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Aspects of the Pluralistic Nature of Mathematics

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Students (and not only students) usually think of mathematics as monolithic and fixed for all time, and of mathematicians (if they give any thought to the creators of the subject they are dealing with) as in total agreement on mathematical means and ends. How many of them realize that not only the methods and results of mathematics but also its basic concepts are tentative rather than final, in flux rather than eternal? That the ideas of number, of function, of continuity, even of proof have ever been different from what they are today? We focus in this essay on the concept of proof.

It is not only that the notion and practice of proof have evolved over time, but that *at any given time* they were often debated by contemporary mathematicians. Since the way that mathematicians prove, or arrive at, their results is usually a reflection of their overall view of mathematics, we will have to consider the broader picture.

From antiquity onwards we note a divergence of views among mathematicians on how best to do mathematics, on what methods to use for attacking problems and establishing results. Some advocate formal, rigorous proofs, others intuitive, heuristic ones (and some do not see the difference between the two). Adherents of the synthetic method battle supporters of the analytic method. Rationalists confront empiricists and formalists oppose intuitionists (to use current terms rather loosely). Of course the tensions between these groups have, in general, been healthy for mathematics (though, perhaps, less so for the protagonists).

This essays consists of examples from various historical periods which illustrate the above themes of the pluralistic nature of mathematics.

Example 1

When we think of proof in ancient Greece, we think, of course, of Euclid. But there was a parallel tradition in Greek mathematics, even in the classical period — an algorithmic, numerical, algebraic tradition inherited from the Babylonians, originating in mathematical astronomy, and applied mainly to problems in astronomy (see Neugebauer, 1969, p. 208; Pederson, 1974).

But also in the area of what we would today call pure mathematics there were apparently two distinct approaches to the subject in the crucial, formative period of ca 450–350 BC, what Struik calls Democritian materialism and Platonic idealism (Struik, 1987, p. 47). Thus Democritus, a founder of atomism, is mentioned by Archimedes as having shown that the volume of a pyramid is one-third the base times the height, presumably using the non-rigorous method of indivisibles employed subsequently by Archimedes in *The Method* and, centuries later, by Cavalieri and others. On the other hand Hippocrates, at about the same time, obtained *rigorously* the quadrature of lunes. (In fact, this is the first result known to have been obtained deductively.) See Boyer (1989) and Struik (1987) for details.

Example 2

We make a great leap and come to the “modern” period. Consider Leibniz and Descartes.

The two had entirely different conceptions of proof and its role, although both tried to mathematize natural, and even human, phenomena. To Leibniz a proof was valid by virtue of its form rather than its content (a view already held by Aristotle). At one point in his development of the calculus Leibniz refers to his infinitesimals as “useful fictions” (Edwards, 1979, p. 264); the “essence” of infinitesimals was apparently of no great concern to him. Descartes would not countenance such notions. This is probably why he did not contribute to the development of the calculus. As Mahoney notes, “His demand for absolute intuitive clarity in concepts excluded the infinitesimal from his mathematics” (1981, p. 58).

To Leibniz mathematical truth is obtained through proof. To Descartes proof is irrelevant to truth. For example, Descartes claimed that God is at liberty to create a Euclidean world in which Pythagoras’ theorem fails! (Hacking, 1980, p. 173). Clearly these two mathematician-philosophers did not see eye to eye on the notion of proof. Nor on how to do geometry — but more of this later.

Example 3

In the 18th century we have an important example of differing perceptions of mathematics in the vibrating-string controversy. Technically, the debate — among d’Alembert, Euler, and D. Bernoulli — centred on the permissible *initial shapes* of an elastic string fixed at both ends and released to vibrate. At bottom, however, the debate had to do with basic approaches to doing mathematics. According to Langer,

Euler’s temperament was an imaginative one. He looked for guidance in large measure to practical considerations and physical intuition, and combined with a phenomenal ingenuity, an almost naive faith in the infallibility of mathematical formulas and the results of manipulations upon them. D’Alembert was a more critical mind, much less susceptible to conviction by formalisms. (1947, p. 17)

D’Alembert’s “critical mind” dictated that the initial shape of the vibrating string must be a twice-differentiable function since it satisfied the wave equation $\partial^2 y / \partial t^2 = a^2 \partial^2 y / \partial x^2$. Euler’s “faith in the infallibility of mathematical formulas” dictated that the solution (obtained by formal manipulations) is valid in cases less restrictive than d’Alembert’s, and his “physical intuition” suggested that the initial shape of the string could be an arbitrarily drawn curve. In fact, using d’Alembert’s general solution Euler claimed he could *graph* the shape of the string in time regardless of its initial shape. D’Alembert countered that Euler’s procedures were “against all rules of analysis” (Lützen, 1978).

Bernoulli, a mathematical physicist, entered the debate soon thereafter, arguing for a solution based largely on the physics of vibrating systems. He termed d’Alembert’s and Euler’s solutions “beautiful mathematics,” but “what has it to do,” he asked, “with vibrating strings?” (Ravetz, 1961, p. 78). Ravetz characterized the essence of the debate as one between d’Alembert’s mathematical world, Bernoulli’s physical world, and Euler’s “no-man’s land” between the two (1961, p. 81). Interestingly, Bernoulli’s work embodied the correct *mathematical* approach.

See Davis & Hersh (1981), Fraser (1989), Kleiner (1989), Kline (1972), Langer (1947), and Ravetz (1961) for details on the above.

The 19th century is replete with examples illustrating the themes we have been discussing.

Example 4

Essential elements of the formalist philosophy of mathematics were expressed by Peacock in 1830 in his *Treatise of Algebra*. Peacock wanted to confer upon algebra “the character of a demonstrative science” (Pycior, 1981, p. 34). (In geometry one proved results, in algebra one computed and solved problems.) To him this meant justifying the laws of operation with numbers and symbols, where the symbols stood for negative and complex numbers.

(These were important aspects of algebra at that time.) To this end Peacock (and others) created so-called symbolical algebra, “the science which treats of the combination of arbitrary signs and symbols by means of defined though arbitrary laws” (cited in Pycior, 1981, p. 35). Concerning the laws Peacock says:

We may assume any laws for the combination and incorporation of such symbols, so long as our assumptions are independent, and therefore not inconsistent with each other. (cited in Pycior, p. 36)

A very modern point of view, indeed!

About ten years later Hamilton invented the quaternions. He seems to have practised what Peacock had preached, namely freedom to create mathematical systems at will, subject only to requirements of consistency. This was not, however, Hamilton’s view of his creation. He viewed the quaternions as “concrete” entities, not as “arbitrary signs.” Hamilton objected to Peacock’s view of the symbols of algebra as marks without meaning, and asserted that one must “look beyond or through the signs to the things signified” (cited in Pycior, 1981, p. 40) — clearly a foreshadowing of an intuitionist’s response to Peacock’s formalist sentiments.

At the same time, not all of Hamilton’s contemporaries viewed the quaternions as “concrete” entities. Hamilton’s friend John Graves, upon learning of their invention, wrote to Hamilton:

I have not yet any clear view as to the extent to which we are at liberty arbitrarily to create imaginaries and to endow them with supernatural properties. (cited in Hankins, 1980, p. 300)

Example 5

In the early 19th century Cauchy’s work initiated a fundamental change of viewpoint regarding the calculus. The change was from an algebraic, global view of analysis to an arithmetic, local perspective. For example, functions were defined in the 18th century as algebraic formulas valid everywhere on the real line, whereas in the 19th century they were defined pointwise: to each (real) number in the domain (which might be an interval rather than all of \mathbb{R}) there corresponds a number in the range — the so-called Dirichlet conception of function. For another example, Lagrange defines the derivative of a function globally in terms of its expansion in a Taylor series, while Cauchy defines the derivative at a point using the notion of limit. See Fraser (1989) for details.

Example 6

The global versus the local, the geometric versus the analytic/arithmetic, and the heuristic versus the formal aspects of mathematics are all reflected in the second half of the 19th century in complex analysis, especially in the works of two of its founders — Riemann and Weierstrass. Riemann’s geometric theory of complex functions, based on the notion of a Riemann surface and Dirichlet’s Principle, is to be contrasted with Weierstrass’s analytic, local theory, based on power series and analytic continuation. Klein compares the nature of their approach to mathematics:

Riemann is a man of brilliant intuition. When his interest is awakened he begins fresh, without letting himself be diverted by the tradition and without acknowledging the requirements of systematization. Weierstrass is first of all a logician; he proceeds slowly, systematically, step-by-step. When he works, he strives for the definitive form. (cited in Bottazini, 1986, p. 291)

Weierstrass was critical of various aspects of Riemann’s work in analysis — both real and complex. A fundamental tool in Riemann’s approach to complex function theory was the Dirichlet Principle which he justified by physical considerations. Weierstrass provided a counter-example to the Dirichlet Principle. Following this, Weierstrass’s approach to com-

plex function theory became dominant. “Only with the works of Klein and the rehabilitation of the Dirichlet Principle by Hilbert,” notes Neuenschwander, “could the Riemannian theory again gradually recover from the blow delivered it by Weierstrass” (1981, p. 98).

Example 7

The synthetic-analytic battle was joined in full vigor in the first half of the 19th century in geometry, especially projective geometry. On the synthesists’ side lined up Poncelet, Chasles, Steiner, and von Staudt; the analysts had among their spokesmen Gergonne, Plücker, and Lamé. The analytic method, argued its adherents, offers more power, generality, and economy of thought. Analysis is antithetical to geometry, countered the synthesists. Geometry and intuition are inseparable, and the analytic method removes all intuition from geometry. (These competing sentiments echoed in the recent Dieudonné-Thom debate about the *teaching* of geometry; see Dieudonné, 1973; Thom, 1971.) The Principle of Duality in projective geometry seems to have been a test case for the two schools of thought. The question was: On what grounds does one justify the Principle? Why does it work? In this instance the analysts had the upper hand — the use of homogeneous co-ordinates made the validity of the Principle of Duality transparent. See Boyer (1989) and Seidenberg (1962) for details.

The analytic-synthetic tension apparently has its roots in Greek antiquity. The Greeks seem to have used the method of analysis to discover geometric results which they would then prove synthetically. (Of course the method of analysis was not to them what it was in the 19th century; see Boyer, 1954.) Descartes believed that in his analytic geometry he was rediscovering the ancient method of analysis. Leibniz spoke out vigorously against the “analytic method” in geometry:

I am still not satisfied with algebra because it does not give the shortest methods or the most beautiful constructions in geometry. This is why I believe that, so far as geometry is concerned, we need still another analysis which is distinctly geometric or linear and which will express *situation* [situs] directly as algebra expresses *magnitude* directly. And I believe that I have found the way and that we can represent figures and even machines and movements by characters, as algebra represents numbers or magnitudes. (cited in Rosenfeld, 1988, p. 171)

Newton, too, favoured the synthetic over the analytic approach to geometry. Thus he called a solution of a geometric problem obtained by analytic means “not a solution but a mere computation” (cited in Shenitzer, in press). It is interesting that the two creators of the calculus — analysis *par excellence* — favoured synthesis over analysis in *geometry*.

Example 8

The formalist-intuitionist controversy of the early 20th century had strong roots in the 19th century. We detail some of them:

(1) Kronecker was perhaps the first genuine pre-intuitionist. His famous lament in response to Lindemann’s proof of the transcendence of π captures some of the problems relating to the formalist-intuitionist debate: “Of what use is your beautiful investigation regarding π ? Why study such problems since irrational numbers are nonexistent?” (cited in Kline, 1972, p. 1198). Because he would not explicitly admit $\sqrt{2}$ (for example) into his domain of numbers, Kronecker viewed the algebraic number field $\mathbb{Q}(\sqrt{2})$ as (essentially) the quotient field of $\mathbb{Q}[x]$ with respect to the ideal generated by $x^2 - 2$. Because he opposed completed infinities, he objected to Dedekind’s ideals and created instead an analogous theory of “divisors,” giving their explicit construction. (To Dedekind an ideal was the set of all algebraic numbers satisfying certain properties; this predated Cantor’s completed infinities.) Both Dedekind’s and Kronecker’s approaches proved fundamental for subsequent developments. See Edwards (1987) for details.

Dedekind responded to some of Kronecker’s criticisms in a footnote to his *The Nature*

and Meaning of Numbers (1963, p. 45): “Herr Kronecker . . . sought to impose certain restrictions on the free introduction of concepts into mathematics, restrictions which I do not regard as justified.” The notion of putting “restrictions on the free introduction of concepts” is a recurring theme in mathematics (as we shall note).

(2) In function theory the following two quotations from Hermite capture some of the issues in debate:

I believe that the numbers and functions of analysis are not the arbitrary product of our minds; I believe that they exist outside of us with the same character of necessity as the objects of objective reality; and we find or discover them and study them as do the physicists, chemists and zoologists. (cited in Kline, 1972, p. 1035)

I turn away with fright and horror from this lamentable evil of functions without derivatives. (p. 973)

The above are reactions to the appearance of various “pathological” functions, such as integrable functions with dense sets of discontinuities in every interval, continuous functions that are not piecewise monotonic, non-integrable functions that are limits of integrable functions, and (especially) continuous nowhere-differentiable functions. Such examples were introduced in the second half of the 19th century, often as counter-examples to widely held notions. See Kleiner (1989) for details.

(3) The late-19th-century debates around function theory were continued and broadened in a famous exchange of letters in 1905 among Baire, Borel, Lebesgue, and Hadamard (see Moore, 1982). It was one year after Zermelo, using the axiom of choice, proved the well-ordering principle, namely that every set can be well-ordered. Although the battleground in this exchange of letters was largely function theory, broader mathematical issues were addressed. Major questions were: Does the definition of a mathematical object (e.g., a number or a function) ensure its existence? And does a (non-constructive) proof of existence of a mathematical object legitimize its acceptance and use without further consideration? Thus the use of uncountably many choices in definitions and existence proofs was classified by Borel as “outside mathematics” (cited in Moore, 1982, p. 93). (B. Peirce’s fundamental work of 1870 on linear associative algebras was similarly designated by Cayley as “outside of ordinary mathematics.”)

On the intuitionists’ side in this exchange were Lebesgue, Borel, and Baire (in increasing degrees of severity). On the opposite side was Hadamard. His response to Borel’s critique is telling:

From the invention of the infinitesimal calculus to the present, it seems to me, the essential progress in mathematics has resulted from successively annexing notions which, for the Greeks or the Renaissance geometers or the predecessors of Riemann, were “outside mathematics” because it was impossible to define them. (cited in Moore, 1982, pp. 96–97)

The sentiments expressed by Hadamard apply, of course, not just to the mathematics beginning with the infinitesimal calculus, but likely even to the mathematics of pre-historic times. P. Davis notes this in connection with the evolution of various number systems:

It is paradoxical that while mathematics has the reputation of being the one subject that brooks no contradictions, in reality it has a long history of successful living with contradictions. This is best seen in the extensions of the notion of number that have been made over a period of 2500 years. From limited sets of integers, to fractions, negative numbers, irrational numbers, complex numbers, transfinite numbers, each extension, in its way, overcame a contradictory set of demands. (1965, p. 305)

(4) The formalist and intuitionist philosophies as expounded in the early 20th century by Hilbert and Brouwer, respectively, are well known. Their effect on the thoughts and works

of others are perhaps less familiar. The following two comments, from von Neumann and Weyl, respectively, give an indication of that effect:

In my own experience . . . there were very serious substantive discussions as to what the fundamental principles of mathematics are; as to whether a large chapter of mathematics is really logically binding or not. . . . It was not at all clear exactly what one means by absolute rigor, and specifically, whether one should limit oneself to use only those parts of mathematics which nobody questioned. Thus, remarkably enough, in a large fraction of mathematics there actually existed differences of opinion! (von Neumann, 1963, p. 480)

Outwardly it does not seem to hamper our daily work, and yet I for one confess that it has had a considerable practical influence on my mathematical life. It directed my interests to fields I considered relatively “safe,” and has been a constant drain on the enthusiasm and determination with which I pursued my research work. (Weyl, 1946, p. 13).

A current version of the formalist-intuitionist debate has the non-standard analysts “batting” the constructive analysts for the proper way to do analysis, and, more generally, mathematics (see Stewart, 1986).

Example 9

A word about the axiomatic method (see Wilder, 1967). The method was essential to some mathematicians (e.g., Hilbert and E. Noether), both as a method of demonstration and as a tool of research, and marginal to others (e.g., Klein and Poincaré). Klein argued that the axiomatic method stifles creativity. Poincaré, in an essay entitled “The Future of Mathematics,” relegated it to the archives:

This domain [axiomatics] must be strictly limited, and . . . there will be nothing more to do when the inventory has been completed, which cannot be long. But when everything has been enumerated, there will be many ways of classifying it all. A good librarian always finds work to do, and each new classification will be instructive for the philosopher. (1952, p. 45)

Of course, Poincaré proved to be wrong. At the same time, a decline in the importance of the axiomatic method from its heyday in the 1920s–1960s is clearly apparent. Thus, considering some of the recent mathematical advances such as the theories of chaos and fractals, tests of primality, use of Yang-Mills equations to study problems in geometry, design and analysis of algorithms, geometric computing, advances in graph theory and combinatorics — in none of these fields is the axiomatic method of the essence. What is of the essence in these subjects is the computer, to which we now turn.

Example 10

“The intruder,” asserts Lynn Steen referring to the computer, “has changed the ecosystem of mathematics profoundly and permanently” (1986, p. 34). It poses new problems for research, provides tools to solve old problems, and suggests new research strategies. Of the sixteen plenary lectures at the recent International Congress of Mathematicians (Berkeley, 1986), which surveyed the current state of mathematics, more than half were on topics connected in some way with computation. This is not to suggest that the axiomatic method and abstraction have ceased to play an important role in mathematics. Thus Faltings’s 1982 proof of the Mordell Conjecture uses high-powered abstract ideas of algebraic geometry in which axiomatics is fundamental. Many other examples can, of course, be given. The point is that axiomatics and abstract mathematics now have a serious competitor in heuristics and more concrete, applications-related mathematics in which the computer often plays an important role. See Peterson (1988) and Steen (1986, 1988) for details.

The computer-aided proof of the four-colour conjecture “shook the very epistemology of mathematics,” notes Steen (1988, p. 613). The non-existence of finite projective planes of order 10 is another recent result proved with the aid of the computer (see Cipra, 1988).

Very long recent “human proofs” (i.e., proofs not requiring the assistance of a computer), such as the classification (completed in the 1980s) of finite simple groups, belong in a similar category. (This proof takes up over 11,000 pages scattered over many journals.) Both types of proof are obtained by *groups* of mathematicians and are unsurveyable by a single human being. The collective efforts (and wisdom) of a group of practitioners are required to give them credence, let alone credibility. Proof is thus coming to be seen as a public rather than a private activity, a social process rather than as an infallible endeavor. A mathematical theorem becomes acceptable (true, valid) if it is used, discussed, and referred to by other mathematicians — that is, if it has stood the test of time. See Davis and Hersh (1981) for details.

Example II

A final emerging idea regarding proof has to do with probabilistic proof. Some results (e.g., primality of very large integers, decidability in logic, non-isomorphism of graphs), even if theoretically decidable, have such long proofs that even a computer cannot write them down. Michael Rabin has suggested that one resort in such cases to proving that the result holds with a very low probability of error. He has done that for tests for primality, and others have done it for the other examples mentioned above. Moreover, it has been argued that such probabilistic proofs are no worse than many deterministic proofs of standard mathematical practice: each is valid with a certain probability of error. See Kolata (1976) for details.

Thus, when proofs are too long for humans we ask the aid of the computer, and when they are too long for the computer, we come back to humans for help. In any case, computer-aided proofs and probability-aided proofs are two entirely new ideas of the second half of the 20th century concerning both the theory and the practice of proof. A welcome sign, indeed (if one were needed), of the health and vigor of late-20th-century mathematics.

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