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Dedicated to A. Heyting on the occasion of his 70^{th} birthday

by

R. L. Goodstein

In discussing the question of the existence of mathematical entities I propose to take the existence of real objects in the real world for granted and to ignore the philosophical problem raised by the use of the word real, a problem which I have attempted to discuss elsewhere [1]. I shall need to stress the distinction between real collections, like the coins in a particular box at a particular moment, and the collections of mathematics, like the residues modulo five, or the triangular primes; and between the models of mathematics, the structures thought to exhibit the consistency of some set of properties, and models in the real world, such as the prototypes of a new invention. Thus in considering the question of the existence of natural numbers I shall be concerned with the existence of, for instance, the number two itself and not with real instances of the number, the pair of robins in my garden or the eyes in my head. I shall assume that the use of number words in these real contexts presents no difficulty and shall confine my considerations to the purely mathematical use of number words. Of course the logical problem of the application of mathematics to the real world is a very interesting one of great philosophical importance; my purpose in ignoring it is simply to confine this essay to one particular aspect of the problem of the existence of mathematical entities. I realize that it may be argued that if the problem of the application of mathematics to the real world is ignored then the problem of the existence of mathematical entities reduces simply to that of the existence of terminology or notation, and that mathematics deprived of application in the real world is just a meaningless game, but I hope to show that view is mistaken.

The problem of the existence of the primary entities of mathematics is analogous to the problem of the existence of the king of chess, or the ace of spades. Does it make sense to talk of the existence of the king of chess? Does the king of chess exist in the real world, or is he a fiction? Certainly there is no object in the real world to which we can point and say this is the king of chess. None of the pieces in a set of chess pieces is in this sense the king of chess, for a piece of wood or ivory can be lost or found but not the king of chess. When we call a particular piece of wood, in a particular game the king of chess, we recognise this particular object as an actor playing a part; we could if we wish give the part to another piece or to a lump of coal. The piece in question is just a sign for the king not the king himself. In saying that something is a *sign* for something, we appear however to be begging the question of existence; surely it may be said we cannot have a sign for something which does not exist. Yet we have a sign for conjunction, does conjunction exist? And we have no-entry signs. What exists in these cases is the *use* of the sign, the role of the sign in language, or in the world of action. The king of chess is no person or object in the real world, but a particular role in a game. So perhaps we must ask, not does the king of chess exist, but does chess exist? If you teach someone to play chess what you teach are the rules of chess; of course just knowing the rules is not playing chess. Although in learning chess there is nothing corresponding to the physical skill that has to be acquired in learning a ball game, (for we can ignore the actual movement of the chess pieces which would require skill only in very exceptional circumstances), nevertheless the rules simply limit the game and to learn to play chess is to learn the tactics and strategy of chess. But to answer the question whether chess exists, all that we can do is to point to the rules. Where are these rules to be found? Presumably there is some agreed set of rules (sentences written in the official rule book of the world chess federation let us suppose) which like the standard yard serves as the original from which copies may be made, yet one is not tempted to think that if this standard rule book was destroyed then chess would cease to exist. But of course if all copies of the rules were lost, copies on paper or in the memories of men then chess would cease to exist however many beautiful sets of these pieces remained, and if other games were played with these pieces then these games would not be chess, but what should we say if the new game played with these pieces was in fact according to the old rules, but no one knew?

If we now turn from chess to arithmetic we see that the answer to our first question, does the number two exist is that the number two is one of the roles played by the signs in arithmetic and that the proper question to ask is not does the number two exist, but does arithmetic exist. This question is immensely more difficult to answer than the corresponding question for the game of chess. There is no one agreed set of rules for arithmetic to which we can point, only various "codifications of arithmetic", such as recursive arithmetic, or some theory of sets. The existence of these diverse codifications has tempted some philosophers of mathematics to think that there must exist some primordial arithmetic (in man's intuition since it is clearly not elsewhere) to which these codifications approximate and this tendency has been strengthened in recent times by the discovery that in some sense no codification is complete, but fails to include in its class of consequences some arithmetical relation that we might have expected to find. Whether or not man has a primordial intuition of arithmetic is a matter for psychological investigation, but it is I think quite irrelevant to the question of the existence of arithmetic. Even if one could find an arithmetic imprinted in the brain this would be only another codification (and doubtless very imperfect). The greatest mathematicians have known at best only tiny fragments of the sum of arithmetical knowledge. Admittedly it could be agreed that what they knew was (like the similar portion of an iceberg) only a part of the arithmetic that lies in their subconscious, but this is only another way of saying it was not in their conscious mind, and could with as much illumination be said to be in God's mind — but not in man's. Even if we found an oracle who correctly answered every arithmetical question asked of him, it would be a sheer assumption to suppose that answers not yet elicited were in fact already known to the oracle. However much we speeded up the questions some would remain unasked and the answers would all have to be verified, so we cannot look to a transcendental source for arithmetic to assure its existence. Rather it seems to me we must regard arithmetic, not as a single game like chess but rather as an evolving series of games. Just as we can talk of an evolutionary development without assuming a final end product, so we can contrast different codifications of arithmetic without having to assume the existence of some whole of which these codifications are part. A comparison with the history of geometry is helpful at this point. Many remarkable geometrical properties were known and a large body of geometrical (ruler and compass) constructions had been worked out, before the first attempts were made to codify geometry. The sense in which these properties were true, and the constructions correct, at this earlier stage is very interesting to contemplate. I think it must be conceded that this body of knowledge belonged at first, not to mathematics but to a stage in the evolution of mathematics from experimental physics. The constructions were correct in so far as they worked, and achieved their aim, on the drawing board. The geometrical facts were of various kinds; some were physical properties of drawings (congruence of triangles) others were already contained in partial codifications which exhibited relationships between some properties and others. When the attempt was made to codify systematically it was this existing body of knowledge that needed to be included in the codification, not some hypothetical universe of truths.

Whether natural numbers are the primary subject of arithmetic, as they are conceived to be in recursive arithmetic, or are properties of classes as in set theory, is not a question about the real world, but about the choice of formalism. It is only by confusing the classes of set theory, a symbol role in a calculus, with real classes that we are led to think that set theory gives numbers a real existence denied to them in recursive arithmetic. The application of arithmetic to the real world, which is made when we treat real classes as number signs (an operation we so misleadingly describe as abstracting the number concept from collections) is outside any codification of arithmetic. Only the transformation from one number sign to another belongs to formalism, for instance from 1111 to 4; what we call the perception that a cat, a dog, a sheep and pig in a field make four animals in the field is a fairly complicated series of steps, the first of which consists in marking a 1 for each animal in the field. Exactly how this is done is perhaps unimportant; we may look at each animal in turn and say "one", or cut a notch on a stick as the stick touches each animal in turn, or make a mark on paper. The remaining steps are those we take in a formal calculus from 1111 through 211, 31, 4 by means of the definitions $11 = 2$, $21 = 3$, $31 = 4$ (where 21 denotes 2 and 1, not twenty-one of course).

I want to turn now from the logical problem of the existence of numbers to the purely mathematical problem of the existence of numbers with certain properties, for instance the existence of an even number which is not a sum of two primes. This may be called the secondary problem of existence, which takes for granted the structure of arithmetic, and is concerned with features of this structure. I used to think that it was an essential logical pecularity of such a question that we knew how to look for an affirmative answer but have no means of finding a negative answer. Thus we may enumerate all even numbers and test each in turn to see whether it is a sum of two primes or not; in this way we may chance upon an even number with the desired property, but failing to find one is no proof that such a number does not exist. But we can put the search for a negative answer on the same footing as the search for a positive answer. We may number all the sentences and proofs of some formalisation Z, say, of arithmetic, in particular the sentence G, say, which affirms that any even number is a sum of two primes, and then we may test each number in turn to see if it is the number of a proof of this sentence. Carrying out the two searches in parallel we may as readily chance upon a negative as an affirmative answer to the original question.

However for our present purpose it is more interesting to suppose that we search not just for a proof of G but also for a proof of its contrary $\neg G$. If we chance upon an even number $2n$ $(n \geqq 2)$ which is not a sum of two primes this would be of no philosophical interest; but suppose we chance upon a proof of $\neg G$. This proof might of course be no more than the verification that some particular number is not a sum of two primes, but it might be an indirect proof which showed that from G a contradiction may be derived in Z. Supposing that Z contains an existential quantifier $\exists$ the indirect proof would establish the sentence

$$(\exists x)(x \geqq 2 \;\&\; 2x \text{ is not a sum of two primes}).$$

Our task is to examine the nature of this existence assertion. Now there are some formal systems in which from the proof of an existence assertion $\exists x\, G(x)$ we can calculate an actual number $\boldsymbol{n}$ for which $G(\boldsymbol{n})$ holds, (using "calculate" in a sense which we must make clearer later on). Leaving such a case on one side for the moment let us assume that a certain sentence $\exists x Gx$ has been proved in a formal calculus, but no number $\boldsymbol{n}$ for which $G(\boldsymbol{n})$ holds has been *found*. Shall we then say that an $\boldsymbol{n}$ for which $G(\boldsymbol{n})$ holds exists but has not been found? It might at first seem reasonable to talk of the existence of something which has not been found, by analogy for instance with Physics where the existence of certain elements was predicted by the periodic table but the elements were unknown until they were produced by nuclear fission. I think however this analogy is a false one. On the one hand the proof of $\exists x Gx$ is not a prediction that an x satisfying

$G(x)$ will one day be found and on the other hand can we speak of the existence of an unknown concept (other than metaphorically), unless we suppose that there is some transcendental mind in which it exists, an assumption which makes mathematics a branch of theology. It would seem preferable to say that a formal system in which $\exists x G(x)$ is provable, but which provides no method for finding the x in question, is one in which the existential quantifier fails to fulfill its intended function; this does not oblige us to reject the formalism, only the intended interpretation of it. Of course there *is* a sense in which we may say that a number exists but has not been found. For example, the least prime greater than $(10^{10})!$ exists but is not known. However, what we mean in this case is that a way of determining this prime is known, although no one has made the journey. In fact in such cases what we are asserting to exist is a *function*. We can say that there is a prime greater than $(10^{10})!$ because the function "the smallest $k \geqq 2$ which divides $n!+1$" supplies a prime greater than n for each n.

Let us now return to consider a system in which a value of $\boldsymbol{n}$ for which $G(\boldsymbol{n})$ holds is somehow determined by a proof of $\exists x G(x)$. Leaving aside the trivial case in which $\exists x Gx$ is itself derived from $G(\boldsymbol{n})$, we can imagine that the required $\boldsymbol{n}$ is determined as a function of the number g say of $G(x)$ and the number p, say, of the proof of $\exists x Gx$; thus

$$n = f(g, p),$$

and given the values of g and p we can calculate n. The burden of existence now falls on the function f. Under what circumstance shall we say that a *function* exists. The question of the existence of a function is bound up with the question of the definition of a function. If we merely say a function $f(n)$ exists if there is a rule which determines the value of $f(n)$ when n is given then of course we have merely replaced the problem of function existence (definition) by that of rule existence (definition), and we have made no progress. Let us start with a particular case, the existence of the sum function in arithmetic. In recursive arithmetic the sum function $x+y$ is defined by the recursion

$$x+0 = x,\ x+sy = s(x+y)$$

(where sx is the successor of x). This is clearly not a definition in the sense of explaining a new term in terms of an old one. The unknown $+$ appears on both sides of the recursive definition $x+sy = s(x+y)$, and the recursion definition does not enable us to eliminate $x+y$ in favour of some other concept. The definition does however enable us to eliminate the plus sign in any such context as $sss0+ss0$, and enables us to transform this sign complex into $sssss0$ in which the plus sign no longer appears. Thus to say that an addition exists (in recursion arithmetic) is just to say that the plus sign has a role in arithmetic and that the sum function serves to transform $\xi+\eta$ for any numerals ξ, η into a numeral. If we now ask how do we know that there *is* a function $f(x, y)$ with the properties $f(x, 0) = x$, $f(x, sy) = sf(x, y)$ we must assume that this depends upon what we mean by function existence. If we are asking for a proof of a formula of the form $\exists f(f(x, 0) = x \,\&\, f(x, sy) = sf(x, y))$ then of course such a proof can be given *in a rich enough system of logic*. But as we have already seen even relatively weak systems with existence quantifiers may fail to admit their intended interpretations, so how much less grounds have we for supposing that a richer system secures the intended interpretation of its quantifiers. No, the purpose of a proof of the formula

$$\exists f(f(x, 0) = x \,\&\, f(x, sy) = sf(x, y))$$

is not to satisfy ourselves of the existence of the sum function, for this it certainly cannot do, but to enable us to introduce the sum function as a term *in a formal system which lacks any other mode of function definition.*

The introduction of function signs into a formal system tempts one to seek to distinguish between the existence of a function and its computation. Let us say that a function f is computable in a formalism Z if for each numeral $\boldsymbol{a}$ there is a numeral $\boldsymbol{b}$ such that the equation $f(\boldsymbol{a}) = \boldsymbol{b}$ is provable in Z, then we may prove that (in a suitable Z) that there exists an f which is not computable in Z. Let $f_i(n)$ be an enumeration of all one variable primitive recursive functions; then it is well known that there is no decision procedure for the class of identities $f_i(n) = 0$. Further, let

$$F_i(n) = 1 \dot{-} \{1 \dot{-} \sum_{x \leqq n} f_i(n)\}$$

so that for each i, $F_i(n)$ is non-decreasing, primitive recursive, and takes only the values 0, 1. Hence (in a suitable Z) we may prove that (for each i) $F_i(n)$ is constant from some n onwards, i.e. we may prove the formula

$$(\exists m)(\forall n)\{n \geqq m \rightarrow F_i(m) = F_i(n)\}.$$

Finally we define $\phi(i)$ to be the least value of m such that

$$(\forall n)\{n \geqq m \rightarrow F_i(m) = F_i(n)\}.$$

Then $\phi(i)$ is not computable in Z; for otherwise to each numeral $\mathbf{i}$ there corresponds a numeral $\mathbf{j}$ such that $\phi(\mathbf{i}) = \mathbf{j}$ is provable in Z, and since $F_i(n)$ is primitive recursive there is a $\mathbf{k}$ such that $F_i(\mathbf{j}) = \mathbf{k}$ is provable in Z and therefore

$$n \geqq \mathbf{j} \rightarrow F_i(n) = \mathbf{k}$$

is provable in Z.

If $\mathbf{k} = 0$, it follows that $f_i(n) = 0$ identically, and if $\mathbf{k} \neq 0$ then $f_i(\mathbf{j}) \neq 0$; thus the assumption that ϕ is computable in Z leads to a decision procedure for the class of equations

$$f_i(n) = 0$$

and since no such decision procedure exists, it follows that ϕ is not computable. Of course ϕ can only be introduced in this way into a formal system admitting a minimal operator, or some corresponding rule of function introduction. The question which interests us here is the one which we are first tempted to ask in the form: is ϕ really a function? Can there be functions which cannot be computed? But the problem is not really one of existence but of word usage. Shall we *call* ϕ a function since ϕ is not computable? We start with the definition that a function is an association of argument with value, and clearly (on this definition) it would make no sense to say that there is a function which has no value for some argument. But the position with ϕ is different, for all that we have shown is that ϕ "cannot be computed" in a certain system, and this may be regarded as a deficiency of the system rather than a failure of ϕ to satisfy the definition of function. And of course we may change the definition of function to cover, for instance, partial functions (defined only for some argument) and even, *in extremis*, to allow the class of arguments for which the function is defined to be unspecified. It may be argued that a function whose values cannot be computed in a certain formal system, i.e. a function sign ϕ for which no equation $\phi\mathbf{i} = \mathbf{j}$ is provable with numerals $\mathbf{i}, \mathbf{j}$, is of no use in the system, but whether this is so or not is purely a practical question and has nothing to do with the problem of existence. As I have remarked before, the intuitionist dispute about existence is at bottom a dispute about the use of the word function.

The role of a function sign f in formalised arithmetic is best described by saying that if f is a function sign then $f0$, $fs0$, $fss0$, $\cdots$

are *numerals*; in other words function signs serve to enrich the class of numerals. A computation of a function for an assigned argument $ss \cdots s0$ is the derivation of an equation

$$fss \cdots s0 = ss \cdots s0$$

between a numeral involving the sign f and one without f. A system which admits non-computable functions is a system which admits numerals outside the set $0, s0, ss0, \cdots$.

Each succeeding category of numbers may pose afresh the existence problem. What are rational numbers, do rational numbers exist? If we answer that rational numbers are ordered pairs of natural numbers a/b $(b \neq 0)$ with the arithmetic

$$a/b = a'/b' \leftrightarrow ab' = a'b$$

$$a/b+c/b = (a+c)/b$$

$$(a/b) \cdot (c/d) = (ac)/(bd)$$

we have thereby solved the problem by making rational number a dispensable concept. Everything we say (in equations) about rational numbers is equivalent to some statement about natural numbers, in virtue of the above equivalence. But can we not doubt the existence of ordered pairs of natural numbers even if the existence of natural numbers is conceded? A pair of numbers is not a concrete object like a pair of shoes. But a pair of numerals, ordered say from left to right is just such a concrete object, and the rules for the use of this ordered pair of numerals, the rational number sign, are precisely what the rules of the arithmetic of rational numbers are. Instead of describing the arithmetic of rationals in terms of the transformation rules of the rational number *signs* we could of course construct the rational numbers within some other formal system, for instance within a formalisation of set theory, since by the well known device, an ordered pair may be represented by a set. But it is important to recognise that the rational number is no more securely based in the real world by this device. The set to which the rational number is thereby reduced, is just as much a role in a formal calculus as the rational number is in the first definition, and since formalisations of set theory are less secure from inconsistency than direct formalisations of arithmetic, constructing the rationals within set theory, rather than outside it, is the more perilous route to follow. The passage from rational to real numbers is more difficult. I do not refer to the technical difficulties in setting up the arithmetic of real numbers, but to difficulties in the concept itself. The Dedekind definition is perhaps the simplest, identifying a real number with a set of rationals, and so making the theory of real numbers a part of set theory. But if we seek for a formalisation of the real number concept outside set theory we can use Cantor's definition, and by introducing function signs into the arithmetic of rationals, identify real numbers with convergent pairs of functions $f(n)/g(n)$; in this way the problem of the existence of real numbers is reduced to the problem of the existence of functions, which we have already considered.

To each level of constructive definition of function (and of convergence) corresponds a class of constructive real numbers. At the strictest level of constructivity we have primitive recursive real numbers and the associated analysis formalisable in a free variable calculus. At the next level, that of general recursive functions, the associated analysis may possibly also be formalisable in a free variable calculus, but since there can be no general procedure for deciding whether a set of equations determines a general recursive function or not, the system lacks the essential attribute of a formalisation that its syntax be finitely characterised; the *natural* formulation of general recursive arithmetic and analysis is by means of quantifiers over natural numbers. In the Dedekind Theory too constructive definition of sets delimit constructive classes of real numbers, and at the various levels of constructivity the Dedekind and Cantor classes of real numbers fail to coincide, or coincide or not according to the constructive level of proof.

In studying the existence problem for complex numbers the interest again lies in the diversity of possible definitions. Since a complex number may be defined as an ordered pair of real numbers with a certain arithmetic, and since an equation between complex numbers is equivalent to a pair of equations between real numbers, the existence of complex numbers *vis-à-vis* real numbers is of the same order of logical difficulty as the existence of rational numbers vis-à-vis natural numbers. The curious product rule for complex numbers in the ordered pair definition presents a purely pedagogical difficulty, not a logical one. It is often said that the generation of the complex numbers as the residue field of the ring of polynomials over the real numbers modulo x^2+1, is logically preferable to the axiomatisation in terms of pairs of real numbers, both because the generation of a field as a field of residues is an important general procedure and because it obviates the artificial definition of multiplication that mars the number pair approach. The generality of the method of residue fields is undeniable but the logical advantage of its use for the generation of complex numbers is illusory. For to define the ring of polynomials, we must first define polynomial, and the polynomial of modern algebra is simply an ordered set, subject to a certain arithmetic more complicated than that of the complex number as an ordered pair of reals. One cannot seek logical priority for a definition which presupposes the existence of ordered sets of arbitrary length in order to establish the existence of an ordered pair. A third method of defining the complex numbers is to introduce a constant i and add to the axioms for a field the axiom $i^2+1 = 0$. This method of course raises the consistency problem in an acute form. The new axiom contradicts a well known property of real numbers; moreover the addition of the axiom $i^2+1 = 0$ to the axioms for an *ordered* field is readily shown to be inconsistent. Now the field axioms, together with the axiom $i^2+1 = 0$ may be shown to be consistent by the method of models. We argue that since the arithmetic of ordered pairs of real numbers satisfies the field axioms and contains an element, namely (0, 1), which satisfies the axiom

$$(0, 1)^2+(1, 0)^2 = (0, 0)^2$$

and since the arithmetic of pairs $(x, 0)$ is isomorphic to the arithmetic of real numbers, therefore this arithmetic of ordered pairs furnishes a model of the axioms showing that the axiom system is consistent — in the sense that any contradiction that could be derived from the axioms could be derived in the arithmetic of ordered pairs and so eventually in the arithmetic of real numbers. Though of course this proves no more than relative consistency it does give the third method of defining complex numbers at least as much security as the first. But again logical priority must be given to the ordered pair definition for the structure which this axiomatisation determines furnishes the model for the consistency proof. It is sometimes argued however that the third method alone is general enough to characterise the complex numbers, and that the other methods merely construct models of the complex numbers. At first sight this criticism may be thought to be valid. If for instance we prove the axioms for Boolean algebra consistent by observing that they are satisfied by the ring of integers modulo 2, the model which establishes consistency, is the logically prior structure but cannot be identified with the concept of Boolean algebra. The difference in the two cases is that the models of Boolean algebra are not isomorphic structures, whereas one can show that every model of the field axioms fortified by the axiom $i^2+1 = 0$ *is* isomorphic to the arithmetic of pairs. It is then just a matter of the choice of language whether we should now identify the complex numbers with the arithmetic of pairs, or call this arithmetic a model of the complex numbers; in either case this arithmetic must be accorded priority.

Can a categorical system of axioms, a system with only isomorphic models, be held to define a concept? Or should we seek to identify the concept with some one of the models? Clearly we cannot ignore the problem of consistency since we would not wish to say that an inconsistent set of axioms defines a concept. A system of axioms may however be proved consistent without appeal to the existence of a model, and so the existence of a model cannot be necessary to give an axiom system the power of creating a concept, if in truth an axiom system has this power. In fact the very use of the language of models is the source of the confusion. When we speak of a model satisfying a set of axioms (as in the case of the arithmetic of pairs and the extended field axioms) we are contrasting, not an abstract system with a concrete realisation as the language leads us to think, but two axiomatic systems. *Each in fact is a model of the other*, for this is what the isomorphism of the models really means. The arithmetic of pairs satisfies the extended field axioms, and the extended field axioms characterise an ordered pair with the same arithmetic. Our reason for calling the first the model, and not the second, is that the first is transparently free from contradiction (relative to the real number system) whereas the other is not.

There is of course another sense in which we speak of models in logic, when we talk for instance of so-called intuitive (i.e. non-formal) arithmetic as a model, or talk directly of an intuitive model. But even in such cases the model is a logical structure, not a mental, or a "real" entity; we reserve the term formal for a particular style (or styles) of axiomatics and different styles of formalisation are by contrast, misleadingly called intuitive (for example when generality is only a feature of a proof and not made explicit by the use of variables).

For more than a quarter of a century the axiom of choice and its equivalents was a subject of controversy. Some mathematicians used it freely and others shunned its use. Eventually Gödel showed that if say, Zermelo-Fraenkel set theory is consistent without the axiom of choice then the addition of the axiom of choice does not introduce a contradiction, (and recently P. J. Cohen showed that the axiom of choice is actually independent of the remaining axioms). Looking back on the controversy the natural question which arises is this: why did not every mathematician who drew inferences from the axiom of choice before Gödel's proof of relative consistency feel that he was wasting his time, since a subsequent discovery of a contradiction would make all his efforts vain? No doubt the mathematicians in question would answer that they believed in the axiom, and as a psychological explanation this perhaps suffices, but of course it is just as easy to believe in something false as in something true — as the centuries of belief in a flat earth shows. And Specker proved the axiom of choice is actually false in a seemingly "natural" formulation of set theory due to Quine. Another defence could be that, since we do not know that set theory without the axiom of choice is consistent, those who deprived themselves of the axiom were no less credulous than those who used it, but this is rather like saying that it is no more foolish to drive a car on an icy road than on a dry one, because there are accidents on dry roads.

Does the axiom of choice *create* the choice set? Can one supply a missing set just by a declaration of existence? Of course no axiom, no declaration of existence, can create a real object, but it is not the purpose of an axiom to create a real object. An axiom has a part to play only in a formal system. The axiom of choice is a *limitation on the use of the word set* in formalised set theory. The acceptance or rejection of the axiom of choice is a decision about the use of a word. Some prefer one use of the word and others a different use. To ask if the axiom of choice is true is to confuse the world of mathematics with the real world. The only relevant question is whether such and such a body of axioms is consistent — and fruitful. Why then should we stop short at the axiom of choice? Why not throw into our formal system more and more axioms, postulating Fermat's last proposition, for instance, or the existence of measurable cardinals? It may be that Fermat's last proposition and its denial are both consistent with (say) recursive arithmetic, so that the addition of this proposition (or of its denial) to arithmetic would be an enrichment of mathematics, but lacking a proof of consistency (and independence) the adoption of Fermat's proposition as an axiom might simply lead to an immense waste of human endeavour. We are not limited in our choice of axioms by the nature of the world but by the state of our knowledge of mathematics.

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