Society's role in mathematics

Mathematicians being as human as they are, there is no question that doing mathematics has always been, and will always be, a social activity: when mathematicians have been productive, they are as excited about their latest creation as musical composers can be about their latest composition, and they are equally eager to share their delight. But it is not only in an artistic or emotional sense that the people doing mathematics form some sort of community for, by and large, they also do so in some very technical respects.

It is, for instance, quite common that a number of mathematicians work on closely related problems, for this is all but unavoidable when a theory that is still to be born is in the air; in such a case, new results obtained by one are often of immediate significance for others. It is this circumstance that, over the centuries, has given rise to the phenomenon of scientific correspondence.

Aside Modern communication facilities like e-mail have the potential of highly intensifying scientific interchange and, consequently, have been hailed as of all-pervading scientific significance, but this seems a rash conclusion since, to the best of my knowledge, the modest speed of "snail mail" has never been a serious bottleneck: I at least never got the impression that, say, the development of quantum mechanics in the 20s was seriously hampered by the absence of e-mail. (On the contrary: pressure to conduct the development of quantum mechanics within the confines of ASCII-code could easily have been harmful.) If the modern communication and cooperation are wellcomed exuberantly, this unwarranted enthusiasm could very well have a political origin, viz. the collectivist desire to play down the potential role of the individual. (End of Aside.)

Next, the mathematical community got the task of setting and maintaining quality standards, and this happened in a way which, in retrospect, may well be typical for mathematics. In the usual reconstruction of the early history of mathematics, mathematical "truths" — such as, for instance, that a triangle with sides of lengths 3, 4, and 5 has a right angle — began as experimental facts gained by observation, until Hellenic genius changed that. The ancient Greeks had at least a double mathematical target: firstly they wanted to generalize and unify — say that they wanted to capture as well that a triangle with sides of lengths 5, 12, and 13 has a right angle — and secondly they wanted certainty and precision: they wanted to know why the ideal triangle with sides of exactly 3, 4, and 5 has an exactly right angle. The pivotal word in the previous sentence is the connective "why": they perceived that such fact, instead of being imperfectly established by observation, could and hence should be established perfectly by a convincing argument. But the notion of "a convincing argument" immediately raises the question "convincing to whom?": if the audience to be convinced is sufficiently gullible, the argument can be gloriously defective! It became the task of the mathematical community to cultivate the scepticism against which the quality of the "convincing argument" would be checked.

Thus the so-called "consensus model" of mathematics emerged, in which a proof was deemed correct (be it "up to the standards of rigour of the day") when it was accepted by the mathematical colleagues because they saw nothing wrong or fishy with it. The situation was not ideal but for the first 20 centuries after Euclid there was no alternative. The mathematical edifice had been erected by drawing logical conclusions from the axioms and neither the logic nor the axioms were really subject to doubt. The axioms, introduced as "self-evident truths" were supposed to capture a given, fixed reality out there, so there was not much flexibility for them, and in the same vein there seemed to be one "true" logic that had to be used to draw the conclusions, so no freedom there either. Philosophers seemed to occupy themselves with logic, but on closer inspection they struggled with the problems of natural language, a medium now infamous for its obscure semantics. But as long as one searches for "convincing arguments" and the party to be convinced insists on understanding the argument in terms of the almost certainly inadequate vehicles of natural language and pictures, there is little else one can do.

<u>Aside</u> I was shocked when I discovered what can only be called The Great Mathematical Fraud, viz. that on the one hand Euclidean Geometry has been presented to the one genera-

tion of schoolchildren after the other as the prime example of a deductive science in all its pure glory, while at the same time all those generations of innocent schoolchildren have been taught to draw geometrical "conclusions" that do <u>not</u> follow from Euclid's axioms! Euclid's axioms just don't suffice, but the fraud could be hidden behind the obscure semantics of prose and picture. (End of Aside.)

With all respect the Greeks have been blamed for not dealing with real numbers in their own right, but dealing with them in the interpretation of lengths, areas and volumes instead. Elementary algebra (about sums, products, ratios etc.) thus became a branch of geometry and remained so until René Descartes (1596-1650) turned things upside down. Descartes's vision was nothing less than to treat via the introduction of coordinates geometry as a branch of algebra. This conceptual turnabout, which has been hailed as the beginning of modern mathematics, was all the more impressive since Descartes had to create in passing the algebra he needed and its notational conventions. We shall return to these later, here we should mention, again with due respect, Descartes's initial error of judgement.

When Descartes had his vision, he was more than excited, he was ecstatic. You see, there was no systematic methodology for solving geometrical problems, at a given moment geometrical arguments seemed to require the inspired introduction of something like an auxiliary line, a circumstance that kept geometry a magic art. In algebra, however, this phenomenon was unknown: the rules of algebraic formula manipulation being known, algebraic problems were solved by turning the crank, so to speak. So for a short while it was felt that the translation into algebra would do away with the need of invention for which geometry was notorious.

As we shall see later, however, algebraic manipulation is not that mechanical and is more than just turning the crank. As the problems we try to solve become more advanced, the amount of manipulation required quickly becomes totally unmanageable unless we find a sufficiently effective way of reducing it. We effectuate such a reduction for instance by an inspired choice of what to name, and it turns out that such a choice is very similar to the inspired choice of, say, an auxiliary line. The temporary assumption that calculation, because mechanical, was trivial was an illusion based on experience with simple problems only. So much for Descartes's initial error of judgement.

Isaac Newton (1642 - 1727) and Gottfried Wilhelm Leibniz (1646 - 1716) were born in the last 10 years of Descartes's life, and the rest of the 17th Century saw a rapid and dramatic development of mathematics indeed. There emerged lots of things the mathematical community had to get used to, had to screen and sometimes had to reject. Seemingly correct arguments had led to obviously erroneous conclusions (e.g. in the case of sums of infinite sequences), the concepts on which Newton based his differential and integral calculus seemed to some contradictory, in short, the "consensus model" of mathematics was in full force, and consensus was not always obtained.

Things really began to change in the 19th Century, the first half of which was dominated by the towering figure of Carl Friedrich Gauss (1777 - 1855), who —among many other things!— set new standards for mathematical rigour. It were these new standards for rigour that made it possible to wean mathematics from the old intuitive arguments, as a result of which the counter-intuitive result could be established that non-Euclidean geometries could be made consistent and hence were conceivable. (These are geometries that do not include the Euclidean axiom that, through a point outside a line, there exists a unique parallel to that line.)

Since Descartes, who had based geometry on real algebra, the Euclidean axioms had already lost some of their exalted status of "the base of everything"; the introduction of non-Euclidean geometries completed their fall from the pedestal. When axioms lost their status of "self-evident truths" that could be taken for granted, they became assumptions or postulates that had to be stated explicitly.

It was around the turn of the century that under the guidance of the uncompromising David Hilbert (1862-1943) we got what became known as "the postulational method". Its reception was mixed. To quote E.T. Bell — from "The Development of Mathematics (1945)—

> "Mathematicians and scientists of the conservative persuasion may feel that a science constrained by an explicitly formulated set of assumptions has lost some of its freedom and is almost dead. Experience shows that the only loss is the denial of the privilege of making avoidable mistakes in reasoning. As is perhaps but humanly natural, each new encroachment of the postulational method is vigorously resisted by some as an invasion of hallowed tradition. Objection to the method is neither more nor less than objection to mathematics."

It were not only the "assumptions" or "postulates", which replaced the axioms, that had to be stated explicitly. In the quest for greater clarity and precision, it soon emerged that the adopted deduction rules, i.e. the "logic" or the "inference rules" had to be stated explicitly. In principle it no longer sufficed to say that one had proved a theorem, one had to add whether the argument was kosher according to, say, intuistionistic logic (L.E.J. Brouwer); in practice the more demanding logics that were proposed were for many decades largely ignored.

For our current considerations that neglect is not important, for, neglected or not, the existence of alternative -not to say: competing- logics shed a new light on what the role of the mathematical community had been: bringing out a vote on the acceptance of the logic implicitly underlying the (usually verbal/pictorial) argument.

As time went on, the need of greater clarity and precision made the inadequacies of the verbal/pictorial argument more and more obvious, and formulae began to play a greater role. In the beginning, formulae were primarily used for descriptive purposes, to characterize or summarize situations or conclusions, while the step from one situation to the next was still reasoned verbally. (In this stage of formalization, syntactically ambiguous formulae are still quite common: already knowing what is "meant", the "understanding" reader is supposed to disambiguate them so that they make "sense".)

In the next stage, the rules of reasoning became stated and applied as rules of formula manipulation and the (this time syntactically unambiguous) formulae began to carry the argument. A new style of doing mathematics calculationally emerged. Personally I think that the calculational style of doing mathematics has great advantages. It helps in not making "avoidable mistakes in reasoning", it leads to crisp proofs that are easily checked — if so desired, mechanically — and thus to a superior product, it is more amenable to the scaling up that is required when we wish to treat programs as mathematical objects, and, most importantly, formula manipulation is much better teachable than the elusive cultivation of one's "intuition".

The ultimate consequence of the adoption of the postulational method is the inadequacy and subsequently the irrelevance of the consensus model of mathematics, as the latter reflected a form of interaction with the community which was needed to compensate for the major shortcomings of informal intuitive reasoning.

Not everyone notices that mathematics is evolving in a direction that makes the consensus model bsolete. Computing scientists, as a rule, tend to be aware of it; familiar as they are with manipulation (mechanized or not) of uninterpreted formulae, they tend to feel quite at home with more calculational arguments. Moreover they need the formal arguments as they cope better than the verbal arguments with the scaling up that reasoning about digital systems quickly requires.

Regrettably this evolution seems to be ignored precisely where the awareness of it would be of the greatest importance, viz. mathematical education at large. In the name of attracting females and other minority students, a sort of feel-good mathematics is promoted in which rigorous arguments don't seem to play a role and that utterly fails to prepare its victims for their future. This is a tragedy.

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