Reflections on My Harvard Experience, 1972-1977

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Introduction and Plan of Project

This is the first installment of what I hope to be a series of reflections on my professional experience, a sort of intellectual autobiography. Upon retirement in January 2024, I had planned to read through all of my professional writings and provide a guide to them for my kids. I started with my thesis, "Unemployment, Distribution, and Capacity Utilization on Equilibrium Paths," 1978 (252 pages), which I had been meaning to re-read for nearly 50 years, to see what I was thinking at the beginning of my career, and, subsequently, what twists and turns my intellectual journey had taken. And that still is my plan, but as I started to write a guide to my thesis, I realized I needed to back up and provide context by recounting and reflecting upon my Harvard experience that led up to my thesis. Thus, this piece describes my overall trajectory, 1972 – 1977, along with recollections and anecdotes of some of my most memorable professors, concluding with an overview of my thesis (and an appendix with chapter-by-chapter summary, with links). I also discuss the studies I undertook outside of the classroom – reading group with classmates, teaching and research assistance, travel, self-study, and a semester at Cambridge University. I include a reflection on the Jewish prominence in economics at that time, and how I experienced it. Throughout the recollection, I provide an account of my engagement with radical political economics and how that evolved, which segues into my transition to the radical department of economics at UMass/Amherst in 1977 – the subject of my next installment. I close this memoir with a tentative outline of the sequence of memoirs to come.

Timeline

Fall 1972: Enter Harvard, after graduating University of Michigan.
    reside at 81 Orchard St., N. Cambridge, until leaving Fall 1977
Fall 1972 – Spring 1975. Coursework, Research Asst, Study Group, etc.
Written Theory Exam, February 1974; General Oral Exam, May 1975
Fall 1975: travel in Southern Europe, reading classical economists
December 1975 – January 1976: Israel, including Kibbutz Merhavia
Spring 1976: Cambridge University, UK. Attend classes; present at Frank Hahn's seminar
Fall 1976 – Spring 1977. Teach and work on thesis, in residence at Harvard
Fall 1977: Move to Northampton to take up my appointment at UMass, Spring 1978
May 1978: Defend my thesis and receive my Ph.D. from Harvard.

Caveats

This account is based solely on my recollections, 50+ years after the fact, including some context of my life and the world. It does not draw on my notes, drafts, or communications during my Harvard years; such materials may be found in storage, but I have not dug them out. Perhaps at some future time, I could dig them out and maybe find that my recollections are false, misleading or even self-serving! So, with that caveat, here are my general reflections.

My Overall Trajectory at Harvard

I entered Harvard in 1972 with a decided iconoclastic bent, coupled with a love for the rigorous methods and temperament of academic economics at that time. More specifically, as an iconoclast, I was keen on challenging the neoclassical orthodoxy, especially since it *could* be taken as showing the efficiency (and, in naive interpretations, the fairness) of a competitive market economy,[1] and I considered myself something of a "democratic socialist" back then.[2]

And yet, I was also a great admirer of the rigor – indeed beauty – of the neoclassical edifice, an axiomatic logical system, based on the rational actions of individuals under constraint and the general equilibrium conditions that reconciled the optimizing actions of disparate agents and ratified the constraints each agent faced. I would say this latter appreciation and orientation grew during my time at Harvard, as I learned from some very powerful intellects, discussed below.

One key aspect of my Harvard experience that I grew to especially appreciate in retrospect, was the *freedom to explore* that I was accorded at Harvard. I do not believe that would have been true at some other graduate programs. I say this not as a criticism of other programs (and, conversely, many would not have viewed this as so admirable about Harvard), but simply as a statement of what made the best fit for me at the time, given the state of my intellectual maturity.

Specifically, I had been accepted at Harvard and MIT in 1972 and had to choose between them.[3,4] MIT had arguably eclipsed Harvard by then, to be the world's most highly rated econ

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[1] To be sure, the interpretation of neoclassical economics as the handmaiden of free market apologetics was always very superficial. My thesis supervisor Kenneth Arrow, who provided the most rigorous development of general competitive equilibrium theory, was himself a sophisticated critic of free markets in many respects (notably health economics) due to the many forms of market failure (i.e. deviations from the very demanding assumptions of competitive equilibrium, including incomplete markets for risk and future goods). Although I was aware of this at the time, I acquired a greater appreciation over my career of how the caricature of neoclassical economics was misguided. For example, as I later explained, the marginal productivity theory of wages – a logical implication of profit-maximizing employment decisions, or even of simple cost-minimization -- in no way implied the result was "fair," as commonly misunderstood by both friends and critics of neoclassical theory (Costrell, 2001, "Discipline-Based Economics Standards," note 25, in Sandra Stotsky, ed., What's at Stake in the K-12 Standards Wars?).

[2] This was a term that had a less radical meaning at the time, as compared to today's Democratic Socialists of America. It was basically anti-capitalist but distinguished one from the revolutionary Marxist left. Although it was somewhat sympathetic to aspects of Marxian analysis, the focus was less on class struggle than on "socialization of investment." This is briefly considered in my thesis (p. 134), and my early publications thereafter, as a possible implication of my formal analyses. I continued to label myself a "democratic socialist" into the early 1980s.

[3] I had also been accepted at Yale, but for various reasons decided against that department early on (I didn't like New Haven and I thought their chapter of the Union for Radical Political Economy was too orthodox in their Marxism). These were the only three programs I applied to. I chose not to apply to Chicago (a department that came to boast a huge number of Nobel laureates), as it was too conservative for my taste at that time. I later become more impressed with the vigorous (some would say aggressive) intellectual climate of that department, as I learned from my dear friend and mentor at UMass, Leonard Rapping, who came out of that department.

[4] I remember well the day the acceptances came in the mail to my house in Ann Arbor. They were due to arrive on April 1, so I came down from our upstairs apartment to wait on the front porch for the mailman. As he approached, the dog who lived in the downstairs apartment rushed out to aggressively threaten the mailman, so he (understandably) skipped our house that day! The next day, I made sure the dog was controlled and got the acceptance letters. My housemates also did well that spring – one got accepted to Yale medical school, another won U Michigan's prestigious Hopwood Award for creative writing. We celebrated by going out to one of Ann Arbor's

program (I believe they eventually claimed more Nobel Prizes). Moreover, Harvard's econ program at that time was known to provide less intensive supervision than MIT – commonly viewed as a shortcoming. The faculty were reputed to be more interested in their research than in their students, except insofar as their students could serve as effective research assistants.[5] As my classmate Luis Alvara Sanchez aptly recalls, each faculty member had his own little empire, so there was less of a "Harvard school" of thought than, say, Chicago or MIT. The course requirements were also less prescriptive. To me, Harvard's laissez-faire and eclectic milieu seemed an advantage, as it could give me more freedom to pursue my iconoclastic goals.[6,7]

There were several ways in which I exploited this freedom while, at the same time, learning what I could from the brilliant professors and talented fellow grad students. Before providing details, there are two general points I want to make about my grad school experience:

First, I was not a star by any means. Of course, all the grad students had been star undergrads, whence they came, but could not all be stars when brought together at Harvard. My performance in class was generally quite middling – I was essentially a B+/A- student[8] (see "Costrell official transcript, Harvard University"). Naturally, this was a difficult adjustment, but such is the lot of most first-year grad students. The standard method of coping is to try and carve out some specific area where one might excel, to be a big fish in some small pond. For me, that tended to go along with my iconoclastic leftist orientation – I would never be able to compete with the top mainstream economists (as did a number of fellow grad students, particularly from subsequent classes, which were generally stronger than ours), but maybe I could stand out as a radical economist. To jump ahead, surely this factored into my excitement at joining the radical faculty at UMass/Amherst, instead of seeking an appointment in a mainstream department.

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fanciest restaurant – then and even now – the Gandy Dancer. Since we all lived on a shoestring (e.g. tuna casseroles and Velveeta cheese for our communal dining), this was a big deal. I had lobster for the first time in my life, paired with a fancy French wine – Pouilly Fuissé, as I recall.

[5] I had such an experience with Martin Feldstein. He was Harvard's leading macroeconomist at the time, so I proposed a macro research project of my own design to draw on his funding. He had his secretary give me a curt note that it would be unfair to his other students for him to fund me to do my own work instead of his.

[6] Interestingly, I was reminded of this contrast recently, reading Glenn Loury's memoir. He was also choosing between Harvard and MIT that same year and chose MIT because of its more intense mentoring. He made the right choice for him, based on his mathematical brilliance, superbly cultivated at MIT by his mentor Bob Solow.

[7] There were additional reasons I chose Harvard. I had been rejected at Harvard as an undergrad, so this was a chance to compensate for that – not a particularly admirable or academically sound reason, to be sure, but probably served a psychological purpose. Perhaps a better reason, at that point of my intellectual journey, was that Sam Bowles and Herb Gintis were there, two luminaries of radical economics. They didn't teach graduate courses at the time, but I got to know them through the Union of Radical Political Economics (URPE). They didn't stay long, as neither of them was awarded tenure, and both migrated to UMass/Amherst around 1975 as key figures in transforming that department into a radical mecca. I was thrilled to have an opportunity to join them in 1977 and got to know them much better as a colleague there for over 20 years. More on that in future recollections.

[8] My first-year grades were 2 B+ (in micro and macro theory), 3 A- (econometrics and 2 semesters of economic history), and 1 A (in the statistics department). Second year, I had an A in the economic history seminar, but no other courses were taken for grades -- audit only; Harvard had almost no course requirements, but one had to at least register for the required number of credit hours under the auspices of a faculty member (pro forma or otherwise).

Second, I transitioned at Harvard from empirical to theoretical economics. This was the big dividing line among economists at that time. Theory held the most prestige – theorists were the high priests of the discipline.[9] They were the ones who won the Nobel Prizes. And rightly so, in my opinion. They were the deep thinkers: Arrow, Samuelson, Solow, et. al. I did not believe I had potential in theory, and I initially considered my path to be empirical.[10] I had become enchanted with econometrics as an undergraduate at Michigan, with the modest goal of using econometrics to poke holes empirically in the neoclassical paradigm.[11]

At Harvard, I continued to think econometrics would be my path – it was one of the two special fields I chose for my qualifying exams. I was a research assistant for Ed Leamer (a Michigan protégé of my undergrad mentor Bob Stern, who recommended me to him). Ed was a brilliant and iconoclastic econometrician – a Bayesian. His most famous paper was (and remains) "Let's take the Con out of Econometrics," a paper that did not ingratiate him with the field.[12,13] More to the point of my intellectual path, what I learned from Ed made me more cynical about standard econometrics; if I was to do econometrics, it seemed the only honest way to do it would be Bayesian – a major undertaking, especially as it was not fully developed or accepted.

At the same time, Steve Marglin's course on alternative theoretical models of growth and distribution (more on that below), together with my own work (some of which came out of Marglin's course), helped give me confidence that perhaps I could become at least a middle-brow theorist (as I came to identify myself). And with that, I approached Ken Arrow – the paragon of economic theory and a true mensch to see if he might be willing to supervise my dissertation.[14]

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[9] This changed dramatically over the latter part of my career, as empiricism without theory became dominant to the near exclusion of theory, an unfortunate development in my opinion.

[10] My first semester at Harvard, there was no suitable econometric course available, so I took a course in the statistics department on linear models from the legendary William G. Cochran. As I recall, he was known as the Grand Old Man of the Linear Model. We did ANOVA's by hand, to better understand them (desktop computers were still in the future). He was a very charming old school Scotsman. The one takeaway that stuck with me over the decades was the proper pronunciation of '~', the notation placed above an estimator like β and commonly referred to as 'β-tilde.' He always referred to it as 'β-*twiddles*,' which I took as authoritative, given the source.

[11] A key episode in my undergrad experience was a guest lecture by Barry Bluestone in the junior honors colloquium of my mentor Bob Stern. Bluestone was a grad student at Michigan, active in URPE – an organization I joined. Barry was an empirical economist, and I was struck by his demonstration (as it seemed to me at the time) that one could use conventional econometrics to make radical points that contradicted neoclassical economics. This became what I wanted to do. (To jump ahead of the story, much later in my career, pursuant to some empirical work that I did in 1988 – which played a small role in the Dukakis presidential campaign – I found that Barry's methodology in related work was highly misleading, resulting in quantitatively exaggerated results. I later published a paper critiquing it. But this is a story for a later chapter of my reminiscences, with more juicy details.)

[12] Ed was denied tenure at Harvard and has spent an accomplished career at UCLA. His denial of tenure always struck me as unwise, as well as unfair. It also suggested to me that Harvard's conservatism was not strictly political (as was suspected in the denial of tenure to Sam Bowles, and, to a lesser extent, Herb Gintis); Harvard was *scientifically* conservative – they did not want to take a chance on Bayesian econometrics. For a very nice WSJ column celebrating Leamer's career (following his passing in 2025), see here.

[13] Fast forward many years, to 2023, I used that paper to great effect in expert testimony for a school finance trial in New Hampshire to debunk the sloppy and dishonest work of the expert on the other side.

[14] I will explain below why I did not ask Marglin.

But this gets ahead of my story. Let me back up and sketch some of the profs from my first years at Harvard who stand out in my memory, even though they may not have had a profound impact on my thesis or later work. I also want to discuss some of the studies I undertook outside the classroom (reading group with classmates; solo reading while traveling abroad; and a semester at Cambridge, UK) and also some teaching, before buckling down to write my thesis.

Memorable Profs

Zvi Griliches

Perhaps my fondest memories are of Zvi Griliches, a favorite of several of us grad students, particularly the Jewish ones. Zvi had a remarkable history – born in Lithuania in 1930 and sent to Dachau by the Nazis. After being liberated from a death march at the end of the war by Patton's 3rd Army, he made his way to Palestine in 1947 as an illegal immigrant and fought with the Haganah. His story was said to be similar to that of the Paul Newman character in *Exodus*.[15]

After studying agricultural economics in Israel, he came to UC Berkeley and then U of Chicago for grad school and joined their faculty as one of the great econometricians of his generation and then moved to Harvard (I believe it was because one of his sons needed special education that was available in MA). Known for an intuitive understanding of how to interpret (and not over-interpret) data, he made his mark studying the econometrics of productivity growth. He liked to critique the temptation to "torture the data until they confessed." The math and statistical theory he used was not the most hyper-powered style that was coming into mode for advanced econometrics, but he had and conveyed a sense of understanding the data and a classic appreciation for the pitfalls of omitted variable bias. He was also personally very approachable (unlike many other faculty). The love between him and his students was palpable.

Although his specialty was econometrics, my first course with him at Harvard was first-semester microeconomic theory, as he filled in for a colleague on leave who usually taught it. He used Henderson and Quandt, a solid text, but not as abstract and high-brow as ones that later came into common usage – probably a good thing for me. A few stories I remember from that course: (i) he had scribbled some equation on the board which we could not read, so someone asked him to write larger; his response was that he had always maintained that writing small was the written counterpart to mumbling. We found this hilarious, since he also tended to mumble at the blackboard. (ii) about halfway through a lecture full of equations, he paused to think: "OK, let's see, what is some more algebra I can inflict on you?" (iii) a mid-term exam included a paragraph from John Kenneth Galbraith's book *New Industrial Society*; the question asked us to critique Galbraith's claim, using microeconomic logic. Since Galbraith was a faculty colleague of his (and a former icon of mine from my undergrad days[16]), I found this striking: it really was the

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[15] For more details of Zvi's experience in the Holocaust, DP camps, and immigration to Palestine, see here, and especially his short interview on his liberation (drawn from a longer interview, not available online).

[16] I had been quite taken with his book *The Affluent Society*, and its critique of advertising. My undergrad thesis was an econometric analysis of the impact of advertising. So I expected to gravitate toward him at Harvard. He did not teach in the graduate program, but convened evening sessions where he would bring a couple of faculty members of

economics temperament at its best; not rude or personal but probing and challenging. "Defend your position," was certainly the famed tenor of U Chicago seminar rooms, and Zvi had brought that from Chicago to Harvard. Sadly, in recent years, that professionally adversarial approach has come under attack in economics as being insensitive and intimidating.

I had more interactions with Zvi as a student in his econometrics course the following year, when I really came to appreciate his intellectual depth, and possibly as a research assistant at one time or another. If I had continued to focus as an empirical economist, I would surely have chosen to write my thesis under his supervision, as a number of my friends were doing (notably including the more leftist students, even though Zvi was not a leftist himself[17]). Many years later, when I learned he had aggressive pancreatic cancer I visited him at his home. His project at that time was tracing his Eastern European Jewish genealogy. I called again later on (when he was no longer taking visitors) and attended his memorial service at Harvard's Sanders Theater.

Janet Yellen

Janet Yellen, who served as Secretary of the Treasury decades later, was another favorite of ours, again, especially among Jewish students, as she was quite demonstrably (accent-wise) a nice Jewish girl from Brooklyn. She was a fresh Ph.D. out of Yale, where she had been a student of prominent Keynesian (and later Nobelist) James Tobin. She was close in age to us grad students, so I think we may actually have called her by her first name (we certainly did so among ourselves). I remember she gave me the latitude to answer an exam question using the writings of a somewhat heterodox Keynesian of the era, named Axel Leijonhufvud, which I appreciated. I very much wanted to get an A or A-minus in her course but only got a B+. She was later denied tenure at Harvard, possibly because she was a Keynesian and the macro group at Harvard was headed by Martin Feldstein, who was not. Much to our surprise, she then left academia to go to the Fed. Although she later returned to academia at Berkeley, clearly her early Fed experience prepared her well for her later positions at the Fed and Treasury.

I have to say, I was disappointed at her inability to resist the politicization that came with being a political appointee as Treasury Secretary; Larry Summers (who was a grad student a few years behind me at Harvard) also held that position, but resisted the politicization a lot better, in my opinion. I liked Janet a lot more than Larry back in the 1970s (when he was considered a conservative student of Feldstein and of course had the prickly personality that he later became famous for), but I had more respect for Larry later on, especially as President of Harvard.

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different views to have a genteel debate, which was the way academia should be. That said, he was somewhat pretentious (albeit in his charming manner) and, as my education continued, I concluded that his gifted and entertaining style of writing was not a substitute for rigorous analysis – as Griliches' test question implied.

[17] To be sure, Zvi was never overt about his political views. In his younger days he had been in Hashomer Hatzair, the leftist Zionist group from Europe that founded many of the socialist kibbutzim, including the one he worked on.

Alexander Gerschenkron

One of the required courses for first-year grad students was Economic History (two semesters). This sequence was taught by a very memorable prof, Alexander Gerschenkron. Gerschenkron was very old school, both in style and content. He was an immigrant with a pronounced accent, which I thought at the time was Russian. I later learned that he came from a Jewish family in Ukraine but moved with his father as a young man to Vienna, where he took his degree. He had a fascinating history (for short version look it up in Wikipedia), which was later chronicled in an outstanding book by his grandson Nicholas Dawidoff, *The Flycatcher*.

Back to my experience with him. He memorably began his course by reading the papal call to join the Crusades, emphasizing the economic opportunities (land) that it would provide the impoverished peasants of Western Europe. I later learned that he was translating from the original Latin as he read it. This is the type of scholar that he was. When one would visit him in his office, packed with very old books (unlike the offices of more modern economists), he had a bottle of Remy Martin cognac – very old school, indeed. And yet, one time when I came in with my motorcycle helmet, he asked what kind of cycle I had; he knew about motorcycles from an early phase of his career, working for a motorcycle firm in Vienna, before entering academia.

He had high standards and was somewhat imperious, but caring at heart. He was known to be conservative (I learned later that he had stood up to the student radicals of 1968 at Harvard) but was open and encouraging to a young leftist like me (as I was at the time).[18] Prior to my thesis, the paper I was proudest of writing during my grad student years was a long paper on the fight for the 8-hour day (it should be somewhere in my storage boxes). I tried to combine historical analysis (not necessarily my strong point) with an attempt at theoretical innovation, integrating power into an economic model. (This was a focus of my thinking in those days.) I worked very hard on it and was in danger of missing the deadline, so I asked for extension. As I recall, he granted me a short extension, but made no promises on whether I would be penalized for it. He memorably told me, "Fear is a great teacher," a line I later repeated to students (tongue-in-cheek) throughout my career. The new deadline was on a Saturday morning, and I recall pulling an all-nighter (not that unusual for me in those days) to finish it up and then taking the bus to Harvard and slipping it under his door. He liked it a lot. As a result, he invited me to join his second-year seminar, which I did (fulfilling the requirement to take a second-year seminar).

This was in the last years of his storied career, and I don't believe he was getting any grad students to work with him – the profession had moved away from old style economic history, with the emergence of cliometrics (econometric economic history).

As to Gerschenkron's impact on me, I can't say that he had any substantive impact on my economic thinking (aside from the freedom I had to write the 8-hour day paper with my attempt at theoretical innovation). But his persona certainly stuck with me, serving as a model of scholarly excellence and of old school standards of conduct and expectations.

[18] Interestingly, in writing this up, I ran across an article showing that he was a committed Marxist in his younger days, but kept that secret upon emigration.

Steve Marglin

Foremost among my key influences at Harvard was Steve Marglin. Marglin had been a wunderkind of microeconomic theory, tenured at Harvard at an early age. By the time I arrived at Harvard (1972), he had turned his attention from microeconomics to radical political economy.[19] In the spring of 1975, he offered a highly original course, combining his iconoclastic and theoretically rigorous orientations: "Neo-Marxian and Neo-Keynesian Theories of Growth, Distribution, and Fluctuations." I learned a lot from this course.

One specific lesson for me was the centrality of growth models to the determination of the equilibrium rate of return on capital (and, hence, the distribution between wages and capital income).[20] More on this below (in discussing my thesis), but the key takeaway *for me* was that in steady state the rate of return on capital must adjust to equilibrate the growth rate of capital with the steady-state growth rate of effective labor (taken as exogenous). This condition fully determines the rate of return on capital, independent of any Marxian model of bargaining power.

More generally, I learned from Marglin that simple models could generate great insight (such as the insight I just mentioned) and would quickly become complicated enough (as the extensive math in Chapters III and IV of my thesis illustrated).

I chose the subject of his course to be one of my two fields for my oral exams in the spring of 1975, and Marglin was the examiner in that area. (My other area was econometrics, and I had Leamer as my examiner for that). Marglin asked me a very good question about Goodwin's Marxian model of real business cycles: why aren't the cycles damped? I had no good answer, but came back to that question a few years later, when I wrote Chapter IV of my thesis.

Marglin would have been a more natural choice to chair my thesis than Arrow, based on the nature of his work, but I knew him to be personally quite difficult, so I did not ask him to be chair or second reader (that was James Medoff). At the final stage of my thesis, I needed a third reader for the defense, and he was the natural choice. At this point, I was already teaching at UMass in the days before my defense (May 1978) and I got a call at the office from Arrow. Marglin had contacted him after reading my thesis and claimed that I had appropriated his work in my Chapter III, a critique of the life cycle model of aggregate saving based on its stability properties. Arrow asked me about it, and I explained that my contribution in Chapter III was certainly related to the model Marglin presented in class but added a dimension. Specifically, as I recall, Marglin had analyzed existence and uniqueness, but not stability – at least not as of the time of my work. I then examined the paper trail of who had done what and when and convinced

[19] His big paper at that time was "What do Bosses Do?", an institutional Marxian analysis of hierarchies in production. However, what I learned from him was not in that vein, but from the theory course discussed in the text.

[20] Growth theory also led to the so-called Cambridge Controversies over Capital, which pitted Cambridge England economists (Robinson, Kaldor, Pasinetti, et. al.) against Cambridge Massachusetts (especially MIT's Solow and Samuelson). As indicated elsewhere in this memoir, I was fascinated by this controversy, which had been viewed as striking at the heart of neoclassical economics. It led me to spend a semester in Cambridge, and I continued to be engaged with it into my early years at UMass. Eventually I concluded it was a lot of sound and fury about very little, nitpicking with a simplified production function used for growth theory, but leaving the full general equilibrium model of neoclassical theory untouched.

Arrow that I had duly acknowledged Marglin's work and that my own contribution was distinct from his. (My UMass colleague Sam Bowles, formerly a close colleague of Marglin's at Harvard, told me that Marglin was well-known to have an expansive interpretation of his intellectual property.) That said, I was certainly nervous in the run-up to my defense, fearing that I might face an unpleasant encounter with Marglin. As it happened, when I showed up for the defense, Marglin had been sidelined with a recurrence of malaria (acquired in India some time earlier) and had been replaced by James Duesenberry, a kind senior economist who had barely had time to read the thesis, let alone take issue with any of it. So all went well.

Ken Arrow

Ken Arrow has been viewed by many as probably the greatest economist of the 20th century, but with none of the arrogance one might associate with other contenders for that title. As his nephew Larry Summers aptly labeled him in his wonderful remembrance, Arrow was a "gentle genius."[21] Arrow did not have many students who worked under him at Harvard, but they were the cream of the crop – future Nobel Prize winners themselves. There really was no good reason for him to take me on, as my work was not only of a decidedly lesser caliber, but also generally unrelated to his own. Nonetheless, much to my pleasant surprise, he graciously agreed.

I had previously sat in on a class he taught (I think it was economics of information) and, in addition to seeing a brilliant mind at work, I was also privileged to witness his legendary skill at twirling a piece of chalk in the air and effortlessly catching it. I had also studied on my own his canonical treatise, *General Competitive Analysis* (with Frank Hahn), which drove home to me the compelling beauty and rigor of general equilibrium theory, along with the strategy of modeling departures from its stark assumptions. This really was a model for me, albeit at a much higher level of abstraction and technique than I could ever reach. Together with the simpler models I learned in Marglin's class, I had a basis for working out my own approach to an integrated model of unemployment, distribution, and capacity utilization (the title of my thesis).

When it came time to write my thesis, I did not have frequent contact with Arrow – perhaps half a dozen meetings – but he gave me free rein and respect, which I will always appreciate.[22] The one exception to his light hand was to provide a key elegant proof, for Chapter IV, replacing the clunky proof I had labored over for weeks. What I tried to prove in several pages of equations, he showed me how to do in a couple of lines, using a method due to some Russian mathematician. This was my most personal experience of Arrow's gentle brilliance.

In the end, Arrow apparently liked my thesis, and that's what counted. He continued to be gracious to me after I graduated, calling me a year into my position at UMass. He called to see if I would be interested in having him recommend me for a job in a mainstream department. At the

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[21] Also in the shorter version published in the *Wall Street Journal*.

[22] He also generally tolerated my playful and arguably unprofessional style of writing at that time, occasionally laced with sarcasm toward the theories I was trying to debunk. Once I started writing for professional journals, that style was beaten out of me by the referees and editors, one of whom memorably called it overly "folkloristic."

time, I was very happy to be at UMass, so I declined. In later years, Arrow happily served as a reference for me, including the all-important letter in support of my tenure.[23]

Many years later, long after he returned to Stanford, I brought my daughter Sarah to meet him, on our 2002 trip to California. He graciously welcomed us to his office, which was strewn with books and papers all over; that helped show Sarah that I was not the only one with such a mess. Of course, that was not the point of the visit, but rather to meet with such a "gentle giant."

One more anecdote. While at Harvard, I would attend High Holiday services at Sanders Auditorium. One year I noticed Arrow sitting in the front row – not in itself a surprise, as I knew he was Jewish. What did surprise me, however, was that he wore a full-sized Orthodox tallis (unlike my bar mitzvah tallis), fully enveloped over his head in the distinctly Orthodox fashion. Arrow was the era's apotheosis of the power of rigorous rational thought. For example, his famous "impossibility theorem" reasoned from sparse, seemingly inarguable axioms to a profound counterintuitive result on the impossibility of voting or any other social decision-making mechanism to satisfy these axioms. Here was that paragon of reason deeply immersed in suprarational spiritual activity in the most demonstrative fashion. The sight always struck me as both a puzzle and a further source of awe and respect for the man. I never asked him about it.

A Digression on Jewish Prominence in Economics at that Time

At that point in time, Jews had risen to an extraordinary degree of prominence in economics at Harvard, and more generally. In a very real sense, economics had become a Jewish discipline.[24] In the case of Harvard, this was particularly remarkable, given its history, not so long previously, of excluding Jews. This was not only true of admission quotas, but also of faculty hiring.[25] Quite famously, Harvard refused to hire Paul Samuelson, their most brilliant graduate student, Fellow of the Harvard Society, and author of the transformational monograph, *Foundations of Economic Analysis*. So he went to MIT in 1940 and built that world-class department from scratch[26] – including many Jews who might otherwise have been good candidates for Harvard.

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[23] I recently found correspondence with him from 1993, when I was applying for jobs to leave UMass. In addition to serving as a reference (for that unsuccessful search), we exchanged views on the economics of educational standards (as I had just written my "very interesting-looking paper" on that topic). We also shared our views "about the excesses of P.C. in the modern university" and "ill-founded demands for recognizing 'diversity' through inappropriate requirements," as he put it. Note that this was 30 years before the public debate over DEI.

[24] See here for a long list of prominent Jewish economists and here for some measures of their prominence.

[25] A notable exception was Wassily Leontief, who joined Harvard in 1932 and became professor in 1946. Leontief's mother was Jewish, but he did not self-identify as such. Another exception was Abram Bergson, hired in 1956; interestingly, his name at birth was Burk, presumably an assimilationist choice by his parents. His first famous article in 1938 was signed A. Burk. A few years later, he and his brother changed their name to Bergson, to be more forthright about their Jewish heritage, even though it was still probably a liability at that time.

[26] Glenn Loury's memoir eloquently and warmly speaks about how the Jews of MIT – Bob Solow, Peter Diamond, and others – mentored him, some years later.

By the time I arrived at Harvard, that department's exclusion of Jews had very much become a stain of the past – all five of the professors I highlight above were Jewish (although I did not know it at the time regarding Gerschenkron). Many others were also Jewish (Martin Feldstein, James Medoff, Dick Freeman, Ben Friedman, Sherwin Rosen, Robert Fogel, Otto Eckstein, Abe Bergson, Robert Dorfman, Jerry Green, Harvey Leibenstein, Albert Hirschman).[27]

By the same token, many of the grad students were Jewish. Among my Jewish friends were Sandy Borins (a friendship that has lasted for decades, due to annual visits with him in Toronto), Alan Weisbard (OBM, who transferred to law school), Mark Schankerman, and Ariel Pakes. Eric Maskin (later Nobel laureate) attended Arrow's class, although we did not interact.[28] A number of Jewish students in adjacent years later became prominent economists, such as Larry Kotlikoff (good friend) and Larry Summers (acquaintance with whom I interacted periodically over the years, especially when he served commendably as President of Harvard).

One memory of Jewish life at Harvard that stands out was the alarm we felt with the outbreak of the Yom Kippur War of 1973. I remember going over to the Harkness Commons (which no longer exists), nearby at the Law School, to watch the war news on their TV (this was an era before TVs were everywhere). Our concern was great with the initial disasters, but relieved as the battle turned. In recalling that time, I am struck by the stark contrast between the atmosphere at Harvard then and 50 years later, following the massacres of October 7. I saw no conflict as a student radical in those years, in supporting Israel. Interestingly, however, I recall an exchange with Arrow in which I congratulated him for a pro-Israel statement he had made (or signed);[29] his response was one of surprise that I supported Israel, since I was a demonstrative student radical. As that exchange indicated, other radicals were already turning on Israel, and as the years went by, their hostility to Israel became complete, as I witnessed early on during my UMass years in the 1980s and 90s, and, of course, at Harvard and so many other campuses following October 7.

[27] By my rough count of the 60 faculty members listed for 1972-73, about a third were Jewish. That proportion seems to still hold today (2025), although a number of those remain from the earlier era and are surely of retirement age. Judging by casual impression of the population of grad students and young professors, I would expect the proportion of Jews to drop.

[28] Maskin was actually in the applied math Ph.D. program, although Arrow was his supervisor.

[29] In writing this up, I ran across a remarkable video of Arrow visiting Jerusalem's City of David at age 93.

Teaching and Research Assistance

As mentioned above, I was a research assistant for Ed Leamer; my transcript indicates this was during my 2nd and 3rd year.[30,31] I had been recommended to Harvard – and to Ed – by my U Michigan mentor, Bob Stern,[32] who had been Ed's Ph.D. mentor at Michigan. Ed was a great guy, pretty laid back,[33] who was then working on the mathematics of Bayesian econometrics, which made him an iconoclast for his field. My assignment was to work independently on some aspect of that, the precise nature of which I cannot recall. I did produce a manuscript for him, which was pretty technical, deriving posterior distribution of some estimator from prior and likelihood. (I believe this paper is somewhere in my files in storage.) I was pretty proud of the work, but I don't believe Ed made any use of it. His main research assistant (and collaborator) was the very talented "Dutch" Leonard. But Ed took care of me, as I was entrusted to him by Bob Stern.[34] When it came time for my oral exams in my special fields, in spring of 1975, I chose econometrics as one of my special fields, with a focus on Bayesian econometrics, so Ed was my examiner in that area.

I also did some teaching. At that time, Harvard allowed grad students to teach a course of their own design for undergrads. I designed a course on growth theory. I had only a few students (possibly only one after others dropped out?), but it was an opportunity for me to become more familiar with the topic. The text I used was Hywel Jones, An Introduction to Modern Theories of Economics Growth. I had already learned that growth theory was central to the long-run determination of the distribution of income between wages and capital, so this teaching experience helped prepare me to write my thesis.

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[30] The term "TIME" on the transcript, denotes activity in lieu of ordinary coursework, to fill the required number of hours for each term. The Leamer entries were for my research assistance. The Duesenberry entries were pro-forma, not denoting any actual activity, but simply a formality to indicate Duesenberry (then chair of the department) was responsible for my reduced course load. After returning from my year as "Travelling Scholar," the entries of Medoff and Arrow for 1976-77 and Fall 1977, indicate that I was writing my thesis under them.

[31] I served in minor teaching assistant roles for other faculty. The one I recall was serving as computer assistant for the microeconomics course taught by Dale Jorgenson. I may also have done some work for Zvi Griliches; I can't remember what it was, but I do know that I had a warm relationship with him, as described above.

[32] Some years later, my family was at a DC museum the day after Thanksgiving, and we ran into Bob Stern, so I introduced him to my parents. My mother got very emotive, thanking him effusively for "saving my son from the gutter." Bob, who was a very mild-mannered man, was amused, and insisted I would have been OK anyway. I maintained a periodic relationship with Bob over the years. I attended a festschrift conference for him in Ann Arbor in 1996 or 1997 – the only undergraduate student of his, along with his many accomplished former grad students, including Ed, who was one of the festschrift's editors. As I recall, I submitted a recollection of him from an undergrad's viewpoint (I don't think it was published). I last contacted him in 2010, when visiting Ann Arbor with Ben, to see if we could meet him; by then, however, he had retired and moved to Berkeley to be near family.

[33] I remember one time I came to Ed with the completion of some work he had asked me to do, and I asked him "what next?" His answer was, "I don't know. Let's go play some golf."

[34] In previous years, Bob had sent Mike Kennedy and Dick Kopcke to Harvard, also under the care of Ed, if memory serves. The year immediately prior to me, Bob sent Mike Aho to MIT. After these 4 years of sending students to Harvard or MIT, I don't believe anyone was accepted from Michigan for a while – my hunch was that this was in part a consequence of a change in admissions director, from James Duesenberry (who held a Michigan Ph.D., so maybe had a soft spot for Michigan grads) to Zvi Griliches, who was more rigorous (see here).

Reading Group, Travel and Self-Study, and a Semester at Cambridge University

History of Economic Thought was not a priority at Harvard (or other top grad programs), but I was interested in it. I had taken a course in it at Michigan from Dan Fusfeld. As I recall, there was an elective on it at Harvard which I sat in on, by Robert Dorfman,[35] but most of my immersion in the subject was in independent study. A few classmates (my dear friend Luis Alvaro Sanchez, Phil Aranow, and visiting assistant prof Tsuneo Ishikawa) and I had a reading group to work through Marx's *Capital* (Volume I and some of Volume III), with an eye to see how much of it could be modernized. We considered closely (i) Marx's "transformation problem" from Labor Theory of Value to a model of prices; (ii) Marx's doctrine of the falling rate of profit from rising organic composition of capital; and (iii) Marx's theory of the immiseration of workers. Ultimately, we concluded that none of these held up under modern mathematical analysis. The one piece from Marx that I did credit was his idea of the "reserve army of the unemployed," which was simply florid language for the downward (or restraining) impact of unemployment on wages – a concept easily reconcilable with standard economic theory, which I incorporated into my thesis.[36] We also delved into 20th century iconoclasts from Cambridge – Keynes, Kalecki, Kaldor, Robinson, Sraffa, and Pasinetti, intrigued as we were by the so-called Cambridge Controversies over Capital, mentioned in a note above.

More importantly for my study of the classical economists, I took a year away from Harvard (1975-76), traveling in the fall in Europe (from England to Greece), during which I immersed myself in Smith, Ricardo, and Mill – I believe I have notebooks from that period. I was particularly taken with Ricardo, who wrote in prose, but clearly thought mathematically. I fondly recall reading Mill on my hotel balcony on the south coast of Crete (Hora Sfakion), overlooking the Mediterranean. I then traveled and worked on a kibbutz in Israel (Merhavia) for about 6 weeks before flying back to England to spend the spring at Cambridge University.

My semester at Cambridge University, immediately prior to writing my thesis, was a bit like going to Mecca.[37] I was basically an academic gate crasher but used my Harvard status to get library and auditing privileges there (my Harvard transcript gives my status for 1975-76 as "Travelling Scholar," which also allowed me to continue taking student loans). I met Robinson, Sraffa, and Pasinetti there – my heroes at the time, due to the Cambridge Controversies. I remember literally sitting at Robinson's feet at some over-crowded seminar. I took a picture of Sraffa riding his bike at the age of 78, which I still have somewhere. I believe I also audited a

[35] Dorfman had been co-author with Samuelson and Solow of the influential volume on linear programming affectionately known as DOSSO. One memory of Dorfman that stuck with me was his wry observation that when Samuelson enunciated his famous "Correspondence Principle," he didn't bother to tell anyone exactly what the correspondence was. (It was an imprecise correspondence between second-order conditions for a static optimum and stability in a corresponding dynamic system, as I wrote up for a teaching note at UMass.)

[36] Alternatively, some interpreted the argument as the impact of unemployment on worker effort.

[37] There were others for whom it was also Mecca. Early on, I heard a familiar booming voice in the halls there and it turned out to be my old Michigan prof, Dan Fusfeld, also a leftist economist. So I spent time with him and his wife that spring at their rental house.

course by Pasinetti and communicated with him after returning to Harvard. I also audited a course by Adrian Wood, on his new book *A Theory of Profits*, which I found underwhelming.

At the same time, however, I attended Frank Hahn's very neoclassical seminar at Cambridge and presented a paper there which became Chapter III of my thesis (and also the basis of my first publication, a much-shortened version in the October 1981 *Review of Economic Studies*, a highly regarded journal).[38] Hahn was Arrow's co-author on the definitive treatise *General Competitive Analysis*. I can't remember whether I had asked Arrow to be my supervisor before or after my semester at Cambridge, but I probably used my connection to him to get into Hahn's seminar and present there. I have a vague recollection that Hahn communicated to Arrow (in good humor) that Arrow had sent him "one of these young radicals," or something to that effect. The fact that I managed to present at his seminar is noteworthy in retrospect, that I wanted to pass muster with rigorous theoretical economics.[39]

Radical Economics at Harvard and my Involvement Therein

As mentioned above, I entered Harvard as an adherent of radical political economics (RPE as it was known at the time). I had already joined the Union for Radical Political Economics (URPE) at Michigan. Harvard had two of the key figures in that field, Sam Bowles (on leave that year at UMass) and Herb Gintis (in the ed school, after being denied promotion in economics), as well as Marglin and Art MacEwan (who then spent his career at UMass/Boston).[40] According to Harvard documents (not just my recollection), 1972 was a period of turmoil in the department over grad student pressure to open up the curriculum (and faculty) to RPE.[41] I was surely one of the agitating grad students. A particular flash point was the department's decision not to award tenure to Bowles, who then decamped to UMass, along with Herb Gintis, to help reconfigure that department as a center of radical economics.

I have only a few specific recollections of that aspect of my experience. First, although Harvard resisted certain pressures, it was lenient in other ways; for example, the department allowed me to fulfill the requirement of presenting a chapter of one's thesis to a departmental seminar by

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[38] At this time, in Cambridge, I did not have access to a scientific calculator or computer to perform the calculations I needed for the paper. So I conducted these calculations laboriously using log tables in the library. I recall a British student observing me doing so, who then asked me for help with the math she was trying to do.

[39] Interestingly, on reading this memoir, my classmate Luis Alvaro Sanchez, pointed out to me that Hahn published with Solow a monograph on micro foundations of Keynesian macro in 1997 and had in fact, been working for some time on the same general issues I tried to tackle in my thesis. Unfortunately, I was unaware of this when I knew him at Cambridge; the paper I presented at his seminar was on savings, not quantity-constrained macro.

[40] Tom Weisskopf had just left, to take up a position at U Michigan. I had played some small role in his hire, as an undergrad on the Michigan hiring committee. (It seems kind of crazy, in retrospect, that Michigan had appeased student agitation of that era by putting an undergrad and grad student on the hiring committee!)

[41] As the Harvard memos show, Leontief, Galbraith, Marglin, and Arrow supported this move, but were outvoted by younger mathematical economists. I believe this applied to the tenure vote for Bowles, but of course, those votes were confidential. Some of these supporters were rumored at the time, but I did not know of Arrow's support. See also a fascinating memo by Galbraith proposing to split the department.

presenting instead to a session of URPE. Second, at one point the graduate student union went out on strike (probably an attempt to gain University recognition as bargaining agent). We marched around with placards, feeling very working class.[42]

Finally, I recall an evening session chaired by Galbraith (most likely the October 30, 1973 session listed here), which debated radical vs. conventional economics. At one point, reference was made by someone (maybe Bowles or MacEwan, who were on the panel, or perhaps a radical grad student in attendance) to Marx's doctrine of the "tendency of the rate of profit to fall." This was ridiculed (by another grad student, as I recall) that such a "tendency" was non-falsifiable. I countered from the floor by similarly mocking, "kind of like the neoclassical tendency toward equilibrium," to the amusement of the leftists in attendance. Honestly, I don't fully recall the incident, but it made a favorable impression on Art MacEwan, who retold the story years later.

In summary, my engagement with RPE at Harvard was mixed. As stated at the beginning of this memoir, I came to Harvard with a radical bent, but also developed an appreciation for rigorous economic theory, as my education continued. This did not immediately translate into an embrace of neoclassical economics, as I tried to apply the rigorous methods of conventional theory to develop non-neoclassical models which might advance the case for some sort of democratic socialism (e.g., the socialization of investment). Thus (to get ahead of the story), when I interviewed with Herb Gintis in 1977 for a position at UMass/Amherst, I objected to being categorized as a "non-political economist" (in the quota system that department had devised, whereby 40 percent of the faculty were to be "political economists" and 60 percent "straight"). I objected, since I considered myself a socialist. Gintis replied that my politics were irrelevant – what mattered was that my methodology was one of conventional economics (rather than institutionalist or the like). As it turned out, his categorization was correct, effectively foreshadowing my eventual disillusionment with the UMass economics department.

But this gets ahead of the story – back to the concluding chapter of my Harvard experience: writing my thesis. In an appendix below, I provide a chapter-by-chapter summary of my thesis. But I will here provide an overview of what I tried to do.

[42] As another gesture of radical chic, the attire I donned in those years included white overalls (as pictures from the period show). I also occasionally wore a white dinner jacket purchased from Goodwill, to which I attached a "Pizza Bob" patch. That referred to the famous sub shop in Ann Arbor which I had frequented, across the street from Nakamura Co-Op where I lived my sophomore year (and which still exists today, long after Pizza Bob's passing). I believe I wore the white dinner jacket (perhaps with my overalls underneath) to my oral examination in 1975.

<u>"Unemployment, Distribution, and Capacity Utilization on Equilibrium Paths"</u> (252 pages)

<u>Context, Overview and Some General Comments</u>

The neoclassical edifice had a huge empirical problem (as it seemed to me at the time) with the existence of involuntary unemployment. Keynes' *General Theory* diagnosed the problem as a failure of aggregate demand (famously codified in the "multiplier"), a concept that had no place in neoclassical theory, where competitive firms can sell as much as they like at the market price. This inconsistency was certainly well recognized at the time: it was commonly joked that one taught microeconomics on Monday, Wednesday and Friday, and macroeconomics on Tuesday and Thursday, while hoping that our students did not notice the two approaches were irreconcilable (despite Samuelson's famous claim of forging a "neoclassical synthesis").

The short version of what I tried to do in my thesis (at least the first two chapters) was to reconcile the essential features of Keynesian economics with some rigorous variant of neoclassical economics, a project known at the time as seeking "the micro foundations of macroeconomics."[43] In addition, I wanted to integrate a modern version of the Marxian concept of the "reserve army" of unemployment with the Keynesian and neoclassical frameworks.

Chapters I and II were explicitly devoted to this project, built around the ideas (respectively) of current and future quantity constraints on firms' demand. In addition, Chapter I argued that the "reserve army" concept, conceived of by Marx as a power relationship between employer and employee, could be naturally understood within a generalized neoclassical price dynamic for the real wage (or, conversely, price of output in wage units). As noted above, this would *not* factor into the distribution between wages and capital – that was fully determined by the requirements of steady-state growth. But it could help determine the equilibrium unemployment rate.

While Chapters III and IV could be understood as falling within the same framework, they nonetheless stood alone from that project, exploring two distinct questions. Chapter III examined the requirements of the savings function for stability of steady-state growth. Specifically, I show that the life-cycle hypothesis for aggregate saving had significant stability problems, unlike the "classical" assumption that savings came disproportionately from profits, relative to wages. Chapter IV went beyond the steady state to extend the Goodwin model of the real business cycle (a key influence on me at the time). That model was built around the twin ideas of Marx's "reserve army," and "classical" savings. I relaxed certain assumptions to explore conditions under which a Goodwin-type of model would show damped cycles (Goodwin's model was undamped) or even direct convergence on steady state – thereby providing a (belated) answer to the question Marglin had posed to me on my oral exams. I also attempted to build excess capacity into the model, in addition to the cyclical behavior of employment and wages.

This was an ambitious project, to integrate Keynes, Marx, and neoclassical economics in a coherent theoretical whole, drawing on the strengths of each, while jettisoning those aspects of

[43] One of the key economists working in this vein was Edmond Malinvaud, of France, a prominent general equilibrium theorist, and intellectual colleague of Arrow.

each that seemed logically weak. I would like to think I was iconoclastic toward each of these strands of thought, while formally integrating their sustainable insights in a rigorous fashion.

The "big think" nature of this project differed from a typical Ph.D. thesis in economics. Generally, one would choose a specific area within the established paradigm, which would advance that paradigm (one thinks of the advancement of "normal science," to use the framework of T.S. Kuhn's *The Structure of Scientific Revolutions*).[44] Such theses would not only be rooted in the established paradigm but also use the current tools of the trade – the most advanced methodology around. By contrast, my methodology was not generally technically advanced. I was of the view that simple models – which become complicated quickly – could yield significant insights, a lesson I learned from Marglin, as mentioned above, without fancy math. To be sure, I enjoyed math, and Chapters III and IV were quite math-intensive (perhaps overly so), but the methods were generally conventional (calculus and differential equations), rather than some of the advanced methods of intertemporal optimization (dynamic programming and calculus of variations) that were coming into prominence at that time (especially at MIT).

My approach was also unusual for the time in drawing on the long history of economic thought. History of thought was no longer a required subject in economics grad school by that time (no more than history of physics was in that field). But as mentioned above, I made a point of reading Smith, Ricardo, Mill, Marx, et. al. on my own, as well as 20th century iconoclasts from Cambridge, England – Keynes, Kalecki, Kaldor, Robinson, Sraffa, and Pasinetti. This explains the long passages in the first two chapters of my thesis (perhaps somewhat dilatory), comparing and contrasting the approach I was developing with these historical strands of thought.

<u>Where Did This Project Go?</u>

To be blunt, my project on micro-foundations of Keynesian macroeconomics (Chapters I and II) failed to gain traction, as I tried to launch my career. This work was considered original by some luminaries in the field, who wrote letters for my tenure case in 1984: Arrow, Bob Solow at MIT, and Yale Nobelist James Tobin.[45] But rightly or wrongly, it did not lead to much success in journal publishing, at least in the American journals. The tenure letters made the case that the quality of my work compensated for the relatively low quantity.

I did publish two pieces from my thesis: a much-shortened version of Chapter III on the instability of aggregate savings under the life-cycle hypothesis in the October 1981 issue of *Review of Economic Studies* (a highly regarded international journal); and a version of Chapter IV on a cyclical model of unemployment and capacity utilization, in the February 1984 issue of *Economica* (a reasonably regarded British journal). Although I did not publish articles based directly on Chapters I or II, I did publish two articles that further developed my thoughts on

[44] There certainly were theses that stood as notable exceptions. For example, Ken Arrow's student Michael Spence wrote a paradigm-shifting thesis that developed the theory of signaling, recognized later with the Nobel Prize. Of course, my work was not in the same league – it did not influence the field at all, let alone shift the paradigm.

[45] My colleague Leonard Rapping noted that I had letters from 2½ Nobelists, as Solow's came a few years later.

demand-constrained models in the March 1983 and December 1986 issues of *The Economic Journal* (the premier journal of Britain's Royal Economic Society).

These were not quite enough for career advancement at a U.S. university, which favored American journals (and higher publication numbers). So, after reaching a dead end on micro foundations of Keynesian macro and (perhaps more importantly, running out of new ideas on the topic), I tried to make an impact in some different areas in the mid-1980s (e.g. market failure in optimal product differentiation and stochastic diversification), which helped make the case for tenure, but also did not make much of a mark on the profession.

At this point, I turned my focus more to the supply side of the economy – productivity – which I came to believe was ultimately more important than the Keynesian problem of demand falling short of supply. Specifically, as I experienced the decline in educational standards among my undergrads at UMass, I turned my theoretical skills to the question of that deterioration. Here I made my mark with a seminal article on the economic theory of education standards (September 1994, *American Economic Review*), which remains my most-cited article even today, 30+ years later. And it also paved the way for my public service for three Massachusetts Governors in the State House, 1999-2006. But these are subjects for future memoirs.

But getting back to my grad student years at Harvard, I had the freedom to boldly explore, and so I did. Although I failed to have any impact on the profession through this effort, I am glad that I took the opportunity to try. I would like to think that I did so with rigor, creativity, and craftsmanship.

Over the course of my career, **I eventually learned that pride in the quality of my own work was at least as important to me as impact on the field or world at large. After all, one really only has control over the quality of what you do – its broader impact is largely beyond one's control.** If there is any lesson I would like to convey, this is it.[46]

Conclusion

Finally, to conclude this reminiscence of my graduate student years, I would say that they were intellectually exciting years for me. Aside from the usual stresses of graduate student life (especially the first year, when most students experience imposter syndrome), I enjoyed the freedom to explore heterodox ideas and the opportunity to learn from some of the top scholars in the field. As I developed confidence, it was an optimistic period of my life, full of ambition that I could change the world through the power of the ideas that I was developing. The fact that my career did not pan out that way – the contributions I eventually made were far more modest and

[46] It is the same lesson my 5th grade teacher (Mrs. Marilyn Shekletski) tried to impress on her students, back in 1960. I always remember her saying that even if you are the most humble pea-picker, you should strive to be the best pea-picker you can be. ("Pea picker" was a term of humility popularized in that era by Tennessee Ernie Ford.) I spent decades trying to achieve something great, but ultimately, I came back to the wisdom she imparted: just take pride in whatever you do.

largely lay in public service, at least as much as in academia – takes nothing away from the excitement I felt at the time: better to have dreamed big, than to never have dreamed at all.

Harvard then was a good place to be. My graduation ceremonies (June 1978) provide a telling contrast with Harvard today. Russian dissident-in-exile, Aleksandr Solzhenitsyn delivered his famous and controversial address, decrying Western weakness and lack of spirituality. Whatever one thinks of the merits of his views, the fact that he could deliver it at all, let alone without protest or disruption, contrasts sharply with the forgettable speakers and politicized environment of recent years.

For me, personally, I experienced Harvard as an intellectually honest environment, even if I did not share the mainstream view of economics at that time.   In retrospect, these years contrasted with the years that immediately followed for me at the University of Massachusetts, where I began so enthusiastically, but so quickly became disillusioned by the intellectual dishonesty I encountered – the subject of my memoirs' next installment.

<u>Tentative Outline of Memoirs to Come (I should live so long …)</u>
1977 – 1982. UMass I. Excitement Followed by Disillusion.
1982 – 1984. University of Toronto. Searching for new direction while broadening my horizon.
1984 – 1989. UMass II. Still searching for new direction.
1989 – 1999. UMass III & BU. Found a new direction: economics of educational standards.
1999 – 2006. State House. Something completely different: Research Director and Policymaker.
2006 – 2023. University of Arkansas. Education Policy Program & Teacher Pension Policy.

Appendix: Chapter Summary of
"Unemployment, Distribution, and Capacity Utilization on Equilibrium Paths" (252 pages)

Here I will distill the essence of each chapter, from the vantage point of reading it nearly 50 years later.[47] At the risk of over-simplifying an admittedly overly verbose thesis, I will extract the key equation from each chapter to show how the whole system hangs together.

Acknowledgements, Table of Contents, and Introduction.

Chapter I. "Momentary Equilibrium and the Dynamics of Distribution."

This chapter aims to replace the standard theory of the firm, where it is assumed that under competition, a firm can sell as much as it wants at the reigning market price – represented by an infinitely elastic firm demand curve (flat line at the market price). I do so by drawing on Paul Sweezy's theory of the "kinked demand curve." Originally put forth as pertaining to oligopoly, I argue that its key feature, the response function of other firms to a change in one's price (matching price cuts, but not price hikes), can just as well apply to a competitive structure. The result is a flat demand curve, but only out to a particular quantity – hence, a quantity constrained demand curve.

What remains to be determined is the price and quantity at which the "kink" occurs, or, more specifically, how it changes. The rest of this chapter takes up the price dynamic. The standard (Walrasian) price dynamic takes the price to adjust up or down as the market experiences excess demand or excess supply. But the only meaningful prices are relative prices – the ratio between one price and another. The logical implication is that the relative price should adjust based on the excess demand or supply for *both* goods. Specifically, the relative price adjustment should be a positive function of excess demand for the good in the numerator and a negative function of the good in the denominator. For a macro-economic study such as this, it is standard to take a single output and a single input (labor), so the key relative price is the price of output in wage units – the inverse of the real wage. Since the focus of the thesis is distribution between wages and profits, I recast the dynamic in terms of the real wage, w. I also represent the state of excess demand or supply by the ratio of demand to supply (rather than the difference) in both the product and labor markets, call them u (for capacity utilization rate) and ℓ (the employment rate), respectively. Formally, I posit that

$\dot{w} = h(u, \ell)$, $h_u < 0$ and $h_\ell > 0$,

where $\dot{w}$ is the rate of change of the real wage (adjusted for technical progress), while h_u and h_ℓ denote partial derivatives. Thus, a higher employment rate tends to raise the real wage, as does a lower utilization rate (by cutting price markups).

A strictly Walrasian dynamic would impose further structure, $h(1, 1) = 0$, such that a stationary state would hold at full utilization and full employment. However, a more general structure

[47]A separate 5-page document, submitted in May 1978 as part of Harvard's thesis requirement, provides a similar summary. Links to a scan of the thesis itself (252 pages in four chapters) can be found here and linked to headings in the text: Acknowledgments, TOC and intro, Chapter I, Chapter II, Chapter III, Chapter IV and Conclusion. In addition, two hard-bound copies can be found among my possessions, as well as two loose-leaf copies.

would allow for the Marxian "reserve army" assumption that stationarity would only obtain for some $\ell < 1$. I also allow for a further generalization of a "damping factor," such that a higher level of the real wage slows the rate of increase:

$(i)\ \dot{w} = h(u, \ell, w)$, $h_u < 0$, $h_\ell > 0$, and $h_w \leq 0$.

Setting this equation to zero specifies the combinations of u, ℓ, and w consistent with a stationary (or steady) state. To get ahead of the story, other parts of the analysis will determine the steady state wage (and profit rate), and utilization rate, so this equation will determine the steady-state employment rate, which may well be less than one, as the Marxian model assumes.

The chapter also includes some rudimentary econometric estimates of (*i*) (due to the insistence of James Medoff, the empirical member of my thesis committee). The main result from these estimates was to generally support the assumptions that $h_u < 0$ and $h_\ell > 0$ (estimates of h_w were generally not significantly different from zero). This finding suggests that the empirical finding of no systematic cyclical pattern in wages (contrary to various theories) is masked by the opposing cyclical effects of employment and capacity utilization. The estimates were also used to assess the Walrasian assumption, $h(1, 1) = 0$, which was generally rejected. Finally, I used the estimates to calibrate the steady-state unemployment rate consistent with various assumptions about the steady-state capacity utilization rate.

Chapter II. "Income Determination"

Much of this chapter is devoted to a fresh look at an old question of Keynesian economics: what is it that fundamentally distinguishes a demand-constrained economy from a supply-constrained economy? In this chapter, I go through a variety of models to conclude that the two key features are the existence of non-produced stores of value, as Keynes argued (especially credit) and the expectation of present and future quantity constraints. The expectation of limited future demand is what constrains investment demand. This, in turn, governs current income, as saving adjusts to investment demand by variation in income, through the familiar Keynesian multiplier.

In the absence of expectations of future demand constraints, it is hard to see how a firm's demand for investment is determinate. The investment demand curve, as a function of the cost of capital (known as the Marginal Efficiency of Capital, MEK) would be a flat curve, with no downward sloping segment, analogous to a firm's product demand curve, which I reformulated in Chapter I. Incorporating expectations of future product demand leads to an analogous reformulation of the MEK curve, as worked out in this chapter. The future demand constraint determines a point at which the MEK turns down, and the demand for investment is determined.

The key equation that comes out of this chapter is that current demand/expected future demand equals v/s, where v is the cost-minimizing capital/output ratio and s is the average propensity to save. This is derived from two facts: investment demand equals v times expected future product demand, and current income equals investment/s – the Keynesian multiplier. Inverting this and applying to realizable steady states, reproduces the standard equation of growth theory (due to

Harrod), where the steady-state growth in expected demand, output, employment, and labor supply must equal the growth in capital (Harrod's "warranted rate of growth"), *s/v*:

$(ii)\ g^* = s/v,$ where $g*$ is the exogenous "natural" rate of growth in effective labor supply.

Now, if *s/v* is a monotone function of the return to capital, *r*, then equation (*ii*) solves for the steady-state profit rate. Then, through the wage-profit frontier, we have the steady-state wage rate w. Cost-minimizing investment generates the equilibrium utilization rate, u (which is one, under constant returns to scale, and something less than one, under increasing returns). Setting equation (i) above (from Chapter I) to zero, then, solves for the steady-state employment rate, ℓ.

The chapter concludes with some comparative static exercises to analyze how one factor or another affects the steady-state employment rate. For example, an improvement in labor's bargaining power (e.g. from unionization) shifts up the *h(•)* function in (*i*) and reduces the steady-state employment rate. That is, improved bargaining power cannot alter the steady-state distribution of income, which is driven by the growth equation (*ii*), so it must be offset by an increase in the unemployment rate (Marx's "reserve army"). Following some other comparative static exercises, the chapter concludes with an analysis of government fiscal policy, culminating in the result that socialization of investment (in lieu of private investment) drives down the steady-state profit rate required to sustain growth.

Chapter III. "Classical Savings vs. Life-Cycle Savings"

As I argue in the introductory pages of this chapter, the choice of dominant savings model, together with the characterization of technology, carry with them deep implications for the nature and role of the return to capital in a capitalist economy. The classical model of savings (explained below) highlights the distributional role of the return to capital in providing the wherewithal for the necessary growth in the supply of capital. The life-cycle hypothesis focuses instead on the effect of the return to capital on the incentive to save for future consumption – an effect that is ambiguous, according to basic microeconomic theory (conflicting income and substitution effects). A third effect is on the demand for capital, rather than the supply: since the cost of capital is tied to the return to capital,[48] a rise in that cost will lead to less capital-intensity; this is the role highlighted by the Solow growth model.[49] The strength of this effect depends on the degree of substitutability between capital and labor.

This chapter examines the relative importance of these three effects of the return to capital, *r*, for the stability of the model's steady-state equilibria. For stability, we need the growth rate of capital (*g* = *s/v*, from Chapter 2, equation (*ii*) above), to be a positive function of *r* in the vicinity of the steady state $g*$. To see this, note that if the growth rate in labor jumps, raising $g*$, the question is whether the system will adjust to raise *s/v* toward the higher rate of capital growth

[48] The implications of this link for the claim that a rise in the profit rate will stimulate investment demand are revisited in my 1983 *Economic Journal* paper, using quantity-constrained Keynesian theory.

[49] This effect was criticized from the UK side in the Cambridge Controversies of the 1960s and 1970s.

required. The immediate impact of the rise in g^* is that labor becomes relatively abundant and capital relatively scarce, so the return to capital rises. If $g = s/v$ rises with the return to capital, the system adjusts toward the new required growth rate in capital, g^*; if not, the system will move away from the new steady state, and we have an unstable steady state.

The analysis of this question, the response of $g = s/v$ to a rise in r, can be parceled out into the two effects on saving identified above – the distributional effect between profits and wages and the incentive effect on individual savings – and the technological effect, based on the degree of substitutability between capital and labor. It is a mathematically intensive chapter – significantly more so than the first two chapters. But rather than pull out the key equations (which are pretty complicated), I will summarize the analysis verbally, albeit oversimplifying.

The analysis begins with the simple Cambridge (UK) model of "classical savings," due to Kaldor, where workers do not save and capitalists save some fraction s_c of profits, rK.[50] This leads to the simple steady-state result, $r = g^*/s_c$. More to the point of this chapter, this model has nice stability properties: s/v will be a monotone increasing function of the return to capital, r, as the distribution effect of r on savings works together with the technological effect on capital-intensity. The chapter provides a generalization of the model beyond the simple two-class case to a multi-class model where the propensity to save varies with the degree to which one's income depends on profits vs. wages, providing the same stabilizing distribution effect.

The rest of the chapter then turns to the main point: the stability properties of the chief alternative theory of aggregate savings, the "life cycle" hypothesis (LCH), due primarily to Tobin.[51] Under this theory, individuals save for retirement while they work, and dissave when they retire. In simplest terms, this reverses the savings propensities of workers and capitalists assumed in the classical model: the workers are savers and the retirees are capitalists, who dissave. As a result, the model's stability properties are problematic.

Specifically, I first analyze a two-period model (work when young, then retire). The number and stability of equilibria depend critically on the degree of substitutability between capital and labor – the technological effect. The key result is that for the empirically relevant range (elasticity of substitution between zero and one), there will be two equilibria – one stable and one unstable. I then show that the stability problem persists in a more realistic model of continuous time with computationally intensive simulations, generalizing results found by Tobin.[52]

When I submitted this analysis for my first publication a few years later in the *Review of Economic Studies*, I realized, under the guidance of that journal's editor, that the significance of my result lie in a third equilibrium – a trivial, but important one at zero capital intensity and

[50] One may think here of retained earnings as well as personal savings; in theory, retained earnings should be reflected in higher share values, so owners would be indirectly saving, unless they sell off the gains.

[51] The LCH of *individual* savings preceded Tobin, due to Friedman, Modigliani, and others. Tobin was the one to embed it in an *aggregate* model of overlapping generations, suitable to be married to the Solow growth model.

[52] The stability problem was not recognized by Tobin because he assumed elasticity of substitution equal to one, i.e. Cobb-Douglas technology. My calculations for the continuous time model were calibrated to the original estimate of the elasticity of substitution equal to 0.57.

output. The problem is that this zero-output equilibrium is stable. If one starts below the unstable positive equilibrium, the system will collapse toward zero output or never take off.

I conclude that since the world does not seem to exhibit the stability problem implied by LCH, this analysis supports the interpretation of the return to capital's classical distributional function, whereby profits adjust to provide for the necessary rate of capital accumulation.

Chapter IV. "Non-Steady-State Equilibrium Paths"

The last chapter takes equation (*i*) and the non-steady-state version of equation (*ii*) (i.e., g instead of $g*$, and with classical savings instead of a constant s) into a model of cyclical growth. The point of departure is Goodwin's model, which shows undamped growth cycles in employment and wages. As noted above, at my qualifying exams in 1975, Marglin asked me why Goodwin's cycles are undamped, and I had no good answer. Writing this chapter, a few years later, gave me the opportunity to explore the model to see how sensitive the undamped result was to variations in the assumptions. I show that reasonable relaxations of Goodwin's assumptions lead to damped cycles or even monotonic convergence on the steady-state growth path.[53]

In the last part of the chapter, I draw on groundwork that I laid in Chapters I and II, which takes increasing returns to scale as a basis for cost-minimizing excess capacity (basically, an adaptation of the theory of monopolistic competition). Taking this into a cyclical context, I show that for certain technologies, I show how the model can generate cyclical behavior of excess capacity, along with employment and wages, which are not dissimilar from observed patterns.[54] However, the model's cyclical behavior of productivity does not match the observed pattern.[55]

[53] As noted above, Arrow provided an elegant method of proof, to show that Goodwin's cycles were undamped (replacing the clunky proof I had labored over for weeks), and also my proof that $h_w < 0$ dampens the cycle.

[54] I should say that this analysis involves page after page of mind-numbing calculus derivations; frankly, trying to read it nearly 50 years later, I could only skim it, hoping that the derivations were correct.

[55] I return to the puzzle of the cyclical behavior of productivity and wages in my 1981-82 paper in the *Journal of Post-Keynesian Economics*.