Concerns of Young Mathematicians

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Ten lessons I wish I had been taught

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Allow me to begin by allaying one of your worries. I will not spend the next half hour thanking you for participating in this conference, or for your taking time away from your work in order to travel to Cambridge.

And to allay another of your probable worries, let me hasten to add that you are not about to be subjected to a tale of past events, similar to the recollections that I have been publishing for some years, with a straight face and with an occasional embellishment of reality.

Having discarded these two possible topics for this talk, I was left in dire want of a title. At last I remembered an MIT colloquium that took place in the late fifties, it was one of the first colloquia I attended at MIT. The speaker was Eugenio Calabi; in the audience, sitting in the front row, were Norbert Wiener, asleep as usual until the time came to applaud, and Dirk Struik, who had been one of Calabi's teachers when Calabi was an undergraduate in the forties. The subject of the lecture was beyond my competence, and after the first five minutes I was completely lost. At the end of the lecture, an arcane dialogue took place between the speaker and some members of the audience, Ambrose and Singer if I remember correctly. There followed a minute of tense silence, as often happens at the end of mathematics colloquia. Professor Struik was the one who broke the ice. he raised his hand and said: "Give us something to take home!". Calabi obliged, and in the next five minutes he explained in beautifully simple terms the gist of his lecture. Everybody filed out with an air of satisfaction.

Dirk Struik was right; a speaker should try to give his audience something they can take home with them. But what? Over the years, I have been collecting some random bits of advice that I keep repeating to myself, do's and don'ts of which I have been and will always be guilty. Some of you have been exposed to one or more of these tidbits. Collecting these items and presenting them in one speech may be one of the less obnoxious among options of equal presumptuousness.

The advice we give others is the advice that we ourselves need. Since it is too late for me to learn these lessons, I will try to discharge my unfulfilled duty by dishing them out to you. They will be stated in order of increasing controversiality.

1. Lecturing

   The following four requirements of a good lecture do not seem to be altogether obvious, judging from the mathematics lectures I have been listening to for the past forty-six years.

   a. Every lecture should make only one main point.

   The German philosopher G. W. F. Hegel wrote that any philosopher who uses the word ‘‘and’’ too often cannot be a good philosopher. I think he was right, at least insofar as lecturing goes. Every lecture should state one main point and repeat it over and over, like a theme with variations. An audience is like a herd of cows, moving slowly in the direction that they are being beaten into. If we make one point, we have a good chance that our audience will take the beaten direction; if we make several points, then the cows will scatter all over the field. The audience will lose interest and go back to the thoughts they interrupted in order to come to our lecture.

   b. Never run overtime.

   Running overtime is the one fatal mistake a lecturer can make. After fifty minutes (one microcentury, as von Neumann used to say), everybody in the audience will turn his or her attention elsewhere, even if we are proving the Riemann hypothesis. One minute overtime can destroy the best of lectures.

   c. Relate to your audience.

   As you enter the lecture hall, try to spot someone in the audience with whose work you have some familiarity. Quickly rearrange your presentation so as to manage to mention some of the person’s work. In this way, you will guarantee that at least one person will follow in rapt attention, and you will make a friend to boot.

   Everyone in the audience has come to listen to our lecture with the secret hope of hearing their work mentioned.

   d. Give them something to take home.

   It is not easy to follow Professor Struik’s advice. It is easier to state what features of a lecture the audience will always remember, and the answer is not very pretty.

   I often meet, in airports, in the street and occasionally in embarassing situations, MIT alumni who have taken one or more courses from me. Most of the time, they admit that they have forgotten the subject of the course, and all the mathematics I thought I had taught them. However, they will gladly recall some joke, some anecdote, some quirk, some side remark, or some mistake I made.

2. Blackboard Technique

   Two points.

a. Make sure the blackboard is spotless.

It is particularly important to erase those distracting whirls that are left when we run the eraser over the blackboard in a non-uniform fashion.

By starting with a spotless blackboard, you will slyly convey the impression that the lecture they are about to hear is equally spotless.

b. Start writing on the top left hand corner.

What we write on the blackboard should correspond to what we want an attentive listener to take down in his or her notebook. It is preferable to write slowly and in a large handwriting, using no abbreviations.

Those members of the audience who are taking notes are doing us a favor, and it is up to us to help them with their copying. When slides are used instead of the blackboard, the speaker should spend enough time explaining each slide, preferably by adding sentences that are inessential, repetitive or superfluous, so as to allow any member of the audience to copy our slide, should they wish to do so. We all occasionally fall prey to the illusion that a listener will find the time to read the copy of the slides we hand them after the lecture. This is wishful thinking.

3. Publish the same result several times.

After getting my degree, I worked for a few years in functional analysis. I bought a copy of Frederick Riesz's Collected Papers as soon as the book was published. It is a big thick heavy oversize volume. However, as I began to leaf through it, I could not help but noticing that the pages were extra thick, almost like cardboard. Strangely, each of Riesz's publications had been reset in exceptionally large type. I was fond of Frederick Riesz's papers, which were invariably beautifully written and which gave the reader a feeling of definitiveness.

As I looked through his Collected Papers, however, another picture emerged. The editors had gone out of their way to publish every little scrap Frederick Riesz had ever published. It was clear that Frederick Riesz had published few papers. But what is more surprising, the papers that I had previously read in various journals had been previously published several times. Riesz would publish the first rough version of an idea in some obscure Hungarian journal. A few years later, he would send a series of notes to the Comptes Rendus of the French Academy, in which the same material was further elaborated. A few more years would go by, and finally he would publish the definitive paper on the subject, either in French or in English.

Adam Koranyi, who took courses with Frederick Riesz, told me that Riesz would lecture on the same subject year after year, while meditating on the definitive version to be written. No wonder the final version was perfect.

Riesz's example is well worth following today. The mathematical community is split into small groups, each one with its own customs, notation and terminology. It may soon be indispensable to present the same result in several versions, each one accessible to a specific group; the price one might have to pay otherwise is to have our work rediscovered by someone who uses a different language and notation, and who will rightly claim it as his or her own.

4. You are more likely to be remembered by your expository work.

   Let us look at two examples, beginning with Hilbert. When we think of Hilbert, we think of a few great theorems he discovered, like his basis theorem. But Hilbert's name is more often remembered by his work on number theory, his Zahlbericht, by his book ''Foundations of Geometry'' and most of all by his text on integral equations. As a matter of fact, the term ''Hilbert space'' was introduced by Stone and von Neumann in recognition of Hilbert's textbook on integral equations, in which the word ''spectrum'' was first defined, at least twenty years before the discovery of quantum mechanics. Hilbert's textbook on integral equations is in large part expository. It leans on the work of Hellinger and of several other mathematicians whose names are now forgotten.

   Similarly, Hilbert's ''Foundations of Geometry'', the book that made Hilbert's name a household word among mathematicians, contains little original work, and reaps the harvest of the work of several geometers, such as Kohn, Schur (not the Schur you have heard of), Wiener (another Wiener), Pasch, Pieri, and several other Italians.

   Again, Hilbert's Zahlbericht, a fundamental contribution that revolutionized the field of number theory, was originally a survey that Hilbert was commissioned to write for publication in the Bulletin of the German Mathematical Society.

   William Feller is another example in point. Feller is nowadays largely remembered as the author of the most successful treatise on probability ever written. However, few probabilists of our day are able to cite more than a couple of Feller's research papers; most mathematicians are not even aware that Feller had a previous life in convex geometry.

   Allow me to digress with a personal reminiscence. I occasionally publish a paper in that branch of philosophy that is now called phenomenology. After publishing my first paper in this subject, I felt deeply hurt when, at a meeting of the Society for Phenomenology and Existential Philosophy, I was rudely told in no uncertain terms that everything I wrote in my paper was well known.

   The same vignette was repeated more than once, and I was eventually forced to reconsider my publishing standards in phenomenology.

   It so happens that the fundamental treatises of phenomenology are written in thick, heavy philosophical German. Tradition demands that no examples ever be given of what one is talking about.

   One day I decided, not without some serious misgivings, to publish a paper that was essentially an updating of some paragraphs from a book by Edmund Husserl, with a couple of examples added. While I was waiting for the worst at the next meeting of the Society for Phenomenology and Existential Philosophy, a prominent phenomenologist rushed towards me with a big smile on his face. He was full of praise for my paper, and he strongly encouraged me to further develop the novel and original ideas presented in it.

5. Every mathematician has only a few tricks.

   A long time ago, an older and well known number theorist made some disparaging remarks on

Paul Erdos's work. You admire Erdos's contributions to mathematics as much as I do, and I felt annoyed when the older mathematician stated, in flat and definitive terms, that all of Erdos's work could be ''reduced'' to a few tricks which Erdos repeatedly relied upon in his proofs. Actually, what the number theorist did not realize is that other mathematicians, even the very best, also rely on a few tricks that they use over and over. Take Hilbert. The second volume of Hilbert's collected papers contains all of Hilbert's papers in invariant theory. I have made a point of reading some of these papers with care. It was very sad to note how some of Hilbert's beautiful results have been completely forgotten.

But it was surprising to realize, on reading the proofs of Hilbert's striking and deep theorems in invariant theory, that Hilbert's proofs relied on a few tricks that he used over and over. Even Hilbert had only a few tricks!

6. Do not worry about your mistakes.

   Once more, let us begin with Hilbert. When the Germans were planning to publish Hilbert's collected papers and to present him with a set on the occasion of one of his later birthdays, they realized that they could not publish the papers in their original versions, because the papers were full of errors, some of them quite serious. Thereupon they hired a young unemployed mathematician, Olga Taussky-Todd, to go over Hilbert's papers and correct all the mistakes. Olga labored for three years on Hilbert's papers; it turned out that all mistakes could be corrected without any major changes in the statement of the theorems. There was one exception, a paper Hilbert wrote in his old age, which could not be fixed; it was a purported proof of the continuum hypothesis, you will find it in a volume of the Mathematische Annalen of the early thirties. At last, on Hilbert's birthday, a freshly printed set of Hilbert's collected papers was presented to the Geheimrat. Hilbert leafed through them carefully, and did not notice anything.

   Now let us shift to the other end of the spectrum, and allow me another personal anecdote. In the summer of 1979, while attending a philosophy meeting in Pittsburgh, I was struck with a case of detached retinas. Thanks to Joni's prompt intervention, I managed to be operated in the nick of time and to save my eyesight.

   On the morning after the operation, while I was lying on a hospital bed with my eyes bandaged, Joni dropped in to visit. Since I was to remain in that Pittsburgh hospital for at least a week, we decided to write a paper. Joni fished a manuscript out of my suitcase, and I mentioned to her that the text had a few mistakes, which she could help me fix.

   There followed twenty minutes of tense silence, as Joni was going through the draft. ''Why, it is all wrong!'', she finally remarked in her youthful voice. She was right. Every statement in the manuscript had something wrong. Nevertheless, after laboring on it for a while, she managed to correct every mistake, and the paper was eventually published.

   There are two kinds of mistakes. There are fatal mistakes, that destroy a theory; but there are also contingent mistakes, that are useful in testing the stability of a theory.

7. Use the Feynmann method.

   Richard Feynmann was fond of giving the following advice on how to be a genius. You have to keep a dozen of your favorite problems constantly present in your mind, though by and large they will lay in a dormant state. Every time you hear or read a new trick or a new result, test it against each of your twelve problems, to see whether it helps. Every once in a while there will be a hit, and people will say: ‘‘How did he do it? He must be a genius!’’

8. Give lavish acknowledgments.

   I have always felt miffed after reading a paper in which I felt I was not being given proper credit, and it is safe to conjecture that the same happens to everyone else. One day, I tried an experiment. After writing a rather long paper, I began to draft a thorough bibliography. On the spur of the moment, I decided to cite a few papers which had nothing whatsoever to do with the content of my paper, to see what might happen.

   Somewhat to my surprise, I received a couple of letters from two of the authors whose papers I believed to be irrelevant. Both letters were written in an emotionally charged tone. Each of the authors warmly congratulated me for being the first to acknowledge their contribution to the field.

9. Write informative introductions.

   Nowadays, reading a mathematics paper from top to bottom is a rare event. If we wish our paper to be read, we had better provide our prospective readers with strong motivation to do so. A lengthy introduction, summarizing the history of the subject, giving everybody his due, and perhaps enticingly outlining the content of the paper in a discursive manner, will go some of the way towards getting us a couple of readers.

   As the editor of the journal ‘‘Advances in Mathematics’’, I have often sent submitted papers back to the authors with the recommendation that they lengthen their introduction. On a few occasions, I received by return mail a message from the author, stating that the same paper had been previously rejected by ‘‘Annals of Mathematics’’ because the introduction was too long.

10. Be prepared for old age.

    My late friend Stan Ulam used to remark that his life was sharply divided into two halves. In the first half, he was always the youngest person in the group; in the second half, he was always the oldest. There was no transitional period.

    I now realize how right Stan was. The etiquette of old age does not seem to have been written up anywhere, and we have to learn it the hard way. It depends on a basic realization, which takes time to adjust to. You must realize that, after reaching a certain age, you are no longer viewed as a person. You become an institution, and you are treated the way institutions are treated. You are expected to behave like a piece of period furniture, an architectural landmark or an incunabulum.

    It matters little whether you keep publishing or not. If your papers are no good, they will say: ‘‘What did you expect? He is a fixture!’’, and if an occasional paper of yours is found to be interesting, they will say: ‘‘What did you expect? He has been working at this all his life!’’. The only sensible response is to enjoy playing your newfound role as an institution. Thank you.

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