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 work to travel to Cambridge.

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

Having discarded these two choices for this talk, I was left without a title. Luckily I remembered an MIT colloquium that took place in the late fifties; it was one of the first I attended at MIT. The speaker was Eugenio Calabi. Sitting in the front row of the audience 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 at MIT in the forties. The subject of the lecture was beyond my competence. 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 period of tense silence. Professor Struik 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 beautiful simple terms the gist of his lecture. Everybody filed out with a feeling of satisfaction.

Dirk Struik was right: a speaker should try to give his audience something they can take home. But what? 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 discharge my unfulfilled duty by dishing them out to you. They will be stated in order of increasing controversiality.

1 Lecturing *top*

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 they are being driven towards. If

we make one point, we have a good chance that the audience will take the right direction; if we make several points, then the cows will scatter all over the field. The audience will lose interest and everyone will go back to the thoughts they interrupted in order to come to our lecture.

b. Never run overtime Running overtime is the one unforgivable error a lecturer can make. After fifty minutes (one microcentury as von Neumann used to say) everybody's attention will turn elsewhere even if we are trying to prove 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 that person's work. In this way, you will guarantee that at least one person will follow with rapt attention, and you will make a friend to boot.

Everyone in the audience has come to listen to your 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 pretty. I often meet, in airports, in the street and occasionally in embarrassing 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 *top*

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 subtly 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 notebook. It is preferable to write slowly and in a large handwriting, with 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 some time explaining each slide, preferably by adding sentences that are inessential, repetitive or superfluous, so as to allow any member of the audience time to copy our slide. We all 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 *top*

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

Academy's Comptes Rendus in which the same material was further elaborated. A few more years would pass, and he would publish the definitive paper, 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' example is worth following. 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 own.

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

Let us look at two examples, beginning with Hilbert. When we think of Hilbert, we think of a few of his great theorems, like his basis theorem. But Hilbert's name is more often remembered for his work in number theory, his Zahlbericht, his book Foundations of Geometry and for his text on integral equations. The term "Hilbertspace" 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, leaning on the work of Hellinger and 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 ofthe German Mathematical Society.

William Feller is another example. Feller is remembered as the author of the most successful treatise on probability ever written. 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 sometimes publish in a branch of philosophy 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. This scenario occurred 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 serious misgivings, to publish a paper that was essentially an updating of some paragraphs from a book by Edmund Husserl, with a few 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 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 *top*

A long time ago an older and well known number theorist made some disparaging remarks about Paul Erdos' work. You admire contributions to mathematics as much as I do, and I felt annoyed when the older mathematician flatly and definitively stated that all of Erdos' work could be reduced to a few tricks which Erdos repeatedly relied on in his proofs. What the number theorist did not realize is that other mathematicians, even the very best, also rely on a few tricks which they use over and over. Take Hilbert. The second volume of Hilbert's collected papers contains Hilbert's papers in invariant theory. I have made a point of reading some of these

papers with care. It is sad to note that some of Hilbert's beautiful results have been completely forgotten. But on reading the proofs of Hilbert's striking and deep theorems in invariant theory, it was surprising to verify that Hilbert's proofs relied on the same few tricks. Even Hilbert had only a few tricks!

6 Do not worry about your mistakes *top*

Once more let me 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 they 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 mistakes. Olga labored for three years; it turned out that all mistake scould 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 to relate 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 on in the nick of time and my eyesight was saved.

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 silence while she went 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 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 ones, which are useful in testing the stability of a theory.

7 Use the Feynman method *top*

Richard Feynman 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, although 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 *top*

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 letters from two of the authors whose papers I believed were irrelevant to my article. 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 *top*

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 occasion 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 already too long.

10 Be prepared for old age *top*

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 he was. The etiquette of old age does not seem to have been written up, 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 newly-found role as an institution.