MODERN MATH" AND ITS CRITICS.

By W. W. Sawyer

The "Modern Math" epoch in U.S.A. is a good example of the way in which the interaction of unrelated forces produces the chaos that is history. I went to the States in February, 1957. Max Beberman was already part way through the writing of the book, that embodied the ideas of a group of research mathematicians. Their aim was to enable students in university to grapple more effectively with research in topology and other abstract branches of pure mathematics. Nothing was further from their mind than to improve the application of mathematics to industry and military competition. They were not merely ignorant of applied mathematics; they felt a definite repulsion from it. No doubt there were some pure mathematicians who did not feel this way, but there were countless incidents that proved it was the general attitude.

The second influence was the Russian launching of Sputnik in October 1957. There was immediate panic in the States. They had imagined they were ahead of the Russians in technology. Now it appeared the reverse was the case. Drastic steps had to be taken at once. One of the subjects on which technology depended was mathematics. They knew mathematics teaching in high schools was desperately bad (as indeed it was). The educationists were totally discredited (as they needed to be) so the Government turned to the mathematicians for advice. As a result millions of dollars were provided to assist the Illinois project, the books that Max Bebermann was preparing.

It is remarkable that the Government were under the impression that "mathematician" was a sufficient description, and that it was unnecessary to distinguish between pure and applied. There were in fact some applied mathematicians in the States. The eminent mathematician, Richard Courant, was in U.S.A. He had been in a German university and was Jewish. In the early days of Nazi rule, Hitler made an exception for Jews who had been in the German army in 1914-1918, which Courant had been. However, he very wisely decided to get out of Germany. So far as I was able to find out, there were only a few applied mathematicians in America in the 1950s - Courant, his relations, and his pupils. This shortage was to cause great difficulties in the development of space technology. It was of course not this minority of mathematicians that the Government consulted.

The great mass of Americans supported the change in mathematical education. Mention of the dates was sufficient for them; Modern Math was discovered after 1900, much of traditional mathematics dated from the 17th century. No one would dream of going back from the automobile to the horse and buggy. The analogy of course was completely unrealistic.

"Modern Math completely useless".

There must have been many in the country who were doubtful of the changes but were unable to speak out; the supporters of modern mathematics had a great advantage - practically no one knew what it was. Maybe we were ignorant of some immensely useful truth it contained.

One who was not held back by any such uncertainty was Morris Kline. From the start he maintained that modern mathematics was completely useless. He mentioned magnetohydrodynamics and other parts of applied mathematics that it was sensible to study. The stuff coming into the schools about union and intersection was extremely trivial (as indeed it was).

He had some excuse for regarding modern work as useless. Dieudonne, a strong supporter of modern mathematics, in a speech made the following statements among others;-
"Modern mathematics has no contact with reality."

"In the past physics provided problems for mathematicians to work on, but to-day we have plenty of problems in mathematics itself."

"None of the really new mathematics of this century has found any practical application whatever."

"It should be studied as an art form."

So Dieudonne and Morris Kline agreed that modern mathematics was useless. In fact both of them were wrong.

Incidentally it is a rather charming idea that professors should be employed to study an art form, and that so long as problems are found to keep them busy, everything is all right.

While I was not sure that Kline's arguments were all sound, I was glad that he was speaking out and helping to destroy the illusion that the Government's policy had universal support. Some years after the start of the controversy, I explained my position to a group of mathematicians which included Morris Kline. I said it seemed to me that Morris produced profoundly true statements and extremely misleading statements in such rapid alternation that I lost touch with the score. My aim therefore had been to support him but not to be identified with him. He did not seem to object to this remark.

My part in the debate.

At the outset I, like many others, was troubled by the fact that my knowledge of recent mathematics was very limited, and I could hardly speak out on that matter until I had repaired this lack. However I entered the discussion immediately, for there was one thing of which I was absolutely certain; the educational proposals were sheer insanity and I spoke on this frequently. from the outset

So far as I know, Morris Kline and I were the only people known nationally as opposing the Illinois program. I could not endorse Kline's statement that modern theories had no practical use. For one thing my knowledge of recent mathematics was insufficient. For another, I had a vague feeling that mathematical results had a way of finding applications undreamed of by their discoverers.

I gradually sorted out the mathematical aspect of the issues, and I know I had completed this by June 1960, for in that month I gave a talk to the Annual meeting of the American Society of Electrical Engineers ***on The Reconstruction of Mathematical Education***. Clearly to arrive at a reasoned position on this topic it was necessary to have a lot of information both on modern mathematics and on its possible applications. I did my best to give an adequate survey of the literature relevant to the problem. One item I quoted was by F.J.Weyl and dealt with papers on applied mathematics that used recent mathematics. It will be sufficient if here I confine myself to my report on Weyl's work for it leads us to heart of the problem. It ran "The general impression given by these papers is that a problem is formulated in classical terms and is developed a certain way along classical lines; at a certain point - which may well be a vital one - some modern theory is brought in."

Thus in this matter both Kline and the Illinois program went wrong , in opposite directions, Kline in denying that modern work was useful, Illinois in attempting the ridiculous task of teaching modern theory in secondary and even in elementary school. A rational curriculum would establish classical mathematics early, and later, perhaps at the end of secondary school or the beginning of university, present the modern theories that could operate on the classical foundation.

My paper was a very long one, and seriously overestimated what could be communicated in the time allotted to a speaker. However the engineers, who had instinctively felt that the Illinois

program was disastrous, were anxious to have their feelings confirmed by a mathematician and let me speak at great length.

The Wider Audience.

Of course the concern about the Illinois Program was not confined to professional engineers. In particular teachers had every reason for wanting to form a correct estimate of it. My Penguin book, ***A Path to Modern Mathematics*** appeared in 1966 and tried to meet this need. It emphasized that modern mathematics did not replace the traditional mathematics but both arose from it and returned to it. It was useless just to learn some words that occurred in modern work. The book therefore gradually built up the concepts of vectors and matrices, so that a final chapter was able to give simple examples of metric and Banach spaces. Incidentally, this chapter quoted the explanation Banach gave in 1922 of why he felt it worth while to develop his concept of Banach space. Banach pointed out that there were theorems in traditional mathematics that occurred in different topics and all used essentially the same proof. How boring to work through this proof ten times! It would be better to list the properties that were common to the ten different situations and prove it once and for all.

When I mentioned this remark of Banach's to a fellow mathematician, he commented, "For most young mathematicians today, their reading does not go as far back as 1922." This meant they had been taught the axioms of Banach space, without ever being told the purpose of this concept - a striking example of the unhistorical approach which is widespread, when a lecturer announces a collection of axioms but gives no explanation of why we are studying this particular collection and not some other.

In my talk to the engineers I said that rigour should not be confined to analysis. There was a great need for rigour in educational discussions. Any proposed innovation should be supported by rigorous arguments, that is to say assertions that could be shown to be true. This rigour was notably absent in years following 1957.

MODM.doc
Version 27/03/01

No record of my father having this published. Still useful? Put on Web?