the step is an automatic one; for two reasons. I shall discuss them in turn.

#### *a) Strict finitary predicates need not be observational predicates*

Dummett's argument, at face value at least, relies upon the fact that the vague predicates in question are observational ones. The use of such predicates runs into inconsistency, according to Dummett, because it is possible to derive contrary statements, e.g. x is red and x is not red. But it only leads to inconsistency in the case of red because we are bound to accept the premises that, on the one hand, something which looks definitely blue (i.e. is not red) to us *is* blue (and not red), and on the other hand, as Dummett's principle describes, that if we cannot tell the difference between a shade which is red and another, we must agree that the second shade is also red. However, although we may accept the first premise without concern, we will only accept the second premise so long as 'being red' and 'looking red' are synonymous. Consider the following simplification:

(1) Shade 01 is (clearly looks) red.

(2) Shade 05 is (clearly looks) blue.

(3) We (being in this case very poor colour discriminators) cannot tell the difference between any two shades next to one another in the sequence (that is we cannot tell the difference between 01 & 02, or 02 & 03, or 03 & 04, or 04 & 05).

(4) If one shade is indistinguishable in colour from another shade, the shades must be the same colour.

(5) Therefore, according to (3), 01 is the same colour as 02, 02 is the same colour as 03, 03 is the same colour as 04, and 04 is the same colour as 05.

(6) Hence, 01 is the same colour as 05, contrary to premises (1) and (2).

Now, the problem comes with (4) – this assumes that there is nothing more to something *being* a certain colour than it *looking* a certain colour. Certainly it may be plausible enough, in the case of colour, that the use of the predicate, as Dummett says, is governed by this principle. As I have said, the arguments for colour predicates are somewhat moot – I have no easy answer here. But the distinction I wish to draw is important to the strict finitist's case. Let us consider another predicate, say 'is one metre long' and see if the same argument will lead us immediately into inconsistency.

In this case, it seems not. We do not, even intuitively for such predicates, wish to maintain clause (4) above. The argument becomes:

(1a) Stick 01 is one hundred centimetres long.

(2a) Stick 05 is one hundred and one centimetres long.

(3a) We cannot tell the difference between the lengths of any two sticks next to one another in the sequence (that is we cannot tell the difference between 01 and 02, or 02 and 03, or 03 and 04, or 04 and 05).

(4a) If one stick is indistinguishable in length from another stick, both sticks must be the same length.

(5a) Therefore, according to (3), 01 is the same length as 02, 02 is the same length as 03, 03 is the same length as 04, and 04 is the same length as 05.

(6a) Hence, 01 is the same length as 05, contrary to premises (1) and (2).

Now (4a) looks plainly false. Note that in this case, we do not need more than 2 sticks for the example. The fact that we cannot distinguish between the length of stick 01 and stick 02 does not force us even to conclude that these two sticks are the same length – in fact, given the experiment, we would happily conclude exactly the opposite, despite the evidence of our senses. What is importantly different in the case of colour (and what Dummett more generally refers to as *observational*) predicates is that the evidence of our senses is supposed to count in the relevant way – it is supposed to govern what is, and is not, red.

In the case of non-observational predicates, it does not appear as if inconsistency follows.

Now, admittedly, predicates such as 'is one metre long' are not vague. They have a perfectly determinate application. Nonetheless, is it impossible that vague predicates could be non-observational (in this sense) and still vague? Predicates such as 'is (or was) a heartbeat in my childhood' have vague application, but it does not follow that the predicate is an observational one. The application of the predicate is not *defined* by the principle that if I say one heartbeat occurred in my childhood, and I cannot see any difference between that and the next, then the next occurred also in my childhood. But the correct (and assuredly vague) application of the predicate is determined by the (intrinsically vague) length of my childhood, something for which I can claim very little responsibility.

We could of course run a similar argument for 'seems like a heartbeat in my childhood', or 'looks one metre long', which then *would* be observational predicates, and run into inconsistency in the way Dummett proposes; but this says nothing about the predicate '*was* a heartbeat in my childhood' or '*is* one metre long'. Dummett's conclusions about the predicate 'is red' stem from his assumption that 'is red' is synonymous with 'looks red'.

Again, I shall not debate that issue here. The important question is whether the predicates that strict finitism is committed to are observational predicates (in the same way as Dummett proposes 'is red' is), or predicates of a non-observational kind, such as 'is one metre long'.

My contention, of course, is that they are the latter. We must be careful not to assume that predicates like 'possible in practice to represent by an Arabic numeral' are synonymous with predicates like 'seems possible in practice to represent by an Arabic numeral'.[11] Of course, we are talking theoretically about such matters, (since the proposed breakdown of possibility is not accessible in the way that the transition between red and, say, orange is, since we can have no examples of impossible representations), but it seems as though external factors will play a very crucial role in the determination of applicability; understood simply (perhaps most simply) as a physical task, for example, we will actually run out of room (or time) in which to inscribe suitably large numbers. Again, the limit might not be precisely determinate (i.e. is vague), but nevertheless, it seems inevitable that it will come.

If this is accepted, we can see immediately that the proposed reformulation of Wang's paradox from using the predicate 'small' to using the predicate 'possible in practice to represent by an Arabic numeral' is not as simple as it may first appear. Arguably, 'Small' may be an observational predicate in the relevant sense – but only if the application of 'small' is governed by what *seems* small to us, rather than asserting something which is genuinely true of, say, a number. There is some support for this suggestion, since 'small' seems context-dependent, such that thirty-five grains of sand will seem a small amount, but a class of thirty-five students will not seem small. Nonetheless, it seems to me as if, at least with respect to numbers, (as in the context of Wang's paradox), some numbers *are* genuinely small, and do not merely *seem* small in certain contexts. So perhaps one might make a similar case that 'small' is not an observational predicate. However, for my present purposes, it is enough to note that (even) if 'small' can be counted an observational predicate, then Dummett's suggestion that another interpretation of 'n is small' is 'it is possible in practice to write down the Arabic numeral for n' is just incorrect; as in this case the former would be an observational predicate, but the latter is not.

[11] There is a side issue here about 'surveyability'. Mark Addis (in his paper 'Surveyability and the Sorites paradox') makes the valid point that we must be careful not to ascribe 'surveyability' as a property of numbers (or proofs, etc.). But just because surveyability depends upon *us* in an important way, it does not follow that we are wholly responsible for adjudicating what is surveyable and what is not. Indeed, in the case of surveyability, it seems as though the concept rests upon the fact that we are not – surveyability is intended to convey a limit to human capacity. If it were determinable by the surveyor, it would be a redundant term.

Dummett is not entirely unaware of the distinction between predicates which involve observational vagueness, and those that do not. He suggests that:

'if we are to have terms whose application is to be determined by mere observation, these terms must necessarily be vague.'[12]

But his mistake is to assume that all *non*-observational predicates which give rise to vagueness do so in virtue of translation into an observational predicate of the kind 'looks . . .' or 'seems . . .' or some such. Dummett attributes vagueness to observational predicates, where the source of the vagueness is the non-transitivity of non-discriminable difference. He allows that there will be examples where the non-transitivity of non-discriminable difference seems to offer a paradox, but on closer inspection, (as with his example of the clock hand) the predicate actually determines a perfectly determinate totality, one that is not vague. That is, Dummett agrees that there are 'cases of non-discriminable difference which give rise to vague predicates [and] ones which do not.'[13] What he does not countenance is that there could be non-observational predicates which are vague, but where the source of vagueness (importantly) may not lie in the non-transitivity of non-discriminable difference.

b) *It is not clear that the source of vagueness, for strict finitary predicates, is the non-transitivity of non-discriminable difference.*

Since we can no longer assume that strict finitary predicates are observational, it no longer follows that the source of vagueness lies in the non-transitivity of non-discriminable difference for such predicates. If there are non-observational vague predicates, as will be the strict finitist contention in reaction to Dummett here, then their application is no longer governed by the observational principle – hence the vagueness need not rest in our inability to recognize observational differences.

Let us again reconstruct the argument for the predicate 'is possible in practice to represent by an Arabic numeral'. The argument, designed to show inconsistency, would run:

[12] Dummett, 'Wang's Paradox', p.112
[13] Dummett, 'Wang's Paradox', p.114

(1b) Number 01 is inscribable, in practice, in Arabic notation.

(2b) Number 05 is not inscribable, in practice, in Arabic notation.

(3b) We cannot tell the difference between the 'inscribability' of any two numbers next to one another in the sequence (that is we cannot tell the difference between 01 and 02, or 02 and 03, or 03 and 04, or 04 and 05 *in terms of inscribability*).

(4b) If one number is indistinguishable in inscribability from another number, both numbers must be equally inscribable.

(5b) Therefore, according to (3), 01 is as inscribable as 02, 02 is as inscribable as 03, 03 is as inscribable as 04, and 04 is as inscribable as 05.

(6b) Hence, 01 is as inscribable as 05, which is contrary to premises (1) and (2).

We are of course firstly at liberty to maintain that 4b) is false, if, as suggested above, inscribability does not depend upon our ability to recognize it. However, it also seems we could make a different objection here as well, to the effect that there just *is* a discernable difference between any two (different) numbers, regardless of the increment between them. Consider the argument above for the predicate 'is inscribable in stroke notation', for the domain of the positive integers. Now we might insist that for any two numbers, even if we take '1' and '2' (or '|' and '||'), there is clearly a difference in inscribability. The above argument will not proceed, because we will not agree with stage 3b) for any two numbers (as long as they differ in size). In fact there is an apparent complication with 'inscribable in Arabic notation', since Arabic notation works in base 10, an 'artificial' counting system. The suggestion here may be easier to grasp for a predicate like 'inscribable in stroke notation'. But the force of the argument is perhaps excellently illustrated by Arabic notation – although, in one sense, we are bound to say that there is no difference in 'inscribability' (in terms of time, effort, length, etc.) between the number '2' and '3', we are hardly likely to say the same for '9' and '10'.[14]

[14] Note here it still seems implausible that there will be a sharp cut-off (say, for example, beyond a certain 'numeral-length') to the inscribability of numbers; because the limit is *vague*.

It must be acknowledged that this response will not suffice without modification for 'inscribable in Arabic notation' – as suggested above, base 10 complicates matters when considering the equi-scribability of numbers. It looks as though we might make such an argument for stroke notation, since clearly it will take us longer to inscribe larger numbers. However, it does not seem obvious that it will take us longer to inscribe the numeral '8' than '1', for example, so we need to have a broader understanding of the kind of predicate advanced by strict finitism. What does 'possible in practice to represent by an Arabic numeral' require, at numbers above the obvious? One possibility is the notion of surveyability. A numeral, in order to be a meaningful representation, must presumably be recognizable as such – and this is where the notion of surveyability usually comes into play. If we cannot recognize whether a number has actually been represented or not, we have no guarantee that the number is in fact representable.

Now, surveyability has neither the obviously favourable properties of stroke notation, (in that it seems obvious to us that higher numbers will always take 'longer' – i.e. there is no non-discriminable difference), nor the immediate worries of Arabic notation (in that it seems clear that some numbers are prima facie as 'inscribable as others of the same order of magnitude). So what are we to say about the predicate 'surveyable'? Firstly, we might simply insist that the argument here is true for surveyability, just as for inscribability in stroke notation. The fact that we are able to survey both the number '1' and the number '45' does not mean we are incapable of recognizing a difference in the attention required to do so – why may not the same be true of the numbers '5' and '6', for example? The suggestion here is that we can always recognize a difference in surveyability between any two numbers, even when those two numbers are next to one another in the sequence.[15] If this suggestion is the correct one, there will be no cases of non-discriminable difference for surveyability – each number will be discriminably more or less surveyable than any other.

[15] Of course, this will not be the case if surveyability is defined in 'black and white' terms – to the effect that there is only ever a difference in surveyability between two numbers if one number is surveyable and the other is not. But as with all of the predicates we are considering, this would be too narrow a definition of the predicate to be of use to the strict finitist, or anybody else. Surely, we can sometimes struggle with (for example) a proof, spending hours studying it before we finally understand (and have thus surveyed) it; such cases, contrasted with the simplest of proofs that seem all but obvious to us on inspection, are taken to establish that there just is a difference in surveyability between some cases of nonetheless 'surveyable' (as opposed to 'not-surveyable') examples.

However, one might argue that there is nothing to substantiate this claim, and that actually there is a counter-intuition, for cases like 'a number and its immediate successor', where it seems perfectly plausible to maintain that the two seem equi-surveyable; that is, there is a non-discriminable difference between them. But then the force of our earlier argument regarding observational and non-observational predicates may be felt again – it does not seem, in the case of surveyable, that it need matter what our discriminative capabilities are: whatever our intuition, it may perfectly well still *be more difficult for us* to survey, say, the number '2' than the number '1'.

The vagueness in all of the cases we are advancing on behalf of the strict finitist, (be it 'surveyable', 'possible in practice to represent by an Arabic numeral', 'inscribable in stroke notation', etc.), does not lie in the non-transitivity of non-discriminable difference – instead, what is vague is the limit to our capacities.

As a result then, my interim conclusion is that Dummett's assumed parallel between the cases of observational predicates such as red, and the predicates endorsed by strict finitism, fails on two counts. Dummett rejects strict finitism on the strength of his first two conclusions, which read (as above):

'(1) Where non-discriminable difference is non-transitive, observational predicates are necessarily vague.
(2) Moreover, in this case, the use of such predicates is intrinsically incoherent.'

Strict finitist predicates do not count as 'such predicates' because they are neither observational, nor is the source of their vagueness the non-transitivity of non-discriminable difference.

However, as I suggested earlier, there may be another way to interpret Dummett's remarks. Dummett's discussion is nonetheless intended to cover *all* cases of vague predicates, and perhaps he does not need to argue that strict finitary predicates are inconsistent *because of* his argument against phenomenal, observational predicates, but rather that they are merely inconsistent in a *similar* way. I have suggested that any analogous claims will also fail due to an essential equivocation in Dummett's discussion of weakly finite and weakly infinite totalities. I would like now to explore this further, and hope to substantiate the claim.

Let us recall Dummett's earlier conclusion that 'the use of any predicate which is taken as being governed by such a principle is potentially inconsistent'. It would be useful to establish, for the case of strict finitism, answers to the following questions: just what is the principle that is supposed to govern predicates such as 'surveyable', 'inscribable', or perhaps 'representable in Arabic notation', and is it indeed potentially inconsistent?

However, it is not clear how we should proceed in finding such a principle to assess. We have already seen that a direct 'translation' of the principle – to the effect that e.g. a similar principle for 'inscribable in stroke notation' becomes 'if I cannot discern any difference between the inscribability of *a* and the inscribability of *b*, and I have characterised *a* as inscribable, then I am bound to accept a characterisation of *b* as inscribable' – will fail to demonstrate inconsistency because it simply does not appear as if the use of the predicate *is* governed by this principle. Dummett's test of inconsistency will not work for strict finitary predicates, if they are not wholly observational; since, again, his argument against colour predicates depends upon the synonymity of e.g. 'red' and 'looks red'.

Dummett certainly provides us with no equivalent principle. But what is clear is that Dummett believes there to be an intrinsic inconsistency in all totalities of the weakly finite and weakly infinite kind – of which presumably, now, observational predicates are simply a special case. Therefore, I shall turn my attention next to a general discussion of weakly finite and weakly infinite totalities, in which I hope to argue against the claims that commitment to such totalities will lead to inconsistency.

# CHAPTER VIII: WEAKLY FINITE AND WEAKLY INFINITE TOTALITIES

Dummett objects principally to all totalities that are weakly finite and weakly infinite in kind, because, he maintains, commitment to one aspect will lead to inconsistency when combined with the other. In short, Dummett maintains that no totalities may be consistently both weakly finite *and* weakly infinite. I argue, on the other hand, that the strict finitist is at liberty to ascribe to the existence of such totalities, since no such inconsistency is apparent. As I shall attempt to show in what follows, Dummett is guilty of not taking the strict finitist commitment seriously enough in his analysis. In fact, Dummett isn't the only commentator to suggest that weakly finite and weakly infinite totalities are inconsistent; a related, more obvious error is to be found in "Surveyability and the sorites paradox" by Mark Addis, who attempts a similar rejection. Although Addis' objection is different from Dummett's and more easily answered, the two objections are broadly similar in character. Exposing the error underlying Addis' objection may help to illuminate the subtler flaw in Dummett's. Essentially, both make the mistake of reading weakly infinite and weakly finite as too harsh a requirement – but where Dummett ultimately (and incorrectly) reduces weakly finite to mean simply 'finite', Addis collapses weakly infinite to just 'infinite'.

In this chapter I shall offer an account of both objections, and also my rejection of each of them. As a result, I hope to show that commitment to weakly infinite and weakly finite totalities amounts to nothing more problematic than a general commitment to vagueness on behalf of strict finitism, and certainly not to some internal inconsistency arising from combining the two defined properties of such sets. I shall begin with an outline of Addis' equivocation first then, in the hope that it will make the related mistake in Dummett more apparent.

## *Weakly Finite, and Weakly Infinite*

Before I begin, however, it would first be prudent to establish precisely what is meant by the terms involved in the concept, actually identified by Dummett himself, of 'weakly finite and weakly infinite totalities'. Firstly, perhaps obviously, a totality can be thought much like a set – as the extension of a predicate, for example (as in the kind of examples I shall consider). There is an important reason why the strict finitist will prefer the term 'totality'; the notion of a completed set is one in common usage – the strict finitist will want to resist commitment to completed (infinite) sets, and the term 'totality' has no such connotation. I shall therefore often prefer the word 'totality', on the understanding that whatever applies to totalities applies to sets, but not necessarily *vice versa*. Totalities may be determinate or indeterminate, and at least finite. "Is a postgraduate in Glasgow's philosophy department" is a predicate which specifies a determinate totality, and one which, as I understand it, is ordinarily finite – that is to say, the predicate has only finitely many instances, and hence the totality has only finitely many members. Much more contentiously, the classical (Platonist) view of the numbers suggests an infinite totality (indeed a completed set) – one with infinitely many members. However, for the strict finitist, who will not countenance talk of infinite numbers, there will obviously not be infinite totalities in this sense. A strict finitist may well admit totalities that are weakly infinite, however; in precisely the vague cases under discussion.

Recall Dummett's definition of "weakly infinite and weakly finite totalities". To be clear, that is totalities that are *both* weakly infinite, and, at the same time, weakly finite. His definitions read as follows:

"Let us characterise a totality as 'weakly infinite' if there exists a well-ordering of it with no last member. And let us characterise as 'weakly finite' a totality such that, for some finite ordinal *n*, there exists a well ordering of it with no *n*th member." [1]

The idea is that such a totality may be 'open-ended', in the sense that there is no last member, and yet 'bounded-above', such that there is a point which it certainly does not reach. As I have already indicated, it is the contention then of both Dummett and Addis that a totality that is both weakly finite and weakly infinite (and hence the strict finitary totalities among them) cannot really be coherent. Both suggest (Addis

[1] Dummett, 'Wang's Paradox', p. 312

explicitly, Dummett implicitly) that the notion of weakly finite cannot be held simultaneously with the notion of weakly infinite. As I have suggested, the principle aim of this chapter will be to establish that certainly Addis' conclusion, but also, I maintain, Dummett's, rests upon a mistaken interpretation of these notions, and a simple fallacy of equivocation. I shall begin with Addis.

*Addis' charge of Inconsistency*

Addis opens his argument with what he describes as the Wittgenstinian distinction between 'intensional' and 'extensional' specification, although this is evidently an inaccurate attribution, as the idea is already found in Russell's *Principles of Mathematics* (1903). The distinction itself requires a little additional definition before we proceed. In Addis' words:

"An 'intensional specification of a set' is one in which a rule is given for generating the set, or some general characteristic for set membership. An 'extensional specification' is one that consists in giving a list of its members." [2]

The reservation I have expressed towards talking about sets and not totalities need not affect the discussion here, I think – I take it that the strict finitist will accept everything that Addis says about (and is able to establish with regard to) sets in the present discussion as holding for totalities as well.

Addis argues that since we can give only intensional definitions of infinite sets, a weakly infinite set must therefore be specified intensionally. Addis also seems to suggest that in general, a finite set may *not* be specified simply intensionally:

"A finite set . . . can be listed and it is insignificant that the list is not, or could not be governed by any specific rules." [3]

[2] Addis, 'Surveyability and the sorites paradox' p.159
[3] *ibid.*

To be fair to Addis, he does acknowledge that we can *sometimes* give intensional specifications of finite sets, but believes this is only possible when, as he describes, the intensional specification is *equivalent* to the extensional specification:

"In the case of finite sets, there will sometimes be a rule or condition for set membership that can be specified extensionally, that is, by giving a list of members and the intensional specification of the set is equivalent to the extensional specification". [4]

Weakly finite sets must therefore, as I understand Addis to be asserting, only be specified wholly extensionally. He further claims:

"The totality can be described either in terms of its finite or its infinite aspects, and Wittgenstein's contention is that both notions cannot be held at once." [5]

Addis' argument is then essentially a development of what he takes to be an objection of Wittgenstein's against the coherence of intensional and extensional sets. Addis' contention is that weakly infinite sets (or totalities) must be specified intensionally, and weakly finite ones must be specified extensionally. From this, Addis arrives at the conclusion that since we cannot specify a totality both intensionally and extensionally at the same time (or in the same 'understanding' – I take this to be the meaning of Addis' 'at once'), the notion of a weakly finite but weakly infinite totality is incoherent.

There are one or two objections to be made about Addis' general reasoning here, before the simple equivocation is exposed. It certainly seems obvious that we can give intensional specifications for finite sets. As I describe, Addis allows for this, but only when the intensional specification is equivalent to the extensional. But it is not at all clear firstly that the intensional specification of the members of a (straight-forwardly) finite set, when available, is ever *equivalent to* the extensional specification. It is not obvious what Addis intends to imply here – from what follows

[4] Addis, p.159
[5] *ibid.*

in his discussion, a plausible interpretation of this would be to suggest that in the case of finite sets, the intensional specification is 'reducible to', or 'actually is' the extensional. But what conceivable cases are there in which the intensional specification of a finite set is reducible to the extensional? Perhaps we might allow that if the rule for set membership (and hence the intensional specification) was as simple as 'must be one of the numbers 1, 2, or 3', then it can be said to *include* the extensional specification {1,2,3}; but very rarely, even in extremely ordinary finite cases, will the intensional specification look like this. My earlier example of 'must be a postgraduate at Glasgow University' does not contain any extensional specification, and indeed the intensional specification may be understood without any grasp of the extensional specification.

Of course, the extensional and intensional specifications of a finite set will pick out the same *members*, but Addis cannot simply require that the specifications are co-extensional in order for them to be equivalent in his sense – since if this is *not* the case, the specifications are just not of the same set in the first place.

Furthermore, it just seems obvious that finite sets *in general* may be specified intensionally, wholly independently of the possible extensional specification. Addis' own example, of the prime numbers less than eighty, may be intensionally specified in terms of a rule (or perhaps combination of rules – 'is less than eighty' and 'is a prime number', for example) for set membership, without our having grasped any extensional specification. Without writing them down, and/or working them out, I have no direct connection with the extensional specification of the set, although I understand well what is to count as a member of the set *from the intensional specification.*

Perhaps Addis' intention is to suggest rather that finite sets may *always* be specified extensionally, if occasionally intensionally, and infinite sets may *always* be specified intensionally, and (presumably) never extensionally. A more natural demarcation on such an interpretation would then be to suggest that finite sets *may* be specified extensionally, and infinite sets may *not*, and that this is the defining difference. He does make a broad statement in this direction:

"There is no extensional correlate in infinite sets and this is the crucial contrast with finite sets." [6]

However, it seems as though Addis, in his discussion of Wittgenstein's distinction, is asserting something further – not that simply the extensional specification 'belongs' in this way to finite sets, but that intensional specification belongs, in a corresponding sense, to infinite sets. As long as finite sets may be specified intensionally, as surely they may, this belonging does not seem equivalent (since in the finite case it is exclusive, but not in the infinite). Indeed, for Addis' argument to progress, he must *require* this stronger demarcation – his argument rests upon the idea that since we cannot hold an intensional and extensional specification of a totality simultaneously, (as Wittgenstein describes), and that since a finite totality must be specified extensionally and an infinite totality intensionally, we may conclude that the notion of a (weakly) finite but (weakly) infinite totality is incoherent. Obviously, if Addis allows that finite sets may be specified intensionally, then the conclusion does not follow – we may give a purely intensional specification of a (weakly) finite and (weakly) infinite totality.

What Addis has not shown is that finite sets *must* be specified extensionally. To arrive at the conclusion that the specifications conflict, he must show not only, as I believe he does, that there can be no extensional specification for an infinite set, but also, as he does not, that specification of a finite set necessarily involves extensional specification.

One final possible reading of Addis might suggest the following interpretation. Perhaps Addis' point is that finite sets possess the property of (or always have the capacity for) being extensionally specified, whereas infinite sets do not possess that property (or have the capacity).

This suggestion is also supported by the claim, quoted above, that "[t]here is no extensional correlate in infinite sets and this is the crucial contrast with finite sets."

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[6] Addis, p.160

The conclusion here is therefore that, since nothing can both possess and not possess the same quality, a (weakly) infinite and (weakly) finite totality is incoherent.

But here, Dummett's definition of *weakly* finite becomes very important. For while it seems intuitively plausible that (strongly, or ordinary) finite sets always admit of extensional specification, it does not seem a requirement of the definition "a totality such that, for some finite ordinal *n*, there exists a well ordering of it with no *n*th member" that it be always possible to provide extensional specification for such a totality. Indeed, to insist that it does is precisely to ignore the kind of paradigm examples that gave rise to the definition in the first place; for example, Addis also refers to the totality of heartbeats in one's childhood. Is it even *intuitive* now that such a totality, while surely weakly finite, admits of *extensional* specification? Surely not.

The essential mistake, then, that Addis makes in drawing his conclusion is that the terms 'finite' and 'weakly finite' are interchangeable. If his argument is intended to prove that infinite sets cannot be finite sets, it seems trivial – such a distinction we might think merely implicit in the definitions of finite and infinite. But it appears - in order for his conclusion to follow - that he has simply failed to notice the discrepancies between *simply* finite and *weakly* finite sets. Assuredly, all (well-ordered) finite sets will be weakly finite. However, it is something else entirely to assert that all weakly finite sets will be finite in the *normal* sense; if this was also the case, what use could Dummett's definition serve? A set is (simply) finite if its members can be correlated 1-1 with the natural (finite) numbers up to n for some n. And this will entail that the set is weakly finite also. But the converse does not hold – being weakly finite does not entail being finite. Moreover, it seems as though the paradigm cases of weakly finite but weakly infinite totalities are precisely the kind of cases that are not finite in the usual sense, and certainly do not admit of extensional specification (the number of heartbeats in my childhood, for example). But without the important assumption that weakly finite sets are also finite in a more general sense, Addis' argument fails to proceed. Addis' conclusion begins as follows:

"As the intensional and extensional views cannot be held simultaneously and consistently this means that a set cannot be both finite and infinite." [7]

Now, as I have said, this interim conclusion might be considered trivial. I have expressed some concern already about the stark polarity Addis attributes to intensional and extensional specifications regarding infinite and finite sets respectively, but even if we grant this, the conclusion is nothing more than that 'a set cannot be both finite and infinite'. But we might think this is a definitive property of finite and infinite sets, at least from the kind of definition proposed by Dedekind that a set is infinite if and only if it can be mapped in one to one correspondence with a proper subset of itself, and finite otherwise. However, Addis continues:

"Since the intensional and extensional specifications of the totality conflict, the argument shows that it is impossible to describe coherently a weakly finite, but weakly infinite, totality." [8]

Here the equivocation is exposed. The intensional and extensional specifications of a *finite but infinite* totality conflict, but that says nothing about weakly finite but weakly infinite totalities unless it can be shown that all weakly finite totalities must be finite totalities in the general sense (and similarly for weakly infinite and 'genuinely' infinite totalities). And this Addis has certainly not shown – nor does the task look like a promising one.

*Dummett's charge of Inconsistency*

I shall turn my attention next to Dummett's discussion of strict finitism, from which we may draw his reasons for believing that there is a general inconsistency in the kind of totalities we are discussing. It is my aim to show that Dummett is guilty of an essentially similar equivocation to Addis, despite the fact that Dummett has undoubtedly provided us with useful definitions of 'weakly infinite' and 'weakly

[7] Addis, p.160
[8] *ibid.*

finite'. There is a sense here in which Dummett appears to overlook the importance of his own definition.

Firstly, Dummett suggests, as I have said, that strict finitism must be committed to weakly infinite and weakly finite totalities, because it is committed to the idea that:

'a vague expression may have a completely specific, albeit vague, sense; and therefore there will be a single specific totality which is the extension of a vague predicate.' [9]

Certainly strict finitism as traditionally advanced will not balk at such a suggestion. However, Dummett, like Addis, suggests that there is a tension between these two properties – 'weakly infinite' and 'weakly finite' – because, as he appears to assume, 'weakly infinite' is equivalent to 'closed under the successor operation'. On the face of it, this looks to be a different objection. Let us see how Dummett discovers the tension.

Dummett asserts that it is a necessary feature of weakly infinite totalities that they should not have a determinate number of members. His argument here is convincing, and indeed the assertion seems undeniable. It is precisely this element of the kind of totalities (and corresponding predicates) that provide us with our interesting examples. But this, he continues,

'should lead us to doubt whether saying that a totality is closed under a successor-operation is really consistent with saying that it is weakly finite [i.e. that it has an upper bound]'. [10]

Dummett has previously however made no mention of a totality that is closed under the successor operation, and since the suggested conflict is between *weakly infinite* and weakly finite, we must assume that he simply *presupposes* at this point

[9] Dummett, p. 313
[10] Dummett, p. 318

that a weakly infinite totality necessarily *is* closed under the successor operation. He immediately precedes the last quote with:

'the definition of "weakly infinite totality" specified that such a totality should not have a last member: whereas, if a totality has exactly *n* members, then its *n*th member is the last.' [11]

Dummett's controversial move then is to reason from this plausible statement to the conclusion that a totality which is not closed under the successor operation must have a last member. Presumably the thought behind the assumption is that if a sequence of numbers is not closed under the successor operation, *there must come a point* at which you cannot add one to *n*; and in that case, of course, *n* is the last member. So unless the totality is closed under the successor operation, it must have a last member. A weakly finite totality, by definition, is not closed under the successor operation, and hence, by the above assumption, it must have a last member. But a weakly infinite totality is such, as we remember, that there exists a well-ordering of it with *no* last member (and hence it must be closed under the successor operation). The two definitions are in conflict.

This then, I take it, is Dummett's attempt to establish inconsistency for weakly infinite and weakly finite totalities. A weakly finite totality is bounded-above, but a totality which is bounded-above is not closed under the successor operation. A totality cannot therefore be both weakly finite and weakly infinite, since to be weakly infinite is to be closed under the successor operation.

However, I wish to argue that it is simply not the case that 'weakly infinite' is equivalent to 'closed under the successor operation'. On the contrary, it is precisely because it is not equivalent that such totalities are characterised as *weakly* infinite, for surely only 'ordinary' infinite sets will be closed under the successor operation.[12] Dummett, in a similar manner to Addis with respect to 'finite', has failed to properly distinguish between 'infinite' and 'weakly infinite'.

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[11] *ibid.*

[12] Indeed, the point is stronger still – for under a strict finitist banner, the term 'closed under the successor operation' will apply to no totalities.

Dummett's argument is, however, at least intuitively convincing, and so is worthy of further discussion. His mistake is, I believe, in assuming that there are only two options – either a totality is closed under the successor operation, in that for every n, if n is a member then n+1 is a member, *or* there is a single determinate last member such that its successor is not a member of the totality. In short, the assertion is that if a totality is not closed under the successor operation, then it must have exactly *n* members. Because, assuredly, a totality that is closed under the successor operation (i.e. a 'strongly', or ordinary, infinite totality) has *no* last member, Dummett assumes that it is therefore also true that a totality with no last member must be closed under the successor operation. But for precisely the vague totalities that we are interested in, this need not be the case. The sense in which such totalities have no last member is not in this strongly infinite sense, but rather in an (appropriately) weaker sense – they have no last member because there is no *precise* last member; and not because their membership is infinite in number. The members of such totalities may be (rather imprecisely) imagined as 'fading out'; for this is exactly the notion of vagueness at play here. There is no candidate for last membership because the totality has no sharp end.[13]

Let me offer an example before I move on to a discussion of the correct strict finitary formulation of 'no last member'. If we take an ordinary view of colour, it seems to us as though there must be definite cut off points between colours, unless we are to fall into the kind of inconsistency proposed by Dummett for such cases (recall the discussion of the previous two chapters). Indeed, given an epistemicist reading of vagueness, there is philosophical justification behind the idea that there are sharp cut-off points between colours, although we are unable to recognise them. Hence, if we take the 'vague region' between blue and green, we might be tempted to say that a shade must be either blue or green, and not both. Blue and green are just such colours, however, that give rise to an interesting situation at the boundary – we have a third identifiable colour, Turquoise, which it seems to us, at least on an informal reading, to be both (and perhaps neither) blue *and* green. In fact, I can give no better definition of

[13] One possible source of confusion regarding the problem is the equivocation for many philosophers and mathematicians between 'totality' and 'set'; but presumably, Dummett has (rightly) avoided the term 'set' because it is not possible to conceive of such totalities as sets, for the very reason that sets have sharp barriers to membership. This should come as no great surprise – the finitist (even in a loose sense, e.g. intuitionists) will reject much of the talk of set theory anyway.

turquoise than that. Now, the epistemicist (and indeed the informal view) may wish to hold that there is simply a further cut off between blue and turquoise, and likewise between turquoise and green. However, to the extent at least in which turquoise is both blue *and* green, it does not seem determinate which shades properly belong to the totality 'blue', and which properly belong to the totality 'green'. It is clear that Blue is not Green, and so the totality of, say, blue shades clearly does not contain some green shades. But it does seem to contain some (or even all) of the turquoise shades. The turquoise shades are however only *vaguely* members of the totality of blue shades – they are neither exactly in nor exactly out.

The precise entailments of the required semantics for such vague regions I shall explore in due course; but for now let us be explicit about the rejection of Dummett's reasoning as I have begun to present it here.

The strict finitist is committed to the claim that there is no last member, to the extent:

$$\neg\exists x\ (S(x) \wedge \neg S(x'))$$

(where x' is the successor to x in a well-ordering of the totality of objects to which the vague predicate S applies)

but will resist the classical equivalence Dummett assumes, to the extent:

$$\forall x\ (S(x) \rightarrow S(x'))$$

The classical equivalence proceeds as follows:

$$\neg\exists x\ (S(x) \wedge \neg S(x'))$$
$$\forall x\ \neg(S(x) \wedge \neg S(x'))$$
$$\forall x\ (S(x) \rightarrow S(x'))$$

But the derivation here relies upon the law of excluded middle, and the strict finitist, I suggest, will want to employ only constructively acceptable logical principles.

Remember that, in the current context, Dummett's charge of inconsistency rests upon the claim that *either* there is a last member, *or* the totality is closed under the successor operation. What the strict finitist will reject then is *not* the quantifier shift (the first step in the above derivation), but the translation of the conjunction into the conditional. The quantifier shift is constructively acceptable, since if it is true that there is nothing in the domain to which the assertion applies, then it will be true of all the things in the domain that the assertion does not apply. What the strict finitist will resist is the classically acceptable translation of:

$$\neg(S(x) \wedge \neg S(x'))$$

into

$$(S(x) \rightarrow S(x'))$$

The reason for rejecting the translation is precisely the objection made to Dummett's understanding of 'no last member'. The fact that there is no *determinate* last member does not necessarily lead to the conclusion that the totality is infinite in scope. The fact that I cannot (presumably in principle cannot) specify a determinate last member of the heartbeats in my childhood, does not stop me from asserting that the totality 'runs out' somewhere in my teens. Now, it is true that classical logic is inadequate to deal with such an intuition, and classically, of course, the conjunction and conditional offered just above are equivalent. Clearly, in order to properly explore the suggestion, we need to offer an alternative logic. There are various approaches to this, which I shall explore fully in the next chapter, but one possible solution, that I raise here just to illustrate the kind of response I think is correct to Dummett's challenge but that I shall go on to discuss in considerable detail in chapter ten, is to move to a three-valued logic: a logic which uses three truth values – True, False, and Indefinite. The reason for introducing a third value is, I suspect, intuitive in this case: let us postulate that there are members of the sequence which are indeterminately part of the (vague) totality, and that this is the source of vagueness. That is to say, if we are considering any vague predicate, the predicate will be True of some objects in the domain, and False of others; but there will be a third category of objects for which the (vague) predication is Indefinite. In this, I suggest, lies the strict finitary interpretation of 'no last member' – it is not that there is no last member because the members of the totality are inexhaustible, but rather because there is no one determinate last member, and instead a 'vague range'.

The response to Dummett's charge of inconsistency for weakly finite and weakly infinite totalities, then, is that 'closed under the successor operation' is not the correct interpretation of 'weakly infinite'. In fact, it is a property (only) of strongly (that is, ordinary) infinite totalities, and hence the identification with weakly infinite totalities is mistaken, unless 'weakly infinite totalities' are reducible to 'infinite totalities'. If this is the case, the analysis is pointless, as it is clear that a totality cannot be both weakly finite and *infinite*. But presumably, Dummett's characterisation of 'weakly infinite' is intended to cover a class of cases, among which we may count the totalities which give rise to strict finitary predicates, which differ from the ordinary cases in a significant respect. Ultimately, however, Dummett's analysis fails to respect this difference, and as a result is guilty of a similar (if subtler) equivocation to that of Mark Addis in his discussion of weakly infinite and weakly finite totalities.

Dummett rejects strict finitism on the grounds that the predicates it endorses are inconsistent in application, but his establishment of inconsistency for *these* predicates relies upon an equivocation between 'weakly infinite' and 'infinite'[14] which the strict finitist can (and will) reject.

*The Story so far . . .*

These remarks conclude the second part of this thesis. In the final part, I shall attempt to give a rigorous definition of the kind of semantics required by the rejection suggested here, and to test such a solution in a wider context. I shall also move on to look at alternative formulations for strict finitism, following my earlier remark that it is not obvious that a strict finitist must be committed to the notion of weakly finite and weakly infinite totalities in the first place.

[14] Or, at least, between 'weakly infinite' and 'closed under the successor operation'; but since the latter is a property only of infinite totalities, the equivocation amounts to the same.

In part two, we have identified vagueness as the principle obstacle for strict finitism as a foundational theory for mathematics, but I have also rejected a number of problems that have been suggested along these lines. While vagueness still remains a potentially troublesome issue, the problems I have discussed here do not seem as immediately fatal to the theory as the objectors intended. Firstly, there is no special problem over what Addis' referred to as the 'psychological and epistemological' elements of Surveyability – the complication that surveyability is determined not only by the objects to be surveyed but also by the abilities of surveyors has been shown only to be an additional source of vagueness, rather than a contradiction in itself. Moreover, we have seen that the charges of inconsistency based on a commitment to Weakly Finite and Weakly Infinite totalities may be rejected, and that there need be no inherent inconsistency in the strict finitist recognising the Sorites paradox. Perhaps the most crucial observation here is that endorsed by Wright's analysis: that the presence of vagueness in the strict finitist theory is insufficient to reject the theory in itself – otherwise the presence of vague predicates in natural language might call for a similar rejection.

# PART THREE: FOR A FEW NUMBERS MORE

## Alternative logics and formulations for the Strict Finitary account

# CHAPTER IX: ALTERNATIVE LOGICS

As we have seen, Dummett's attempt to establish the inconsistency of weakly finite and weakly infinite totalities rests upon the assumption that the strict finitist, and indeed perhaps anyone attempting to provide a coherent answer to the sorites paradox, must rely upon classical logic in order to derive a solution. As Dummett suggests, such a solution does not look promising, as any formulation of the totalities he discusses within a classical framework apparently entails a contradiction, since as we have seen, the weakly infinite requirement:

(1) $\neg\exists x\,(S(x) \wedge \neg S(x'))$

entails (classically):

(2) $\forall x\,(S(x) \rightarrow S(x'))$

which is straightforwardly inconsistent with the weakly *finite* requirement when used as a premise in a Sorites type argument.

But, as I have already suggested, it is not clear that a strict finitist (nor, again, anyone attempting to find a viable solution to Sorites paradoxes) is bound to use classical logic. One of the apparent *advantages* of the epistemic response to vagueness is that it requires no revision of the logic, but it is nonetheless only one competing theory in many. It should also perhaps be of little surprise that classical logic is insufficient to support the strict finitary case – intuitionism, after all, requires an extensive revision of the logic, and strict finitism is, in some sense at least, more demanding than intuitionism.

The focus of this chapter will therefore be an examination of alternative logics for the strict finitist. I begin with a look at intuitionistic logic, but will quickly describe the need to move to a three-valued logic, which I shall also present. I argue that a three-valued logic is sufficient to reject Dummett's charge of inconsistency, and outline the rejection here also. I shall finish the chapter with an acknowledgement that three-valued logic suffers from a problem known as 'higher-order vagueness', and prepare the ground for a proper assessment of the issue in the next chapter.

## *Weakly finite and weakly infinite totalities and Intuitionistic logic*

Following the observation that some revision to the logic is required in order to avoid Dummett's troubling conclusion regarding the consistency of weakly finite and weakly infinite totalities as he defines them, a natural step would be to consider whether intuitionistic logic, as an existing system, will be sufficient to defend such totalities against the criticisms levelled by Dummett. Although I have already hinted at the need for (at least) a trivalent logic, we might be spared such enquiry if the existing intuitionistic programme proved robust enough for the purpose. Let us then initially take a more cautious step forward, and examine the case for an intuitionistic solution, without resort to a three- (or indeed many-) valued logic.

At first glance, intuitionistic logic seems well equipped to resist Dummett's classical contradiction – since under intuitionistic logic we are not entitled to pass from the truth of (1) above, to the second claim (2). Let us recall the classical derivation of the equivalence, as I outlined it in a previous chapter:

$$\neg\exists x\ (S(x) \wedge \neg S(x'))$$

$$\forall x\ \neg(S(x) \wedge \neg S(x'))$$

$$\forall x\ (S(x) \rightarrow S(x'))$$

Now, I suggested in Chapter Eight that the strict finitist will object to the move from the negated conjunction to the conditional, and not to the quantifier shift. This stands well in line with the intuitionistic logic. Under such logic, the quantifier shift is allowable[1], so insofar as the first step of the derivation is concerned, intuitionistic logic is in accord with classical logic. Where intuitionistic logic will diverge is at precisely the point where I suggested the strict finitist should object to the classical logic, over the shift from the negated conjunction to the conditional. This equivalence is not provable intuitionistically; and thus intuitionistic logic provides us with a way of resisting Dummett's argument for inconsistency, at least as it is presented. Since we cannot pass from (1) to (2) above without recourse to the principle of bivalence, we are not entitled to the conclusion, and hence do not in this way derive a simple contradiction from the definitions of 'weakly finite' and 'weakly infinite'.

However, although Dummett himself makes no case against an intuitionistic response to the problem, there is a way to reformulate the problem in a way that is consistent with intuitionistic logic, such that it remains problematic for any such account.[2]

The reformulation relies upon a form of the Intuitionistic Least Number Principle. The *classical* version of the Least Number Principle amounts to the following stipulation: if the number 1 has a certain property A and a larger number *n* does not, then there must be a least number among the set of numbers between 1 and *n* which does not possess the property A. Or:

$$\frac{A(1) \qquad \exists n\ \neg A(n)}{\exists x\ (A(x) \land \neg A(x'))}$$

[1] Although one must be careful with quantifiers under intuitionistic logic, since the construction of a proof to the effect of establishing the translation is only available in some cases, and not in others. The move from $\neg\exists x\ A(x)$ to $\forall x\ \neg A(x)$ is allowed, but the move from $\neg\exists x\ \neg A(x)$ to $\forall x\ A(x)$ fails. This is not because the quantifier shift from $\neg\exists x$ to $\forall x\ \neg$ fails (the shift from $\neg\exists x\ \neg A(x)$ to $\forall x\ \neg\neg A(x)$ is acceptable) but rather because double negation elimination is required to take us from $\forall x\ \neg\neg A(x)$ (the proper intuitionistic equivalent in this case), to $\forall x\ A(x)$.

[2] I am grateful to Dr Patrick Greenough (Univ. of St. Andrews) for the observation, and indeed the structure of the argument that follows, to the extent that intuitionistic logic is not sufficient to dispel the objection.

This follows of course from the idea that either A(n) or ¬A(n) must hold for every *n*: because there is at least one number in the set which does possess the property, and at least one which does not, all the others must fall into one or other category. Hence, there must be a least number among those which fall into the category of not possessing the property. As such, the classical least number principle (CLNP) therefore rests upon the law of excluded middle, and so it is invalid under intuitionistic logic. However, there is an intuitionistic version of the least number principle, which does not rely upon the law of excluded middle. It makes a weaker claim than the CLNP, but it is nonetheless problematic for the case currently under consideration. We may express the Intuitionistic least number principle (ILNP) as follows:

$$\frac{A(1) \qquad \exists n\ \neg A(n)}{\neg\neg\exists x\,(A(x) \wedge \neg A(x'))}$$

So, the ILNP asserts instead that if the number 1 has a certain property A and a larger number *n* does not, then it is *not false* that there is a least number among the set of numbers between 1 and *n* which does not possess the property A. (Since the argument that follows will return to the notion of surveyability, I shall from here on re-substitute for the general property 'A(x)' the predication 'is Surveyable', 'S(x)'):

$$\frac{S(1) \qquad \exists n\ \neg S(n)}{\neg\neg\exists x\,(S(x) \wedge \neg S(x'))} \qquad (4)\ (\text{ILNP})$$

The principle is problematic for our account of Surveyability in the following way. The first premise we may of course assume, and is not at all in dispute – i.e. that one is a member of the totality (of surveyable numbers); and the second premise is simply equivalent to the previously accepted definition for weakly finite – that is, that the totality is bounded above, such that one can specify a number(-candidate) which is not a member of the totality. And so it seems as if we are intuitionistically entitled to draw the conclusion (4). Now, although the conclusion (4) won't collapse into the classical equivalent:

$$\exists x\ (S(x) \wedge \neg S(x')) \quad \text{(CLNP)}$$

it is nonetheless (intuitionistically) still in contradiction with the strict finitary definition of weakly *in*finite (that of 'no last member') provided in the last chapter. This was of course:

$$\neg\exists x\ (S(x) \wedge \neg S(x'))$$

So it seems that, while intuitionistic logic is able to resist the classical charge that Dummett poses, it is itself insufficient to provide a natural solution to the general problem – which is that the definitions of 'weakly infinite' and 'weakly finite' seem incompatible and are logically contradictory. We must therefore return instead to a discussion I began briefly at the close of the last chapter, and investigate the potential of multi-valued logic systems.

### *The many-valued approach*

I have already mentioned more than one multi-valued logic, and I should perhaps offer a brief account of the distinctions between models. I outlined in Chapter Six the 'degree theory of truth' approach to vagueness, and noted there the commitment therein to a multi-valued logic. In fact, I also suggested that a degree theoretic account looked unpromising for a strict finitist, not least because it is committed to a very fine-grained notion of 'degree', and an (at least potentially) infinite number of values. In the last chapter, I began to sketch a different multi-valued approach, using only the (three) values True, False, and Indefinite. So how many values is enough?

The question is actually rather a tricky one, and save for rejecting the answer 'infinitely many', I shall defer answering the question for the present, and return to it shortly. What is apparent, however, is that further to a rejection of *infinitely*-valued logics, the strict finitist must, I think, and for obviously similar reasons, be careful also not to appeal to *unsurveyably-many*-valued logics. To avoid unnecessary complication then, it seems prima facie preferable to have as few as possible, and indeed a specifiable amount. Hence, for much of what follows here, I shall be investigating the potential of a trivalent (three-valued) logic, but I will also give some attention to the possibility of a 'surveyably-finite'-valued logic.

*A Three-valued approach*

A trivalent logic, then, introduces a third truth-value. The truth functionality of the classical operators (and corresponding truth-tables) must be revised to accommodate this third value. The idea of a trivalent logic is not particularly new[3] – and the by-now standard term for the third value in the literature is 'Indefinite'. Any proposition will take one of three truth-values: true, false, or indefinite. So what are the candidates for statements with indefinite truth-value? Presumably the paradigm vague cases we are interested in. Take for example the predicate 'is tall'. Now it certainly looks true to say of some people, say, those over six feet, that they are tall. Equally, there are certainly people of whom it would be false to say that they are tall – say, those who are less than five feet in height. But now, it may well be that it is simply inappropriate to assert, of many of the remaining people, that they are tall, or that they are not tall. In such cases, it may well be neither true nor false to say that someone of, say, 5 feet 10 inches is tall. Indeed, it may look as though it is as true to say that they are tall, as to say that they are not tall. This may give us a useful model with which to define the negation operator.

[3] Kleene presents a comprehensive, if technically concentrated overview in his 1952 book *Introduction to Metamathematics* (c.f. pp. 332-40). The truth tables presented here are essentially the same as those he presents.

Let us take this as a definition of the truth value of the negation operator in trivalent logic: the negation of any statement is false if the original statement was true, true if it was false, and indefinite if it was indefinite. Notice that the classical results are preserved for cases involving only true or false statements, but where indefinite statements are involved, we may have further statements with indefinite truth-values. This is represented below:

| P | ¬P |
|---|---|
| T | F |
| I | I |
| F | T |

Thus the classical truth values are preserved for the values true and false, but indefinite statements introduce a new level of complexity into the logic. Notice also that the negation of an indefinite statement is itself indefinite. This follows from the example I provided above – if there are some people of whom it is indefinite to say that they are tall, it may well be (and seems likely that it is) just as indefinite to say that they are not tall.

The same adjustment of truth values for statements involving indefinite components holds for all of the classical connectives. Consider the case of conjunction; intuitively, if I say of two people that they are tall, i.e. I assert that A is tall ∧ B is tall, then it will clearly be false if one of them is obviously not tall, and true if both of them are obviously tall, but if one or both of them lies within the indefinite range *and neither is false*, it seems as though the truth value of the conjunction will be similarly indefinite. So, a conjunction will be true when both its conjuncts are true, false when either is false, and indefinite otherwise:

**P ∧ Q**

| | | Q | | |
|---|---|---|---|---|
| | | T | I | F |
| | T | T | I | F |
| P | I | I | I | F |
| | F | F | F | F |

Similarly for disjunction: a disjunction will be true when either of its disjuncts is true, false if they are both false, and indefinite otherwise:

**P ∨ Q**

| | | Q | | |
|---|---|---|---|---|
| | | T | I | F |
| P | T | T | T | T |
| | I | T | I | I |
| | F | T | I | F |

And finally, a conditional will be false when the antecedent is true and the consequent false, true when both antecedent and consequent are true or when the antecedent is false, and indefinite otherwise.

**P → Q**

| | | Q | | |
|---|---|---|---|---|
| | | T | I | F |
| P | T | T | I | F |
| | I | T | I | I |
| | F | T | T | T |

The assignments of the values here might look a little odd at points, so let me take a moment to justify them[4]. Firstly, consider the case when the antecedent is false and the consequent is indefinite. The classical parallel suggests that if the antecedent is false, the conditional is (vacuously) true whatever the truth value of the consequent. This seems intuitive, in fact: since the ordinary (classical) case for the (vacuous) truth of any conditional when the antecedent is false rests upon the idea that, for example, if the sea is dry then [*anything*], because as a matter of fact the sea is not dry. If the ordinary explanation of the truth conditions is accurate, then it looks equally plausible whatever the actual truth value of the consequent, be it true, false *or indefinite*.

[4] This assignment of truth values also matches the table you get if you simply *define* the conditional P→Q as ¬(P ∧ ¬Q).

But now consider the value assigned when the antecedent is indefinite, and the consequent true. Perhaps we should not be surprised by an assignment of values under which the conditional comes out true whenever the consequent is true, following the remarks above and a similar analogy with the classical case – after all, on a classical account, the conditional comes out true whenever the consequent is true; but the model is perhaps not so immediately intuitive in this case. It may be objected that all we can assert in this case is that classically, whenever the consequent is true, a conditional comes out true whether the antecedent is true or false, which is not to say it comes out true *whatever* the value of the antecedent. Clearly, the conditional can't be false, but this doesn't amount to the assertion that it must be true given a trivalent semantics. The issue here centres around the precise definition of Indefinite, on these semantics. Indeed, under different interpretations, the worry expressed here will spread to other values of the truth table. I will discuss this issue in more detail shortly, but let us take it for now, for the purpose of these truth tables, that the indefinite value means something like 'might come out true or false' – in which case the assignment of 'true' to the case where the antecedent is indefinite and the consequent true seems correct, since whether the indefinite antecedent turns out true *or* false, the conditional will remain true. Moreover, the assignment of truth values for the conditional as presented here have the advantageous feature that the resulting truth table matches that of $\neg P \vee Q$, so that we preserve the equivalence. Though not in itself a motivating factor, the result is reassuring.

Lastly, with regard to the assignment of truth values to the conditional I have presented, it is sometimes thought that trivalent logic is unattractive because it gives a truth value for $A \rightarrow A$ of indefinite when A is itself indefinite, whereas this ought to come out always true (and indeed is obviously a classical tautology). But presumably it *ought* to come out always true (classically speaking) only because it can never be false – since if it is false then it implies a contradiction. But the idea that it is never false is certainly endorsed by the trivalent semantics. Admittedly, it is not a genuine tautology, and indeed there may well be no genuine tautologies in a three valued logic. But the fact that the conditional does not always come out *true* does not have the same consequence as it does in a bivalent semantics – what remains of importantance is that the conditional *does not come out false*, since that (and only that) would imply a contradiction. When A is true or false, the conditional is true, and when A is indefinite, the conditional is indefinite; hence, the conditional is never false. In fact, Michael Tye endorses precisely this response to the objection in 'Vague Objects' (p. 545), suggesting that cases like the conditional A→A will be what he describes as 'quasi-tautologies' in a three valued semantics. (A quasi-tautolgy is defined by exactly the characteristics offered here: while a quasi-tautology cannot be false, it may be true *or* indefinite).[5]

### *Three values vs. Dummett*

So, given the trivalent semantics outlined above, we are now in a position to effectively tackle the general problem, alluded to by Dummett.

To see precisely how, imagine a series of heartbeats, ranging from birth into adulthood, and let us assume an arbitrary point p, such that it is true that all heartbeats, up to and including p, are heartbeats in my childhood. Now, let us take a second arbitrary point r, such that it is false for r and all subsequent heartbeats that they are heartbeats in my childhood. Between p and r is a range of heartbeats for which we are unsure – the vague region in question. Now, let us say that it is indeterminate whether or not these heartbeats are heartbeats in my childhood or not. (For the purpose of the following demonstration, let us also set an arbitrary point somewhere within that region: heartbeat q).

For the present case, I am considering only the simplest example. Of course, it is unlikely to be the case that we wish to define the truth value assignment as sharply as this,

[5] Tye also presents (in the same article) a similar account of the conjunction A∧¬A, which also comes out indefinite when A is indefinite, contrary to the (classical) intuition that this should always come out false, as a contradiction. Following the same thought, Tye suggests that such cases are 'quasi-contradictions', in that while they cannot be true, they may perfectly consistently be false *or* indefinite.

such that although the division between True and False is vague, our assignments of p and r are such that it implies that the division between True and Indefinite (and indeed False and Indefinite) is not. I shall discuss the attendant difficulties at a later stage, but for now let us consider the example where p and r are sharp points, so that we may gradually introduce the role of the indefinite truth value. The idea being presented at this stage is only how that role will differ from the traditional conception of bivalent accounts, and so the simple case will be sufficient for the explanation at this stage.

Using such an example then, we may give independent sense to the strict finitist claim that there is no last member, while resisting the classical equivalence of closure under the successor operation – which was the suggested solution to the dilemma that I tentatively offered in Chapter Eight – and at the same time offer a robust alternative to Dummett's derivation of inconsistency from the definitions of weakly finite and weakly infinite.

Firstly, let us consider the claim that there *is* a last member on this model - that is, let us try and satisfy: $\exists x\ (S(x) \wedge \neg S(x'))$

where the domain is the heartbeats in an adult lifetime, and S corresponds to the predicate 'is a heartbeat in childhood'. Now, the existential quantifier will be true just in case there is at least one instance where the conjunction is true, false where there are no instances of the conjunction which are true *or indefinite*, and indefinite otherwise. Let us consider all heartbeats in the sequence.

Before p, $\neg S(x')$ is False, making $(S(x) \wedge \neg S(x'))$ False
At p, $S(x)$ is True, but $\neg S(x')$ is Indefinite making $(S(x) \wedge \neg S(x'))$ Indefinite
At q, both $S(x)$ and $\neg S(x')$ are Indefinite making $(S(x) \wedge \neg S(x'))$ Indefinite
At r, $\neg S(x')$ is True, but $S(x)$ is Indefinite, making $(S(x) \wedge \neg S(x'))$ Indefinite
After r, $S(x)$ is False, making $(S(x) \wedge \neg S(x'))$ False

Therefore, we arrive at the conclusion that it is never True that $\exists x\,(S(x) \wedge \neg S(x'))$; or rather, we are not committed to the claim that there *is* no last member.[6]

However, for closure under the successor operation, we need the universally quantified conditional to hold. But now it seems as though we have reason to resist the conditional, for all x. The universal quantifer will be true just in case all instances of (in this case) the conditional are true, false just in case there is at least one instance of the conditional which is false, and indefinite otherwise. Again, let us consider all the heartbeats in the sequence.

> Before p, the conditional will hold; that is, S(x) is True and S(x') is True, making $(S(x) \rightarrow S(x'))$ True.
>
> Equally, after r, S(x) and S(x') are both False, making $(S(x) \rightarrow S(x'))$ True.
>
> However, at q (at least), S(x) and S(x') are both Indefinite, making $(S(x) \rightarrow S(x'))$ Indefinite.

Hence, we can not conclude that it is *true* that:

$$\forall x\,(S(x) \rightarrow S(x'))$$

since the universally quantified conditional here comes out indefinite. The sorites is not sound, but there is no rule of inference which fails. Consider the paradox:

$$S(1)$$
$$\underline{\forall x\,(S(x) \rightarrow S(x'))}\;.$$
$$\forall x\, S(x)$$

[6] There is an important distinction to make here, in that, given a three-valued logic, refusing to endorse or accept the truth of a statement does not amount to an assertion that the statement is *false* – simply that it is not true. The principle of bivalence will obviously not apply in a trivalent system. Hence it is not that we may here conclude that statement is false, since we have shown it to be indefinite. It is perhaps more correct to speak of a *rejection* of the (truth of the) statement, as opposed to a denial (an assertion that the statement is false).

The universally quantified form of the second premise is indeterminate, and so the conclusion cannot be true.

By now it should be obvious how I intend to translate this to the case of surveyability. A trivalent approach will assert that, concerning the borderline vague members of the totality of surveyable numbers, it is indefinite whether each borderline member is surveyable or not. There will be numbers which are clearly surveyable, (proto-)numbers, or 'number-patterns', which are clearly unsurveyable, and then there will be a vague region of numbers[7] which are neither clearly surveyable nor clearly not. But although we are entitled to the assertion of weakly infinite for a totality such as 'the surveyable numbers', since there will be no number such that it is True that it is surveyable, and yet False of its successor that it is surveyable, we are not entitled to assert that the totality is closed under the successor operation, since the definition of closed under the successor operation relies upon an interpretation of $\neg\exists x\ (S(x) \wedge \neg S(x'))$ where $\neg S(a)$ implies 'it is false that' S(a), and not merely 'it is not true that' S(a), and is instead (on the current model) *either* false or indefinite. Hence Dummett's charge of inconsistency fails under a trivalent approach.

## *Problems for trivalent logics – the preservation of Monotonicity*

I would like to end the chapter on such a successful note, but it is only fair to acknowledge that the three-valued approach is not without difficulties; the first of these is a problem regarding the assignment of truth values for the conditional as I have presented it here. I offered some justification above for assigning the truth values in this way (following Kleene), but it should be noted that such an assignment will nonetheless produce some rather odd and undesirable results. One of the most problematic is that the truth table for the conditional does not preserve monotonicity. That is to say, the conditional: 'For all x, if x' is small and x is smaller than x' then x is small', will have

[7] Perhaps 'potential' numbers would be (heuristically) better here, if rather 'loosely-speaking'.

indeterminate instances, simply because 'x' is small' will have indeterminate instances – hence the conditional itself must be indeterminate. But this is extremely counter-intuitive – the conditional just ought to be true.

There is no obvious way to resolve this issue, and although I shall devote the next chapter to a discussion of some further problems for trivalent logic, and a look at some of the other odd results, it is principally because of results of this kind that I shall ultimately prefer, in the final chapter, a formulation of strict finitism which is not committed to more than a bivalent logic.

*Problems for trivalent logics – Higher order vagueness*

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There is a further problem for trivalent logics in general, (however the conditional is expressed), and it is to finding a way out of this difficulty that the next chapter will be devoted. For now, let me outline the general problem, and identify its strongest objection.

Let us begin with taking the results above and applying them to a wider context once again. Now, in addition to a pleasing response to the worries expressed by Dummett for the strict finitist, three-valued logic provides us with a fresh way to tackle the more general problem of sorites paradoxes. Using a trivalent semantics, a purported paradox of the form:

1 is surveyable
<u>For all n, if n is surveyable, then n+1 is surveyable .</u>
All numbers are surveyable

is no longer paradoxical, since a denial of the second premise no longer entails

$\exists x\,(S(x) \wedge \neg S(x'))$

This is because there will be no point in the well-ordering at which the predicate 'is Surveyable' is true for x and yet false for the successor of x; instead, there are some values of x for which S(x) is true, then some values for which S(x) is *indefinite*, and *then* further values for which S(x) is false.

Prima facie, this is a neat response, but there is certainly something suspicious about the solution. The problem, by now well-identified, is that of higher order vagueness. The idea behind the higher order vagueness objection is simply to observe that even if one can no longer assert that the existence of an upper bound entails that there comes a point along a well-ordering of the totality such that it is true that S(x) and false that S(x'), one may insist instead that there must come a point where it is true that S(x), and indefinite that S(x'). So unless the proponent of three-valued logic is willing to commit to the idea that there exists an x such that S(x) is true, but S(x') is indefinite, then the vagueness is simply moved one step back (or higher), and the difficulty is not truly averted. To express this idea formally, we need a further truth functional operator for indefinite; define $\nabla$ as 'it is indefinite that', such that $\nabla$S(x) stands for 'it is indefinite that x is surveyable'. And to make the point explicit, let us write, for S(x) (where S(x) is true, and not indefinite[8]), Def.S(x). Now, the 'higher-order' sorites runs like this:

$$\text{Def.S}(1)$$
$$\underline{\forall x\ \text{Def.S}(x) \rightarrow \text{Def.S}(x')\quad .}$$
$$\forall x\ \text{Def.S}(x)$$

Now a denial of the second premise entails

$$\exists x\ (\text{Def.S}(x) \wedge \nabla S(x'))$$

which amounts to the assertion that there is a sharp boundary between true cases and indefinite ones. (Analogously, for example, between men who are definitely bald and

[8] Strictly speaking, neither indefinite *nor false*, of course – a similar introduction can be made for Def.¬S(x) where S(x) is false and not indefinite (or true).

men who are indefinitely bald). And this may look as unpromising as the original bivalent claim did, to the extent that there is a sharp boundary between true cases and false ones.

However, I qualify this statement by saying it ‘may look’ so, because although I think this is in fact the correct response here, I wish to register a slight misgiving, which I shall return to later, and it is this: it does not seem to me *precisely* as implausible in the trivalent case as in the bivalent one that there may be a sharp cut-off point. It may at any rate be *easier* to sort a group of men into three groups containing those that are bald, those that are not, and those that are neither bald nor not bald, than to sort the same group of men into two groups, containing those that are bald in one and those that are not in another. This suggests that the concept is not *as* unintuitive as in the bivalent case. Although as I have said I think that in the trivalent case the idea of sharp boundaries is still implausible, since it is easy to think of examples where it would not seem appropriate, I will offer further considerations shortly that might make this observation poignant.

One way to attempt to meet the challenge head on would be to introduce a further truth value, something like indefinitely-indefinite, to define a vague region between those values for which S(x) is definitely true, and those for which S(x) is indefinite (and between indefinite and false) ; but it should be fairly clear that this will simply push the problem back another step – that is, take it to a higher order again. Then, of course, we have to introduce another truth value, indefinitely-indefinitely-indefinite, and so on, moving away from trivalent semantics into four-, five-, six-valued logics and beyond. Each step is unlikely to resolve the problem, unless we abandon a finitely-valued theory, and return to the idea of the degree of truth theorists, who help themselves to a (potentially) infinite number of truth values. But even in this case the problem of higher order vagueness is not resolved – infinitely-valued degree theories still have a sharp three-fold classification between truth to degree 1, truth to degree 0, and the remaining degrees of truth, such that we can ask which is the last member of the totality corresponding to a truth value of 1. So even infinitely valued theories seem to have no advantage over finitely valued ones with respect to higher order vagueness.

I don't want to say too much more about the possibility of finitely-valued logics in this regard – as it seems as though they will always be burdened in this way with the problems of higher order vagueness – except to reiterate the point I made above: with each broadening of the indefinite operators, the claim that there might be sharp cut-off points between truth-values is, I suggest, more and more plausible. Indeed, for any sharpening[9] of a particular predicate, it certainly looks as though we will find a finite-range suitable for precise delineation; if asked to sort eight men into at most eight groups of baldness, the first containing definitely bald, and the last containing definitely not bald, we will not find it particularly taxing. As long as the values are sufficiently fine-grained for the relevant sharpening, there will be no need to introduce further values *in the particular case*; and any entailment of the existence of sharp boundaries will be unproblematic in this way. More on this later; but for now there is a more pressing problem – for there is an apparently easy reformulation of the problem which will still cause problems, no matter how many truth values we retreat to. (Indeed, the problem, as reformulated, remains, as far as I am aware, a problem for the degree theory of truth also).

The problem is essentially this – regardless of the number of (indefinitely-, indefinitely-indefinitely-, etc.) indefinite values, we may still always ask for the sharp boundary where S(x) *stops being definitely true* – and force an entailment of $\exists x(\text{Def.}S(x) \wedge \neg\text{Def.}S(x'))$ following any attempted rejection of the second premise in the sorites argument. Regardless of the 'fine-grainedness' of our multi-valued logic, such a stipulation will always look arbitrary: that a man with n hairs is definitely bald, and yet a man with n+1 hairs is not definitely bald, remains as improbable as in the very first case considered. Moreover, given more complicated cases of vagueness, as in the case of the predicate 'is surveyable', due to our inconstant ability as surveyors it follows that any such stipulation will (sometimes) be inaccurate.

[9] A sharpening of the predicate is essentially an application in use; hence, an application of 'small' when applied to the size of stars will clearly include many more potential quantities than when it is applied to balls.

By way of offering further considerations to mitigate this problem, but also because a proper account is by now overdue, I shall open the next chapter with a brief discussion of the precise meaning of the truth value 'Indefinite', before I attempt to provide any positive solution on behalf of a trivalent semantics to the issue just raised.

# CHAPTER X: HIGHER-ORDER VAGUENESS

Since three-valued logic offers us a profitable response to Dummett's charge of inconsistency, it is worth trying to defend it against the main criticism levelled against it, which is that it is susceptible to higher-order vagueness. Michael Tye offers a solution to the problem, which I shall outline and adapt in this chapter to offer a plausible defence of a three-valued logic for strict finitism. Without necessarily adopting Tye's model with respect to *vague objects* in the world, I hold nonetheless that the theory, when applied to the vagueness arising in the strict finitary formulation, will offer an intuitive pleasing method of rejecting Dummett's charge without entailing further problems of a 'higher-order' kind. I shall conclude this chapter with the consideration of a third alternative logic for the strict finitist, (one which will presumably be particular to the strict finitist), that of *surveyably-finitely-valued* logic.

As promised, however, and before we progress any further with the discussion of multi-valued logics in any sense, I shall begin with a discussion of the truth value 'Indefinite'.

## *The truth value 'Indefinite'*

One way to try to address the problem raised at the end of the previous section might be to provide a more rigorous analysis of what is meant by the term 'indefinite'. For example, one may raise the objection that there must be a sharp boundary between Definitely True and ¬Definitely True *only* if it is clear that Indefinite must always entail ¬Definitely True.

On the face of it, this may seem an obviously valid entailment. Indeed, various interpretations of Indefinite support such an entailment. The 'standard'[1] interpretation is

[1] If indeed there is a standard interpretation; although three-valued logic is not the most popular of theories, there have been various models of trivalent logics, with corresponding variations to the truth tables and to the definition of Indefinite – and by 'standard' I mean to imply only the more common interpretation.

that Indefinite is *neither* True nor False. Michael Tye, whose truth tables I have replicated here, and whose analysis of vagueness forms the basis for what follows in this chapter, defines Indefinite much in this way. He writes:

"The third value here is, strictly speaking, not a truth-value at all but rather a truth-value gap. In my view there are gaps due to failure of reference or presupposition and gaps due to vagueness". [2]

He footnotes this rather brief explanation as follows:

"Where a gap is due to vagueness, I maintain that something is said which is neither true nor false. I deny however that anything is said in the case where a gap is due to failure of reference. I am inclined to extend the latter view to gaps due to failure of presupposition".[3]

Hence, when it is asserted of a borderline member that it is a member of the (vague) totality, the truth of the assertion is indefinite in the sense that it is neither true nor false. It counts (for Tye at least) as a legitimate assertion – that is, one in which something is asserted – but it takes neither truth value.

Such a definition will of course invite precisely the kind of response outlined in the previous section, but can alternatives fare any better?

One possibility is to suggest that the value indefinite means both True and False. Something like this has been offered by Van Bendegem, for example, who (following Priest), describes an assignment of the third value to the set {T,F}, or {0,1} contrasting this with the usual assignment of value ½ to the Indefinite, if T is 1, and F is 0. If indefinite is regarded in this way, the problem raised above looks initially less threatening, since there will be no point between true members and borderline members

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[2] Tye, 'Vague Objects', p.544
[3] *ibid.* (footnote in original)

where assertions indicating their membership of the set are no longer *true*; but the proposed solution here looks a little shaky. There will come a point, of course, at which statements are no longer True – but in this case, it will be at the boundary between Indefinite and False, and not at the boundary between True and Indefinite as before. We might certainly expect it to be *not true* of non-members (that is to say, candidates of which assertions of membership are false) that they *are* members of the totality; but the sharp boundary implied by the observation looks equally as problematic as the first higher-order vagueness problem outlined above. The point is made explicit by the corresponding observation that there ought to come a point, on such an account, at the original boundary between True and Indefinite where assertions cease to be *not-False*; that is, there is an implied sharp limit on True assertions that are simply True and not False as well.

Perhaps the next plausible position is to consider the Indefinite value as taking *either*[4] the value True or False, but not determinately one or the other. In this way, one might attempt to simply side-step the challenge posed by the kind of objections we are here considering; one might say that there is no sharp boundary between the Definite members and the borderline members, simply because the borderline members might still possess the value Definitely True, (or simply 'True'), although they might not, and hence they are Indefinite.

We must be careful with such a response, however – it is not enough to hold that the borderline cases take one of the bivalent truth values but we *don't know which*, so we call them Indefinite; this looks tantamount to an Epistemicist response, and we have already rejected that as unpromising for our account. We don't want there to be a 'fact of the matter' about *numbers, per se*; at least with respect to whether or not they are surveyable, since we have already acknowledged that capacity to survey varies by instantiation.

[4] I am considering here the suggestion that Indefinite means True or False in an exclusive sense. I think there is a corresponding question about a stipulation of Indefinite as True $\vee$ False in the inclusive sense; this seems (in a similar way) to avoid the problem of higher order vagueness, since there is no point at which to ask when the assertions of membership are no longer *True*; but in the end, it is still the case that a case satisfying this condition will possess one of the following truth values: True, False – whether or not it possesses the other.

But if we do not mean that we just do not *know* which truth value borderline cases take, in what sense can we mean that Indefinite means *either* True *or* False? One way of thinking about this kind of idea might be to try to draw an analogy with a similar case in physics. Modern quantum physics teaches a great deal of counter-intuitive 'facts' about the world. Among them, it tells of how there is a peculiar property of electrons, such that, according to Heisenberg's uncertainty principle, you can measure either the position or the velocity of an electron, but you may never attain both. There is a sense in which the act of measuring 'forces' the electron into a 'new' state, a state such that it possesses the measured property. Now, let us imagine we are interested in measuring one or the other. Call the state in which the electron is at the point of measurement of position P, and the state in which it is at if instead we measured velocity V. Now, prior to any measurement, what are we to say of the state of the electron? As I understand it, the electron can't be in state P, or in state V, but nor can it be correct to say that it is in state P AND V – since the act of measurement will *alter* the state such that it is P OR V but not both. Furthermore, it can be no more correct to say that the electron is in neither state P NOR V (that is, in state P or V or neither), since if that were the case then the act of measuring will tell you nothing about the electron you were trying to measure. Instead, then, the electron is in an *Indefinite* state prior to measurement. Nor is this simply an *epistemically* indefinite state, as I discussed in the previous paragraph. The act of measurement is such that it *brings about* a physical state, but nonetheless a state that pertains to *that* electron.

Perhaps then, this is a useful way to think about Indefinite as a truth-value. It is not a demonstrable truth-value in reference to the truth values True or False, but is a value in its own right. It is in some sense correct to say that if a proposition is indefinite it is neither true nor false, but *only in some sense*; that sense being that it does not possess the truth value True, nor False, but instead Indefinite – but what this is *not* to say is that there is an *absence* of truth or falsehood, since it is precisely that their presence cannot be confirmed or denied that leads to the proposition having the truth-value Indefinite in the first place. Just as it is wrong to assert either that an electron is in a particular state prior to measurement or (at the same time) that it is *not* in such a state, so it may be wrong to assume that because Indefinite is distinct from the values True or False it is itself entirely neither.

However, this is not to do so much, at this stage, as provide a particularly useful account of the value Indefinite; and perhaps all it can really suggest is what it is not. Indeed, the quantum case just begs the question regarding the third value, because we have no useful model of indeterminacy in *that* case either. Aside from illuminating the problem, it does not seem as if the analogy can provide any clear definition of what it is for Indefinite to mean *either* True or False, since the quantum case seems to cry out for adequate definition in exactly the same way.

The conclusion here then is that in general, the standard interpretation of Indefinite, such that a statement which is Indefinite is then neither True nor False, looks as at least as promising as the alternatives. It is extremely difficult to avoid the issues of higher order vagueness, and as such it is perhaps better to formulate a strategy to meet them head on. It is to such a solution that I shall turn my attention in the next section.

## *Tye vs. higher order vagueness*

Michael Tye offers a solution to sorites problems using a version of three-valued logic, as I outlined above. What is importantly distinct about Tye's account however, and a feature that I believe will enable us to provide a useful response to the problem we are left with (that of the precise termination of Def.S), is that Tye maintains that as well as there being three classifications for candidates for membership of a vague totality, corresponding to whether the relevant predicate is true, false, or indefinite when applied to that member it is furthermore indefinite whether there are any 'remaining' members of the (or any) given vague totality that "are neither members, borderline members, nor non-members".[5] The theory is hard to conceptualise, so let me begin with Tye's own example:

[5] Tye, 'Vague Objects', p.536

"Consider Mount Everest. It seems obvious that there is no line which sharply divides the matter composing Everest from the matter outside it. Everest's boundaries are fuzzy [vague]. Some molecules are inside Everest and some molecules outside. But some have an indefinite status: there is no objective determinate fact of the matter about whether they are inside or outside."[6]

So far, the example is sufficiently similar to those I have already provided – it is true of those molecules that are inside that they are part of Everest, false of those that lie outside, and indefinite of those which are borderline cases. But Tye continues:

"Are there any remaining molecules? To suppose that it is true that this is the case is to postulate more categories of molecules than are demanded by our ordinary, everyday conception of Everest and hence to involve ourselves in gratuitous metaphysical complications. It is also to create the need to face a potentially endless series of such questions one after the other as new categories of molecules are admitted. On the other hand, to suppose that it is false that there are any remaining molecules is to admit that every molecule fits cleanly into one of the three categories so that there are sharp partitions between the molecules inside Everest, the molecules on the border, so to speak, and the molecules outside. And intuitively, pretheoretically, it is not true that there are any sharp partitions here. What, I think, we should say, then, is that it is objectively indeterminate as to whether there are any remaining molecules."[7]

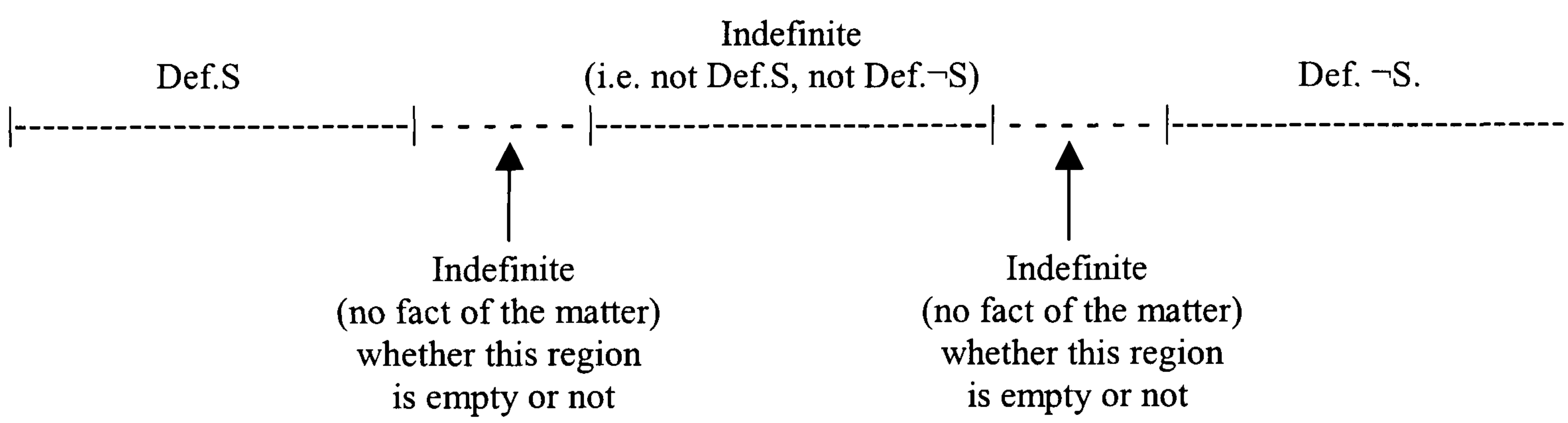
I should perhaps be careful to point out that Tye is advancing a more general thesis than I am at this point; to the extent that there are, or could be, vague *concrete* objects, such as mountains. Since, as far as I can see, the debate over whether there are or can be vague concrete objects – that is objects which themselves are vague *independent*

[6] Tye, 'Vague Objects', p.535
[7] *ibid.*

of our ability to categorise them – is very much unresolved,[8] and given that my concern here is primarily with *mathematical* objects (and moreover with a determinedly anti-realist, that is, constructivist, account of mathematical objects), I shall abstain from commitment regarding the generality of Tye's claims, and restrict my application of Tye's solution solely to the case of vagueness regarding (mind-dependent) abstract objects, as in the considered case of the totality of surveyable numbers.

Tye's suggestion, then, is that in addition to there being an indeterminacy at the level of membership (for parts of a vague object, for vague sets/totalities and so on) to the extent that while some candidates are genuine members and some are not it is indeterminate whether a further class of candidates are genuinely members or not, it is *further* indeterminate whether or not there are any *remaining* candidates not covered by these three classes.

Roughly, Tye's model is something like this:




Moving away from the concept of concrete objects, Tye describes the application to sets in general:

[8] Although the focus of criticisms of Tye's position has been concerning his assertion that there are vague concrete objects; that there is 'real-world' vagueness. There is no need to make such a commitment in order to utilise the principle he advances in the way that I shall do here.

"A set S [is] vague, if, and only if, (a) it has borderline members and (b) there is no determinate fact of the matter about whether there are objects that are neither members, borderline members, nor non-members."[9]

The extension to the case of (surveyable) numbers is then apparent. It is true of some number-patterns that they are surveyable (and hence, to be precise, on a strict finitist account that they are *numbers*), false of others, and indefinite (in the sense of *indefinite* as a distinct truth value) of some other, borderline cases, that they are surveyable (that they are admissible numbers). Furthermore, it is indefinite whether or not there are further 'classes' of number-pattern – whether there are number candidates not captured by the three categories.

It may perhaps seem counter-intuitive to consider even the *possibility* of further number candidates in this case; particularly to someone who is resistant to admitting a borderline class in the first place. Obviously, I suggest that we have good reason to expect borderline cases with respect to Surveyability, since any attempt to describe it as a precise predicate seem doomed to failure. But in terms of intuitive justification for the possibility of further candidates, (over and above that implied by the problems of higher-order vagueness), I offer similar considerations as Tye does with respect to Everest. Firstly, to suppose that there *are* further 'classes', borderline-borderline members and so on, is to postulate more categories than our analysis has so far suggested. But secondly, to suggest that there are not entails that there is a sharp cut-off between numbers which everyone can (always) survey, and those candidates which not everyone can (reliably) survey. And, of course, that there is a corresponding sharp boundary between those number candidates which some people can (sometimes) survey, and those which no-one can (ever) survey. And here too, as in Tye's case, such boundaries seem simply implausible in this case.[10] In fact, if they did seem plausible, we might with some justification simply choose one or other boundary as a limit for surveyability – hard surveyability, on the one hand, which stops just as soon as anyone cannot (always) survey

[9] Tye, 'Vague Objects', p. 536

[10] No less implausible, I think, than the original idea that there might be sharp cut-off points between what is true and what is false, on such an account – it is just that we do not need, in this case, to actively postulate any further categories (and corresponding truth values) than the three we now have.

a given candidate; or soft surveyability on the other, where the precise limit is determined at the point at which even the very best surveyor (under optimum conditions) can no longer survey. If we were entitled to such boundaries, the problem of the sorites paradox disappears – we have something like an epistemic solution. But, (pretheoretically perhaps), neither the assertion that there *are* further classes of number candidates beyond surveyable, indefinitely surveyable, and not surveyable, nor the assertion that there *are not* seem plausible. The extension of each class just looks to be itself indefinite. Hence we have good, intuitive reasons for taking Tye's suggestion seriously in the case of Surveyability.

Tye's case offers us a fresh approach to problems of a sorites kind, and moreover, as I have already intimated, it provides the tools with which to deconstruct the problem of higher-order vagueness that we were left with earlier in this Chapter. Tye himself considers the application of his idea to the case of sorites paradoxes. For any sorites paradox (of the general form:)[11]

| | |
|---|---|
| $S(1)$ | (1) |
| $\underline{\forall x\,(S(x) \rightarrow S(x+1))}$ . | (2) |
| $\forall x\, S(x)$ | (3) |

he maintains:

"that (2) is either false or indefinite and *not* that it is false as the classical reasoning supposes. Secondly, (2) is, in fact, indefinite in truth-value."[12]

The problem, according to Tye, that we have been facing all along rests upon the idea that denying the truth of the second premise in the sorites argument is taken to be an assertion that the premise is *false*. But this need not be the case – in a three valued logic,

[11] Tye's actual example is that of bald men: his premise (1) is that a man with *zero* hairs on his head is bald; I use here my own formulation of the general paradox from a previous chapter in order to try to retain uniformity.
[12] Tye, 'Vague Objects', pp. 547-8

a denial of the truth of a statement is not an assertion of falsehood; rather the assertion that it has a truth-value *other than* True; that it is *either* false or indefinite. This is an important step, as we shall see below, but is already enough to mitigate many of the problems we have so far encountered.

Moreover, as should be plain by now, it is Tye's contention that a universal quantification of the second premise of the sorites paradox will be indefinite. Since there are no assignments under which (2) is false (because of the implausibility of *any* sharp boundaries for vague predicates), yet there are assignments under which both the antecedent and the consequent are false, the second premise understood as a universally quantified statement will itself be indefinite. That is, Tye maintains that:

$$\forall x\ (\ S(x) \rightarrow S(x+1)\ )\quad (2^*)$$

is indefinite.

Tye's next move is to suggest that if (2*) is indefinite, then statements of the form:

$$\neg\exists x\ (S(x) \wedge \neg S(x')) \qquad (4)$$

must be indefinite also, since (4) is equivalent to (2*). Then, of course, if (4) is indefinite, the negation of (4) must also be indefinite, and so it has not been shown that there is (for example) an n such that it is surveyable and its successor is not.

We must be a little careful here, however, given what I have already said regarding the equivalence of the conditional and the conjunction in these expressions. They are *only* equivalent if the (or at least the second) negation operator is interpreted in the proper way – not as an assertion of falsehood, but as a denial of truth. The point here is perhaps more clearly made with reference to the boundary between 'true' cases and borderline ones. Applying the same analysis, it should be clearer that

$\exists x\,(S(x) \wedge \nabla S(x'))$     (5)

is indefinite. Since the 'first-order' sorites has already been resolved simply by the introduction of a third truth-value, I will not embark upon a further critique of Tye's extrapolation – it is sufficient for my purposes that what he says (or perhaps should have said) applies straightforwardly to (5), if not, without the correct interpretation, to (4).

Now we can return to the problem I set out to solve here. Recall that my earlier discussion of the challenge of higher-order vagueness culminated with an identification of a general, and at that point, seemingly insuperable problem; that of the boundary where S(x) *stops being definitely true.* It seemed as though, whatever the prospect for many-valued theories, we might conceivably always ask at what point 'full' or definite truth stops, and the first (no matter how finely-grained) non-true value starts. But now, we have a model with which to offer a solution to this problem. We have already seen that (5) is indefinite. But since (5) is indefinite, it follows that:

$\exists x(\mathrm{Def.}S(x) \wedge \neg\mathrm{Def.}S(x'))$     (6)

will be indefinite also. And so the paradox here is averted also. Conceptually, this final point is harder to grasp, so let me try to elaborate a little. The idea is essentially that the point at which the numbers stop being definitely surveyable is itself not precise – but 'not precise' *in the sense that it is indefinite whether it is precise or not.* Because it is indefinite whether there are members of the set between those which are Def.S and those which are ∇S (which are indeed ¬Def.S), the *extension* of Def.S, in the case of vague predicates, must be itself indefinite. It no longer seems good sense to ask whether there is a sharp boundary between definitely surveyable numbers and 'indefinitely-definite' surveyable numbers; to do so is precisely to ignore the meaning of indefinite.

Tye's solution to the general problem presented by higher order vagueness is thus to embrace the rejection of the second premise of sorites-type arguments, but to maintain that this will not, as is usually objected, lead to a further, higher order sorites centred around the boundary between True and Indefinite. Although this border is assuredly vague, it is not vague in some 'higher-order' sense; instead, the presence of further members, such that would be admitted by moving to four- or five- values and beyond, is *itself* indefinite, in the first order sense.[13]

One might perhaps wonder, at this stage, why Tye admits the presence of any borderline cases at all – he might have offered instead an account which suggested that it was indefinite whether there were any borderline members at all, between those members which are definitely extensions of a given predicate, and those which are not. But, recall, his initial criterion requires of a vague set that it has borderline members. Presumably, the reason here is that it is just intuitive, at this stage, that vague sets *do* have borderline members; it does not seem intuitively plausible to suggest that it is indefinite whether the set of surveyable numbers has any borderline members; for example, clearly, there are cases of men with some hair where we are uncertain as to the correct classification of their baldness or lack of it, and hence the set contains borderline members. Secondly, since a trivalent logic will be necessary anyway, to make the claim that it is indefinite whether there are further members, the logic already supports the inclusion of 'first-order' indefinite members.

What is perhaps most attractive about Tye's account is that it incorporates the property of vagueness into the *account* of vagueness. Many of the theories of vagueness attempt to solve the problem by providing a precise description of the notion – that is, they offer a reductive account of vagueness which is *itself* determinate. Even in many trivalent solutions, the meta-language attempts to be precise, in that sentences in the meta-language are expected to have only bivalent truth values. It is *true* that there are three candidates for truth function, and *false* that there are any more than this. Tye's approach, on the other hand, preserves the vagueness in the meta-language. Perhaps, for some, this won't look like much of a solution at all. Perhaps Tye seems to have pushed

[13] Tye acknowledges that some objectors may here wish to suggest vicious circularity, but his response is succinct and convincing on this point. He has three reasons for denying this claim; for a full explanation see Tye, 'Vague Objects', pp. 545-6

the problem back, and is now guilty of a different kind of higher-order vagueness. But I think rather that we should *expect* the vagueness to infect the meta-level; if it does not, we seem to lose the original concept. It is, I suggest, an implausible task to precisify the extension of vague operators, since to do so is tantamount to insisting that they are not really vague at all – or only vague in a weak, misunderstood sense. Surely vagueness is stronger than that. Once we admit that vagueness can be (in any sense) sharply delineated, we lose the original intent, and moreover will leave ourselves open to precisely the kind of problems posed by higher-order vagueness and the like.

*Surveyably-finite-valued logic as a solution to Higher Order Vagueness?*

I believe that the analysis above is the best solution to the problem of higher-order vagueness, and I offer it as a solution on behalf of the strict finitist attempting to advance a constructivist foundation for mathematics which retains commitment to what Dummett has identified as weakly finite and weakly infinite totalities. Tye has, at any rate, illuminated a methodological error in the analysis of many of those who reject the three-valued approach to vagueness, in that a denial of the second premise in sorites arguments is not equivalent to an assertion that the premise is *false*. This alone should give us cause to consider the validity of such rejections, even if it proves to be the case that Tye's solution cannot have quite the range of application he desires it to (to the extent that he wants to establish a corresponding vagueness about concrete objects), and even if it were not the case (although I suggest it is) that the analysis as presented proves sufficient to provide a robust account for the vagueness inherent in the case of predication involving (mind-dependent) abstract objects, such as in the example of Surveyability.

However, as I have also suggested before, I am not convinced that this is the *only* solution for strict finitism, and that the theory must stand or fall with the solution presented here. In the next chapter, I shall begin to look at alternative formulations, which do not accept the purported commitment to weakly finite and weakly infinite totalities in the first instance. Dummett seems to simply assume that the commitment is necessary, and there is, I maintain, a good deal more to be said here.

Before I proceed to this discussion, I wish to conclude this chapter with an examination of what I referred to at the opening of the previous chapter as 'surveyably-finite-valued logic', and raise one or two further issues there. I consider this as an alternative solution to that offered above in light of Tye's work, and one which is certainly still in line with the commitment to weakly finite and weakly infinite totalities; although it is, I believe, ultimately unpromising. Nonetheless, it seems a natural extension of the earlier ideas I have presented, and so I offer it partly for the sake of completeness.

The conjecture of a surveyable-finite-valued logic follows on from consideration of infinite-valued logics; for example, the logic proposed by degree theory. As I have said, infinitely-valued theories are obviously not available on any kind of strict finitist approach. And the major problem for finitely-valued theories[14], like many three-valued theories, is that of higher-order vagueness; no matter how many values we retreat to, we always meet the objection that there might then be sharp boundaries between members of a given totality when divided into groups possessing these distinct values.

However, there is a further thought here, and that is simply that for all *practical* applications, we will always only need a finitely-grained theory. That is to say, in the example mentioned previously, if we consider a set of eight men, we will only ever (at most) require eight truth values to properly ascribe any (vague) predicates to them. The problem of which of them are bald is no problem if we can have (at most) eight 'degrees' of baldness into which to categorise them, such that it is 100% True of this one, say, 90% of this one, 65% true of this, and so on. Moreover, for any finite set, we will only ever need a finite number of truth-values. For any given case, the practical assignment of truth values will only ever require a finite number of values. The problem of sharp boundaries need not arise – there *are* (unproblematic) sharp boundaries, provided that we have the requisite number of values to distinguish between different cases. Now clearly, eight is

[14] Though not necessarily exclusively – as I have previously remarked, the most forceful of the higher-order objections looks to be a problem for infinitely-valued theories too.

not sufficient for all cases, so we might need a good deal more for a general theory of this kind. Indeed, generalising for all cases, it might look as if we need to move towards at least a potentially infinite number – at least, it must be large enough to distinguish between cases in the largest possible set with the largest possible variety. And even if we need not necessarily accept infinite sets at this point, a number of this magnitude is clearly well without the bounds of surveyability – and hence should be rejected by the Strict Finitist, it seems.

But now let us consider the case of Surveyability only. It is, I expect, plausible to suggest that 1 is 100% surveyable (so possesses the first truth value, say $T_1$) and some number-pattern (2k, in earlier examples) is 0% surveyable (and so possesses the last truth value, say $F_0$). Now, for all numbers in-between, they will possess a degree of truth with respect to surveyability of equal to or between $T_1$ and $F_0$. The precise extension of each truth value, whether by complexity, or individually with respect to each number will depend upon the successful application of criteria already much discussed, but it should be reasonably clear that we will never require more truth-values than there are positive integers.[15] And so, with respect to numbers, as a Strict Finitist will define them, we will only ever need a finite number of truth values to correctly assign the Surveyability predicate to them. Moreover, since it will never be false of this number (the number of truth values) that it is surveyable, it follows that only a surveyably-finite number of values will be required. So it seems as though we may operate with a surveyably-finite-valued logic which will nonetheless be able to correctly assign the truth of the predicate 'is surveyable' to every object in the domain.

As I said, this seems in some ways a natural extension to the previous discussion about many-valued theories. However, I have also described it as an unpromising solution, one of the principle reasons being that it doesn't seem easily generalisable as a solution to vagueness. Perhaps, insofar as it refers to numbers (and numbers as mind-

[15] This may, in fact, only be entirely clear if we are considering the totality of positive integers. But as I explained in a previous chapter, I think similar considerations may be applied to various other totalities, such as the reals; I find it reasonable that the number 0.2 will be just as surveyable as some integer, although the precise value will depend upon which integer, given both notation and ease of intuitive grasp, is roughly equivalent.

dependent constructions at that), there is some potential application here, but one might think that any proposed solution to the problems of vagueness should be capable of wider application, and not advanced simply on a needs-related, individual case basis. There is also perhaps a more specific objection here too - the thought may be that if there can be vague objects, for example, like mountains, whose properties do not depend upon us in any relevant sense, then a *surveyably*-finite-valued logic will be inapplicable, since the variation required in the 'degrees' of truth would depend upon the actual amount of variation in objects (in the case of the mountain, something like the variation in relative proximity of molecules), and not on our intellectual limits.

Well, to both the general and specific objections, I should firstly like to observe that I do not think that a surveyably-finite-valued solution is restricted *solely* to the case of surveyability; and while I shall devote no more time to its development here, I suggest that it seems plausible that it would equally well deal with vagueness of any kind that in some sense depended on, or arose from interaction with, us. And furthermore, the idea that vagueness might, as a whole, require more than one solution is not altogether unpalatable. More than one commentator in the literature has drawn a distinction between different *kinds* of vagueness – Dummett himself makes a distinction between what he describes as 'observational' predicates, including among them those vague predicates whose vagueness has its source in the non-transitivity of non-discriminable difference, and those predicates where we might think the source of vagueness were simply an under-determination of (something like) semantic convention. So, if such distinctions are genuine, need we expect there to be a general solution? It may well be the case that if there is more than one *source* of vagueness, there may be more than one problem here. As a result, as long as a surveyably-finitely-valued logic were *locally* generalisable, that is to say that it could be applied to any vague predicates that were relevantly similar (in *kind*, or rather such that the sources of vagueness in each case were of a sufficiently similar kind), then I do not see why one should necessarily expect it to be generalisable in any wider sense.

Of course, this is not to determine the precise scope of application of the solution, and it may turn out that the scope is too narrow to be of genuine use. It will not be ordinarily applicable to colour predicates, for example, unless they are as Dummett describes, entirely governed by what *looks* a certain colour to us. If this is the case then, again, our ability to recognise different colours will be important, and we will not need any more truth values than the 'grainedness' of our perception warrants. But even if Dummett is right about colour predicates in this way, it still looks entirely contingent that the number of colours that we are capable of discriminating is itself *surveyable* (assuming, in fact, that it is). Moreover, I have taken pains elsewhere to suggest that surveyability is *not* of the kind of vagueness that Dummett describes as Observational in this sense, so even if the application were to be (contingently) applicable in the case of observational predicates, it is unlikely to be because they share a 'type' with the predicate 'is surveyable'.

Since I can provide no clear solution to the objections outlined, I do not propose to take the idea of surveyably-finite-valued logic seriously as a solution to vagueness, even in the restricted case. There does however remain something intuitive about the idea that the more truth values we have access to, the less unappealing the notion of sharp boundaries becomes, when presented with any practical assignment of truth. And the thought that for our *understanding* of truth, at least, a surveyable number will suffice for *any* given assignment in this way, also remains intuitively plausible[16].

[16] In addition, the surveyably finite-valued approach relates quiet closely to the broadly epistemicist approach to finitism that I shall outline in the next chapter. So we shall return to something like it shortly.

# CHAPTER XI: ALTERNATIVE FORMULATIONS

Finally, I wish to suggest and examine some further possibilities for the strict finitist, following on from a much earlier observation that the commitment to weakly finite and weakly infinite totalities has been simply assumed by some commentators, and it does not seem to me to be a necessary feature of strict finitary theories. I hope I have shown that commitment to such totalities is not, in fact, inconsistent for the strict finitist, and I have offered an account of vagueness for surveyability, following the work of Michael Tye, that attempts to avoid the problems raised by Dummett and others. To the extent that such a solution may continue to be supported, I suggest that strict finitism, even one committed to totalities of the kind proposed, remains a viable theory for the foundation of mathematics. However, a number of considerations have led me to question whether, in fact, the strict finitist *should* admit that the totality of (surveyable) numbers is both weakly finite and weakly infinite as Dummett defines them; and I will outline these considerations, and the corresponding alternative formulations, in what follows here.

It is perhaps worth taking a moment here to explain the structure of this debate, and its relation to what has been discussed in previous chapters. I see the question of whether a strict finitist needs to be committed to these totalities in the first place as essentially distinct from the issue of whether or not the contradiction suggested by Dummett can be countered. That is to say, although I have offered a rejection of the purported inconsistency, I suggest that the strict finitist has the option of not needing to do so, if she rejects instead the idea that the totalities to which she is committed need be both (or indeed either) in the first place. Now, assuredly, there remains a problem for totalities like 'the number of heartbeats in my childhood', and if the strict finitist accepts the definitions for 'weakly infinite' and 'weakly finite' she must surely agree that such a totality seems to be both, and hence problematic; but this looks like a perfectly *general* problem, and a problem indeed for Dummett. Presumably Dummett must agree that such a totality is both weakly finite and weakly infinite – and yet it seems odd to doubt as a consequence that there is a genuinely plausible totality of heartbeats in my childhood. The problem of the apparently contrasting definitions need not necessarily be addressed by the strict finitist, then; since, as I shall show in this chapter, she is entitled to accept only one (or indeed neither) of the definitions as applying to the (purely mathematical) totalities to which she is committed.

Let us begin with an examination of the first of these possibilities: an acceptance of the weakly finite definition, and a rejection of the weakly infinite.

### *Rejecting the 'weakly finite'*

The strict finitist has (at least) two options in rejecting the definition of the totality as given; firstly, we may reject the claim that the totality in question is *weakly infinite*. That is to say, we might try to give an account in which there *is* a last member, even if we cannot give the precise last member. This may sound like a broadly epistemicist response; and I have so far rejected epistemic accounts of vagueness for surveyability, partly because it does not seem plausible that there will be a 'fact of the matter', at the world-level, as it were, regarding mind-dependent constructs, and secondly, because it does not seem plausible that this fact of the matter obtains independently of the differing capacities of individual surveyors. However, I will go on to consider a special case, later in the chapter, which I believe may provide a plausible route to this sort of solution, while avoiding the apparent conflict with a more traditional epistemicist account.

The second option, and the one I shall begin with, is that the strict finitist might deny that the totality of (surveyable) numbers is *weakly finite* (that is, we may deny that the totality is upper-bound – deny that we can genuinely give an example of a number which is not in the totality). The suggestion that we cannot stipulate a number such that it is not in the totality is actually intuitively plausible in the strict finitist case, especially considering the constructivist root of strict finitism: it is not as if we can provide an example of an *unsurveyable number*, strictly speaking. Note that this is a significant difference between the case of surveyability and various other cases of vagueness – while we are clearly able to provide examples of non-bald men, say, the strict finitist will not be willing to admit that there genuinely are examples of non-surveyable numbers. As I have earlier described, the strict finitist should, properly speaking, resist the classical use of 'number', since to count as an admissible number for strict finitism is a harsher requirement than it has traditionally been, and speak instead of proto-numbers (or number-patterns, or some such) as contrasted with genuine *numbers* – the latter of which will of course only ever be surveyable.

We are still left with a traditional problem, in this case that of identifying the last number. But remember, under this option, the strict finitist remains happy with the definition of the totality as being weakly infinite, and so no definite last member should be expected. The totality is not infinite in the traditional sense, of course – it is still not closed under the successor operation, for example. The claim that the totality is not bounded-above is not tantamount to admitting that it is (strongly) infinite – just because there is no boundary it does not follow that the totality extends indefinitely. Conceptually, the distinction here is rather like that between the potential infinite and the actual infinite.

Let us look at the suggestion a little more closely. In rejecting only the weakly finite part of the definition, the strict finitist retains the idea that the totality is indeed weakly infinite, and that it will therefore be the case that there is no number that is surveyable but such its successor is not. Formally, where S(x) applies the predicate 'is surveyable' to x, and x' is again the successor of x:

$$\neg\exists x\ (S(x) \wedge \neg S(x')) \qquad (1)$$

But at the same time, the strict finitist in this case is rejecting that the totality is weakly finite, such that it is *not* the case that there is an example beyond the upper boundary:

$$\neg\exists x\ \neg S(x) \qquad (2)$$

The strict finitist will now make one of two claims. Firstly, he may say that there is no number such that it is not surveyable, because all numbers, (as properly understood), are surveyable – by definition. That is, from (2), we can straight-forwardly (and using only classical logic) derive:

$$\forall x\ S(x) \qquad (3)$$

It may look at first glance as though this is too strong a claim on behalf of the strict finitist. If the domain is *surveyable* numbers (since numbers are surveyable by definition on this response), then it is simply tautological to say of them that they are all surveyable. But this is perhaps to be expected on such an account, since what we are essentially suggesting is that the totality of possible numbers *is that* of surveyable numbers. It is precisely a rejection of the platonist conception of the number line which motivates this position in the first place.

The alternative response at this point might be to resist the strong claim that all numbers are surveyable, and instead stick with (2), and make a weaker (more agnostic) claim, in line with intuitionistic ideas about mathematics, that *there is no number which lacks the property of being surveyable.* Notice that in intuitionistic logic, (2) does not entail (3), so, as long as commitment to (at least) intuitionistic logic is acknowledged, the strict finitist is entitled to the latter claim without admitting the former.

It is not clear that this second response is particularly advantageous over the first, however; even in (2) the strict finitist would have to agree that the range of the quantifier includes only surveyable objects, and so if there is a genuine problem with the stronger assertion (3) there is an equal problem for this account. The strict finitist is entitled to assert (3) here because if it is false that something is surveyable, it is also false that it is a number. So S(x) cannot be false for anything in the domain.

Additionally, the strict finitist might have a good independent reason for rejecting the definition of weakly finite as provided by Dummett. Dummett's definition is essentially that a totality is weakly finite if and only if there exists a finite ordinal n such that there is a well-ordering of the totality with no nth member. But now consider the finite ordinals themselves. Presumably Dummett's strict finitist should think this is a weakly finite totality – but that requires a well ordering of it with no nth member for some *finite ordinal* n. And this looks to entail an immediate contradiction.

The rejection of the 'weakly finite' is then I suggest also a promising alternative for the strict finitist. I shall now move on to a discussion of the rejection of the 'weakly infinite' restriction; the strict finitist is, as I have suggested, able to reject either or both of these restrictions, and the theory I shall ultimately offer in this chapter relies on neither definition, as we shall see, but arises out of considerations relating to the rejection of the weakly infinite restriction. It is, I think, entirely compatible with the weakly finite restriction, although, especially following the remarks above, it is not obvious why anyone should wish it to be so at this stage.

### *Rejecting the 'weakly infinite'*

The rejection of the weakly infinite constraint may seem like an odd route for the strict finitist. Certainly, the claim that there is no last member is the focus of many problems for strict finitism, especially when coupled with the suggestion that this nonetheless does not entail that the members continue on *ad infinitum*, as it were. And the stipulation of an actual last member might suggest that commitment to vagueness need not after all be necessary for strict finitism. But any such stipulation looks implausible, along lines I have already much discussed. Various important questions must be answered. Broadly, the most significant ones are as follows:

i) If there is a last number, what is it?
ii) Why can't we just add one to it?
iii) And doesn't it have to be the same for all people?

In an earlier chapter, I commented that the second of these three questions might be met be a strict finitist who was tempted to stipulate a last (largest) number; the strict finitist, I remarked, might simply insist that if it is the last number it is precisely so because you cannot add to it. Various philosophers and mathematicians have already begun to develop an arithmetic along these lines – Yesinin-Volpin for example. But it still seems to me that the problem raised by the third question looks insuperable for any account that stipulates *of numbers* that they are or are not surveyable. People have differing abilities to survey; the need to generalise does not take adequate account of the relativity of the 'human factor' in such accounts. Moreover, when it comes to answering the first question, most commentators are silent. This is perhaps to be expected, since the construction of such a number is assuredly difficult: indeed it is certainly at (or at least among) the limit of what we can actually achieve. So what are we to say? That there is a largest number, but we don't (perhaps can't) know what it is?

This is of course the response suggested by the epistemicist account of vagueness, but I have also previously remarked that this looks perhaps especially unpromising for an account of surveyability, largely because any such account must surely answer yes to question three above, when such an answer just looks wrong in the face of what actually goes on when surveying numbers. But perhaps the questions are separable, as I have separated them in this list; and one might reasonably ask whether the epistemicist response is sufficient *if* a convincing solution to the problem of multiple surveyors might be found.

I hope to provide answers to all these questions in what follows; and not least, to provide an account of a form of strict finitism that I believe will enable us to deny that strict finitism is committed to weakly infinite totalities in the first place – indeed, they may simply require only a strongly (ordinarily) finite one – while providing us with satisfactory answers to the problems raised in questions i)-iii) above. Before I begin, let me be certain I have been clear about the motivation: we reject the idea that strict finitistic totalities are weakly infinite, and so we reject the claim that, for the totality of surveyable numbers:

$$\neg\exists x\ (S(x) \wedge \neg S(x')).$$

More specifically, we are here considering the plausibility of a strict finitism that is committed to the positive claim that there is a largest surveyable number:

$$(\ \exists x\ S(x)\ ) \wedge (\ \neg\exists y\ (y>x)\ )$$

Firstly, let me outline precisely what I intend when I suggest that we may *stipulate*, with such a theory, that there is a last number. Admittedly, my aim here is to explore an alternative formulation, which is intended to overcome the related worries of a 'vague limit' to the admissible numbers, by a stipulation, *in a sense*, of a precise limit. I do not, however, wish to make that stipulation arbitrarily – nor do I anticipate the uncovering of a particular number x, such that no greater number is surveyable. It seems to me that such alternatives are unpromising. Following the first option, it seems that any arbitrary stipulation simply will not do, for the perfectly plausible reason that even if I were to uncover a number such that at that precise moment I was incapable of understanding (that is, of surveying) another - slightly greater - one, there is no reason why I may not do so at another time, nor indeed would it prevent someone with a better capacity for surveying doing precisely that. Moreover, while a completely arbitrary stipulation may trivially solve the problem of vagueness – if I were to suggest, for example, that there are precisely $10^{10}$ surveyable numbers and no more – I have no better reason for choosing one particular number over another; indeed no reason to choose any one at all.

On the other hand, the project of uncovering a number such that no greater number is surveyable, just as a matter of fact, seems equally doomed to failure – why should there be such a number? Indeed, if we are serious about surveyability, it must be admitted that one's ability to survey varies, and the ability varies between individuals. Unless the number lies considerably above my own ability to survey, to allow for better minds, present or future, then it seems ridiculously counter-intuitive to suggest that there might be a matter of fact about numbers such that even with the apparent mental capacity to construct and survey numbers above a certain point, we would not be able to do so. And if the limit were considerably above my (or anyone else's) ability to survey, it is unclear how we would go about identifying it; moreover, the stipulation, construed in this way, seems to possess a most peculiar quality – the proposal entails that there is a number, above which I cannot survey, which itself lies well above my capacity to survey.

Well, fortunately, these worries do not require serious attempts at answers, since I have already said that neither of these options amounts to what I wish to propose here. Instead, I want to extend the constructivist line of the strict finitist in a natural, if perhaps unusual direction. One motivation behind the strict finitist claim, as I have previously described, is that the intuitionists do not go far enough – in short, they are wrong to stop at in principle possibility (for the construction of numbers), and instead should desire *in practice* possibility. Now, I suggest, there is a further line of thought that suggests that even this is not rigorous enough, on a constructivist account; and that what we should be concerned with is *in practice actuality.* The central thought is then something like this: the genuine numbers are only those mental constructions that *have been* (surveyably) constructed[1]. At any given time *t*, the totality of numbers comprises precisely those numbers that have been surveyably constructed *up to time t.* Similarly, the largest number at time *t* is always precisely the largest number that has been surveyably constructed by time *t*. Note that the requirement is not that the number is present in a mind at the time – one does not have to actually *be* surveying a number for it to count; such a requirement would hopelessly narrow the surveyability constraint. Rather, numbers are mental constructions, and as such do not exist *until they have been constructed.*

This may seem counter-intuitive for a number of reasons, but let me deal with the first and most obvious objection straight away. Initially, it seems wildly implausible to suggest that even the numbers 3 and 22, for example, were not to count as legitimate numbers prior to their construction in a mind. But let us consider the following analogy. Suppose I wish to construct a new alphabet. Perhaps I have a new language in mind, or else I am trying to catalogue one which has so far only had a spoken component, and I do not wish to use an existing language in case I mis-represent the spoken language as a result. Now, let us say that I construct fifteen letters in this alphabet, no more and no less. Now it is correct to say that my alphabet has fifteen letters in it, and it is absurd to suggest that it has infinitely many (or indeed any higher number of) letters in it, but that only fifteen have been identified. Nor does this preclude me from adding to the alphabet at a later stage – after cataloguing the

[1] Not that I wish to imply that there can be *un*surveyable mental constructions. I include the prefix here rather to remind the reader that the constructivism under consideration is *defined* by surveyable operations.

rudimentary words and phrases, I discover that my alphabet is insufficient to portray the subtleties of the spoken language properly, so I add five more letters to compensate for the deficit. Now surely it is correct to say that there are twenty letters in the alphabet, but not that there always were. There is an obvious objection, of course, that numbers are not like letters, and that alphabets are designed with a certain number of letters, even if that number may be expanded; whereas numbers are representative of a formal system, with rules governing their continued creation, according to a fixed framework. Well, the first response to such an objection is simply a note of caution: we must be careful not to think about numbers in the familiar platonistic framework – remember that the claim of the constructivist is that numbers are mind-dependent constructions; our framework is not as fixed as all that. But this note aside, we may press the analogy further to respond to such a challenge. Let us say that I tire of my task of cataloguing the language, or else that it is so vast in linguistic variety and vocabulary that I cannot possibly hope to catalogue it in the course of my lifetime, and so instead I leave instructions for anyone who wishes to carry on. I want to preserve the original intent, that is the faithful representation in a dedicated and corresponding alphabet, so among my instructions I include certain rules for the creation of further letters of the alphabet, such that my rules constrain how the letters may be formed, but do not constrain how many are constructed; I may say that they must all contain a certain number of strokes, for example, or else that the shapes that appear in them appear in certain complicated and recurring patterns. And my rules are so specific that those who pick up my endeavours have no choice but to create letters of exact shape in a certain order when they find a need for them, even though I have not done so, for I do not know how many will be needed, and of course I do not wish the alphabet to have redundant letters. Now, even though there are rules for the addition of letters, is it now true to say that the alphabet has a limitless number of letters in it? No – it has precisely the number required; precisely the letters that have been constructed as needed. If I do nothing more than I have already done except to make explicit my rules for continuing, then the alphabet has twenty letters in it. If, later, someone finds a need for two more, and constructs them in accordance with my rules, there are twenty-two letters in the alphabet.

This example may seem fanciful, but a little reflection reveals that it is not so outlandish a suggestion. Japanese, for example, has three alphabets, one of which is a pictorial alphabet, which continues to expand, as new letters are required for previously non-existent words (such as those corresponding to new inventions or advances and discoveries in one of many diverse fields). Presumably there are *some* rules for the creation of new letters in such an alphabet – they must not be identical with pre-existing letters, and they must bear some similarity to letters for similar words, etc.

Let me take care to identify the intended analogy here. The suggestion is not, of course, that numbers are simply an alphabet, but rather that, as in the example of the new alphabet above, they are mental constructions that must be constructed in order to exist. Indeed this seems to me to be an intuitive requirement of the constructivist account – it seems a little peculiar, as an ontology, to allow things that are only potentially so, to be so.

There is one other pertinent analogy to be drawn from the example at this point. It may be objected that numbers are different in this sense – that we may ask, of a string of numerals, whether it marks a surveyable number; and we may do so prior to any attempt to 'mentally construct', or survey the inscription. But this is surely true also in the case of letters for the expandable alphabet – I may make a few random marks on a piece of paper and ask if it marks a letter that is in the completed alphabet. On the one hand, of course, there is no way to answer the question unless the letter (or number) *is* constructed, (in the relevant sense)[2], or at least unless an attempt is made. Moreover, such a question presupposes a determinate set of constructions (letters or numbers), taken, as it were, over all time. But precisely the distinguishing feature of such an account is that the size of the set can only be judged at a given time, and is determined even then by contingent events.

To recap, the claim of fanatical finitism is this: the numbers (and statements, and proofs) that we should legitimately accept as mathematical entities are only those which *have been* surveyably constructed. The totality of (legitimate) numbers is always determinate, that is, it has a precise membership, but the size of that totality

[2] We must be careful here to avoid a further complication regarding the notion of 'surveyability'; it is no complaint to object that surveyability does not arise in the example of the alphabet. Certainly, in this case, there is no issue of surveyability, at least not one that is relevant to the example. The analogy is drawn instead between 'required by the language' for the letters and 'surveyable' in the case of numbers. The language exists, and is not infinite in scope; hence the scope of the alphabet is limited.

varies with time, such that the members of the totality at time *t* comprise precisely those numbers which have been surveyably constructed by time *t*. The largest number at time *t* is simply the largest number to have been surveyably constructed by time *t*.

So how does fanatical finitism offer answers to the three broad questions I asked at the outset of this section? Firstly, to the question 'What is the largest number?', fanatical finitism will I suspect provide a standardly epistemicist response; we do not know what the largest number is, since there is no (practical) way to canvass the largest number that has been surveyed.[3] Nonetheless, there will be a fact of the matter.

To the second question, the familiar objection to strict finitism, which runs 'Why can't we simply add one to the largest number?'[4], we are in a position to give a fresh answer, which seems to have at least intuitive merit – the answer is of course, *you can*, although you haven't. One can (or rather, may well be able to – at the upper bound of practical possibility, this is still a contingent matter) add one to the largest number, but only at time *t'*, at which time the totality will include the new number, and the newer number will itself be the largest number.

Thirdly, with respect to the final question, which raises the concern that different people have differing abilities to survey, we may answer that the limit is indeed the same for all, since what matters on this account is the optimum actual ability to survey. Notice that this observation allows us to provide a broadly epistemicist response to question i), without encountering the problematic idea that different abilities will render it redundant.

*Problems for Fanatical Finitism*

I can already foresee a number of potential objections to fanatical finitism, so let me now say what I can in order to ameliorate the problems that might arise as much as possible.

[3] There is nothing to say that this is in principle impossible, however; presumably, there could be a way to do so.

[4] Of course, the objection is misconceived when raised against strict finitism, but that does not prevent its common employment as we have seen – the difference is that the strict finitist will in response retreat to the still controversial refuge of a vague totality, whereas the fanatical finitist does not have to.

The first of these arises from an apparent tension between the notion of surveyability, to which fanatical finitism is still firmly committed, and the apparent rejection of vague totalities. To begin with, fanatical finitism must be committed to the surveyability criteria – it is not enough to say that they have simply been constructed; this leaves open the logical possibility that God, or some superior form of life, might have already constructed them. So it must be committed to the idea that numbers must be surveyably constructed in order to count as admissible. But, as we have seen, the notion of surveyability seems intractably vague, whereas the motivation behind fanatical finitism is to avoid the problems of vagueness in the first place. So how can it do so, while retaining the surveyability criteria?

The answer lies in distinguishing between sources of vagueness. I have already acknowledged that on a traditional account, there are two sources of vagueness – one arising from the apparent fact that adding one to a number cannot affect the transition from surveyable to non-surveyable, (a vagueness which fanatical finitism is attempting to eradicate) and the other arising from the varying capacity of human beings as surveyors (which it may perfectly well allow for). The important feature of fanatical finitism in this instance is that it establishes a precise limit on the admissible numbers at any given time – a determinate size for the totality of numbers. The vagueness inherent in surveyability in the sense implied by the objection here only arises if we ask 'can so-and-so survey number such-and-such?', where the answer might well be indeterminate – the number might lie in a vague range for the surveyor. But the totality of numbers is not vague, and nor is the fact about any given number as to whether it is admissible or not – both will depend purely on a contingent but perfectly determinate state-of-affairs. Hence the totality of numbers that have been surveyed will not be vague.

Secondly, it might be objected that the theory is inflexible with regards to future practice. There are two ways that I can see a potential objector might formulate this worry. On the one hand, they might object that the size of the totality of numbers is inconstant – the totality may well be much bigger at future times. Moreover, the largest number today need not be the largest number tomorrow. This way of forming the objection is not a serious cause for concern, since it doesn't really amount to an objection at all, other than at an instinctive level; the fanatical finitist may simply agree that this is a feature of fanatical finitism – the largest number *does* vary over time. But note that this is perfectly reasonable within a constructivist framework: indeed, we might expect such a relationship. The set of numbers at time *t* will depend upon what has been (relevantly) surveyably constructed *by time t.*

Alternatively, the objection might be put in terms of truth; statements which are true or false at time t will not necessarily remain so at some future time – and usually, we want mathematics to be immune to such revision. Otherwise, the laws of mathematics may become temporally-dependent: any statement about, say, all the numbers may not be true when larger numbers have been added. But isn't this in fact to be expected also, on a constructivist account? The fact that it is not what we usually expect of mathematical truths is no objection in itself, and it seems that mathematical theories or statements are held to be true, and then revised in light of further work (and in this sense, construction). Mathematics makes fresh discoveries in ways not entirely distinct from those in the empirical sciences. Nor is it the case that all mathematical laws are subject to revision in this sense: since some of the laws govern what may be constructed, it seems that *these* laws at least will be immutable, similarly for any laws derived from them. For any further statements, if they are provable at time t, they will be true at time t, but not necessarily true at some later time, if for example a counter-example is constructed. This may seem a bitter pill to swallow, perhaps – since it allows that some statements will be true, and then cease to be true. When scientific discoveries are made, the earlier claims are (and always were) false. But to the extent that the new scientific postulates are considered to be true (and indeed that anything we discover may be true), this does not seem too far removed from the practice suggested by fanatical finitism.

Furthermore, the idea that the 'expandable' totality of numbers may have consequences for the truth of statements about numbers is not a worry peculiar to fanatical finitism. Strict finitism in general, even one asserting a vague totality of numbers, should be reluctant to deny that the totality can grow in size as our ability to survey grows (both in terms of intellectual ability to achieve intuitive grasp of a number and our continuing development and understanding of new notations).

I suppose that one might also raise a problem here about guaranteeing that we are all thinking about the *same* mind-dependent abstract objects. Assuredly, this is a problem for anti-realist theories in general, and so is hardly worthy of special attention here, except that particular to fanatical finitism seems to be the possibility that a number exists even though one cannot personally survey it. Do numbers then exist for all once *someone* has constructed them? This seems very odd if one is intrinsically unable to construct a number oneself, particularly as the number is mind-dependent in the first place. There is also a sense in which this sort of assertion seems to grant the construction mind-independence. We may of course always say of a particular string or arrangement of numerals (or whatever is notationally relevant) that it marks a genuine number *because* someone has constructed it (as long as it is true that someone has, of course – I do not mean here that mere inscription of an arrangement of numerals will suffice), but this does not seem to properly address the difficulty.

What I think is the correct response here is to recall that there are still rules governing the construction of numbers. Individually, we use the same shared rules to construct numbers as mind-dependent entities, and hence if we try to reproduce a number constructed by another, the rules should ensure that we are all referring to the same constructions. The number is not independent of the mind in which it is constructed, but simply once it has been constructed its actual constructibility is guaranteed. Other people are free to construct the same number, or even 'new' numbers, as long as the construction is consistent with the rules; as long as we are attentive, we will be constructing the same numbers.

Finally, a major objection, I suspect, will be concerning the possibility of 'gaps' in the number line. Since there is nothing in the notion of surveyability that requires, for a number to be surveyably constructed, that all the numbers below it are surveyably constructed, it remains both possible and likely that some 'numbers' remain *actually* unconstructed even though there are larger numbers still which have actually been constructed. However, it is not immediately obvious why such gaps will be problematic; the objection that something is missing from the classical picture is not enough to cause a problem, since that will be true of *any* finitist account. Unless a demonstrable issue arises, the fanatical finitist may simply acquiesce that there are indeed 'gaps' below the largest number.[5]

5 After all, there are only 'gaps' by comparison with the traditional (classical platonist) number line. And it is not clear that even this is a useful comparison for the finitist.

Tentatively, then, I conclude this section with the claim that fanatical finitism is an interesting and promising alternative for the strict finitist. A good deal more development and analysis is required before the theory may be considered robust, but at least in light of the early (and perhaps only the obvious) objections, I hope to have established its potential. If indeed it can be supported and defended against further criticism, it would offer an elegant solution to the problems of vagueness for the strict finitist.

## *The role of Fanatical Finitism*

Fanatical Finitism, then, is interesting for a number of reasons. Firstly, if it can be shown to be robust, it provides a neat answer to the challenge that (strict) finitism[6] is committed to vague totalities, and hence incoherent; and one that avoids commitment to vague totalities altogether. Moreover, if its coherence can be established, then to the extent that the strict finitist is entitled to claim that commitment to constructivism should not stop at intuitionism and should proceed to strict finitism, so too, it seems, is the fanatical finitist entitled to claim that that commitment to constructivism should lead inexorably to fanatical finitism.

On the other hand, Fanatical Finitism is interesting because, if it can be shown to be *in*coherent, the consequences for constructivism in general are potentially serious. Crispin Wright, in his article entitled 'Strict Finitism', suggests that there is a generally accepted Modus Tollens in criticisms of the constructivist (and finitist) tradition, to the extent:

"arguments essentially analogous to those which the Mathematical Intuitionists . . . use to support their revisions of classical logic and mathematics lead to a yet more radical *strict finitist* outlook; this outlook, however, is incapable of issuing in a coherent philosophy of mathematics; therefore there must be something

[6] Fanatical Finitism is intended as a (still more vigorous) form of strict finitism, and I shall take it that many of the definitions established by strict finitism may be employed by the fanatical finitist also – both are interested in the notion of surveyability, and both will employ it to repudiate an infinite totality of numbers.

amiss with the arguments which lead to it, and by analogy, with the original intuitionistic arguments also."[7]

In essence, then, Wright's proposed Modus Tollens is this:

(1) If the arguments for Intuitionism are compelling, then essentially similar arguments for Strict Finitism are compelling.
(2) Strict Finitism is incoherent.
(3) As such, the arguments for Strict Finitism cannot be compelling.
(4) Therefore, the arguments for Intuitionism cannot be compelling.

Wright also suggests that the intuitionist's response is usually to try to reject the major (the conditional) premise in the argument, but convincingly demonstrates that the major premise is sound. Instead, he claims that the strict finitist is entitled to reject the minor premise[8], and hence neither strict finitism nor intuitionism is defeated. But as Wright further implies, strict finitism is of great interest not only for its own sake, but also for this inextricable connection with intuitionism.

Similarly, then, I suggest that, even if incoherent, Fanatical finitism may still be of interest in its relation to other constructivist formulations. I suggest there is a similar link between strict and fanatical finitism, such that if fanatical finitism is demonstrably incoherent, we have a further Modus Tollens, to the extent:

(1*) If the arguments for Fanatical Finitism are compelling, then essentially similar arguments for Strict Finitism are compelling.
(2*) Fanatical Finitism is incoherent.
(3*) As such, the arguments for Fanatical Finitism cannot be compelling.
(4*) Therefore, the arguments for Strict Finitism cannot be compelling.

[7] Wright, 'Strict Finitism', pp.203-4
[8] I shall not recall here Wright's defence of the major premise, as, although I find it convincing, its success or failure will not affect what is to follow over much – and it is sufficiently removed from the present debate to be an unnecessary digression within this study. With regard to the minor premise, as we have seen elsewhere, Wright's defence of Strict finitism allows that the strict finitist must be committed to vague totalities, but suggests that such a commitment does not entail incoherence nor call for the rejection of the thesis any more than the presence of vague predicates in natural language call for the rejection of the language.

Of course, the Modus Tollens proposed by Wright is still in force, and so the incoherence of arguments for Fanatical Finitism would ultimately entail the incoherence of arguments for Intuitionism.

I have so far presented Fanatical Finitism as coherently as possible, and so my principal aim here is, I trust, apparent: to deny the truth of the minor premise in the second Modus Tollens, just as Wright does on behalf of strict finitism in the first, as he presents it. Although I do not wish to attempt to properly establish here the truth of the major premise[9] – since the primary concern of this research is regarding the coherence of (various forms of) strict finitism – I would however like to close with a few remarks in this direction.

Let me then explain why it is that I maintain that someone committed to the idea of constructivism should find it hard to resist continuing further down the road, and, just as it is claimed that strict finitism is the 'natural outcome' of intuitionist reasoning[10], so it may prove that fanatical finitism is the natural outcome of strict finitary – indeed general constructivist – reasoning. Consider the constructivist constraint. It is natural, I suppose, on an anti-realist account to take it that meaning is conferred, and not intrinsic; that is to say, in order for a proposition to be meaningful it must be meaningful *to us*.

Intuitionism, we may recall, places a limit based on what it is in principle possible for us to construct; and the strict finitist places a yet stricter limit based on what it is in practice possible to construct. But constructions must surely be understood as mind-dependent abstract objects, and hence the real requirement here is that for a number, proof, or mathematical statement to have meaning, it must be constructible in the mind. The intuitionist is criticised for not taking this criterion seriously enough – for a proposition involving or asserting a mathematical object, proof, or statement to have meaningful content it must have meaning to us; and the notion of in principle possibility does not guarantee this properly. The proposition need not have meaning to us, just to something (even very remotely) like us, whose

[9] This is of course a crucial question with respect to constructivism in general, and one that I suggest is worth pursuing; just not one that is strictly relevant to the topic in hand.

[10] Wright has made such a claim, in fact: "strict finitism remains the natural outcome of the anti-realism which Dummett has propounded by way of support for the intuitionist philosophy of mathematics". Wright, 'Strict Finitism', p.269

only real semblance to us is that it must be in some way constrained to act in time, and space (although not necessarily our time and space; its space-time may well be limitless, even if ours isn't). In any event, the meaning of statements which are not within our in-practice capabilities to properly construct will not be accessible to us, and indeed will only be accessible to some equally *in principle* possible beings.

Hence the strict finitist asserts that we must be concerned with in practice possibility. But what is it for a mental construction to be *in practice possible*? If the objection is that mere in principle possibility is not sufficient to guarantee meaning to potential constructions, then why should the objector stop at the distinction between 'in principle' and 'in practice'? Why not object to the modal qualification as well? The thought here is this – what is it for an (in practice) possible construction to have meaning, unless the construction is actual as well? Can a potential construction really be said to have meaning, independent of its *actual construction*?

There are perhaps two issues here, which first became apparent in my earlier discussion about anti-realism in ontology vs. anti-realism in truth value. One might be tempted to separate issues of ontology from issues of meaning, or truth. If our constructivism is of the former kind, which is to say that the only numbers (for example) that *exist* are those which we can construct, then an interpretation of 'can construct' here that suggests there might be numbers that exist but which we have not constructed seems absurd; any suggestion that the 'can' is potential rather than actual – indeed, that it means 'could' rather than 'have' – seems to undermine the whole basis for the ontology.

Indeed, it seems to me that fanatical finitism is the intuitive option following constructivist thinking – there is after all something rather odd about admitting *potential* constructions[11] into an anti-realist, constructivist ontology. Numbers simply are mind-dependent constructions. Why should we be obliged to admit any potential numbers that are not (yet) constructed?

On the other hand, one might try to take the anti-realist in truth-value route, and attempt to avoid this troubling conclusion. Instead, we might try to assert that numbers (proofs, statements) are guaranteed meaning in virtue of the fact that we

[11] 'potential' in either sense here; potential as in 'in principle', or potential as in 'possible but not actual'.

*could* construct them. A statement is guaranteed a truth-value as long as it is effectively decidable.

But here, the strict finitist complaint against intuitionism looks less well-founded in the first place. If we are allowed potentiality in the weaker sense of 'possible but not actual', then why not in the stronger sense of 'in principle'? We no longer seem to have quite the same grounds for making the practice/principle distinction in the first place. Moreover, we saw in chapter two that it is implausible to suppose that truth-value and ontology come apart in this way for the constructivist.

It is my conjecture, therefore, that the first of these suggestions is the only feasible route for the finitist, and one that appears to lead even further down the constructivist path towards fanatical finitism. It becomes incumbent on the 'traditional' strict finitist, and moreover on the intuitionist, to identify a point at which the progressively harsher constructivist requirements may be sensibly halted; or else their future looks as bright or dark as the future of fanatical finitism itself.

# CONCLUSION

At the outset of this research, my aim was to provide a robust defence, and proper account of the theory of strict finitism. I have divided the final content of my thesis into three parts, to reflect the differing orientation of each. In part one, chapters one through four, I offered an account of the theory of strict finitism. Drawing upon the existing literature, in the first chapter I distinguished between strict finitism and a more liberal finitism, called 'classical' finitism by Tiles, which embraces certainly the mathematical intuitionists' school of thought. The distinction is important, since the only qualification for entry into the broad school of 'finitism', is I assert, a rejection of the traditionally (platonistically) conceived infinite – that is, the *actual* infinite. Strict finitism then goes further than the intuitionists in rejecting *potential* infinity also.

I went on to establish the theory of strict finitism as a thoroughgoing anti-realist constructivism; an anti-realism with respect to both the objects of mathematics, and the truth values of mathematical statements. Furthermore, I made the claim in chapter two that it is impossible for a strict finitist (along constructivist grounds at least) to be an anti-realist with respect to either ontology or truth values, whilst holding a realist position with regard to the other. Indeed, I remain sceptical of any attempt to do so. In chapter two I also introduced the notion of surveyability, and distinguished between the 'weak' and 'strong' strict finitist claims. The weak claim need not be revisionary, since it is a claim only about what we can deal with, and not about what numbers there *are*; as such, I suggest, a strict finitist as I have characterised him will be interested only in the strong claim, which carries with it an ontological commitment. The weak claim approaches that advanced by Wittgenstein, who is sometimes considered a strict finitist for this reason; in my view, although Wittgenstein's characterisation of the notion of surveyability is strict finitist in outlook (and indeed instructive), his refusal to carry this idea through to the ontological level means that he should not be properly characterised as a strict finitist in the full sense.

In chapter three, I took Wittgenstein's criteria for the surveyability of proofs, and showed how they could be usefully adapted to provide an account of surveyability for numbers. In broad terms, the only numbers (as traditionally understood) that are admissible on a strict finitist's view are those which are surveyable, which is to say i) that they are reproducible, in the sense that they may be written down, ii) that they are checkable, in that one may perform a check that each inscription in the representation is correctly reproduced (that one has written a 6, and not an E, for example) and iii) that they are intuitively graspable, in that their magnitude may (in actuality, and not potentiality) be grasped and understood by the human mind. In an attempt to further define this notion of intuitively graspable, I suggest that one has an intuitive grasp of magnitude of a particular number if, when presented with *any* other surveyable number, one can readily rank the pair in order of size.

To conclude the first part, in chapter four, I looked at the problem of differing notation and its effect on the surveyability of numbers. I dismissed the idea that notation is unimportant in demarcating the numbers which are surveyable from traditionally accepted 'numbers' which are not, but I was equally unconvinced by the notion that notation (or perhaps more correctly, the *complexity* of a representation) should be sufficient grounds for such demarcation. Instead, I suggested a hybrid model, in which complexity and magnitude take equal weighting; with the net effect that the three stage criteria outlined above be amended such that the third criterion reads: iii) that an intuitive grasp of magnitude can be conveyed *in some notation* to the surveyor.

From these four chapters, I contend that the model of strict finitism I offer is the best model to represent the theory in terms of the way it has been presented or hinted at in the literature to date. The question of notation is not one that has been seriously addressed, and the role of numbers, rather than proofs or statements of mathematical truths, has received comparatively little attention in the existing work on surveyability; I hope that the first part of this research goes someway towards repairing these deficits.

In the second part of this thesis I considered the principal objections raised by contemporary philosophers against the theory of strict finitism. The most serious of these is a charge of inconsistency levelled at the theory by Michael Dummett, although I also considered three separate charges of inconsistency raised by Mark Addis. Perhaps the most obscure of these is what I described in chapter five as the surveyability dilemma, and I present there a thorough analysis, and broad rejection, of the alleged dilemma facing the strict finitist, as it is proposed by Addis. Addis contends that the dilemma arises when the strict finitist attempts to answer the question 'Can surveyor X survey a proof (or number) even if he is not actually surveying it at that time?'. If the strict finitist admits there is a determinate answer, Addis claims he is committed to counterfactual realism, whereas if he argues that there is no determinate answer, Addis claims that the 'range of applicability of the concept of surveyability' is too restricted to be of any use. I argued that counterfactual realism is not the problem for strict finitism that Addis thinks it is, and furthermore that the strict finitist is at liberty to answer that there is no determinate answer *only in some cases*, dependent on the particular proof (or number) concerned.

The remaining objections to strict finitism that I considered in part two centre around the observation that strict finitism is committed to vague totalities, and in chapter six I gave a general introduction to the problem, containing a first look at the Dummettian analysis of the sorites problem, as well as some preliminary remarks on two standard ways of dealing with vagueness: epistemicism and the degree theory of truth. I also considered the idea that the strict finitist may be able to reject sorites paradoxes on the ground that they themselves are not surveyable, but found Crispin Wright's argument against this possibility convincing. I also made an important point regarding Wright's conclusion, however – a point missed by Addis in his consideration of the argument. Addis claims that Wright's argument is proof of a further inconsistency for the strict finitist; Wright in fact concludes no such thing. I warned against the mistake made by Addis, and endorse Wright's assertion that vagueness is only a problem for strict finitism inasmuch as it is a problem for ordinary language, which also employs vague predicates.

In chapter seven, I continued with a discussion of Dummett's analysis, and reject the claim that strict finitism is intrinsically incoherent in an analogous way to the way in which Dummett claims to have established incoherence in the case of colour predicates. I reject the analogy on two counts – firstly, that strict finitary predicates need not be 'observational' predicates in Dummett's sense, and secondly that the source of vagueness in strict finitary predicates is not the non-transitivity of non-discriminable difference. However, I also acknowledged that Dummett might intend his case to be more general than his analogy implies, and so in chapter eight I moved on to a consideration of weakly finite and weakly infinite totalities.

In that chapter we saw that not only Dummett, but Addis also, claim that weakly finite and weakly infinite totalities are intrinsically inconsistent. Allowing for the sake of argument that the strict finitist needs to be committed to such totalities as Dummett defines them, I rejected both charges of incoherence. Addis's mistake involves a quite crude confusion of the notion of weakly finite sets with that of finite sets. Dummett's mistake is subtler, and his charge of inconsistency certainly more challenging to the strict finitist, but ultimately I suggested that he fails to respect his own definition of weakly infinite, when he assumes that a totality must *either* have a last member, *or* be closed under the successor operation.

Dummett's point is nonetheless compelling given the framework of the prevailing bivalent logic, and I argued that this might give us good reason to abandon the logic, rather than the strict finitist position. After all, as Dummett himself admits, the contradiction is equally problematic for such established practices as colour-ascription. As a result, my task in the final section was to explore alternatives for the strict finitist program, largely in response to the challenge presented by Dummett. The task I undertook in part three was two-fold; firstly, to examine alternative logics for their capacity to deal with the problem, and secondly, to look at alternatives to accepting Dummett's definition of the kind of totalities that strict finitism needs to be committed to. The first of these aims was accomplished in chapters nine and ten.

In chapter nine, I consider intuitionist logic as an alternative, but conclude there that it is insufficient to meet the challenge; Dummett's point can simply be reconstructed in purely intuitionistic terms to present a problem of equal magnitude for the strict finitist. Consequently, I explored a version of trivalent logic as a further alternative, and assert that the introduction of a third truth-value, 'Indefinite', will allow us to avoid the ruinous conclusion that Dummett reaches regarding totalities that are weakly finite and weakly infinite in kind.

The principal objection to adopting a trivalent semantics as a solution to the problem of vagueness, despite its promise when dealing with cases like Dummett's, remains the problem of higher-order vagueness. I acknowledge this at the end of chapter nine, and so chapter ten is devoted to addressing the problem. I suggested there that Michael Tye has an attractive method for dealing with higher-order vagueness, and one which looks to render the objection impotent. If this is the case, there remains no real barrier to the strict finitist who wants to proceed with

Dummett's definition of weakly finite and weakly infinite totalities, while at the same time deflecting the charge of inconsistency.

I concluded chapter ten with a brief look at the ultimately unpromising alternative of a surveyably-finitely-valued logic, before turning my attention in the final chapter to more general alternatives for the strict finitist program.

In chapter eleven, then, I considered the twin definitions Dummett offers of 'weakly finite' and 'weakly infinite', and made the assertion that the strict finitist is entitled to reject one or both of the definitions. With respect to the 'weakly finite', I argued that the strict finitist does not need to acknowledge that there *are* cases, for any of the totalities to which she is committed, of genuine candidates for membership which are clearly not members of the totality. In the case of numbers, for example, the strict finitist need not admit that there are 'numbers' (even proto-numbers, or 'putative' numbers) which are not surveyable – hence the description of the totality of numbers as being bounded-above is incorrect for the strict finitist.

Furthermore, I outlined a particular development of the strict finitist's claim which is entirely consistent with a rejection of the definition that Dummett provides of 'weakly infinite'. Fanatical Finitism, as I referred to it, suggests that what is of paramount importance to establishing what numbers there are is *the numbers which actually have been constructed.* To that extent, the totality of numbers is not without a last member – since there will be a determinate last member at any given time – and so is not weakly infinite. I consider some preliminary objections, but can find no reason why fanatical finitism should be dismissed as a serious (and broadly strict finitary) theory.

The options for the strict finitist, and the potential for further research, look very promising as a result. I hope to have provided good reasons to be suspicious of the various rejections of strict finitism in the existing literature, and further reasons still to consider the theory alive and well as a foundation for mathematics. There are several projects that present themselves as themes for further research. The first of these is a proper development of a strict finitary arithmetic in line with the three-valued semantic supported in chapters nine and ten. Then there are the perhaps distinct tasks of developing on the one hand a strict finitism that is reluctant to acknowledge that it is bounded-above, and suggests that the domain of discourse for numbers is the domain of discourse for surveyable numbers, and on the other hand a strict finitism of the kind I have described as fanatical finitism. This last task is, as I have suggested, an important one, and not just for the strict finitist alone. For if a full defence of the position proves impossible, the ramifications for constructivism as a whole are potentially serious.

In addition, in the context of a wider research into the prospects of a grand rejection of the infinite that I mentioned in the introduction to this work, I conclude that strict finitism is a promising finite alternative to acknowledging the abstract infinite in mathematics. And if the scientific theories regarding the finiteness of the universe (some of which are still admittedly in their infancy) are vindicated, to the extent that there are no genuine instantiations of the infinite apart from the abstract existence attributed to it in pure mathematics, what real remaining justification can we have - given that there exists a consistent, (if revisionary), *finite* alternative foundation for mathematics - for retaining it?

BIBLIOGRAPHY


Addis, Mark, 'Surveyability and the "Sorites" paradox', 1995, *Philosophia Mathematica* 3(2), pp.157-65

Bernays, Paul, 'On Platonism in Mathematics', 1935, reprinted in *Philosophy of Mathematics*, Benacerraf & Putnam, CUP 1983 (2nd Ed.)

Benacerraf, Paul, 'Mathematical Truth', 1973, *Journal of Philosophy* 70, pp.661-80

Benacerraf, Paul, 'Tasks, Super-Tasks, and the Modern Eleatics', 1962, Journal of Philosophy, 59, pp. 765-84

Benacerraf, Paul, & Putnam, Hilary, *Philosophy of Mathematics*, CUP 1983 (2nd Ed.)

Bridges, Douglas, 'Constructive Mathematics', 1997 *Stanford Online Encyclopedia*, Edward N. Zalta (ed.)

Brouwer, L. E. J., 'Consciousness, philosophy, and mathematics', 1948, reprinted in *Philosophy of Mathematics*, Benacerraf & Putnam, CUP 1983 (2nd Ed.)

Dummett, Michael, *Truth and Other Enigmas*, Duckworth 1978

Dummett, Michael, 'Wang's Paradox', 1975, *Synthese* 30, pp.301-24

Dummett, Michael, 'Platonism', 1967, in *Truth and Other Enigmas*, Duckworth 1978

Dummett, Michael, 'Realism', 1963, in *Truth and Other Enigmas*, Duckworth 1978

Dummett, Michael, 'Wittgenstein's Philosophy of Mathematics', 1959, in *Truth and Other Enigmas*, Duckworth 1978

Forbes, Graeme, 'Thisness and Vagueness', 1983, *Synthese* 54, pp. 235-59

Gandy, Robin, 'Limitations to Mathematical Knowledge', 1980, in *Logic colloquium '80*, van Dalen, Lascar & Smiley eds. pp.129-46

Goguen, J.A., 'The Logic of Inexact Concepts', 1969, *Synthese* 19, pp. 325-73

Hale, Bob, & Wright, Crispin, eds., *A Companion to the Philosophy of Language*, Blackwell 1997

Hale, Bob, & Wright, Crispin, 'Benacerraf's Dilemma Revisited', 2002, *European Journal of Philosophy*, vol. 10 No. 1, pp. 103-129

Keefe, Rosanna, & Smith, Peter, *Vagueness: a reader*, Cambridge MIT 1999

Kielkopf, Charles, ''Surveyability' should not be formalised', 1995, *Philosophia Mathematica* 3(3), pp.175-8

Kleene, S.C., *Introduction to Metamathematics*, Amsterdam : North-Holland Pub. Co., 1952

Malinowski, Grzegorz, *Many-Valued Logics*, Clarendon Press, Oxford 1993

Moore, Adrian, ed., *Infinity*, Dartmouth 1993

Moschovakis, Joan, 'Intuitionistic Logic', 1999, *Stanford Online Encyclopedia*, Edward N. Zalta (ed.)

Sainsbury, Mark, & Williamson, Timothy, 'Sorites', 1997, in Hale and Wright, eds., *A Companion to the Philosophy of Language*, pp.458-84

Shapiro, Stewart, *Thinking about Mathematics*, OUP 2000

Thomson, J.F., 'Tasks and Supertasks', *Analysis*, 15, pp. 1-13

Tiles, Mary, *The Philosophy of Set Theory*, Oxford: Blackwell 1989

Tye, Michael, 'Vague Objects', *Mind*, Vol 99, No. 396, pp. 535-57

van Bendegem, Jean Paul, 'Finitsim in geometry' 2002, *Stanford Online Encyclopedia*, Edward N. Zalta (ed.)

Van-Bendegem, Jean-Paul, 'Strict finitism as a viable alternative in the foundations of mathematics', 1994, *Logique et Analyse* 37, pp.23-40

van Dantzig, D, 'Is $10^{10^{10}}$ a finite number?', 1955, reprinted in *Infinity*, A.W. Moore, 1993

Welti, Ernest, 'The philosophy of Strict Finitism', 1987, *Theoria: Revista de Teoria, Historia y Fundamentos de la Ciencia* 2, pp.575-82

Wittgenstein, Ludvig, *Remarks on the Foundations of Mathematics*, edited by von Wright, Rhees and Anscombe, Blackwell 1978

Wright, Crispin, 'Strict Finitism', 1982, reprinted in *Infinity*, A.W. Moore, 1993

Wright, Crispin, *Wittgenstein on the Foundations of Mathematics*, Duckworth 1980

Yesinin-Volpin, Alexander, 'The Ultra-intuitionistic Criticism and the antitraditional program for foundations of mathematics', in *Intuitionism and Proof theory*, Kino, Myhill & Vesley (eds.)1970