

Fourteen Things that I Learned from “You and Your Research”

Yi Zeng

International WIC Institute, Beijing University of Technology
Beijing, 100022, P.R. China
yzeng@emails.bjut.edu.cn

Thank you for Prof. Yao recommending me Hamming’s “You and Your Research”, which told us how to be a first-class researcher and do great research. I summarized fourteen things that I may need to pay attention to. We can see that the rules are not that complex and maybe most of us know them. But the point is that I may unconsciously forget about them when I do scientific research. I may need to quote Hamming’s words “ It’s just that simple and that hard! ” [1].

Think Big and Act Small: The reason why Hamming could get the Turing award may be because he always think about great research, the so called “Nobel Prize type of work” [1]. This remember me of Prof.Zhong’s talk about “Nobel Prize” and “Turing Award” all the time, and at first I just don’t believe that we have the ability. But now I could understand him. The reason why he encourage us to think that big is that only in this way, can we set up a great goal and work very hard for it. Just like Hamming said, the problem is not really about the “Nobel Prize”, but about whether we could think big and make up our mind to pursue it. Even if we could not get the “Nobel Prize” and “Turing Award”, we at least used to work on that type of great research. Making a strong will is very important, that will help us keep our dream in mind all the time. Hamming suggests us to ask ourselves “each of you has one life to live ... Why shouldn’t you do significant things in this one life” [1], “Yes, I would like to do first-class work.” [1], “Yes, I would like to do something significant.” [1]. Through later discussions, hamming also pointed out great minds need us to work from the foundations.

Look for the Difference: When Hamming was not that famous, as he said, a “stooge”, he is always looking for the difference between those who do great things and those who have done great research. He always asked “How did you come to do this?” [1], which I may not ask, because I sometimes just admire those great people but seldom ask how to be one. This undoubtedly leads him to the way of being the one who will come out good research results.

Luck Favors the Prepared Mind: Hamming quote Pasteur’s words in his talk on “luck favors the prepared mind”. It is not the first time that I see this sentence, actually I see it everyday when I open my laptop, because it is one of the mail title that Prof. Yao gave me, and the one I use to be my desktop background. Hamming took Einstein and Shannon as examples, talking about their continuous contribution to the scientific world, which is of course not just about luck. It is their strong will to overcome all kinds of challenging difficulties that leads to their success. I would like to cite Prof. Yao’s words in one of our

mail “Once you prepared well, you will harvest the fruits, sooner or later” [2]. A prepared mind is essential for our way to success.

Hard work can make up for a lack of intelligence: After reading Hamming’s talking about Bill Pfann’s and Clogston’s stories, I got excited, because I saw someone that may have not very strong background or to some extent “not so smart”, but work out very good research results. That means through hardworking, I can overcome my weakness so that they will not hold my way to good research. What I need is just working into details and concentrate. No one is good at every aspect. Although I don’t have a strong background on Mathematics and Computer Science, but through Bill Pfann’s story, I believe hard working and getting prepared can recoup my weakness.

Work Hard When You Are Young: Hamming pointed out that Einstein’s and most quantum mechanic researchers’ best work is done when they are young [1]. Another example is Donald Knuth, he finished the first three volumes of “The Art of Computer Programming” in his early thirties. John Tukey’s story [1] also told us the importance of keeping working hard when we are young. This made me feel terrible, because up until now, I haven’t thought seriously about really important scientific questions and haven’t contribute anything which is important to the development of science. What I need to do now is to keep working hard. Later, Hamming also pointed out that continuous hard working will work out more results [1]. In another perspective, Knuth is still working on the seven volumes of the book series, which tells that we can enjoy the process of learning and doing research, which could be a life long thing.

Working Hard Is Not Enough: A quotation from Edison “Genius is 99% perspiration and 1% inspiration.” shows that hard working is not enough for scientific research. Spending more time thinking about the nature of the problem may be much more efficient than keep trying different methods to solve a problem.

Some Key Questions I need to ask myself: Hamming used to ask Dave McCall the following questions, which I think should be keep in mind all the time during my research life: “(1) What are the important problems of your field? (2) What important problems are you working on? (3) If what you are doing is not important, and if you don’t think it is going to lead to something important, why are you at Bell Labs working on it?” [1]. When I was a master student, I used to think that doing research is just to find a hole in a domain and try to fix it, which of course is wrong. Of course, doing some improvements for an algorithm is important in some field, and it maybe a starting point for our research, but it may be not the most important problems in our field, especially for we Ph.D students. What we need to do is figuring out the most important problems and choosing one of them to work on. Hamming also pointed out that if some problems cannot be solved considering current scientific progress, maybe it is not the important one for now which you need to choose [1]. Many people could figure out the most important problems for a domain, but I also need to keep in mind that what I can do at first considering my background and other

constraints. The point is that having a relatively long and important goal is essential for me in my life.

Work with the Door Open or the Door Closed: The saying “The closed door is symbolic of a closed mind” [1] changed my understanding of scientific research. During my very first year of Ph.D study, I was working on Granular Computing, and actually I am still working in this area, but later my research interests changed to Knowledge Retrieval (KR). I began thinking that those two surely related in some aspects, but they belong to totally different area. So I began concentrating more on KR, and in a period of time, I even don’t want to work on Granular Computing anymore. Later I proved myself to be wrong. Granular Computing is a unified methodology for problem solving [3], granular structures are powerful tools to represent the structure of the complex world and problems. Knowledge Retrieval is a concrete problem in intelligent system and human centered computing [5], and the trinity model of granular computing is actually the methodology for this research, especially when the relationship between knowledge structures and granular structures are considered [4]. But at first, I even think my research on Granular Computing may hold the way of my research on Knowledge Retrieval. If I closed the door of Knowledge Retrieval and don’t work on Granular Computing, it may lead to a final failure.

Three Things to Promote Our Research Results: Hamming pointed out that in order to get credit, we have to consider following three things:

“(1) Writing clearly and well to attract more audience;” [1]

Prof. Yao mentioned many times on how to write a good paper. That is we need to stand on the perspective of the reader, namely, did the readers get what we want them get through reading our paper? Is it possible that our writing misleads the readers? All of my papers are revised more than 30 times with Prof. Yao, and now I could see that even we didn’t make everything clear, we present our best.

“(2) Learning to give reasonably formal talks;” [1]

After every talk I gave about my research, I usually feel that something is not clearly presented, or something should have a better explanation. That means I didn’t present reasonable formal talks, and if some questions were asked about what I didn’t prepared well, maybe I could not handle it. Especially for a conference talk, it is much more important to present the background, motivation, general methodologies, and results. Digging into the details may not be a successful talk [1]. Mastering a good speech skill is one of the most important thing in a researcher’s life.

“(3) Learning to give informal talks.” [1]

An informal talk after meals or talks during the break of a conference may bring more audience and collaborators.

Enjoy the Process of Scientific Research: Hamming discussed whether it is worthy to be a great scientist. And the results shows that “the success and fame are sort of dividends” [1] and we need to enjoy the process and the pleasure of finding things out [6]. When I was writing the Granular Computing book, I

always feel that it was a task for me. One of my lab mates told me that when I consider it to be a task, I will never enjoy the process of writing a book.

Don't Be Ego Assertion: As a researcher, we surely need to have our own idea about things, but that doesn't mean others' opinions are not important. I sometimes feel that I need to stick to my mind, but later I may find that I should have listened to other's suggestions.

Understand Your Weakness and Your Strengths: Again, I would like to quote Prof. Yao's another letter "... you may spend sometime to think about your strength and weakness. So, our plan is to explore the strength and make up for the weakness." [7]. Hemming also pointed out that "... the successful scientist changed the viewpoint and what was a defect became an asset ..." [1].

Don't Try to Solve Problems Just by Reading: Hamming pointed out that the purpose of reading is not to get the answer but to know "what is going on and what is possible" [1]. "You read; but it is not the amount, it is the way you read that counts" [1]. Just like Einstein's saying "Imagination is more important than knowledge".

Don't Get Famous When You Are Not Ready: Hamming referred Brattain and Shannon, as well as those researchers in the Institute for Advanced Study in Princeton, as examples. When people get famous, they may don't have time to work into small problems and into details, which is bad because they may work out even greater research results if they continue working seriously in that area. People need to pay for being famous. I remember one question that Prof. Yao asked me, "Do you want to be a famous professor, or a professor that could work into real scientific problems by himself?". In that case, I think I should keep in low profile even when I have chance to get famous. The fact is that people will not remember us because we used to be famous, but because we used to leave something useful to the society.

Listed fourteen things are the ones I think very important to my future study. May be I cannot get that far, but at least I will try to follow those points and keep them in mind.

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