

Reflection of Some Aspects of My Development

First, I would like to thank S.Y. Cheng to initiate such a wonderful "family affair" for my retirement. I like to thank the organizers and all the participants, especially for Professor ZHENG Zhemin and HSIANG Wu-chung who has come a long way to participate this activity.

Professor Zheng and Professor Hsiang are associated with some important aspects of my development.

First Professor Zheng. In 1979, he invited me to give one month summer course at Institute of Mechanics of the Chinese Academy of Science on Asymptotic Methods, thus starting my interactions with the Chinese academic world. Out of the subsequent 12 years before 1990 when I came to Hong Kong, I went back to China on lecture tours in eight years. Twice I spent almost half-a-year in China.

I think the first time I met Professor Hsiang was the summer of 1969 when we both went back to Taiwan to lecture. But our association dated back much earlier.

He is associated with three important aspects in my development.

The first is the founding of the Yung Yung Society (堦堦堦) at Taiwan University in 1953. The society was the only comprehensive student society free from KMT control at the time of white terror in Taiwan. Amazingly the Society lasted over 20 years. The experience and the chance to make many friends had a profound impact to my development. I would consider that experience was the most important education I got in Taiwan University. Wu-chung was once an inactive member of the Society.

The second important aspect is the Tiao Yu Tai Movement, 堦堦堦堦堦堦. Besides broadening my world views, I also got to make many more friends, mostly younger friends. Wu-chung was also an active member in the movement. We had worked together and we got to know each other better.

The third important aspect is HKUST. When Chia-Wei Wu and C.Y. Chien invited me to join HKUST and asked me to build the Department of Mathematics, I hesitated. I am not really a mathematician and I have never dealt with pure mathematicians professionally before. I called Wu-chung and asked his advice. His advice was very encouraging. He said: "The important thing to be DH is that: 1. to serve the people. 2. do not block young people's development. Other things are less important. When you are over 50, your peak in research is over anyway." I took his advice and took the job, and I have tried to make every effort to facilitate the development of my young colleagues.

Today, close to the end of my professional career, I should say my development is definitely not a role-model for my young friends.

First of all, although I am a professional scientist, I have probably spent only half of my time on Science. Wu-chung has criticized me as being lazy. He has a valid point. I have been indeed somewhat lazy on Science. But I have not been lazy as an intellectual. Therefore, I can only say I am lucky that I can still survive in this tough academic world.

My academic career has been tortuous. I have never dreamt that I would be the head in a department of mathematics.

I met a high school class mate on my recent trip to Kunming. He said that he thought I would pursue a literacy career. But to pursue a career in humanity or social science in Taiwan of 1950 is dangerous unless you were willing to be a loyal party member. Purely for survival, I chose Civil Engineering, the least technical discipline in engineering and science.

After graduation from Taiwan University, I got a fellowship to study at Brown University which I had never heard of before, and it was practically applied for me by an elder brother of mine, who was then in US.

My master thesis was on experimental solid mechanic, the dispersion of waves in an elastic bar. My advisor was Professor Kolsky, then a visiting professor from England. We used the so-called Hopkinson bar. Later some people also called it Kolsky bar. We used the explosive lead azide to produce the impact. I had to prepare those explosive pellets in an unfinished basement lab. After each bang, I had to develop the photographic film and wait more than half hour to see whether the result was good or bad. At that time the polaroid camera was not available yet.

Kolsky was visiting professor for only two years. So I had good excuse not to continue for PhD study at Brown. I would like to see more of the America. So I chose to go to Caltech after getting my master's degree, although I also got fellowships from Harvard and Stanford. On my way from northeast to southwest, I stopped half-way in mid-west and got a summer job at the Indiana Highway Department in Indianapolis. I managed to design a pier of a highway bridge for that summer.


An important factor in choosing Caltech was to follow the footsteps of H.S. Tsien (堦堦堦堦堦). He was the idol of most Chinese engineering students then. (Yang & Lee have not got their Nobel Prizes yet). I chose the Department of Engineering Science so as to be able to learn more physics and mathematics. It happens that this new department was also the brainchild of Tsien. But when I got to Caltech in 1957, he had left already. The Engineering Science Department was led by Professor Plesset, and my fellowship, the highest paid then at Caltech, was also administered by him. So I just chose him as my advisor, and switched from experimental solid mechanics to theoretical fluid mechanics. The area of study is the dynamics of bubbles in liquid.

The PhD thesis work usually has a very important influence in later career development. People still associate me with bubbles even though most of my works for the last thirty years are not on bubbles. Of course, I have kept on eye on its development, and once for a while I may write a paper related to bubbles. On the other hand, my later works in some sense are indeed related to bubbles because bubble dynamics has a very rich content.


First of all, a bubble in a liquid is marked by an interface between two fluids, a liquid and a gas. Thus it is related to all the interfacial problems, for instance, water waves. It has stability problem for the bubble shape. Thus it is related to all kinds of interfacial stability problems like Rayleigh-Taylor, Kelvin-Helmholtz stability problems and the Faraday problems. For vapor bubbles, there are problems of heat and mass transfer and phase transition. The collapse of bubble has analogy in laser fusion. The nonlinear oscillation of bubble leads to chaos. There are also bubbles in superfluid liquid helium. And these are the areas I have later explored.

My early academic advancement was quite smooth. After my PhD degree, I stayed as a Research fellow at Caltech for three years, then became an Assistant Professor of Engineering Science. I should mention that at that time Caltech seemed to be particularly hostile to Chinese on faculty appointment. For many years, although Caltech had many Chinese research staffs, only two persons were regular faculty members: Y.C. Fung and T.Y. Wu. I felt really excited then to become an Assistant Professor (Sunney Chan joined Caltech as assistant professor about the same time). My salary was the US\$10,000/yr. and I was also consulting at JPL one day per week for 100 dollars per day. With total amount of income of \$15,000/yr. in 1963, taking into consideration of inflation, I think that was the period of the peak of my earning power in my life.

Academically, it is also around this time, I started to become more mature and truly independent. Previously, I carried on just because my advisor suggested this or that problem, or by studying someone's paper and saw that I could extend the work in some direction. I did not have a feeling why should I do this. It was about this time I was asked to write a survey article on bubble dynamics, I started to take a look on the entire field of this subject. After I had finished my work, not only I had gained a clear idea what were the importance behind each subject of study, what were the tools needed to solve these problems, but also knew that what were the important problems still remain unsolved. I knew then that I had become one of the very few experts in that area. That is why I often suggest to my young friends to write a survey article at certain stage. Otherwise you may not know why you are doing this research except that some other people have done similar problems.

Then, came the set-back of my academic career. After 3 years of Assistant Professorship, came the contract renewal time. Normally, it should be the time for promotion to Associate Professor (I had never heard about "tenure" then. So naive!) Instead, they just gave me an extension of contract of three more years. I was very upset. Even though Professor Plesset told me I should not over-react, I knew that I had to leave. Later I learned that the reasons the committee members gave were: 1. They did not know me very well, because I do not interact and socialize with them. 2. They started to try to avoid  keeping their own graduates. So far because of the predominant eminence especially in aeronautics and fluid mechanics, many faculty members were Caltech products. 3. They had too many fluid dynamists. Out of a little over 200 regular faculty member in Caltech, there were then 33 fluid dynamists scattered in various departments.

Many of my young colleagues are facing the substantiation review now or soon. Not every one will be successful. I would like to share my experience with you. Do not despair. It might be a good thing. It will force you to reflect on what is important in your life. When you have been smooth sailing, you tend to think this career is the only kind of life in the world, and this place is the only place suitable for you to live and work. It is not so. The world is much bigger and life offers a vast vista of opportunities. Looking back, I think it is good thing that I was forced to reflect on my future directions. Moreover, to stay in one place for 7 or 8 years is probably long enough. One should start to move to a better or different place. The important thing is to have made those years fruitful and pleasant.

The following year, I managed to take nine months leave from Caltech to visit Professor C C Lin of MIT. I intended to learn the astrophysical problem of galactical dynamics from him. Although I learned a lot, I did not pursuer this direction of study. However I gained much more in other aspects. Even though Caltech was a great place, however being there for almost ten years, you became a , frog at the bottom of a well. You tended to think that was the entire world. When you got out, you realized other people do not think the place is that great. There are other important people and important things. I had lunches with Professor C C Lin three or four times each week. I got to know his philosophy, his way of thinking, his method of dealing with scientific and other problems. It was most beneficial to me.

I went back to Caltech for one more year, then moved to Brown, in the Division of Applied Mathematics. Thus formally becoming an applied mathematician. I was so ignorant then that I did not know any thing about tenure. So I did not bargain about the tenure at the time of appointment, even though all the other associate professors had tenure. I had to go through another tenure review two years later.

The set-back in my career led me to reorient my priority in life. I made up my mind that I was going to have my own agenda in life, not to be a prisoner of the academic position of certain institution. Freedom is more important. However, these set-backs of course were very upsetting and irritating. It is extremely important that you have an understanding and supportive wife, that I fortunately have. You have ups and downs, you make this plan or that, you become irritable and impatient. And your family suffer in the dark.

My former advisor Professor Kolsky had come to Brown permanently before I went to Brown in 1968. I sort of repay back his education to me, by doing my last experimental work in solid mechanics in 1970. Kolsky was a top experimentalist in the style of GI Taylor. In early 1980's, a Chinese visiting scholar came to Brown to study with him. He was greatly disappointed that Kolsky's equipment was not as fancy as the equipments in Shanghai. I told him that is exactly the greatness of Kolsky. Simple equipment to produce first class results.

I said that I was not a role-model for young colleagues. I was not on another aspect. I am not good in writing research proposals and I do not go to Washington to wine and dine with agency program chiefs. But it may not be just coincidental that I got my tenure and promotion exactly in those years I happened to get some grants. As Department Head, I was bound by my duty to encourage my colleagues to write proposals and to get grants. But I hate it myself. Of course there is a price you have to pay. Your salary increase would be lower than your colleagues and you may not have the summer salaries. For me, this gave me the full justification to spend summer months in China. We had no house,

partly because we were poor. Here again, you need to have a supportive wife.

Then in 1990, we came to HKUST. It has been a most rewarding period for me. Not only that I came to know you, almost all of you I did not know before, but also it has been one of the most productive years for me, despite heavy administrative duties. I managed to have 6 books published, and together with colleagues and students, have made good progress in two original areas of study. We also worked together to set up the various options for undergraduate studies, its implementation and success are heart warming to me.

There are not that many departments of mathematics which house under one roof number theorists, statisticians and fluid dynamists. I think it is a good thing. But it is also difficult. It requires mutual respect and tolerance of difference in values and standards. We have so far been fairly harmoniously together. I hope we shall continue to be like this, hopefully even better.

This symposium, to have some of the colleagues to present their recent achievements, is great. This time it is for the sake of my retirement. I would suggest that it can be a regular annual affair. To have one third of colleagues plus one or two visitors to present their works each year. Furthermore, the material of presentation could be written up in the form of survey articles and then issued in printed form as an annual departmental publication. If we can maintain a high standard, it could become another Communication for Pure and Applied Mathematics.

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