Excerpts from "Ten Lessons I Wish I Had Been Taught," by Gian-Carlo Rota
***Notices of the AMS*, Vol 44, No 1, Jan 1997 pp. 22-25**

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.[…] The advice we give others is the advice that we ourselves need.[...]

Do Not Worry about Your Mistakes

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 the mistakes. Olga labored for three years; it turned out that all the 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 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.

Excerpts from "The Practices of Mathematicians: What Do They Tell Us About Coming To Know Mathematics?", by Leone Burton
***Educational Studies in Mathematics* 37: 121–143, 1999.**

In 1997, I undertook a study of thirty-five women and thirty-five men in career positions as mathematicians in universities in England, Scotland, Northern Ireland and the Republic of Ireland. I wanted to find out how they came to know mathematics, what their feelings were about coming to know mathematics and what career experiences they had had that might have influenced their thinking about knowing mathematics.
[...]

The mathematicians gave the following grounds for why they found collaboration beneficial:

- talking is a good way to get a problem done,
- it shares the work,
- you benefit from the experience of others,
- it increases the quantity and quality of ideas,
- you have someone off whom to bounce ideas,
- it enhances the range of skills,
- you get into areas that you might not have thought of going into,
- you learn a lot from more senior colleagues,
- under the pressure of writing up, you mustn't let others down,
- there is someone to take over when you reach a deadend,
- you share 'the euphoria' with someone,
- you feel less isolated,
- you can benefit from a novice/expert combination.

As one participant honestly admitted:

> With a collaborator we make faster moves because my stupid ideas get sat on faster. We also generate more ideas between us (Male Professor).

And others:

> I definitely have strengths and weaknesses and working with others helps to add to their strengths and compensate for your deficiencies (Male Lecturer).
>
> If you are collaborating with mathematicians, you feel much less isolated. Because mathematics is an isolating experience you often feel burnt out, dried up, you have no skills and everyone else does. When you start working with someone else you discover that there are things that you can do, perhaps better than they can, and things that they

> can do that you can't. You can draw on their knowledge without having to go through the pain of having to look everything up. It is like having a library that does its own selections for you (Female Reader).

[...]

The notion of community of practice (Lave and Wenger, 1991) became a relevant one to apply to what these research mathematicians had to say. Jean Lave and Etienne Wenger wrote:

> A community of practice is a set of relations among persons, activity, and world, over time and in relation with other tangential and overlapping communities of practice. A community of practice is an intrinsic condition for the existence of knowledge, not least because it provides the interpretive support necessary for making sense of its heritage. Thus, participation in the cultural practice in which any knowledge exists is an epistemological principle of learning (1991, p. 98).

[...]

These mathematicians seem to me to be engaged in a field which, epistemologically, is personally- and culturally/socially-related where communities of practices are recognised and sometimes criticised, sometimes treasured.[...] Within these communities of practices, there are well-established patterns of behaviour to which I have drawn attention. The most important is that mathematics is no longer seen, by the majority of mathematicians, as an individual activity and all of the reasons which they gave for collaborating can be found in the education literature with respect to utilising group work in classrooms. The experiences of these mathematicians help, I think, to emphasise the flow and inter-dependence in meaning making between the socio-cultural formulations and individual acquisition. Learning is *neither* wholly individual *nor wholly* social.

The importance of stupidity in scientific research

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Accepted 9 April 2008
Journal of Cell Science 121, 1771 Published by The Company of Biologists 2008
doi:10.1242/jcs.033340

Journal of Cell Science

I recently saw an old friend for the first time in many years. We had been Ph.D. students at the same time, both studying science, although in different areas. She later dropped out of graduate school, went to Harvard Law School and is now a senior lawyer for a major environmental organization. At some point, the conversation turned to why she had left graduate school. To my utter astonishment, she said it was because it made her feel stupid. After a couple of years of feeling stupid every day, she was ready to do something else.

I had thought of her as one of the brightest people I knew and her subsequent career supports that view. What she said bothered me. I kept thinking about it; sometime the next day, it hit me. Science makes me feel stupid too. It's just that I've gotten used to it. So used to it, in fact, that I actively seek out new opportunities to feel stupid. I wouldn't know what to do without that feeling. I even think it's supposed to be this way. Let me explain.

For almost all of us, one of the reasons that we liked science in high school and college is that we were good at it. That can't be the only reason – fascination with understanding the physical world and an emotional need to discover new things has to enter into it too. But high-school and college science means taking courses, and doing well in courses means getting the right answers on tests. If you know those answers, you do well and get to feel smart.

A Ph.D., in which you have to do a research project, is a whole different thing. For me, it was a daunting task. How could I possibly frame the questions that would lead to significant discoveries; design and interpret an experiment so that the conclusions were absolutely convincing; foresee difficulties and see ways around them, or, failing that, solve them when they occurred? My Ph.D. project was somewhat interdisciplinary and, for a while, whenever I ran into a problem, I pestered the faculty in my department who were experts in the various disciplines that I needed. I remember the day when Henry Taube (who won the Nobel Prize two years later) told me he didn't know how to solve the problem I was having in his area. I was a third-year graduate student and I figured that Taube knew about 1000 times more than I did (conservative estimate). If he didn't have the answer, nobody did.

That's when it hit me: nobody did. That's why it was a research problem. And being *my* research problem, it was up to me to solve. Once I faced that fact, I solved the problem in a couple of days. (It wasn't really very hard; I just had to try a few things.) The crucial lesson was that the scope of things I didn't know wasn't merely vast; it was, for all practical purposes, infinite. That realization, instead of being discouraging, was liberating. If our ignorance is infinite, the only possible course of action is to muddle through as best we can.

I'd like to suggest that our Ph.D. programs often do students a disservice in two ways. First, I don't think students are made to understand how hard it is to do research. And how very, very hard it is to do important research. It's a lot harder than taking even very demanding courses. What makes it difficult is that research is immersion in the unknown. We just don't know what we're doing. We can't be sure whether we're asking the right question or doing the right experiment until we get the answer or the result. Admittedly, science is made harder by competition for grants and space in top journals. But apart from all of that, doing significant research is intrinsically hard and changing departmental, institutional or national policies will not succeed in lessening its intrinsic difficulty.

Second, we don't do a good enough job of teaching our students how to be productively stupid – that is, if we don't feel stupid it means we're not really trying. I'm not talking about 'relative stupidity', in which the other students in the class actually read the material, think about it and ace the exam, whereas you don't. I'm also not talking about bright people who might be working in areas that don't match their talents. Science involves confronting our 'absolute stupidity'. That kind of stupidity is an existential fact, inherent in our efforts to push our way into the unknown. Preliminary and thesis exams have the right idea when the faculty committee pushes until the student starts getting the answers wrong or gives up and says, 'I don't know'. The point of the exam isn't to see if the student gets all the answers right. If they do, it's the faculty who failed the exam. The point is to identify the student's weaknesses, partly to see where they need to invest some effort and partly to see whether the student's knowledge fails at a sufficiently high level that they are ready to take on a research project.

Productive stupidity means being ignorant by choice. Focusing on important questions puts us in the awkward position of being ignorant. One of the beautiful things about science is that it allows us to bumble along, getting it wrong time after time, and feel perfectly fine as long as we learn something each time. No doubt, this can be difficult for students who are accustomed to getting the answers right. No doubt, reasonable levels of confidence and emotional resilience help, but I think scientific education might do more to ease what is a very big transition: from learning what other people once discovered to making your own discoveries. The more comfortable we become with being stupid, the deeper we will wade into the unknown and the more likely we are to make big discoveries.