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1947–1982

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Stigler Center

for the Study of the Economy and the State

**The Department of Economics at the University of
Chicago, 1947–1982**

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The Department of Economics at the University of Chicago, 1947–1982

by

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Abstract

This paper is about the Department of Economics at the University of Chicago, between 1947 and 1982. The paper has the form of a conversation between the two authors and covers issues such as the existence of a “Chicago School,” the Department’s governance, the personalities of some well-known members such as Frank Knight, Milton Friedman, and Robert Mundell, teaching, and the “Chicago boys.” It also deals with the relation between members of the Department and those of other leading Schools.

Keywords

Chicago School, Milton Friedman, monetarism, Frederik Hayek

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1. Introduction

This paper is about the Department of Economics at the University of Chicago, as seen by one of its long-term faculty members – Arnold C. Harberger – and one of its former students, Sebastian Edwards. The period covered is from 1947 through 1982. That is, thirty-five years that span from the time Harberger arrived as a student to the time Edwards graduated with a PhD. A few years later, in 1984, the two co-authors were reunited, this time as colleagues, at UCLA. The approach that we have decided to follow is somewhat unusual: the paper has the form of a conversation between two people from different generations, two colleagues and friends, two professional economists who have travelled together around the world providing advice to governments on almost every continent.

Of course, we played very different roles in the Department. Harberger was a faculty member for over 30 years, and chairman for 12 years. He was a colleague of some of the most famous economists of the second half of the twentieth century and early decades of the twentieth-first, and hired many of them. After four years as a student, Edwards went to UCLA, where he has spent most of his career. He looked at Chicago from afar, but always with great interest, not only because of the links established during his period there, but also because of the role that some Chicago economists – the so-called “Chicago Boys” – played in Latin American economic reforms, including in his native Chile.²

We have labelled the approach “a conversation” for two reasons: First, we know each other too well for it to be a simple interview. Second, in 2016, Harberger finished taping an “Oral History” (Harberger 2016), which covers in significant detail most of his life, including episodes related to the Department. Of course, conversations can take many forms. In this one, we have proceeded as follows: Edwards suggests a topic, and Harberger talks about it; Edwards interrupts, and disagrees, and Harberger replies – sometimes he insists on his point of view, at other times he offers a variation on the subject. In selecting the themes for the conversation, Edwards often relies on previous exchanges and late night chats between the two of them that took place over many years in places as different as Jakarta, Managua, Buenos Aires, Moscow, Washington D.C. and Tegucigalpa.

² For the “Chicago Boys” and the Chilean market reforms, see Edwards and Edwards (1991) and Edwards (2010).

Thousands of pages have been written about the Department of Economics at Chicago.³ Some works have been celebratory and others have been critical; some have been deep, and others have been superficial. Because of this abundance of material, we have tried to focus on aspects of the Department that have not generally been highlighted in existing work. This means that some of the most often discussed issues in the existing literature will not be covered in detail (or will not be covered at all), while some lesser-known ones will be emphasised.

The various discussions that led to this article took place over several months in Los Angeles, California, during the time of COVID, with visits to each other's homes, and behind masks. Most of it, but not all, has been taped. However, even when the tape recorder was on, Edwards took copious notes, which completely filled a notebook. Sometimes, Harberger would visit Edwards, and vice versa. The conversation is not organised in a chronological way; it is structured around topics. In order to keep things on track, we have made an effort to clarify what happened when and who was involved in the different episodes being narrated. The chapter deals with ideas, professional debates, controversies, and people. In order to make it easier to read, we have resorted to a tried-and-tested technique: Harberger's words are in non-italics and Edwards' are in italics.

2. The Department in the 1940s and 1950s: The Fourth and Fifth Floors

Sebastian Edwards (SE): You enrolled in the Department of Economics in the fall of 1947, after having spent a year studying international relations at the University of Chicago. In this segment I would like to talk about two issues. First, the general atmosphere in the Department, as seen from the perspective of a graduate student. Second, I'd like to talk about the faculty in 1947. At the time, the Department had a large number of very prominent members, including some credited as having founded the "First Chicago School". People like Frank Knight, Lloyd Mints, Henry Simons, and others. Also, Milton Friedman had just arrived as a faculty member, in 1946. At the same time, some very well-known scholars had recently left, including Jacob Viner who went to Princeton, Paul Douglas who became Senator for Illinois, and Oskar Lange who went on to have a distinguished career in communist Poland. So, the topic I'd like you to address is simple: How was the Department when you joined? From today's perspective, what do you think were the most salient characteristics of the place? And, at the time, how was it different from other leading departments such as Harvard and Yale?

³ This includes memoirs by some of its leading members; see Friedman and Friedman (1998) and Stigler (2003).

Arnold C. Harberger (ACH): In 1947, there were lots of students, but it really did not feel crowded. It was very exciting and to us it was clear that it was a special place. The fact that the Cowles Commission was there added to the sense that this was a unique place. I was hired as a research assistant at Cowles in March 1949 and I shared an office with Stanley Reiter, who ended up having a very distinguished career at Northwestern.

SE: What kind of research were you doing at Cowles? And how did it fit with the general work being done at the Commission?

ACH: I was working on problems related to endogeneity in the estimation of supply and demand curves. And I was interested in the subject because of the controversy surrounding the elasticities of import and export, and the way in which they determined whether a currency devaluation would be effective or not. The question was determining exogeneity, and lots of people were interested in it. There was a difference in emphasis. For me, it was sufficient when working on an applied problem that the correlation was small enough, as to minimise the simultaneity bias. Tjalling Koopmans, on the other hand, insisted that there should be zero correlation; the variables had to be orthogonal. Koopmans at the time was either running the Commission or about to take over.

SE: Well, this seems to have been work related to the so-called “elasticities pessimism controversy”. In a sense, if elasticities were too small, then a very important element in the market system would not work properly, as devaluations or changes in the price of foreign currency would be ineffective. Did you see your work at the time as related, in any way, to Milton Friedman’s crusade for flexible exchange rates? Well, Friedman was not the only one at Chicago. Lloyd Mints had also written in favour of flexible rates.

ACH: Quite a few people at the time were working on similar problems. Lloyd Metzler was also interested in the subject of exchange rates and adjustment, including issues related to international transfers. But I would not say that there was explicit coordination among the different people. My work was in no way part of Milton’s crusade.

SE: So tell me more about your disagreements with Koopmans.

ACH: Koopmans was a very good economist, and a very good human being. I remember his modesty and his interest in looking at the world from different

perspectives. But we had occasional disagreements. At an American Economic Association (AEA) meeting I presented a paper and he was the discussant. He was very critical, and accused me of building a second story of a house that had no basement, or foundations. Paul Samuelson, who was chairing the session, came in my defence, and gave a 10 minutes speech, where he basically agreed with me.

SE: And which paper was this?

ACH: “The Measurement of Waste”, which was eventually published in the *American Economic Review* (Harberger 1964).

SE: When you were there, did you and your fellow students think that you were part of a distinctive school, the “First Chicago School”?

ACH: As I said, we realised that we were in a very special place. But I am not sure that we thought that this constituted a “School”.

SE: Some people have said that the Chicago School started with Frank Knight and from him different branches spurred out: Henry Simons, Milton Friedman, George Stigler. Some people say that what made it operational was Milton’s monetary analyses, plus Viner’s and Friedman’s price theory, and George Stigler’s and Ronald Coase’s views on externalities, regulatory capture, and transaction costs.

ACH: If one talks about the existence of a “Chicago School”, one also needs to talk about T.W. Schultz. He was incredibly influential, very talented, and a great economist. He and D. Gale Johnson ran an extremely prestigious agricultural economics programme at Chicago. When I think of the Chicago School, I think of three principles: theory is of utmost importance, and should guide economic thinking; theoretical constructs should be confronted with the real world, they should be tested, there should be a lot of data analysis; and when in doubt, always come back to fundamental price theory, to the functioning of markets.

SE: When you came to Chicago in 1947, Frank Knight was still a member of the faculty. Was he revered and admired by everyone?

ACH: Well, I would say yes. He was on the Committee on Social Thought. He was a member of both the Department and the Committee. And his office was adjacent to the Committee, on the fifth floor, and not adjacent to the bulk of the Department on the fourth floor. So, we didn’t see that much of him in the halls and corridors.

SE: Did you take classes from Frank Knight?

ACH: I took every class from Frank Knight. I have a joke about him. He had a house on Kimbark Street, across the Midway, and this house had an attic. And I pictured that his attic contained a huge barrel, and in this barrel were written all kind of aphorisms – one liners so to speak – and every time he had to make a speech he would go up there and he would crank the barrel and get one draw, and another draw, and so on. So, after a while there was nothing you ever heard from Frank Knight that you hadn't heard before, but you hadn't heard it quite the same way.

SE: Did Frank Knight teach Price Theory, the famous 301 course, the course that at one point or another was taught by Viner, Milton, Gary Becker, Sherwin Rosen, and yourself among others?

ACH: I don't think so.

SE: Which courses did you take from him?

ACH: I don't remember the numbers, but a history of thought course, maybe more than one.

SE: Who else besides Frank Knight represented the old generation when you arrived in Chicago as a student?

ACH: Well, Lloyd Mints. Lloyd Mints is very underappreciated. I went back to his notes, and he taught monetary economics from the fact that prices of international goods, as he called them, were determined in international markets. And the doings in this country didn't have to do with that. They had to do with the exchange rate, and the balance of payments and things like that. He was far more subtle, far better than what he was given credit for.

SE: I think that Mints is making somewhat of a comeback in the history of thought community.

ACH: That's well deserved in my opinion. And to me, he got undervalued because he didn't publish. He published on banking theory, which I believe was his dissertation. And beyond that, it's hard to find pieces by Mints. And, it's a shame in a way.

SE: Who else comes to mind from those years, besides Knight and Mints?

ACH: John U. Nef. He was on the fifth floor as well, as a member of the Committee on Social Thought. And he belonged there, in the sense that he viewed economics from a broader perspective than the rest of the Department. The rest of the Department were “tools oriented”, supply, demand, and the forces of inflation, and that kind of thing. And Nef was just *sui generis*. He would come and read his lecture to you. And these lectures were all organised, and written out, and so on. I have this imitation of him. From time to time he would pause and say: ‘As Gustav Schmoller, the very famous German economic historian from the nineteenth century once said to his classes in Berlin, “*Es ist alles so furchtbar kompliziert!*”’, which translated means “It is all so terribly complicated!”’ And that is a quote from John U. Nef. Verbatim!

SE: Did you take a class from Henry Simons?

ACH: No, I did not. And that is an interesting story. In the spring of 1946 I still had not transferred officially from international relations to economics, but I was already taking quite a few classes in the Department. I tried to enroll in Simons’ course. But during the first meeting he said that he wanted to restrict the course to those students who were already writing a dissertation or were about to begin one. I was not in that group yet, so I could not take his class. And, at the end of the quarter he committed suicide.

SE: Let me go back to the fifth floor of the Social Sciences building. Your last year as a student was Friedrich Hayek’s first year at Chicago. And he was a member of the Committee on Social Thought, so he was up there on a different floor, with Knight, Nef and others. And when you came back as a member of the faculty in 1953, he was still there. Hayek left Chicago in 1964, so you were colleagues for about ten years. Did you interact with Hayek?

ACH: No.

SE: Not at all? He didn’t come to the workshops?

ACH: No. He had his own group, and there was almost no interaction between him and the members of the Department, as far as I remember. He never came to the regular workshops.

SE: You didn’t talk to him?

ACH: Not really.

3. The Workshops System

SE: Tell me how you ended up joining the faculty at Chicago? How did it happen? You were an Assistant Professor at Hopkins and you got an offer to go to Chicago. Tell me the details of that story. As I remember it, it is in some ways related to the famous Chicago workshops' system.

ACH: T.W. Schultz read my projections on the demand for materials for the year 1975, and decided that I was the man for public finance at Chicago. In that work, I created an image of the 1975 economy, with so many automobiles, and so many washing machines, and so many refrigerators, and so much this, and so much that. And from that I derived the demand for materials. This was very distinct from running a regression and forecasting forward. You can see how terrible job a regression would do. I don't think it's the right way, anyway. The reason had to do with history: going back you had the 1920s and the Great Depression and the war, and then inflation. There was no point in using regressions.

SE: Had you taken a formal public finance course at Chicago as a student?

Yes. That was the only B I got in economics. The professor was Roy Blough, who had been a Treasury official, and the Teaching Assistant was Jim Buchanan. In a way, it was funny that I would come back to Chicago and be in charge of the field where I got my only B.

SE: The public finance workshop, which you ran, was very famous in the 1950s and 1960s. It attracted lots of students, including people like Bob Lucas and many Latin American students. So, tell me, when did the workshops start, and what was so special about them?

ACH: Well, the workshops started with Milton. And agricultural economics, with T.W. Schultz and D. Gale Johnson; that was effectively a workshop, whether you call it that or not. And that was alive and well, and strong and powerful. And when I came to Chicago in 53, Schultz practically forced me to have a workshop in public finance. And this was because he and Milton were looking for funding from the Rockefeller Foundation for workshops in general. The idea was that workshops were good, that this was a start. Here was a new faculty member, and why not get him started on the right track. So, I was hardly given an option.

SE: So, tell me in what way was the public finance workshop different?

ACH: It was different from Milton's in the sense that I gave much more freedom to people, and so I had very good students, you know. Gregory Chow, Dick Muth, Bob Lucas, among others.

SE: Someone told me that most of the Latin American students arrived about 20 minutes late to the workshop. And that that became a regular feature of it. Is that true?

ACH: Yes, that's true. Always a little late.

SE: Was Finis Welch, the labor economist, your student?

ACH: No. He was a card playing buddy. We played bridge.

4. Fortress Chicago

SE: When I arrived as a graduate student, in 1977, my classmates and I had no doubt that we were in a special place, in a unique institution. We also knew that we were in a place that was populated by people with very strong points of view regarding the functioning of markets, the role of monetary policy, the causes of inflation, and the limitations of fine tuning. We also had the feeling that intellectually we were besieged, that the "Chicago view" was a minority view. We did not dislike this. In fact, I think that many of my classmates liked it; we considered ourselves as being in what I would call "Fortress Chicago." In many ways, we thought that it was a fortress built around Milton Friedman and his views. What are your thoughts about this? How did you feel as a member of the faculty, and earlier, as a graduate student, about the notion that Chicago was almost on its own defending certain principles?

ACH: I resist very much the idea that Chicago was basically a sounding board for Friedman. In point of fact, we had as many people voting Democratic as voting Republican. The thing is that the other leading department had mostly Democrats. It's not that we were predominantly Republican; we had some, and they didn't have any (or had very few), so to speak. The question is: What determines the Chicago School? My belief was that the Chicago School meant believing that market forces were extremely important in determining how things worked out in the real world, and there was no one at Chicago who disagreed with that principle.

SE: Let me push you on this: Our sense in the late 1970s – this is just when the “rational expectations” revolution was taking hold – was that there was only a handful of schools with majority views similar to those dominant at Chicago: UCLA, Rochester, maybe Carnegie. At the policy level, there was the St Louis FED monetary model, but not a lot more. Given this situation, how would you characterise relations between faculty members in the top schools?

ACH: In Cambridge and particularly at MIT, and within MIT, Paul Samuelson and Bob Solow, made snide remarks about Chicago, and especially about Milton Friedman’s attitude and interests, and I would say there was a kind of belief that we were all kind of clones of Friedman. This, of course, was not true, and certainly not true of Ted Schultz, Harry Johnson, D. Gale Johnson, Bob Mundell. There makes no sense of talking about clones, knowing the personalities of the people involved. But the outside view was that most of us were, at least partially, Miltonian clones. I would say that Milton didn’t want to have only people like him in the Department. In the appointment process he liked diversity.

SE: Was Milton Friedman liked by his colleagues? Did the other faculty members like him?

ACH: I have the feeling that the answer is ‘Yes, but...’ Certainly, Harry Johnson had a hard time with Milton. I don’t know if he wanted to have as much lustre around him as came around Milton; it might be that. Harry’s economics were close to Milton, but not by any means as doctrinaire, so to speak, as Milton’s.

5. Partial vs General Equilibrium

SE: One of the common criticisms of Chicago during the 1950s and 1960s was that the type of economics practised by Milton Friedman and George Stigler, just to mention two people, was nothing more than glorified partial equilibrium analysis based on Marshall. This criticism came, mostly, from people like Samuelson, Tobin and Solow. What is your reaction to this idea?

ACH: Well, I always had a very good relationship with Samuelson and a pretty good relation with Tobin. And general equilibrium was present in most of my work.

SE: Yes, your “Incidence of the Corporation Income Tax” (Harberger 1962) and your “Three Numeraires” (Harberger 1974) papers are great examples of general equilibrium.

ACH: I think that in my own case, my grounding on trade was serious, and trade leads you to global general equilibrium analysis of a manageable sort, rather than of an n-equations and n-unknowns sort.

SE: Let me stay with Samuelson. What was the origin of your good relations with him?

ACH: As I noted, it originated with my paper “The Measurement of Waste”, in an AEA meeting in the 1950s. Koopmans was the discussant and he berated me for having built on the second story without having a strong first story. And Samuelson, who was the chairman at the session, took ten minutes to put Koopmans down.

SE: Let me try to dig some more on Samuelson, Chicago and you. There is a long passage in Samuelson’s Foundations (1948) where he criticises the concept of “consumer surplus”, which is very central to a lot of your work. It also plays important roles in work by Friedman, Stigler, Johnson. Another concept that has been criticised due to a “lack of general equilibrium properties” is effective rates of protection. This concept is also associated with Chicago, through Johnson’s work on protection and tariffs. I personally find “effective protection” to be a very useful concept.

ACH: Yes. And Samuelson in some ways impeded the advancement of “consumer surplus economics” for twenty or thirty years because of what there was in *Foundations*. But consumer surplus is, and always has been, a valid concept. So is effective protection. They are particularly useful when we talk about intermediate goods and inputs, and the cost of the protective structure.

6. Governance of the Department

SE: The impression one gets looking back is that during those years [1960s and 1970s] the Department was very small, and very well run. Tell me a little bit about the governance, the way in which the faculty interacted with each other, the role played by junior faculty, and relations with other parts of the University.

ACH: I think that the fact that there were very few chairmen is important. From 1945 to the mid-1980s there were only four chairmen. Albert Rees ran the Department for three years. The rest of the time the chairmen were Ted Schultz, D. Gale Johnson, and myself.

SE: Overall, you were chairman for 12 years, probably one of the longest, if not the

longest, maybe only comparable to James Laughlin. How were relations among the faculty? Was it hierarchical? Or was it pretty much horizontal?

ACH: Governance was much more democratic than at Harvard or Yale, mostly because of the small numbers of faculty. Never more than 25, while Harvard and Yale probably had 45 or so. Junior members spoke up frequently and freely.

SE: Any regrets about letting people go? Someone you should have kept?

ACH: Marc Nerlove in a sense. He went to Minnesota, then Rochester, then Maryland, if I remember correctly.

SE: What was your biggest conflict as chairman?

ACH: What I remember is not a conflict, but an embarrassing situation, related to an appointment in public finance. I wanted to bring a very distinguished member of the field, and had spoken to him about joining the Department. He looked at the possibility in a very positive way. But when we took the vote, I was surprised to find out that there was a large group who opposed him, mostly junior faculty. There were enough “no” votes, so I thought maybe we should take a second vote. But the second time it came in even more negative. It was very embarrassing, but to my delight my friendship with this scholar remained intact.

SE: You once told me about the conflict that emerged between Hans Theil and Arnold Zellner. If I remember correctly, this story also involved the Business School, whose Dean at the time was George Shultz.

ACH: Yes, the Theil-Zellner conflict was deep and serious. The curious thing is that we got Arnold Zellner, mostly because Hans Theil wanted him to join the faculty. So here comes Zellner, Theil’s favorite, and once he arrives he begins to behave like an equal. Theil, who came from the highly hierarchical European tradition, just couldn’t take that. And in the end we had this terrible period where the econometric prelim once a year would contain Theil, and once a year would contain Zellner, but never contained both Theil and Zellner. Not even George Shultz, with his enormous negotiating skills, could solve the impasse. Theil ultimately quit Chicago and went to Florida, where he was probably treated like the professor that he expected to be treated.

SE: Going back to the Department chairmanship. Did you have a problem with Harry Johnson splitting his time between the LSE and Chicago, and later between Geneva and

Chicago? It's been rumored that there was a problem later on with other faculty who wanted to split their time between Chicago and another department. So, did you have any misgivings or conflict with Harry?

ACH: Harry Johnson was a very prominent and important figure. We were very happy to have half of Harry's time. But we did not want that option to be spread; we did not want that to become a common practice. So, generally, we didn't allow this from other faculty.

SE: Did you make an effort to retain Bob Mundell when he said he was going to Canada?

ACH: I feel that that was, in a sense, a no-brainer. Bob was of the idea that he was going to set up in Canada a center for international studies, where he would draw people from all over the world. And this was a dream in his mind. I don't remember what kind of counter-offer we made, or even if we did make one. Because he was dead set on having this experiment. And when it didn't work out he went to Columbia.

7. Journal of Political Economy (JPE)

SE: What was the JPE's role in the life of the Department, if any? The JPE had a life of its own, I assume.

ACH: The editorial team always included members of the Department, as well as members from the Business School. But we didn't have Department meetings about the JPE.

SE: Were you ever attracted to the JPE? Did you want to become an editor? There were some members of the faculty – I can think of Harry Johnson, Bob Mundell, Jacob Frenkel – that loved that position. I know that you were never an editor. Did you ever consider it?

ACH: Never! I would much rather be chairman than editor of the JPE.

SE: Why? Many people would find that an odd answer. Running the JPE is an incredibly influential position.

ACH: Well, it doesn't go with my personality. Very early on I realized that refereeing was extremely time intensive for me. It took me many, many hours to go through a

paper. Thus, editing the journal was not the optimal allocation of my time.

8. People

SE: To me, an interesting thing about Chicago is that so many faculty members were, or became, household names, the greatest example, of course, being Milton Friedman. But there were also many great scholars that were completely unknown to the general public, even to policy makers. So, let me read you some names of people that were in the faculty when I was a student, or earlier, and give me your impression about them. Let's start with Lester Telser.

ACH: At some point there was an "attack" by Hopkins on Chicago, and offers went out to Carl Christ, Lester Telser, and myself. And the end result was that Christ went to Hopkins, I stayed on, and Telser, I believe, shifted from the Business School to the Department. That is my recollection. I believe that this was in the mid-1960s.

SE: Tell me about Arcadius Kahn. (That was the only B I got as a graduate student at Chicago!)

ACH: Arcadius was a wonderful person, a very careful economic historian. His book on the economic history of Russia is a classic. He spent time in a concentration camp. He was very close to D. Gale Johnson, and D. Gale respected him greatly.

SE: Let's talk about Margaret Reid. I didn't get to meet her during my time at Chicago. Of course, I saw her all the time, always carrying a pile of books with statistical data (or so it seemed to me).

ACH: She was a regular and valued member of the Department. She taught consumer economics. She was a key person with T.W. Schultz in Iowa, and he, quite wisely, brought her and D. Gale with him when he moved to Chicago. And Milton gave her a certain amount of credit in his "permanent income hypothesis". For a number of years, she was a key person at Chicago, and she carried her own weight, so to speak.

SE: What about George Tolley.

ACH: George was a good economist, a more than an occasional wheel in the agricultural side. He complemented T.W. Schultz and D. Gale Johnson. And he carved out for himself the field of urban economics; he had a very respectable reputation among people in that field. He had a quiet personality.

SE: Let's now close this section with Larry Sjaastad. And, before you say anything, I want to make a relevant point: Larry was in 134 dissertation committees. That is an amazing number. He didn't publish much, but just being in all those committees is a major contribution. In terms of economics, and in particular on the merits of fixed exchange rates in a disinflation process, I had great disagreements with him. But I am still amazed by his role on all of those committees.

ACH: Yes, Larry's advice was sought by many thesis writers. He was dependable and helpful. On his exchange rates views he was influenced by Mundell and later by Harry Johnson. I always thought that the preferred exchange rate regime depended on the nature of the country's exports, or sources of foreign exchange. In Panama it's the Canal, and it's a very stable flow. So having a super peg rate makes sense. In Chile, with copper being the main export, it doesn't make much sense to have a rigid exchange rate.

9. Teaching

SE: Let's talk about teaching now. When I arrived as a student in 1977, the entering class was about 60. And the graduating class, those who got a PhD, was 12 or 15. The 60 entering class was a very unusual number for a top programme. The entering class at MIT, Harvard or Yale was about 15, almost the same number as those who graduated at Chicago?. So let me start with the following question: How large was the entering class when you arrived in Chicago as a student?

ACH: We had about 200 graduate students in economics when I was there because we had all the people from the war. People from 41, 42, 43, 44 and 45, and we were all being financed by the GI Bill of Rights. So, we got our \$75 a month, and if you got a fellowship – it didn't matter which one – you only got another \$25. So \$75 from the government plus \$25 from the University, \$100. And that was enough at the time, before too much inflation.

SE: And when you came back as faculty in 1953, how large was the entering class?

ACH: About 60, I am guessing.

SE: So the tradition of having a very large entering class – as the one I had – went way back?

ACH: Right.

SE: You once told me that at Chicago there was the view that a Master's degree was an earned degree, a useful degree for professional economists that would do good work in government. It was a genuine degree, it was not a consolation prize for those who didn't pass the core exam. That was different from other top programmes.

ACH: Well, we made a bit of fun of Harvard and Yale because they practically guaranteed that every entrant would get a PhD. And we thought that that was for the birds! On the other hand, I think that two-thirds of the people who entered Chicago had the capacity to get the PhD, and two-thirds of them actually got the PhD.

SE: Another characteristic of the teaching programme that made it special was that during the first year every core course was taught twice, by different faculty. For instance, I took the Price Theory courses (301 and 302) twice: From Gary Becker (301 and 302), and from you (301) and Sherwin Rosen (302). Same with macro. I took 331 and 332 from Bob Lucas, and a different version of 331 from Don Patinkin, who was visiting from the Hebrew University at the time, and 332 from Jacob Frenkel, who taught a very thorough open economy version of macro, tailored after Mundell's course.

ACH: And, did you find it worthwhile taking the same course twice?

SE: Totally. I benefitted from different perspectives, different emphases, different points of view, different reading lists, different personalities. Was this architecture, so to speak, of two versions of the core courses offered every year, done on purpose? And if so, what was the reason?

ACH: I think that the idea of there being two came about because of the glut that followed the Second World War. There would not have been two 301s and two 302s if it hadn't been for this glut. And when the entrance of 100 students tapered off, we chose to have an entering class of 60, and that also justified teaching two of each core course per year.

10. The "Chicago Boys"

SE: Now that we are talking about students, maybe this is the time to talk about the "Chicago Boys" A lot has been written, and there was a documentary. So, maybe we should cover aspects of the Chicago Boys that are not that well known. So, let me start

with some background: the “Friedman and Pinochet” controversy was huge, and you were affected by it. There was that “Inquiry Commission” by Chicago students – mostly undergraduates, I think – that made a lot of noise, and also targeted you. During that discussion, Friedman repeatedly said that he didn’t know much about Chile, and that although he did meet for about an hour with Pinochet, he never advised the government. He added that his personal relations with Chilean students were almost nonexistent; he sat in only one dissertation committee for a student from Chile. He later got to know some of the Chicago Boys, but he never developed a close relationship. On the other hand, some of the faculty embraced the Latin Americans and became quite close with them. Gregg Lewis, Larry Sjaastad, Harry Johnson, Bob Mundell and, of course, yourself. So, here is my question: Was there ever any pushback by the faculty about having so many Latin American students?

ACH: I don’t think so because they were so good. I never found any objections to our policy of admissions of Latinos and I was, in fact, very proud of the fact that we really knew which were the good places, and who were the good teachers that recommended students. To mention some: Tucumán, two or three places in Buenos Aires, in Santiago – both the Catholic and the national universities – and ITAM in Mexico. We were very satisfied with the quality of the students that we got.

SE: Many of your Latin students ended up holding positions of great responsibility in Latin America. There has been at least one president – Nicolás Ardito Barletta in Panama – plus many cabinet members and central bank governors. I remember once when Argentina was about to name a new central bank head. The outgoing person was a student of yours (Pedro Pou) and the two candidates were also former students, “Chicago Boys” (Roque Fernandez and Mario Blejer). How do you feel about this whole thing?

ACH: Well, of course, it is a source of great pride and satisfaction. But, in addition to the big names, a large number of our students worked as professional economists, making sure that good policies were put in place. In that regard, the whole area of project evaluation was very important. And not only in Latin America, but also in places like Canada.

SE: Did the other members of the faculty value the fact that many of the Latin students went into government, instead of academia?

ACH: I think so.

SE: When I was a student at Chicago in the late 1970s and early 1980s there were two groups of international students that dominated the foreign group: Latinos and Israelis.

ACH: Yes. But the Israeli group was much smaller.

SE: True, but the point I want to make is that there was an overlap there. There were quite a few Argentine and Uruguayans that had done their undergraduate work in Israel, either at Hebrew or at Tel Aviv. People like Mario Blejer, Leo Leiderman, Pablo Spiller. So tell me a bit about the Israel connection, in general. As I said, I took my first macro course from Don Patinkin, who was visiting. At that time, many Israeli professors spent some time in the US where salaries were much higher. As the chairman, did you look at Israel to attract visitors?

ACH: I think that the situation was very individual. Certainly, we didn't make a list of Israeli professors who were about to take sabbatical, and discuss which ones we should try to get. It came in different ways. Patinkin wanted to come to Chicago, and we were very happy to have him, so we got him. Same thing with Yair Mundlak, and others.

11. The Advantage of the Chicago Department

SE: Maybe we should end with a peroration of sorts, with a summary of what you see as the great advantage of the Chicago Department during the years we have been talking about. How would you summarise this advantage?

ACH: A clear advantage was that, among the leading departments, it was smaller; it was only half, or less, than Harvard and Yale. Hence, just about every professor at Chicago was not a supernumerary, everyone was important in his or her own field. The range that we had, from Milton, Harry Johnson, Bob Mundell, T.W. Schultz, John U. Nef, Jim Heckman. Everybody was a real leading person. Because of the size, the number of people responsible for a field was small, and that allowed us, for instance, to have Harry Johnson and Mundell in international, at the same time. So, it was a great Department. Now, our small size had to do with the fact that the University of Chicago didn't have the great mass of undergraduates that other schools had. At the end, I think that we go back to my three points discussed above. The Chicago School was about the connection between theory and applied analysis. Always test the implications of the theory, confront data with predictions, and do it again and again. Take both theory and data seriously.

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