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Theory of Share Tenancy after 50 Years

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Abstract:
This paper was prepared for "The Theory of Share Tenancy: After 50 Years," a conference organized by The Ronald Coase Center for Property Rights Research at the University of Hong Kong, November 24-26, 2017. The paper first appeared in Chinese as a series of column articles in ifeng.com (凤凰网). The author traces the complex intellectual origins of The Theory of Share Tenancy and an adventurous career that followed.

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Theory of Share Tenancy (Cheung 1967) was my UCLA PhD dissertation. Academic regulations required that, after examination and approval, the manuscript be duly submitted to the University Library for records and filing. This was in May 1967. Library regulations were very severe: the format was specified to the inch, and no deviations were accepted. There were no word processors at that time, of course, and even though I had secretarial assistance for typing it was only after several trips that the librarians were satisfied.

1 Controversies and diversities

Armed with the Library's receipt, I went to the Registry to collect my official doctor's diploma. There I discovered that an administrative fee of US$50 was required for foreign students. My first reaction was that how can a piece of paper certifying that I possess what I already possess be worth that much money? I was on the point of leaving when the great historian Warren Scoville, from whom I learned so much about European history, ran out of the office. It was two years since I last saw Scoville, and I had no idea that he had become Registrar. The following conversation then ensued: "Scoville: It is not right that foreign students be charged $50 for a doctor's diploma, but regulations are regulations. Colleagues from the Economics Department are very impressed by your work, and it is we professors who are determined to award the PhD to you. So, can I pay the fee on your behalf? Cheung: No, no, I should do it." A sum of $50 from my pocket was, of course, immediately forthcoming.

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Let me say this loudly: Without great teachers like Scoville and many others whom I will mention below with gratitude, the name of Steven N.S. Cheung would not be known in Economics. But I still think I had a point then. It was a bother to find somewhere to store my diploma, and from 1967 to now nobody who mattered has cared a jot whether I am Mr Cheung or Dr Cheung. On the other hand, I do care – and with great pride – about the work that earned this doctorate. My $50 PhD diploma went into the drawer to gather dust, but when the University of Chicago published *Theory of Share Tenancy* (in 1969) and sent me the first copy off the press, the book spent many nights in bed beside me.

Fifty years have passed. That is a long time: Multiply it by 19 going back, and we would be contemporary with the great Chinese poet Su Dong Bo. It was very kind of my friends and colleagues to organize a Conference on "*Theory of Share Tenancy* after Fifty Years" to mark the occasion. Many participants came to Shenzhen, mainly from the US, Mainland cities, and Hong Kong. One person who was not there really touched my heart – Arnold Harberger, who wanted so much to come but (at 93) was forbidden to fly long distances on doctor's orders. I owe Harberger a great debt. In February 1967, when only one chapter of *Theory of Share Tenancy* was written, he sent a cable inviting me to Chicago. I was dazzled: Harberger was then chairman of the greatest gathering of economists on earth. Fired by Harberger's call and by Armen Alchian's constant encouragement, I completed the rest of *Theory of Share Tenancy* in 6 weeks.

To address the Shenzhen Conference, I gathered together a few notes on the writing of the book and its academic impact. Some of the material has appeared before: In connection with the re-issue of *Theory of Share Tenancy* in 2000; with the publication of my collected English scientific papers in 2005; and in *Man and the Economy* (Cheung 2016). These three articles mainly offered disjointed reminiscences and painted one or two portraits from memory, so the present piece can be viewed as a more conscious attempt towards an intellectual autobiography.

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Throughout the years, many friends have broached the subject of a Steven N.S. Cheung autobiography. The first (in the 1980s) was S.M. Lin, then Editor-in-Chief of the *Hong Kong Economic Journal* newspaper. The latest (two years ago) is Colin Xu of the World Bank, who almost convinced me. I am still not committed, because only giants in history are qualified to write autobiographies, and I do not consider myself to be one. Of course, I cannot prevent other people from writing my life-story. I hear that several people are planning to do so. To keep the facts straight, it may not be amiss if I recount what happened in connection with *Theory of Share Tenancy* over the past 50 years and dot the *i*'s and cross the *t*'s.

1.1 Controversies ad hominem

I remember how, as a freshman in UCLA, my psychology professor remarked that mankind is blessed with incomplete and gradually fading memory. Otherwise we would remember all the unfortunate things that has happened, and life would be unbearable. So it is with me – I tend to remember the good things, but little about the bad things. With that *caveat*, let us begin.

I am without doubt a controversial person in the eyes of many people. It is not my fault. All my life, I have done things my way, without regard to what other people may think or feel. But for some reason, many people want to have something to say about how I live. Especially in the academic world, this attitude of mine has brought about some futile and unedifying controversies.

The first concerns my penchant for life and livelihood at-first-hand, 'out there' in the 'real world'. Also, I have been involved in a wide range of commercial activities. The result? People criticize me for not being a 'serious' scholar, or worse, to have abandoned 'serious' research. This was especially the case after I returned to Hong Kong in 1982. My critics do not seem to understand that economics is an empirical science, and that the 'real world' is its laboratory. Wandering 'the streets' and observing what is going on is lab-work in economics. Similarly, it is only by active participation that one obtains first-hand information on how many businesses work. I have said so many times: Scientists who attempt to explain something which has not happened are wasting their time.

Other people do not share these misplaced criticisms. My practice of studying real situations at close quarters has been much lauded, especially by academic colleagues in the US. It was thus with the field work I did among the orchards of Washington State, which produced "Fable of the Bees" (1973). It was thus with "Why are Better Seats 'Under-priced'?" (1977), which I wrote after spending a busman's holiday visiting Hong Kong theatres and observing ticket-pricing behavior. And it was thus with what was deemed by Armen Alchian to be the best of empirical research in his experience – my [1978] anti-trust consultation studies of the oil industry in California.

It is a pity that the California oil studies were a consultation report, and so cannot be published. Not only was it praised to the sky by Alchian, colleagues in Standard Oil were moved to describe the two-inch-thick tomes as the 'Bible'. The professionals took me everywhere and showed me everything, and then left me to put the parts together into a meaningful and coherent whole. The works were beautiful pieces of analysis and data interpretation, and the petroleum people concurred that I was able to explain the phenomenon of oil-exchange between different companies in the industry and its contractual foundations.

1.2 Protecting one's brain

Another occasion for negative controversy arose because of my participations in many 'extra-academic' areas. In endeavors like photography, calligraphy, prose-writing, and collecting, I can justly claim to have progressed way beyond the dilettante. My career as a collector began because of research into information economics. In the other areas, I became involved mainly because I wanted to give my brain a rest.

There is one unhappy memory I cannot erase. I had a favorite brother, Ng Lun, who was one year my senior. Ng Lun was brilliant, but after topping the class as freshman at the University of Pennsylvania he succumbed to schizophrenia. His early death in 1967 really hurt, and still does. Since it is possible that my brother's problem may be hereditary in some way, I have been chary about taxing my brain. This is especially so because I can think quickly, deeply and creatively, but once airborne over an intellectual problem there would be nothing to hold my brain back from self-inflicted exhaustion. Though I share Milton Friedman's view that the brain needs continuous exercise to stay strong, I am also very aware that over-exercise may lead to unfortunate consequences in some cases. So it was that once-in-a-while, I would give my brain a holiday by branching out to extra-academic activities like photography, prose writing, calligraphy, and collecting.

I began calligraphy at 55. I think this is the most efficacious way to let go with one's artistic feelings. And it is cheap – quality paper and ink are readily available, and not a lot of space is required. Good brushes are difficult to find, though – but I have been lucky in amassing several hundred top-notch sheep's hair brushes in China twenty years ago. I never thought I would achieve master level in calligraphy, but serious collectors in the auction market seem to think so. For my part, I keep things externally meaningful by donating the proceeds to education charities, and internally rewarding by endeavoring to find the best expression for my feelings in each and every brush-stroke.

1.3 The Garden of Eden parable and nonsense economics

To come to collecting. In a talk earlier this year in Guangzhou, I proposed a 'Garden of Eden' way of thinking about economics. Remember the Bible: In the Garden of Eden, Adam and Eve had everything for free. Since prices are zero, what is enjoyed is all consumer surplus. In orthodox economics, where quality of life is argued to depend on per-capita GDP, it follows that Adam and Eve would be living in dire circumstances. They enjoy consumption without limit, and yet GDP in the Garden of Eden would be zero! So, what is being taught in today's classrooms?

It follows that I do not believe the US is the world's largest economy, and that China is only second. The reason is that this result follows from orthodox monetary calculations. But let us think more deeply: It is readily observable that the average price of property in China is double that in the US, and that there are many more modern multi-story buildings in the country. If wealth is measured in terms of real estate capital, therefore, China's 'economic index' would be higher. It is also readily observable that China's population is 4 times that of the US, with less racial discrimination. Assuming the same distribution of talent, if wealth is measured in terms of human capital, China's 'index' would also rank above the US.

So we see what the problem is. If we look at housing with sea view the price of housing is much higher in China than in the US. This means that with higher price the Chinese property owners would be richer, but their consumer surplus would be lower. Applying the Garden of Eden parable, the US would therefore rank higher in real income. So, how do we measure GDP so that it reflects underlying market reality? Heaven knows, but I am sure that the orthodox approach is pretty much nonsense.

The fact that individuals enjoy quality accommodation at relatively low price is closer to the Garden of Eden. It is sadly true that we cannot find such a situation in China. However, we in China possess an Eden-like Garden not found in the US – our culture, civilization, and objets d'art. This Chinese Garden is wide and deep and multi-faceted in expanse, and possession is not necessary for enjoyment. There is a difference, however. To enjoy sea view one does not have to learn; but to enjoy Chinese culture, there is a high cost of learning.

In 1975, I spent two months exploring information cost in Hong Kong's jadeite market, and then turned to investigate other collector's items bearing high costs of information. With these investigations I later derived the Jade Theorem to interpret observations generated by information costs, and a Warehouse Theory to interpret observations pertaining to capital accumulation. These are beautiful works.

Beginning in the l980s numerous bulldozers worked the grounds of China, and vast quantities of ancient relics were unearthed. I routinely followed because of their high costs of information. Using funds from my mother's foundation I began collecting paintings and calligraphic works in the l980s. Fifteen years later I turned to trade these art works for the ancient relics. There is a simple rule: in markets bearing high costs of information a participant who masters the information and how the associated markets work will be an easy winner. It defies George Stigler's wisdom that the price variations are often so enormous that the observations make no sense. These senseless observations are consistent with an exaggerated version of my Jade Theorem.

In 2006, my mother's foundation is converted into a charitable one, and the said collection will be arranged for a museum.

2 The lives of our intellectual progeny

Richard Dawkins' *The Selfish Gene* (1976) is an important book, but it is a pity that the analysis did not focus on man as the dominant race of animals. This is especially the case as far as I am concerned, because of a question that I have always found interesting: Is the importance individuals attach to posthumous reputation a consequence of gene-selfishness? We have no evidence that this happens with other animals, but it is commonly observed among human beings.

It may be because of religion that people used to believe fervently in rebirth or life after death. Belief in an after-life may also explain the phenomenon of burial goods: It was a widespread practice in China, particularly before the Ming Dynasty (1368–1644). Nowadays, most people do not bother about the next life. Of course, we are concerned about the material well-being of our children and succeeding generations, but that is a different thing and it is readily explicable in terms of Dawkins' selfish gene hypothesis. But why should an individual be anxious about posthumous reputation? Even the great Song Dynasty man of letters Su Dong Po (1037–1101) – unquestionably a genius – harbored such thoughts before he died.

Writers of the past have expressed this sentiment in subtle language of poetry. Su Dong Po famously wrote in *Chi Bi Fu*: "The great river flows east, washing away generation after generation of heroes." We also find: "On this great stage, heroic deeds are soon carried away by the wind and rain." (Xin Qi Ji, *Yong Yu Le: Reminiscing the Past at Jing Kou North Mansion*.) And: "Our monuments stand, but shrouded in mists of the ages." (Sun Ran Weng, *The Long Couplet of Daguanlou*). Do we not discover beneath the pathos a concern over reputation after death?

Remember how, after completing the monumental *Chi Bi Fu* ('*The Red Cliffs*'), Su Dong Po hid the manuscript in fear over the Emperor's possible disapproval. But he could not stop himself reciting the piece every night in the back garden, until even the old woman next door knew it by heart. Why did Su Dong Po write something he knew may never be published while he lived? My answer is that he knew that the *Chi Bi Fu* would become immortal. In this, Su was right: The great river is still flowing east, but its waters have not effaced the name of Su Dong Po!

2.1 Why do we want our ideas to live on?

Artistic endeavors have a small but not insignificant chance of 'long' life. I have a few prose pieces written dozens of years ago, which are still widely read and have even appeared in school syllabuses. In economics, matters are altogether different. Here, it is hard for an article to 'live' long, not even for a few years after publication. In a subject where the adage 'there is nothing new under the sun' is more than half-right, it is rare for research to achieve longevity. At the very least – and this is a truly difficult thing – the author must establish a novel way of framing and analyzing a problem. If a sufficient number of other economists were convinced under the auspicious of scientific methodology, this would produce a new paradigm. A long life may then be possible.

I care little about posthumous reputation, but I care very much about the longevity of my ideas. In Dawkins' language, my articles are the children of my intellect. After birth – publication – my intellectual children leave home to live among other people's intellectual children, and what would happen then is something over which I have no control. However, after so much blood, toil, tears, and sweat, I cannot help but care deeply whether the lives of my intellectual progeny are long or short. It is for this reason I say that, even though gene-selfishness cannot explain the individual's concern about reputation after death, it can explain the individual's desire that his ideas would live on beyond his mortal existence, in the annals of intellectual endeavor.

Articles in economics are numerous as the stars in heaven, but most of them are hardly read after publication. Sometimes a paper may attract widespread attention over a year or two, and then fade away. I do not attach much importance to such phenomena. Instead, what I feel to be interesting and valuable concerns a work which would persist in a life of its own, lighting up once-in-a-while as if in a gesture to say "Father, I am still here years after leaving home". When this happens, it is a deeply satisfying reward.

2.2 Some facts about intellectual longevity

I do not have the kind of intellectual children who burn bright, but then are soon extinguished. *Theory of Share Tenancy* – reckoning the book and the two chapters published separately in *Journal of Political Economy* (1968) and *Journal of Law and Economics* (1969) as a single work – has earned some 2000 entries in the *Social Science Citation Index* over the past 50 years. Not bad, but there are many instances of higher numbers. What is more significant is that *Theory of Share Tenancy* is still alive in the annals of economics, appearing every so often in the learned literature with undiminished vigor. Colleagues have continued to praise it to be seminal in the analysis of contracts, many believe it to be the *fons et origo* of principal-agents theory, and in any discussion of neo-institutional economics it is referred to as one of the foundational contributions.

As far as longevity is concerned, the important thing is not citations but whether the article is able to enter reading lists in major universities. Citations usually appear in footnotes, which means they are rarely read with attention by third parties. On the other hand, students study articles in reading lists thoroughly, because they wish to pass examinations. Once an article earns a place in the reading lists, therefore, it tends to live long both in original form and in oral tradition. From this perspective, my intellectual progeny is likely to outlive those of colleagues from the same generation. Chinese students studying abroad find 'Steven N.S. Cheung' in reading lists all the time. Several years ago, a university in Oregon issued a list of 'must-read' articles in economics. I have nine of them.

I discovered a few years ago that longevity is reflected by dollars and cents. It is true that apart from *A Tangerine Seller Speaks* (1984), I have not produced another bestseller. But as far as already-printed books go, I have not fared badly at all. When *Theory of Share Tenancy* was published in 1969 by University of Chicago Press, it sold for a few dollars each. Today, 'as-new' ones sell for $2000, while used copies in good condition fetch $800. People who spend so much money to buy a slim volume of 188 pages would keep and (re-)read it, would they not? Just for the sake of curiosity, I asked my wife to look up the prices for my 1982 booklet *Will China Go 'Capitalist'*? The first edition cost £1.50: now, it is US$180. Even the second edition, which came out four years later with an *addendum*, sells for more or less that much. My author's copies from the *Institute of Economic Affairs* were all given away years ago, so I would have to pay dearly to possess one of each now. I was happy to discover that a French translation of my 1983 *Journal of Law and Economics* article "The Contractual Nature of the Firm" (Cheung 1983) appeared last year. Another spark of life from a child of my intellect: "Father, I am still alive and well 33 years after leaving home".

The same has happened to my Chinese writings. The first edition of *The Economic System of China*, which was published in Beijing in 2009, sold for RMB25. On the Internet, it is now RMB180. Last year, my Hong Kong publishers issued a special edition of *Economic Explanation*. The limited run of 2000 sets sold for HK$1000 per set. Over this book's gestation period of seventeen years (which matches that of Marshall's (1890) great *Principles of Economics*), it has gone from three to four to five volumes. *Economic Explanation* is likely to live as long as *Principles* (1st edition 1890, 8th edition 1920 and still in print) and it is an interesting experiment to chart the special edition's price increases over the next twenty years.

2.3 Satisfaction tinged with regret

It is every author's dream to create works which stand the test of time. You ask: When I see my intellectual children growing older but not weaker over time, am I happy? Well, yes – but the satisfaction is tinged with regret. As a young economist I did not know it would be so easy to write articles which live long. Indeed, 'over-kill' would be an understatement to describe my efforts to solve the classical problems in economics and produce something seminal. If I had known at the time that it is really not so difficult to write articles which live for 50 years, I would have re-allocated my resources to publish more on less-difficult-but-still-interesting topics.

But I did not know then: hence the feeling of regret. It is outweighed by the satisfaction of having done things which are still analytically significant after 50 years. I had to wait some thirty years before I was sure about *Theory of Share Tenancy*. But sometimes, 30 years may not be sufficient. In 1972, I published "The Enforcement of Property Rights in Children, and the Marriage Contract" in *Economic Journal*. Forward 30 years to 2002, and at that time I had little hope of that article being alive in another 20 years' time. It is now 45 years from 1972, and though "The Enforcement of Property Rights in Children, and the Marriage Contract" has only been cited 84 times, I am confident that it would live long, for more than 50 and perhaps as much as 100 years.

Writing long-lived articles in economics is not that difficult: The rub is human mortality, and more importantly, intellectual mortality. This is especially since the time (in the above senses) it takes for the novice to learn the why's and how's subtracts from the already small endowment of human and intellectual vigor. A short while ago I completed *Economic Explanation*(Cheung 2017). This *magnum opus* took 17 years to write, and at 81 I intend it to be my last intellectual child in economics. Before I retire honorably from research, I would like to share a few thoughts on one more aspect of intellectually long-lived articles: What properties are idiosyncratic of such works?

Using my own writings as case study, I would say there are three necessary conditions for an article to live 50 years or more. First, it must say something novel; second, it must be interesting; and third, it must be true.

2.4 A new approach is not necessarily esoteric

To say something new does not mean that something new is created. The old adage is right: 'There is nothing new under the sun'. To create something new for an article is an approach the economist must avoid, however attractive it is as a publication technique. Rather, I mean by 'new' a way of looking at economic phenomena which nobody has thought of, and which ideally should titillate by suggesting one or two sparkling ideas. Long-lived articles are to be especially enjoyed by the intelligent, so that afterwards the reader can only exclaim in wonder and admiration: 'The ideas are so simple, but why hasn't somebody thought in this way before?" In this way, the first necessary condition for intellectual longevity would be satisfied. The economist must never conflate 'new' and 'esoteric': To do so would mean likely failure with regard to this condition.

Let me present one or two examples from my own writings. In *Theory of Share Tenancy*, I imagined the landlord to slice the land piece-by-piece like a loaf of bread, and then asked: How many such slices need be assigned under contract to the tenant to maximize net revenue? With this new approach I found the solution in

2 hours, with the result that the share-cropping analysis is still alive and well after 50 years. The second example concerns my 1973 *Journal of Law and Economics* article "Fable of the Bees". Traditional analysis viewed honey as one product, and pollen transmission as another product. My new approach added them together to form one product, with price equal to the corresponding sum. The idea was simple, and yet it is profound enough to be cited and applied for the past 44 years.

2.5 'Interesting' is a matter of style

I must first point out that new ideas (in the above sense) are necessarily interesting, but not the other way round. Interesting-ness comes from style. (This is also the case with people – some individuals are born with style, but some are not.) It is possible for an article with no style to live long, but this is not what I wish to talk about here. As far as I am concerned, style comes from two things: the way the author reasons, and the author's choice of examples to illustrate this reasoning.

Interesting examples are generally concrete. Concrete examples are drawn from the real world, and the more 'exaggerated' (in the sense of being above the ordinary) the observations, the more interesting the example promises to be. Both the author and reader are then happy (exaggerated observations help the reasoning and help the understanding). This was what happened when I studied rent control in Hong Kong in the 1970s: There were exaggerated examples galore, especially with regard to the very special contracts underlying rooftop accommodation and heavily sub-divided units.

Perhaps more important to interesting-ness is the author's reasoning. There are often several ways to prove a point, and I like to choose a way that complements the article's new-ness. Here is a new idea argued in a new way, there is another new idea argued in a new way, and soon we would have an article that one reads and re-reads with pleasure, which would then live long.

Consider the following articles: "*Will China Go 'Capitalist*'? (1982), and "*The Economic System of China* (2008, 2014). Because they are interesting, I am confident that both works would live long. As far as the second article is concerned, I knew this would be so before even starting to write. All the ingredients for interesting-ness were present: China's economic transformation, a historic event which happens perhaps once in a thousand years; being Chinese, I can write well in the language of billions; and the unique enrichment of content which comes from my long study of everything concerned with China; and my deep knowledge of institutional economics. Friends and American colleagues felt that nobody else could have done it, and so a slim volume of a hundred-odd pages is now fair on its way to classic status.

2.6 Truth is maintained by facts

Finally, a few words on truth as a necessary condition for intellectual longevity. This is the most important, and most difficult, thing for an author to achieve. In science, truth must be tested against facts. And because the world is a complicated place, the simpler the theory the easier it would be to isolate a set of crucial facts to do the testing. This also means that when choosing examples, the more detailed the supporting facts, the smaller the chance of making mistakes in the application. The difficulty is that generally, detailed facts cannot be obtained second-hand. And this is so, no matter how trustworthy the secondary source. Without support from detailed facts and concrete examples in this sense, it would be hard to convince the reader that the author is analyzing things that are true. And without widespread belief that it is true, the article would not be able to live long.

So the mantra is to push the facts as far as they would go, so as to explore the implications as far as they would go. I remember that when writing "Why Are Better Seats 'Underpriced?'" (1977), the Editor of *Economic Enquiry* wanted me to reduce the article's length by several pages. Though the argument can be readily developed to other areas, I acceded to his wishes and stopped short in the concluding remarks. So it came to pass that the paper's central idea was later taken over and mis-applied to the analysis of "efficiency wages". When that happened, I suddenly remembered something I heard from Paul Samuelson when I was a graduate student at UCLA: According to Samuelson, the author must seek to squeeze every implication from a paper and make them known. How true!

3 I sat at the feet of masters

In the autumn of 1959, the freshman Steven N.S. Cheung entered UCLA. I was 24 at that time. A little over 10 years later, I was appointed full professor at the University of Washington. That happened because of an unsolicited recommendation of a senior professor (Vernon Mund) with unanimous support from all full professors in the department of economics. To achieve full professorship at 34 was 5 or 6 years before the average age. Though I entered university at 5 or 6 years late, I still managed to save 10 years. This led people to say that I was a super-economist.

Super-economist or not, I was super-fortunate in my teachers. Though I was not good academically in primary and secondary school (I did not even pass the first grade in high school in Hong Kong), I managed to gain much more experience of the real world than my later classmates and colleagues in the US. During the second world war, when I was gleaning a bare existence among the rice fields of Guangxi, I was already learning about what I was later to describe so vividly about Chinese farming in Chapter 8 of *Theory of Share Tenancy*. Without this experience the book's explanation and interpretation of Asian agricultural data, which won applause from so many economists, would not have been possible. Numbers are one thing and facts are another, and without both one cannot really do economics research.

I lived through famine, and I know what it is like to be poor. After the war, when I was in primary school in Foshan, I was never warm in the body or full in the stomach. Then I returned to Hong Kong, where (being unsuccessful academically) I spend much of my time wandering the streets, playing around but also observing and learning. From 1953, when no school would take me, I went to work in my father's company and also spent some time in a factory. These years spent in the real world were essential to my subsequent development as an economist. Let me elaborate.

3.1 Science and the ability to predict

Economics is an axiom-based empirical science. So are all natural sciences. But in the social sciences only economics is founded upon explicit axioms. Consider history: Because it is not axiomatic, history is able to explain *ex-post*, but it cannot predict *ex-ante*. I admire the scholarship of historians and have learned much from them, but the historian is unable to say what will happen before the event. In 1981, I predicted that 'China will go capitalist'. This was a prediction before the event, and I was able to do so because economics is axiom-based. In other words, because of the existence of axiomatic foundations, economics can both explain and predict. This is the same thing Newton explaining and predicting the fall of the apple. The ability to predict then leads to the possibility that a prediction may be falsified by observations, and that is why hypothesis testing is the central feature of an empirical science.

Fundamentally, there are three axioms in economics. First, we have the law of demand; second, the concept of cost; and finally, the implications of competition. It was in the early 1970s that I proposed the now-famous parable of the banknote. Economics predicts that a $100 bill left on the pavement would disappear without fail. Such an explanation-*cum*-prediction is unique to economics among the social sciences. By the law of demand, if there is no police around the price of taking the money would fall, so (other things being equal) it would more quickly disappear. A man hurrying to a date would be more disinclined to pick up the money (other things being equal), since opportunity cost would increase. Finally, the greater the number of passers-by the more individuals would compete to pick up the note (other things being equal), and the rules of the competing game would emerge to determine who the winner is.

This parable may seem trivial, but fundamentally, these three axioms are all that economic analysis requires. In 1981, when I predicted that 'China will go capitalist', my reasoning was based on just these axioms. If we deem the law of diminishing marginal productivity to be similar in meaning to the law of demand, then everything I have written in economics these 50 years – the countless articles in English and Chinese – are all applications of the three fundamental axioms. All these years of thinking has produced one fruit, a simple but powerful way to analyze economic phenomena.

3.2 The real world as economic laboratory

Economics is an axiomatic empirical science, so it requires the support of facts. In the natural sciences, facts are found in laboratories. The laboratory of economics, on the other hand, is the real world. It is therefore beholden on economists to enter the real world, to run around its nooks and corners each and every day, and to observe. I did this during the *wanderjahre* before I entered UCLA, and have continued to do so. Though I was six years over normal age in 1959, I was also carrying real-world knowledge very much above the normal. With such a head start, I was able to progress rapidly.

Unlike in the natural sciences, it is possible in economics to first do laboratory work, and then to learn the theory. Some years ago my friends Vernon Smith and Charles Plott proposed 'behavioral economics,' in which laboratory experiments are modeled after the natural sciences. Though Smith gained the Nobel Prize for this work, it is something I have not been interested in. The real world is ever so complicated, therefore simplifying it artificially in a laboratory cannot be expected to be useful. The laboratory procedure may work under rigorous experimental control in subjects like chemistry or biology, but not when the economic real world has to be so much simplified as to lose relevance. The real world is there for all to see, and it is from careful observation that one learns how to identify facts to support analysis.

It is my advice, dear reader, that economics students today should study carefully *Theory of Share Tenancy*. The question of whether it is right or wrong is not important. What is important is the realization that this book cannot have been the written by a genius without knowledge of the real world. The PhD theses of Irving Fisher (*Mathematical Investigations in the Theory of Value and Prices*1892) and Paul Samuelson (*Foundations of Economic Analysis*1947) are without doubt works of genius, but they were written without significant reference to facts. My *Theory of Share Tenancy* does not compare in mathematical rigor, but it is firmly based on what happens in the real world.

3.3 I was super-fortunate in my teachers

My formal education in economics over the period of autumn 1959 (when I entered UCLA) to autumn 1965 (when I went to Cal State Long Beach as assistant professor) was 'the most and fastest' in my life. Very much aware that it was my academic last chance at 24, I signed up for every interesting course in sight in UCLA. I learned so much – from William Allen and Warren Scoville as undergraduate, and from Robert Baldwin, Jack Hirshleifer, Karl Brunner, and Armen Alchian in graduate school. My teachers were relatively young and not-yet superstar famous then, unlike today when Internet data bases are bursting with their references and citations. I went to Chicago as post-doctoral fellow in 1967, and discovered that because Milton Friedman and George Stigler had stopped teaching price theory in the early 1960s, I had actually been trained in the best economics department in the world. (It is a pity that this happy situation did not last, for Baldwin and Brunner were to leave UCLA around the time I went to Long Beach in 1965.)

My view is shared by many friends and colleagues: The 1950s and 1960s represent the golden age of American university education. After the Second World War the US was predominant in the world, but sadly from the 1970s students became anti-establishment, anti-professors, and the education system became myopically-focused on publication counting and journal rankings. It was sad, though personally I did not suffer from these negative developments. At the University of Washington (where I went in 1969), I was protected by the department head (Douglass North) from the papers-crunching. I felt like a prince in academia.

Looking back, I had no intention of becoming a scholar in 1959. I just wanted to discharge a debt to my father, who believed in me when he died in 1954. Then Warren Scoville, who taught me European economic history at UCLA, suggested that I should attempt graduate school and that I should work under Armen Alchian. Scoville thought I would go far in economics, and so I followed his advice.

3.4 Two years waiting for professor Alchian to return

I entered UCLA graduate school in 1961. At first, my aim was to earn a master degree and then return to Hong Kong to care for my mother. Armen Alchian was away visiting at Stanford, so I took the foundation analysis courses taught by Robert Baldwin. Baldwin was trained (and taught) at Harvard. His clearly-delivered and deeply-informed lectures were based mainly on the works of Alfred Marshall, John Hicks, Joan Robinson, and Paul Samuelson. I worked hard, and Baldwin was later to say that I was his best ever student.

My MA came in 1962. I had intended to quit, but Armen was still in Stanford. To have something to do while waiting for his return, I enrolled in the PhD program. From the autumn of 1962, I spent most of my time in Jack Hirshleier's lectures. These were based mainly on Fisher and Friedman. Since I was already a MA, I was present as a sit-in student. No status and no credits, of course, but nobody bothered. I had a tendency to interrupt the classes with questions and argue the problems with all and sundry, and everybody knew that the hoo-ha was encouraged by the professor. After 6 semesters Hirshleifer did not tell me that I was his best student, but a letter supporting my promotion to full professor in Seattle he compared Steven N.S. Cheung to Irving Fisher!

At that time, a PhD candidate must pass coursework in 4 areas, the most important of which was economic theory. On past performance I was confident of doing well, but as I wanted to take the examinations only after listening to Alchian, I put everything back for two years to the autumn of 1964. It was my best decision as a student.

Alchian finally returned in the autumn of 1963. I rush to hear him, only be scared out of my wits – I could not understand a thing! Professor Alchian gave out no reading lists, walked up-and-down talking more to himself than the class, and did not seem to care whether the students received systematic instruction. But I was sure of my ability, and persevered for 6 semesters. (Even after moving to Long Beach in 1965, I drove back to UCLA every week to attend Alchian's lectures.) In the middle of this time, after three semesters of hard thinking, I understood. It was like attaining enlightenment, after which I became confident that one day I may reach the same heights. The idea of spending my life in academia took hold then.

3.5 Karl Brunner was a great scholar

I must not fail to talk about another of my great teachers in UCLA, Karl Brunner. At first, Karl did not think much of me. In the early 1970s, however, he wrote me a letter praising my works. At a meeting in San Francisco in 1995, I discovered Karl Brunner sitting in the front row, right opposite the lectern where I was to deliver a talk. I was so happy that I nearly forgot what I was supposed to say. Then followed the simple but to me profoundly important exchange. Brunner: "Steve, you are now an economist!" Cheung: "Prof, I have worked hard!"

Everybody who knew Brunner agree that he was a great economist. But even today, with the facility of Internet data bases, I think he is under-rated. Brunner was the most rigorous mind I have ever seen in economics, period. I hope that the younger generation of Chinese economists would take note of this man.

3.6 My graduate school contemporaries were superb

I had 5 or 6 classmates in the UCLA graduate school who were best of the best. Jack Hirshleifer once remarked that he has never seen such a gathering of bright young minds. Hirshleifer taught at Harvard and Chicago before he came to UCLA, so such a statement cannot be deemed to be off-hand. The group would meet almost every day to talk economics, especially about scientific method. Friedman (1953) had published his famous paper "On the Methodology of Positive Economics," and we students went at the problem hammer-and-tongs. We were privileged for 3 reasons. First, we knew by heart Alchian's (1950) seminal paper "Uncertainty, Evolution, and Economic Theory" in *Journal of Political Economy*. Second, we all went to Rudolf Carnap's lectures on the philosophy of science, and so were well-schooled in the problem of empirical testing of hypotheses. Third, and the most important for me, was the influence of Karl Brunner. He was also present in Carnap's lectures, and of course knew more than us students put together. I (and other classmates) often asked Brunner questions about logical rigor in economics, and (for me at least) the brief replies have remained fresh in my memory.

3.7 Fisher was better than Keynes

At that time, I was all at sea about Keynes' 'saving –equal- investment equilibrium'. Then Brunner told me: "Don't worry about *ex-ante* or *ex-post*, and don't worry about voluntary or involuntary. Observed saving is always equal to observed investment. What is not observed is not observed, and only in equilibrium would the non-observables be equal. This is the same as supply-and-demand analysis: the observed quantity sold is always equal to the quantity purchased. What is not observed – quantity demanded and quantity supplied – are matters of volition, and they are made equal in equilibrium." These few words from Brunner taught me that in economics, we cannot observe 'equilibrium'—equilibrium in economics is a concept not a fact.

At that time, I was deep into Fisher's (1930) *Theory of Interest*, where saving and investment are different angles looking at the same thing. With Brunner's help, I was able to understand who was right and who was wrong. After abandoning Keynes for Fisher I was able to point out that some types of investment, like purchasing antiques, would have no effect on output and employment, so that the foundations of Keynesian economics is seriously flawed.

If the student is lucky, a short remark from a great teacher can lead to years of productive thinking. It is a pity that I have had no opportunity to thank Brunner face-to-face how he explained Keynes to me in one minute. I once asked him: "What do you think of Fisher's *Theory of Interest*?" Brunner's reply: "It is a great book!" Because of this, I read and re-read Fisher, to my great benefit.

4 In search of my way in economics

An outsider would say that I advanced very rapidly over the years 1959 to 1965 in UCLA. If I did not have to wait from Alchian to return to the university, chronologically I would have saved two years. Alchian often remarked that he had never seen a student work so hard for so long. Looking back, I did not work that hard, nor did I pretend to do so. Apart from academic work, I spent quite a bit of time doing photography and fishing. To earn pocket money I would deliver newspapers, mow lawns, grade papers, or work at the till in the university carpark. (The last two jobs often at the same time.) I was research assistant to Robert Baldwin and Karl Brunner, and from 1962 teaching assistant in the department of economics.

Much of my free time was spent in the student's amenities center. I was good at pools, bridge, and I was university singles and doubles champion in table tennis. What I did not do was to go to parties. My friends outside of economics were all art- and music- lovers.

4.1 Ploughing through the literature

The articles I studied over that time – really studied – could not have been more than two dozens. I also flipped through several hundred books, but only quickly. During the 3 years when I was auditing Hirshleifer and Alchian, I was always in the library. At that time the library had no closing hours, and I occupied a cubby-hole which was just large enough to accommodate desk, chair, bookshelf, and mattress. There was a power-source where you can heat up canned food, so I was able to literally live in the library. A few students in other disciplines did likewise.

I would push a cart around and collect books and journals, quickly turning the pages to find something new, and then the library assistants would replace the material. (Library regulations did not allow students to do that.) I did a lot of book-flipping, because I soon discovered that most of the time it was just the same ideas being put into different words. I also learned that authors tend to group themselves in 'schools'. Once in a while I would come upon something new, and then it was time to pause and think.

4.2 Enlightenment from studying Knight

I spent 2 years poring over Coase's classic "The Problem of Social Cost" (*Journal of Law and Economics*1960). I was also doing the same with Frank Knight's equally classic "Some Fallacies in the Interpretation of Social Cost" (*Quarterly Journal of Economics*1924). I saw that Knight and Coase were saying similar things; only Coase wrote more clearly and introduced the idea of transaction costs. I have often written about Coase's influence on me, so it is time to complement this by talking about Knight.

I learned two very important things from Knight. First, his paper began by arguing that in economics, assumptions must correspond to facts. (At first blush, this is saying something different from Friedman's later proposition that 'realism of assumptions does not matter'.) Ernest Nagel's (1963) paper on assumptions had taught me that assumptions appear in different types. So, which assumption is the most realistic *a la* Knight? After some hard thinking, I finally came to the conclusion that in economics, assumptions which are concerned with constraints must correspond to facts. This idea then led me to a proposition which has remained unchallenged in the profession: If a laboratory experiment requires a clean test-tube, we cannot use a dirty test-tube and then assume that it is clean.

The second thing I learned from Knight followed from studying his criticism of the "case of two roads" in A.C. Pigou's *Economics of Welfare* (1920). Knight was of course correct, but more importantly, implicit in his analysis was the fundamental idea of 'dissipation of rent'. This term was coined in H. Scot Gordon's "The Economic Theory of a Common-Property Resource: The Fishery" (*Journal of Political Economy*1954), but doubtless it was in Knight's mind long before that. Indeed, Gordon's diagrams are so similar to Knight's that the latter work should have been cited. (Coase's paper also did not refer to Knight.) It is thus that a genius author pays the price for prolixity when writing a great paper. Drawing upon Knight and Gordon, I went on to publish "The Structure of a Contract and the Theory of a Non-Exclusive Resource" (*Journal of Law and Economics*1970) and "A Theory of Price Control" (*Journal of Law and Economics*1974), both of which applied and developed the concept of rent dissipation. Forty years on, these two of my intellectual progeny look set for a long life.

I met Knight for the first time in 1968. It was at Robert Mundell's party in Chicago, and I lost no time to say to the great man how much I learned from his 1924 paper. Knight thought a bit, and then replied with a sigh, "That was a long time ago." I have not referred to Knight that often in my English publications, and yet the *Wikipedia* article on Knight lists among the 5 "influenced by" economists the name of Steven N.S. Cheung. The other 4 individuals named are all Nobel Laureates, and though I have yet to join this privileged group, it is a great honor that other economists can detect in my writings the spirit of Frank Knight.

4.3 Choosing a doctoral research topic

I began to think about a dissertation topic during the two years spent waiting for Alchian to return. At that time, requirements for a PhD in economics in major US universities were severe: UCLA awarded one every few years, against nowadays when several individuals gain the degree every year. Regulations stipulate that a doctoral thesis must be original. I found that books in the library were generally based on one another, with footnoted references galore. So, how to be 'original'?

I immediately rejected welfare economics. Baldwin's lectures and my own reading made clear that this area is a dead end. Hirshleifer's teaching on Fisher were inspiring, and (later on) he suggested that I try to introduce uncertainty into the theory of interest. That was one of the big problems – it still is – and though it attracted much effort from economists (Hirshleifer included), there has been no break-through. Fisher has (almost) done it all, and the remaining problems seemed to require a life-time and not three or four years.

I tried to add risk or uncertainly to Fisher for a few months, but found no valid way to measure them. So I turned to a thesis requiring hypotheses testing. I knew quite a bit about scientific methodology, and it was Alchian's favorite subject. Friedman and his Chicago colleagues were busy testing hypotheses in the quantity theory tradition, but though their efforts were attracting much attention (and though Milton and I became close friends later), I did not find the topic interesting. What I found to be interesting was another Chicago topic – Aaron Director's oral tradition on tie-in sales – but could not accept that the objective was price-discrimination. It was, however, under Director's influence that I chose the contractual approach when analyzing share tenancy in agriculture. That was in 1966, when I was teaching at Long Beach.

While searching for a thesis topic, I consulted Alchian and Hirshleilfer. Alchian said that it was my dissertation, so I alone should decide on what to do and how to do it. He then suggested that I study a few PhD master-works and then see what happens. So did Hirshleifer, and so I went and read Fisher (*Mathematical Investigations into the Theory of Value and Prices*1892), Knight (*Risk, Uncertainty and Profit*1921), Lerner (*The Economics of Control*1944), and Samuelson (Samuelson 1947). Apart from Knight, the theses in question were all 'pure theory' and so had little to do with hypotheses testing. Knight's thesis was undoubtedly a great work, but I found his fundamental distinction between "risk" and "uncertainty" unconvincing.

To produce original research in hypotheses testing, the subject must of course be 'new'. Behind the hypothesis would be a theory, which in some degree would have to be original. However, I found the materials on economic development, though original, were stupid and embarrassing. My teachers – Alchian, Hirshleifer, Baldwin – taught me price theory most thoroughly, so I decided to seek my dissertation topic in that area. But where can I find a hypothesis which was new and interesting, and empirically testable? I had never done it before, and so found the exercise to be excruciatingly difficult. Of course, once I wrote up *Theory of Share Tenancy*, it became easy: For an experienced pathfinder, going down a rocky road does not need much effort!

4.4 Rent control and the Meiji Restoration

My first serious topic was rent control in Hong Kong. I soon discovered that interpreting and analyzing the quantity of complicated data available would likely consume more months and years than a PhD candidate can afford. Afterwards as professor in Seattle I returned to this problem with a vengeance, and produced "A Theory of Price Control" (*Journal of Law and Economics*1974) and two other articles on rent control and housing construction. This "Theory" article, which I am confident would leave a lasting impact, makes up for the early disappointment.

I then turned to Japan's economic growth after the Meiji Restoration. Here was a new and interesting topic, especially since I discovered an English language work on Japanese economic history which discussed in some detail the changes in land use- and transfer- rights from the Tokugawa era to the time of the Restoration. I was determined to work on contracts and land ownership, and here was a very useful reference to a clearly-defined time and place. Alchian and Hirshleifer were encouraging. But I did not know Japanese, and Alchian raised the question of whether data contained in one English-language work would be sufficient, and whether other references were available.

I started to search the literature, all the way to Prof. Henry Rosovsky at UC Berkeley. We had a long talk on the problem, during which Rososky explained that the empirical material was all in Japanese, and that a PhD student who is unable to study the data first-hand would find the topic impossible. Years afterward I learned that Rosovsky subsequently moved to Harvard, and people there considered him to be the best head of their economics department ever. Rosovsky was another master I was fortunate enough to meet as a student, and I still remember that day in Berkeley he told me Coase's social cost paper was very deep and difficult.

4.5 A respite in photography

In the summer of 1964, I decided to take a break from economics, leave the library, take up my camera and resume my one-time profession in Toronto in 1958. I did a lot of photography, mainly in a small garden near the UCLA campus. Professors and classmates all wondered where I had disappeared to, but in two months I managed to discover a new way to choreograph light through the lens. The results were striking, and I remember the many professors, friends, and colleagues who were present at my photography exhibition in UCLA in 1965. An enlarged collection was shown in Long Beach two years later, which received enthusiastic reviews in the newspapers. The response was so overwhelming that, because Long Beach Museum was planning for a new building, the curator proposed it would open with an exhibition of my new photographic works. To prepare for this event, I travelled around the US northeast for two weeks taking pictures. The results were magnificent, but alas! Nearly a hundred rolls of film were then stolen in New York City. The next time I took up serious photography would be 20 years later in Hong Kong. I managed some very good works in portraiture and lighting, and in 1988 I did Friedman's official portrait. This work has since appeared regularly in the Internet, so it is another of my artistic progeny which would live long.

5 Cal State Long Beach and the birth of theory of share tenancy

UCLA regulations at the time require the individual to pass qualifying examinations in four areas or fields of economics before attempting a PhD dissertation. The most important of these was economic theory. Because I decided to take the examinations only after understanding Alchian's lectures, I had two years to mull over a thesis topic before taking these exams.

The qualifying examinations were a serious matter, which at that time distinguished the US PhD from the British PhD. Some people argue the American version was more difficult because of the additional requirements, while others say that British thesis-writing was more demanding. Graduate coursework for PhD at UCLA generally took two to three years, and though the examinations were not easy, most of my classmates who failed to go all the way stumbled because of the thesis.

There were two semesters in the academic year, so the general rule was one qualifying examination per semester. In the autumn of 1964, I sat all 4 papers in 5 days, scoring three first and one second. My classmates stood in awe, but they did not know I was following Alchian's advice when I prepared for those exams: economics is economics, period, no different fields! I had been thinking about economics all day, every day, so I just answer the exam questions with economic analysis, regardless of what field the questions belong. The good examination results, however, did not mitigate my worry: the difficult part – the dissertation – was still to come. Having tried rent control and the Meiji Restoration for thesis topics without success, I decided to go somewhere for a change of scenery and then try again. That was in the spring of 1965.

1965 was perhaps the year when it is the easiest to find a job in economics. Before I even started to mail out applications, I had 4 job offers. Three places – University of Alaska, University of Sussex, and University of New South Wales – sent employment contracts without receiving my application for a job. The market for economists was so good that many mathematics and physics students changed over to the subject.

I went to California State College at Long Beach, mainly because it was just an hour's drive away from LA, Alchian, and Hirshleifer. I just walked in there, asked for a job, and received an offer the next day. My appointment as assistant professor was for 3 years, beginning in the autumn of 1965. The pay was double what I was earning as teaching assistant at UCLA, but in return I had to teach double the load – 12 hours a week. A few teachers and friends frowned upon my choice, saying that once an individual enters a teaching college, no research-oriented university would consider.

5.1 Help from Eldon Dvorak

It was my great fortune that in Cal State Long Beach I was assigned to share an office with Eldon Dvorak, who was then associate professor. Eldon was a few years my senior and a PhD graduate of Washington University in Seattle. Though he had not published much at the time, Eldon was well-trained and a good teacher and administrator. Later he was to promote the Western Economic Association, which is now the second-largest professional society of economists in the US.

Just a few weeks after I arrived, Dvorak was telling colleagues that one day in the future, Cal State Long Beach would be boasting that Steven N.S. Cheung once taught there. Hyperbole, of course, but it did me no harm! A year later, he (and some students) got the state college board to give me the Distinguished Teaching Award in economics among 18 state colleges. That did me a power of good, because the award removed all questions about language and communications skills on the part of a Chinese instructor. My photographic exhibition in Long Beach Museum was also arranged with the help of Dvorak. He found me a $500 grant from the College, and when funds were running short, Dvorak went and hammered together several dozen photo frames in his garage.

5.2 Land reform in Taiwan

Things began to look up in the spring of 1966. I found a Chinese article describing how in 1949, after land reform which drastically reduced the landlords' income share, farm output in Taiwan increased significantly. It was puzzling: More regulation, and yet output goes up? I thought it must be anti-Communist propaganda, but still I went to the library to see what facts I could unearth. The library at Cal State Long Beach was very small, but for some reason it possessed a complete set of *Taiwan's Agriculture Yearbook*. Much to my surprise, this government publication contained nary a word of propaganda: Nothing but rows and columns of detailed statistics.

A complete set, one for each year. I borrowed the whole set, and found myself looking at data carefully classified according to location, type of farmland, type of crops, and time under fallow. There were also detailed output and statistics. After two weeks of close scrutiny, I was not able to find any inconsistency or methodological contradictions in the data.

It was mainly with these data, and the knowledge of Chinese farming I gained when wandering in the fields of a poor Chinese village during the war. I was eight then, but I vividly remembered at thirty. The result is Chapter 8 of *Theory of Share Tenancy*. Alchian was much impressed, and asked to see the source. After expressing surprise that a government department should spend so much time and effort collecting such detailed data for no apparent reason, Alchian suggested that I double-check with the Taiwanese authorities. The answers that came back met with Alchian's approval. Some years later, Ronald Coase was to remark: "Regarding the quality of empirical work in economics, Cheung's *Theory of Share Tenancy* is unsurpassed."

5.3 Solving the puzzle in one night

Regarding the derivation of the theory proper, in March 1966 I sat down one night to find the conditions under which a regulation that reduces the landlord's share in farm income would increase output. I first worked out a theory of sharecropping with no such regulation. Understanding that the share contract was not in terms of a price, but a percentage share, I extended Aaron Director's ideas on tie-in sales and introduced an additional stipulation into the contract. The solution emerged in two hours: the competitively determined sharing percentage requires one more contractual stipulation than a fixed-rent contract to attain equilibrium under competition. Share tenancy, owner farming, farming with wage laborers, and renting land on fixed rent – all share the same result. What was delightfully surprising was that when I added the government's restraint to reduce the landlord's sharing percentage, farming output must rise to produce another competitive equilibrium.

I have recounted in earlier writing how it took me 3 hours to convince Eldon Dvorak that my analysis was correct, and how the 11-page manuscript I prepared to explain my findings when presented at UCLA in May 1966 was rejected by everybody in the room. Professors and schoolmates alike simply could not accept my conclusion that government regulation will increase output! One day after the disastrous UCLA presentation, Alchian called and asked for permission to use my 11-page manuscript for his seminar discussion. I was delighted, and over the next month I enjoyed the pleasure of receiving phone calls from friends attending that seminar, saying nobody managed to find a flaw in my analysis. However, it was not until the spring of 1968, one year after I earned my doctorate, that with a stroke of luck I found actual samples of tenancy contracts used in Chinese agriculture, showing clearly the stipulation I added to the share contract in that memorable evening of March,1966.

5.4 Green light from Alchian

After their seminar discussions, Alchian told me to start writing the dissertation. He said I should expect to take 2 years, but I finished everything in 8 months. My main analytical problem emerged in June 1966 – a big one – involved the empirical measurement of marginal magnitudes. Changes in the average productivity of land are available in the data, for different crops in great details. But how could I confirm with these average figures the central implications of my thesis, namely, under the share restriction imposed by the Taiwan government, the marginal products of tenant labor will fall and the marginal productivity of land in tenant farm will rise, resulting in an inefficient situation because the marginal products of the same factor become unequal between owner cultivation and tenant cultivation under the share restriction!

5.5 Chapter 8 is a masterpiece

The task was to confirm the implied changes in marginal products of land and of labor, through manipulating the observable changes in the average products of land for different crops. Chapter 8 of the sharecropping book presents the findings. This is amongst my best empirical works, on par with my works on the petroleum industry and my light-hearted piece on the bees, in rigor and in convincing power. It is a pity that people do not read that chapter. When Alchian read it in l967, he rushed to Hirshleifer's office and exclaimed: "Steve got it!" When Harry Johnson read it in 1968, noticing I open the chapter by saying that I intend to use changes in the average product of land to deduce the implied changes in marginal products of land and labor, he wrote on the side of the manuscript: "This is impossible!" But when he got to the end of the chapter, he wrote: "This is truly a great chapter!"

6 Climbing up the ladder: the University of Chicago

I had a photographic memory as a student. Now I am over 80, and many things have been forgotten. A few discrepancies in times and dates are not important, for my primary purpose in writing about my intellectual journey is to recall how neo-institutional economics developed, and how it took a wrong track towards analytical disaster. I am well-suited to do so, because I was at the center of things from the beginning.

6.1 First important publications

In November 1966, I felt the core chapter on the theory of sharecropping was ready for publication. I sent the manuscript to two places – first to the University of Chicago Press, and second to *American Economic Review*. A positive reply from UC Press quickly arrived: On the strength of one chapter they would be happy to publish the whole book, which the Press hoped would be completed soon. The editor of *AER* also quickly replied: Good work, but a revision to use the traditional 50:50 share was required. I did not reply to AER. In 1968 I re-submitted the manuscript to *Journal of Political Economy*. Back came editor Robert Mundell's comment: "You are wasting ammunition: Divide your paper into two parts and *JPE* will print the first. The second part you send to Coase's *Journal of Law and Economics*." I revised the first part of the manuscript at the beginning and end and returned to Mundell. The second part? It took half a year to incorporate some new and important materials I found in Chicago, and then I published it in *JLE*. Thus were born the two noted papers: "Private Property Rights and Sharecropping", *Journal of Political Economy*1968; and "Transaction Costs, Risk Aversion, and the Choice of Contractual Arrangements", *Journal of Law and Economics* 1969.

6.2 Evsey Domar, Arnold Harberger, and my appointment in Chicago

In December 1966, I received out of the blue an invitation from Evsey Domar to his New Year party. I knew, of course, who Domar was: He was professor at MIT and was then visiting the Rand Corporation in LA. We were not personally acquainted, but I went nonetheless. In the middle of the party Domar asked where Steven Cheung was, took me into the kitchen, told me Hirshleifer had passed a chapter of my thesis to him. He said on the strength of that I should go to MIT. Of course I said 'yes', but after a month Domar wrote to say there were no openings at MIT. Domar also said the he had sent my chapter to D. Gale Johnson at Chicago in the expectation that I should follow Kenneth Arrow and Robert Mundell and join the department of economics there as a post-doctoral fellow.

I had criticized Johnson's work rather severely in that chapter, and so thought it better not to follow things up. Hirshleifer called to say otherwise, and so I had to put in an application. It was a very short letter, but two days later Arnold Harberger, who was department head, cabled to say I had won the award. The stipend was $8000 a year tax-free, and I could do whatever I wished in Chicago for one year.

The difficulty was that I had only completed one chapter of my PhD thesis: So how could I be appointed post-doctoral fellow? I asked for a one-year postponement, only for Harberger to brush off my arguments by saying that in Chicago nobody would bother with such a trivial matter. Though I was much affected by a family tragedy in Hong Kong at that time, I spent Alchian's gift of $500 to hire a typist and finished the thesis in 6 weeks.

6.3 Friendship with D. Gale Johnson

I went to University of Chicago as post-doctoral fellow in autumn 1967. I have described elsewhere the larger-than-life characters I encountered there, so I will confine my attention to what happened to *Theory of Share Tenancy* in Chicago. The first thing I did was to bring what I thought would be the book's final draft to University of Chicago Press, where the editor promised speedy publication. My next stop was the office of Professor D. Gale Johnson, world-authority in agricultural economics and Dean of the Faculty of Social Sciences.

Johnson was the best academic administrator I have ever met. He was fair, objective, keen on promoting talent, and never said anything before thought. He was well aware of my criticism of his work, but as chairman of the selection committee was adamant that I and none other be awarded the post-doctoral fellowship. He was busy all day, every day, but always had time for me. I had discovered some new and important materials in the libraries at Chicago, but UC Press was getting impatient. So I went to Johnson for advice. The reply: "Normally, when University of Chicago Press wished to publish your work, one would say 'yes, yes, right away'. But your book has a chance to become classic. For a scholar, this may be an once-in-a-lifetime opportunity, so you should spend another year to perfect the manuscript." I followed Johnson's advice, and I was fortunate enough to find important materials in the Chicago libraries I did not know existed when I wrote the dissertation in UCLA. The new material allowed me to expand two sections in the manuscript into two lengthy chapters, and to say something new and important. The finished product is the *Theory of Share Tenancy* as published by the University of Chicago Press in 1969.

6.4 Chicago's great libraries

The University of Chicago Main Library is the best I have ever used. Books and articles galore, of course, but the most remarkable thing about the library is the service. Any book you want, even if it is not available in the institution itself, they would borrow it from elsewhere very quickly. In the event that the item is 'not for loan', a photocopy would be supplied post-haste. Using the library, I read up everything written on sharecropping since Adam Smith. This became Chapter 3 of *Theory of Share Tenancy*. The students who read that chapter carefully would learn what 'scholarship' is all about. It is a great library that generates great scholarship.

6.5 Scholarship in Cambridge UK

The library's resources allowed me to trace a number of articles and books listed in A.C. Pigou's classic *Economics of Welfare* (1920) back to their sources. Pigou made many references to land tenancy in agriculture, especially about how tenant farming would lead to inefficiency and divergence between private and social costs. Anything Pigou referred to that I had not read before, I would ask the library to find for me. In the end, I had to conclude that Pigou's 'reference list' did not support his claims at all!

I also discovered something similar about a sharecropping paper by Henry Higgs (1894), published in *The Economic Journal* as a lead article when Alfred Marshall was the editor. That article covers only one sharecropping farm in France, and the sharing percentage happens to be 50–50. (Marshall then used 50–50 as custom determined in his analysis of sharecropping) Pigou was Marshall's student, representing the Cambridge tradition, and the materials I dug up from the Chicago library suggested that that tradition was really sloppy. So here is the fate of economics as an empirical science: when someone produces by imagination a "representative case"; another economist cites this, and then another. Lo and behold! Something is then established as fact.

6.6 Chicago's Oriental Library, small but a treasure trove

I also went often to University of Chicago's Oriental Library. It was small, but I discovered a gold mine there. In the 1920s and 1930s, J.L. Buck (husband of Pearl Buck) was researching Chinese agriculture in what is today's Nanjing University. I had, of course, read his works, but did not know that Buck had many able assistants who also wrote (in Chinese) on the subject. I discovered their works in the Oriental Library. Their data matched those in several Nationalist government reports on Chinese agriculture (which were also in the library), so I was confident that here was something valuable.

The new materials I found in the two Chicago libraries allowed me to do a number of things. I was able to add to the analysis in Chapters 3 and 4 of *Theory of Share Tenancy*, to write the two appendices at the end of the book, and to discuss in further detail fixed-rent and share- agreements in 22 Chinese provinces. When D. Gale Johnson saw the factual sharing data of China, which I put in an appendix, he exclaimed that had he known these data what he wrote on sharecropping would be completely different.

6.7 The beginnings of contract economics

I also found in the Oriental Library samples of written contracts on farming in China. They were all there: Fixed-rent contracts, sharing contracts, iron-sheet contracts, fixed-rent contracts with escape clauses for bad harvests. It was an important find, for here is additional historical evidence of the things I have been analyzing. But far more importantly is the implied question. According to my share-cropping analysis, different contracts would lead to the same outcome. Then how to explain what I had found? Why is it that sometimes we would observe one kind of contract and at other times another kind? Asking this question marked the beginning of contract economics.

After some hard thinking I settled on two explanations: risk aversion and transaction costs. The resulting article in the 1969 *Journal of Law and Economics* ("Transaction Costs, Risk Aversion, and the Choice of Contractual Arrangements") received high praise from pundits like George Stigler and Aaron Director, and led to the development of principal-agent theory and the award of more than one Nobel Prize. On my part, I did not feel comfortable with the analysis even then. It is only now, after several decades of hard thinking, that I finally manage to properly answer the question of why different contracts are chosen.

7 The bliss of dawn in Washington and the disaster in neo-institutional economics

Two years after Chicago I went to University of Washington in Seattle. Friedman said I was abandoning my place in the sun for somewhere out in the wilderness. Coase tried to dissuade me, saying that in Chicago I would have a chance to become the next Alfred Marshall. Harberger said if it was a matter of money he could arrange. But for me, Chicago was too much of an academic cauldron. There were visitors and seminars by the dozen every week, and a never-ending flood of manuscripts to read and comment upon. It would not do if I did not participate. Ten years had lapsed since I began studying economics. It was time for me to find a place where I could think things out alone. Anything coming out I liked to discuss with colleagues, but to produce interesting ideas I needed time by myself. I also decided then to stop reading the literature. For whatever I wanted to know I would consult colleagues.

7.1 Douglass North and Yorem Barzel

I arrived Seattle in autumn 1969, and after a few months was made full professor. More important was what the department head (Douglass North) and the dean (George Beckmann) told me: "Publish or perish" did not apply in my case. All I needed to do was to do what I liked. North was committed to promote and encourage ideas, and I was to be given free rein in the department.

Even more important was my friendship with Yoram Barzel. This man's brain was subtle and could detect the smallest of flaws in an argument. With his company I felt free to wander the strange seas of thought alone, safe in the knowledge that whatever I produce would not err in reasoning. We were to discuss and argue, and argue and discuss for 12 years. Barzel was away when I wrote "The Enforcement of Property Rights in Children, and the Marriage Contract" (*Economic Journal*1972). All of my other English publications, from "The Fable of the Bees" (*Journal of Law and Economics*1973) to *The Economic System of China* (2008), were read and criticized by Barzel.

I had a manuscript with me when I arrived in Seattle. It was later published as "The Structure of a Contract and the Theory of a Non-Exclusive Resource" in *Journal of Law and Economics*1970. In this article, I pointed out that it is impossible for a contract to be 'complete': In practice, any contact must additionally refer to things like custom, religion, and common law. A complete contract to rent an apartment would require lawyers to write pages and pages and then more pages! It is unfortunate that the idea of "incomplete contract" later became an industry and won Nobel Prizes.

7.2 The Washington School

The description "Washington School of Economics" likely originated in North's (1990) book, *Institutions, Institutional Change and Economic Performance*, perhaps the most cited work in twentieth century economics. There, in a footnote (pp. 27–28) I was singled-out as the founder. The individuals in question were myself, Barzel, a few young assistant professors, several superb students, and North with his small group of historians. It was a small cohort, but strong enough to ensure continuation after 1982, when North and I left. Barzel deserves most of the credit for this, for he nurtured a number of young economists who published in the Washington tradition. In the 1970s, North and I had no inkling that discussions and arguments among a few individuals and the resulting publications would create a school of thought in economics.

What was the Washington School's agenda? First, hypotheses testing a la *Theory of Share Tenancy*. Barzel was expert in statistics, but most Washington group would not let the data speak for themselves. They went the route of hypothesis testing. Second, the focus on transaction costs. After the seminal work of Coase, this concept was not often applied to explain real-world phenomena. Even a master of transaction costs like Harold Demsetz was mainly concerned with policy implications. We in Seattle were completely uninterested in normative analysis, only with the problem of how transaction costs can help explain observations. Third, the importance assigned to the idea of rent dissipation. The key influence here is my "The Structure of a Contract and the Theory of a Non-Exclusive Resource" (*Journal of Law and Economics*1970), which supplied the first valid analysis of how rent dissipates. Barzel was a strong supporter of the idea: Indeed, he proclaimed my 1974 *Journal of Law and Economics* article "A Theory of Price Control," which proposed the idea of reducing rent dissipation to interpret the effects of price control, to be the best piece of economic analysis he had ever read. Unfortunate, this article is so deep (perhaps poorly written) that few individuals have been able to understand my arguments and extend them. It does not matter! The series of articles I published in my Washington years have all become classics.

7.3 The development of neo-institutional economics

Amongst the forerunners of neo-institutional economics, the names of F.A. Hayek (1899–1992) and F.H. Knight (1885–1972) must be mentioned. I had the privilege of meeting them when they were still alive. In the 1960s, the *mise en scene* of institutional analysis contained a cast of five individuals. By age, they were:

First, Aaron Director. Director was the source of the Chicago 'oral tradition' of tie-in sales and founding editor of *Journal of Law and Economics*. Without Director, Coase's seminal contributions would not have gained prominence. I got to know Director when I was in Chicago, after which he read, criticized, and encouraged everything that I did. Barzel heard me praise Director so much and so often that he also became a good friend of the great man.

Second, Ronald Coase. Coase's contributions to economics are seminal: "The Nature of the Firm" (*Economica*1937), "The Federal Communications Commission" (*Journal of Law and Economics*1959), and "The Problem of Social Cost" (*Journal of Law and Economics*1960) appear in every economist's 'must-study' list. I knew Coase's works by heart as a student, but only got to know him when I went to Chicago in the autumn of 1967. The Coase-Cheung story is now classic in the profession, and I had the honor to write the entry "Ronald Harry Coase" in the *New Palgrave Dictionary of Economics*.

Third, Armen Alchian. Armen taught me in UCLA, and I can also claim the honor of writing the entry "Armen Albert Alchian" in the *New Palgrave*. Alchian is acknowledged to be the founding father of property-rights economics. I sat at his feet for 6 semesters. He (and Jack Hirshleifer) was my mentor during the writing of *Theory of Share Tenancy*.

Fourth, Harold Demsetz. In 1962, I was his grader in UCLA. After Demsetz went to Chicago Alchian asked me to read a thick manuscript by Demsetz. It was subsequently published as two separate articles. Demsetz's application of transaction costs to analyze Pareto optimality in welfare economics was impressive, and I considered his critique of government intervention as amongst the best ever. However, in my price-control piece I argued that if all constraints are taken into account, the Pareto optimality must always be satisfied.

Fifth, Steven N.S. Cheung. I claim to be in the middle of things from the beginning, because I not only was a friend of Director, Coase and Demsetz, but also was taught by Alchian for six semesters. Alchian's ideas on property rights were presented mainly in his lectures. My contributions to the neo-institutional economics began with *Theory of Share Tenancy*, and then everything I have written thereafter are nearly all on contracts.

7.4 Outside adventures in Seattle

In 1967, the National Science Foundation gave me a grant to research patents and trade secrets. My interest here is mainly concerned with patent and trade secret licensing. A lot of money was spent, but the result is disastrous because it was too costly to decipher the documents! Two of my team members managed several respectable publications; I wrote a long and learned report to the NSF; but that was it. However, my paper on trade secrets is still being cited, and there is a chance that my lengthy report will someday be properly recognized.

Another adventure concerned a large anti-trust study directed at the oil industry. I spent three years on two reports, totaling 375-pages. Alchian considered them the best empirical research ever. These are consulting reports. More than 40 years have lapsed, and I may ask for permission to publish them. However, the reports are so highly specialized that the interested population must be very small. Time flies, the present-day readers may not believe that crude oil was selling around $1.25 a barrel when I wrote those reports.

7.5 Consequences of risk aversion and shirking

The Washington School was noted for its contribution of neo-institutional economics. I consider this school to be analytically superior to what subsequently emerged as the subject's mainstream, although we lost out in the number of publications. I consider several mainstream ideas were methodologically disastrous, and that it was all due to Chapter 4 of *Theory of Share Tenancy*, my paper "Transaction Costs, Risk Aversion, and the Choice of Contractual Arrangements", published in *Journal of Law and Economics* in 1969.

The problem I investigated was new and important: How is the form of any contract determined? In the answer I made two wrong moves, one with small and one with enormous consequences. The first mistake concerned risk aversion and its application to the choice of contracts. Uncertain what 'risk' was, I used output variance as a measure. The harmful consequences were small, and years later I replaced risks with information costs to obtain satisfactory analysis.

My second mistake – I proposed the idea of "shirking" – led to serious consequences. Alchian and Demsetz made use of this idea in "Production, Information Costs and Economic Organization" (*American Economic Review*1972), which became that journal's most-cited paper. My point, on the other hand, was (and still is) that shirking is impossible to observe, and that we cannot use non-observable magnitudes to formulate testable hypotheses.

7.6 Posthumous fame to Guangxi boatmen

In 1970, Toronto's John McManus was my guest in Seattle. I chatted to him about what happened when I was a refugee in wartime Guangxi. The journey from Liuzhou to Guiping was by river, and there were men on the banks whose job was to drag the boat with ropes. There was also an overseer armed with a whip. According to my mother, the whipper was hired to do just that by the boatmen!

My tale went the rounds, and it was seized by a number of neo-institutional economists. I tried to dissuade McManus, tell him not to publish his piece based on my Guangxi story, but he went ahead nonetheless. (See his "The Cost of Alternative Economic Organizations", *Canadian Journal of Economics*1975). In 1976 Jensen and Meckling (1976) published a widely-cited paper in *Journal of Financial Economics* ("Theory of the Firm: Managerial Behavior, Agency Costs and Ownership Structure"). As a result of all this, the Guangxi boatmen and their hired whippers gained posthumous fame. However, this could be a story invented by my mother – the smartest person I have ever known – to entertain a boy of seven!

Oliver Williamson's famous work *Markets and Hierarchies* (1975) introduced the idea of "opportunism" and a raft of similarly unobservable terms. However, it is logically impossible to test hypotheses derived from non-observable entities against real-world observations. Williamson's work offers no testable hypothesis. The same criticism applies, of course, to my original idea of shirking: And though I admit that analysis of unobservable variables may be deemed to be economics, this kind of economics is not the empirical science which Marshall tried to teach and which underlies *Theory of Share Tenancy*.

7.7 The case of oil pipes and oil tankers

In 1978, Klein, Crawford and Alchian (1978) published a famous paper in *Journal of Law and Economics* ("Vertical Integration, Appropriable Rents, and the Competitive Contracting Behavior"). The idea of "holdup" proposed by the authors is also not observable, and hence cannot properly derive testable hypotheses. The article originally contained the following example: To avoid holdup, an oil company would not rent pipelines but it would rent tankers. I was consulting for the oil industry at that time, and so was able to produce facts which showed exactly the opposite – the common practice was to rent pipelines and own tankers. Klein, et al. merely deleted their example of pipelines and tankers, but that was all.

Shirking, threats, blackmail, holdup, opportunism *ad hoc genus omne* are interesting ideas which may produce examples. However, they are not observable and therefore cannot be used to derive testable hypotheses. For an axiomatic empirical science such approach is not useful. A similar remark can be made about many concepts in the currently dominant game theory: what is not observable cannot properly be used to frame testable hypotheses.

8 Economic prediction and explanation: the case of China

In the summer of 1979, Arthur Seldon of London's Institute of Economic Affairs consulted me over a question raised by Prime Minister Margaret Thatcher: Will China go capitalist? He said a 500-word reply would suffice. Intrigued by the question, I took Yeung Wai Hong with me to re-visit Guangzhou (and my sister's family) after 22 years.

I met a number of senior cadres during the three-day trip. I was very sensitive to economic phenomena. The general poverty observed was not surprising to me, but the hierarchical ranking of officials, with detailed consumption privilege assigned to each rank, caught my attention. My first interpretation of this is that, since people are born unequal, in a propertyless state human rights must be unequal to produce social equilibrium.

8.1 Predicting China with certainty

This is of course correct, but two years later I saw the deeper truth: the ranking of cadres is a way to reduce rent dissipation under the condition that the rights to income or wealth are not delineated. The conclusion followed immediately: Economic reform in China must take the route of transforming consumption rights determined by rank to a system where consumption rights are determined by wealth or delineated property. This is the key to economic reform, but how can that happen? I realized after two years that the cadre ranking I observed in Guangzhou was an arrangement to reduce rent dissipation under the condition that the rights to property are not delineated. The conclusion followed immediately: Economic reform in China must take the route of transforming consumption rights as determined by rank to a system where consumption rights are determined by delineated property rights. This is the key to economic reform, but how can that happen? I found the answer in 1983: economic reform in China can be implemented through a general application of the responsibility contracts used in agriculture. At that time I had already published *Will China go Capitalist? (Institute of Economic Affair 1982)* This slim volume of less than a hundred pages emphatically predicted in no uncertain terms that China would proceed on a path towards the market economy.

So it was that 500 words became a small book. The Institute of Economic Affairs was happy and wanted to publish immediately, but on my side there was trouble. Almost everybody who read my draft disagreed with my prediction.

The strongest criticisms came from T.W. Schultz and Gary Becker. Schultz, who had been awarded the Nobel Prize (in 1979), wrote to protest that economic theory is not capable of predicting economic reform. Schultz was famous, a good academic administrator, but he had not published anything showing the predictive power of economics. Becker (who was to win the Nobel Prize in 1992) was a different proposition altogether. Over the years Becker and I shared mutual admiration: I admired his analytical ability, and he praised my creativity. Strangely, we seldom were able to agree on the finer points of economics and methodology. For example, Becker thought my paper "Why are Better Seats Underpriced?" (*Economic Inquiry*1977) was wrong, because, according to him, everybody at Chicago thought it was wrong, while I thought that that little piece was beautiful and would withstand the ravages of time.

In predicting that "China will go capitalist" I had nailed my colors to the mast. If proved to be wrong, I would have to find a place to hide for a long time. However, I saw two clear changes in constraints: Going back would be impossible, and so I took the plunge. This came before I hit upon the idea of the responsibility contract as an effective tool to reform China.

8.2 Clearly observed changes in constraints

It is my firm belief that if changes in constraints are clearly identified and sufficient to describe the problem at hand, then the predictions of economic theory would be as powerful as predicting an apple from the tree will fall to the ground. In 1984, when I observed that state employees were rapidly turning to contract workers, I wrote that the economic reform in China has reached a point of no return. I was lambasted and pilloried for this, but I knew I was right. The institutional change in question required only a functioning market, but (as Fisher showed) it does not require observable interest rates and credit facilities. A contract worker can then discount expected future earnings to determine the present value of his human capital, and thus transforms into a small capitalist. So long as the labor market exists, a rate of interest is implied. To turn back on the path of economic reform would require the acquiescence of millions of such capitalists, which would be so costly as to be impossible. Economics is not a "soft" science at all: the only thing that is "soft" is the carless specification and identification of constraint change on the part of its practitioners.

As befits its status as an empirical science, economics can both explain and predict human behavior. *Will China go Capitalist?* was an *ex-ante* prediction. It was very difficult to identify and understand the crucial change in constraint. I succeeded mainly because only one special-interest group was involved—the ranking comrades—and because important constraint changes were evident. I was fortunate, not in guessing, but in observing changes in constraints that allowed me to exploit economic theory and predict what would happen. In addition to the intellectual satisfaction of a scientist, I was also rewarded by being part of an economic reform that ushered in a new era in world history.

8.3 A serious problem with the Coase Theorem

The reader is advised to read *Will China go Capitalist?* carefully. In particular, the book pointed out a serious error in the important Coase Theorem. According to Coase, if transaction costs are zero, or low enough, the market will emerge to perform. I, on the other hand, point out that if transaction costs are zero, there would be no market. This argument is something that Coase agreed is correct; so did Kenneth Arrow. Neither Coase nor I thought this little new idea was important, but over the years its importance is increasing. For without this simple insight, we cannot go on to ask: Why are there markets? It just cannot be that markets exist because of the costs of transaction. This question had me stymied for years and years, until I found the answer when I wrote in *The Economic System of China* in 2007.

Under what conditions would there be markets? This is the question. Observe that factor payments (and hence income distribution) can take place without markets. On the other hand, the functioning of markets involves a gamut of transaction or institutional costs – policing, legal, financial, and managerial costs to name a few. It is generally estimated that in advanced market economies, these costs account for more than 70 % of GNP. If transaction costs were zero, allocation and distribution under visible-hand direction would release a lot of resources for other uses.

The first hint of an answer came to me in the mid-1990s, when I proposed a more general definition of transaction costs. The costs that do not exist in a Crusoe-economy would all be transactional or institutional. A few years later, I saw that dissipation of rent is also something that would not exist in a Crusoe-economy. It followed that rent dissipation is a type of transaction cost. A few more years later, a beautiful idea emerged: Markets emerge through the introduction of legal, policing, etc. avenues which increase transaction costs, so as to support the use of the one and only criterion of competition under which rent is not dissipated: the market price. Markets exist because market price is the only criterion of competition that entails no rent dissipation. An important substitution theorem then follows: increase in market transaction costs to substitute the dissipation of rent. I call this the theorem of transaction costs substitution in my huge treatise entitled *Economic Explanation*.

8.4 From Cowperthwaite to S.M. Lin

In the summer of 1981 Yeung Wai Hong called to convey a message from Sir John Cowperthwaite (Hong Kong's former Financial Secretary) that I should apply for the soon-to-be vacant chair in economics at Hong Kong University. Just a few months earlier Coase was encouraging me to return to Hong Kong, to explain how markets work and help out in the economic reforms rumored to take place in China. He was persuasive, saying that I knew both Chinese and more institutional economics than anybody alive. On Coase's repeated urging, I decided to leave Seattle and return to the place where I was born.

That was in May 1982. In an academic conference four years before I met Mr S.M. Lin, owner of the *Hong Kong Economic Journal*. In its heyday under Lin, *HKEJ* was something special in the world of Hong Kong newspapers. Academic articles were favored, especially in economics, and it was a secondary concern whether readers understood what was said. Bright young economists like Alfred Hau and Yeung discussed in *HKEJ* anything and everything – Keynes, Hayek, Friedman, transaction costs, etc. etc. This approach was crucial to winning the hearts of Hong Kong's professional elite.

I was asked to write for Mr Lin soon after taking up the chair at HKU. But I had to wait until the fall of 1984, because I had not written in the Chinese language before. My column, twice a week in *HKEJ* beginning autumn, 1984 soon received a large audience. It was not long before people would say these as the founding articles of Chinese economic prose writing. I had fun, readers had fun, and so I continued. My output was pretty impressive: Three books explaining economics and its applications in non-technical language entitled *A Tangerine Seller Speaks* (1984), *The Future of China* (1985), and *On China Again* (1987). The last two volumes were supported by data materials provided to me by assistants from China, and Beijing reprinted the last two volumes (2000 copies each), bearing a stamp "For internal Reading only".

8.5 Two rewards from my deep concern about China

I am no reformer. But I vividly remember many of my boyhood friends starved to death during the war, after growing up and became a well-trained economist, I do care. Beijing apparently appreciated my intent, and they provided whatever research data I wished for. They took me to the mainland to whichever city or factory I wanted to investigate. Did I affect the course of events? Hard to say! All I hoped for was that my articles be read. In this the results have far exceeded my expectations.

I have been very fortunate in academic endeavor. Economic reform as spectacular as that of China is something that happens perhaps once in a thousand years. From the purely academic point of view, the two most important English-language articles about this epochal event came from my pen. And good they were! The first was *Will China go Capitalist?* (1982), and the second *The Economic System of China* (2008, 2016). The latter is a continuation of *Theory of Share Tenancy* in the context of China's economic reform. If I had not managed to do a good job with *Theory of Share Tenancy* 40 years ago, I would not have been able to produce *The Economic System of China*. To be able to witness a historic event and to write about it, is truly great blessing and justification of my life-long love affair with economics.

8.6 The view from Kunshan

In 1997 I visited Kunshan to find a factory site to continue the production of a polishing compound invented by my father. It was my mother's wish that this insignificant invention must not die out, so the profit-motive was not present. What I saw on the side was, on the other hand, very important. The competition between the different regions in China was of an intensity I had never seen before. This reminded me of the market behavior of firms selling the same product, and so the question immediately arose: Why? It was not until 2003, after a remark from a local official, that I found the answer: Land-use rights have become the property of the individual county, or xian. It was then open to the xian authorities to attract investment by blandishing the sale of such rights. Because property rights are clearly delineated, the xian can be understood to be transformed into a firm, so intensive competition would emerge.

The analysis is deep and difficult. I was fortunate in that under value-added taxation, the county's income would be determined as if there were sharecropping. Value-added tax must be paid whether or not profit is earned, so it is really a rent. I was able to draw on earlier research on the responsibility system to understand that competition among xians would act layer-by-layer in their economic activities. The remaining problem was to explain why the value-added tax rate, being the same across all xians, would produce efficient results. I solved this great puzzle in 2004 by remembering a footnote in Marshall's *Principles of Economics*, which I read as a graduate student.

8.7 Life may be short but heaven is merciful

I possess the scholar's persistence. I also possess a killer instinct, in that I would not stop until the problem is solved. Economics is an axiomatic empirical science, so one can focus both on *ex-post* explanation and *ex-ante* prediction. What is always required is lab-work—knowledge of the real world. Economists have only one reliable laboratory—the real world—but it is complicated and beyond our control. There is no choice: we have to enter the real world and observe. This is hard work, but it is something I have enjoyed doing for nearly 60 years.

Life is short: I am now 82, by the grace of heaven and the skill of doctors. Still, I decided to wait until 65 before beginning my *magnum opus*. It was only after decades of real-world observations that I felt ready to put pen to paper. The Chinese version of *Economic Explanation*(Cheung 2017) has taken 17 years to complete, and it has grown from 3 to 4 to 5 volumes and 1300 pages.

8.8 One man bravely doing proper economics

Economic Explanation proclaims economics to be an empirical science. It is therefore full of hypotheses testing *a la* my *Theory of Share Tenancy*. To do this I exploited data in the widest sense, from observations in the street to statistics to history ancient and modern. Analysis of transaction costs is also found everywhere in the book: With the methodologically proper combination of data and theory, interesting and empirically meaningful hypotheses are proposed and tested in everywhere. Compared to the Marshallian tradition, there are two major differences.

First, apart from quantity demanded, all non-observable entities are excluded. Quantity demanded is an entity created by the economists, not a fact, but one cannot introduce the law of demand without it. With apologies to Becker, utility does not appear in all of my work. With further apologies to Jeremy Bentham (1748–1832), I think that economics would be far better as a science had he not written *An Introduction to The Principles of Morals and Legislation*(Bentham 1789) to found the utilitarian school in philosophy. Economists playing around with utility functions are more often than not falling into the trap of tautological reasoning.

Second, to widen the scope for transaction costs to enter the analysis, I chose to simplify economic theory to three fundamental axioms: The law of demand, the concept of cost, and the implication of competition. The price of this simplification is that, when applying the axioms in all their power, the reasoning has had to twist and turn in deep and greatly varied ways.

To the day of his death (in 2013), Ronald Coase had been urging me to bring good economics in China up to speed. He was not happy with the way economics has developed, just like me when I arrived Seattle in 1969. It is unrealistic to expect one man to do the job, but events over the past two or three years suggest that Coase and I have not been fantasizing. Today, I feel there is a chance that economics would take to the skies in China. My reason is that individuals studying *Economic Explanation* are mainly businessmen or officials. I am convinced that they are doing so not for the sake of examinations or to be first-in-class, but because they believe that economics is useful.

Steven N.S. Cheung, Professor Emeritus of the University of Hong Kong, linda@lindasu.com. This memo, written in Chinese to commemorate the Shenzhen conference in 2017, is translated into English by Michael T. Cheung.

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