A DEFENCE OF MATHEMATICAL PLURALISM

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11 November 2004

Abstract

We approach the philosophy of mathematics via a discussion of the differences between classical mathematics and constructive mathematics, arguing that each is a valid activity within its own context.

1 Introduction

The philosophy of mathematics has been the subject of such intense scrutiny over many years that it now needs whole books to outline the variety of positions that can be taken towards its foundations. In spite of this there is no agreement even about whether one should adopt a broadly realist (Platonic) or anti-realist position towards its basic entities. Gödel's theorem is accepted as mathematically correct but its interpretation – whether it establishes that proof and truth are distinct – still divides the philosophical community.

In this paper we by-pass these problems, and start by discussing two *frameworks* within which mathematics may be developed, namely classical mathematics and constructive mathematics. These frameworks are distinguished according to what they permit as acceptable methods for proving theorems. They assign different meanings to terms such as 'number', 'set' and 'exists', and this fact controls what is acceptable in the proofs of theorems.

Classical mathematics is presently the mainstream of the subject. Some people define it as what may be proved within the context of ZFC set theory. For the majority of mathematicians, who have never studied formal set theory, it allows the free use of the law of induction, naive set theory, the axiom of choice and the law of the excluded middle. We impose a further restriction on the use of the term classical mathematics in order to avoid having to address the current debate about the acceptability of computer assisted proofs, to which a future article will be devoted. Namely we will only regard a theorem as a part of classical mathematics if its proof is reviewable in its entirety by a mathematician using only pen and paper. This has the effect of embedding classical mathematics in human culture: the scope of the term depends on the time chosen, and is not determined by some notion of what is in principle capable of being proved by an ideal mathematician with an indefinitely prolonged life.

By constructive mathematics we will mean the subject as formulated by Bishop [1967]. This is characterized by the avoidance of the law of the excluded middle and the insistence that one should only refer to a mathematical entity as existing if one has a finite algorithm for constructing it. In logical terms it is a subset of classical mathematics with some rather strange conventions and definitions, but from another point of view it is more general than classical mathematics; see Richman [1990] and Billinge [2003]. It is distinct from Brouwer's intuitionistic mathematics, which is not compatible with classical mathematics. The detailed development of constructive mathematics is substantially different from that of classical mathematics. Familiar theorems are often rewritten in unconventional forms and have different proofs, and the entities that it studies are also different, particularly within set theory.

The difference between the two frameworks arises from the fact that they assign different *meanings* to the quantifier $\exists$. A classical mathematician intends this to refer to Platonic existence, whereas a constructivist means that he could write down an algorithm for constructing the entity in question. The need to avoid the law of the excluded middle in constructive mathematics follows in an obvious manner from the meaning assigned to $\exists$. Hellman [1989] and others have proposed that one should use two different symbols, such as $\exists_{int}$ and $\exists_{cla}$, for the two concepts, and distinguish in a similar manner between later notions which might be defined in terms of one or other of these. While this would be possible, it would probably cause more confusion than keeping the two frameworks entirely separate, and acknowledging that the two paths gradually diverge.

Although we start with a substantial discussion of associated philosophical issues, one of the main theses of this paper is that constructive mathematics is of value *even to those who reject the philosophical assumptions of its founders*. It provides a clear and precise insight into difficulties that numerical analysts and others who are interested in obtaining quantitative information about solutions of equations face on a regular basis. Such matters can be resolved classically, but constructive mathematics provides a more *systematic framework* for doing so. The debate about which is the 'right' way to do mathematics is sterile and counterproductive. Each of the frameworks is valid and has advantages in appropriate circumstances. As we explain below, this pluralistic viewpoint is not our invention, but we believe that some of the arguments that we marshal in favour of it are novel.

Before continuing, we need to address some matters of terminology. We will avoid using the term 'realism', because it means various things to different people – Burgess [2004] describes it as one of the most overused and misused terms in philosophy. Platonism is now often called realism, although it is quite different from scientific realism; the alternative term idealism is now rarely used. Popper [1977] claimed that his World 3 objects are real, because they affect human behaviour and through them other material bodies. Bishop [1967, p.10] also calls his own position straightforward realism, but we will call it constructivism. We call our position mathematical pluralism, and emphasize that it should not be confused with scientific relativism. Science is the study of the external world, while mathematics is one of the ways we systematize our thoughts. It is hardly surprising that our ability to think carefully is helpful when we are trying to understand the world, but our thought processes and the external world are quite different. Philosophical beliefs about the two need not be related.

Our support for mathematical pluralism does not imply that we agree with all of the views put forward under the banner of cultural pluralism. The acceptance of an alternative framework for doing mathematics must be based upon whether it can deliver new insights, even to those who do not initially have any particular interest in it.

2 Existence and Truth

Many of the monographs and papers on the philosophy of mathematics are essentially studies of the nature of mathematical objects and in what sense, if any, they may be said to exist. On the other hand, mathematicians adopt a naive attitude towards the existence of the objects that they study, and seem not to suffer from this. We cannot even summarize the literature on this subject, but choose a few quotations from recent books.

Balaguer [1998] devotes a whole book to the study of various varieties of Platonism and anti-Platonism, but eventually concludes there is no way of separating what he calls Full Blooded Platonism from a version of anti-Platonism called fictionalism.

> It's not just that we *currently* lack a cogent argument that settles the dispute over (the existence of) mathematical objects. It's that we could *never* have such an argument ... Now I am going to motivate the metaphysical conclusion by arguing that the sentence – there exist abstract objects; that is there are objects that exist outside of space-time (or more precisely, that do not exist in space-time) – *does not have any truth condition* ... But this is just to say that we don't know what non-spatiotemporal existence amounts to, or what it might *consist* in, or what it might be *like*. Balaguer [1998, p. 22]

Carnap, quoted below, would have agreed with Balaguer's conclusion fifty years earlier, and said that it was clear from the start, because the existence of theoretical objects is an internal question that makes no sense in an absolute context. While

Carnap's views are frequently dismissed at present, Burgess [2004] has recently suggested that some can be recast in a more acceptable manner.

It is more difficult to extract a single message from Shapiro [2000], which is more a systematic review of the various theories than advocacy of one view. We choose the following, which refers to recent developments in the field.

> As we have seen above – several times – a fundamental problem for realism in ontology is to show how it is possible to refer to, and know things about, mathematical objects if we have no causal contact with such objects ... What is it about the practice of mathematics and science which allows them to proceed with terms that refer to objects with which we have no causal contact? What does this say about mathematical objects? Shapiro [2000, p. 250]

Shapiro also discusses at length the Putnam-Quine arguments that the indispensability of mathematics in science supports mathematical realism. In Shapiro [2000, Ch. 9] he describes Hartry Field's criticism of the argument and the attempt to develop an nominalist version of physics in Field [1980]. The indispensibility argument discussed in this paper is only a small part of this much larger debate. We intend only to ask whether physicists use tools from pure mathematics that the constructivists are not capable of supplying. Our answer is negative.

The first strand of our argument is to separate pure mathematics from applied mathematics. This can be justified on historical grounds. In the year 1600 mathematicians had already developed Euclidean geometry, trigonometry and some aspects of number theory, the Chinese had computed π to (the equivalent of) five decimal places and methods for solving polynomial equation using complex numbers had been developed by Cardan and Viète. It is implausible that the truth of these theories depends upon subsequent developments in physics.

We next emphasize that mathematics is not a single coherent entity: physicists and pure mathematicians think about it in quite different terms[1]; see Davies [2003a] and Davey [2003] for details. From the point of view of physicists QED is one of their most successful and highly confirmed theories. Mathematicians, on the other hand, regard it as a set of recipes that are not linked to any coherent theory as they understand this term. The renormalized series expansions that are obtained in QED are not approximations to any known entities, but most physicists are not particularly concerned about this because they yield numbers that correspond to experiment extraordinarily well. To put it crudely, they assume that series expansions (and other mathematical techniques) are useful until experience proves

[1] Any comments about physicists and mathematicians must be over-simplifications, because there is a continuous range of attitudes on the physics-mathematics spectrum, but it is important not to fall into the trap of assuming that the values of typical mathematicians and of physicists such as R Feynman, P W Anderson and M Berry are similar on the basis of quoting the very few people who straddle the two subjects.

otherwise, and consider that they have understood a subject when this utilitarian approach succeeds. Their very robust attitude towards mathematics explains why Newton, Laplace, Maxwell and other scientists were able to develop very successful mathematically based theories long before the real number system was formalized at the end of the nineteenth century.

Physicists may describe space-time as a continuum, but they know that distances less than the Planck length have no physical meaning, and that the actual structure of space-time may well be quite different from that presupposed in general relativity. Davies [2003b] has showed that non-relativistic quantum mechanics can be recast in a discrete space without measurable changes to any of the conclusions. A feature of his model is that the rotational symmetry of space is an emergent property in the continuum limit, and not a property of the model itself. Wolfram [2002] has argued that much of physics might be recovered from the discrete theory of cellular automata. Whether or not he is right (and few are convinced by his advocacy at present), our current mathematical descriptions of the physical world are not necessarily the final word. We may one day realize that we have relied on continuous mathematical models not because they are right, but because, before the advent of computers, the relevant equations were easier to solve.

We follow Carnap [1950] to a limited extent in arguing that existence, *at least in pure mathematics*, is an internal question.

> Above all, (the acceptance of entities within a linguistic framework) must not be interpreted as referring to an assumption, belief, or assertion of "the reality of the entities". There is no such assertion. An alleged statement of the reality of the entities is a pseudo-statement without cognitive content... The acceptance cannot be judged as being either true or false because it is not an assertion. It can only be judged as being more or less expedient, fruitful, conducive to the aim for which the language is intended. Carnap [1950].

See also Maddy [1997, p. 95] and Shapiro [2000, pp. 124-132]. By choosing to work within a particular mathematical framework, one agrees to refer to the objects of that framework as existing. However, if on another occasion one decides to follow up a theory with a different and inconsistent set of axioms, then one might have to deny the existence of the 'same' objects. There is an analogous situation for legal systems. Although they may have a lot in common, an action that is considered to be a crime in one may not be in another. It does not make sense to ask whether driving on the left-hand side of the road is an offence in itself: the answer depends on the country in which one lives. Similarly it does not make sense to ask whether every bounded set of real numbers 'really does have' a least upper bound; it depends upon whether at the time of asking this one is working in the constructive or the classical framework. They consider two different but quite similar real number systems. Most mathematicians would say that the least upper bound exists because they always work within the framework of classical mathematics, often without even being aware that there is an alternative.

We accept Kreisel's dictum that the important issue is not the existence of mathematical objects, but rather the objectivity of mathematical statements; see Tennant [1997, p. 20]. We define a theorem to be a statement made within a particular mathematical framework together with some proof of that statement. The same statement in a different framework is regarded as a different theorem. A statement with no proof and no explicit counterexample is called a conjecture. When talking about mathematics, as opposed to the philosophy of mathematics, one does not have to discuss truth, epistemology, transcendence etc. A mathematician might *say* that 'a theorem X is true', but this means exactly the same as 'X is a theorem' as defined above, and does not refer to any theory of truth as discussed, for example, by Tennant [1997, p.159]. When mathematicians say *as mathematicians* that they do not know whether Goldbach's conjecture is true, they mean exactly the same as when they say that nobody has yet found a proof of Goldbach's conjecture. If (in 2004) they say that they believe that Goldbach's conjecture is actually true even though no proof exists, they are not discussing the nature of truth, but speculating about what theorems might be proved in the future.

Of course mathematicians often have philosophical beliefs, particularly in relation to Platonism, but when they express these in public they regularly get bogged down in unresolvable disagreements. The Royal Society meeting in October 2004 on 'The Nature of Mathematical Proof' provided ample evidence for this. It is a matter of observation that mathematicians only agree with each other to the extent that they adopt the above notion of truth.

A proof is a chain of statements that make sense in the relevant context. These are rarely formalized: the author invites the reader to agree that each one follows from those before by an application of the relevant rules, whatever they are. There are three outcomes to such a request. The reader agrees, or persuades the author that a certain step is not valid, or requests expansion of the argument at some point. It is expected that a consensus will eventually be reached. The choice of framework (or 'rules of inference') determines whether use of the law of the excluded middle or the axiom of choice is permitted, and this precedes the process of checking particular proofs.

I shall base my defence of constructivism on its mathematical interest. From a philosophical point of view consistency seems to be a more objective criterion than being of interest. However, Gödel's theorems imply in most contexts that one can only use this criterion negatively, that is to eliminate theories. One might use the applicability of a theory to physical science as a way of judging its interest, but many pure mathematicians would not accept this, because they have no interest in physical science. In addition the applicability of a theory may vary with time. It would be worthwhile to investigate how mathematicians judge the interest of a theory, but this would be a sociological rather than a philosophical enterprise, with a variety of quite different answers. Ultimately the editors of journals and their referees decide what is published, and the interest of a paper is one of the important criteria for making these decisions. The fact that it could hardly be otherwise does not imply that they always get it right: Bishop's book received at least one very hostile referee's report according to Nerode et al. [1985, p. 80].

If one looked only at completed theories it would be easy to conclude that mathematics is entirely syntax, and that it has nothing to do with human beings. One sees only axioms, definitions and proofs of theorems. These are almost never written down in a completely formal manner, but the belief is that they could be. However, mathematics is a goal-directed activity, and the goals are set by human beings. The definitions may be adjusted several times while the theory is being constructed, so the appearance of the final product is misleading. The more experienced a mathematician, the greater the extent to which he is able to skim through a proof looking for the ideas, only returning to check the details once he is sure that he understands the overall strategy. Usually this aspect is the main element of conversations between mathematicians. Linear reading of a text is reserved for situations in which one has so little understanding of a new field that nothing else is possible. From the lofty perspective of the experienced mathematician, a theory is defined by its intuitions; get these right and the details can be filled in later. See Atiyah et al. [1994], Thurston [1994] and Rota [1997] for vigorous responses to the proposal by Jaffe and Quinn [1993] that mathematicians should pay more respect to the ordered logical development of the subject, and Stöltzner [2002] for a commentary on this unique debate. The contributors express a wide variety of opinions, but almost all seem more interested in informal discussions between mathematicians than in the production of a systematic body of formal proofs in the style of Bourbaki.

3 Classical Mathematics

Maddy distinguishes between classical mathematics and the philosophical commitment to set-theoretic reductionism in the strong sense. She declares

> I will assume that set theory provides a foundation for mathematics in the modest sense outlined here: for all mathematical objects and structures, there are set-theoretic surrogates and instantiations, and the set theoretic versions of all classical mathematical theorems can be proved from the standard axioms of the theory of sets (ZFC)'. Maddy [1997, p. 34]

In this sense ZFC provides a useful common setting for relating different areas of mathematics.

The reasons for not going beyond this are well-known and rehearsed by Maddy [1997]. They include the following. There are several different models for the natural numbers in terms of set theory, and none seems to be obviously the best. Most people take it for granted that ZFC is consistent, but this is not provable and may (possibly) be false. Most people would agree that the consistency of our ideas about natural numbers is more certain than the consistency of ZFC. Euclidean geometry was created long before ZFC was thought of, and it would be odd to argue that it can only be regarded as a part of classical mathematics because it can be modelled within ZFC.

Maddy's statement does not imply any commitment to set-theoretic realism in the sense of Gödel. He described himself as a Platonist, and considered that sets had an objective existence: his own theorems proved the deficiencies of formal mathematical reasoning rather than the failure of certain statements to have any truth value. For Gödel ZFC was a partial list of properties of an external Platonic reality. He was confident that the continuum hypothesis was either true or false, because it referred to entities that either existed or did not. Such views are now generally regarded as mysticism.

The enormous achievements of classical mathematicians in the twentieth century are well known, and do not need to be listed. The other side of the coin – the lack of a logical justification for that method of developing the subject – are not frequently mentioned. There was a vigorous debate early in the twentieth century over the acceptability of the axiom of choice. Even its inventor, Zermelo, eventually agreed that the most compelling reason to accept it was the fact that without it mathematicians could not prove large numbers of results that they needed (Maddy [1997, p. 56]). They accepted it for pragmatic reasons, hoping that a justification would later appear. Now we know that there can be no proof of the axiom, but many mathematicians seem to regard it as being so obvious that it needs no serious discussion. Cohen, who finally proved that the continuum hypothesis was independent of ZFC, disagreed.

> Historically, mathematics does not seem to enjoy tolerating undecidable propositions. It may elevate such a proposition to the status of an axiom, and through repeated exposure it may become quite widely accepted. This is more or less the case with the Axiom of Choice. I would characterize this tendency quite simply as opportunism. It is of course an impersonal and quite constructive opportunism. Nevertheless, the feeling that mathematics is a worthwhile and relevant activity should not completely erase in our minds an honest appreciation of the problems which beset us. Cohen [1971]

The consequences of accepting the 'truth' of classical mathematics have become clearer as time has passed. Very recently Friedman has discovered a series of straightforward finite theorems in mathematics that can only be proved on the assumption that certain very large cardinals exist. One can take several philosophical attitudes towards this mathematical result, but the author himself wrote that his

> findings raise the specific issue of what constitutes a valid mathematical proof and the general issue of objectivity in mathematics in a down to earth way. (Friedman [1988])

In our language the issue is whether his theorems are so contrary to intuition that they demand a revision of the framework of mathematics. This could go in several directions, but Friedman expects that the use of new axioms in mathematics will steadily increase as time passes. Those who regard large cardinals as outside the framework of acceptable mathematics may be persuaded by his results to adopt a non-realist, or non-bivalent, view of mathematical truth.

Our final example of the consequences of accepting classical mathematics is taken from Ruelle [1993, p. 148], but originates from Gödel himself. It is worth repeating because it illustrates the issues involved in this debate particularly clearly. The story has to be told in the language of a classical Platonist.

Given a Turing machine and a natural number n, one can consider the finite set of programs of length up to n. When put into the machine each program terminates in a finite length of time or continues for ever. Let $T(n)$ denote the maximum running time of those programs that do terminate. If there existed an algorithm for computing the function $T(\cdot)$ then there would be a decision procedure for the Halting problem: given a program of length n let it run until time $T(n)+1$ and, if it has not halted, it never will.

The question is whether the function $T(\cdot)$ should be regarded as existing. A Platonist will answer yes, because he considers that the meaning of the definition is clear. The existence of $T(\cdot)$ is an elementary consequence of the assumption that every meaningful mathematical statement (in this case that every program either halts or does not halt in a finite but unspecified length of time) has a determinate value independent of our being able to know it. Within this context one has to admit that $T(\cdot)$ increases with unimaginable rapidity. If it were bounded above by $2\hat{}(2\hat{}(2\hat{}(2^n)))$ or any other computable function, then by using that function in place of $T(\cdot)$ above one would have produced a decision procedure for the Halting problem.

Within the context of constructive mathematics the function $T(\cdot)$ does not exist. We know, thanks to Gödel and Turing, that $T(\cdot)$ is not computable, so statements about it have no meaning to a radical constructivist. In Bishop's language it is a part of God's mathematics, not of ours.

4 Bishop's Philosophical Position

Bishop's version of constructive mathematics is what one obtains if one starts from the natural numbers and systematically avoids the use of the law of the excluded middle. This description already implies that every theorem of constructive mathematics is also a theorem of classical mathematics. It may also be thought of as the result of ascribing a new meaning to the symbol $\exists$. In order to assert $\exists x P$ one should present a means for constructing x; it is not enough to prove that $\neg(\forall x \neg P)$. In Chapter 1 of his book, Bishop justifies adopting this stricter interpretation of $\exists$ as follows.

> We feel about number the way Kant felt about space. The positive integers and their arithmetic are presupposed by the very nature of our intelligence and, we are tempted to believe, by the nature of intelligence in general ... We are not interested in properties of the positive integers that have no descriptive meaning for finite man. When a man proves a positive integer to exist, he should show how to find it. If God has mathematics of his own that needs to be done, let him do it himself. Bishop [1967, p. 2]

In spite of other comments of a similar type in the opening pages, Bishop was not a radical constructivist in the sense of Brouwer or Dummett. In the preface of Bishop [1967, p. x] he wrote

> We are not contending that idealistic mathematics is worthless from the constructive point of view. This would be as silly as contending that unrigorous mathematics is worthless from the classical point of view. Every theorem proved with idealistic methods presents a challenge: to find a constructive version, and to give it a constructive proof.

Billinge [2003] discusses all of Bishop's philosophical writings and comes to the conclusion that they cannot be rounded out into an adequate philosophy of constructive mathematics. This is certainly justified by the standards of academic philosophy, but Bishop was trying to persuade mathematicians of the merits of his ideas, rather than philosophers; see the comments of Nerode et al [1985] after his death in 1983.

If we accept this, some of Billinge's criticisms seem misplaced. Bishop's insistence that mathematics should start with the integers followed not only Kronecker, but the standard development of the subject in university mathematics courses, where the rational, real and then complex numbers are constructed from that starting point. Few mathematicians would regard this statement as controversial. Billinge also states that he does not address the possibility that the classical mathematician "has an entirely non-epistemic interpretation of 'x exists', which does not entail 'x

can be found' even by some infinite being". These passages are Bishop's explanations of why he considers classical mathematics less interesting than constructive mathematics. In Bishop [1985] he makes it clear that for him meaning is the fundamental notion, not existence. Two of the fundamental principles listed in Bishop [1985, p. 5] are: (B) do not ask whether a statement is true until you know what it means; (D) meaningful distinctions deserve to be maintained. When, in Bishop [1985, p. 7,7], he argues that 'classical mathematics fails to observe meaningful distinctions having to do with integers', he goes on to make it clear that he is criticizing the Platonic notion of existence.

Bishop's failure to persuade the mathematical community of the value of his point of view was partly a result of his polemical approach and partly because the time was not ripe. His book was published before computational issues started to have a real impact on the way pure mathematicians thought. Bishop himself said that his work was most appreciated by logicians and computer scientists; see Nerode et al [1985, p. 80]. Today the merit of his ideas should be acceptable to a much wider audience on a purely pragmatic basis, without asking them to accept a new and uncomfortable philosophical position. This is the approach I follow below. If there is to be a philosophical debate about the merits of constructive mathematics, this should come after it has established its mathematical credentials, not before.

5 Constructive Mathematics

In this section I have two goals. The first is to refute the indispensibility argument for classical mathematics by establishing that constructive mathematics is rich enough to allow its application to a wide range of problems in physical science. The second is to explain why classical mathematicians should be interested in Bishop's programme. I achieve both by considering a number of theorems in turn, explaining the reasons for the differences between the two versions of mathematics. I show that by adopting the constructive framework one can handle certain numerical problems on a systematic basis, whereas classical mathematicians have to deal with them piecemeal. As a pluralist, I do not, however, claim that constructive mathematics is preferable in all situations.

Producing examples to demonstrate the differences between classical and constructive mathematics often exploits the difference between recursive and recursively enumerable subsets of **N**. Another method is to define a sequence whose behaviour as $n \to \infty$ depends upon whether some famous conjecture is true or false.[2] Hellman [1989] calls this the 'method of weak counterexamples' and argues (against Dummett) that it is not possible to formalize the idea that there is an indefinite number of such examples without going outside the framework of intuitionism. Here one has to decide whether one is a radical constructivist, or merely regards

[2] If one uses the Goldbach conjecture, for example, then one puts $a_n = 0$ if $2n$ may be written as the sum of two primes, and $a_n = 1$ if it cannot.

the appeal to such examples as providing informal motivations.

We go even further down this route, showing that the impossibility of proving something in constructive mathematics is regularly *associated* with the extreme difficulty of showing it numerically for quite ordinary functions. We emphasize that several of the examples considered below do not, strictly speaking, fit into the constructivist framework, but we feel that in spite of this they help to explain why constructivism is of interest. Some of our examples below also illustrate a principle of Bridges: a classical proof of the existence of an entity often has no exact constructive analogue if the entity concerned is not unique; see Schuster [2003, 2004].

Let us start with the intermediate value theorem for continuous functions of a single real variable. Bishop [1967, p. 5] explains why the intermediate value theorem cannot be proved in a constructive framework. In the context of constructive mathematics one cannot find a value of the variable x for which $f(x) = c$ by the method of bisection, because being able to evaluate $f(x) - c$ with arbitrary accuracy does not imply that one can determine whether it is positive or negative.

Slight modifications of the intermediate value theorem are, however, valid constructively. If $f : [a, b] \to \mathbf{R}$ is continuous and $f(a) < c < f(b)$ then, given $\varepsilon > 0$, one can constructively find $x \in (a, b)$ such that $|f(x) - c| < \varepsilon$. In addition the intermediate value theorem itself may be proved constructively under a mild extra condition on f (being locally non-constant), which is almost always satisfied. See Bridges [1998] and Bishop-Bridges [1985].

We explain the differences between the classical and constructive versions of the intermediate value theorem by means of two examples, one ersatz and one genuine. Let

$$f(x) = \log(1 + x^{45}) \tag{1}$$

on $(-1, 1)$. *Given the formula* it is evident that the only solution of $f(x) = 0$ is $x = 0$, but one would need to calculate to very high accuracy to determine from its numerical values that the function is not identically zero throughout the interval $(-1/4, 1/4)$. However many digits one uses in the numerical computation, a similar difficulty arises if one replaces 45 by a larger number. In applied situations a closed formula is frequently not available, and the above problem may be a pressing one.

A genuine example presenting exactly the same difficulties is obtained as follows. Let

$$g(x) = \begin{cases} x + 1 & \text{if } x \leq -1 \\ 0 & \text{if } -1 < x < 1 \\ x - 1 & \text{if } 1 \leq x. \end{cases}$$

Then one cannot solve $g(x) = c$ even approximately if c is an extremely small number for which one does not know whether $c = 0$, $c < 0$ or $c > 0$.

When reading texts about constructive mathematics, one needs to be aware that

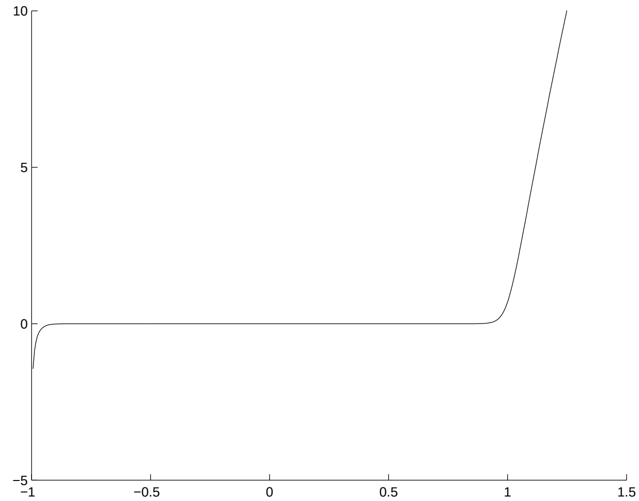

Figure 1: Graph of the function $f(x)$ defined by (1)

common notions do not have the same definitions as in classical mathematics. For example in Bishop [1967, p. 34] a continuous function on an interval $[a, b]$ is *by definition* uniformly continuous; in classical mathematics this is a fact which needs proof. See Schuster [2003] for a discussion of this point, and a constructive definition of pointwise continuity. When Bishop refers to the existence of a uniform modulus of continuity, he means, as usual, that a construction of the modulus is provided. For almost all (classically defined) continuous functions on $[a, b]$ a suitable uniform modulus of continuity is easy to produce, so this restriction is not practically important.

In constructive mathematics every continuous function on a closed bounded interval has an infimum; Bishop [1967, p. 35] provides a procedure for computing this with arbitrary accuracy. Constructively one cannot necessarily find a point at which the infimum is achieved. This can be a serious problem in numerical analysis as well. If ε is a very small constructively defined real number and one does not know whether $\varepsilon = 0$ or $\varepsilon > 0$ or $\varepsilon < 0$ then it is not possible to determine whether the polynomial

$$p(x) = x^4 - 2\pi^2 x^2 + \pi^4 + \varepsilon(x-2)^2 \tag{2}$$

takes its minimum value near $x = \pi$ or $x = -\pi$. For functions arising in applied mathematics that are not given by explicit formulae this can again be a serious problem.

Hellman has tried to put some flesh on the indispensibility argument for classical mathematics in several recent papers, in which he claims that there are no constructive versions of some key results in mathematical physics. It should be noted that Hellman is more critical of radical constructivists than of liberal constructivists; see Hellman [1989] and Hellman and Bell [2002]. Hellman's arguments have been analyzed by Billinge [2000], who concludes that they are not philosophically decisive;

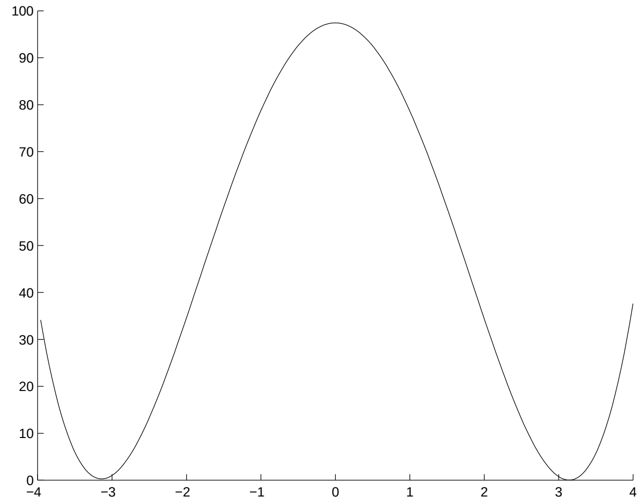

Figure 2: Graph of the function $p(x)$ defined by (2) with $\varepsilon = 0.01$

note, however, that she is only discussing whether radical constructivism is viable, and does not mention pluralism. We start with Hellman [1993a], which deals with Gleason's theorem, considered by some (but not the author of this paper) to be of considerable importance in the foundations of quantum theory. This concerns the (non-distributive) lattice $\mathcal{L}$ of closed subspaces of a Hilbert space $\mathcal{H}$ of dimension greater than 2. In this lattice the analogues of set-theoretic complements are orthogonal complements. Gleason's theorem states that if μ is a normalized, countably additive measure on $\mathcal{L}$ in a suitable sense, then there exists a non-negative, self-adjoint operator S on $\mathcal{H}$ with trace 1 such that

$$\mu(L) = \mathrm{trace}(SP_L)$$

for all $L \in \mathcal{L}$, where P_L is the orthogonal projection with range L. Hellman showed that a different version of Gleason's theorem cannot be proved in constructive mathematics. Nevertheless, Gleason's original version of the theorem, stated above, is constructively valid; see Richman and Bridges [1999] and Richman [2000]. The difference between the two versions relates to the use of the spectral theorem for self-adjoint operators, discussed next.

In Hellman [1993b] the author showed that one version of the spectral theorem for unbounded self-adjoint operators is not constructively valid. However, in his original book Bishop [1967, p. 275] had already proved a different version, for a commuting sequence of bounded self-adjoint operators, which is completely acceptable even to classical mathematicians. After Hellman's paper appeared, Ye [1998] published a constructive version of the spectral theorem for an unbounded self-adjoint operator. In 1989 an extraordinarily useful explicit integral formula for the operators involved in the functional calculus was given by Helffer and Sjöstrand, namely

$$f(H) = -\frac{1}{\pi} \int_{\mathbf{C}} \frac{\partial \tilde{f}(z)}{\partial \bar{z}} (zI - H)^{-1} \, \mathrm{d}x \, \mathrm{d}y$$

where H is a (possibly unbounded) self-adjoint operator and $\tilde{f}$ is a certain smooth extension of f from the real line to the complex plane; see Davies [1995, Ch. 2]. While not proved in the constructive formalism, there is little doubt that it could be. Hellman's focus on the issue of domains and unboundedness is misguided for two reasons. Firstly a standard technique for dealing with an unbounded self-adjoint operator is to study the associated bounded Cayley transform or the resolvent operators. The relationship between these is entirely constructive and the spectral properties are mapped into each other; see Davies [1980, 1989, 1995] for references to some of the enormous body of literature on this subject. Secondly an unbounded operator becomes bounded as soon as one makes its domain into a Banach space by endowing it with the graph norm

$$|||f||| = \sqrt{\|f\|^2 + \|Af\|^2} \, .$$

For many elliptic differential and pseudo-differential operators the domain is a Sobolev space and the graph norm is equivalent to the associated Sobolev norm. It may not always be decidable whether a vector in the Hilbert space is also in the domain of the operator, but this is not relevant to whether the notion of an unbounded operator is constructively coherent; see Bridges [1995], the response of Hellman [1997], and Dummett [2000, pp. 275-277].

Once again, the difficulty of determining whether a vector lies in the domain of an unbounded self-adjoint operator is not just a problem for constructivists. The classical theory progressed much more rapidly after it was realized that it was sufficient to specify an explicit domain of essential self-adjointness (a so-called 'core' of the operator) or even a core for the associated quadratic form; see Davies [1980]. A considerable amount is now known about the spectral theory of whole classes of differential operators whose domains cannot be identified as standard function spaces; see, for example, Davies [1989].

Over the last fifty years an extraordinary effort has been put into the numerical implementation of the spectral theorem for a wide variety of bounded and unbounded self-adjoint operators. It turns out that aspects of the classical spectral theorem that are not present in the constructive version correspond to quantities that may not be computable in numerical examples. Upper and lower bounds on the eigenvalues of self-adjoint operators, whether bounded or unbounded, can often be obtained by variational methods as described, for example, in Davies [1995, Ch. 4]. These can be made computationally rigorous if one uses interval arithmetic, discussed below, and they are certainly constructive. One cannot, however, always compute – or give a constructive meaning to the notion of – the multiplicity of an eigenvalue. If two eigenvalues cross each other as a parameter varies, it is usually impossible to find the exact value of the parameter for which they coincide.

The above facts are also relevant in a non-self-adjoint context. Let A_s be Fredholm operators which depend norm continuously on a parameter s. It is known that the index of the operators is constant, although the multiplicity of any eigenvalues may change discontinuously as they cross. It is not a coincidence that the numerically unstable eigenvalue multiplicities are of less interest than the index in the applications of Fredholm operators to global analysis.

If $c \in \mathbf{R}$ then the operator $A : l^2(\mathbf{Z}) \to l^2(\mathbf{Z})$ defined by

$$(Af)(n) = \begin{cases} cf(n+1) & \text{if } n = 0 \\ f(n+1) & \text{otherwise} \end{cases}$$

has classical spectrum $\{z : |z| = 1\}$ if $c \neq 0$ and classical spectrum $\{z : |z| \leq 1\}$ if $c = 0$. If c is a very small constructively defined real number and one is not able to determine whether or not $c = 0$, then the spectrum of A cannot be computed even approximately even though A is well-defined constructively. This implies that there exist straightforward bounded operators whose spectrum will probably never be determined.

The principal axes theorem is the classical result that every self-adjoint matrix has a complete orthonormal set of eigenvectors. The theorem is not constructively valid, and finding such an orthonormal set numerically may be computationally infeasible because of this; see Bridges [1981, p. 21] and Scedrov [1986]. As an elementary example, suppose that a self-adjoint matrix A_s depends upon a real parameter s and that two simple eigenvalues are extremely close to each other[3] when $s = a$. It may be the case that the eigenvectors of A_s change extremely rapidly, or even discontinuously, as s increases through the critical value. A very simple example of this phenomenon is provided by

$$A_s = \begin{bmatrix} \varepsilon & s \\ s & -\varepsilon \end{bmatrix}$$

If $\varepsilon = 0$ the two eigenvalues $\pm s$ cross at $s = 0$. However, for non-zero ε, however small, the two eigenvalues never cross, and the eigenvectors change very rapidly as s increases through 0. Although the eigenvalues and eigenvectors are analytic functions of s in both cases, the radius of convergence at $s = 0$ does not depend continuously on ε. These computational difficulties have to be reflected in any constructive version of the spectral theorem. What is of physical interest in such a situation is the rank two spectral projection associated with the pair of eigenvalues. Under suitable conditions this is numerically stable, and constructively definable. The classical version of the spectral theorem provides no insight into the existence of these computational problems, and does not suggest how they might be overcome. One *can* understand such problems in classical terms, but the constructive approach provides a systematic framework for doing so.

[3] A more complicated account is needed when several eigenvalues are very close to each other or coincide, but the techniques needed to deal with such cases are well understood.

Finally in Hellman [1998] the author showed that the Hawking-Penrose singularity theorem is not constructively valid. This is an interesting observation, since the theorem has been extremely influential in the subject. It is embarrassing for a radical constructivist but not for a pluralist. It remains extremely hard to say much about the nature of the singularities; one certainly cannot identify them as black holes without the benefit of a 'cosmic censorship hypothesis'. It is very likely that if a detailed description of the singularities becomes possible classically, that description will also be constructively valid. See Frank [2000] for a fuller discussion of all of the above examples, and others not mentioned here.

We next turn to the constructive theory of Banach spaces. A particular issue here is the constructive distinction between bounded linear functionals on a Banach space and normable linear functionals; see Bishop [1967, p. 297]. In numerical analysis an analogous problem[4] arises with functions such as

$$f(x) = \sin(\tan(\tan(\pi * x))) \tag{3}$$

defined for all except a countable number of points in $(0,1)$. It is elementary that f is measurable and that $|f(x)| \leq 1$ for all x at which it is defined. However, the L^2 norm of this function is given by

$$\|f\|_2^2 = \int_0^1 \sin^2(\tan(\tan(\pi * x)))\ \mathrm{d}x,$$

whose evaluation would involve serious difficulties both numerically and analytically, as attempts to plot its graph indicate immediately.

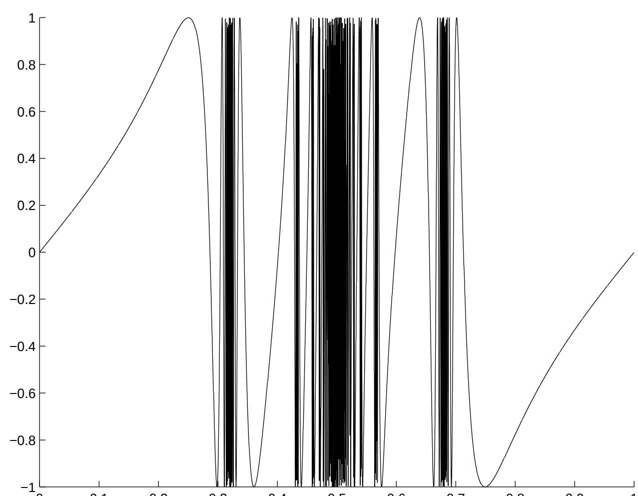

Figure 3: Graph of the function $f(x)$ defined by (3)

[4] We are *not* claiming that the given function can be used to provide an example of a bounded but un-normable linear functional on L^2. To obtain such a functional involves using similar but more extreme constructions.

The following example is less relevant to numerical analysis but gets closer to the reason for the distinction between bounded and normable linear functionals. For every positive integer n put $b_n = 1$ if the nth decimal place of π is the start of the first sequence of a thousand consecutive sevens in its decimal expansion; otherwise put $b_n = 0$. The sequence b is constructively defined even though we do not know (in 2004) whether such if such a sequence of a thousand consecutive sevens exists. Classically $b \in l^2(\mathbf{N})$ but constructively we cannot assert this, because we are not able to evaluate $\|b\|_2$ with arbitrary accuracy. Even if we were assured that a sequence of a thousand consecutive sevens existed, we would still not be able to place b in $l^2(\mathbf{N})$ constructively unless we were given some information about how large n had to be for $b_n = 1$.[5] Nevertheless the formula

$$\phi(c) = \sum_{n=1}^{\infty} c_n b_n$$

is constructively well defined for all $c \in l^2(\mathbf{N})$ and defines a bounded linear functional ϕ on $l^2(\mathbf{N})$. The moral of this is that the constructive version of $l^2(\mathbf{N})$ is smaller than the classical version, but it is still constructively complete because constructive convergence is a stricter notion than classical convergence.

A linear operator A on a Banach space $\mathcal{B}$ is said to be bounded if there exists a constant c such that $\|Ax\| \leq c\|x\|$ for all $x \in \mathcal{B}$. Its norm is the smallest such constant if that exists; of course classically this is not an issue. The constructive distinction between bounded and normable operators explained in Bishop [1967, p. 263] is related to the fact that there is no effective classical algorithm for determining the norms of bounded operators on most Banach spaces. This is why finding the best constants for various Sobolev embeddings and other operators of importance in Fourier analysis has occupied mathematicians for decades. The same problem occurs for large finite matrices – standard software packages only provide routines for computing the norm of a sufficiently large $n \times n$ matrix with respect to the l^p norm on $\mathbf{C}^n$ for $p = 1, 2, \infty$. If n is of the size needed to model three-dimensional vibrating structures, even the case $p = 2$ is non-trivial.

We finally discuss the status of the Hahn-Banach theorem in both classical and constructive mathematics. The classical version of the theorem states that if $\mathcal{L}$ is a closed linear subspace of a Banach space $\mathcal{B}$ and ϕ is a bounded linear functional on $\mathcal{L}$, then ϕ may be extended without increase of norm to $\mathcal{B}$. The theorem is one of the few advanced results of mathematics for which a complete formal proof, checkable by a computer, has been written down; see Nowak-Trybulec [1993] and Bauer-Wenzel [2004]. We are entitled to regard it as true, but only subject to the acceptance of Zorn's lemma, which is one of the ingredients in the formal proofs.

If one assumes that $\mathcal{B}$ is separable, then there is an alternative and more con-

[5] Taken literally this sentence is nonsensical. Classically there is no problem in asserting that $b \in l^2(\mathbf{N})$. Constructively one could not be given an assurance about the existence of n for which $b_n = 1$ without also being given the relevant information about how large n had to be. This is a familiar problem when one tries to compare two incommensurate frameworks.

structive approach. One can apply the one-step extension process for the linear functional iteratively, at each step including one more element from the countable dense set that is provided. After a countable number of steps the functional has been extended to a dense subspace of $\mathcal{B}$, and one can apply a completion procedure. This proof is not incapable of being criticized, in that the classical (i.e. Platonistic) formulation supposes that a countable number of successive steps can be completed, but it is more constructive than the general proof. By following this idea one obtains the constructive version of the Hahn-Banach theorem in Bishop [1967, p. 263], for normable linear functionals defined on a subspace of a separable Banach space.

Although the non-separable, classical version of the theorem applies in greater generality than the constructive version, we argue that if one examines the applications of functional analysis to the theory of partial differential equations, one finds that this extra power is to some degree spurious. The classical Hahn-Banach theorem implies that every Banach space $\mathcal{B}$ has a Banach dual space $\mathcal{B}^*$ with various properties. The precise structure of $\mathcal{B}^*$ has been determined for a large number of Banach spaces, including all of the standard Sobolev spaces and L^p spaces for $1 < p < \infty$. In all of these cases one can identify the dual spaces explicitly without reference to the Hahn-Banach theorem. Among those Banach spaces that analysts use frequently, almost all for which the dual space can be described explicitly are separable. The explicit constructive description of all normable linear functionals on L^p is given in Bishop [1967, p. 256] (before his proof of the Hahn-Banach theorem).

Two of the most important non-separable Banach spaces in analysis are $L^\infty(X, \mathrm{d}x)$, the space of essentially bounded functions on a measure space $(X, \mathrm{d}x)$, and $M(\Omega)$, the space of finite, countably additive, signed measures on Ω. In addition the entire theory of von Neumann algebras deals with non-separable Banach spaces. In all these cases the dual space is extremely abstract, and of limited use. Attempts at detailed descriptions involve substantial forays into formal set theory (see Kaplan [1985]). The Banach dual space of $l^\infty(\mathbf{Z})$ has a description in terms of the Stone-Čech compactification of the integers. This has a certain abstract fascination, but it seems to have no application to the theory of PDEs or other areas of analysis.

Our conclusion is that the constructive version of the Hahn-Banach theorem is adequate for many areas of analysis in which it is used in the course of solving problems in applied mathematics rigorously. This does not imply that one should reject the classical version, since classical mathematics can give valuable guidance about what might later be solved more explicitly.

6 Avoiding Unnecessary Philosophical Choices

Although Bishop was a pioneer in constructive analysis, prior to that he had done important work in several complex variables and the theory of uniform algebras. It might therefore be thought that he would have been a pluralist, but in fact he developed strong views about the 'philosophically correct context' for doing mathematics. It is perfectly possible to admire his work on constructive and finitistic mathematics respectively, but not to agree with his philosophical attitudes, which were not well developed.

One can identify three strands of opinion about the status of constructive mathematics, even after eliminating strict constructivists, such as Brouwer and Dummett, who consider that classical mathematics is ultimately incoherent. The first is that classical mathematics is the final arbiter of truth, but constructive methods may sometimes be pragmatically useful. In this view Bishop's book is simply a convenient formalization of the constructive approach, and has no foundational significance.

The second view (espoused by Bishop) is that constructive mathematics is the real thing, but that it may sometimes be helpful to use classical mathematics to provide intuition about what might later be proved 'properly' within constructive mathematics. Since classical mathematics is an extension of constructive mathematics, any theorem proved in classical mathematics will (almost surely) not be disproved in constructive mathematics.

The pluralist attitude, which is the one proposed in this paper, is that one need not and should not make such philosophical choices. One should simply accept each mathematical theory on its merits, and judge it according to the non-triviality and interest of the results proved within it. According to Schechter, this is increasingly the attitude that logicians take towards their subject.

> The "existence" of f – or of any mathematical object, even the number "3" – is purely formal. It does not have the same kind of solidity as your table and your chair; it merely exists in the mental universe of mathematics. Many different mathematical universes are possible. When we accept or reject the Axiom of Choice, we are specifying which universe we shall work in. Both possibilities are feasible... (Schechter [2004])

Among those who have expressed support for pluralism in mathematics or logic one should also mention Carnap [1950], Feferman [1977, p. 151], Hellman and Bell [2002] and, no doubt, many others.

One still has to ask whether the pluralist view is philosophically coherent. One might regard mathematicians who adopt it as anti-realists in practice, whatever they say, because their willingness to contemplate mathematics with the continuum hypothesis or axiom of choice one day and without them the next shows that they do not have any deep beliefs about the existence of the relevant entities. It is also possible to argue that a pluralist is a realist in denial, because in the last resort he accepts that the correctness of a proof is judged using two-valued classical logic: every step of a proof either is or is not correct, once one has agreed the framework for judging correctness.

Both of the above criticisms of pluralism result from attempts to force it into preconceived frameworks. The task of philosophers is to describe and analyze the best mathematical practice, not to make judgements about the validity of one or other part of it. Mathematicians, for their part, serve the scientific community best by providing a variety of different ideas and methodologies. This provides the best insurance against internal contradictions that might appear in one framework, and the best opportunities for discovering new ideas. I do not apologize for defending the free market in mathematics as others do in economics, and for much the same reasons. History is full of those who wish to control or at least to impose a uniform description on events, and such tendencies need to be resisted.

Acknowledgements We should like to thank D Bridges for helpful comments.

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