

On Research Taste

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On this wonderful occasion of celebrating Professor Yu-Chi Ho's 75 years birthday, I want to write a few words to show our appreciation for Professor Ho's teaching and contribution to society.

Academic Legacy

After I finished my PhD degree at Harvard (under the supervision of Professor Ho), I went to work in industry - first at Bell Labs, followed by a long career at DEC and Sun Labs. More than twenty years later, I went back to academia in 2002, to teach at The Chinese University of Hong Kong where I have remained since. As we all know, a big ingredient of university life is doing research, and the fun part of this job is that you have a great deal of autonomy in deciding what you do. Furthermore, you are in the position to influence others, your students and other colleagues in the research community, in defining what is valuable and interesting to work on. Your choices, here I loosely refer to as one's *research taste*, does not only shape your research output, but also influence others, especially your students. This is perhaps the true *legacy* left behind by a scholar's work. This revelation is particularly vivid for me after I returned to university, and started making my own research choices and working with my own students.

Research taste is more like art than engineering, it is elusive and cannot be easily enumerated or quantified. But I want to try to describe what impressed and influenced me most from working with Professor Ho. Needless to say, such a personal perspective can be quite biased and incomplete.

Practical but Strategic

Working in engineering or applied sciences, our research problems are mostly related to practical problems in the real world. But this does not always

mean a researcher *cares* about the real problems. I would only consider someone *practical* if he tries to understand the real problem; tries to avoid making unreasonable assumptions, and sometimes refuse to work on certain problems even if it is a hot topic, because by his judgement these problems are irrelevant in practice.

In the beginning of my PhD years, I remember Professor Ho introduced me to consider a bio-engineering project working with a hospital famous in curing burnt patients. If I took on that as my research direction, it would have been truly avant-garde considering the huge interest in medical-related engineering problems today. We also briefly worked on power control problems, collaborating with Professor Ho's former student working at BBN. Finally we settled on working on databases, a topic of considerable commercial as well as academic interest at the time.

I consider it a very good trait to be practical - it helps to ensure your research work has some customers. Of course, it is not necessary to be practical to produce useful research. Some people are born with talent to see what can become useful, or what is *elegant* which is intrinsically useful. But I am glad I inherited a little of this practical-mindedness in my own research taste.

For practical problems, there are different research questions. The type of research questions we tend to be interested in are more *strategic* (or systems level) issues. This is probably due to the discipline we were in - our group was called *Decision and Control*! While in industry, I tend to choose to work on system performance modeling analysis and design. Later on in university life, I also choose to work on economic type of issues (whether in networking or problems in other social settings). For example, some of my past research interest include network congestion control and pricing; some recent research problems I am studying with my students include college admission matching, and academic publication processes and recommendation systems. I think my taste for what I refer to as strategic questions is also a distinct trait that I inherited from Professor Ho.

Pioneering

I recall Professor Ho often reminded us the importance of being a pioneer in research. He explained it to us in the most pragmatic words - if you are the first one to start a field, you don't have to be perfect to get a lot of credit for your work.

In the process of supervising student research, I find that it is quite common for students to develop a habit of jumping into a "hot" research

area, and trying to publish something quickly by working on incremental ideas. They are not aware that the hot topic they found is often no longer so hot, and the basic story they are going to tell has already been told many times in other papers. On the other hand, an accomplished researcher in my department (and a good friend), Raymond Yeung, tells me that he would consciously try to run away from a field when it gets too hot.¹

Of course, how to become a pioneer is not easy. I think it requires one to identify the right problem, make true discovery, and develop the power to motivate others in your community to share your views and ideas. In Professor Ho's career, he pioneered several fields of study.

Intuitive and Insightful

In one of my regular meetings with professor Ho, he asked me to try to explain my idea more clearly. I still remember vividly how he explained the need to explain something clearly - there are not that many great ideas in the world, and besides, every idea, including the great ones, can be explained in simple terms in five minutes - I must have failed miserably in trying to utter what I wanted to say! But this insistence in explaining an idea intuitively is to get out the *insight*. Mathematics can be thought of as a language that helps us express ideas rigorously, and cover all extreme cases, but it is not always the easiest form to understand. Insight (or explaining something intuitively) can be thought of as an art that pick out the essence of the idea to make it clear.

Recently, a colleague of mine told me that one of my co-authored papers got included as someone's 10 recommended networking paper to read [1]. This was particularly pleasing to me because this recommender made his choice based on *elegance and insight*. Although pointing this out is rather self-congratulatory, but I hope that this serves as an example of Professor Ho's legacy, and the fruit of trying to be practical but strategic, to be pioneering, and to be intuitive and insightful in one's research.

References

- [1] V. N. Padmanabhan, "10 networking papers: Elegance and insight," *Computer Communications Review*, 2008.

¹Of course, sometimes it is not so easy to leave a field if it is something you started, as the university would expect you to get more grants using your fame.