Mathematical Intuition vs. Mathematical Monsters

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Logic sometimes breeds monsters. --Henri Poincaré

DELTA: But there isn't a theorem in the world which
couldn't be falsified by monsters. --Imre Lakatos

Abstract:
Geometrical and physical intuition, both untutored and cultivated, is ubiquitous in the research, teaching, and development of mathematics. A number of mathematical "monsters", or pathological objects, have been produced which—according to some mathematicians—seriously challenge the reliability of intuition. We examine several famous geometrical, topological and set-theoretical examples of such monsters in order to see to what extent, if at all, intuition is undermined in its everyday roles.

1. Varieties of mathematical intuition. The philosophical literature on mathematical intuition rightly concentrates on the question whether there is any sort of direct intuition of basic mathematical objects, structures and propositions, and, if so, to what extent that constitutes a foundation for mathematical knowledge.[1] My intention here is, rather, to direct attention to the ubiquitous employment of mathematical intuition at a more everyday level in research, teaching and the development of mathematics, and to ask to what extent challenges to the reliability of intuition undermine its uses in these roles. As it turns out, this will eventually return us to the more fundamental philosophical questions.

The word *intuition* as used by mathematicians has a variety of meanings, only a few of which will be touched on here.[2] One sense is the common "Ah, hah!" *Erlebnis* of a flash of insight or illumination on the road to the solution of a problem. The classic account by Henri Poincaré in his article "Mathematical discovery"[3] contains both anecdotal material and an analysis of this role of intuition in the creative process. Both were

* This is an expanded version of a paper presented by invitation to the Twentieth World Congress of Philosophy, Boston, MA, August 8-11, 1998, as part of a panel on Mathematical Intuition; my co-panelists were Charles Parsons and Mark Steiner. I wish to thank the following for their helpful comments on a draft of this paper: Michael

Beeson, Martin Davis, Matthew Foreman, Torkel Franzen, Reuben Hersh, Charles Parsons, Stephen G. Simpson, Robert Solovay, and Robert Tragesser.

elaborated by Jacques Hadamard in his well-known book, *The Psychology of Invention in the Mathematical Field*[4] where the sequence leading to discovery is described as consisting, to begin with, of conscious preparatory engagement with the problem, followed by a period of gestation; next comes the crucial juncture of illumination, and the process is rounded out with rigorous verification. Less vivid than the Poincaré-Hadamard account, but equally common, are the mathematicians' *hunches* as to what problems it would be profitable to attack, what results are to be expected, and what methods are likely to work. How much of this is peculiar to creative research in mathematics as opposed to other areas of science is another matter.

The meanings I wish, rather, to emphasize here, are those falling under the heading of *geometrical and physical intuition*. I would question whether there is such a thing as innate, "raw", untutored intuition of these or indeed of any kind. In any case, it is clear that our intuitions can be cultivated through training and practice. These may accord with tacit knowledge gained through experience, but, equally, one may gain intuitions that help one maneuver through subject matter that is initially highly non-intuitive. Moreover, intuitive knowledge or understanding is not simply separated from that obtained by more or less systematic reasoning—the two frequently go hand in hand, and neither is dispensable in practice.

In the teaching of mathematics, both geometrical and physical intuition are constantly called upon at all levels for motivation of notions and results, and even in some cases for proofs. As examples of the latter, no proof of Pythagoras' theorem can be more directly convincing than those involving dissection and rearrangement of figures, in some cases in combination with some elementary algebra. Given the geometrical and physical applications of the calculus, it is not surprising that the corresponding intuitions should be called on regularly in the teaching of that subject. But those same intuitions, suitably cultivated and extended, serve to carry one confidently into the study of analysis in higher dimensional spaces and then on into functional analysis. There too, as in linear algebra, geometrical intuition is frequently appealed to in the use of notions of vector addition, length, angle, projection, etc. And the near universal appearance of analogues of Pythagoras' theorem in analysis and higher geometry is a linchpin in the extension of one's intuition from familiar ground to the most diverse settings.

Topology serves to cultivate its own distinctive intuitions as rubber sheet geometry. Closed orientable surfaces in three dimensions provide a playing ground where one can adapt those intuitions to the notions and techniques of combinatorial topology in order to deal with less visualizable manifolds. And, as a final pedagogical example, a good current course in axiomatic set theory will start with the intuitive conception of the cumulative hierarchy and appeal to it to justify the Zermelo-Fraenkel axioms and various plausible extensions. Moreover, one returns to that in modified forms in the constructible and relative constructible hierarchies employed in various consistency and independence results.

Such examples can be multiplied a thousand-fold. The point here is not to enumerate them, but rather to recognize the ubiquity of intuition in the common experience of teaching and learning mathematics, and the reasons for that: it is essential for motivation of notions and results and to guide one's conceptions via tacit or explicit analogies in the transfer from familiar grounds to unfamiliar terrain. In sum, no less than the absorption of the techniques of systematic, rigorous, logically developed mathematics, *intuition is necessary for the understanding of mathematics.*[5] Historically, and for the same reasons, it also played an essential role in the development of mathematics. The precise mathematical expression of various parts of our perceptual experience is mediated to begin with by intuitive concepts of point, line, curve, angle, tangent, length, area, volume, etc. These are not uniquely determined in some Platonic heaven. Mathematics models these concepts in more or less rigorous terms (sufficient unto the day), and then interweaves them to form more elaborate models or theories of physical experience as well as purely mathematical theories. The adequacy of explication of the basic concepts can only be tested holistically by the degree to which these theories are successful.[6]

2. Geometrical and topological monsters. I don't know who first applied the word "monster" to examples of counter-intuitive "pathological" functions and figures of the sort that began to emerge in the latter part of the nineteenth century. My earliest source is the following from one of Henri Poincaré's essays dating from 1906, "Mathematical definitions and education"[7]:

> Logic sometimes breeds monsters. For half a century there has been springing up a host of weird functions, which seem to strive to have as little resemblance as possible to honest functions that are of some use. No more continuity, or else continuity but no derivatives, etc. ... Formerly, when a new function was invented, it was in view of some practical end. Today they are invented on purpose to show our ancestors' reasonings at fault, and we shall never get anything more out of them.

The appearance of monsters was a direct result of the nineteenth century program for the rigorous foundation of analysis and its arithmetization, i.e. for the triumph of number over geometry, at the hands most notably of Bolzano, Cauchy, Weierstrass, Dedekind and Cantor. That program grew in response to the increasing uncertainty as to what it was legitimate to do and say in mathematics, and especially in analysis. One could no longer rely on calculations that *looked* right, or depend on physical applications to justify the mathematics. The completed program of arithmetization substituted the real number system for the measurement line and "ε, δ" definitions and proofs for limit concepts and arguments. The central notions which then emerged for functions were those of continuity and differentiability (both at a point or in a region) and integrability. In those terms, the notion of a curve in n dimensions was defined simply as a continuous map f on a closed interval $[a,b]$ to n-dimensional space $\mathbf{R}^n$, and the tangent to such a curve at a point was then defined in terms of the derivatives of the components of f, when those exist. Use of these precise explications sufficed to verify rigorously many of the intuitively evident properties of continuous functions and curves in the prior informal sense, e.g. that a continuous f from $[a,b]$ to the real numbers $\mathbf{R}$ takes on a maximum and minimum on that interval, and that for differentiable f, such extrema can be located among the points where the tangent to its curve is horizontal. Of course, it was familiar and expected that reasonable functions could have isolated points of discontinuity and that a continuous function could have isolated points where there is no tangent to its graph. It was thus a surprise when Weierstrass produced an example of a function which is everywhere continuous and nowhere differentiable. Then Peano produced an example of a space-filling curve, i.e. a continuous function from the closed interval $[0,1]$ to $\mathbf{R}^2$ whose range is the unit square $[0,1] \times [0,1]$, thus violating the intuition that a curve is a one-dimensional object. Moreover, there is no reasonable assignment of length as a measure to Peano's curve. It was to such objects that Poincaré was reacting as "monsters".

By contrast to Poincaré, the mathematician Hans Hahn (one of the principals in the Vienna Circle and the teacher of Kurt Gödel) argued against intuition in mathematics in a famous 1933 essay, "The crisis in intuition".[8] Asserting its complete unreliability, he made use of a number of mathematical monsters to support his critique. Hahn's main target was the Kantian view of space as one of the forms of pure intuition.[9] Besides presenting simplified examples of a continuous curve without a tangent at any point and of a space-filling curve (in a form due to Hilbert), Hahn also described examples challenging intuitive topological concepts. One, due to Brouwer, is that of a map of three "countries" which meet each other at every point of their boundaries. Another, due to

Sierpinski, produces a curve which intersects itself at every point. Typically, these objects are constructed as limits of reasonably well-behaved functions. For example, the Peano-Hilbert space-filling "curve" is a limit of curves that first go through every quadrant of the unit square, then more quickly through every sub-quadrant, and so. The Sierpinski "curve" is obtained by successively deleting the interior of an inscribed equilateral triangle within an initial such triangle; it is the skeleton of what's left in the limit.[10]

Hahn draws the following conclusion from such examples in his essay:

> Because intuition turned out to be deceptive in so many instances, and because propositions that had been accounted true by intuition were repeatedly proved false by logic, mathematicians became more and more sceptical of the validity of intuition. They learned that it is unsafe to accept any mathematical proposition, much less to base any mathematical discipline on intuitive convictions. Thus a demand arose for the expulsion of intuition from mathematical reasoning, and for the complete formalization of mathematics. That is to say, every new mathematical concept was to be introduced through a purely logical definition; every mathematical proof was to be carried through by strictly logical means... The task of completely formalizing mathematics, of reducing it entirely to logic, was arduous and difficult; it meant nothing less than a reform in root and branch. Propositions that had formerly been accepted as intuitively evident had to be painstakingly proved.[11]

As to this last, Hahn cited the example of the Jordan curve theorem, according to which every simple closed curve in the plane is the boundary of two open connected regions, one (the "interior") being bounded, the other (the "exterior") unbounded. It had been pointed out by Camille Jordan that it is necessary to formulate explicitly this bit of tacit intuitively obvious knowledge for the proper development of complex analysis, but it turned out to be devilishly difficult to prove even for reasonably well-behaved simple closed curves, namely those with polygonal boundary; after several faulty attempts by Jordan and others, it was finally proved in general for continuous boundaries in 1905 by Oswald Veblen. Note, however, that the problem with intuition in this case was not due to a challenge by a monster, but rather the apparent necessity to use complicated rigorous methods even for intuitively simple results.

Another topological monster which Hahn could have cited, but didn't, is the so-called Alexander "horned" sphere (devised by James W. Alexander in 1924), which relates to higher-dimensional versions of the Jordan curve theorem. Two subsets X and Y of a topological space are said to be homeomorphic if there is a one-to-one continous map from X to Y whose inverse is also continuous; this seems to explicate the intuitive notion of rubber sheet equivalence. The Alexander horned sphere is a subset S^* of $\mathbf{R}^3$ which is homeomorphic to the unit sphere S^2 in $\mathbf{R}^3$. S^* is formed by successively growing pairs of "horns" from S^2 which are almost interlocked and whose end points approach each other; the first steps in its construction can be visualized by posing the thumb and forefinger of each hand toward those of the other hand as if one is going to interlock them, then growing a smaller thumb and forefinger on the end of each of these, etc.; S^* is what is obtained in the limit. While S^2 is homeomorphic to S^*, its ambient space is not: there is *no* homeomorphism of $\mathbf{R}^3$ with itself which takes S^2 to S^*, and this is because the complement of S^*, unlike that of S^2, is not simply connected.[12]

Without in the least bit denying the necessity of developing mathematics—in particular analysis and topology—in a rigorous manner, evidently (in view of my remarks in section 1) I disagree with those who, like Hahn and others, believe that intuition has no value and that it must be expelled from mathematics. What, then, is one to say about the geometrical and topological monsters that are supposed to demonstrate the unreliability of intuition? The answer is simply that these serve as counterexamples to intuitively expected results when certain notions are used as explications which serve various purposes well enough but which do not have all expected properties. Unless one thinks that curves, for example, are laid up in a Platonic heaven as continuous functions from an interval to $\mathbf{R}^n$, the arithmetized notion of curve must be treated as a model of an intuitive concept which itself isolates and describes in an idealized form certain aspects of experience. An explication that is closer to most ordinary experience requires of a curve that it is at least piece-wise differentiable. That less-stringent definitions of this notion may be valuable in modelling unusual parts of experience such as Brownian motion or fractal geometry (see ftn. 10) is not thereby denied; no one explication need be assumed to fit its purpose in all theories. Similarly, while the use of homeomorphism as the mathematical definition of the conceptual rubber sheet stretching of a sphere and other familiar surfaces (such as tori, etc.) serves to verify many expected properties (e.g. forms of connectedness, "hole"-iness, etc.), it does not model fully the informal concept. Thus one does not meet the kind of pathology represented by the Alexander horned sphere in the restriction to differentiable manifolds and diffeomorphisms between them. Of course, special applications of topology in scientific modeling may require more delicate distinctions, as, for example, René Thom's "catastrophe" theory required a central focus on singularities of differentiable mappings.[13]

Though it is understandable for the time, given the continuing deep influence of Kant's views through the work of the neo-Kantians in philosophy, it seems to me that Hahn's focus on the Kantian account of geometric intuition is misplaced so far as mathematics is concerned, and that the examples brought forward against the unreliability of intuition serve a quite different and more general purpose. Namely, it is standard mathematical practice to seek best possible results of an expected kind, and one way to achieve such is to make weakest possible assumptions on the given data. In this respect the mathematical monsters serve simply to provide counter-examples to further possible improvements.

3. Paradoxical decompositions of sets. The internal process of the foundations of mathematics which had been dominated by the arithmetization program in the nineteenth century was transformed via Cantor's revolutionary *Mengenlehre* into the set-theoretization program dominating twentieth century foundations. Several monsters established by set-theoretical methods, the so-called Hausdorff Paradox and the Banach-Tarski Paradox, emerged early in the first quarter of the new century in connection with a problem of both geometrical and analytical character. For the basic technical information in the following I have drawn heavily on the excellent comprehensive expository work, *The Banach-Tarski Paradox*, by Stan Wagon.[14] The background is this: a continuing concern for analysis at the turn of the century was the need for a satisfactory suitable general theory of integration in one or more variables, for example to deal rigorously with Fourier series in sufficient generality. From the beginning, formal manipulations with these series *looked right*, but aroused serious concerns over their justification, especially as they led to fairly discontinuous functions, and because the relation between trigonometric series representing a function on a subset of its domain and the function itself was rather complicated.[15] Now, one of the traditional applications of integration was calculation of areas and volumes, and it in turn could be conceived geometrically in terms of such. Extended to higher-dimensional spaces, the problem of integration was thus subsumed under the problem of assigning to rather general subsets A of n-dimensional space $\mathbf{R}^n$ a *measure* $m(A)$ as "volume", to satisfy the following minimal intuitive requirements, where $m(A)$ is always a non-negative real number:

(i) m is *finitely additive,* i.e. if A and B are disjoint then $m(A \cup B) = m(A) + m(B)$,
(ii) m is *isometry-invariant*, i.e. if A is transformed into A' by a rigid motion then $m(A) = m(A')$, and
(iii) *m normalizes the unit "cube" C in $\mathbf{R}^n$,* i.e. $m(C) = 1$.

The least class of sets on which such m can be defined consists of all finite disjoint unions of isometric images of the unit cube. But one can clearly improve that to encompass all cubical subdivisions of the unit cube as well. Efforts were made by Jordan and others to extend the domain of such m far beyond that, so as to include the kinds of sets needed for the analytic applications of integration. It turned out that an adequate theory for the latter required *countable additivity* in place of finite additivity of m, and the most satisfactory definition of measure which incorporated that was first obtained by Henri Lebesgue in 1902. This yields a class of sets called *measurable*, which includes n-dimensional cubes of all sizes, normalizes the unit cube, is isometry-invariant, and is closed under countable unions and complements. In the case $n = 1$, the class of measurable sets includes all intervals $[a, b]$ (with standard length $(b - a)$ as measure), is closed under countable unions and complements, and is *translation-invariant.* Lebesgue raised the question whether there is a translation-invariant countably additive extension of the standard measure of intervals which is defined on all subsets of $\mathbf{R}$. This was answered in the negative by Giuseppe Vitali in 1905, i.e. he proved the existence of sets which are not measurable in the sense of Lebesgue; Vitali's proof made essential use of the Axiom of Choice (AC) in order to choose one element out of each member of an uncountable collection of non-empty sets. The same negative result applies also in all higher dimensions.

Following this work, it was natural then to return to the minimal requirements (i)-(iii) and ask whether there is a function m defined on *all* subsets of $\mathbf{R}^n$ which satisfies those requirements. In 1914, Felix Hausdorff succeeded in answering this question in the negative in all dimensions n greater than 2, while leaving it open for dimensions 1 and 2. His result, known as *Hausdorff's Paradox*, makes use of the following notion: two sets A and B in $\mathbf{R}^n$ are said to be *equivalent by finite decomposition* if A can be decomposed into a finite number of pieces which can be transformed by rigid motions and reassembled to form B, i.e. if there exist $A_1,..., A_k$ and $B_1,..., B_k$ such that A is the union of the A_i, B is the union of the B_i and each A_i is isometric to B_i ($i = 1,...,k$). Then a set A is said to be *paradoxical* if it is equivalent by finite decomposition to the union of two disjoint copies A' and A'' of itself. The appellation is appropriate; it does not seem intuitively possible that there are non-empty sets which are paradoxical in this sense. In fact it can be shown that *there is no paradoxical subset of* $\mathbf{R}^1$. Yet, what Hausdorff proved (by an ingenious extension of Vitali's argument, again using AC) is that: *there is a denumerable subset D of the unit sphere S^2 in $\mathbf{R}^3$ such that the set $A = S^2 - D$ is paradoxical.* This could then be used to show that there is no finitely additive, isometry-invariant measure on all subsets of $\mathbf{R}^n$ for $n \geq 3$ which normalizes the unit cube. Building on Hausdorff's method, in 1924 Alfred Tarski and Stefan Banach proved the even more surprising result that: *for each* $n \geq 3$ *the unit ball in* $\mathbf{R}^n$ *is paradoxical.* Moreover, they proved that *any two bounded subsets of* $\mathbf{R}^n$ *each with non-empty interior are equivalent by finite decomposition.* Stated colloquially, according to the Banach-Tarski (B-T) paradox, one can "cut up a pea into finitely many pieces and rearrange them to form a ball the size of the sun!" The negative measure-theoretic consequence of the Hausdorff Paradox is immediate from this. That left open the question for dimensions one and two; it was not until 1929 that Tarski was able to show (again using AC) that the previous negative result was best possible: there *is* a finitely additive, isometry-invariant measure which normalizes the unit interval, resp. square.

There has been much discussion of the significance of the B-T Paradox centering on the role of AC, the Axiom of Choice. A few years after Paul Cohen invented the method of forcing to establish the independence of AC from the axioms ZF of the Zermelo-Fraenkel system of set theory, Robert Solovay proved a major metatheorem, whose consequence is that if we replace AC by the so-called Axiom of Dependent Choices DC then *the B-T Paradox is not a theorem of* ZF + DC, granted the consistency of ZF.[16] Moreover, DC serves to carry out all "positive" uses of the Axiom of Choice in the part of real analysis that requires measure theory. Thus the counter-intuitive consequences of B-T can simply be avoided by giving up full AC in favor of DC, without any genuine mathematical sacrifice.

But in this respect the situation of intuition vis à vis the monster contains one new aspect which is rather different from the geometrical and topological cases discussed in the preceding section. It was not a question there of *how* the monstrous examples are established but rather *of what* they are examples. Granted the precise definitions of continuous function, curve, etc. used in them, there is no dispute as to the proofs that a specific continuous function is nowhere differentiable or that a curve is space-filling, etc. Here, rather, the proof is put in question (at least by some), and it is the necessary use of AC that makes it questionable; so, the monster is "avoided" by simply barring the use of AC from proofs. But one can reasonably take the opposite position that the monster has not ceased to exist thereby, only that one has somehow hidden from it in this way. After many years of controversy over the role of AC in various parts of mathematics,[17] it is fair to say that most mathematicians accept it in practice as either obvious or an unavoidable necessity. If they have qualms about its use, they may be further comforted by Gödel's relative consistency result that ZF + AC is consistent if ZF is consistent. A much stronger position is taken by working set-theorists, who recognize AC (along with all the axioms of ZF) as an evident truth about sets. Indeed one's intuitions about "arbitrary" sets would seem to make AC obvious, since such sets are supposed to exist independently of how one constructs or describes them. So, according to this view, the monster is real and needs to be embraced (or at least allowed to sleep in another room of the house) regardless of its unusual nature. What one has here, then, is a conflict of two quite different intuitions. From the common-sense point of view of physical-geometric intuition, the B-T paradox is a patent impossibility, while from that of set-theoretical intuition, it is just another theorem, albeit a surprising one. It is exactly the latter that was expressed by Banach and Tarski in their 1924 article, in which the B-T theorem is granted as seeming "perhaps" paradoxical; but then they point out the apparent necessity of AC for results which "agree with intuition".

More recent results of Randall Dougherty and Matthew Foreman may require some reconsideration of the role of AC in paradoxical decompositions.[18] One of the theorems they prove without the use of AC is: *If A and B are any two bounded non-empty open subsets of* $\mathbf{R}^n$ *where* $n \geq 3$, *then there is a finite pairwise disjoint collection of open subsets of A whose union is dense in A which can be rearranged isometrically to form a pairwise disjoint collection of open subsets of B whose union is dense in B*. Not only does the proof not make use of AC, it is completely constructive in the data. However, the conflict with physical-geometrical intuition here is not as blatant as with the B-T paradox, involving as this does the technical notions of open set and of one set being dense in another. A set A is open if each point of A is contained in an open ball (a kind of bubble) included in A. This seems clear enough, but when one sees examples of "wild" open sets, even though explicitly described, intuition of the possibilities of what can fall under the general concept of openness fails. Similarly for the notion of one set being dense in another; the closure of an open set can add a set of points which is impossible to visualize.

Must intuition run for cover in the face of the various results on paradoxical decomposition described here? If these are monsters such as those we discussed in the preceding section, then the answer, as there, would be a simple: No. The view there was that the geometrical and topological monsters of the sort cited by Hahn simply serve as counterexamples to intuitively expected results when certain precise notions are used as explications of intuitive concepts, notions that work well enough in various situations but—as one sees from the counterexamples—not all. To what expected result would the Banach-Tarski paradox be a counter-example? If anything, it is the *negation* of their theorem: there is *no* way to cut up the unit ball in $\mathbf{R}^n$ (no matter what n) into a finite number of pieces and rearrange them so as to produce two copies of the same. Like the

Jordan curve theorem, that's hardly a result one would expect to state unless forced to consider it for other reasons. But suppose we did think to state it. Then the main informal concept that needs to be examined is that of a "piece" of a geometrical object, and secondarily how we are to get at that piece by "cutting up" the object. It is interesting here to compare the situation with a result in the plane concerning these notions which *does* accord with intuition. That is the theorem of Bolyai-Gerwien (around 1832): *Two polygons are congruent by dissection if and only if they have the same area*.[19] The notion of congruence by dissection is similar to that of equivalence by decomposition, but is on the one hand more restrictive and on the other hand more liberal. The restriction is that the pieces used must themselves be polygons. The liberalization is that we can ignore boundaries when re-fitting pieces together. So, here, taking the notion of "piece" to be a polygon is very intuitive, but it is also somewhat arbitrary. The arbitrariness can be seen by going up one dimension. It is a result of Dehn (in answer to the third problem on Hilbert's famous list of twenty-three) that a regular tetrahedron is not congruent by dissection to a cube using polyhedral pieces. And, in fact, all known computations of the volume of a tetrahedron require a limiting process of one kind or another. So there are no natural geometrically specified "pieces" to obtain such volumes by finite dissection procedures. But that doesn't conflict with intuition, since there is no intuitive expectation that we could do that in the first place.

This returns us to the question: What is a reasonable explication of the notion of "piece" of a geometric object, such that no ball in $\mathbf{R}^3$ is paradoxical for pieces of that kind? There does not seem to be any unique candidate for this on offer, but one would expect that a piece is not *any* set of points but rather one with a well-defined volume or, more generally, measure satisfying the three basic requirements. However, this is not an *intrinsic* explication of what constitutes a piece of a geometrical object. But let us suppose one has such an explication for which the Banach-Tarski paradox shows that it gives reasonably "best possible" results, and thus that we can assimilate *this* monster to the kind of geometrical and topological monsters discussed in the preceding section. Even if so, I think there is still something *very* disturbing about the Banach-Tarski paradox that separates it in character from those examples. Simply put, the conflict between common-sense geometrical intuition and the Banach-Tarski paradox seems so egregious that it may force one to question the very basic intuitions about arbitrary sets which lead one to accept the principles lying behind the paradox, namely the principles of Zermelo-Fraenkel Set Theory together with the Axiom of Choice—or, if not that, then at least the relevance of those principles to applicable mathematics.

If common-sense and set-theoretical intuitions are in actual conflict, then one or the other must be rejected (but see the Appendix below). Few would argue for the rejection of the set-theoretical position, on the grounds that it is the best current foundation of mathematics we have and it thereby accounts in a systematic and coherent way for all the mathematics that is used in physical applications. The supporter of set theory may argue that even though non-measurable sets don't actually arise in such applications it is not reasonable to exclude AC just on that account, since its manifold uses otherwise to obtain results in accord with everyday mathematical intuition justify it pragmatically.[20] This way of defending set theory, including AC, is a version of the Quine-Putnam indispensability arguments. Against that, I have made the case that all, or almost all, of scientifically applicable mathematics can be formalized in a system W conservative over Peano Arithmetic and thus do not require the assumption of any essentially set-theoretical notions and principles at all.[21] The cases of applications that are not at present covered involve highly speculative models in quantum theory. So one can come down on the side of common-sense intuition in a full rejection of set theory, while saving the mathematics needed for scientific applications. No doubt, the silent majority will not opt for either extreme, but will continue to accept, at least tacitly, the set-theoretical way of thinking in everyday mathematics while ignoring its bizarre consequences.

4. Conclusion. In my original plans for the lecture for which this paper was prepared I had planned to include one further topic for discussion, namely the work on higher axioms of infinity in set theory, some of which may be considered to be monstrously large, at any rate beyond the intuitions (such as they are) for "Cantor's Absolute". Because of the limitations of time, it became clear that I would have to content myself with the main points in sections 1-3 and not get into that topic at all. In any case, the issues involved are still different from those in sections 2 and 3, and require extensive discussion; I have already dealt with them in part in a recent paper[22], but the discussion in relation to intuition deserves to be carried further.

To conclude, I return to the question raised in the introduction: to what extent do the challenges raised by monsters to the reliability of intuition undermine its uses in its everyday roles in research, teaching and the development of mathematics? I have argued that intuition is essential for all of these, but that intuition is not enough. In the end, to be sure, everything must be defined carefully and statements must be proved. And one service that the monsters lurking around the corners provide is forcing us to don such armor for our own protection. But if the proofs themselves produce such monsters, then the significance of *what* is proved requires closer attention, and that has to be dealt with on a case-by-case basis.

Appendix for proof-theorists.

Other than in its use of AC, the proof of the B-T theorem actually makes very little use of ZF principles, and is quite predicative in that respect. This raises some interesting proof-theoretical questions, which I formulate using the classical system Z^{ω} of finite type over arithmetic and its restricted versions using predicative primitive recursion and restricted induction in my article "Theories of finite type of related to mathematical practice" (1977).[23] For the application of AC in the proof of the existence of non-measurable sets and in the Banach-Tarski theorem, one only has to make a selection from equivalence classes of a definable equivalence relation between real numbers. Logically speaking, this comes down to considering arithmetical formulas E(a,b), where a, b range over *sets of natural numbers*, satisfying the formula Equiv(E) which expresses that E is a partial equivalence relation. Then the required special case of AC is:

$$AC_E \qquad \mathrm{Equiv}(E) \rightarrow (\exists\, f)\, (\forall a,b)\, [\, E(a,b) \rightarrow E(a,fa) \wedge fa = fb\,].$$

Here f is a function variable of type 2, mapping sets of natural numbers to sets of natural numbers. As in my article op. cit., I use μ for the non-constructive (unbounded) minimum operator, which allows us to eliminate arithmetical quantifiers, and (μ) for the axiom expressing that it is a Skolem operator in this sense. In the following, I presume one may verify that the B-T theorem is provable in $Z^{\omega} + (\mu) + AC_E$ for suitable arithmetical (or equivalently, quantifier-free, E), and already in its third-order part; however, I have not gone through the details.

Questions.

Q1. Is the system $Z^{\omega} + (\mu) + AC_E$ conservative over its third order part? Same for the restricted version. (I conjecture both are the case.)

Q2. What are the proof-theoretical strengths of the systems indicated in Q1? (I conjecture that these systems are predicative, and indeed that the restricted system is conservative over PA.)

Q3. In which of the systems of Q1 is the existence of Lebesgue non-measurable sets provable? Same question for the Hausdorff and Banach-Tarski paradoxes. (This is simply a question whether anything more than restricted primitive recursion and restricted induction is needed.)

So what? If the systems needed to prove these "monsters" turn out to be predicative in strength, as I conjecture, that may support an accomodation between common-sense intuition (with which predicative systems may be considered to be in accord) and that part of set-theoretical intuition needed to produce them, instead of a conflict between them. But, let's see.

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Notes

[1] An extensive part of that literature is devoted to Kant's ideas about the intuition of space and time, ideas which will not concern me here. In this century, claims for certain basic intuitions have been advanced primarily by some of the mathematicians concerned with the foundations of mathematics, and in this respect they have concentrated on the structure of the natural numbers (e.g., Poincaré, Brouwer, Hilbert), the continuum (e.g, Poincaré, Brouwer and Weyl), and the cumulative hierarchy of sets (e.g. Gödel). There is much interesting contemporary logico-philosophical literature dealing with these issues. I mention only a few sources, beginning with a number of articles by Charles Parsons, first of all his essays on Kant's philosophy of arithmetic and on the iterative concept of set in his collection of essays (1983); see also the articles Parsons 1979, 1994 and 1995. On the iterative concept of set see, further, Boolos 1971. Tieszen 1989 relates Husserl and Gödel, while Folina 1992 provides a valuable study of Poincaré's thoughts on the intuition of the natural numbers and of the continuum.

[2] One interesting suggested list of meanings, useful as a point of departure, is provided in Davis and Hersh 1981, pp. 391-399; see also *ibid.* pp. 301-316. A systematic effort to analyze the psychology of mathematical intuition is provided by Fischbein 1987. See also the pioneering work, Westcott 1968. (I am indebted to Reuben Hersh for bringing my attention to these and a number of other works on intuition in mathematics.)

[3] From Poincaré 1952, pp. 47-63.

[4] Hadamard 1949.

[5] This has been emphasized particularly by William P. Thurston in his interesting article, "On proof and progress in mathematics" (1994), written as part of a discussion by a number of leading mathematicians of the earlier provocative article by Jaffe and Quinn 1993.

[6] For more extended accounts of the sources of mathematics in everyday experience and the mathematical modelling of experience, see MacLane 1986, especially pp. 34-36 and 415-417, and Lakoff and Nunez 1997.

[7] Poincaré 1952, p. 125. Poincaré was, however, anticipated in this usage long before that by Leibniz, in his "Critical thoughts on the general part of the principles of Descartes" as it appears in the edition Leibniz 1958. One finds there on p. 1179 two diagrams, one labelled "According to Descartes: a monstrous figure", while the other is labelled "According to the truth--an orderly figure." The context is Leibniz' discussion, op. cit. p. 665, of Descartes' rules for the geometry of motion, where he writes: "Descartes acknowledges that it is difficult to use his rules because he sees that they conflict with experience. But in the true rules of motion there is a remarkable agreeement between reason and experience. ... From the Cartesian rules no continuous line whatever can be derived [in the case considered] for the results which correspond to the continous line representing the variable data; on the contrary, a figure is produced which is most erratic and

contrary to our law of continuity." (I am indebted to Robert Tragesser for bringing this passage to my attention.)
The word "monster" as applied to pathological counterexamples in mathematics was popularized in more recent years through the work of Lakatos 1976.

[8] Hahn 1933.

[9] But Hahn remarks in an aside that Kant's thesis that arithmetic rests as well on pure intuition has been successfully opposed by Russell's execution of the logicist program!

[10] This and the Sierpinski "sponge" (not cited by Hahn), which date from c. 1915, anticipate as examples the notion of fractional dimension; cf. Mandelbrot 1983, pp. 142-144.

[11] Hahn 1933, p. 93.

[12] Cf.Hocking and Young 1961, pp. 175-176.

[13] See Thom 1975 and Zeeman 1977.

[14] Wagon 1985; see that book for references to most of the results cited in this section.

[15] Incidentally, Cantor's own work began with the question as to how complicated could a set of points of an interval be, outside of which any representation of a function by a trigonometric series must be unique.

[16] Solovay 1970.

[17] Detailed at length in Moore 1982.

[18] Dougherty and Foreman 1994. Note that first announcement of this work, in 1992, appeared seven years after the publication of Wagon's book.

[19] See Wagon 1985, pp. 21-23.

[20] Wagon takes this line of argument, op. cit., pp. 218-219.

[21] The arguments for this are made in two essays of my book (1998), namely, "Weyl vindicated: *Das Kontinuum* seventy years later" and "Why a little bit goes a long way. Logical foundations of scientifically applicable mathematics."

[22] Feferman 1999.

[23] Feferman 1977.