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WHAT IS MATHEMATICAL TRUTH ?

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I urge first that mathematics should be interpreted realistically and objectively. But unfortunately, belief in the objectivity of mathematics has generally gone along with belief that "mathematical objects" have an unconditional and super-physical reality, and with the idea that mathematical knowledge is strictly *a priori*. But actually, the criterion of truth in mathematics is the success of its ideas in practice; mathematical knowledge is corrigible and not absolute; thus it resembles *empirical* knowledge in many respects.

Superficially, the only method allowed in mathematics might seem to consist in deriving conclusions from axioms which have been fixed once and for all. But actually, quasi-empirical methods might be used successfully in mathematics. One might well ask why *deductions* from principles that are (more or less) self-evident should not be supplemented by hypotheses which are "evident" because they have been confirmed by mathematical "experiments". Why not use *both* deductive proof *and* confirmation by mathematical "experiment" in the search for truth?

Indeed, this seems almost necessary. All the statements (e.g., about number theory) that can be proved from the axioms form a recursively enumerable set, whereas Gödel's Theorem shows that the set of truths of elementary number theory is not recursively enumerable. Hence there must be "synthetic" truths in number theory, and a refusal to use quasi-empirical methods debars us from ever finding a single one of them.

Actually, mathematicians have been using quasi-empirical and even empirical methods all along. Thus, consider the basic postulate that there is a one-to-one order-preserving correspondence between the points on a straight line and the real numbers. On this all of analytical geometry and hence the topological theory of manifolds is founded. The Greeks could not establish this correspondence because they were unable to generalize the notion of "number" suitably. The correspondence was accepted by Descartes *not* on the basis of any construction of the reals from the rationals, but rather because the geometric evidence was so strong for it!

Indeed, after this correspondence principle had demonstrated its fertility in physics as well as mathematics, we could hardly have been persuaded to abandon it even if contradictions had turned up, although we would certainly have done something to remove the contradictions.

The same story repeats itself with the calculus. If the epsilon-delta methods had not been discovered, then infinitesmals

would have been postulated entities (just as "imaginary" numbers were for a long time). If the calculus had not been "justified" Weierstrass style, it would have been "justified" anyway.

A more recent example is provided by Zermelo's Axiom of Choice. In his famous 1908 paper [C11, pp. 183-98] Zermelo defends his axiom against the critics of his 1904 paper. Peano, in particular, had pointed out that the axiom appeared to be independent of those in Peano's *Formulaire* [C25] and had gone on to suggest that Zermelo's proof of the proposition that every set can be well-ordered was, therefore, no proof at all, since it rested on the "unproved" assertion of the Axiom of Choice. Here is Zermelo's reply [C11, p. 187]:

> First, how does Peano arrive at his own fundamental principles and justify their inclusion in the *Formulaire* since, after all, he cannot prove them either? ...This axiom (of choice), even though it was never formulated in textbook style, has frequently been used, and successfully at that, in the most diverse fields of mathematics, especially in set theory, by Dedekind, Cantor, F. Bernstein, Schoenflies, J. König, and others... Such widespread use of a principle can be explained only by its self-evidence which, of course, must not be confused with its provability. No matter if this self-evidence is to a certain degree subjective -- it is surely a necessary source of mathematical principles, even if it is not a tool of mathematical proofs... The question that can be objectively decided, whether the principle is *necessary for science*, I should like to submit for judgement by presenting a number of elementary and fundamental theorems and problems that, in my opinion, could not be dealt with at all without the principle of choice.

In my opinion, Zermelo is right on two counts. First of all, he is right that self-evidence is somewhat subjective, but nonetheless counts for *something*. In empirical science too, it is wrong to think that intuition plays no role at all. It is especially noteworthy that what Zermelo characterizes as "objective" is not the "self-evidence" of the Axiom of Choice but its *necessity for science*. Today it is not just the Axiom of Choice but the whole edifice of modern set theory whose entrenchment rests on its great success in mathematical applications -- in other words, on "necessity for science".

The fruitfulness of quasi-empirical methods in mathematics is also illustrated by Euler's discovery that $1 + 1/4 + 1/9 + \ldots$

$= \pi^2/6$. His analytical "proof" was in no sense rigorous, but confirmation to 30 decimal places made the result indisputable.

Empirical and probabilistic arguments in the theory of numbers are also persuasive. Thus if the "primenes" of successive odd numbers is approximately independent, the density of prime pairs should be asymptotically approximatley $1/(\log n)^2$. Since the integral from one to infinity of $1/(\log u)^2$ is infinite, there should be an infinite number of prime pairs $(p,p + 2)$. This reasoning seems plausible to many mathematicians.

I suggest that it would be of great value to study such quasi-empirical methods systematically, in view of their widespread use.

Coming back to philosophy, I advocate the following very simple and elegant formulation of realism: A realist, with respect to a given theory or discourse, holds that (1) the sentences of that theory or discourse are true or false; and (2) that what makes them true or false is something *external*.

[After digressing to give some arguments in favor of the idea that mathematics is essentially *modal* rather than existential, Professor Putnam returned to two arguments for *realism* in the philosophy of mathematics, in terms of mathematical experience and physical experience. The amazing *consistency* of mathematics, in spite of the fact that no finitistic consistency proof is possible (Gödel), is most impressive. But would mathematics be so consistent if its statements were incapable of *interpretation*? Is not the fertility of mathematics also due to its truth under some interpretation? May not realistic interpretations provide a basis for both the consistency and fertility of mathematics?

This position seems also to argue against Brouwerian intuitionism. In particular, Newton's Law of Universal Gravitation is intuitionistically false. Actually, Brouwer's Dissertation takes the point of view that physical objects, other people, and even future states of his own mind are all "fictions".

In closing Putnam commented on the recent view that quantum mechanics is a complete realistic theory, that there is nothing special about measurement, and that we just happen to live in a world that does not obey the laws of Boolean logic. Advocates of this new quantum logic only claim that quantum logic is true given the precisely specified operational meaning of the logical connectives. Mackey and Jauch go so far as to suggest that there is some other study, called "logic" (with no operational meaning at all) that they are not challenging. But the fact is that, if quantum logic is right, then not only the propositional calculus used in physics is affected, but also set theory itself. The effect may be that the answer to basic questions about, say, the continuum, will come in the future not from new "intuitions" alone, but from physical/mathematical discovery.]

[A more extended account of the ideas expressed here will

appear in Prof. Putnam's forthcoming book *Mathematics, Matter, and Method*, Cambridge University Press, 1975. -- Ed.]

DISCUSSION

The discussion began with a consideration of how geometry had become combined with algebra. Boyer disputed Putnam's claim that Descartes set up a one-to-one correspondence between points on a line and real numbers -- Descartes' geometry was one of line segments but not vice versa. In answer to Putnam's query concerning who first assumed that there was a (real) number for each point on the line, Regoczei suggested Cantor, and Dieudonné proposed Bombelli May said that the development of the real number system occured over two centuries and that the explicit statement of a one-to-one correspondence between points and real numbers came quite late.

Browder then proposed an alternative explanation to that of previous speakers for the unconcern of contemporary mathematicians with foundations. In the period 1900-1910 when a number of eminent mathematicians were involved with foundations, concern centered on the practical question of saving certain fields of mathematics, particularly measure theory, which depended heavily on Cantorian methods. Browder inferred that the mathematicians' present unconcern with foundations reflected the absence of such a pressing need today. Regarding Bishop's talk, Browder stated that Hermann Weyl thought non-constructive methods valuable, particularly in mathematical physics, because constructive methods restricted the range of problems accessible to research. Browder also denied Bishop's claim of a crisis in contemporary mathematics and asserted that most mathematicians would admit the existence of such a crisis only from the breakdown or inconsistency of present mathematical research. Lastly, Browder contended, mathematicians become interested in the meaning of their results only as a way of solving a difficult problem.

Putnam replied that one could be interested in philosophy of mathematics not only for such technical reasons, but also as an individual with an involvement in his culture, of which philosophy is a part. Referring next to Bishop's talk, Putnam said he considered Bishop's hypothetical reaction of Hilbert as capitulation rather than cooperation, because it ignored the question of the truth of LPO and the interpretation of the connective "or" -- in physics as well as in mathematics. Bishop denied that Hilbert would have been capitulating, on the grounds that mathematics can be applied to physics only to the extent that it is inherently "constructive." [This seems to be related to P.W. Bridgeman's "operational" view of physics. -- Ed.]

The remainder of the discussion ranged widely over the relationship between mathematics, the real world, and the philosophy of mathematics. Repeatedly speakers returned to constructivism *vs.*

realism -- as reflected in Bishop's and Putnam's talks.

Dou regarded it as sometimes necessary to be Platonistic (in the sense of admitting a typical existence of mathematical objects) in order to explain the applicability of mathematics to the real world; indeed, most mathematicians prefer Platonist or formal mathematics because it works and is beautiful. He recalled a paper (*Comm. Pure Appl. Math.* 7 (1954), 159-193.) in which P. Lax *conjectured* a theorem because of an "experimentally mathematical proof"; but Dou believed that such a proof is much closer to Physics than Mathematics. [In connection with this question, see also Polya's book [C26]. -- Ed.] Likewise, although many mathematicians believe in the existence of mathematical objects, Dou preferred Bishop's approach, because the question of Platonic realism falls outside of mathematics. Putnam replied that many important applications of mathematics are within mathematics, while May made the clarification that what Putnam called mathematical experience -- perviously created mathematics -- is part of the real world.

The discussion then turned to the meaning of existence in mathematics. Putnam regarded Dou's mention of the existence of mathematical objects as misleading. For Putnam, the important question concerned rather the objectivity of mathematics, i.e., whether any statement in mathematics is in some way true or false. Then Dou asserted that existence has a different meaning in mathematics than in physics, a contention which Dreben found debatable.

Crowe asked Putnam how he thought the history of mathematics was related to the philosophy of mathematics. Putnam replied that he was trying to relate the philosophy of mathematics to what mathematicians actually do.

Turning to other interdisciplinary relations, Freudenthal asked whether the ability to place a rocket on the moon was produced by good mathematics or good physics. In a similar vein, he then inquired whether the inability to name the next Vice-President meant that mathematics was bad. Putnam answered that the rocket's accuracy certainly indicated the success of both mathematics and physics, and that terms such as "operator" had the same meaning in both. But the failure to predict the next Vice-President concerned political science, not mathematics.

Moore asked Bishop why he objected to applying mathematics to psychology and other fields outside the physical sciences. Bishop replied that most mathematics used in psychological, social and economic research was phoney and merely decorative. May concurred that most mathematical economics was unimportant mathematically and useless economically. Finally, Bishop regretted the appearance of "applications" to social science in introductory calculus texts.