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Reminiscences of Forty Years as a Mathematician

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The Committee of the American Mathematical Society charged with assembling historical volumes on the occasion of the Centennial very kindly invited me "to write some kind of autobiographically oriented historical article for inclusion in a Centennial volume." The present article was written in response to that invitation.

I give my personal views of some trends and events of the past 40 years. It is my hope that this will give younger mathematicians and perhaps also a future generation some insights into the conditions and events which molded our generation.

1. My early development and education as a mathematician

Although none of my ancestors were mathematicians or scientists, I developed a strong liking for mathematics early in life, certainly before entering high school. From that time on, it was my first choice for a future vocation.

The high school I attended in the middle 1930s was in some ways better and in other ways worse than those of today. Although it was a large high school in terms of the total number of students, the curriculum was rather narrow and rigid in comparison with present day secondary schools. For example, there was no possibility of taking a calculus course in high school then. On the other hand, the high school faculty occupied a more honorable and elite position in the society of that day, and on the whole they were very competent and devoted to their profession. There were relatively more nonskilled jobs available in that era, so nonacademically inclined students did not feel so much pressure to stay in school. The proportion of students who went on to college was much smaller than today.

The mathematics curriculum in colleges then was more slowly paced than today. In the freshman year, it was standard to take courses called "College Algebra," "Trigonometry," and "Analytic Geometry." Only in the sophomore year, after passing these three courses, did the student normally begin the study of calculus. Fortunately I had taken these three freshman courses at my high school, and had studied the calculus on my own from a library book, so I was permitted to take the standard course in differential equations while still a freshman. Of course I became a mathematics major; among the courses I took for the major which are no longer offered today were "Theory of Equations" (*not* Galois Theory) and "Solid Analytic Geometry." Apparently mathematics departments did not feel the need to push their undergraduates to the more advanced topics as fast as they do today.

At the end of my sophomore year I transferred from the small college in my home city to the University of Chicago. This was the heyday of the presidency of Robert M. Hutchins at Chicago. He gained great fame (or notoriety) by abolishing football at the University in 1939 (Chicago had been a member of the Big Ten Conference). Of perhaps more importance, he introduced many reforms in the college curriculum and academic life, a good proportion of which have become the accepted practice today. For example, class attendance in college classes used to be compulsory, and it was the custom for the instructor of each course to carefully take the attendance at every class meeting, and record who was absent. Hutchins introduced the then revolutionary idea of making class attendance purely voluntary. Needless to say, most of the reforms (but not all) had a positive effect, and the University of Chicago in those days was a very exciting place intellectually. I have always considered myself very fortunate to have been a student there.

By the time I received my B.S. degree in mathematics in June of 1941, World War II had already been going on for almost two years in Europe, and it was virtually certain that the United States would eventually be involved. I had been able to take a number of graduate courses on the way to my bachelor's degree, so I was able to complete the requirements for my M.S. degree at Chicago by March 1942, just before I went on active duty in the U.S. Navy. My service in the Navy was destined to drag on for almost exactly four years; now, more than forty years later, it seems almost like something that happened in another life, or on another planet.

After the War, I returned to the University of Chicago, my financial support guaranteed by the famous "G.I. Bill," with the intention of studying for a Ph.D. in mathematics. Little did I know the changes that were in store there.

To explain these changes, it is necessary to go back in history a bit. The University of Chicago was founded in 1893, and its Mathematics Department quickly became one of the most eminent, if not the most eminent, in the entire U.S. However by the time World War II started, the Department had clearly declined from this lofty position. After the War the Dean of the Division of Physical Sciences decided to make a determined effort to turn things around and to regain this former distinction in mathematics; he hired Professor M. H. Stone of Harvard University as the new Chairman, and apparently gave him *carte blanche* in his efforts to improve the Department. The rest is now history; Stone did his job, probably beyond that Dean's wildest dreams. It can very reasonably be argued that during the mid and late 1950s, the University of Chicago regained its former top rating. Seldom does one see such a dramatic change at an American University!

While such a change was clearly very beneficial for the University of Chicago, it was quite the opposite for my own career. By 1947 I was already 27 years old, had spent four years in the service, and was eager to write a Ph.D. thesis. But most of the faculty members then present were either planning to retire soon or resign and go elsewhere so that M. H. Stone could make some of his new appointments. Indeed, there was a period of several years when there were very few or no Ph.D.'s in mathematics granted at Chicago due to the enormous turnover in the faculty.

At this stage John Kelley, an assistant professor who had just accepted a position at Berkeley, took pity on my plight and wrote a strong letter to Norman Steenrod at Princeton urging him to take me on as a Ph.D. student. It was then the summer of 1947; Steenrod wrote back that applications for admission to the graduate school at Princeton were due several months previously, but if I was willing to be a Teaching Assistant, I could be admitted at that late date. Due to the great number of World War II veterans who were returning to colleges and universities there was a general shortage of experienced teachers.

The Princeton Mathematics Department at that time was smaller than now, but every one of the tenured faculty members had a world wide reputation. For example, when I took the reading examinations in foreign languages, my examiner in French was Claude Chevalley, and in German was Emil Artin. The Committee for my oral examination (the general qualifying exam) was S. Bochner, W. Hurewicz,[1] and N. Steenrod (my thesis advisor). It was Steenrod's first year at Princeton; he had just arrived from Ann Arbor, Michigan. Thus I was his only Ph.D. student and did not have to share his time with several others, usually the case for his students in later years.

The Mathematics Department at Princeton was then located in the old Fine Hall, which was surely one of the most comfortable and agreeable buildings

[1]Hurewicz was a Visiting Professor from MIT that semester.

that ever housed a mathematics department. There was a spacious, pleasant common room that was the center of activity for the graduate students. It seems likely that some of the students learned more from the discussions and arguments in the common room than they did from the faculty lectures in the classrooms; certainly the department policies strongly encouraged the idea that the students should learn from each other.

The reader will readily understand from what I have said so far that World War II must have had a profound effect in many ways on my generation. But it must also be emphasized quite strongly that the Great Depression, which started in 1929, had perhaps an even greater effect. The economist John K. Galbraith made this point quite clearly:

> Measured by its continuing imprint on actions and attitudes, the depression clearly stands with the Civil War as one of the two most important events in American history since the Revolution. For the great majority of Americans World War II, by contrast, was an almost casual and pleasant experience. **[5]**

These two consecutive world-wide cataclysms affected different people in different ways, and by different amounts, but everybody was affected. In order to understand the development and history of twentieth century mathematics, they must be taken into account.

There was one other national trauma which had an effect on American mathematicians of my generation: the McCarthyism of the late 1940s and early 1950s. For a few mathematicians, the effects of McCarthyism were absolutely devastating. One of the most brilliant of my fellow graduate students at Princeton, who got his Ph.D. degree in the late 1940s, was unable to get a job for several years during the 1950s. Nobody ever accused him of being a Communist, or of even being subversive, but no institution dared to hire him because of the politics of his father. Later, after McCarthyism died out, he became a full professor at one of our most prestigious universities. However, most of us survived the McCarthy era with minimal damage. But the events of that period reminded us vividly that anti-intellectualism abounds, and that the academic world is always dependent on the good will of the proverbial "man in the street" for its survival.

By contrast, the extreme student activism of 1968–1970 was a temporary movement and passed away in a few years. The main lasting effect on the colleges and universities of America was to force them to improve teaching on the undergraduate level.

2. Changes in the Research Environment Since I Received My Ph.D.

One of the principal causes for a change in the research environment is rather obvious, although it is not discussed very often: the great increase in the number of active research workers in mathematics. I have no hard statistics on this, but from a few bits of evidence I would guess that the increase since World War II has been by approximately one order of magnitude, i.e., approximately a 10-fold increase. Whatever the exact numbers, the main effect is the greatly increased speed with which the research frontier is being pushed back. Nowadays if there is an important breakthrough in some area, there is a horde of bright young mathematicians waiting to pounce on it, to exploit all the consequences, and solve all the reasonable problems that are opened up. It is quite possible that after a few years there will be nothing left to do in such an area except to write up a nice expository account, and then wait for another major breakthrough.

By contrast, I can imagine that in the early years of this century there would have been very few mathematicians available to work out the consequences of any major breakthrough. Those who did choose to work in the area of such a breakthrough could proceed in a more deliberate manner, without much competition, and they could count on fruitfully spending a substantial portion of their career in this one area.

Obviously this imposes an additional burden on a research mathematician; if he wishes to remain genuinely productive, he may have to change to a new field of research several times during his career. As research is being done at an ever faster rate, it will generally become more difficult to learn all the facts and techniques of a new field which are required to do research at the frontier.

Another effect of this great increase in research mathematicians is, and will be, the unwitting duplication of research in different parts of the world. What I have in mind here are cases where mathematicians in different places, working in complete ignorance of each other, end up doing essentially the same research. They may start from a different point of view, or even from a different area of mathematics, working with different techniques. Of course communication and travel are much easier and better now than they were before World War II, but there is no way that the individual mathematician can keep up with what all the other mathematicians of the world are doing.

Possibly each reader will have his own favorite example of this phenomenon. One of the most famous examples is the parallel and independent development of gauge theories and Yang-Mills fields by physicists and the theory of connections and curvatures in principal bundles by mathematicians (for an interesting discussion of this, see the book review by M. E. Mayer [**6**]). In some ways this is not a good example, because mathematics and physics are distinct, separate disciplines.

The following example is from my own experience. In 1961 J. F. Adams [**1**] gave a rather simple definition for a *right* action of the Steenrod algebra on the mod p cohomology of a closed manifold (the usual action of the Steenrod algebra is a *left* action). This idea was used in an essential way by E. H. Brown and F. P. Peterson in their brilliant work in 1963–1964 on the relations among characteristic classes of a differentiable manifold (see [**3**] and [**4**]). Working independently in China in 1963, Yo Ging-Tzung defined a sequence of endomorphisms $Q^0, Q^1, Q^2, \ldots$ which operate in the mod p cohomology of a closed manifold (see [**7**]). Yo used these cohomology operations to study imbeddings and immersions of manifolds. In the 1970s, my Ph.D. student, David Bausum, made essential use of these cohomology operations and various other results of Yo in his thesis [**2**]. After his thesis was accepted in 1974, Bausum noticed that Yo's operations Q^i were really special cases of the right operations of the Steenrod algebra due to Adams and used by Brown and Peterson; for example, if u is a mod 2 cohomology class in a closed manifold, then

$$Q^i u = u\, Sq^i.$$

To the best of my knowledge, nobody noticed this before Bausum, and I have never seen it mentioned in print. Yo wrote his formulas somewhat differently from Adams, Brown, and Peterson, used a different notation, and had a different goal in mind.

Another factor causing changes in the research environment has been the fluctuating level of financial support by the Federal Government for mathematical research. Before World War II, federal support of mathematical research was a set of measure zero. Immediately after the War, the Office of Naval Research started such support on a low scale (I was the lucky recipient of an ONR postdoctoral research assistantship for the years 1948–1950, which supported me to the tune of \$3500 per annum, a tidy sum in those days). In the early 1950s, Congress established the National Science Foundation, and it started making research grants. I remember that Professor A. A. Albert of the University of Chicago felt it necessary to send around a letter at that time urging mathematicians to apply for a grant; the whole idea was so new that nobody was quite certain what to do, or what the result would be.

The amount of money available for NSF grants increased rather slowly until 1957. In that year the Soviet Union launched the first successful artificial satellite, the famous "Sputnik". It had been generally assumed by the American people that the US would be the first to achieve any such technological feat, so this event caused great consternation and soul-searching. The prevailing cold war propaganda had pictured Russia as a crude, backward, undeveloped country that was incapable of such a thing. In this view, it seemed that this backward country had suddenly gotten ahead of us. Thus there was a great hue and cry to the effect that we had to "catch up with the Russians." Scientists and engineers of all kinds jumped on the band wagon and urged greatly increased Federal expenditures on scientific education and research of all kinds. For a while it almost seemed as if there was no limit to the amount of Federal funds that would eventually be available.

But of course such a situation cannot continue long, and toward the end of the 1960s the Vietnam War and other factors straining the Federal Budget led to a decrease in government support for science at all levels. The cutback came rather quickly, and seemed quite painful for a while.

In hindsight, the events I have just described seem almost like a fable with a moral attached. In the early and middle 1950s, nearly everybody in the academic world understood that the popular picture of Russia as a very backward third world country was far from true. All mathematicians knew of outstanding work over a period of many years by Russian mathematicians, for example. Then the success of the Sputnik did not mean that the Soviet Union was suddenly light years ahead of us technologically, and that we had to catch up. The fact that we mathematicians joined in demanding increased funds for research to "catch up with the Russians" had as an unspoken corollary the possibility that as soon as the public perceived that we had "caught up with the Russians," there would no longer be any point in such expenditures, and they could be stopped. The fact that there may have been some legitimate reasons for increased government expenditures for scientific research and education became irrelevant in such a scenario.

In more recent years the question of the Federal Budget in general, and the amounts that go for any particular item such as scientific research, have become a hostage of the great political debates of our time. Such powerful political and social forces seem to be at work that there is very little that mathematicians in particular or scientists in general can do about these questions.

A third factor that has had some effect on the research environment has been the great fluctuations in the relations between supply and demand in the job market for mathematicians. In nineteenth century England some of the best scientific work was done by men who were amateurs, in the sense that either they didn't have any job (i.e., the landed gentry) or they had a job that had nothing to do with science (such as a clergyman). But in twentieth century America it is almost absolutely necessary to be a professional in order to be able to do effective research as a scientist or mathematician. At times when the supply of Ph.D. mathematicians has exceeded the number of jobs available, it has meant a loss of mathematicians to other professions. Of course one can make the argument that there have always been *some* jobs available, so that presumably the most able and talented mathematicians can stay in the profession (and these are precisely the ones who can make the greatest contribution). But this argument is not entirely convincing. For one thing, in some cases it may be actually some of the most talented who decide to leave the profession for more lucrative jobs, because the jobs available in mathematics are not too secure or well paying. Moreover, some of the most talented young mathematicians may also have the most abrasive personalities, and thus have trouble getting a job for that reason. Finally, at any given time, the question of the availability of plenty of good jobs probably has a profound effect on the number and quality of students who want to start graduate study to work for a Ph.D. In any case, there *have* been great fluctuations in this relation between supply and demand, with many repercussions on the profession generally. In the early 1960s there were plenty of jobs available for everybody. In the late 1970s the situation was reversed and many Ph.D.'s must have left mathematics. It would be interesting to see a concise yearly summary of statistics on these changes; there must be data available in the files of the American Mathematical Society and elsewhere.

In the preceding paragraphs I have discussed in rather cold, unemotional terms the fluctuations in government support for mathematical research and the supply and demand of jobs for mathematicians. It must be strongly emphasized, however, that these changes had a profound effect on the morale and outlook of mathematicians in general, even those holding tenured positions. In general, the periods when there were adequate government support of research and plenty of jobs available tended to be times of euphoria and optimism. When the federal funds were suddenly cut back, or when our Ph.D. students had great difficulty getting a job, everybody's morale suffered.

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