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The Use of Mathematics

R. W. HAMMING

The idea that mathematics is a socially useful invention of the human mind rather than merely the "art for art's sake" of the pure mathematicians differs enough from what is usually taught (by implication) in school that it seems necessary to trace some of the steps by which I came to this view.

As a graduate student in 1940 I was already somewhat interested in the use of mathematics and had decided to write a thesis in the field of differential equations. My fellow graduate students asked, "Why do a thesis in a field where there is so much known? Why not do one in abstract algebra or topology where (at that time) there is so much less known?" My reply was, "I want to be a mathematician, not just get a degree."

As a graduate student interested in becoming a mathematician I read books in the University of Illinois library on the history of mathematics, on probability (which was then seldom taught and not regarded as part of mathematics), and Bôcher's book [**1**] on how to carry out many parts of the abstract algebra I was being taught. I knew that I would not be asked questions on such topics, but never mind, they seemed to me to be needed if one were to be a mathematician.

From the history of mathematics, and a bit on the foundations, I learned that Hilbert had added a number of postulates that involved intersections and "betweenness" to Euclid to make geometry more rigorous and avoid the known "proof," based on a false drawing, that all triangles are isosceles. At first it struck me as odd, that no theorem in Euclid was thereby shown to be false; though once a theorem had used one of the new postulates all subsequent theorems that depended on it could not have been "proved" by Euclid. On thinking about this point, I soon realized that, of course, Hilbert picked the new postulates so that exactly this would happen. But this opened the door to the realization that Euclid had been in a similar position; he knew that the Pythagorean theorem and many others were "true," hence he had to find postulates that would support them. He did not lay down postulates and make deductions as it is presented in school. Indeed, the postulational method merely allows the elaboration, but also tends to prevent the evolution, of mathematics.

Later I learned that Boas, who had edited *Mathematical Reviews* for years, had asserted that many of the new theorems that appear each year are true, but the given proofs are false. Even as a graduate student I had seen many proofs of theorems "patched up"; the theorem did not change thereby.

Many years afterwards, I came upon Lakatos' *Proofs and Refutations* [**2**] that made it finally clear to me that the proof driven theorems are quite common. Indeed, that booklet made me see clearly what G. H. Hardy had said [**2**, p. 29] about the nonexistence of proofs of anything in mathematics; that with a rising standard of rigor we could never say we had a final proof. If you found a proof that Cauchy's theorem was false, it would be interesting; but I believe the result would be new postulates to support a proof. Cauchy's theorem is "true" independent of the postulates.

Continuing my scientific biography, in the normal development path of becoming a mathematician, during the first few years after getting my degree I had published (or had accepted) several notes and papers. But the Second World War brought me to Los Alamos where I found I was to run the computing center that was calculating the atomic bomb behavior. Others had set up the system and got it going, and I was to be the caretaker, as it were — to keep things going so that they could get back to more important physics.

The Los Alamos experience had a great effect on me. First, I saw clearly that I was at best second rate, while many of the people around me were first rate. To say the least, I was envious, and I began a lifelong study of what makes great science and great scientists. The answer, glibly, is style — "working on the right problem at the right time and in the right way." Anything else is unlikely to matter much in the history of a field. I saw clearly that my few published papers and notes were mostly third rate with one, at best, second rate.

Second, I saw that the computing approach to the bomb design was essential. There could be no small scale experiments; you either had a critical mass or you did not. But thinking long and hard on this matter over the years showed me that the very nature of science would change as we look more at computer simulations and less at the real world experiments that, traditionally, are regarded as essential.

Third, the accuracy of the computations, as judged by the Alamogordo test, was quite impressive to me, and hence I learned that we can simulate reality quite accurately — sometimes!

Fourth, there was a computation of whether or not the test bomb would ignite the atmosphere. Thus the test risked, on the basis of a computation, all of life in the known universe. This computation involved not only the physical modelling but also the question of whether the received mathematics and its postulates are relevant to calculations about reality. And more mature thought showed that also our standard system of logic was involved. How sure is one that they are completely safe to use? For example, "... the real numbers cannot be uniquely characterized by a set of axioms." [3, p. 98] Are we therefore sure they are always appropriate and can we depend on everything they produce? To paraphrase Hilbert, "When rigor comes in meaning departs."

Thus, severe doubts of the relevance of the official mathematics were raised in my mind, and since then have never gone away. One knows that the foundations of the current set theory approach are unsound and serious paradoxes arise: for example the Banach-Tarski paradox that a sphere can be cut up into a finite number of pieces and reassembled to make a sphere of any other size you wish. We act, and must act, on results obtained by modelling the real world, and the relationship between the two is a matter of grave importance. The past unreasonable effectiveness of mathematics is no guarantee of future successes. Indeed, we know that when *forces* were first introduced in the late Middle Ages, the scalar model of forces had to be replaced with a newly invented vector mathematics. Thus past mathematics is not always the proper tool for new applications! Although quaternions form an algebra and vectors do not, most of the time vectors are preferable; mathematical standards of elegance are not the sole criterion to use.

While I was still at Los Alamos, I began to study numerical analysis, such as it was in those days, because I believed I ought to understand what I was computing, and I never had a course in the subject. I soon found that the Simpson approximation

$$\int_0^1 f(x)\,dx =$$
$$\frac{1}{6n}\left[f(0) + 4f\left(\frac{1}{2n}\right) + 2f\left(\frac{2}{2n}\right) + \cdots + 4f\left(1 - \frac{1}{2n}\right) + f(1)\right]$$

was peculiar. Viewed as estimating the average height of the integrand from $2n + 1$ equally spaced samples, it is difficult to believe that near the middle of the range one point has twice the sampling importance as its adjacent ones! A review of the derivation shows that everything is proper, there is no mistake in the derivation. What must be wrong is that what you are doing is estimating the integrand locally by parabolas, and the approximating function has discontinuities in the first derivative at 2nd, 4th, 6th,..., $2(n-2)$ points. If that is what you are integrating, then of course the formula is reasonable (the usual error term suggests the integrand has a fourth derivative in the whole interval), otherwise it simply does not meet the test of common sense!

At the end of the war I went to Bell Telephone Laboratories (BTL) in Murray Hill, New Jersey saying to myself and others that I would stay three years and learn more about the uses of mathematics and then return to teaching. Either I was stupid, or there was more to learn than I had thought, because I stayed thirty years. Shortly after joining BTL I was teaching a night course in Numerical Methods, and I found myself saying (following the book), "To get the derivative of some data you pass a polynomial through the data, take the derivative of the polynomial, and then evaluate the derivative at the sample point." As I was saying it I realized that the error term

$$(x-x_1)(x-x_2)\cdots(x-x_n)f^{(n)}(\theta)/n!$$

means that almost certainly the approximating polynomial is crossing the original curve at the sample point, and it would be hard to choose a worse point. Fortunately I was saved by the bell, but on the long ride home I had time to think that probably you would be better off not to use the point where you wanted to estimate the derivative, but only the others, differentiate that polynomial, and then evaluate it at the point you wanted! Again, nothing is wrong with the given formula I had presented nor the given error terms — just the whole idea was foolish!

A third example of being mathematically correct but still wrong is finding a maximum performance as a function of a single parameter. In Figure 1, on the left you see what the computer experts say is "well conditioned," and on the right "ill conditioned," because they believe that the value of the parameter is hard to determine in the second case and easy in the first. But to the engineer, the first case means that any slight manufacturing variations, or aging of components, or wear and tear, will change the performance greatly, while in the second case it will not, and the design is robust! In this case the conventional attitude is exactly opposite to the practical!

Early in my career at BTL I found myself working on guided missiles (which grew into space flight over the years) as well as a wide variety of telephone and other scientific questions. Because I was interested in studying great scientists I soon adopted the rule that when I had a choice I would work with the best people I could find rather than choose by the problem.

One consequence of this was that I early began ten years of working with John W. Tukey. I say "working with" in the sense that I did not formally report to him but not in the sense that we were equals in intelligence. He

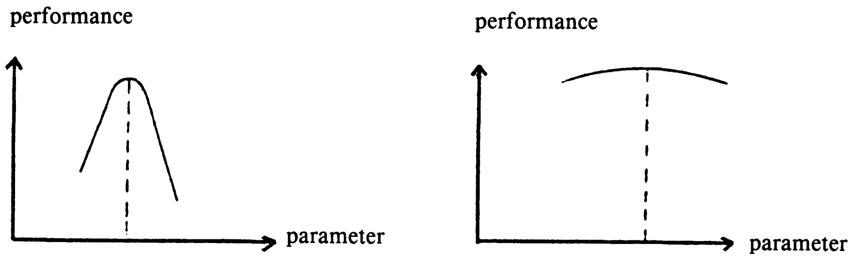

FIGURE 1

was clearly a genius from whom I could hope to learn a lot, and indeed I got much of my postdoctoral education from him.

John was involved in a thousand things at any one moment: being a professor at Princeton, working at BTL one or two days a week, working for a time for RCA and assorted chemical companies, and going often to Washington for various purposes. As a result he often had little time to meditate on matters, and thus it happened on a few occasions that with more time available I did make some contributions to the joint work. John did at times let his cleverness run away with his common sense, but when he took the time to think he was fabulous to watch. John, and his work, were usually closely connected with the real world, and what he derived on paper one week, or I computed for him, was translated into action and verified by what happened the next week. Often the consequences of being wrong could involve human lives or large sums of money — or both!

Because I was, for a long time, one of the few people at BTL who had a realistic grasp on the potentialities of computing, I found that many other problems of pressing importance came to me for computing help. It would be invidious to name some of the people from whom I learned and omit others, so, settling for only J. W. Tukey as an explicit source of my education, I will pass on to discussing some of the things I learned.

One day while I sat in my office thinking, it occurred to me that no one could ever come into my office and ask me to compute a noncomputable number! Indeed one cannot describe one in any acceptable fashion. What then are these numbers and where do they come from? On thinking it over one sees that the Cauchy condition that *any* convergent sequence defines a number is what brings in the uncountable number of noncomputable numbers rather than *any describable* convergent sequence. Hilbert in a formalistic sense would say that the numbers exist. But in what practical sense do they exist? If a result depends on their existence dare I act on the result? As a result of these thoughts I came up with the remark, "If whether an aeroplane would fly or not depended on some functions being Lebesgue integrable, but not Riemann, then I would not fly in it." If you take the unit interval and remove the computable numbers, then if you believe in the formalism you have a noncountable set left — no number of which you could ever hope to name! And if you did this for each unit interval then by the axiom of choice you could select one number from each and form a new set. Can such things and such sets be relevant to the use of mathematics other than to create new mathematics from old mathematics? What actions depending on them would we dare take in the real world? This is somewhat the attitude of the constructivist school of mathematics [**4**]. Obviously, there is no one single model of mathematics to be used in all cases; the one *you* use must depend on the particular situation as *you* see it.

Having pointed out some of my doubts about the reliability of the received mathematics when using it in the real world [**5**], let me turn to the other side of mathematics, the unreasonable effectiveness of it. Again and again, what we did in our offices by making marks on paper or computing on a computing machine was soon verified in real life. Furthermore, generalizations from a particular case often illuminated things for us.

Let me take one particular simple case. The problem was to compute from eleven equally spaced points $[0(.1)1]$ of the experimentally determined function $f(t)$ the expression

$$g(x) = \frac{d}{dx}\int_0^x \frac{f(t)}{\sqrt{x-t}}\,dt.$$

It is clear you cannot differentiate under the integral sign and that it is going to be difficult to do it purely numerically. I soon observed that if

$$f(t) = t^n$$

and I used the substitution

$$t = x\sin^2\theta$$

I would have

$$g(x) = \frac{d}{dx}\int_0^{\pi/2} \frac{x^n \sin^{2n}\theta}{\sqrt{x}\cos\theta} 2x\sin\theta\cos\theta\,d\theta = 2\frac{d}{dx}x^{n+1/2}\int_0^{\pi/2} \sin^{2n+1}\theta\,d\theta.$$

Let W_{2k+1} be the Wallis numbers

$$W_{2k+1} = \int_0^{\pi/2} \sin^{2k+1}\theta\,d\theta.$$

The result is then

$$g(x) = \frac{(2n+1)}{\sqrt{x}}x^n W_{2n+1}.$$

Now if I can represent

$$f(t) = \sum_{k=0}^{n} c_k t^k$$

then I have

$$g(x) = \frac{1}{\sqrt{x}} \sum_{k=0}^{n} (2k+1) W_{2k+1} c_k x^k.$$

What is most important is that I could exhibit all the approximation to the physicist in the domain of the observations; I could plot (for his inspection and approval) the detailed curve of the polynomial $f(t)$ I used. There is no other approximation being made.

The popularity of this fact with the physicist caused me to study and abstract what went on, and as a result, I have used this *general idea* a number of times. Indeed, partly from this experience, I learned not to attack a given problem as an isolated one, but like a mathematician, embed it in some suitable class of problems and attack the class as a whole. As any good mathematician well knows, often the general case is easier to solve than the particular case with all its confusing details. The trick is, of course, to make *realistic* generalizations that are fruitful and relevant.

Yes, the habits of the mathematician to extend, generalize and abstract, *when held under reasonable control* are very valuable in practice. Indeed, more than the results, it is the methods of science and engineering that are important for progress. It is so easy, and so common, to get lost in the particular details that one loses the larger picture all too often. One of the traits that I seem to have found in the great scientists I studied is that they have the ability to see the whole as a whole and not get lost in the particulars.

An example of this that I recall is the time, long ago, when we kept hearing reports of "superdirective radar antennas" from the military group. Each day had a new, fabulous result. The mathematicians had some trouble in convincing them that the designs would not work, that upon close inspection of the details of their designs they would find something like 1,000,000 volts at one point and 1 micron away $-1,000,000$ volts, along with currents they could not possibly produce. Mathematics often plays that role — keeping the whole in a proper perspective and balancing the various components in the complex design so that one component is not optimized at the expense of another, that the whole, as a whole, is balanced and well designed. It is curious to me that mathematicians so often play the role of the conscience of the large projects, that the larger views which mathematics can give are sorely needed by the people who are lost in the technical details of the work. This is not meant as a criticism, only as a delineation of one role of mathematics as I have seen it.

If mathematics is to play this central role and not just fill in local details, then one must constantly question the appropriate mathematics to use —

which often may have little to do with, or even contradict, conventional mathematics as taught in the universities at the present.

Probability is a particularly vexing field of use. The Kolmogorov postulates clearly give an elegant mathematical structure for proving theorems, but are also clearly static and use measure theory. Yet so many modern probabilists, especially the Bayeseans, use a dynamic probability whose postulates have not been spelled out, whose assumptions are not clearly stated. Often we must act on probability results, and one is not so confident as one could wish to be in many cases. I doubt that measure theory is a safe foundation for actions in the real world. What then is the kind of probability I dare act on? Certainly not the kind I find in most advanced texts on the subject!

Along with the greater and greater abstraction of recent mathematics which has proved to be so effective in settling problems that arise in the field of mathematics itself, we need also the study of the more robust mathematics that seems to fit the real world applications. There seems to be, currently, a small trend in this direction, but if we are to compete with the rest of the world in the useful applications of mathematics then this aspect needs more attention in our teaching of the coming generation. I am wary of the abstractions that simply raise, as I often called it, the "falutin' index." Yet I clearly see the need for some abstractions to surmount the sea of details in the literature that constantly inundates us. In my opinion, we need to teach mathematics, not pure mathematics, but it is hard to draw a line between the two. In the use of mathematics, just as in pure mathematics, there is a large component of art that almost by definition cannot be taught easily, if at all. About all one can do is exhibit many cases and hope that from these the students grasp what it is.

I now turn to the satisfactions of the life devoted to doing useful mathematics. First, you are apt to learn much more mathematics than the pure mathematician who tends to research only a few fields or even a single one. In mathematics to a great extent the problems pick you, and you find yourself studying the relevant mathematical fields with an intensity that casual study cannot give.

Second, because of the importance of the work there are often implicit deadlines that tend to push you forward. Hence over a lifetime you learn more mathematics than you would otherwise.

Third, you often find that some long forgotten (by current teachers) field needs to be examined again. And because your interests are different from those of your predecessors you often find new results!

Fourth, new fields of mathematics, such as Information Theory and Coding Theory, arise from the "use of mathematics" view, and one finds oneself in on the ground floor as it were.

Fifth, because the same mathematics tends to arise in many different fields you get alternate, and often fruitful, versions of it, and may thereby be led to new discoveries, or at least cross fertilization.

Sixth, as you look around the world in which you live you see many consequences of your work. A number of famous pure mathematicians have bragged that their work was pure and uncontaminated by use, but that sounds to me like a psychological defense mechanism. This opinion is strengthened by the observation that when they are occasionally told that some of what they did has been used, then they very often show great interest and are later heard to brag about it! It is not rare that useful mathematics changes the world we live in. In my case it ranges over so much of the background of life we lead today that almost anywhere you look you see consequences of research I was closely or at least loosely involved in. Of course, if one person does not create something, then generally speaking someone else soon will, but it is customary to recognize as the creators the first persons or else those who translated the ideas into wide practice.

Lastly, the material rewards tend to be greater, not only in money, but in opportunities to travel to interesting places, to meet other people doing important and exciting things, to see new developments as they are being created in the laboratories, and to get a broad education beyond mathematics. For example, in studying the foundations of probability one soon sees that Quantum Mechanics uses, in effect, complex probabilities yet after more than fifty years mathematicians are still looking only at the real aspect. Thus to see what is going on in probability theory, one is brought to study Q.M. both in its foundations and in some of its applications. Doing useful mathematics can be an exciting, rewarding life!

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