

The Training of an Economist *

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The high-school that I attended in my native city Chicago was among the more progressive ones, so my graduating class of 1939 was already able to benefit from the then-novel vocational aptitude tests. My results showed a high aptitude for mathematics. But we were still living in the shadow of the Great Depression and everyone knew that mathematicians went hungry. So the advice to me was to become a statistician – with the explanation that a statistician was a mathematician who could make a living.

And it was very important for me to know my future vocation: for I was applying for a scholarship to the Central YMCA College in downtown Chicago (which subsequently evolved into Roosevelt University) – and who would grant a scholarship to someone who did not even know what he wanted to be? But when at the subsequent interview at the College I proclaimed my intention of becoming a statistician, I was told, “Oh, you mean an economist”. And that is how I became an economist.

There was, of course, a milieu during my high-school years (1935-39) conducive to such a choice. On the one hand, the sciences in the 1930s did not have the fascination and excitement of today.

□ Professor Patinkin's article was completed shortly before his death on 7 August 1995.

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This paper reproduces in part material from an introductory chapter of “Reminiscences of Chicago: 1941-47” that was published in my *Essays On and In the Chicago Tradition* (1981). A much extended version of this material was published in Hebrew as the second part of an article entitled “From Chicago to Jerusalem” (1994). This paper is a further extension of the Hebrew version. I am indebted to the Duke University Press and to Am Oved Publishers, respectively, for permission to reproduce the aforementioned materials.

The biological sciences were largely taxonomic and the physical sciences had not yet entered the atomic era that mushroomed with the bomb. On the other hand, the economic and social problems of the depression were part of our everyday experience. My father was in partnership with my uncle in a small business, which succumbed to serious financial difficulties. Fathers of close friends of mine had been unemployed for long periods, if not years. When we went downtown, we would see World War I veterans sitting at street corners behind up-ended empty fruit boxes in the bitter cold of Chicago winters, selling apples and pencils.

In addition, my high school was one with a high degree of political consciousness. Among my fellow students as well as teachers were sympathizers, if not members, of socialist or communist parties – and if I myself was not attracted to them, it was because I was at the time an Orthodox Jew, and so unable to accept the anti-religious views of these parties. We were emotionally involved with the struggles of the Spanish Civil War – with our sympathies being (of course) with the Republican side. And we agonized over the growing power of Hitler and the shame of Munich. Traditionally, the graduating class gave a public performance of a play which it had chosen: the one my class chose (at, I suspect, the suggestion of one of our teachers) was Karel Čapek's 1923 play, *R.U.R.*, the story of "Rossum's Universal Robots" and their revolution against man, their exploiter. (Only recently did I learn that Čapek was not a communist sympathizer: far from it, he was an ardent supporter of Thomas Masaryk's democratic leadership of the ill-fated Czechoslovakian republic of the interwar period.)

I took only two courses in economics during the two years I spent at Central YMCA, and both were poor. On the other hand, I benefited greatly from excellent teachers and courses in mathematics, philosophy, English composition, and art appreciation. From the viewpoint of the last of these courses, the downtown location of the College was particularly beneficial, for it was within walking distance of the Art Institute of Chicago with its wonderful collection of renaissance and impressionist paintings on which we were frequently required to write reports for class discussion.

In my third year (1941) I transferred (as I had from the beginning hoped to do) to the University of Chicago. Why had I not begun my studies at this world-famous university in my own city? Though my parents probably could have paid the tuition fees involved, I did

not want to impose this burden upon them and so wanted to get a scholarship to cover these fees. Now, scholarships for entering students at the University of Chicago were then granted only on the basis of a multiple-choice competitive examination that was always given on a Saturday. And as an Orthodox Jew it never entered my mind (or the mind of anyone else in the community in which I lived) that one would violate the Sabbath on that account. Nor in those days, before minority rights became the concern that they are today in the United States, was the University willing (despite repeated pleas to it) to make special arrangements to enable Orthodox Jews to take the examination. (One such proposal was to keep such applicants under supervision – and at their expense – in a hotel near the University during the Saturday of the examination, and to let them take it as soon as the Sabbath was over.) In contrast, scholarships for entering third-year students were granted by the University of Chicago on the basis of their grades in the institution in which they had spent their first two years, and I successfully applied for one.

I should however add that I do not think that I lost much by not beginning my studies at Chicago, for as just indicated, I benefited from many excellent courses at the Central YMCA. I should also add that in any event one could not specialize in a specific subject at the University of Chicago until the third year of undergraduate studies, at which time one entered one of the Departments in the Divisions. The first two years at the University were devoted to the College, with its famous four survey courses in the humanities and in the biological, physical, and social sciences, respectively.

So I entered the Department of Economics of the University of Chicago in the fall of 1941 and (with an interruption during most of 1943) continued my studies there until receiving my Ph.D. degree in the summer of 1947. These, as it turned out, were the final years of the traditional "Chicago School" – that School which, with due regard to the differences among them, can be identified with the teachings of Frank H. Knight, Jacob Viner, Henry C. Simons, and Lloyd W. Mints. For by the academic year 1946-47 – by which time my formal course work had been completed – Viner had moved to Princeton and Simons had died. And though in the same year Milton Friedman returned to Chicago (where he had taken his M.A. degree in 1933) to continue with the school's fundamental ideological advocacy of free-market economic liberalism, in other important respects

Friedman's teachings differed significantly from those of the traditional Chicago School.¹

Simons taught the undergraduate course in price theory, and Mints the one in money. Simons' course introduced us to partial-equilibrium analysis by means of book 5 of Marshall's *Principles*, and to general-equilibrium analysis by means of chapter 4 of Gustav Cassel's *Theory of Social Economy* (1932) – which reproduced Walras' system without mentioning his name! But our main text was Simons' own famous mimeographed *Syllabus* for the course. This began with a brief summary of Frank Knight's *Economic Organization* (1933) – to which we were also referred – which created the new and exciting vision of the market economy as an efficient system of allocating resources. (The term "invisible hand" was to appear only later, in Frank Knight's classes themselves.) But the distinctive feature of the *Syllabus* were the challenging numerical problems in the theory of demand and supply under both perfect and imperfect competition which we students had to solve, usually after intensive discussions amongst ourselves. (On one of the most challenging of these problems, my "Multiple-plant firms, cartels, and imperfect competition", 1947, was based.) From this *Syllabus* I learned much more than the subject matter itself: I learned the hard way that passive learning – just reading the material and listening to the lectures – was not enough; that full understanding of the principles of economic analysis could be achieved only after actively sweating through their application to specific problems, with pencil and paper in hand. I learned it then – and have applied it ever since in my own teaching, which has been based on the submission by students of weekly written exercises which are then graded and returned to them with comments.

Simons also gave an undergraduate course in public finance, in which context I read his famous pamphlet on *A Positive Program for Laissez Faire* (1934). I still remember my aesthetic enjoyment of its clean and incisive style (Mozart, not Beethoven – he once told us – was the music for him), and my intellectual enjoyment of its trenchant argument. What was particularly exciting were the same qualities that made Marxism so appealing to many other young people at the time: simplicity together with apparent logical completeness; idealism combined with radicalism. For Simons carried out his approach to its

¹ For details of these differences, see Patinkin (1969 and 1979).

logical extreme, with the unshaken and unshakable conviction of a world reformer that life would be better if only his policy recommendations were carried out. Market competition was to be assured by opposing with equal vigor all forms of monopoly, business as well as labor unions. The instability of the banking system was to be solved by requiring 100 percent reserves; and, of course, mass unemployment – with the waste and sufferings that it represented – was to be prevented by a contracyclical policy of varying the quantity of money so as to stabilize the price level.

Furthermore, a high degree of equality was to be achieved by a progressive income tax, applied to receipts of any kind – not only (as he had emphasized in his 1938 monograph *Personal Income Taxation*) ordinary income, but capital gains, inheritances, and gifts as well (on which type of income the tax would in some cases be levied on a five-year moving average, to take care of the inequity that would otherwise be generated by the progressivity of the tax – *ibid.*, p. 154).

Simons' fundamental belief in the principle of progressivity led him to be as restrictive in the tax deductions that he was willing to allow as he was encompassing in his notion of taxable income. Irving Fisher's argument that only consumption was income might be analytically correct, but it need not bind the legislator, who is free to define income for tax purposes as he wishes. And in one of his classroom *obiter dicta*, he opined that in computing taxable income, payment of alimony should not be deductible, for "when a man moves into a higher income bracket, he develops a demand for a higher-class wife; so payment of alimony is just another consumption item". (This misogynist attitude – he was then in his early 40s and still a bachelor, a situation that changed with his marriage a year or two later – also led him to observe that "American wives are billboards on which their husbands advertise their financial success".) And he derided the hypocrisy of the so-called sumptuary taxes: if, because of concern with the health of its citizens, the government decides to curtail the consumption of certain goods, then it should levy a prohibitive excise tax on them, which would accordingly yield very little revenue. This, however, was clearly not the case for the "sumptuary taxes" on tobacco and liquor, which were actually designed to yield significant revenues in a highly regressive way. In the same spirit he cynically observed that "luxuries are goods that some people think others should not consume".

In contrast with the exciting ideas so forcefully and systematically set out in his monograph and pamphlet, Simons' classroom presentations were somewhat haphazard. In my mental image of him he is slouched in one chair, with a knee drawn up over the arm of another, drawling on in a dry, laconic, half-cynical way. But despite his lethargic presentations, Simons' intellectual impact was such that we all left his classroom "simonized" to some extent or other.

Lloyd Mints' undergraduate course in money and banking was quite different: he lacked Simons' spark, nor did he aspire to it; indeed, he regarded Simons as his mentor. At the same time Mints' classroom presentations were clear, systematic, and very effective. We started with the quantity theory, and used Irving Fisher's equally clear and systematic *Purchasing Power of Money* (1913) as our textbook. And on the banking system we read another classic - C.A. Phillips' *Bank Credit* (1920). Led on by Mints, we shared that exciting moment of realizing that banks were not simply passive recipients of money deposits, but actually created these deposits.

Another one of my undergraduate teachers was Paul H. Douglas, who, though he more than shared the activist macroeconomic policy-view of the Chicago School, did not share its free-market philosophy. By the time I took his course in labor economics (winter 1942), Douglas had already begun to shift his interests from the academic world to the political one. He had been elected city alderman of Chicago in 1939, and ran for the democratic nomination for U.S. Senator in 1942. The United States had just entered the war, and after failing in his bid for the Senate, he volunteered (at the age of fifty!) for active duty in the Marine Corps. Indeed, the course I took was the last one Douglas gave before joining the Marines.

For Douglas, more than for any of the other teachers in the department (with the possible exception of its chairman, Simeon E. Leland, with his red cheeks and booming voice), the classroom meeting was a dramatic encounter, and the classroom itself a stage on which to pace back and forth, declaiming more than lecturing, and pausing on occasion to fend off premature student questions by slowly intoning, with a wave of his hand, and in what was (as I learned only recently) a variation on a Protestant hymn, "Lead kindly light, one step at a time...". Whenever we passed his open office door, we could see the three-dimensional representation (by means of a "surface" marked out by a "continuum" of small balls at the end of long, thin rods of varying heights) of the famous Cobb-Douglas

production functions that Douglas (together with Cobb) had estimated empirically in their classic work *The Theory of Wages* (1934, p. 216). But because of his impending departure from academic life, he was no longer very active in research.

In a place by himself in the department was John U. Nef, who in his somewhat effete manner gave the undergraduate course in European economic history. Nef, the prototype of the soft spoken, cultured European scholar-gentleman, opened a window for us on the life, language, and culture of France in particular. The subject of the course was the Industrial Revolution; but because of his own work on *Industry and Government in France and England: 1540-1640* (1940), Nef spent a disproportionate amount of time on this earlier period. Needless to say we read from the classics of modern European economic history: Tawney, Lipson, Clapham, Hammond and Mantoux; but we also read Thorstein Veblen's *Imperial Germany and the Industrial Revolution* (1915) and learned from it about the advantages of the latecomer to industrialization. More than anything else, Nef was concerned with conveying a broad picture of historical development - a picture of the complex interrelationships of economics, politics, war, technology and culture. Toynbee's monumental six-volume *Study of History* (1934) was then the rage, and Nef had us read selected parts of its first volume and compare them with the introduction to Spengler's *Decline of the West* (1926). But what had even more of an impact on me was the "assignment" which consisted of the Epilogue to Tolstoy's *War and Peace* - and which also led me the following summer to read the whole book. Nef's course was the one *par excellence* which fulfilled that function of a university concerned with the transmission and development of broad cultural values.

I received my B.A. degree in spring 1943, and after an interruption returned to Chicago in winter 1944 to begin with graduate studies in economics, supplemented by a series of courses in mathematics which corresponded more or less to what was required for a bachelor's degree in this field. It was wartime, and there were few students in the Department, the men among us having been rejected for one reason or another (in my case, chronic ear problems that dated back to infancy) for army service. So we were a closely knit group of at most fifteen or twenty students. Among my fellow students were Marianne Abeles, later Ferber (now at the University of Illinois), Sonia Adelson, later Klein (now with the Wharton Econometric Forecasting Associates at the University of Pennsylvania), the

late Jacob Cohen (University of Pittsburgh), the late Robert Ferber (University of Illinois), Bert Hoselitz (University of Chicago), Ray Kosloff (the only Israeli in the class, formerly director general of Electrochemical Industries Ltd., Haifa), Jack Leticche (University of California, Berkeley), and Rolf Weil (Roosevelt University).

On the other hand, the teaching staff of the department at Chicago was barely touched by the exodus to Washington (to serve in government agencies dealing with the economic problems generated by the war) experienced by other economic departments in the United States. On the contrary, it was reinforced by two new groups of economists: a group of agricultural economists led by Theodore W. Schultz; and a group of mathematical economists and econometricians who formed the new nucleus for the Cowles Commission for Research in Economics, led by Jacob Marschak. The latter were to reestablish Chicago as a center of econometrics after its role in this field (and, indeed, in empirical economics in general) had almost come to an end as a result of Henry Schultz's untimely death in an automobile accident in 1938, as well as Paul Douglas' already noted departure for the war, both of which events took place against the background of Knight's continued anti-quantitative bent and Simons' disinterest in such work. And though Viner's attitude to quantitative economics was basically positive, it was not enough of a counterweight.²

Most of my formal graduate instruction, however, was at the hands of the veteran teachers of the Department. Thus it was at this time that I had my first real contacts with Knight and Viner.³ With Knight it occurred when I tried sitting in on his course on Price and Distribution Theory, the first course in economic theory required of all graduate students. I found myself then quite confused by the middle-aged (he was then in his late 50s), medium-height, plumpish and moustached man who stood at the side of the large elliptical wooden table in one of the seminar rooms on the first floor of the Social Sciences Research Building – leaning on the back of a chair,

² Viner's attitude is implicit in his empirical study (originally his doctoral thesis at Harvard) *Canada's Balance of International Indebtedness 1900-1913* (1924), and explicit in his subsequent cautiously encouraging comments on quantitative economics, which were however accompanied by sceptical remarks about alleged "statistical laws" (1928, pp. 45-6).

³ The following paragraphs reproduce material from my memorial essay, "Frank Knight as teacher" (1973).

occasionally puffing on a corn cob pipe and rambling on in a high-pitched voice and in a disjointed manner on mysterious issues that certainly cast no light on the newly revealed truth which was then being enthusiastically explicated everywhere – in words, in graphs, as well as in mathematical formulas – of "marginal revenue = marginal cost". And after a few such bewildering experiences, I gave up in despair.

But not for long. The following year I registered for the course, and this time succeeded in taking fairly coherent class notes. But I must admit that my greatest pleasure and benefit from the course came when, toward the end of my graduate studies, I sat through it once again.

Many factors lie behind this long road to understanding and appreciation of Knight as a teacher. First of all, Knight gave little emphasis in his teaching to those things to which the beginning graduate student is normally attracted – namely the technical aspects of the discipline, and the newer the better. In part this was due to the fact that Knight was just not interested in these aspects, and in part because he took this knowledge for granted and wanted to get at the more fundamental issues that lay behind the assumptions and implications of the analysis.⁴ And this was not simply a reflection of the period and of the generation – for my class notes of Jacob Viner's parallel version of the graduate theory course show that Viner (in addition to being concerned with the broader issues of analysis and scholarship) gave considerably more emphasis to the technical aspects of the analysis than did Knight. Thus, for example – as might be expected from his pioneering article on "Cost curves and supply curves" (1931)⁵ – Viner provided a fairly detailed presentation of the properties of these curves under the assumptions of imperfect as well

⁴ Knight did, however, devote a lot of time explaining the three phases of the curve depicting the relation between total output and the input of labor, with capital equipment (or land) fixed (namely, increasing returns, diminishing returns and negative returns). In this context he also emphasized that in practice we are always in the phase of diminishing returns, and that the phase of increasing returns to labor corresponded to that of negative returns for the fixed factor of production.

⁵ With its famous complaint about the stubborn draftsman who refused to comply with Viner's instruction to draw the long-run average cost curve as one passing through the minimum points of all the short-run curves, and not lying above any of them. It is a mark of Viner as a scholar that in subsequent reprints of this article he left this error uncorrected in order to provide "pleasure" to "future teachers and students" (Viner 1952, p. 227).

as perfect competition, though Knight too paid some attention to the theory of imperfect competition.

But this was not the only cause of the beginning graduate student's difficulties with Knight's course. For, to tell the truth, Knight was not a good teacher in the sense of systematically introducing and developing a subject. Nor did he make a pedagogic effort to motivate the student to understand the subject in question by explicitly relating it to the general framework of economic analysis. All this, Knight took for granted as being known to the student – and devoted himself instead to forcing the student to rethink the basic issues of economic theory as he saw them.

Another difficulty with Knight's lectures was that at crucial points they frequently (and unawares to the student) turned into brief and cryptic summaries of views that Knight had developed at length in various of his writings – to which for the most part he did not explicitly refer. Thus a brief reference to the invalidity of "productivity ethics" (i.e., the view frequently identified with J.B. Clark that paying a factor of production in accordance with its marginal productivity generates a just distribution of income) or to "commutative justice versus distributive justice" (where the former has the same meaning as productivity ethics and the latter means a distribution of income which accords with the ethical values of society, with the distribution that it regards as just) could hardly convey the depth and meaning of Knight's famous essay "The ethics of competition" (1923). Nor could passing references to the long-since forgotten debate about "real-cost versus alternative-cost theories of value" (i.e., the view that the source of value of a product are the efforts that were invested in producing it *versus* the view that the source of value is the alternative product that could have been produced with those efforts) mean much to a student who was unaware of Knight's many writings on this question.

For this reason I remember characterizing Knight's lectures at that time as being like a general-equilibrium system – which could not be solved until the student was familiar with the set of Knight's articles that specified the relationships between the various parts of the lectures, and thus converted their unknowns into knowns. And I still remember my feeling of satisfaction when I began to see the system of equations as a whole – and things began to fall into place.

The general image of Frank Knight in the profession is of a conservative, so it seems odd to think of him as a radical economist – but that is one of my basic impressions of the man. Knight was not, of course, a radical in the sense of being an advocate of Marxian economics: he had little patience with – and, indeed, had intellectual contempt for – the labor theory of value. But for the modern economist Knight was – or, more accurately, could have been – a far more effective radical than Marx: for in contrast to Marx, Knight understood the workings of the market system; but he went on by a deeper analysis of these workings to deny the ethical foundations of this system. In fact, there is little in the writings of the modern day "radical economists" as described, for example, by Lindbeck (1971), that was not more trenchantly said by Knight in his essay – published just over seventy years ago – "The ethics of competition" (1923, especially pp. 45-58). To my mind, these fourteen pages are among the most radical ever written in economics, and even now they remain sharply imprinted in my memory as among the most exciting things that I read during my student days. And in my early years of teaching price theory, these pages were part of the required reading for students.

Two further comments about Knight's lectures: First, one of the points that he repeatedly emphasized was that the term "competition" in "perfect competition" did not have the connotation of personal rivalry. "The Iowa farmer growing corn", said Knight, "does not feel that he is in competition with his neighboring farmer". Indeed, market competition is a way of coordinating the activities of an economy. Second, though I do find in my notes from one of Knight's lectures (28 July 1945) a reference to his classic *Risk, Uncertainty, and Profit* (1921), I do not find any reference in any of my notes to the basic distinction that he drew in this book between risk and uncertainty. Nor did Knight assign us any readings from this book.

Viner as a teacher was more systematic and precise than Knight, more sharply to the point in classroom discussion and in replies to student questions. Indeed, Viner's sharpness – as he sat, short and upright, holding in both hands on the table in front of him the index cards from which he lectured – sometimes deterred such questions. On the other hand, Viner was less dogmatic than Knight, less likely to see things in terms of black-and-white, much more a man of the world. Since their graduate-theory courses were so different (in both style and content) most of us took both of them, enjoying as a

by-product the running debate between them on the relative merits of the real-cost (Viner) and alternative-cost (Knight) theories of value. At the same time, their courses did have the common feature of using Marshall's *Principles* as the basic text and correspondingly devoting much attention to the assumptions underlying Marshall's demand curve and his related notion of consumer's surplus. Indeed, this was the background of my article many years later on the subject (1963).

Knight also gave his famous course in the history of economic thought. Here he had us read the classic works of Smith, Ricardo and Mill and then write a term paper on the classical cost-of-production theory of value. Though Viner did not offer a formal course in the history of thought, he frequently dealt with questions of doctrinal history in his courses on international trade theory and policy, as well as in his theory course proper. Thus his international trade course was based to a large extent on his classic *Studies in the Theory of International Trade* (1937), with its celebrated chapters (really, brief monographs that constituted roughly half the book) on mercantilism and on the famous nineteenth-century English currency controversies (namely, those of the bullionist *versus* anti-bullionist, and the subsequent currency school *versus* banking school) which at that time (and to a certain extent even now) are a basic part of the education of a monetary economist.

Here again Knight's and Viner's approaches differed. For Knight tended to analyze doctrinal developments from the viewpoint of the current state of economic theory, against which he measured them; in contrast, Viner was more concerned with scholarship as such – with (as he was to later express himself) “the pursuit of broad and exact knowledge of the history of the working of the human mind as revealed in written records”.⁶ I will always remember with deep appreciation the patient help and interest with which he guided and encouraged my own first efforts in this direction. These took the form of a seminar paper on “Mercantilism and the readmission of the Jews to England in the seventeenth century” for his course on International Economic Policies. With Viner's further encouragement, I revised this paper for submission to one of the professional journals – and revised it again after it was initially rejected. Both because of its

⁶ Quoted from Viner's eloquent Brown University convocation address, entitled “A modest proposal for some stress on scholarship in graduate training” (1950, p. 369).

association with Viner, and because of its reflection of my joint interest in economics and Jewish studies, it has always been a source of gratification to me that this was my maiden scientific publication.

In retrospect, I realize that in their combination of work on economic theory and the history of economics, Knight and Viner served as role models for me: that, although I was not conscious of it at the time, they provided the example that I was later to follow in devoting roughly one-fifth of my *Money, Interest, and Prices* (1956 and 1965 edn.) to “Supplementary notes and studies in the literature”. And Viner also influenced me in his approach to the history of doctrine, as contrasted with that of Knight.

Knight and Viner also served as joint editors of the *Journal of Political Economy*. As I look back now on one of the articles which they published at that time – and which created quite a stir among us students – I realize that it can serve today as an instructive example of the objectivity and lack of personal considerations that should guide journal editors. I am referring to Robert Bishop's “Professor Knight and the theory of demand” (1946), which was a long and detailed criticism of Knight's “Realism and relevance in the theory of demand” (1944), which in turn was a methodological criticism of the indifference-curve analysis presented in Hicks' *Value and Capital* (1939).

The main teacher of graduate economic theory was, however, Oskar Lange, who had joined the Department in 1939 to continue, *inter alia*, with the work in mathematical economics that had been started by Henry Schultz. In a sequence of systematic courses, Lange brought us to what were then the frontiers of micro- and macroeconomic theory. These courses covered the imperfect competition theories of Joan Robinson (1933) and Edward Chamberlin (1933), against the background of the earlier duopoly theories of Cournot and Bertrand; Hicks' *Value and Capital* (1939) and the related Paretian welfare economics, together with Lange's (1936) and Lerner's (1944) confident application of the latter to the economics of a market-directed socialist economy; and business-cycle theory, beginning with a historical discussion of Say's law, continuing with an exposition of Marx, going on to a detailed presentation of Keynes' *General Theory* (based on Lange's own interpretation in his “Rate of interest and the optimum propensity to consume”, 1938) and of the advantages of Keynesian macroeconomic policies, and concluding with the business-cycle theories of Kalecki and Kaldor. But Lange's most valuable course for me was

the one on Mathematical Economics (i.e., what was then called mathematical economics!). Here he systematically took us through the Mathematical Appendix of Hicks' *Value and Capital* (1939), as well as Paul Samuelson's path-breaking article on "The stability of equilibrium" (1941), subsequently reproduced as chapter 9 of the latter's *Foundations of Economic Analysis* (1947). My lecture notes from this course served me as a "reference volume" for many years to come.

Lange was an excellent expositor – clear, systematic, and thorough, with the wonderful ability of getting to the essence of a problem and presenting it in as simple a way as possible. Indeed, too simply: for sometimes he left no intellectual challenge for the student, no need for the effort of understanding which is so akin to that of intellectual creation. Another characteristic of Lange's classroom performance was the slow, patient and methodical way (so much in contrast with Viner's) with which he frequently answered student questions. Nor was Lange embarrassed if he had to think silently on his feet for several moments before offering to answer – sometimes concluding even such a pause with the simple statement that he had to give the question further thought and would defer the answer to the next class meeting. And having said this, he would limp back to the blackboard and continue with his exposition.

There was a striking contrast between the teaching styles of Lange and Knight. Where Knight devoted much attention to probing (in his rambling and often obscure manner) into the meaning of the basic definitions and assumptions of the analysis ("perfect competition", "perfect foresight", "wants", "costs", "capital", "equality", and the like), Lange (in his contrastingly clear and systematic manner) was primarily concerned with drawing the logical implications of these assumptions. So where Knight taught economic theory in a loose, "literary", philosophical fashion – and was antipathetic to mathematical economics – Lange was formal, rigorous, complete, and frequently made use of mathematical tools. And where Knight was basically unsympathetic to the new developments in economic theory (read: Keynes and Hicks) – and even, I would say, instinctively critical of them – Lange was an early convert as well as an efficient expositor and refiner. Again, and most ironically, it was the socialist Oskar Lange who extolled the beauties of the Paretian optimum achieved by a perfectly competitive market – and Frank Knight who in effect taught us that the deeper welfare implications of this optimum were

indeed quite limited. Thus Knight and Lange complemented each other in a most wondrous way – thereby increasing the productivity of each of them in the teaching process. From the implicit dialogue that thus took place between these two teachers, we students were the direct beneficiaries.

During 1944 Lange became increasingly active in the affairs of the Polish government-in-exile. These activities sometimes made it necessary for him to be out of town, at which times Leo Hurwicz would take over his classes. There was that memorable occasion in 1944 when Lange was absent for an unusually long period – and when the mystery of his absence was suddenly solved by a front-page newspaper picture showing him meeting with Stalin in Moscow. After the summer quarter of 1945 Lange resigned from the University to become the first post-war Polish ambassador to the United States. According to the student gossip at that time, Lange subsequently returned to Poland in the hope of playing a leading role in a Socialist party which would function as a loyal opposition. If so, it was a hope that did not long endure. Ever since, I have thought of Lange as a tragic figure.

This view of mine of Lange was only reinforced by two further events. First was the publication in 1954 of an English translation of an article that he had published the year before in Polish on "The economic laws of socialist society in the light of Joseph Stalin's latest work".⁷ Second, was an occasion in 1957 – after Gomulka's relaxation of restrictions on Jewish emigration from Poland – when an economist who had been a student of Lange's there came to Israel and brought a letter of introduction to me from him, with no indication whatsoever in it that he knew me. I have often wondered whether because of the political situation that then existed in Poland, Lange was afraid to indicate that he was personally acquainted with an Israeli.

Together with these theory courses, I continued to take courses with Simons and Mints, as well as with Simeon Leland. The latter's

⁷ See especially the editor's note at the beginning of the translation which points out that while there is considerable overlap between this article and Lange's 1936-37 articles on socialism, the 1954 article differs significantly from it in two respects: the attention devoted to Stalin's views (as is obvious from the title) and the omission of any reference to those of Kautsky, the ideologue of the social democrats in their controversy with communists, whose views Lange had sympathetically considered in detail in his earlier articles.

courses in public finance (with its frighteningly fat bibliography) was largely descriptive and institutional in nature. Simons' course on the Economics of Fiscal Policy emphasized the theoretical questions discussed in his closely-reasoned monograph on *Personal Income Taxation* (1938), but dealt with other problems as well. Mints was less effective in his courses than in his undergraduate one. He devoted about half of his graduate course on money to the traditional quantity theory and the teachings of Hawtrey and Robertson, and the other half to Keynes' successive works – the *Tract, Treatise on Money*, and *General Theory*. In his Banking Theory and Monetary Policy course, Mints lectured from the manuscript he was then completing for his subsequent book *A History of Banking Theory* (1945). By means of repeated arithmetical examples (*ibid.*, p. 34), he also gave us a life-time inoculation against the fallacies of the real-bills doctrine. Both Simons and Mints also devoted much attention to the question of proper monetary and fiscal contracyclical policy for an open economy.

Mention has already been made of the two new groups of economists who joined the Department in 1943. The group of agricultural economists – Theodore W. Schultz, D. Gale Johnson, and William H. Nicholls – had been exiled from Iowa State University for having dared (in that dairy state!) to support research showing that margarine was as nutritious as butter. Toward the end of my formal course work, I took a course with Nicholls on imperfect competition, an introductory course with Johnson on agricultural economics, and a graduate one in this field with Schultz (who by then had succeeded Leland as Department chairman). I must confess that what I remember best from these courses are not matters of agriculture but the painful struggle through the abstruse chapter on Alfred Marshall in Talcott Parsons' *Structure of Social Action* (1937), which Schultz had us read. Though this struggle was not completely successful, it did serve the important purpose of forcing us, at least momentarily, to consider the assumptions of economic analysis from a new viewpoint, the viewpoint of the forces at work in society as a whole. And it forced us in particular to ask ourselves (*à la Marx*) why centers of economic power in a society would permit the development of the legal and institutional arrangements necessary for the functioning of a competitive economy that would curtail their power.

When I first came to Chicago, H. Gregg Lewis (who in addition to his teaching duties served as an invaluable adviser to all of us) was

almost the sole active representative of econometrics, and indeed of quantitative economics in general. He had earlier assisted both Douglas and Henry Schultz, and was then working on his doctoral thesis "The elasticity of demand for steel" (1947). Lewis' course on statistical correlation introduced us to the mysteries of multivariate regression analysis; and we were required to demonstrate our mastery of these mysteries by laboriously carrying out (on the mechanical desk-calculators of the time) a term project (*sic!*) consisting of the estimation of one such regression for four variables. That boring, time-consuming experience was enough to convince us of the desirability of devoting much thought to the economic meaning of an equation before undertaking the task of fitting it to the data – the kind of thought that is all too frequently missing in these days of instant estimation.

The temporarily lagging state of econometrics in Chicago was replaced by one in which Chicago once again became a leader in the field with the advent in 1943 of the second group of economists mentioned above – the group that constituted the Cowles Commission for Research in Economics, under the directorship of Jacob (or Jascha, as we rapidly learned to call him) Marschak. Within a short time after Marschak's arrival in 1943, Ted Anderson, Trygve Haavelmo, Leo Hurwicz, Lawrence Klein, Tjalling Koopmans, Herman Rubin, and (somewhat later) Kenneth Arrow had joined the staff – some of them with joint appointments in the Department. And it was my good fortune to receive a fellowship from the Social Science Research Council that enabled me to spend the academic year 1946-47 as a junior member of the Commission for the purpose of writing my doctoral thesis (about which more below). After the completion of the thesis in 1947, I stayed on for another academic year as a research associate, with a joint appointment as assistant professor in the Department. So my memories of the Cowles Commission are from this two-year period.

My first contact with Marschak was in an advanced graduate course in mathematical economics devoted to solving the problems in the second half of R.G.D. Allen's *Mathematical Analysis for Economists* (1938). Later I took a seminar with Marschak in which we went through Tinbergen's pioneering econometric model of *Business Cycles in the United States of America, 1919-1932* (1939). In the first of these courses, I was the only student in the class – and this gave me an unusual opportunity to get to know Marschak as a teacher. His

method was to stimulate you into thinking, to question and question again what seemed to be obvious in order to bring you to look deeper. Sometimes you were irritated by his repeated prodding, but you ended with a deeper understanding and appreciation of the fundamentals of the problem.

I recall being particularly irritated at some point a couple of years later when I was in the final stages of writing my Ph.D. thesis. The thesis was full of references to "classical economics" and the "classical system"; and Marschak (who was chairman of my thesis committee) suggested that I document these references with citations of the relevant literature. I remember my feeling at the time that his question was needlessly nagging: for surely "everybody knew" what neo-classical economics was. It took me three or four years – and the unpleasant (but most salutary) experience of having my wrist publicly slapped in a note in *Econometrica* by William Jaffé (1951) that pointed out that in my 1948 article in that journal (see below) I had egregiously misrepresented Walras' theory of money – to learn to appreciate the importance of Marschak's suggestion. In the seminar on the history of monetary thought that I gave over many years, I always told this story of the "sins of my youth" to my students, as an object lesson on the meaning and importance of proper scholarship. In any event, it was Jaffé's criticism that created for me the challenge to later undertake the intensive and detailed study of the development of Walras' theory of money that I then published as Supplementary Note C in *Money, Interest, and Prices*. In it, I thanked Jaffé (who by then I had met personally) both for his original criticism and for his comments on a draft of my Note (though I did not agree with all of them, just as I did not agree with his translation of some of the passages in Walras' *Eléments d'économie politique pure* in his [Jaffé's] 1954 translation of this book into English – see Patinkin 1956, p. 402, n. 61; 1965 edn., p. 559, n. 61).

I must digress briefly at this point to tell another story about the writing of my Note C. At the end of Israel's War of Independence in January 1949, Jerusalem remained a divided city, with Western Jerusalem (the New City) in Israeli hands, and Eastern Jerusalem (including the Old City, but with the exception of an enclave in Israeli hands on Mount Scopus, on which the Hebrew University of Jerusalem and the Hadassah Hospital were located) in Jordanian hands. Although the University library with most of its books was thus stranded on Mount Scopus, it had been possible during the early

stages of hostilities to bring its catalog down to the temporary quarters in the Terra Sancta building that the University had rented in the New City. When in 1954 I began work on Note C, I had a copy of the second edition of Walras' *Eléments* which – purely by chance – I had purchased a few years before. I also had a copy of the 1952 reprint of the 1926 definitive edition. And the catalog showed me that – again purely by chance – in the library on Mount Scopus were to be found all the other editions (namely, the first, third, and fourth) of the book. So near, but yet so far!

But there were the convoys which under U.N. supervision went up every two weeks to Mount Scopus with relief forces for the army unit stationed there and with supplies for them, and sometimes with visitors as well. And if the desired article was not too large, a frequent solution was to find a well-endowed Hadassah matron who was going up on the convoy and would volunteer to smuggle the article down, hidden in places where no U.N. soldier would ever dare to search. So once again Walras was close to the heart of a woman – and I had all five editions of the *Eléments* on which to base my Note C on the development of Walras' theory of money.

I return now to my account of the Cowles Commission. In those days, it was a small, intimate group. And in the two years that I was associated with it, I also enjoyed the warm social contacts that all of us had with Jascha. It was on these occasions that one could best enjoy his quiet and charming wit, as well as his fascinating stories. I particularly remember one evening at his apartment when he told us about his experiences as a young man in 1916 as a political prisoner for several months (until freed by the Kerensky revolution) in a Czarist jail. "The jails", I remember him saying, "were the only place in Russia that the Czar's censorship did not reach: there were no restrictions on the books and other reading material that circulated among us, nor on the intense discussions that we had of them. So these prisons were actually the free universities of Russia – and that is where I read more and learned more than in any comparable period in my life".

Jascha did not share the Zionist beliefs that had been part of me since childhood. But he knew that I was hoping after completing my doctorate to go to what was then Palestine to teach at the Hebrew University of Jerusalem. So when in mid-1945 he received a letter from a friend of his in Palestine who wrote on behalf of the Hebrew University to inform him that it was planning to establish a depart-

ment of economics and asking him if he would consider coming to head it, Jascha replied that he himself was not interested, suggested the names of several established Jewish economists in the United States, and then added the names of some of his students, including mine. And at the beginning of 1946, Jascha suggested that I write the University to present my candidacy. This I did, in a letter to Martin Buber, world-famous social philosopher and theologian, who was then head of the Committee for the Social Sciences in the Faculty of Humanities. (A separate Faculty of the Social Sciences was established only in the late 1950s.) From that began a correspondence with the Hebrew University, as well as meetings with some of its visitors to the United States, that ultimately led to my coming to Israel together with my wife in early 1949 as a Lecturer in Economics at the Hebrew University, a few weeks before it reopened after having been closed for almost a year as a result of Israel's War of Independence.⁸

In the fall of 1945, I completed my preliminary examinations for admission to the Ph.D. program and began the difficult search for a thesis topic. The first one that I thought of undertaking – and even prepared an outline for – was “The international economic position of Palestine”. In this connection I even had some correspondence with Oscar Gass, who in early 1946, together with Robert Nathan and Daniel Creamer, published the path breaking and influential book *Palestine: Problem and Promise*. But neither Gass nor others whom I consulted on the question felt that adequate data were available in the United States to carry out the proposed study, and so I dropped that idea.

Next – under the influence of the pioneering work that Lawrence Klein was then carrying out at the Cowles Commission on an econometric model of the United States in the interwar period – I

⁸ Jascha's friend was one Ernst Kahn, whom he had known from the time in the early 1920s in Germany when they were economic correspondents of the *Frankfurter Zeitung*. In the archives of the Hebrew University there is a copy of Marschak's aforementioned reply to Kahn, dated 5 June 1945.

When I came to Israel, I brought regards from Marschak to another of his friends from the time of their work together as economic correspondents of the *Frankfurter Zeitung*, namely, Fritz Naphtali, who was then a prominent member of the Israel Labor Party and later (1952-55) a Minister of Agriculture in the Israel Government.

Further details about my contacts with the Hebrew University that led up to my appointment there are contained in part 1 of the Hebrew article referred to in footnote * on p. 359.

thought of constructing an econometric model for the manufacturing sector alone, and even began collecting some of the relevant data. I also progressed enough in the theoretical analysis to prepare a paper entitled “Market-adjusting and inventory equations” which I presented at the 1947 meetings of the Econometric Society (see abstract in Patinkin 1947). This was a most memorable event for me: not only for its having been my first participation in a scientific conference, but also for the fact that the chairman of the session was none other than Irving Fisher himself. Unfortunately, I did not have enough presence of mind to have a photograph taken of the two of us together.⁹

In the course of my work on this dissertation topic, I became confronted with the problem of dealing with unemployment in the manufacturing sector, which led me to ask myself about the general meaning of “involuntary unemployment”. And I still remember my excitement when I thought of interpreting it in terms of an inconsistency in the system which prevented it from reaching an equilibrium position and thus *inter alia* caused workers to be off their supply curve of labor, and hence not acting in accordance with their desires. With the brief note that I then hurriedly wrote to expand upon that idea, I rushed to Gregg Lewis who (in his characteristically quiet way) was encouraging, while at the same time emphasizing that much work remained to be done. Marschak was also encouraging. And this was the beginning of my doctoral dissertation “On the consistency of economic models: a theory of involuntary unemployment”, which was submitted in the spring of 1947.

As indicated above, the chairman of my thesis committee was Marschak; its other members were Gregg Lewis, Paul Douglas (who had returned to the University after being wounded in combat, but who was subsequently to leave for the United States Senate after his successful 1948 election campaign), and Theodore O. Yntema (who was actually no longer on campus). Only with Marschak and, especially, Lewis did I have some contacts. The thesis consisted of two parts: the first dealing with the mathematical consistency of a general-equilibrium system with money; and the second with unemployment interpreted as the manifestation of an inconsistent system. The main ideas developed in this second part were actually those in the afore-

⁹ I remember Fisher as a short, bearded and wizened old man. Three months after the meetings he died at the age of 80.

mentioned note with which I had first come to Lewis. They are the ideas presented – in essentially the same form as in the original thesis – in my 1949 “Involuntary unemployment and the Keynesian supply function” article. At the time, I considered these ideas to be the major contribution of the thesis. They were subsequently developed into chapter 13 of my *Money, Interest, and Prices* (1956), which chapter I also regarded as a major novelty and contribution of the book. And for many years afterwards I was disappointed that my approach to the problems raised in it did not attract more attention, a situation which changed with the development in the late 1960s and 70s of the literature on disequilibrium macroeconomics (see pp. xvi-xx of the 1989 edition of the book).

Though Lange was no longer in Chicago at the time I wrote my thesis, his influence is most apparent in it, particularly in its first part. For though this contains some criticism of Lange’s work, its point of departure is clearly Lange’s classic paper (in the Henry Schultz memorial volume), “Say’s Law: a restatement and criticism” (1942).

Practically all of the first part of my thesis appeared in two *Econometrica* articles: “Relative prices, Say’s Law, and the demand for money” (1948) and “The indeterminacy of absolute prices in classical monetary theory” (1949). The 1948 article and the first three parts of the 1949 one were more or less unchanged from the thesis. These first three parts were primarily devoted to demonstrating the invalidity of the traditional dichotomy of general-equilibrium theory between the determination of equilibrium relative prices, on the one hand, and the equilibrium absolute price level, on the other. According to this dichotomy, in a Walrasian system of n excess-demand equations – with the first $n - 1$ referring to commodities and the n^{th} to money – the commodity equations depended only on relative prices and so were not affected by an equi-proportionate change in all of the absolute prices (the “homogeneity postulate”), and accordingly these $n - 1$ equations determined the equilibrium values of relative prices, while the equilibrium value of the absolute price level was then determined by the excess-demand equation for money (cf., e.g., Leontief 1950 and Modigliani 1944, § 13). And I pointed out that by the interdependence of the n equations specified by Walras’ Law, if none of the commodity equations depended on the absolute price level, then neither could the equation for money. Like in my thesis, the first few paragraphs of the fourth part of the 1949

article were devoted to a description of various *ad hoc* ways in which the commodity equations could be made dependent on the absolute price level. However, the last ten paragraphs of this part of the article – which presents what I then termed a “modified classical system”, in which there was no such dichotomy, but in which the classical neutrality of *money à la* quantity theory nevertheless held – did not appear in the original thesis.

Let me digress at this point from this account of my studies to discuss these ten paragraphs, for they have had a significance for me beyond their specific content. These paragraphs – which I afterwards considered to be the most important in the published article – were (to the great annoyance of the managing editor) inserted in galley proof. And I still have vivid memories of that “moment of truth” when everything suddenly fell into place: when after having long been troubled by the problem, I suddenly realized that the economically meaningful way for the commodity demand equations to depend on the absolute price level (and thus to avoid the invalid dichotomy) without violating the neutrality of money was to have them depend on the real value of money balances. In retrospect, it was clear that all the elements of this solution had been present in the original dissertation discussion which these ten paragraphs replaced, but I had not realized it until that moment.¹⁰ And it is this personal experience of knowing, but not knowing – knowing something, but not realizing its “obvious” implications for other problems with which I was concurrently dealing until a later point of time, an experience that I have had on other occasions as well – that has strongly influenced my subsequent work in the history of doctrines, especially that dealing

¹⁰ A more concrete manifestation of my failure to recognize this solution in my dissertation is the fact that what I then called the “Pigou effect” was represented in it – and, correspondingly, in the aforementioned 1949 *Economic Journal* “Involuntary unemployment and the Keynesian supply function” article – by an expenditure function which depended, not explicitly on real money balances, but on the absolute price level. The same was true of the savings function $S = \Gamma(r, Y, p)$ in the paper which I originally published on the Pigou effect, “Price flexibility and full employment” (1948, p. 547), which is an extensive elaboration of the corresponding discussion in my thesis. Significantly enough, this function was rewritten as $S = \Gamma(r, Y, M/p)$ when three years later (i.e., after the publication of my 1949 *Econometrica* article) I made some revisions in the article for republication in the American Economic Association’s *Readings in Monetary Theory* (Lutz and Mints eds., 1951, p. 258; see also the reference to my 1948 article in the aforementioned ten paragraphs that were added to my *Econometrica* article – p. 23, n. 22).

with the discovery of the "General Theory". For as presumptuous as it may be, we naturally project from our own experiences in trying to understand how the minds of others work.¹¹

Here are two examples of the personal experience that I have just referred to. First, although I was a discussant of Clower's well-known article "The Keynesian counterrevolution: a theoretical appraisal" (1965) when it was first presented at a 1962 conference, only some time after I had published the second edition of my *Money, Interest, and Prices* (1965) (on which I was working at the time of the conference) did I realize that his "dual decision hypothesis" about the behavior of consumers who were recipients of income from labor was simply the obverse side of the contention in my chapter 13 that the inability of firms to sell the quantity indicated by their supply curve for output, drawn as of a given real wage rate, would make them unwilling to employ the amount designated on their demand curve for labor for that wage. Similarly, Clower's "dual decision hypothesis" stated that since the labor market was not in equilibrium, workers could not assume that their income would correspond to the amount of labor that they were willing to supply at the given real wage, but that it would instead be determined by the smaller amount that they would succeed in selling to employers – and that the corresponding smaller income would determine their demand for goods. As a result of my failure to realize this point in time to take account of it in the second edition of my book, I did so only much later, in the new introduction to its 1989 edition (pp. xvi-xviii), by which time it had

¹¹ This methodological approach has been the source of an ongoing, friendly disagreement between Paul Samuelson and myself on whether Richard Kahn's 1931 multiplier (the mathematical equivalence of which to the theory of effective demand Samuelson demonstrated at a 1975 conference) constituted an anticipation of the "General Theory" (see the record of this discussion in Patinkin and Leith eds., 1977, pp. 80-7, 115-19 and 122-4). In the last round of this disagreement at a 1987 conference, I concluded my reasons for rejecting Samuelson's contention on this score by turning to him and saying that his "extraordinarily quick and encompassing mind makes him less inclined to believe that others did not understand what seems so obvious to him", and that "these qualities are a comparative disadvantage in the study of the history of the development of thought" – to which Samuelson's on-the-spot retort was, "Just like judo: using your opponent's strength to throw him" (Patinkin 1991, p. 409).

In his contribution to the aforementioned 1975 discussion (pp. 115-16), Samuelson reminded us how ideas sometimes come suddenly, as in the case of Archimedes in his bathtub and Shackle while doing the dishes. I am glad to provide further support for this aquatic principle of discovery by noting that the realization of how to introduce the absolute price level into the system without violating the neutrality of money came to me while standing next to our washing machine, watching the tumbler go around.

long-since been made by Barro and Grossman (1971, pp. 83-4) and others. Indeed, it may have been from the Barro-Grossman article that I realized this.

Second, only after several years in which I had (in an undergraduate course on macroeconomics) been teaching the Friedman-Schwartz (1963, pp. 784 ff.) equation for the supply of money (i.e., the money multiplier) did I realize that equation 21 on p. 300 of the 1965 edition of *Money, Interest, and Prices* was a variant of it.

There was another aspect of the ten paragraphs that I added at the proof stage to my 1949 *Econometrica* article that I would like to mention. With their detailed economic interpretation of the mathematical results of the article, these paragraphs also differed in style from my thesis, and hence (since they more or less reproduced the material of the thesis) from the style of the 1948 article and the first three parts of the 1949 one. For like most doctoral students (then, and I am afraid even more so now), I attributed too much importance to technique and formal mathematical analysis. And so my thesis gave much emphasis to the rigorous derivation of theorems from definitions, assumptions, and preliminary lemmas, while devoting inadequate attention to the economic interpretation of the analysis. Thus some of the concluding theorems in my 1948 article (viz., theorems XII and XIV) described the properties of a system of demand equations derived from the ridiculous assumption that the individual maximizes a utility function dependent on the *nomina* quantity of money held!¹²

In the years following my 1949 article, I gradually developed the philosophy that the mathematical analysis of any economic problem is not complete until it is given an intuitively appealing economic interpretation. From experience over the years I also learned that when there was a contradiction between the mathematics and the intuition, it is not always the intuition that was at fault, but frequently an implicit (and sometimes explicit) misguided assumption in the mathematics. Thus resorting to intuition as well as mathematics provides a most useful check on the analysis. It is a way of carrying out a fruitful dialogue with oneself. And it is the dialogue that I later carried out between the text and mathematical appendix of *Money, Interest, and Prices*.

¹² For this reason, I did not include this essay in my 1981 book of Chicago essays (see *ibid.*, p. 11, n. 18).

These last ten paragraphs in the 1949 article also included a significant addition to the issues explored in my thesis. In particular, there is no mention in my thesis and accordingly (except for these paragraphs) in my two *Econometrica* articles of the relation between the quantity of money and nominal prices, and of the neutrality of money in particular. This additional issue was even more explicit in my subsequent 1954 "Keynesian economics and the quantity theory" article. In it I enthusiastically demonstrated the erroneous nature of Keynes's contention that absence of a speculative demand for money was a necessary condition for the validity of the quantity theory (*General Theory*, pp. 208-209) – and pointed out that this contention was the result of Keynes' implicit assumption of money illusion in his liquidity-preference function (*ibid.*, p. 199). It was in the process of writing this article that I decided to write my 1956 book and even referred to it as "a larger work in which I am now engaged" (Patinkin 1954, p. 125, n. 7). In retrospect, perhaps I was too enthusiastic: for when the book finally appeared, its emphasis on the quantity theory had become so great, that its title (in sharp contrast to that of my dissertation) did not even contain the word "employment". I remember that at the time I was not happy about this, for (as indicated in the introduction to the book – 1956, pp. 2-3; 1965, p. xxv), I regarded the discussion of the theory of unemployment in chapters 13-15 of the book as constituting its "second major theme". I also remember toying with the possibility of adding "employment" to the title, but realized that this would make it sound most presumptuously similar to the title of another book, published twenty-odd years before it.

Let me return now to my experience at the Cowles Commission. This provided the opportunity of personal discussions with its various members not only in connection with my thesis, but on general economic questions as well. Thus I remember in particular the long hours spent with Trygve Haavelmo as, pipe clenched between teeth, he discoursed quietly on a wide variety of subjects: the nature of the simultaneous-equation bias as analyzed in his path breaking "Probability approach in econometrics" (1944), through which he guided me; the meaning of a derivative in economic analysis; involuntary unemployment; Slutsky's analysis of the generation of cycles by random shocks; and the like. Indeed, I still have the notebook in which I would afterwards summarize the main points of these "private seminars".

Similarly, there were stimulating discussions with Lawrence Klein on the manuscript of his then forthcoming *Keynesian Revolution* (1947b), and it was this excellent work which started me thinking about many of the problems discussed in the second part of my thesis. Equally stimulating were the discussions with Klein of the major work he was then beginning on providing empirical clothing for Keynes' theoretical model. (There were also our weekly squash games under the West Stands of Stagg Field, which came to an abrupt end when the courts were mysteriously closed off to the public. Only after the war did Larry and I, together with the rest of a world awestricken by the atomic bomb, learn of the "alternative use" to which these squash courts had been put – that it was here that Enrico Fermi and his co-workers carried out the first nuclear chain reaction, which in turn led to the construction of the bomb.) I also had enlightening discussions with Kenneth Arrow and Herman Rubin on different aspects of statistical theory, as well as of certain mathematical problems that arose in the course of my work. Leo Hurwicz helped all of us in, among other things, clarifying the properties of homogenous functions. He also essentially tutored me through the multilithed edition of S.S. Wilks' *Mathematical Statistics* (1943).

And then, of course, there were the valuable Cowles Commission seminars. Naturally enough, these were primarily devoted to reports on work-in-progress by staff members. The presentation of these reports was followed (or rather, constantly interrupted) by critical comments, a recurrent theme of which was the necessity (emphasized especially by Marschak) for basing the analysis – and the resulting empirical equations – on the principle of profit or utility maximization.

Needless to say, there were also papers by visitors. Among these were Ragnar Frisch and Jan Tinbergen on the occasion of their respective first post-war visits to the United States. Abraham Wald also came to give a series of lectures on statistical inference. Once the famous atomic physicist Leo Szilard (who together with Fermi played a leading role in generating that first sustained chain reaction under the West Stands, and who was also a friend of Marschak and Hurwicz) gave, at his (Szilard's) own request, a paper on a monetary scheme he had devised to solve the problem of underspending which he saw as the cause of depressions. The scheme involved the issuance of two kinds of money – one red, to be used for spending, and one green, to be used for saving and on whose holding a penalty was to be

imposed. Though I do not remember, I suspect that it was (gently) pointed out to Szilard at the time that such schemes had long since been proposed by Silvio Gesell and other monetary cranks.¹³

The seminars were frequently also attended by people who were not members of the Cowles Commission. Among these were Herbert Simon, who was teaching at the Illinois Institute of Technology, elsewhere in the city (later he was to become more formally affiliated with the Commission). Milton Friedman, who had joined the Department of Economics in 1946, would occasionally participate too. Indeed, one of my sharpest memories is of a seminar by Lawrence Klein in which I first heard Friedman advance the simply but powerful suggestion that a minimum test for the predictive efficiency of an econometric model is that it do better than a "naive model" which stated that the future would be like the past. I have since often wondered whether or not Friedman thought that up on the spur of the moment, as well he might. In the year after receiving my Ph.D. degree – during which I was also an assistant professor in the Department – I benefited from additional contacts with Friedman. He was working on his "Monetary and fiscal framework for economic stability" (1948) and I on my "Price flexibility and full employment" (1948), and I had stimulating discussions with him of both these papers. During this year I also had many fruitful contacts with Lloyd Metzler and Evsey Domar, both of whom had just joined the Department.

Every research organization has its golden period when its members are unified with a sense of mission about a common goal. Since my own work was peripheral to it, I can uninhibitedly express the opinion that those were the golden years of the Cowles Commission. Its mission was to base the estimation of structural equations from time series on firm probabilistic principles. And in accomplishing this mission it led to a revolution in econometric methodology by simultaneously demonstrating the inappropriateness of ordinary least-squares and developing alternative estimating procedures. Thus was Error in the form of simultaneous-equation bias analyzed and exorcised, and Truth in the form of maximum-likelihood estimates identified and enthroned. Armed with this truth, Lawrence

¹³ I am indebted to Lawrence Klein for reminding me about the details of Szilard's scheme. To the best of Klein's memory, the title of Szilard's paper was "A market economy without trade cycles".

Klein developed his econometric models of the United States economy (1947a, 1950), which were to become the prototype of much work in this field, and Tjalling Koopmans proclaimed the virtues of such models as compared with the "Measurement without theory" (1947) carried out by the National Bureau of Economic Research. But within a short time it became evident that despite the methodological superiority of the new estimating procedures, the predictions they yielded were no more accurate than the earlier ones. Since then, aided by the revolution in computer technology and by the great improvement in the quality and comprehensiveness of the data, econometric modelling has progressed¹⁴ and has met the test of the market. Nevertheless, this experience at the Cowles Commission – reinforced by others¹⁵ in the years which followed – left me with a good deal of scepticism about our ability to derive empirical macroeconomic structural relations which will stand up under the test of time.

That is the Chicago that I remember from my student days. It is undoubtedly an idealization of the past to think of it as a time when giants walked the earth. There are always giants. But it is not an idealization to say that of the giants in economics who did then exist, an unusually large number were walking the corridors of the University of Chicago. And the fact that they were giants of different views, varieties and vintages only increased their impact on us lesser beings.

POSTSCRIPT

In view of my work, especially in recent years, on the history of macroeconomic thought – for which the careful examination of texts is of course the basis – this account of my training as an economist would not be complete without some reference to the studies that I carried out at a Yeshiva (Talmudic academy) for many years, and indeed up to receiving my B.A. So let me first note that a minor manifestation of the interrelationship between these studies is the explicit reference to the Talmud in my discussion at the beginning of chapter 13 of *Money, Interest, and Prices* to the problematic aspects of the concept of involuntary unemployment. In a footnote to this discussion, I re-

¹⁴ But so have the naive models, to which (by virtue of their ignoring structural relations) the present-day autoregressive integrated moving average (ARIMA) models correspond.

¹⁵ With the notable exception of the impressive developments with respect to the empirical consumption function.

ferred to the Talmudic dictum that in certain cases of private law where the consent of an individual is required (e.g., in certain circumstances where the court decides that a husband – who by Jewish law is the only one who can grant a divorce – must grant one to his wife) the court is permitted “to coerce him until he says ‘I am willing’”.

There is, however, an implicit reference in the opening sentence of the book, “Money buys goods, and goods do not buy money” – which led into the following sentence, which described the Leitmotiv of the book: “The natural place, then, to study the workings of monetary forces is directly in the markets for goods”. This was an allusion to the Mishnaic statement in the fourth chapter of *Bava Metzia* that begins with “Gold acquires silver, and silver does not acquire gold”.¹

It may be that my references to the Talmud in these and other writings (see, e.g., the opening paragraph of my 1978 article) have led some commentators to describe my way of interpreting texts as “Talmudic”, and I do not think that they have meant it as a compliment (see Gerrard 1991, p. 283; Littleboy 1990, pp. 16-17). (Nor do I think that they have ever seen a page of Talmud.) If by “Talmudic interpretation” one means an interpretation based on the careful reading of texts, then I plead guilty. That is precisely what years in a Yeshiva instills in you: the patience to spend hours reading and rereading a difficult text in the Talmud, paying attention to its context, and to the nuances and minor differences of phrasing that provide clues to its meaning. And I would regard as a major influence of my Yeshiva experience that it instilled in me the confidence that if I were patiently to read and reread a text in economics in this way, I would eventually succeed in penetrating to its meaning, in understanding the intent of its author.

On the other hand, my Yeshiva experience left me impatient with the far-fetched contentions that the Talmud frequently resorts to in its attempt to show that there are no inconsistencies between the legal views expressed on different occasions by some *tana* or *amora*.² Indeed, my increased annoyance with these attempts left me receptive to the opposite belief that man is frequently an inconsistent animal who does not realize the full implication of what he says – a belief that I have on more than one occasion applied in my work on the history of economic thought (see above, footnotes 9 and 10, and the text to which they are attached).

But in addition to the aforementioned general influence of my Yeshiva experience on my way of reading texts, there was also a specific approach that I

¹ What this means is that, unlike, for example, Western commercial law, payment of money does not consummate the transaction, but rather the acquisition of the good in question. Correspondingly, the gold is the object of the purchase and the silver the means of purchase, which is why the acquisition is not completed until the consumer takes possession of the gold.

² Early and latter first century interpreters of the *Mishnah*.

learned from the rabbi in whose class we began to study the Tosafot (annotations to the Talmud). The authors of the Tosafot would pose a question that would supposedly undermine Rashi's³ interpretation of a certain passage in the Talmud. So the rabbi would then say: “Even Rashi knew how to learn [Talmud]”. Our task as students was incomplete until we not only understood the words of the authors of the Tosafot, but also Rashi's way of thinking which gave rise to the interpretation. In retrospect, it seems to me that I undertook this approach when, not satisfied with the conventional dichotomy (see above), I tried to explain (in chapter 8 of my book) the way of thinking that brought economists to this invalid conclusion.

So much for my formal education at both University and Yeshiva. I would, however, like to add that I also regard as an important part of my training as an economist my first few years at the Hebrew University of Jerusalem. For there were then only two other teachers of economics – Alfred Bonn , who specialized in the economics of the Middle East, and Edmund Silberner, in economic history. So in those first years, I of necessity taught most of the courses of the usual undergraduate curriculum in economics. And this came after only one year at the University of Chicago prior to my emigration to Israel when I had taught undergraduate courses in statistics and mathematics for economists. There is no better way to learn about a subject than to teach it. And this wide range of teaching in the early stages of my career provided me with a broad foundation in economics that stood me in good stead for many years.

Yet another beneficial aspect of my first years at the Hebrew University was the exceptional quality of the students, several of whom were within a few years to become my colleagues in the department. They were older students, indeed almost my age, whose long military service – in World War II or in the Palmach and Hagannah, followed by service in Israel's 1948 War of Independence – had repeatedly forced them to defer or interrupt their studies. So by natural selection, those who persevered despite these long delays were the better and more highly motivated ones. And when they were finally freed from their military duties and able to come to the University, they did so with a hunger for learning and with a single-minded desire to satisfy it. With such students, one learns as well as teaches. Indeed, they were the guinea pigs on whom I tried out a typescript draft of *Money, Interest, and Prices* in the graduate course in monetary theory that I taught in 1954 and 1955, and from whom (as indicated in the Preface to the published book) I received many valuable comments.

As academics, we generally distinguish between our formal training ending with our Ph.D. and the informal educative process (like that described in the preceding two paragraphs) with which we continue afterwards as an integral

³ Rashi was a world famous 11th century commentator of the Talmud. The authors of the Tosafot were a group of commentators a century and more later, many of whom were Rashi's pupils and descendants.

part of our professional careers. In my case, however, I would include as part of my formal training that year, almost a decade after my doctorate, in which I sat at the feet of Simon Kuznets at Johns Hopkins trying to prepare myself for the responsibility of assuming in 1956 the directorship of the Falk Institute for Economic Research in Israel, the establishment of which Kuznets had recommended to the Falk Foundation in the United States. It was a year that I spent regularly attending Kuznets' seminars and taking notes like any other student: learning that statistical measurement – and the measurement of national income in particular – was not an end in itself, but a means to the end of analyzing economic phenomena and that of economic growth in particular; and, above all, becoming acquainted with a new kind of economic history – not only with what is now called cliometrics (of which Kuznets was a pioneer), but with a history in which with a breathtakingly broad and imaginative sweep Kuznets drew an exciting picture of the complex interactions of economic, demographic, technological, social, as well as (and perhaps above all!) cultural factors which determined the economic development of nations. In my many discussions with him in the years that followed, Kuznets also instilled in me the recognition that in carrying out research in the history of economic thought, one should ask himself what facts about the economy were available to the economists whose respective writings were being studied. And it was this principle that later led me to undertake my 1976 study on the interaction in the 1930s between the revolution in macroeconomic measurement (i.e., the development of national-income accounting) and the revolution in macroeconomic theory (i.e., Keynesian economics).

And so I am proud to consider myself as a student not only of Frank Knight, Jacob Viner, Henry Simons, Jacob Marschak, and Oskar Lange, but of Simon Kuznets as well.

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