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Steven N.S. Cheung's Reminiscence of Himself – A Reply to Ning Wang

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1 Introduction

It is not correct to say that I was a C student in school in Hong Kong. I did not pass even the first grade in high school. Later, in 1957–1959, I tried to make up some ground by attending high school classes in Toronto. There, I met a man by the name of Jason Wong. Knowing that no university in Canada would accept me, Jason helped me apply to institutions in the US. Thus it was that I entered UCLA in the fall of 1959, as an old-undergraduate of nearly 24. They had a lenient acceptance policy for the overage. It is one of my deep regrets that I did not see Jason again. I met his brother Jeffery some seven years ago, only to learn that Jason had passed away. I owe Jason enormously, because he believed in me: That over-aged and university-reject I may be, if given the opportunity I would surpass all the “properly-brought-up scholars” he knew.

I am sorry that I seldom communicated with Jason during my UCLA-Chicago-Seattle years. Then, I had no time for anything but study and research, research and study. My last contact with Jason was in 1982, when I sent him my just-published pamphlet, *Will China Go Capitalist?* He replied that it was from the hand of a grand master, and that should my prediction prove accurate, my name would go down in history.

2 Growing up in the wild

As a boy, I was never a lag-behind: Indeed, I excelled at almost everything I found interesting. But I was unlucky. Although born into a well-to-do family, when I just turned six the Japanese invaded Hong Kong, and in 1942 my mother was forced to take seven of her ten children into Mainland China. We were refugees and starved for three long years. Here and there my mother managed to put me in a school, a month or two at a time, for whatever grade and in

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whatever classroom that had a place for me. In 1944–1945, for one whole year I did not get to eat a single bowl of rice, slept on the floor in a mud hut, wandered around in the wild, and stole from the poor peasants to feed my younger sister and myself.

At that time, my arms and legs were rotting due to malnutrition. My mother was told that there was no chance my younger sister and I would survive. But she was a courageous woman, and decided to let me go hunt for food with my sister on my back. It would be better to take that chance than to give up and die. Miraculously, we survived.

Two things made life bearable during that *annus horribilis*. In the small and poor village there was no pen and paper, but there was a refugee who was a Classical scholar. This man had with him several volumes of Chinese literature, and whenever I scavenged enough straw and tree branches to feed the fire he would read the works out loud by its light. In this way, I memorized hundreds of Chinese poems and essays. The result is that when I began to write in Chinese in 1983 my style of blending modern and classical Chinese was already well honed, even though I did not remember how to do many of the characters because I learned classical Chinese by hearing and not by reading. That a chair professor had to hire an editor to check the use of wrong characters was grist for Hong Kong gossip mills for years. (That did not deter some universities in China today from assigning my prose writings for their literature classes!) I should add that in the process of re-learning the characters, I decided to try my hand at calligraphy. Though I began late, at age 55, my brush works are selling in the auction houses. I sell them for donation. Not expensive, but they sell.

The second thing was that after a year wandering around the fields, I gained hands-on knowledge of Chinese farming. In the fall of 1966, when I used Chinese agricultural data to test sharecropping hypotheses (Cheung 1969a), I knew so much and was able to interpret the numbers with such insight and imagination that Armen Alchian, Gale Johnson, and Harry Johnson all joined in warm applause. (Gale was so impressed with that he asked me to teach a course in agricultural economics at the University of Chicago!) This material was ultimately published as Chapter 8 of *Theory of Share Tenancy* (Cheung 1969b). I could not have written that chapter without starving in a poor Chinese village.

3 Failure in classroom and success in the street

From 1945 to 1954 I attended three schools, one in a small town near Canton and two in Hong Kong, all old and reputable establishments. I did poorly in all three.

However, in each of these schools there was one teacher who ignored my failures and told me that some day I would go far. After I returned to Hong Kong in 1982 to assume the Chair of Economics at the University of Hong Kong, I managed to arrange and meet two of these old teachers, thanking them for their encouragements when I was failing thirty years earlier. When I finally found the whereabouts of the Canton teacher in 2005, I was told he passed away in 2003. I owe this man, because when he failed me in the spring of 1948, he also told me that someday, somewhere, I would be noted for my thinking surpassing all the scholars he knew. Before my departure, he took me to a quiet corner on campus, asked me to sit with him on a bench, and explained to me that I failed because I thought so strangely. He said no one around could teach a student like me, including him. He speculated that someday, somewhere, I might run into someone who could. It was turbulent time, and people were running because the communists were coming to Canton. I, too, left Canton for Hong Kong in 1948. I was 12.

My failures at school may well be offset by my successes in the street. I was Chinese checker champion in Canton at the age of 11, and by 15 for pocket money I played Chinese chess blindfolded against several opponents at the same time. In 1952 I taught a younger school dropout the game of table tennis, and he later won the world championship in Hungary in 1959. In Hong Kong I ruled the sea in fishing, the ground in marble playing and coin throwing, the rooftops in kite flying, and the hills in bird catching. In 1955, at 19, I picked up a camera and on the first day of trying I produced two works that won prizes and exhibited internationally. Only at school I failed. But because of my successes in the street I never considered myself a failure and wondered what I should do for a comeback.

4 Encouragements from my father

My father had only two years of schooling. He wrote Chinese and English beautifully by self-learning. A quiet man, firm on his principles, he was a successful businessman respected in the early stage of industrial development in Hong Kong and was given the honor as the father of electro-plating in that city. As the head of a large family he seldom talked to me when I was growing up. He died in 1954, the same year I got dismissed from school. I was 18.

About two months before my father passed away he summoned me to his room in the hospital and gave me his first serious father and son talk. Only the

two of us. He said he knew all along that I did poorly at school, apologized that he had neglected me because he was occupied in his business. He said he had given up hope on me, but changed his mind. He said for about a year he invited people who knew me to come to visit him, talked about what I did, and he reached the view that I was the most promising young man he had ever known.

At the end, he said, “Doing poorly at school is not the end of a career. The doctors told me I only have two or three more months to live. After I am gone I want you go to my business firm and learn, wait for another chance to get yourself a good education. You must remember I respect a scholar more than any other profession.”

This another chance came three years later, when I took a business trip to Toronto to negotiate with an exporter of nickel anodes subject to import restriction in Hong Kong imposed by the United States. We wanted to find a way to get around that restriction. In only two days I came up with a solution agreeable to the Canadian exporter. But then I decided not to return to Hong Kong, implicitly surrendering my right to inherit a share of my father’s business in exchange for \$100 Canadian a month of financial support. In Toronto I was floundering for not knowing what to do for an education. One year later I met Jason Wong. I was 22.

5 Why I failed at school

As an old man now and recalling the past, it is not difficult to explain why I did so poorly during my early schooling period. Two reasons stand out. First is I got used to run around in the wilderness during the war, and continued to do so when the war was over. I liked to skip classes, to wander alone in the fields or to sit by the seashore, fishing, pondering, or doing nothing. And there were the street games when other children showed up. In other words, when as a boy I was wild. A second reason is that whenever I asked a question in class in Hong Kong, a teacher would often punish me for asking what he considered as an irrelevant question. Most teachers were hostile to me. This was a sharp contrast with what happened in the first night I audited Jack Hirshleifer in the fall of 1962. I raised a seemingly irrelevant question. Jack stood up and asked for my name. When he could not find it on his student list because I was an auditor, he wrote down my name carefully, making sure he got the spelling right, and then told his colleagues that he found a most unusual student from China.

6 From Bill Allen to Armen Alchian

In UCLA, my first major was business administration. But I found accounting boring. On the other hand, Bill Allen, who taught Econ101, was such an entertaining and inspiring teacher that I soon shifted over to economics. At 24, I was old compared to my classmates. I knew I had to step on it. I took as many as five or six courses from Bill. Warren Scoville, my professor in economic history, inspired me to think about graduate work. He did not gush as Jason Wong did about my academic prospects, but Warren made sure I was always within earshot every time he remarked that if I tried for a PhD I would go far.

Some years later I learned from a colleague in Seattle that Scoville and Alchian were not close friends at UCLA. However, it was Scoville who told me that if I decided to do graduate work I should go for Alchian. He said Alchian was one of the best economists in the world. So it was UCLA and nowhere else when I graduated in 1961. Armen was visiting Stanford at that time, so I had to wait for him to return. My original plan was to obtain a master's degree and go back to Hong Kong, but when the MA happened in 1962 Armen still had one more year away. I therefore decided to go for the PhD, and waiting for Armen I spent one year auditing, reading and thinking. With that additional year and the straight A's gained in all the advanced courses, I was well prepared when at last the time came to attend Armen's classes.

7 My teachers at graduate school

My chief teacher in theory during the MA year was Robert Baldwin, who introduced me to Marshall, Robinson, Hicks, and Samuelson. Baldwin said I was the best student he ever had. While waiting for Armen, I audited Jack Hirshleifer's courses. Jack taught me Fisher and Friedman. I repeated his lectures for six semesters. I became the class star, because Jack would routinely invite questions and answers from me. Often, the lectures became a dialogue between the two of us. Jack never told me I was his best student, although he said so to other people. In a letter he later wrote to Doug North in Seattle, however, Jack compared me with Fisher!

A special mention must be made of Karl Brunner. Karl apparently did not like me at the beginning, but changed his mind later on and wrote me a glowing letter on my works not long after I arrived Seattle. Karl is the most rigorous economist I ever knew, and although in my UCLA days I thought he was unnecessarily rigorous, I grew to appreciate the man and his logical mind

more and more. I regret that when I began my sharecropping thesis in 1966, Karl had already left UCLA.

I do not share Karl's approach, which was to apply formal logic right from the beginning, even when ideas were just taking shape. I prefer to begin with hunches and intuition, and then let the hypotheses emerge. Of course, when we come to the argument stage logic must be applied. To explain an observation I like to begin by thinking along many different lines, let the ideas float around for a while, and then settle down to rigorously analyze one or two hypotheses. The analytical rigor of my works comes from the influence of Karl.

8 No one encouraged me to learn mathematics

I never took a course in mathematics or econometrics at UCLA. When as an undergraduate I asked Bill Allen what I should choose for a minor, math or history, he said history. In retrospect, that was an excellent advice, as it is evident in the empirical richness in all of my subsequent writings on economics. In graduate school, no teacher asked me to learn math, and I was confident that I could learn what was needed as I go along. I learned calculus in two weeks with the help of friends, to prove some propositions in sharecropping. But I knew that the theory was correct before I applied the mathematics, so the equations were for window dressing only. For some time afterward I invented my own mathematics, but stopped doing so when Gene Silberberg told me that my creations, though correct, were ugly. John McGee and Yoram Barzel did not encourage me to learn mathematics. They believed that since I was able to reason with imagination and intuition, the use of technical methods would hinder rather than enhance the workings of a strangely interesting mind. Both Harry Johnson and John Pippenger, after reading Chapter 8 of my sharecropping book, said that it made them doubt the usefulness of econometrics. On the other hand, because I had to use econometrics to analyze crude oil data when consulting for a petroleum company, I took two lessons from Barzel and graduated. Later, Yoram remarked on several occasions that he had never seen a person learn so much so quickly.

9 Inside the chambers of Alchian and Hirshleifer

To return to Armen. The economics Alchian taught was all Alchian's and no one else! I had already taken the graduate theory courses for credit, so I could

only audit Armen's lectures. Again I did so six times, beginning in 1963. Armen had two rules: Auditing students were not allowed to sit in the front row, and they were not allowed to ask questions in class. I therefore chose a seat close to the door. As soon as Armen left the room I rushed out to ask questions during the five-minute walk to his office. Armen would ask in return whether I had read certain articles relating to my questions, and whenever I replied "no" he would stop talking. I therefore got myself prepared, reading up everything I could find as being related to the question I wanted to ask. Armen's answers would then come, in the most stimulating and inspiring ways. This process went on for several months, and then Armen decided to open his office door and ask me in. After a year or so, he would let me sit down and talk economics.

In the spring of 1966, when I completed the first major theoretical chapter on sharecropping, I send copies to Jack and Armen. Driving up from Long Beach to UCLA, I dropped in to see Jack first. He praised the piece sky high. But when I went to see Armen, he returned my draft with dozens of question marks and corrections. I was almost in tears when I left.

Returning to Long Beach, I sat down to study Armen's comments and questions. So concentrated was I that when I digested everything it was eleven a.m. the next day. I then wrote Armen a short note, promising that the next draft would be much improved. A month later I sent out my second draft of the same chapter, and drove up to UCLA again. Jack: "Brilliant, Steve, brilliant." Armen: "When you look for a job and ask me to write a recommendation letter, I will say you can think clearly and write clearly."

And when I turned in my empirical chapter (the said Chapter 8), Armen queried the reliability of every source I cited. I had to contact the Department of Agriculture and Forestry in Taiwan to ask how their data were collected. When every datum was verified to his satisfaction, Armen made only one remark: "We knew all along you are good material to teach, and that is why we inserted extra pressure on you. Now you know what good research is all about." My habit of paying attention to factual details stems from that experience.

Armen had the curiosity of a child and asked questions with child-like simplicity and directness. That is why in a 1976 conference to honor him, I submitted a paper asking a child-like question: "Why are Better Seats Under Priced?" During coffee time I sat with Armen's close friend Bill Meckling, and Bill said to me, "Steve, do not ever change the type of subject matters you choose to write or the style of your execution. You have such interesting taste that only Armen could produce a student like you."

10 Long Beach and Eldon Dvorak

Some remarks need be made about my job at Cal State College at Long Beach during 1965–1967. In 1965, it was very easy to find a teaching job in economics. I received unsolicited offers from England, Australia and Alaska. I went to Long Beach because it was a one-hour drive to UCLA. I needed to consult Jack and Armen about my dissertation. My great fortune at Long Beach was to share an office with Eldon Dvorak, the man who later played an important role in the development of the Western Economic Association. After only several months Eldon was telling colleagues that Cal State Long Beach will someday be remembered of having employed Steven Cheung. Of course it was a hyperbole, but it did me no harm, and in 1966 Eldon and Long Beach students help me to earn the best teacher award in economics among the 18 state colleges. This was good for my immediate academic future, because prospective employers would naturally query whether a Chinese could speak English well enough to teach properly.

Teaching 12 hours a week at Long Beach was a heavy load, but Eldon, a senior member of the department, would arrange for me the best possible schedule. In the early spring of 1966, I read a Chinese article saying that after land reform in Taiwan, introduced in 1949 to reduce and restrict the landlord's rental share to substantially below the market level, agricultural output increased sharply. This did not make sense to me, so I went to the library to see what I could find to support or refute this claim by the Taiwanese government.

It just so happened that the library possessed a complete set of the *Taiwan Agricultural Year Book*, containing extremely detailed reports of annual yields of different crops in different localities. At first, I thought that for political propaganda, the Taiwan government had inflated the figures to demonstrate the success of land reform. But after several weeks I could detect no lies: Agricultural yields did jump after control of the landlord's share percentage was imposed. It would be very difficult to cheat when the data were so meticulous and detailed, and I could find no inconsistency after weeks of careful checking.

One evening in March 1966, I sat down and first worked out the equilibrium of sharecropping without the sharing restriction, then injected the sharing restriction, and to my great surprise found that agricultural yields would rise. It took me only one evening to come up with this theory. After two more days spent going over the analysis repeatedly and finding no flaw, I asked Eldon to sit down to listen carefully to what I have done. I proceeded very slowly, step by step, and after each step I waited until Eldon fully understood and agreed. Eldon asked many questions along the way, and I would say, "Slow down, Eldon, slow down." I would not continue until he fully understood my reasoning at every

turn. After three hours Eldon said, “Steve, this theory of yours is going to be earthshaking.”

It is a simple theory, obvious almost, but very difficult to accept. When I presented my findings to a UCLA seminar in May 1966, everybody disagreed. When the first of my sharecropping analysis (Cheung 1968) appeared as a lead article in *JPE* in October 1968, the journal received a number of comments saying I was wrong. Bob Mundell, then *JPE* editor, asked whether I would like to reply, but I said no.

I will always remember Eldon fondly. In the spring of 1967, he solicited a small grant of \$500 to finance my one-man photographic exhibition at the Long Beach Museum of Art. He himself even made wooden frames for the pictures in his own garage when the money ran out. It was to become the most successful art exhibition in the history of that museum, with headline coverage in the newspapers, viewers drawn from afar, and they twice extended the length of the exhibition period.

11 My association with Coase

In 1962, I made a copy of Coase's 1960 paper on social cost and carried it in my pocket for months, read it over and over again until the sheets fell into small pieces. What happened was that I could not understand the meaning of externality. Though it was a popular topic at that time for some reason I had never discussed externality with Armen, and even stranger is that in 1967 I discovered that Coase himself had never heard of that term. No teacher at UCLA could answer my query on the meaning of externality, although they all claimed they knew what it was. My efforts to understand externality eventually led to the publication of a paper on the structure of contracts in 1970, followed by a paper on bees in 1973 (Cheung 1973), and a pamphlet on social cost in 1978 (Cheung 1978).

My deep understanding of Ronald's 1960 paper ignited a friendship which has become legendary, and which in all likelihood will be recorded in the economic history of China. It began in the fall of 1967, when I walked into Ronald's office at the Chicago Law School and introduced myself: “Professor Coase, my name is Steven Cheung, a student of Alchian, I had spent several years reading your paper on social cost.” He was sitting and reading, raising his head, and asked: “What did I say in that paper?” I replied, “Your paper is about the constraints subject to which contracts are made.” He stood up, and said, “At long last somebody understands me. Let us go to lunch.”

12 A happy share of the take and a Coase error

It was the intellectual highlight of my life, to be able to walk up and down the Chicago campus with Coase and discuss economics during my tenure there in 1967–1969. Talking about the future of economics, I explained my view that Coase's 1960 paper would change economics. Nearly half a century has past, and if one does not mind the tedious task of going through the Chinese Internet, one would find that I am credited as the founder of contract economics, Coase the founder of transaction costs, and Alchian the founder of property rights. I am happy with my share of the take, but would not mind trading with Ronald for his. As for Armen's property rights, I find it too difficult to obtain testable propositions and therefore use the associated concepts less and less over time. But Armen's beautiful insights on pricing and competition are so interesting that I use them more and more. I may be the only student who inherited fully Armen's thinking. Picking a seat close to the classroom door has paid-off handsomely!

An great moment came when, in writing a piece on the future of China in 1981 (Cheung 1982), I found a flaw in Coase (1960): If transaction costs are zero, there would be no market! Both Ronald and Ken Arrow later concurred with this view. I did not know how important this discovery is until twenty years later, when I began to include the dissipation of rent as a part of transaction costs. But I had to wait until a few years ago to come up with a beautiful theorem of transaction costs substitution: Markets emerge as the result of one type of transaction costs (the cost of using the market) substituting for another type (the dissipation of rent).

13 The Chicago school

In the Foreword to a collection of my English writings, Coase noted that when at Chicago I studied the thinking of eight grand masters, including himself, and absorbed and extended their ideas and made them my own. It was the Chicago School at its peak, with a gathering of economic talent surpassing any other group in history. I venture to predict that it will never be seen again.

But the Chicago School was on the verge of decline. Uzawa left the same year I did (1969), Mundell a year or two later, then Friedman and Director retired, Harberger moved to UCLA and Griliches to Harvard. Harry Johnson was seldom around. Although the replacements were superb, the departure of Milton and Aaron and Al spelled the end of a paradigm. The critical mass of the Chicago School disintegrated at that time. Putting brilliant minds together is a

necessary but not sufficient condition to form an academic school of historic significance. When Al Harberger tried to persuade me to stay in Chicago, he rightly pointed out to me that the 1967–1969 group was unsurpassed and in all likelihood will never be seen again.

At Chicago, I was a small figure surrounded by giants. I shared an office with a wonderful Donald McCloskey, who taught me how to write better English. I adored and imitated the writing style of Stigler. George was a truly brilliant man. He belonged to the Business School, and I loved to drop into his office to get knocked about by him. Once he dropped into my office, with Don also sitting there to listen, and said, “Steve, I have everything essential for a great economist, but no original ideas!” I replied, “George, I am full of original ideas, but nothing else!” He knew all along I adored him, and to show him that I, too, could handle the history of economic thought, I wrote a long chapter on the development of sharecropping theory, detailing the thinking on the subject from Adam Smith to Gale Johnson. After he read the manuscript I went to see him and told him that is how the history of thought should be handled. George knew I scored one-up on him and said: “But you are wrong in saying that Marshall understood the concept of cost. He did not!” And he went to the bookshelf to find a copy of Marshall’s *Principles*, and turned to the page in which there was one sentence demonstrating that Marshall did not understand cost. Yes, Stigler was not only brilliant, he was one of the finest scholars I had the honor of knowing.

14 Milton Friedman

Although Friedman became a close friend of mine in later years (he came to Seattle with Rose to officiate at my wedding), he was too busy to give me much time when I was at Chicago. I learn from Milton that an economist’s soul should command such a high price that he should not say anything he does not believe in. Armen drew a line for me, dictating that while an academic economist may make policy recommendations, he or she must not step beyond to participate in real action. I take pride for having followed Milton’s rule and Armen’s rule all my academic life.

It may be interesting to add that I once worked briefly as a professional portrait photographer in Toronto, and when attending graduate school at UCLA there was a group of photographers following me to develop a new and interesting style. In the fall of 1988 I took a portrait of Friedman in Hong Kong. He was so happy with the portrait that he instantly said he would never release another photo of him to the press. True to his words, that picture I took of Milton

is now seen everywhere. I also took a portrait of Rose, and one for Milton and Rose. With the Internet spreading these images my friendship with Milton and Rose will go down history. Of course the future history may also remember that I took Milton and Rose to Beijing to meet one General Secretary in 1988 and another General Secretary in 1993, but the staying power of these meetings cannot compare with the photos I took of the legendary couple!

15 Frank Knight and Aaron Director

I had the great honor of meeting Frank Knight at a Mundell cocktail party, and I seized the opportunity to tell the old professor how much I adored his 1924 paper on social cost and learned from it. I cannot understand why the Nobel Committee did not give Knight the Prize in the several years after 1969 when he was still alive. It is my great honor that, in the entry on “Frank Knight” in *Wikipedia*, the names of five economists Knight influenced are mentioned: Milton Friedman, James Buchannan, Ronald Coase, George Stigler, and Steven Cheung.

Besides Coase, in the Law School at Chicago there was Aaron Director. I stood in awe of Aaron’s firm stand for truth. His intellectual power overwhelmed me. In the spring of 1969 I presented a paper on the choice of contracts at Stigler’s workshop. A day later I was alone having lunch at the Quadrangle Club. I saw Aaron walking slowly towards me. I politely stood up. Aaron said, “The paper you presented yesterday was the best I have read in several years.” He then turned and walked away. I was standing there alone, and tears came down from my eyes. Aaron liked my works, and his preference was one main factor dictating that my works bear the same characteristics over the years.

Since my UCLA days I was thoroughly familiar with Aaron’s oral tradition on tie-in sales. In fact, it was my knowledge of tie-in sales that helped me to work out the sharecropping theory in one evening. There have been at least three books that say my sharecropping theory is a derivative of the Coase Theorem, which may be true, but what ignited my solution was tie-in sales.

During that evening at Long Beach, what puzzled me was that in the traditional analysis of the market there is a quantity and a price, but in a share contract there is no price. Therefore, there must be other stipulations for the contract to function. That is, the share contract must have a structure. Now a tie-in sale is a structured contract. Viewed in such light, all that was necessary for me to do is to add one more stipulation to a share contract to obtain the equilibrium identity with a wage contract or a fixed-rent contract.

Some years later in Seattle, I had dinner with Aaron at Yoram Barzel's home. Aaron asked how I thought of his tie-in sales hypothesis. I replied that the interpretation of tying the paper cards to meter the intensity of the machine use is certainly a brilliant idea, but the associated price-discrimination hypothesis is wrong. The machine rental is the same for all users, and the card price is also the same, so where is the discrimination? If discrimination is inferred to entities that are not explicitly priced, then everything we purchase in the market entails price discrimination. I also told him that the current interpretations of price discrimination in terms of different price elasticities are all rubbish!

Aaron then continued to ask how I would interpret the tying of cards to the machine. I replied that it must be a maintenance contract: With a slightly higher price for each card used, the more intensive machine user paid a higher implicit maintenance cost, while the explicit maintenance charge was zero.

16 Decision to leave Chicago for Seattle

By the spring of 1969, when I was ready to seek my first real academic job, I had been trained and befriended by eight masters: at UCLA I had Alchian, Brunner, Hirshleifer, and Baldwin; at Chicago I had Friedman, Stigler, Coase, and Director. No student ever, anywhere, would have such luck. That is why Yoram Barzel wrote in 1995: "On arrival in Seattle in 1969 Steve was, in my current view, the top transaction-cost property-rights economist in the profession."

Coase and some other friends were baffled as to why I left Chicago, and I cannot say I left with no regrets. It is true that Hyde Park was not a good place to live at that time, and Seattle has a beautiful sea. But given my dedication to research, these cannot be the true reasons for that decision. Over the years I have come to understand the reason: I wanted some time alone to do my own thinking. Chicago's workshops, lectures, and papers to referee were too intense, and I wanted to be left alone to think free from outside influences. Of course I like to consult colleagues when I come up with something novel, but when thinking I want to be left alone. It was in 1969, too, that I decided not to read other people's writings, that there is a period to read and a period to think, and in the thinking period I should not read. When I came up with something new I often consulted colleagues as to whether it has been proposed somewhere or sometime before, and it was very rare that an idea I came up with was not original. Only once, when I wrote a draft on the theory of inter-individual effects, that I was informed a key point I made in that paper was noted in an earlier paper by Buchanan and Stubblebine (1962).

17 Thirteen years in Seattle

That is why after I arrived Seattle, I found a great colleague in Yoram Barzel. Yoram has a wonderfully subtle mind, capable of catching even the smallest flaws in an argument. I routinely tried him out when I came up with something new, and when he did not object I knew I was on solid ground. In my recent obituary on Doug North, I expressed my gratitude for the protection he so kindly rendered to me. It was a time when publish or perish was the rule, but Doug and Dean Beckman informed me that this would not apply to me. I was to do my own thing, and the rank of full professor was bestowed only a few months after my arrival. The nine papers I produced during my Seattle period have by now all become classics.

In Seattle we had a good group of colleagues and some superb students. Two of my doctorate students, John Umbeck and Chris Hall, were great talents. But I was a terrible thesis adviser because of my tendency to wander away from what I read with my wild imagination, and if a candidate follows my wandering he would never finish his thesis. Furthermore, I made a serious error of judgment in thinking since it was so easy to dig up an Umbeck and a Hall (yes, Hall was a reject who I fought to get him back, and told colleagues to treat him like a star) there would be many of them forthcoming. This turned out to be wishful thinking: Umbeck and Hall were so unique that I was not able to see their likes again.

A special mention should be made of my paper on price control (Cheung 1974). Most people say that it is my best paper, a view I do not share. Nevertheless it is a piece which cost the greatest effort per page, and the manuscript was not really completed but abandoned when Coase urged me to let it go to meet the *JLE* deadline. In retrospect I should have listened to Harry Johnson, tear up the manuscript and write the piece all over again from page one. But I was truly exhausted.

This original piece did not begin with the inspiration of Coase, as some people thought, but from rooftop squatters in Hong Kong. For pre-war premises subject to severe rent control post-war, the controlled rent came to about 10 % of the market rent. The other 90 % had no exclusive claimant, and must therefore tend to dissipate. However, rent dissipation was salvaged to a large extent by the prevalence of partial subletting and rooftop squatting. Attempts to reduce rent dissipation are implied by the postulate of constrained maximization, but there are many ways to achieve that. Under what conditions can we predict the emergence of partial subletting and rooftop squatting? My paper on price control offered two propositions to guide the prediction of the choices to reduce rent dissipation.

As Yoram recently noted, my years in Seattle were most productive for both of us. In my case, however, this is true for my English writings only: After I took up the Hong Kong Chair in 1982 my Chinese writings have continued with full force until the end of 2014. Friends with command of both languages assume the view that my contributions in Chinese exceed those in English by a factor of three.

I would have published much more during the Seattle years except for two endeavors which stood in the way. The first is a well-funded investigation of patent rights and trade secrets and their licenses. This turned out to be too difficult a task: We purchased several hundred licenses but did not know how to decipher them properly – the costs of soliciting expert help were too high. The second is my consulting commitments to a petroleum company. The data and documents made available to me were first-class, and the two reports I wrote to analyze crude-oil pricing and crude-oil exchange prompted Alchian to say it was the best empirical work he ever saw. However, as a matter of contract agreement they could not be published.

18 The Washington school of economics

In 1990, Doug North (1990, p.27) wrote that there was a University of Washington approach to analyze transaction costs, and that I was the originator. Doug's assertion is perhaps what led to the saying that there was, and in some sense still is, a "Washington School of Economics", or a "Washington School of New Institutional Economics". Gary Shiu has observed that the School's unique feature is the attention paid to the piece-rate contracts; Barzel would say property rights is the central feature; and North would pick transaction costs.

In my view, the unique feature of the Washington School – if there has ever been one – lies in the attention paid to the dissipation of rent. This idea first emerged in the work of von Thünen in the nineteenth century, Pigou applied it in the example of two roads and private and social cost in 1920, Knight then demolished Pigou in a brilliant 1924 paper, and Gordon applied Knight's arguments to ocean fishery in 1954. Gordon coined the term "dissipation of rent".

When I reread Gordon's classic paper in the spring of 1969, I decided his equilibrium point made no sense. This led me to write a paper (Cheung 1970) on the structure of contracts and non-exclusive resource, providing a correct analysis of the equilibrium of rent dissipation. This was in 1969, before I went to Seattle. In Seattle, further developments followed: (1) with non-exclusive or common property rights, under a variety of circumstances rent may not dissipate completely. Hence

non-exclusive property rights may at time serve certain purposes; (2) if price is not used as a criterion for competition, all others criteria must to some degree entail the dissipation of rent; (3) reducing rent dissipation is implied by constrained maximization, hence to analyze the effects of government regulations we should approach through this route. In fact, I later viewed the comrade-ranking system in communist China as an arrangement to reduce rent dissipation. After I left Seattle and I came up with (4): transaction costs must include all costs that do not exist in a one-man economy. Rent dissipation would not therefore occur in such a case, so that the dissipation must be a type of transactions cost. After some 20 years I obtained (5): the emergence of the market is the result of using market transaction costs to replace rent dissipation, hence the theorem of transaction costs substitution, which I noted earlier. Finally (6): if at the equilibrium margin there remain some rent which logically should dissipate but does not dissipate, the analysis must be wrong.

To my knowledge no other group paid so much attention to rent dissipation and the associated equilibrium analysis. But in Seattle, Yoram and I and other colleagues discussed it as a matter of course. There are other salient features of the Washington School, such as the dedication to derive testable hypothesis and the complete disregard of utility analysis, but it is dissipation of rent that stands out as uniquely “Washington”.

19 Going to Hong Kong to observe China

My decision to leave Seattle to take up the Chair in the University of Hong Kong was a more difficult decision to make than leaving Chicago for Seattle. Coase urged me to do so, but Doug made it plain that I would have a clear shot at the Nobel Prize if I didn't. However, influenced by Armen and Aaron, I was a truth-seeker and not a prize-seeker. Furthermore, my mother in Hong Kong was getting old, and I always cared for the Chinese people. On top of everything I had written a piece in 1981, which predicted in no uncertain terms that China would go capitalist! Friedman and Becker and Schultz thought I was crazy and disagreed loudly. So I wanted to go to Hong Kong, to seek first-hand observation of what was going on in China.

I came to Hong Kong in May 1982, and soon knew that I had to give up writing in English. But I had never written in the Chinese before. After some preparation I launched my Chinese writing career in November 1983, and soon produced three books: *A Tangerine Seller Speaks* (1984), *The Future of China* (1985), and *On China again* (1986). I hit hard, but I hit sincerely. Beijing did not mind the criticisms and provided me with assistants. Whatever factual contracts

I asked for, they provided. To my delight, the last two volumes were pirated and 2,000 copies of each printed “for internal reading only.”

My paper on “The Economic System of China,” written for Coase in 2008, recounted the great events of the 1980’s (Cheung 2014: pp.70–71):

There was no better time, no better place, and perhaps no better sales person, than this writer in popularizing the ideas of Coase to the Chinese in the 1980s. The ideological gate was beginning to swing open: Comrades knew what they had believed in did not work, and were looking for something new. I was appointed to be Chair of Economics of the University of Hong Kong in May 1982, the best position at that time to follow China’s development. I knew Ronald’s works by heart, and people knew he was a good friend of mine. I was expert on Chinese culture and history, so comrades could not tell me that I did not understand China, as they routinely told outsiders. I could write in Chinese, and soon developed a style people say was popular thought distinctive. On top of all this are the seminal ideas of Coase, which at that time were easy to sell. Had China then been like the China now, I would be out of luck.

First is the idea of transaction costs. Chinese living under the earlier regime were all too familiar with such chores and headaches as memorizing political slogans, waiting in line, making connections, dealing through the backdoor. They had to spend hours every day doing these things. When I wrote that if these costs were reduced income would shoot up, even the most stalwart defenders of the old regime could not handle the challenge. The level of transaction costs were so high at that time, things often did not make sense. This was clear enough, but it took time and many articles to convince people that transaction costs could not be reduced unless the system was changed. I should take credit for that.

Change the system to what? This time, it was more difficult to do the convincing. The first point made in my 1979 paper, that market pricing is the only economic criterion that entails zero rent dissipation, was not difficult for people who had to stand in line for hours to comprehend, but the proposition that the use of market price is uniquely associated with private property was difficult for comrades to swallow. The word ‘private’ did not bear a single respectable connotation in Chinese culture at that time, and ‘private property’ was in strict contravention with the official stance of maintaining a socialist or communist state.

It was here that Coase’s idea on the delineation of rights worked magic, especially since I as the scientific sales person knew that the same product can be sold with new packaging. When I took the Friedmans to meet the General Secretary of the Chinese Communist Party in the fall of 1988, Mr. Zhao lost no time in elaborating to Milton the importance of delineation of rights. This dialogue is on record, and it is in print in a number of places. I take credit for selling Coasian economics to the General Secretary.

20 A masterpiece at last

I wrote (to date) 1,500 articles and 20 books in Chinese. Soon after my *retirement from the University of Hong Kong in 2000, I began to write, also in Chinese, what

is now regarded as my *magnum opus*: *Economic Explanation* (Cheung 2014). This took me fourteen years, reaching 1,147 pages after several rounds of revision.

The idea of writing *Economic Explanation* began in the summer of 1969, when I was in Hong Kong on vacation. To my surprise, I could not explain many observations when I visited some markets and factories in the city. It started me thinking: peers and colleagues had already regarded me as a top price theorist, but a good undergraduate in physics would not experience such an embarrassment interpreting everyday physical observations. There must be something clearly and importantly missing in the economics paradigm. On the other hand, my sharecropping work has shown that economic theory can explain observations. I therefore came to the conclusion that the entire corpus of economic theory must be overhauled and re-written.

In Seattle, North, McGee and Barzel also thought that traditional economic theory must be overhauled, and they all pointed to me as the person to do so. I was willing to do so, but the process is slow: From 1970 on, it took me 44 years to come to a satisfactory conclusion. The following are my present views.

First, hypothesis testing requires the use of observable entities. As a result, I have discarded all except one non-observable entities: utility, taste, shirking (my invention in (Cheung 1969a)), all the terms used in game theory – are discarded. The only non-observable entity I keep is quantity demanded. Second, transaction costs must be introduced to interpret many observations. Third, a useful economic theory must be simple, with sufficient empty (logical) space to make room for the incorporation of transaction costs. Hence, the entire theoretical structure I use consists of only three elements: the law of demand, the concept of cost, and competition and its implications. Finally, for any observation seeking theoretical interpretation, we must obtain facts from the real world.

In *Economic Explanation*, all the cases discussed are grounded on facts, and I routinely seek the support in factual details. The testing of hypotheses appears everywhere. Traditional explanations for price discrimination, tie-in sales, full-line forcing, dumping, unemployment, monopoly and competitive pricing, information costs and searching behavior, capital accumulation, monetary system, etc. etc. are interpreted along different lines. I even argue that in many situations the marginal cost curve cannot be constructed, and equilibrium is defined in terms of the survival of the fittest.

21 Life is fading

I am now 80, in the evening of my life. Looking back I do not see how I could have lived more fully. I have studied economics intensely for 57 years. Not as

long as Coase, but long enough. What energy I have left I will devote to a final revision, and then I will send *Economic Explanation* to MT Cheung to translate into English and fulfill the promise made to English-reading colleagues. New insights have cropped up after the book's Chinese publication. The Jade Theorem can be further elaborated. More important is my new interpretation of the Say's Law, which I managed to incorporate with Adam Smith's idea of division of labor by arguing in terms of an imaginary piece-rate contract. The new theory is so beautifully simple that it is no less satisfying than the share-cropping solution I worked out that evening in Long Beach half a century ago.

My relentless search to obtain facts and details through field instigations (often running around in the streets) has been criticized as unworthy of a professor. My critics, including some fellow economists, apparently did not understand that economics is an empirical science, so the practitioners of economic research must treat the real world as a laboratory. Facts found often diverge greatly from what we read – for example, the economic system of China as I observed and analyzed it is miles different from that described in official reports. I have long held the view that the most stupid thing a scientist can do is to try to explain things that do not exist. Even for the simple phenomenon of price discrimination, the economist's neglect of the associated observations has led to a theory that is logically sound but empirically irrelevant. We cannot appreciate a picture by looking at a portion of it.

22 A museum is contemplated as my last project

Going into the facts and details is interesting, but it wears the researcher down. In my long and near-obsessive involvement with economics I have often taken to some sidetracks to keep me sane. So I have attempted photography, calligraphy, prose writing, and art and relics collection. I have had much luck in the last endeavor. This led to the idea of setting up a museum and the donation of the admission receipts to assist poor children in China. I have not forgotten my early experience as one of them, and I believe that if given the opportunity the poor children of today may grow up to become scholars like me. In recent years I have visited some poor villages, and was happy to see that children were enjoying far better living conditions than I during the war. But I think they should have better learning opportunities.

It began in the spring of 1975, when I told Yoram that on my forthcoming sabbatical I would investigate the jade market in Hong Kong and *explain* information costs. I was not happy with current thinking, and believed that

the constraints should be investigated by looking at items bearing high costs of information. I wanted to focus on the physical attributes of goods, not individuals or market prices. My choice was to begin with jadeite, well known for its high information costs at the various stages of production and marketing, and then move to art works and finally to relics.

Three factors combined to lead to my plan, though the significance of the museum may exceed what a man can do. First, my mother has endowed a foundation, which has provided sufficient funds to form a substantial collection. (Remember collector items in China were very cheap before the turn of the century.) Second, I predicted that with China growing fast the market for art works and relics would soar. It did, and having a head start allowed the foundation to trade and further acquire. Third, for centuries before the Ming Dynasty, the burial practice in China was to put beautiful things in the tombs, particularly in the case of the rich and high-ranking officials. Beginning in the 1980's, unearthed relics have revealed a civilization so brilliant and so developed that it is beyond belief. I may be the earliest collector of ancient Chinese relics because I started to investigate information costs in 1975. And because I concentrated on physical attributes, I soon became a self-taught expert of art works and relics. Alongside the emergence of massive new finds beginning in the mid-1980, I worked out a theory to determine what is authentic and what is not. In all of this, I was a man who happened to process the right training and worked in the right place when it was the right time for China to unveil her brilliant past.

My mother's preference was to donate to the church. But there are few churches in the poor villages in China, so I think it is better that the money should go to the children. Not many people believed me when I say the museum will be a great draw so entrance money will yield a tidy sum. But then nobody believed me when I said I would do well in economics. I am Captain Incredible!

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Editor's note: this text is Professor Steve Cheung's responses to a set of written questions I sent to him for an interview for this special issue. Instead of answering my questions point to point, Steve graciously offers us a thoughtful self-contained essay (for this reason the original questions are not included here), an narrative on his distinct views of economics, and, particularly valuable for young readers, the fortuitous path that made him the economist we all know and admire.