WHERE MATHEMATICS COMES FROM BY GEORGE LAKOFF AND RAFAEL E. NUÑEZ BOOK REVIEW

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The authors, a linguist and a cognitive psychologist, have written what they claim is a comprehensive and pioneering study of the way humans perceive and conceptualize mathematics. Not only that, but they also claim to disprove widely-held (by both the public and mathematicians themselves) "myths" underlying the predominant views of the philosophy of mathematics.

I am a mathematician, not a psychologist, and I am in no way qualified to comment on the quality or significance of the authors' work from a cognitive psychology standpoint. I will thus mostly limit my evaluation to the mathematical and philosophical content of the book. Unfortunately, although the work is thought-provoking and raises some important issues of interest to mathematicians, it is seriously, even fatally, flawed in these respects. It is hard to imagine how reputable mathematicians such as those quoted on the back cover could have endorsed the contents if they actually read the manuscript.

The authors superficially appear to be broadly knowledgeable about mathematics; many aspects of modern mathematics are mentioned and briefly discussed, usually in appropriate contexts. One eventually begins to feel that much of these discussions is simply the mathematical equivalent of name-dropping, however, and that the authors are mostly just repeating verbatim statements they heard in conversations with mathematicians. This unfortunate conclusion is hard to avoid when one sees the amateurish mistakes they make in almost every real attempt to discuss mathematical concepts. The authors' knowledge and understanding of mathematics may be a mile wide, but it is only an inch deep. My impression is that their command of philosophy can be similarly characterized. (I must acknowledge that my own expertise in philosophy is neither broad nor deep, although what expertise I do possess is mainly in the philosophy of mathematics.)

I am reminded of a couple of quotes:

> "And yet I must not omit to mention, that some persons, more ostentatious than learned, have laboured about a kind of method not worthy to be called a legitimate method, being rather a method of imposture, which nevertheless would be very acceptable to certain meddling wits. The object of it is to sprinkle little drops of science about, in such a manner that any sciolist may make some show and ostentation of learning ... being nothing but a mass and heap of the terms of all arts, to the end that they who are ready with the terms may be thought to understand the arts themselves.

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> Such collections are like a fripper's or broker's shop, that has ends of everything, but nothing of worth."
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> *Francis Bacon*[1]
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> "[Using the machine] the most ignorant person at a reasonable charge, and with a little bodily labour, may write books in philosophy, poetry, politics, law, mathematics, and theology, without the least assistance from genius or study."
>
> *Jonathan Swift*[2]

Limits and Continuity. The most obvious mathematical howlers in the book are the authors' repeated misadventures with limits. Their explanation of how the human mind understands infinity and, thus, limits, is through what they call the *Basic Metaphor of Infinity (BMI)*. I have no comment about the accuracy of this theory except to say that I have no particular reason to doubt it. But the authors seem to think that once the BMI has been formulated, no further justification for the existence of limits is usually needed, and they assert that limits exist because of the BMI in many instances where the conclusion is unwarranted or at least requires real proof.

The real problem, though, comes when they try to give and explain the actual (Weierstrass $\epsilon - \delta$) definition of limit. They get it backwards at first (paraphrasing, they say that $\lim_{x \to a} f(x) = L$ means that, whenever $f(x)$ is close to L, x must be close to a), and their clumsy and convoluted attempt to explain the definition in words makes it clear that the mistake is a real misunderstanding of the definition and not just an inadvertent or typographical error. They finally write the definition correctly on p. 312, but evidently do not realize the difference between this version and the earlier one (their description in words on p. 310 is still backwards). A student in my junior-level Real Analysis class who could not do a better job than this of writing and explaining the definition would be in serious danger of failing the course.

An even more gruesome blunder occurs in the argument on pages 416-419, based on the assertion that any algebraic manipulation of limit expressions is justified [i.e. commutes with taking limits] by "simple algebra" since "limits are always numbers." A Real Analysis student who believed that this was even an outline of a valid proof would almost certainly fail. Even a calculus student who has seen indeterminate forms should be able to recognize that this argument is baloney. (Incidentally, contrary to the authors' assertion that texts do not explain the stated result, actual proofs can be found even in some Business Calculus texts.) It is hard to take seriously anything the authors say about mathematics after reading stuff like this (how can they expound intelligently about how humans understand mathematics if they don't understand it themselves?)

The authors also confuse several other basic mathematical concepts:

***The graph of a function* vs. its *range*.** This confusion leads in part to their conclusion that there can be no such thing as a space-filling curve. Closely related is their confusion of *curve* and *parametrized curve*.

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[1] Speaking of the Lullian Art, *De augmentis scientarium*, Book VI, Chapter 2.

[2] Describing a machine which generates random phrases, *Gulliver's Travels*, Part III, Chapter 5.

Claiming that graphs of continuous but nondifferentiable functions are not curves because they do not satisfy the conditions spelled out by mathematician James Pierpont in 1899 for what he thought a curve should intuitively be is about like saying that a recently-discovered exotic creature is not an animal since it does not conform to the definition of animal given by some 19th century biologist. (By the way, these graphs satisfy more of Pierpont's conditions than the authors think.)

Continuity* vs. *differentiability. They talk about "naturally continuous" functions without giving a real definition, but state that such a function [perhaps meaning its range or graph?] must be a path traced out by a moving particle, which must at each instant of time have a definite [instantaneous] direction of motion (this after criticizing the mathematicians' view of a line as a collection of points, claiming that this view "discretizes" the intuitive notion of a "continuous" line!) They then go on to say that the term "continuity" is "mistakenly" applied to functions satisfying the $\epsilon - \delta$ definition (p. 323).

They even assert that every continuous function is analytic! (p. 442) The explanation of why this should be true on p. 444 is a masterpiece of gobbledygook, although it contains a kernel of truth (I suspect the authors unsuccessfully tried to reconstruct an argument they heard from a mathematician; the text bears a suspicious resemblance to the standard heuristic argument that two analytic functions agreeing on a nondiscrete closed set agree everywhere). They also don't understand that infinite sums cannot always be treated like finite sums (e.g. they regard term-by-term differentiation of a power series as a triviality).

By the way, would they consider the function $f(t) = (t^3, t^2)$ to be continuous? Its range is the graph of a continuous but nondifferentiable function. And if a moving particle must follow a differentiable path, what about a billiard ball bouncing off a cushion, or a baseball being struck by Mark McGwire's bat (p. 345)? What about Brownian motion of a particle? (Of course, balls bouncing off hard surfaces do not behave exactly as point masses at small scales of time and distance, nor is Brownian motion truly fractal at submicroscopic scales, and the actual physics of collisions is rather complicated, but every mathematical description of motion according to standard laws of physics is really just an approximation.) The graph of $f(x) = x \sin 1/x$ (p. 316-317) looks like the motion of a pinball wobbling down an ever-narrowing trough toward an exit hole. Couldn't this be the path of a moving particle? And is the function $g(x) = x^2 \sin 1/x$ continuous? It is differentiable, but if a moving point tried to follow this path, its instantaneous direction of motion at time 0 would vary discontinuously.

Algebraic completeness* vs. *order-theoretic (geometric) completeness. This is one of many instances where the same term is used in mathematics to mean entirely different things in different contexts, which can be quite confusing to the uninitiated who are inclined to make too much of the coincidence of terminology. Other common examples are the terms *closed*, *separable*, and *independent*. In the case of algebraic vs. geometric completeness, the problem is compounded since the concepts are not entirely independent; geometric completeness implies a certain amount of algebraic completeness (e.g. the existence of $\sqrt{2}$). The authors tend to take mathematical terms too literally (but see the section on lines below); in particular, they really do seem to regard the real numbers as "real" (in the sense that any mathematical concept is "real" from their perspective) but not the imaginary number i. Mathematicians generally regard the terms "real" and "imaginary" as historical artifacts which are still used merely for convenience; there is nothing inherently more "real" about $\sqrt{2}$ than about i.

Nonstandard Analysis. The authors are in way over their heads when they try to discuss nonstandard analysis. They seem to think they have discovered (or invented) a new and better way to do nonstandard analysis via what they call "granular numbers," which they suggest mathematicians like poor Abraham Robinson couldn't, or at least didn't, think of since they were constrained by traditional mathematical reasoning (p. 254). This is, of course, nonsense. It is hard to figure out exactly what structure the authors have in mind for the granular numbers, since the construction is not really spelled out except to say that it all comes from the BMI, along with an imprecise reference to the compactness theorem of model theory (incorrectly stated). And in attempting to apply the compactness theorem, they consider the set of all numbers satisfying nine axioms, whatever it means for an individual number to "satisfy" the associative law and the other field axioms. But the basic idea is to construct an ordered field containing $\mathbb{R}$ and also an infinitesimal.

There are simple and well-known ways to do this. Perhaps the easiest is to form the field $\mathbb{R}(X)$ of rational "functions" in one variable with real coefficients [$\mathbb{R}(X)$ is formally constructed as the field of quotients of the polynomial ring $\mathbb{R}[X]$, and the elements of $\mathbb{R}(X)$ are not really functions, but they can be thought of this way, or rather as equivalence classes of functions, with two functions identified if they agree except at finitely many points. Thus $f(X) = X + 1$ and $g(X) = \frac{X^2-1}{X-1}$ represent the same element of $\mathbb{R}(X)$ although they are technically different functions since they have different domains.] Give $\mathbb{R}(X)$ an ordering (called the *Fréchet ordering*), as follows. If f and g are elements of $\mathbb{R}(X)$, then f and g can be thought of as functions on some interval (n, ∞), and there is an m such that either (1) $f(t) < g(t)$ for all $t > m$, (2) $f(t) = g(t)$ for all $t > m$ (i.e. $f = g$ in $\mathbb{R}(X)$), or (3) $f(t) > g(t)$ for all $t > m$. Set $f < g$ if case (1) holds. Alternatively, an f in $\mathbb{R}(X)$ is positive if, when it is written as a quotient of polynomials, the coefficients of the leading terms of the polynomials have the same sign, and $f < g$ if $g - f$ is positive in this sense. $\mathbb{R}(X)$ then becomes the desired ordered field, containing $\mathbb{R}$ as the "constant functions." The element H, where $H(X) = X$, is infinitely large, and $\partial = 1/H$ is an infinitesimal. The ring $\mathbb{Z}[X]$ of polynomials with integer coefficients sits naturally as a subring of $\mathbb{R}(X)$, and this subring would be a reasonable interpretation of the "integers" in $\mathbb{R}(X)$.

The problem with using this $\mathbb{R}(X)$ to do nonstandard analysis is that there is no transfer principle, which is the logical cornerstone of nonstandard analysis (and which was Robinson's main conceptual breakthrough). The authors do not seem to have any understanding or appreciation for the transfer principle, which among other things allows an (indirect) interpretation of the completeness property of the real numbers. They try to finesse completeness by asserting that expressions like ∂^π are defined, apparently via the BMI. But even making sense of b^π for an ordinary positive real number b requires use of limits and the completeness axiom, so it is unclear what ∂^π (much less something like $H^{H^{\cdot^{\cdot^{\cdot}}}}$, which they also claim exists) should mean.

The authors claim that their granular numbers are superior to the usual hyperreal numbers of nonstandard analysis (of which there are actually many variations, whose differences are mostly irrelevant for nonstandard analysis purposes so long as they can model the theory of real numbers via a transfer principle) since specific infinitesimal numbers such as ∂ can be explicitly named in the granular numbers. But this can also be done in the standard constructions of hyperreal numbers via ultrafilters. It is actually rather subtle to build a model for the hyperreal numbers (in which the completeness axiom has an interpretation via a transfer principle), and this is one of the main reasons why a rigorous theory of calculus via infinitesimals was not developed in the 19th century.

In fact, it is not easier to prove basic theorems of calculus from scratch using nonstandard analysis than it is using $\epsilon - \delta$ arguments, since for rigorous nonstandard analysis arguments a substantial amount of mathematical logic (e.g. the Łoś transfer principle) is required. This is the main reason nonstandard analysis has not proceeded very far in replacing the "traditional" approach used in calculus classes, not that "many mathematicians [are] wary of [infinitesimals] – so wary that they will not grant their existence." (p. 226) This may be an accurate description of 19th century mathematicians (including Cantor!); but I do not know of any modern mathematicians who do not fully accept nonstandard analysis as legitimate mathematics, except possibly for some constructivists who also reject substantial parts of "classical" analysis. (Whether analysts actually *choose* to use nonstandard analysis arguments in their work is a different question: since it can be shown that exactly the same theorems about analysis on $\mathbb{R}$ can be proved using nonstandard analysis as using traditional arguments, and there is even an algorithm of sorts for translating a proof in one form to a proof in the other, use of nonstandard analysis can be regarded as strictly a matter of taste. My own view is that nonstandard analysis provides an alternate point of view which can occasionally be useful in motivating a result or finding a proof. I generally think the more ways we have of looking at something, the better.)

Le trou normand. In this section, an example is discussed which is presented as a "paradox." At first glance, this discussion seems rather silly; but I came to realize that it is a good example of the type of early or incomplete intuition which mathematicians typically deal with (albeit on a much more sophisticated level) in the course of their research.

The essence of this example is a sequence of curves which "converges" to a limit curve, although the arc lengths of the curves do not converge to the arc length of the limiting curve. I doubt that any mathematician would regard this as a paradox, and not for the reasons discussed in the book. First of all, one must say what is meant by the limit of a sequence of curves. This poses no difficulty to mathematicians, although the authors do not seem to realize that standard mathematical notions of limit exist beyond limits of numbers. In this case, the curves in the sequence are graphs of functions, as is the limit curve, and the simplest way to phrase the limit is to say that the sequence of functions converges uniformly to the limit function. The "paradox" is then resolved by noting that if $f_n \to f$ uniformly, then it is generally not true that $f_n' \to f'$ in any sense (even if the derivatives all exist, which need not be the case). The problem is not, as suggested in the book, that the f_n are not differentiable everywhere; they could have used, for example, the functions $g_n(x) = 2^{-n} \sin(2^n x)$, which also converge uniformly to 0. The simplest example where arc length fails to converge under uniform limits is to approximate the diagonal of the unit square, which has length $\sqrt{2}$, by "staircase" curves which have arc length 2. In fact, with a little more reflection this disconnect should be expected. The absolute value of the derivative of a function says how *rapidly* the function is changing, not how *much*. A function can change rapidly, i.e. briefly have a large derivative, even if it is almost constant, if the changes are small enough. The derivative can even be large (in absolute value) most of the time, if it oscillates rapidly between positive and negative.

It is pretty clear from the text that the reason the authors regard this example as a paradox is faulty intuition about how arc length should behave under limits. One can see from the discussion how they could be fooled into expecting something unreasonable by looking at the situation from a point of view which turns out to be not a very good one. I, and probably most mathematicians, have experienced something similar in our own work: our early intuition on a problem or example is often not well-focused and therefore sometimes misleading. (Of course, when a result is proved which is contrary to our intuition, we know better than to consider it a paradox; we instead look to understand why our intuition was wrong.) I think we tend to learn through experience to recognize when our intuition is solid and when it is suspect, and we know to try other ways of looking at our problem to enhance or modify our intuition; but I, and many others, have still had the experience of publicly making conjectures which turned out to be false and even foolish in retrospect.

Ignoring Differences. The authors state on p. 251:

> "... *[I]gnoring certain differences is absolutely vital to mathematics!* This idea goes against the view of mathematics as the supreme exact science, that science where precision is absolute and differences, no matter how small, should never be ignored."

This statement was made in a specific context (calculus via infinitesimals) and, to be fair, we should not suggest that the authors intended it to be a universal statement about mathematics. But it must be pointed out that the unitalicized statement is really quite contrary to the nature of mathematics in general (and that the italicized statement is a broad principle of mathematics).

As good a definition of mathematics as any is that it is the study of pattern and form. In the practice of mathematics, we are continually identifying common patterns or forms in disparate objects and forming theories applicable to objects of the identified form, ignoring any other differences (regarded as "irrelevant" in this context) these objects may have. As a closely related idea, we are accustomed to consider equivalence relations and identify together equivalence classes. Thus we often identify two functions if they agree almost everywhere, or two isomorphic groups, regarding the objects as the "same" for present purposes.

This same process occurs in nature. A dog is able to immediately recognize a strange dog, even a strange breed of dog, as a dog, while also being able to recognize familiar dogs as individuals. On a much simpler level, a hydrogen atom can "recognize" another hydrogen atom, even one it has never encountered before, and perhaps bond with it; it would never try (excuse the anthropomorphic language) to do the same with a helium atom. In these cases, the recognition is made through detection of certain characteristic properties of the object recognized, while at the same time ignoring other "irrelevant" properties (this is obvious in the dog's case; in the other case, a hydrogen atom will react in essentially the same way with a deuterium atom as with another hydrogen atom).

The Continuum. Chapter 12 is one of the sections of the book which raises some real, thought-provoking issues on the conception of mathematics. This chapter impresses me as being on a more sophisticated level than the previous ones and (speaking as a nonexpert) appears to intelligently discuss issues of real cognitive importance. However, although I find little in this chapter to directly criticize, I do take issue with some of the premises and conclusions of the authors.

Descartes may have been the one to formalize the notion that points on a line can be identified with numbers, but the basic idea really goes back at least to Euclid. One of the essential features of Euclidean geometry (the name in Greek refers to "measuring the earth") is that any two points on a line, or in space, have a numerical distance between them, at least once a fixed unit distance has been specified (Euclid refers to "magnitudes" of lines [line segments] instead of numerical distances between points, but discusses equality, addition, subtraction, multiplication, and division of magnitudes, so his magnitudes correspond naturally to [constructible] nonnegative real numbers; indeed, the principal conceptual advance of Descartes' time over ancient Greece was the abstraction of the notion of "magnitude" or "quantity" into "number"). The only thing missing then, besides the abstract concept of number, is the idea of arbitrarily fixing one point on the line and calling it 0, and a unit distance and positive direction, which can be simultaneously specified by giving one additional point on the line to be called $+1$; once this is done, identifying all other points on the line with numbers, and vice versa, via distance and direction from 0, is almost immediate. (Identifying points in a plane or three-dimensional space with ordered pairs or triples of numbers is not quite so straightforward: not only must an origin and unit distance be chosen, but also a set of perpendicular axes and an orientation.)

It is unfortunate and misleading that the authors use the word "discretization" for the idea that points on a line can be identified with numbers or, more generally, that a line is a set of points. This term tends to reinforce what in my experience in teaching calculus and analysis is the biggest conceptual problem students have in understanding either the real numbers or geometry: the idea that for every point [number] there is a "next" point [number]. This misconception is tied in with the notion of infinity: slight variations are commonly heard references to the "last" term in a sequence or the terms in a convergent sequence being "eventually" equal to the limit. Even visualizing the rational numbers as an ordered set is difficult for students (neither the term "discrete" nor "continuous" can be accurately applied to this ordered set). I imagine experience with this type of misconception is the main reason mathematicians "wince" (p. 272) when asked whether points on a line touch.

The question of whether a line is composed of a set of points has a long history and was the subject of fierce debates during the sixteenth and seventeenth centuries, involving not only mathematicians but also philosophers and theologians and in particular the Jesuit order of the Catholic Church; see A. Alexander, *Infinitesimal: How a Dangerous Mathematical Theory Shaped the Modern World* for a detailed but flawed discussion.

As only partially recognized by the authors, there are several distinct notions of space, which must not be confused: intuitive space, mathematical space (of which there are many kinds, geometric or Euclidean space being only one of them), and physical space, which according to theories of modern physics may not be much like any of these: it may fail to be flat (relativity), three-dimensional (relativity/string theory), or continuous or homogeneous at small scales (quantum mechanics). Despite the misconceptions described in the previous paragraph, humans do indeed seem to regard intuitive space as being "continuous" in some sense, perhaps even more "continuous" than the real numbers.

Although geometric space can be pretty naturally coordinatized using numbers, the authors are correct in distinguishing geometric space from intuitive space, and the notion that intuitive space can be thought of as a set of points is rightly challenged. (Of course, this idea has been discussed since the days of Zeno; cf. the work of C. S. Peirce.) The authors do not quite put it like this, but one major conceptual problem is that we can have linearly ordered sets which are "larger" than $\mathbb{R}$. This problem can be dismissed as just another overly literal interpretation of mathematicians' use of the term "linear ordering" as a synonym for "total ordering." However, in this case I think such criticism would be off base, and that the authors have identified a real conceptual difficulty. I do indeed visualize linearly ordered sets as points on a line, even linearly ordered sets which cannot be embedded in $\mathbb{R}$ such as the granular numbers or the first uncountable ordinal, and I imagine many other mathematicians do likewise. This conception raises a serious set-theoretic question: if an intuitive line is so rich in points that any totally ordered set can be embedded in it, can we really regard a line as a set of points? There is an apparent contradiction: it is not difficult to construct totally ordered sets in which the intervals have arbitrary cardinality. In fact, Hausdorff concocted a totally ordered "set" (class) in which every interval contains a "subset" order-isomorphic to the class of all ordinals. (See, for example, [**?**, p. 88-89] for a description.)

The Equation $e^{\pi i} = -1$. The authors seem obsessed with this (admittedly remarkable) equation, and spend a lot of time discussing it. They assert: "Relatively few mathematics teachers understand [the equation $e^{\pi i} = -1$] even today" (p. 383). Earlier, on p. 7, they describe this as "one of the deepest equations in all of mathematics." Actually, modern mathematicians regard this equation as rather elementary; while someone of the level of mathematical sophistication of Lakoff or Nuñez may well regard it as deep and mysterious, it is in fact well understood by almost anyone with an undergraduate degree in mathematics. The simplest "explanation" of the formula is that it is an immediate consequence of the way the complex exponential function is *defined* (e^z for z a complex number has no intrinsic or intuitive meaning and must be defined). "Understanding" the formula then amounts to understanding why the standard definition of the complex exponential function is reasonable, and indeed forced by analytic and geometric considerations. This is explained in the first few days of every Complex Analysis course. In fact, the definition of e^x must be successively extended from the case of x a positive integer, the only case where it has a "natural" meaning (once the number e itself has been defined, a different story), to the cases where x is zero, a negative integer, a rational number, an irrational real number, and a complex number; it turns out that at each step there is a unique way to extend the definition preserving the usual rules of exponents and continuity and differentiability of the function, although none of the extensions are entirely obvious intuitively. (See J. Conway and R. Guy, *The Book of Numbers*, p. 254-256, for a simple geometric explanation of the formula $e^{\pi i} = -1$.)

Philosophy. The authors repeatedly profess their love of mathematics and their "deep respect and admiration" for mathematicians, calling some of them their "heroes," but one of the recurring themes of the book is a rather vicious attack on the predominant philosophies of mathematics, which are characterized as "myths" and the "Romance of Mathematics" whose adherents are ridiculed. The main belief they attack is the existence of "transcendent mathematics" outside the human intellect, which is roughly the essence of realism or Platonism. I personally find the arguments substantially groundless and sometimes offensive.

The authors' charge (p. 341) that mathematicians perpetuate the Romance of Mathematics "myth" and use language incomprehensible to the general public in order to impress nonmathematicians and feel superior is preposterous; in my experience, there is no group which is collectively less interested in impressing outsiders than mathematicians (if anything, the public perception of mathematics suffers precisely because mathematicians are so generally uninterested in what nonmathematicians think of their work.) Mathematical notation and terminology is entirely designed to facilitate communication between mathematicians, with no regard for anyone else. This attitude is nothing new – consider the following exchange between Thomas Hobbes and John Wallis in 1656 (Wallis was one of the leading mathematicians of the seventeenth century, and Hobbes was not):

> HOBBES: "[The page] is so covered over with the scab of symbols, that I had not the patience to examine whether it be well or ill demonstrated."
> WALLIS: "Is it not lawful for me to write Symbols, till you can understand them? Sir, they were not written for you to read, but for them that can."

The principal argument which supposedly justifies the authors' conclusions is that by definition, the human mind cannot understand or conceive of anything outside the realm of the human intellect, and thus it cannot be known whether there is any transcendent mathematics and in any event it cannot be part of the "embodied" mathematics of the human mind. OK, this is a fair argument (except, I think, for the last statement), if rather shallow philosophically, but to say this disproves the existence of transcendent mathematics is just as false as saying that Paul Cohen disproved the Continuum Hypothesis. It is also hardly new or revolutionary; it is a lightweight version of well-known philosophical theories going back at least to Kant. Consider, for example, the following quote from Henri Poincaré:[3]

> "A reality completely independent of the spirit that conceives it, sees it, or feels it, is an impossibility. A world so external as that, even if it existed, would be forever inaccessible to us."

Actually, the same argument shows something more: solipsism cannot be disproved. Recall that solipsism is the philosophical belief that nothing exists outside one's own mind; the world around us, the people we meet, and even our own physical bodies are merely figments of our imagination. A solipsist could very easily "prove" that solipsism is correct using the authors' arguments essentially verbatim.

Nonetheless, most people are not solipsists, and rejection of solipsism seems to me a lot different than belief in God, to which the authors rather dismissively

[3]H. Poincaré, *Mathematics and Science: Last Essays* (translated by J. W. Boldue), Dover Books, 1963.

equate Platonism. It is clear that the authors themselves are not solipsists (if they were, neither would be likely to acknowledge the other as coauthor!) so, for the rest of this discussion, let us assume that the physical world we perceive around us really exists.

Under this assumption, I think in fact there is rather overwhelming evidence that at least basic mathematical concepts have an "existence" outside the human mind. After purportedly rejecting existence of transcendent mathematics in the introduction, the authors begin Chapter 1 by describing how young babies can subitize (instantly recognize and distinguish) groups of two and three items. Whoa! What do they mean by "two" and "three"? If these concepts exist only in the human mind, then it isn't very surprising that human minds, even very young ones, can conceive them. But if they do not exist outside the human mind, by what mechanism can a human mind make a distinction between them? Do hydrogen atoms exist? Do helium atoms exist? Are they the same? If not, in what way do they differ? Are hydrogen atoms on earth the same as hydrogen atoms ten million light years away? Although there is a substantial philosophical question of what it means for an abstract concept to exist, I think it's pretty hard to argue that whatever the definition, the concepts "one" and "two" don't exist in the outside physical world. These concepts are really not essentially different from the concepts "hydrogen" and "helium", which are also abstract concepts grouping together objects all over the universe because of some common properties.

If "one" and "two" exist as transcendent concepts, then what about the rest of the natural numbers? Do they all exist, and if not, what is the first one which does not? (I think it could be legitimately argued that very large natural numbers, say, for example, prime numbers with more than $10^{10^{10^{10}}}$ decimal digits, do not "exist" in anything like the same sense that one and two do. But there may not be a clear transition in between: as numbers get larger, their "existence" may just gradually become more obscure; certain larger numbers with special properties that can be compactly described or denoted, such as $10^{10^{10^{10}}}$ itself, may be more "real" than random numbers of the same order of magnitude.) How far beyond the first few natural numbers do mathematical concepts have transcendental existence? There may be no way to know whether there is any limit to transcendent mathematics, or what the limit may be. But to argue that there is no such thing as transcendent mathematics at all seems very shaky (*if* the physical world exists).

What about truth value for mathematical statements such as Goldbach's Conjecture (every even number greater than 2 is a sum of two primes)? It is conceivable that there is a counterexample, but that the smallest counterexample is so large as to be far beyond the ability of the human mind to comprehend it (even if this is not true for Goldbach's Conjecture, it is very likely true for some other number theory statements which are simple enough to be formulated by humans). Couldn't it be said that the statement has a definite truth value, even though it may forever be beyond humans' ability to know what it is? This example seems much different than the Continuum Hypothesis (which, by the way, I do not regard as being incorrectly named), which is dependent on set-theoretic assumptions and constructions for its very formulation and thus need not have a definite truth value independent of (or even dependent on) its context.

Philosophers of mathematics have studied and argued about these notions for centuries on a much more sophisticated level than I have here. The question of mind-dependence or mind-independence of mathematics is a central one in the philosophy of mathematics, and is a much more subtle one than the authors realize; see, for example, [**?**, p. 2-4] for a brief survey which already has considerably more depth and substance than anything in the book under review. The authors' shallow discussion of philosophy makes no real scholarly contribution to the subject.

> "Philosophers, when they have possessed a thorough knowledge of mathematics, have been among those who have enriched the science with some of its best ideas. On the other hand, it must be said that, with hardly any exception, all the remarks on mathematics made by those philosophers who have possessed but a slight or hasty and late-acquired knowledge of it are entirely worthless, being either trivial or wrong. The fact is a curious one; since the ultimate ideas of mathematics seem, after all, to be very simple, almost childishly so, and to lie well within the province of philosophical thought. Probably their very simplicity is the cause of error; we are not used to think about such simple abstract things, and a long training is necessary to secure even a partial immunity from error as soon as we diverge from the beaten track of thought."
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> *Alfred North Whitehead*[4]

Cognition and Mathematics Education. Let me finish on a more positive note. We can all hope that the authors' ideas about how humans conceptualize mathematics, and future work in this direction, can ultimately help discover ways to make mathematics education more effective. Many of us have been concerned about the poor state of mathematics education as it exists today (and probably has always been), and we have tried to do something about it without notable success so far. Any help that cognitive scientists can give in suggesting better approaches to mathematics education would be extremely beneficial, and if the authors' work accomplishes nothing beyond helping to do this it will have been worthwhile despite its many mathematical and philosophical flaws.

Added Note. This review was written some years ago, before I read the various reviews and discussions available on the internet. Opinions in other reviews vary, of course, but many have commented on the mathematical inaccuracies. The authors have acknowledged some mathematical errors, and have disingenuously blamed most of them on the book editor. This is about as credible as Ayn Rand claiming that she was actually a communist, but that her editors surreptitiously put some lines in her books making her look like a right-winger. The copy of the book which I read was evidently the first printing, and I have not seen newer printings, but I am very skeptical of the authors' claim that the mathematical errors have been "corrected" in later printings; the mathematical errors in the copy I read were so pervasive that I estimate that to make the book mathematically credible, at least ten to twenty percent of the text would have to be substantially rewritten. I doubt that the authors would even be up to the task. Anyway, it's not an editor's job to fix the authors' mistakes, although these should be pointed out when noticed; authors are ultimately responsible for what their books say (didn't they read the proof sheets?) I had some serious disputes with a meddlesome copy editor of one

[4][**?**, p. 81-82].

of my books, and eventually prevailed (and I wouldn't have allowed the book to be published if I hadn't). We don't know who the editor of this book was, but, unlike my editor who was at least a mathematician, I suspect this editor knew no more mathematics than the authors, judging from the serious and elementary mistakes allowed through.

I do not know either author personally. I know very little about Nuñez at all. Lakoff is a well-known authority in his field, and I have read and appreciated some of his nonmathematical writings. He has also been active in politics, and he and I seem to have similar political views. He is clearly a brilliant and knowledgeable man within his areas of expertise. Unfortunately, these just do not include mathematics (or philosophy).

From the tone of this review I might be accused of elitism or intolerance of books about mathematics written by authors who are not members of the "club." While there are some mathematicians with such attitudes, the vast majority of mathematicians (I believe myself included) are quite unimpressed with an author's paper academic qualifications or lack thereof; in mathematics, probably more than in any other academic subject, a person's work speaks for itself. There are instances of persons with little or no formal mathematical training who produce work which is respected and embraced by mathematicians; perhaps the most famous example was Ramanujan. There are numerous instances over the years of leading research journals publishing papers written by undergraduates or authors unknown in the research community when these papers make a notable contribution to the subject. It is true that it is harder for "outsiders" to write intelligently about mathematics than other academic subjects since the world of mathematics is rather insular. But I have read many worthwhile books and articles on mathematics written by nonmathematicians, and I am not dismissive of them simply because they have occasional mathematical errors (so do a lot of mathematics books written by mathematicians!); many are valuable to me even when I disagree with some of the assertions or conclusions. I would even put this book on such a list, although frankly near the bottom.

On the other hand, the opposite side of the coin is also accurate: most mathematicians have a highly developed bull**** detector, and do not suffer fools gladly. This standard is not just applied to outsiders: as a former journal editor, I have read a number of scathing referee's reports about manuscripts written by authors with solid and even eminent credentials in mathematics (I have occasionally written such reports myself, although they are definitely the exception).

My attitudes about this book are probably similar to the reaction the authors would have if I tried to write a book about psychology. We should all try not to be ultracrepidarians.

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