Professional Memoir[*]

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Jane Templeton graduated in Seattle's Roosevelt High School's first class, in 1927. The Templetons lived right across 68th Street, in a house built the same year as the new high school. Jane enrolled as an art student at the University of Washington, hoping to become a fashion artist. But the art courses at the U were trivial—"tie-dyeing" she later dismissed them—unrelated to the world she wanted to enter. She got the idea to study in New York, and she and two girl friends planned to rent an apartment and spend a year there. In 1930, when she was 20, Jane took the two day trip to Chicago on the Northern Pacific, and spent a few days visiting with her Aunts Ruth and Mary. Then she was on to New York, beginning to enjoy the image she found she had back east, as a girl from the edge of the country.

But the New York art school was a disappointment too: Maybe schools were not where you learned design and the skills of a commercial artist. She picked up some work freelancing, looked around for a job, and—amazingly, in 1930—found one. She became the girl Friday for a fashion artist with the single name Leonard—LAY-o-nard—a recent immigrant from Austria, as far ahead of his American counterparts

[*]A lecture given in the Nobel Economists Lecture Series at Trinity University, San Antonio, Texas.

(I imagine) as were the physicists and psychiatrists who were also emigrating from central Europe around that time. He liked to talk to Jane about every aspect of the business: She was smart, talented, eager to learn, and fun to be around. Jane knew this was the real thing, and there were no more thoughts of school. But she was homesick, and never considered staying longer than the year she and her friends had planned. She returned to Seattle, and on the strength of the portfolio put together from her work for Leonard, got a job drawing newspaper ads for Best's Apparel.

All this and much more my brothers and sister and I learned from stories about our mother's New York adventure. We saw some of it first hand during the war, when she resumed work to supplement our father's earnings in the Seattle shipyards. She would take the three of us (then), ages 6, 4, and 3, all of us struggling to keep up with her clicking heels, on the half mile walk to the bus and then on the long bus ride to the McDougal's store, downtown. There we would to sit still while she handed over her drawings from last week and discussed her assignments for the week to come, and put the articles to be advertised into a shopping bag. On the return home, we received praise—if earned—for our successful venture into the world of adult work. At home, she took out a dress, shoes, a belt from the shopping bag. She hung the dress up on a hanger near the drawing board, in the living room of the shabby house we rented during the war, and put the accessories in front of her, at the top of the board. She set out her paints—pure white, glossy black, elegant grays—and her tiny brushes, and as I watched there came into being on paper a confident and attractive woman, stylishly dressed in what I had seen as nothing more than an ordinary dress, limp on the hanger.

2

My own train trip east took place in September, 1955, when I entered the University of Chicago as a scholarship student. Like my parents, I had graduated from

Roosevelt High School, but I did not want to follow most of my classmates to the University of Washington. My mother chose to view my first year at Chicago as a temporary stay, as her year in New York had been. I may have seen it this way myself at first: My letters home made references to a possible transfer to the University of Washington, to study engineering. If so, it was not for long. I was homesick, for sure, and had doubts about my ability to succeed, but when my first quarter transcript showed a couple of A's—not just in calculus, where I could coast on my high school training, but in Humanities I, where everything was new—I knew I was at Chicago to stay.

This Humanities course and several others I took over the next three years, were part of the glorious Hutchins fourteen year long general education courses that not long before had entitled a student to a Bachelor of Philosophy degree. By the time I got to Chicago the Hutchins curriculum had been partially abandoned, and supplemented with enough conventional college courses to entitle you to a regular, four year BA or BS in some specific field. But it was the Hutchins "great books" courses that were the real excitement for me, and except perhaps for the science majors, for most of us. It was these courses that gave us the sense of getting a distinctive education, of entering into a new world of culture and ideas, of becoming a new person.

Early in my first year, we were assigned to write a page or two on Brahms's Variations on a Theme by Haydn. A group of us checked out the LP from the library and sat around a dorm room, playing it over and over. My musical experience then consisted of Christmas carols and the top 40. What was the connection between Haydn's theme and all this music that Brahms claimed were variations? What could one write on such a topic? I'm sure I would not enjoy re-reading whatever it was that I handed in, but I can still hear Brahms's music in my head. I learned to listen to serious music with pleasure, and was grateful for what Allan Bloom much later described as this improvement in my ability to care for my soul.

The only science course I took in college was Natural Sciences II—a biology course. We read a modern anatomy text, and also selections from Darwin, Mendel, and others. I remember struggling over an incomprehensible paper on embryology by Spemann and Mangold, and one by another German author called "The continuity of the germ plasm" that had mysterious overtones.

But there was nothing spooky about Mendel's genetic theories. They were clear, they made some kind of sense (though there was nothing molecular in our Nat Sci II readings), you could work out predictions that would surprise you, and these predictions matched interesting facts. We did a classroom experiment with fruit ‡ies, focused on eyes, and pooled the results. Our assignment was to write up the results in a lab report and compare them to predictions from a Mendelian model. I had not enjoyed the actual lab work but I liked writing the report and spent the better part of my weekend on it. It was the …rst time I can recall ever working out the predictions of a scienti…c theory from its basic principles and testing these predictions against experimental evidence.

On Sunday evening, my friend Mike Schilder returned to the dorm from a weekend that had clearly not been occupied with fruit ‡ies. The report was due Monday, and he asked to copy mine. I agreed, in part just to get some reaction to a report that I was very pleased with. Mike came back in half an hour, and told me: "This is a good report, but you forgot about crossing-over." "Crossing over" was a term introduced to us to describe a discrepancy between Mendelian theory and certain observations. No doubt there is some underlying biology behind it, but for us it was presented as just a fudge-factor, a label for our ignorance. I was entranced with Mendel's clean logic, and did not want to see it cluttered up with seemingly arbitrary fudge-factors. "Crossing over is b—s—," I told Mike.

In fact, though, there was a big discrepancy between the Mendelian prediction without crossing over and the proportions we observed in our classroom data, too big to pass over without comment. My report included a long section on experimental error, describing the chaotic scene that generated the data and arguing that errors could have been large enough to reconcile theory and fact. I handed it in as written. Mike, on the other hand, took my report as it stood, except that he replaced my experimental error section with a discussion of crossing over. His report came back with an A. Mine got a C-, with the instructor's comment: "This is a good report, but you forgot about crossing-over."

I don't think there is anyone who knows me or my work as a mature scientist who would not recognize me in this story. The construction of theoretical models is our way to bring order to the way we think about the world, but the process necessarily involves ignoring some evidence or alternative theories—setting them aside. That can be hard to do—facts are facts—and sometimes my unconscious mind carries out the abstraction for me: I simply fail to see some of the data or some alternative theory. This failing can be costly and embarrassing to me, but I don't think it has any e¤ect on the advance of knowledge. Others will see the blind spot, as Mike did with crossing-over, keep what is good and correct what is not.

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The reality addressed by natural science always seemed to me someone else's province. In public school science was an unending and not very well organized list of things other people had discovered long ago. In college, I learned something about the process of scienti…c discovery, but what little I learned did not attract me as a career possibility. Introductory physics and chemistry were huge courses, designed to weed out pretenders, which I feared I would soon be discovered to be. More important, I think, was a sense that these areas were in good hands, that they would progress in pretty much the same way with my participation or without it.

What I liked thinking about were politics and social issues. My parents had become politically aware in the 1930s, and politics were always the leading topic of discussion in our house. My letters home, from undergraduate days through middle age, were full of politics and social commentary. I had read widely on these matters, and with this background, I got advanced placement in social science. It was not until my second year at Chicago that I could take Social Sciences III and the History of Western Civilization. I had never worked so hard or with as much enthusiasm as I did in these two courses.

When the time came to specialize, I wanted to prolong this excitement. By my second year I was fully socialized to Chicago's pristine intellectualism, and occupational considerations played no role in this decision. Since everything interesting seemed to begin with the Greeks, I majored in history, specializing in the history of ancient Greece and Rome and the European middle ages. I was drawn to the idea that economic forces were centrally important, but somehow never took an economics course. The general education courses from the Hutchins era paid no attention to economics. When I mentioned an interest in economics to my instructor in Social Sciences III, he suggested Heilbroner's The Worldly Philosophers and Galbraith's American Capitalism, books written for people who dislike economics and want their prejudice entertainingly humored. Neither book gives any clue to what economics is or what economists do.

I won a Woodrow Wilson Fellowship for graduate study in history, and was admitted at the University of California at Berkeley. The trip west was my honeymoon: Rita Cohen and I were married in New York in the summer of 1959. My weakness in languages—a classical historian needs ...ve—discouraged me from pursuing my interests in ancient and medieval history. I took an English history seminar course—tweedy students imitating every mannerism of their tweedy professor—and courses in economic history from Carlo Cipolla and David Landes. For Landes, I wrote a bibliographical paper on 19th century British business cycles: a topic I chose from Landes's list because it looked like the most theoretical, and so it was. I could see that without a better economics background I would always be on the fringe of a topic like this. I wanted to transfer into economics, but even with Landes's support the Berkeley economics department was not encouraging.

Rita and I were also homesick for Chicago. I talked the Social Sciences Division at Chicago into accepting me back as a graduate student. I talked the Woodrow Wilson Foundation into transferring my fellowship to Chicago. With the help of my brother Pete, who passed through Berkeley on his way to the Rose Bowl in Pasadena, we loaded a rented trailer and headed back to Chicago.

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By the summer of 1960 my young wife was pregnant, I had lost my Woodrow Wilson Fellowship, and I had spent the better part of the year making up for the fact that I had had no economics as an undergraduate. But I had found a field that I loved and that I was good at, and could hardly wait to start the regular first year graduate program with Milton Friedman and the rest of Chicago's faculty.

At the end of spring quarter, I had looked in the back of Kenneth Boulding's textbook and read Boulding's description of Paul Samuelson's Foundations of Economic Analysis as "the most important book in economics since the war." I had a summer to prepare for the beginning of graduate work in earnest, and there was no point in wasting the time on the second most important book. I got a copy from the library and all that summer, during lunch and coffee breaks from my research assistant job, I worked through Samuelson's first four chapters. When I got home each night, I wrote out what I had learned that day, line by line. It is impossible to invest this much in a single book unless you trust it, and though no one at Chicago had confirmed Boulding's recommendation for me, I immediately sensed that Samuelson was dealing with essential material in a way that was congenial to me.

Of course, I knew that Samuelson was regarded as a leading—perhaps the leading—economist in the world. I also knew that as an undergraduate at Chicago, he had taken Jacob Viner's graduate course along with Milton Friedman and George Stigler. In the preface, Samuelson says the book had "been conceived and written primarily in 1937," which is to say it was the product of Samuelson's early years of graduate school, at Harvard, when he was about the age I was then, in 1960. With this as background, the book's broad sweep and supremely con…dent tone was exhilarating. He patronizes the feared Viner: "As Professor Viner has pointed out with great insight...," crediting Viner with understanding a mathematical point that Samuelson has just shown on the preceding page. He disagrees with Alfred Marshall—then the god of Chicago economics: "I have come to feel that Marshall's dictum that 'it seems doubtful whether anyone spends his time well in reading lengthy translations of economic doctrines into mathematics, that have not been made by himself' should be exactly reversed." He goes out of his way to quarrel with our feared teacher, Milton Friedman, by disagreeing with a point in Friedman's 1935 article on elasticities, and this after dismissing the whole idea of elasticities as unimportant "except possibly as mental exercises for beginning students!" Here is a graduate student in his 20s, reorganizing all of economics in four or …ve chapters, right before your eyes, and let Marshall, Hicks, Friedman, and everyone else get out of the way!

I could see that some of this style was just youthful brashness, and I imagine there are passages that Samuelson would say di¤erently now. But what a waste it would have been if he had waited until his views had matured. The book would have lost the excitement of discovery that carries the reader forward, and the patient instructions to the student on how economics is done that only an author who has only recently learned something for himself can provide e¤ectively. Samuelson was the Julia Child of economics, somehow teaching you the basics and giving you the feeling of becoming an insider in a complex culture all at the same time.

I loved the Foundations. Like so many others in my cohort, I internalized its view that if I couldn't formulate a problem in economic theory mathematically, I didn't know what I was doing. I came to the position that mathematical analysis is not one of many ways of doing economic theory: It is the only way. Economic theory is mathematical analysis. Everything else is just pictures and talk.

5

Before the various University of Chicago libraries were consolidated in Regenstein Library, economics Ph.D. students studied in the Business and Economics library, on the top floor of what is now Stuart Hall. The room had been designed as the Law Library, and its Gothic arches and leaded glass windows lent a welcome seriousness to our efforts to gain some intellectual control over the vastness of economic knowledge. In this room, Neil Wallace set an issue of the Review of Economics and Statistics on the table in front of me: "Look at this." The issue contained a symposium on monetary policy, including the papers that had been presented along with commentary by assigned discussants. Neil had opened it to the opening paragraphs of Milton Friedman's comments on a paper by the Harvard economist Seymour Harris. I read the introduction, in which Friedman criticized Harris for mixing "prediction and prescription so thoroughly that it is difficult to tell when he is recording what is going to be done and when recommending what should be done." Section II then began:

> The role of the economist in discussions of public policy seems to me to prescribe what should be done in the light of what can be done, politics aside, and not to predict what is "politically feasible" and then to recommend it. Accordingly, I shall not attempt a detailed criticism of Harris' comment.[1]

[1] Reprinted in Milton Friedman, Essays in Positive Economics (Chicago: The University of

What excited me about this passage—and what I knew had excited Neil—was the confidence of Friedman's dismissal of Harris in this public, face-to-face interchange, and his own focus on what he saw as the economics of monetary policy without regard for how popular he or his analysis would be with his listeners. This focus and fearlessness was what we admired in Friedman's class: He was a moral example to us, just as Frank Knight had been for him and George Stigler.

Friedman rarely lectured. His class discussions were often structured as debates, with student opinions or newspaper quotes serving to introduce a problem and some loosely stated opinions about it. Then Friedman would lead us into a clear statement of the problem, considering alternative formulations as thoroughly as anyone in the class wanted to. Once formulated, the problem was quickly analyzed—usually diagrammatically—on the board. So we learned how to formulate a model, to think about and decide which features of a problem we could safely abstract from and which we needed to put at the center of the analysis. Here "model" is my term: It was not a term that Friedman liked or used. I think that for him talking about modeling would have detracted from the substantive seriousness of the inquiry we were engaged in, would divert us away from the attempt to discover "what can be done" into a merely mathematical exercise.

The quality of discussions in Friedman's classes was unique in my experience. He did not call on students by name, as I recall, permitting me and many other classmates to experience the intensity of engaging Friedman directly only vicariously. It was not dismissal that I feared—no graduate student would have been dismissed the way Harris had been—but the exposure of my confusion next to Friedman's quickness and clarity. He would engage a particular student in a dialogue, and once engaged no escape but assent was possible. Exit lines like "Well, I'll have to think about it" were no use: "Let's think about it now," Friedman would say. Friedman

Chicago Press), 1953.

not want to win the debate by convincing the rest of the class he was right, using the student as a foil—any experienced teacher can do that. He wanted the student he was engaged with to join him in thinking the problem through to the end, to be not merely exhausted but truly convinced. Of course, this meant that Friedman was completely exposed too.

Students at Chicago, then as now, formed into study groups to prepare for the Core Exam that tested for knowledge of the entire …rst year curriculum. The Core questions were like the questions Friedman used in class: Loose, notation-free statements of some economic question. Our job was to formulate the question sharply, focusing on essentials, and then answer it. Neil and I were in a study group of four or …ve, and we enforced the rule that everyone should prepare in advance. I still have the typed notes I prepared for these meetings. Looking at them again after all these years, I …nd little that I would change. The economics I learned from Friedman and Samuelson is a uni…ed and manageable body of knowledge. It can be learned in a few months, and anyone who has mastered it can think about any problem in economics. I passed the Core Exam in winter quarter, 1961, less than a year after I had completed my …rst, remedial, undergraduate economics course.

6

My doctoral thesis was concerned with estimating the degree of capital-labor substitution in U.S. manufacturing. It was a part of a larger project that Arnold Harberger organized, involving many students, to calibrate a general equilibrium model he was using for tax analysis. The question involved capital, so a dynamic theory would seem to be called for, but I based my econometric work on a static marginal productivity condition that did not require either measuring capital or studying its determinants. These may have been econometric virtues, but they only postponed my entry into economic dynamics. I couldn't wait to …nish my thesis and get on to something more interesting and ambitious.

The example I wanted to follow was the work of Dale Jorgenson, who was visiting at Chicago during my last year there. His study of the investment decision of the firm mixed theory, econometrics, and careful measurement in a way that I much admired (and still do). The modeling of firm and industry dynamics was at the top my agenda in 1963, when I became an assistant professor at the Graduate School of Industrial Administration (GSIA), the business school at Carnegie Tech.

Jorgenson had derived a formula for a firm's long-run capital-output ratio, but the theory did not explain why the adjustment to such a long-run target should be slow, as it appeared to be in the data. I modified the model by adding a penalty for rapid change—an adjustment cost—and used the calculus of variations to derive both the long-run target and the gradual approach to it from a single set of assumptions. I sent the paper to Dale, who directed me to an earlier paper by Eisner and Strotz that contained everything I had done. This was a setback, but somehow a stimulating one. I worked out a theory of firm-determined technological change, using the same variational methods. I generalized the Eisner-Strotz theory to many capital goods. It seemed there were enough unsolved problems in firm dynamics to support a lot of theorists.

Dick Schramm's GSIA thesis showed how to make a multiple capital good model operational econometrically. His model involved a lagged adjustment of price expectations to movements in actual prices. I remember a seminar presentation of Schramm's at which Jack Muth asked Dick why he had not assumed rational expectations. Of course, we all knew and admired Jack's paper "Rational Expectations and the Theory of Price Movements," but none of us knew how to exploit this idea econometrically. At that time, I thought of rational expectations as an elegant rationalization of the kind of lagged adjustment of expectations that Schramm assumed in his thesis, not as an alternate model. This was a half-truth that kept me from seeing the force of

Jack's objections.

Schramm's thesis dealt with decisions of competitive firms, taking prices and expected future prices as given. So did the Eisner and Strotz paper, and my extensions of their work. Since these theories did not treat the determinants of prices, there was no internal inconsistency in the non-rational expectations they assumed. Serious inconsistency did emerge, though, when I made an entire Marshallian industry the unit of analysis, instead of the individual firm. In my "Adjustment Costs and the Theory of Supply" I took the industry demand function as a given, and solved for the time path of prices along with the paths of production and capital stock. I postulated "myopic" expectations—price expectations formed by simply extrapolating the current price into the future—and then deduced from this and other assumptions an equilibrium price path that was not constant but rather grew or declined in a predictable way. In such a model, you could see the profit opportunities that firms were passing up. Why couldn't they see these opportunities too? But if they did, the model couldn't be the right way to describe their behavior. Now I could go back to Muth's 1961 paper and see that this inconsistency was exactly his argument: In his words, "the marginal revenue product of economics" should be zero, or close to it. If your theory reveals profit opportunities, you have the wrong theory.

The context in which I came to this understanding was deterministic, so the assumption of rational expectations reduces to perfect foresight, using a terminology that I take from Buz Brock. Calculating a perfect foresight equilibrium for an industry, say, sounds as though it should involve solving a fixed point problem in a space of price paths: the actual path implies a forecast path, which in turn implies an actual path. This sounds hard, and it can be. But under competitive conditions, one can show that an industry over time will operate so as to maximize a discounted, consumer surplus integral—a problem that is mathematically no harder than the present value maximizing problem faced by a single firm. Who, exactly, is solving this planning problem? Adam Smith's "invisible hand," of course, not any actual person: But the mathematics of planning problems turned out to be just the right equipment needed to understand the decentralized interactions of a large number of producers. One can calculate equilibrium quantities over time using this fact, and then read the equilibrium prices o¤ the demand curve. I saw this in a particular context just by comparing the Euler necessary conditions for the equilibrium problem to those from the consumer surplus maximization problem. In fact, it is a consequence of one of the classic theorems of welfare economics.

In my …rst years at GSIA I worked furiously to pick up the mathematics of optimization over time—calculus of variations, the maximum principle of Pontryagin, Bellman's dynamic programming—and of the di¤erential equations systems these optimization problems produced. I worked on applications of these methods to problems in economics and operations research. Theoretically-minded economists of my cohort were doing the same thing all over in the world. I was studying the dynamics of investment at the …rm and industry level, as were many others. Still others were studying optimal growth of an economy, optimal accumulation of human capital, optimal advertising and R and D investment, and so on.

In 1966 I was invited to a workshop on economic dynamics, conducted in Chicago by Hirofumi Uzawa, that involved some of the best people in this group of younger theorists. Uzawa was a charismatic …gure, an enormous in‡uence on theoretically-minded students, who had moved to Chicago from Stanford just after I had left. The seminars ran all day, through dinner, and into the evening. Discussions were noisy and intense, but friendly and constructive: I remember people going to the blackboard in the middle of a talk, to show how a speaker's argument had gone wrong and to try to help him …x it up.

Dave Cass spoke about work that he and Menahem Yaari were doing, trying to understand the logic of a model of an economy with an in…nity of overlapping generations that Samuelson had published in 1958. This paper had not been on reading lists of mine but I was immediately attracted by its simplicity and the role in had for money. This was the …rst model I had seen in which paper money had a useful role to perform, and had a command over goods in equilibrium. This was exciting, and the following fall I began to use this set-up in my graduate courses in macroeconomics.

The paper I had written for the Uzawa conference was never …nished: I couldn't establish the mathematical result that was to be the paper's centerpiece. I proposed to Ed Prescott, a friend of mine from his student days at GSIA who was then on the faculty at Penn, that we work together on it. Even with Ed's help, the original problem proved intractable, but we found ourselves thinking together about the behavior of …rms in a competitive industry that is subject to unpredictable demand shocks. Ed had used dynamic programming methods in his thesis, and introduced me to a paper by David Blackwell that worked out the underlying mathematics in a useful and beautiful way. We applied these methods to the problem of maximizing expected, discounted consumers' surplus in the industry. The solution takes the form of a Markov process or stochastic di¤erence equation describing the evolution of capacity and production in response to recurring shocks to demand. Buz Brock and Leonard Mirman were thinking about optimal growth in similar terms at about the same time.

But then how can the solution to this maximum problem be related to a competitive equilibrium in which …rms have rational expectations? Part of the answer lay in reformulating the rational expectations hypothesis in a way that did not require the linearity Muth had assumed in his analysis. The other part of the answer came from applying the competitive general equilibrium theory of Arrow, Debreu, and McKenzie, theory developed in the 1950s and gradually being extended to ever more general mathematical contexts. The potential of this body of theory for more applied work was, at that time, unrecognized. Prescott and I saw that it would be helpful to us, and plunged into a self-taught crash course. Putting the pieces together, we wrote "Investment Under Uncertainty."

In 1963, I had thought of a competitive industry in terms of …rms solving short- and long-run, deterministic pro…t maximization problems, under the (false) belief that current prices would maintain their current values forever, and with the passage from one to the other and all the e¤ects of unpredictable shocks tacked on as afterthoughts. Five years later, I thought of the same economics in terms of …rms maximizing expected discounted present value, with rational expectations about the probability distributions of future prices, and with stochastic shocks and adjustment costs both fully integrated into the theory. From an objective point of view, this transformation can be viewed as a product of decades of research by many economists. From my subjective viewpoint, it was the most rapid, radical change of view I have ever experienced as an economist.

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In those early years at GSIA, my closest friend was Leonard Rapping, a Chicago Ph.D. about two years ahead of me. As Leonard said in a 1982 interview,

> I expect that we talked once or twice every day from '63 to '68. We talked about everything: about economics, about politics, about business school problems, just about everything. We would have co¤ee from about three until …ve. We were the two people who would always be at the afternnon co¤ee hours. Everyone else would be back at their o¢ ces.[2]

Maybe it was inevitable that we would do joint research, but it was surprising that this should turn out to be research in macroeconomics—a …eld in which neither of us

[2] Arjo Klamer, Conversations with Economists (Savage, Maryland: Rowman and Little…eld), 1983, p. 223.

had even taken a prelim. We got there by the back door, through labor economics: At Chicago, we had both studied with Gregg Lewis.

At that time, many people were regressing changes in nominal wages on the unemployment rate, a statistical relation known as a Phillips curve, interpreting the negative coe¢cient of unemployment as describing a trade-o¤: lower unemployment means higher wage in‡ation. People had just begun to consider what kind of economics might underly this statistical connection. Leonard and I started discussing supply and demand models of joint wage and employment determination, and tried these out in the classroom. We decided to formulate and test such a model more carefully. To get an aggregate time series of decent length, back then, one needed to go back to 1929, so our project turned into an attempt to understand wage and employment behavior through the depression and war years.

Like everyone else, we thought that the ultimate source of employment ‡uctuations in both these periods came mainly from the demand side: ‡uctuations in spending on goods and services. But all the evidence we knew about indicated that labor supply was very inelastic. How could we get large employment swings out of demand shifts in such a setting? We used a device based on the old Keynesian idea of “money illusion:” People in booms (like World War II) are willing to supply a lot of labor temporarily at the high money wage rates available then, without realizing that when they spend these dollars it will be at higher prices. Symmetrically, people in depressions temporarily pass up jobs at low money wages, not realizing that these low wages will buy a lot more goods than they would have two years earlier. This was the idea we used to reconcile an elastic short-run supply of labor with an inelastic long-run supply. I thought of my parents, both working throughout the war and saving most of what they made to build up a down payment for the house they bought when the war was over.

In our model, wages and employment levels were always in a kind of temporary equilibrium, and this equilibrium had the property that in‡ation would stimulate employment. What about unemployment? We added a labor force participation equation that, combined with the rest of the model, gave us a Phillips curve.

Leonard and I did a careful job of working through the decision problems of households and …rms in a world subject to these "illusions," and another careful job putting together a set of compatible time series. The econometric work came out well, and the ambitions of the project began to dawn on us. "Bob, we're going to be famous!" Leonard would say. It frightened me, and I managed to be out of town on the day that Leonard presented the paper to our colleagues at a GSIA seminar. We were both right. Albert Rees refereed the paper at the JPE: His outraged reaction was later published as a comment. But why outrage? Another economist, we heard, called the paper "fascist economics." Fascist! For writing down a labor supply curve and taking it seriously!

Before our paper was …nished, Milton Friedman had used his Presidential Address to the American Economic Association to argue that in the long run, the unemployment rate would be independent of the in‡ation rate: There would be no Phillips-like trade-o¤ between in‡ation and unemployment. Friedman's argument was theoretical, but his premises all seemed to Leonard and me to hold for our model. Yet our model did imply a long-run trade-o¤. Later, we came to see that this di¤erence was due to our use of adaptive, rather than rational, expectations, but at the time we simply accepted it as an unresolved puzzle.

Edmund Phelps had been trying to use an equilibrium framework to think about unemployment and wage determination at about the same time, and he had reached the same theoretical conclusion that Friedman had. While making the rounds of the seminar circuit he had run across others engaged in the same kind of investigation he and Rapping and I were engaged in. He assembled these papers in a book, The Microeconomic Foundations of Employment and In‡ation Theory, now universally known simply as the "Phelps volume." This was the kind of fame that Leonard and I had dreamed of, and the book and the conference Ned organized around it gave us the …rst experience either of us had had of being at the forefront of an important research area.

In the introduction to the Phelps volume, Phelps had written that "...perhaps Lucas and Rapping are 180 degrees to the truth," by which he meant that perhaps we should have emphasized income e¤ects in our theory of employment ‡uctuations rather than the substitution e¤ects we did emphasize. On a social occasion soon after the conference—when Leonard and I were still high—we joked about this remark. This shocked Elayne Rapping. She said: "All you two care about is being cited by a well-known economist, about being famous. It doesn't matter to you whether you are right or 180 degrees o¤." Elayne's remark stung, and she was certainly right that the source of our good mood that evening was the boost the conference had given to our ambitions. The desires for fame and truth are not inconsistent, though, and in fact we were laughing because we were so sure that Phelps's criticism could be dealt with. But this was not the occasion to try to explain income and substitution e¤ects to Elayne, and anyway we knew that she was as pleased with our success as we were.

Not long after this occasion, Leonard and Elayne were swept up in the opposition to the war in Vietnam, and moved far to the left, politically. Leonard lost interest in the kind of economics we had done together. When the JPE published Rees's comment on our paper, both of us signed the reply, but I had written it alone. Our friendship had been based on conversations that ranged much more broadly than our joint work, and we tried to maintain them, but without success. Soon after, Leonard moved to the University of Massachusetts, which was then becoming a center for radical economics.

8

By the end of the 60s, I was leading two lives as an economist. With Rapping, I was an empirical macroeconomist, estimating Phillips curves and aggregate labor supply functions. Working with Prescott, I had immersed myself in the mathematics of dynamic programming and general equilibrium theory and was applying these methods to construct tractable, genuinely dynamic models. In my graduate teaching, I was putting these elements together, using the overlapping generations models that I had learned about from Cass to build models of monetary economies that would, I hoped, be helpful in addressing questions of macroeconomic policy.

At the Phelps volume conference, Ned Phelps had pushed one such question to the front of my thinking: If Rapping and I were right that monetary shocks affected people's willingness to supply labor supply by "fooling" them about their future options, then we needed to explain why everyone gets fooled in the same direction. Why isn't the worker who is over-optimistic about his job prospects offset by another who is too pessimistic? Phelps outlined an answer to this question in his essay in the volume, based on the idea that workers at any one location are short on information about what is happening elsewhere. When a worker sees a wage change, he thinks it is specific to his own market, not realizing that the same thing might be happening everywhere. I thought I could capture this effect with the kind of overlapping generations models I was using in class. I tried out one version in 1968. Further work led to "Expectations and the Neutrality of Money," submitted to the American Economic Review in 1970 and finally published in the Journal of Economic Theory in 1972.

The paper contained a careful and explicit construction of a theoretical example of an economy in which the motives, opportunities, and information of every economic actor was unambiguously spelled out. Expectations were rational. In this setting, as in Friedman's AEA address, there was no long run trade-o¤ between employment and in‡ation. Yet the model also implied the kind of correlations between employment and in‡ation that were then widely interpreted as hard evidence that such trade-o¤s did exist. I felt I understood for the …rst time both why Friedman and Phelps were right in arguing there was no long run trade-o¤ between unemployment and in‡ation and why econometric tests continued to reject this "natural rate" view.

Working out this example took me to the limit of my technical skills and beyond: It was not easy reading, nor had it been easy writing. It built closely on Rapping's and my view of the labor supply decision as well as on the formulation of rational expectations that Prescott and I had developed working on "Investment Under Uncertainty." It is easy for me to see the in‡uences of Phelps, Rapping, Prescott, and Cass in this paper, but the combination was new and striking: No one else was doing macroeconomics this way in 1970. The paper made my reputation.

9

My family and I spent the summer of 1970 in Seattle, driving our underpowered Plymouth Valiant into the headwind for days to get there. I had an o¢ce at the University of Washington. My ambition for the summer was to write a paper on Phillips curves for conference to be held that fall at the Federal Reserve Board. I had been invited, I assumed, as a spokesman for the Friedman-Phelps position that there was no long run trade-o¤ betwen unemployment and in‡ation. My plan was to translate what I heard learned from writing "Expectations and the Neutrality of Money" into linear examples that would make it clear to a much wider readership why the standard tests for "long run" e¤ects—tests of the sort Rapping and I and dozens of others had used—were not informative about the Friedman-Phelps hypothesis. This paper became "Econometric Testing of the Natural Rate Hypothesis."

The stay in Seattle was a pleasure—I enjoyed being close to my Seattle family—but by the summer's end, I found I missed the more intense atmosphere of GSIA and was eager to get back to Pittsburgh. On my …rst day at the o¢ce Marty Geisel handed me a working paper by Tom Sargent, saying only: "I think you will be interested in this." Sargent's paper involved exactly the logic of the paper I had just completed (which of course Tom hadn't seen), applied to Irving Fisher's predicted relation between expected in‡ation and interest rates. Rational expectations connected expected in‡ation, which we cannot observe, to actual in‡ation rates, which we can, in just the right way to give Fisher's idea content. Sargent had based a test of Fisher's theory on this observation—the …rst econometric application of Muth's idea.

I gave my paper in Washington in the fall. It was the …rst time that many in attendance had heard of rational expectations. James Tobin gave it a generous review when he summed up the proceedings of the conference: It was clear that I had made my point. When my session ended, an economist of about my age whose work on Phillips curves I knew and respected came up to the podium and told me: "You just explained why everything I've done in the last few years is worthless." This shocked me: I was viewing myself as an underdog in the conference setting, and was not prepared to assume this very di¤erent role. I protested: "Oh no, that wasn't what I said at all." He insisted: "Yes it was. It was exactly what you said. And you were right." I had not experienced anything like this before.

As Sargent's test of the Fisher hypothesis showed, the idea that rational expectations implied restrictions across equations was not special to Phillips curves. When Allan Meltzer and Karl Brunner asked me to give a paper on empirical Phillips curves for the …rst meeting of the Carnegie-Rochester Conference Series, I decided to try to illustrate this fact with a variety of econometric applications. The resulting paper, "Econometric Policy Evaluation: A Critique," explained in detail how rational expectations undermines the then-conventional uses of econometric models in simulating the e¤ects of policy changes. This "Lucas critique," as it came to be known, is probably the most in‡uential paper I have written.

My critical writing on the Keynesian macro-econometric models of the day showed that simulations of these models could not be expected to give accurate answers about the e¤ects of changes in economic policy. Parallel work by Sargent, and Sargent and Neil Wallace, showed the same thing. These demonstrations e¤ectively ended the role of these models in policy debates, and eliminated the main intellectual basis for monetary and …scal …ne-tuning of the economy. We were certainly not the …rst economists who were skeptical of Keynesian econometric models—the whole monetarist tradition of Milton Friedman, Allan Meltzer, and Karl Brunner anticipated our conclusions—but Sargent and Wallace and I were critics from within. We were committed to the use of explicit models to evaluate policy changes, we knew the work we were criticizing in detail, we could articulate the reasons for our skepticism in a way that those we criticized could see the arguments and respond usefully to them. Macroeconomic debate had changed course.

10

Not long after "Investment Under Uncertainty" was …nished, Ed Prescott returned to GSIA as a faculty member. I was working on monetary theory, a topic that Ed has never been much interested in. I was also thinking about unemployment, using a theoretical setting in which workers are distributed over a large number of distinct markets, and these markets are subject to persistent shocks to demand. If your market is hit with a bad shock, wages are likely to be low for a while. You are tempted to leave, to set out for brighter prospects elsewhere, but this will entail a spell of unemployment. The risks must be balanced, and how this balancing comes out depends on what everyone else is doing. After some weeks of work, I felt I was very close to having a successful mathematical model of this situation, a theory of what Milton Friedman had called the "natural rate of unemployment."

But the pieces just would not fall into place. I believed that my formulation had the economics of these interactions just right, and that the problem was just that I didn't know and couldn't invent the mathematics to work out the implications of the theory. Once again, I asked Ed to collaborate, and after I had taken him through the model, he agreed. This was late on a Friday afternoon, so we stopped working and went downstairs to the faculty-student TGIF.

After the party, walking home by myself through the Schenley Park golf course, thoughts of regret came over me. I had formulated, on my own, a great theoretical problem, and had carried the analysis of that problem close to completion. Now I had shown the problem to Ed, who would surely see a quick resolution of the mathematical issues that I had been stuck on. He could write the paper by himself and since I had shown him no actual results, claim credit for himself. He might not even thank me for "helpful discussion" in his opening footnote! As soon as I got home I took out my typewriter and, in a kind of paranoid frenzy that lasted through the weekend, I wrote a draft of the entire paper: Introduction, Theorem 1, Theorem 2, and so on, right through to the end.

On Monday I felt foolish as soon as I saw Ed, but I handed him the draft anyway. Of course we soon discovered that most of the results stated in the draft were false, and that we had no idea how to prove the theorems that were possibly true. How could it have been otherwise? If I had really known how to finish the paper I wouldn't have asked Ed to work with me! So we began work in earnest.

Some days, perhaps weeks, later I arrived at the office around 9 and found a note from Ed in my mailbox. The full text was as follows:

"Bob,

This is the way labor markets work:

$$v(s, y, \lambda) = \max\{\lambda, R(s, y) + \min[\lambda, \beta \int v(s', y, \lambda) f(s', s) ds']\}.$$

Ed"

Of course since our project was well underway, we had agreed on notation: s stood for the state of product demand at a particular location, y stood for the number of workers who were already at that location, $R(s, y)$ was the marginal product of labor implied by these two numbers, and $v(s, y)$ stood for the present value of earnings that one of these workers could obtain if he made his decision whether to stay at this location or leave optimally. Other features of the equation were as novel to me as they are (I imagine) to you.

The normal response to such a note would, I suppose, have been to go upstairs to Ed's office and ask for some kind of explanation. But theoretical economists are not normal, and we do not ask for words that "explain" what equations mean. We ask for equations that explain what words mean. Ed had provided an equation that claimed to explain how labor markets work. It was my job to understand it and to decide whether I agreed with this claim. This took me a while, but I saw that Ed had replaced an assumption of mine that workers who leave any one location hit on a new location at random—maybe a worse location than the one they had left—with the alternative assumption that searching workers were fully informed about options elsewhere and bee-lined for the best destination. Mathematically, this meant that a single parameter—Ed's λ—stood for two different things: the present value of earnings that all searching workers would have to expect in order to leave location and the present value that a particular location would need to o¤er to receive new arrivals.

Mathematically, Ed's equation was a very familiar, comfortable object for me to analyze: Once I convinced myself that it described some sensible economics, it took a few minutes to see that its properties could be established by standard methods and that these properties were interesting and reasonable. By lunchtime, I could see that I was to be a co-author of a very sharp paper, unlike anything anyone had seen before, a paper with a potential for helping us to think about important events.

If I had to pick a single day to represent what I like about a life of research, it would be this one. Ed's note captures exactly why I think we value mathematical modeling: It is a method to help us get to new levels of understanding of the ways things work. No one could have written Ed's equation down at the beginning of an inquiry into the nature of unemployment: It is too far from earlier ways of thinking to be grasped in one step. The new understanding that this equation represents could only be gained through a trial-and-error process, involving formulating and analyzing explicit models. It is this struggle to capture behavior in tractable models that leads us deeper into the economics of market interactions, and forms the progressive element in economic thought.

11

In October, 1978—leaf season—the Federal Reserve Bank of Boston sponsored a conference at the Bald Peak Colony Club in New Hampshire. Ed Prescott and I rented a car at Logan Airport and drove up together. Night fell before we reached the conference center and we got lost on country roads. We stopped for directions at a crossroads store, but after a few minutes of laconic "Nope, yep" New England conversation we realized that the two old men in the store were amusing themselves at our expense, conveying no information. We left in disgust and anger. The incident heightened my sense of entering foreign territory.

On the drive, neither Ed nor I discussed the papers we were to give at the conference. Ed had been reading Simon Kuznets's work, and treated me to a review of Kuznets main findings. I was grateful for the chance to learn something from this central figure in research on economic growth without actually having to read him. We talked about the kinds of theoretical models that might fit the regularities that Kuznets's had documented. What better and more productive way to deal with anxieties about how one's work will be received than looking beyond to the next project?

When the conference began, the next day, it became clear that this was not the occasion for such anxieties. The attenders included representatives of the Keynesian establishment—including Paul Samuelson himself—and some "new classical" rebels. In my memory, the conference was a kind of triumph of the new classical views. My own paper was a reprise of Milton Friedman's 1948 paper, "A Monetary and Fiscal Framework for Economic Stability," emphasizing the support that recent research provided for Friedman's positions, with no pretense of new findings. I viewed it as my day to stand up and be counted as a Chicago economist. I later wrote to my parents:

> The influence my work has had was astonishing to me. I was very nervous about my presentation, which was extremely negative on what most of this group is up to, yet people were lining up in the question period to take their turn to say how right I am...

The question period I referred to was chaotic, and I remember people calling for me to denounce work by John Taylor and Stan Fischer that, like some of my own work, attempted to account for real effects of monetary instability. I also preferred my approach, but I understood that this preference was not really defensible empirically.

I said that on the basis of the evidence available now, I did not see how it was possible to distinguish between my views and Taylor's. Looking back on the occasion, an exciting one for me, I am pleased and a little surprised that I managed such a level-headed reply.

Though I did not see it at the time, the Bald Peak conference also marked the beginning of the end for my attempts to account for the business cycle in terms of monetary shocks. At that conference, Ed Prescott presented a model of his and Finn Kydland's that was a kind of mixture of Brock and Mirman's model of growth subject to stochastic technology shocks and my model of monetary shocks. When Ed presented his results, everyone could see they were important but the paper was so novel and complicated that no one could see exactly what they were. Later on, as they gained more experience through numerical simulations of their Bald Peak model, Kydland and Prescott found that the monetary shocks were just not pulling their weight: By removing all monetary aspects of the theory, they obtained a far simpler and more comprehensible structure that …t postwar U.S. time series data just as well as the original version. Besides introducing an important substantive re-focusing of business cycle research, Kydland and Prescott introduced a new style of comparing theory to evidence that has had an enormous, bene…cial e¤ect on empirical work in the …eld.

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By the time of the Bald Peak conference, I had moved from Carnegie-Mellon to the University of Chicago. I was just over 40. The work that was later recognized by the Swedish Academy of Sciences was done. I had arrived at a research style that has continued over the years to lead me into new ways of seeing things.

In the more than twenty years since, I have continued to devote virtually all of my time to teaching and research. I have written on many subjects and participated in collaborations with Andy Atkeson, Esteban Rossi-Hansberg, and, especially, with Nancy Stokey, that have been as interesting and fruitful for me as those with Leonard Rapping and Ed Prescott. Perhaps at some later occasion I will review these years, too: I have enjoyed them, produced some work I am proud of, and accumulated enough memories for many more memoirs.