But do let me say something: Thanks so much for being a great adviser. You have always made economics exciting, exciting, exciting. And whatever strange little ideas I’ve had, whatever direction my research has gone, you have always been incredibly supportive (while at the same time challenging my ideas to improve them).

—James Schmitz, congratulatory message, October 11, 2004

The Joy of Being a Teacher-Researcher

I was asked to lecture on my development as an economist, and this is what I will do. Before doing this, however, I will introspect a little on the related question of why I turned out to be successful as a teacher-researcher. I hyphenate teacher and researcher because in my case these two activities are joint activities, and cannot be separated. I love the enthusiasm of former students for economic research and teaching, as shown in the above quotation. In this essay, I will be discussing several collaborations with past teachers and students. I have a great debt to my students, from whom I have learned so much, and I think they have learned from me. At the end of this essay, I list the students whose dissertation I signed or played a major role in thesis supervision. I dedicate this essay to these special people.

Virtually all my research is joint research, and when I say “joint,” I mean with one other equal collaborator. Both collaborators contribute to all phases of the research, and both are senior authors on the resulting paper or papers. These collaborations typically originate in discus-
sions that give rise to an economic problem that warrants analysis. My style is to get things done in the morning and wander the halls talking to people in the afternoon. In these talks a good research problem sometimes arises. Even when it doesn’t, I often learn something that enhances my stock of useful economic knowledge. I emphasize that discovery of a problem is a joint activity, and assigning credit for discovering a good research problem is impossible.

A little about my personality, which is related to why I gravitated to economics, is as follows. Throughout my life I liked figuring out puzzles, and making complex things simple and orderly. I get joy out of understanding why something is true, and I am not content to just know that it is true. As an example, I played a fair amount of bridge in high school. I remember when my partner, Joseph Dodge, stated the principle of restricted choice for finessing. I just had to either disprove the principle or understand why it was true. In the process of figuring out why the principle is true, I rediscovered Bayes’s theorem for the case where the probabilities are rational numbers, as they are in bridge. Understanding the theory underlying the principle of restricted choice put my mind at ease.

Another feature of my personality is that I am a skeptic; I have to be convinced. I remember many arguments with my father, who was also a skeptic. This personality trait has served me well, because often what is generally accepted economic knowledge should not be. When working on a problem with someone, I have intense concentration, and solving the problem becomes important to me. In these joint efforts, it does not matter who has the insight that leads to the solution to each of the many subproblems that come up along the way to solving the problem. What matters is that the problem gets solved.

I am a bit of a dreamer, and in high school I wanted to be a rocket scientist. It was the Sputnik era, so the fact that I dreamed of being a rocket scientist is not surprising. This dream led me to major in physics my first three years at Swarthmore College, a small liberal arts college eleven miles southwest of Philadelphia. A number of lucky events led me to becoming an economist. One stroke of luck occurred in my junior year at Swarthmore. That year I had two all-day honors laboratories—one in physical chemistry, and one, I believe, in electricity and magnetism. I did not like the all-day laboratories, and this led me to drop out of the
physics honors program and major in mathematics. Another lucky event was that I took a course in the engineering department in my senior year. The course was taught by an enthusiastic, supportive professor named Sam Carpenter. This course influenced me to go to graduate school in operations research—a discipline that brought mathematical modeling into management. This was then a new and exciting field. I attended Case Institute of Technology for graduate studies.

At Case I learned recursive methods, which proved so useful when made applicable to economics. On a methodology note, a statement made by Russell L. Ackoff, then the head of the operations program at Case and one of the fathers of operations research, sticks in my head to this day and continues to influence my approach to research. He said that there is no theory without measurement, and no measurement without theory. He went on to say that a science progresses through the back-and-forth between theory and measurement.

I was lucky that experimental physics was not to my liking and that I took the engineering course that led me to Case, where I learned a set of tools that proved to be invaluable in helping to make macroeconomics part of economics. At Case I decided to go on for a Ph.D., and my choice set was Case and the Graduate School of Industrial Administration (GSIA) at the Carnegie Institute of Technology. The program at GSIA was multidisciplinary, not interdisciplinary. My program was economics, statistics, and industrial administration. The latter field included accounting, finance, and marketing, which in large part are subfields of economics. I am not sure why I chose GSIA, but I was lucky I did. Otherwise, I would not have become an economist.

My Development as an Economist as a Student at GSIA

Michael Christopher Lovell, Teacher and Collaborator

When I arrived at GSIA, the dean was Dick Cyert. He had the policy of matching, in an informal way, an incoming Ph.D. student with a faculty member. I was matched with Mike Lovell, another stroke of luck. Mike helped me so much to gain confidence in my ability to do research. We wrote two joint papers, one in the old business cycles tradition of Samuelson’s multiplier-accelerator model, but with money. The primary
contribution of the paper was to make a point in a debate that was then ongoing in macroeconomics. But we innovated methodologically in that paper in assuming rational expectations for the targeted capital stock, which was an argument of the investment equation in the model we developed and used. We determined the operating characteristics of the model for various monetary and fiscal policy rules. This model was not a dynamic general equilibrium model and therefore not in the tradition of what has become macroeconomics (or for that matter, what virtually all of aggregate economics has become).

The other paper was a statistics paper in which we analyzed least squares estimators with linear inequality constraints. It turns out that proving the obvious—the constrained estimator is better than the unconstrained estimator—was very difficult. Indeed, in the process of trying, we proved that the obvious was not true in general by coming up with a counterexample. We did prove that under weak conditions, the restricted least squares estimator was better than the unconstrained estimator. We conjectured but did not prove that the result holds under more general conditions. I must admit that not being able to prove the conjecture has bothered me for over forty years. Sometimes when I’m on an airplane and I have some time to waste, I come back to this problem. This says something about my personality, I guess.

The skills I learned from Mike Lovell have contributed to my success as a teacher, and I owe a big debt to him for what I learned. Unlike factors determined by the genes, the skill in helping students develop into teacher-researchers has been transmitted to my students and to students of their students. Key is helping students to believe in themselves and be honest with themselves.

An important event in my development as an economist was meeting Janet (Jan) Simpson in Pittsburgh in 1964. She became my wife a year later. She has a nonconformist streak and did not expect me to make much money. She was more interested in having a family and a career than in having high consumption levels. She raised three children, got a Ph.D. in industrial psychology after the children were all in school, and then had a successful career working for an industrial psychology consulting firm.
I was lucky that she is adventuresome. In 1974, I obtained support from the Guggenheim Foundation and the National Science Foundation for a year’s leave. I was advised to take this opportunity to visit MIT, which was a very attractive possibility, but at Jan’s urging decided to visit the Norwegian School of Business and Economics (NHH), where Finn Kydland was then a faculty member. Going with three young children aged seven, five, and one to a city with no English-speaking school was quite an adventure. Nobody in the family spoke Norwegian when we arrived, though by the time we left my oldest son was fluent, my daughter understood it but wouldn’t speak it, and my wife could communicate a little.

It was a crazy career decision, but that year at the NHH working with Finn proved to be a productive one. Finn and I wrote our paper “Rules Rather Than Discretion and the Time Inconsistency of Optimal Plans,” one of the two papers for which we were awarded our Nobel Prize. I guess being away from the mainstream has some advantages. We were relatively free to think differently and go our own way. It turned out that visiting Norway was not a career sacrifice as I, and my colleagues, thought would be the case.

Robert E. Lucas, Jr., Teacher and Collaborator

Robert (Bob) E. Lucas, Jr., and I arrived at GSIA in fall 1963, he as a new assistant professor and I as a Ph.D. student. I was fortunate to take two of Bob’s courses at GSIA. The first was econometrics, where I learned some of the principles of using the statistics discipline to draw scientific inference in economics. The second was a capital theory course, where I learned some invaluable economic theory. Many of the things he said with respect to methodology I learned to appreciate only later.

There were many methodological discussions at GSIA. Herbert Simon, who was awarded a Nobel Prize in 1977, would argue that firms face mathematical problems that are unsolvable and therefore the assumption of profit maximization in economics should be abandoned. Herb was right about firms facing combinatorial problems that are unsolvable. Bob accepted this fact, and went on to argue that the way to proceed is to treat firms as if they maximized and, for that matter, to treat
all decision makers in economic models as if they consistently maximized. This led to the then-radical view that assuming maximizing behavior is the way to proceed even when agents are faced with dynamic problems and there is uncertainty. I learned from Bob that we have to build our economic intuition from the study of model economies that we can analyze.

Bob was on my dissertation committee, but it was the great statistician Morris DeGroot and my adviser Mike Lovell who provided me with constructive criticism when writing my dissertation. My dissertation was primarily a contribution to statistical decision theory and not to economics. After my defense Bob asked in a nice way, “Why are you doing that?” I knew that he knew that what I was doing was the logical thing to do if one accepted the assumptions underlying the then-dominant macroeconometric model framework, and I was sure that he thought my research was innovative and well done. Consequently, I was puzzled by his question. Collaborating with him on the paper “Investment under Uncertainty” a few years later, I came to understand what he was saying with that question.

The history of our collaboration is as follows. Edmund (Ned) Phelps, who was at the University of Pennsylvania when I was, identified a number of papers concerned with laying the micro (which now means neoclassical) foundations for macroeconomics and put together the book *Microeconomic Foundations of Employment and Inflation Theory*. Ned brought the authors of these papers to a meeting at Penn, where the authors presented their papers and were subject to the constructive criticism of the other contributors.

I was not an author and so did not participate, but I did go to the reception after their long day. At that reception, I talked with Bob Lucas about an interesting problem. We thought that we could solve it, and we decided to submit our idea to the North American Summer Meeting of the Econometric Society. Submission meant writing up and sending in an abstract for a paper. The paper was accepted for presentation. The problem turned out not to be straightforward, and as a fallback Bob proposed extending his 1967 “Theory of Supply” paper to uncertain environments in order to develop an investment equation. The paper did much more than extend his paper to uncertain environments. The con-
cept of recursive competitive equilibrium, a tool needed to apply the dy-
namic economic theory to modeling aggregate phenomena, is developed
there. Equilibria are Markov processes with stationary transition prob-
abilities, and the behavior of model time series can be compared with the
behavior of the time series of the economy being studied.

With this collaboration, I understood the comment he made after I
defended my dissertation. He was telling me to use dynamic economic
theory to model aggregate phenomena. With this collaboration I became
a Bob Lucas student and an economist. To return to the theme of this
lecture, Bob Lucas is the most important person in my development as an
economist. Having been at the right place at the right time was another
stroke of luck.

The principle guiding my research is the importance of theory interact-
ing with measurement. I came to the conclusion that there was no hope
for the approach being used in macroeconomics of empirically searching
for the policy invariant laws of motion of the economy. I had read the
Lucas critique. Something else was needed. Given this, I stopped teach-
ing what was then called macroeconomics and began using dynamic
equilibrium models to study aggregate economics as practiced by Bob
Lucas.

I disagreed with Bob on one point: the importance of the national ac-
counts, behind which there is a lot of theory. These accounts are based
on a recursive capital theoretic framework for technology, which is the
framework used in capital theory, which in turn is the theoretical frame-
work used in aggregate economics. I made the big switch from orga-
izing empirical knowledge around equations to organizing empirical
knowledge around preferences and technology, or in layman’s terms,
around the ability and willingness of people to substitute. Supply and
demand in general equilibrium theory have no empirical counterparts,
and are used only to establish the existence of equilibrium. I love the title
of Tom Sargent’s paper “Beyond Supply and Demand.” Tom was my
valued colleague for one year at Penn and five years at the University of
Minnesota.

I came to think in Bob’s economic language, and we came to very
quickly agree where we disagreed. By this I mean we agreed on each
other’s logic, while differences in conclusions were a matter of different
assumptions. Differences in assumptions are something to be resolved by better measurement.

After I returned to GSIA in 1971 as a faculty member, Bob and I became colleagues. We had an additional collaboration, “Equilibrium Search and Unemployment,” a paper he loves as evidenced by his lecture in this series. We had a number of jointly supervised students who had highly creative dissertations that exploited the methodology of using common knowledge dynamic competitive equilibrium models. With common knowledge, information differs across people in the model economy, but everyone knows the probability structure of the world. These analyses also assume that people maximize expected utility. Our student Jean-Pierre Danthine, for example, used this methodology to develop a model with the prices of securities revealing information about the information set of others.

I remember Bob holding court at coffee times at 10:30 a.m. and 3:00 p.m. The topic of discussion was economics and more generally social science. They were serious discussions.

My Development as an Economist at the University of Pennsylvania

My first academic position was at Penn, where I began my shift from statistician to economist. A group of exceptional young economists there contributed to my transformation. I also interacted with a number of talented students there. One of those students, in particular, influenced my development as an economist.

Thomas F. Cooley

The one student at Penn with whom I subsequently collaborated is Tom Cooley. As a graduate student, he engaged in the life of the 1960s to the fullest. The senior faculty knew he was talented, but had doubts that he would bother to complete the program, given his lifestyle as a student. We became friends, and over a beer would talk about some of the practices and problems of economic forecasting. He was supported by WEFA, Larry Klein’s forecasting center at Penn, as was I during the summer. At the end of his program, to the surprise of the faculty, Tom put his mind to research and wrote an outstanding dissertation, with little guidance
and in a very short time. I learned later that the senior faculty gave me credit for Tom’s dramatic turnaround, which I did not really deserve. We subsequently wrote a series of papers in which we built on the ideas that arose in our discussions.

Tom went on to have a highly successful career as an econometrician and then shifted to the new macroeconomics nearly fifteen years after receiving his Ph.D. He has contributed in an important way to macroeconomics. Beginning in 2002 he became the dean of NYU Stern School, where he has been highly successful. Tom keeps amazing me with his accomplishments. We developed some new empirical tools in the hope of saving macroeconomics as then practiced. The fact that these tools were not of much use in macroeconomic forecasting, despite their mathematical elegance, led me to the Lucas dynamic economic theory approach to macroeconomics.

My Years at GSIA as a Carnegie Mellon Faculty

Finn Kydland

After returning to GSIA in fall 1971 as a faculty member, I met Finn Kydland, a truly great stroke of luck. He had completed his qualifying examinations and was writing a dissertation. He was not in the best of positions because his mentor, Sten Thore, with whom he had come to GSIA, had gone back to Norway. Both Finn and I were out of the operations research tradition. We knew recursive methods and could use the computer to solve maximization problems. I gave Finn feedback on the third essay of his dissertation dealing with a dominant firm problem. I remember so distinctly when he made a claim to which I said, “That can’t be right.” In the most confident way, Finn said, “I don’t make mistakes,” and he doesn’t. He has to be certain of something before he will make a statement.

The year before he returned in fall 1973 to NHH, we began working on the use of recursive methods to evaluate policy rules. I joined him in Norway a year later. The lifestyle at NHH was then very different from what it is at other academic places I have been. Lunches were tea and a piece of goat cheese on a piece of bread. In the late afternoon, there were only three people in the building: Finn, me, and the woman who waxed
the floors. That woman was annoyed by us getting in the way of her doing her job. Everyone else was home working on their house. A common sight in the Bergen area was a cement mixer in the front yard, along with a pile of stones and a half-built stone wall. They called the area along the fjord, just a little way from NHH, Porridge Hill, because most of the disposable income of the people living there was going to making mortgage payments. There was very little left to spend on food, which was very expensive.

The first half of that year at NHH, I did a lot of thinking about economics, but did not get much done. Then it dawned on Finn and me that the fact that the time consistent policy rules we had been computing are not optimal was an important finding. In the spring, we sat down and showed how bad outcomes could be if there is discretion, and best action is chosen in the current situation given the state of the economy. We wrote the first draft of the paper “Rules versus Discretion: The Time Inconsistency of Optimal Plans,” an extended version of which appeared in 1977.

This was not the last of our major collaborations. Finn visited GSIA in 1977–78 after visiting Minnesota the previous year. We became more interested in a positive theory of business cycle fluctuations than in a theory of policy. We started interacting theory with observation, but were not that successful in that endeavor because we were beginning modeling at the linear-quadratic economy level.

Academic year 1978–79 was my turn to be on leave. I had presented our not-very-successful paper at Northwestern that year and received some criticism. Back at GSIA for the summer, Finn and I had extended discussions about what to do differently. We decided to investigate if beginning with the neoclassical growth model as the starting point would be more fruitful. We had to extend that model to include the labor-leisure decision, because business cycles were fluctuations largely accounted for by fluctuations in the labor input. Starting out with the extended neoclassical growth model turned out to be incredibly fruitful.

This exploration was carried out in summer 1979 at GSIA. Finn and I worked away with the programming in the basement of GSIA, and developed a new methodology that has proven so useful. We found that if people are highly willing to intertemporally substitute market time or, in
more popular jargon, “make hay while the sun shines,” and if productivity shocks are persistent and of a certain magnitude, business cycles of the nature observed are a prediction of the extended neoclassical growth model. I used the tools of statistics to estimate the parameters of the process governing the productivity shock. I found that the process was highly persistent and that the shocks were of the magnitude that gave rise to fluctuations of the magnitude observed.

Whether people were as willing in the aggregate to reallocate their work effort over time as needed for the neoclassical growth model to predict business cycles was the open question. Finn and I knew we could get a good fit to the aggregate observation by choosing a high value for the parameter governing this willingness. The needed number was far larger than the one that economists had estimated using panel observations for continuously employed, full-time, prime-age males. A fact for this population was that when compensation per hour worked went up, market hours increased little. This conflict between individual and aggregate behavior was subsequently resolved, and I will come back to this point when I discuss another student with whom I only recently collaborated.

**Rajnish Mehra, student and subsequent collaborator**

Another student at GSIA with whom I have had subsequent fruitful collaborations is Rajnish Mehra. He is the antithesis of Finn. In each of our three collaborations, he initiated the project and stuck my name on the paper at an early stage. Each of these papers addressed a good problem, but the early papers had flaws and required a sizable effort before we had something worth reporting to the profession.

Our “Equity Premium Puzzle” paper, written in 1979 and appearing in 1985, was important in my development as an economist. There we used theory to determine how much of the historical difference in the average return on the stock market and virtually risk-free short-term debt is a premium for bearing nondiversifiable aggregate risk. Our finding was that nondiversifiable risk accounted for only a small part of the difference in historical average returns.

The reason this paper was important in my development as an economist was that it led me to stop thinking in terms of finding the model to fit
some data set to simply using theory and measurement to make quantitative statements. Given that the big difference in these average returns was not a premium for bearing aggregate risk, Raj and I concluded that it had to be for something else. Recently, important progress has been made on finding what other features of reality account for most of the difference in average returns.

We had problems getting the paper published and had given up trying. Robert King and Charlie Plosser came into my office at the Minneapolis Federal Reserve Bank and encouraged the submission of the paper to the *Journal of Monetary Economics*, which we did. This was another stroke of luck. The paper turned out to foster a considerable body of very interesting research.

**Other Collaborators at GSIA**

When I returned from Norway in 1975, there was a new young assistant professor on the faculty: Robert M. Townsend. Being a Neil Wallace student at Minnesota, he was careful in his use of language. He opened my eyes when he told me the way to deal with problems with private information was to add incentive constraints to other constraints that determined whether or not an allocation is feasible. Subsequently, we wrote a couple of papers in which we extended the theory of valuation equilibrium to environments with private information. It was a matter of finding a commodity vector with the needed properties—easier said than done. With this commodity vector, households’ problem is to maximize utility given prices of the commodities subject to their consumption and budget constraints, and firms’ problem is to maximize value given prices and technology constraints. In addition, all markets must clear.

**My Years at Minnesota**

In late fall 1979, to my joy, I received an offer from the University of Minnesota, which was then the best place in macroeconomics and a great match for me, given its abundance of smart, adventuresome students who were well trained in economic theory. I have Tom Sargent to thank for engineering this appointment, along with Walter Heller, who provided some crucial support. This was another stroke of luck.
One student whose dissertation I signed as adviser, but met only briefly a couple of times, was Richard Rogerson. He resolved the puzzle of why aggregate observations were saying people are highly willing to substitute work hours over time, whereas panel studies of individual behavior were saying they were not. The version of Rogerson’s dissertation that I approved had two essays. He presented a short third essay at his defense. He had observed that most variation in aggregate hours worked is accounted for by fluctuations in the number working in a given week and not in the number of hours worked per week per full-time worker. This led him to introduce a labor indivisibility where people either work or don’t work in a given week. He found that the model explained both the micro and macro observations.

Gary Hansen, another of my students with whom in subsequent years I had fruitful collaborations, took the Rogerson labor indivisibility and introduced it into the neoclassical growth model. He found that predicted business cycle fluctuations were even bigger than those observed. His paper was beautifully written, and had all the noncrucial elements stripped from the business cycle model that Finn and I developed. His paper immediately got great attention. This was another stroke of luck.

A few years later Andres Hornstein, another student and valued collaborator, developed a model that endogenized this labor indivisibility. Very recently, Rogerson, his student Wallenius, and I came up with another endogenization, which I think will better interact with measurement, including differences in workweek lengths across occupations. Much remains to be done to ascertain whether my expectations will be realized.

Stephen Parente
At Minnesota I did not teach macroeconomics. I taught a valuation equilibrium and recursive methods course in the macro theory sequence. I taught the students the language of economic theory, not how to use it. You have to know the vocabulary and grammar of a language before you can use it productively. I also taught an industrial organization course where a variety of economic questions were addressed.

In the 1980s, growth and development returned to center stage in economics. I had read Paul Romer's economic development paper and
Bob Lucas’s mechanics of economic development paper on endogenizing growth, and wanted to develop some aggregate models with explicit micro foundations. Some students became very interested in this topic and requested that Minnesota again have a field exam in this area. I shifted my teaching to that area to fill a need, as Minnesota had enough good people willing and able to teach industrial organization.

At that time, everyone was emphasizing the importance of the savings rate and the accumulation of factors of production, including not only physical productive assets but also human capital. Stephen Parente, in a highly original dissertation, took another approach that focused on the role of barriers to efficient production. Countries were poor because they were good at setting up barriers to efficient production. He developed a dynamic micro-based general equilibrium model where barriers were key in determining living standards. After he wrote his dissertation, we began to talk about how to modify his model so that it could be quantified in the hopes of replicating the success of the extended neoclassical growth models in the study of business cycles. This turned out to be a ten-year collaboration that resulted in our book *Barriers to Riches*.

**More Ambitious Exercise**

In our business cycle research, Finn and I focused on the business cycle facts, which were a set of statistical properties of the economic time series. Along with Gary Hansen and Tom Cooley, I addressed the question, Did technology shocks cause the 1990–1991 recession? The answer is yes, but the behavior of these shocks in that period implied a recovery much sooner than occurred. The fact that there is a deviation from theory is evidence that macroeconomics has become a hard science like the natural sciences had become many years before. No longer was macroeconomics an exercise in storytelling with many explanations of every observation. Incidentally, this deviation from theory has been resolved with the inclusion of tax rates, which were increased in the early 1990s, but the importance of taxes for business cycles was only later evaluated and measured.

At the Minneapolis Fed in 1999, Hal Cole and Lee Ohanian broke a taboo and used the neoclassical growth model to study the Great Depres-
sion. Motivated by the study, and the very strong support and encouragement of Arthur Rolnick, director of research, Tim Kehoe and I organized a conference at the Minneapolis Fed on great depressions of the twentieth century. All contributors used the neoclassical growth model to study a great depression. My paper was written with Fumio Hayashi, and it examined Japan’s lost decade of growth between 1992 and 2002.

In that paper, we didn’t know what expectations of future productivity and tax shocks to impute to the model people. We simply imputed the model people with perfect foresight, hoping the findings would be robust to the expectation assumption. As found out later, our hopes were realized. We found that this lost decade of growth, which greatly depressed output per working-age person, was due to a lack of productivity growth. (Why productivity growth was absent is less well understood.)

We found that great depressions in output per working-age person corrected for trend occurred in a number of countries in the last thirty years of the twentieth century. I was surprised that Japan, New Zealand, and Switzerland had experienced great depressions in this period, and not so surprised that a number of Latin American economies had as well.

The Federal Reserve Bank of Minneapolis

Ellen R. McGrattan
Ellen McGrattan addressed the expectations for robustness conjecture and found that our conjecture stood up. She generated equilibrium realizations of a dynamic stochastic neoclassical growth model. She introduced an economist who incorrectly uses the perfect foresight to study the artificial economy. She then compared what that economist would conclude with what an economist who knew the correct expectation assumption would conclude. For all practical purposes, the conclusions are the same.

The Value of the Stock Market

In 1999, it was again time to fill the Federal Reserve Bank of Minneapolis Quarterly Review. More important, Gary Stern, president of the Minneapolis Bank, wanted to know more about stock market valuation and whether there was something the Fed should be doing. My motivation
was not just intellectual curiosity; I was worried about the stock market because that is where all my retirement savings were. I didn’t know what I should do and didn’t think anybody else knew either. There was no scientific basis for saying the market was overvalued or undervalued. The *Quarterly Review* issue had two articles: one written by Ravi Jaganathan and Ellen McGrattan taking the traditional finance approach, and the other written by Ellen McGrattan and me.

Ellen and I took an approach different from the one standard in finance. The standard approach exploits the equilibrium relation that equity values are equal to the present value of dividends. We exploited the equilibrium relation that the value of corporate debt plus equity is equal to the value of its productive assets. With our approach, all the variables used to derive the prediction of the fundamental value of the stock market were current-period national account variables. We innovated in developing a way to deal with unmeasured productive assets using corporate accounting profits. The value of the Coca-Cola brand name is large, as are the value of Microsoft know-how and the value of drug companies’ patents. Our initial finding was that at the end of 1999, the value of the stock market was as predicted by theory. The stock market peaked in March 2000 at a level 15 percent above what it was at the end of 1999, so relative to theory the market was overvalued in March 2000.

We presented this paper in a bag lunch seminar at the Minneapolis Fed. These seminars get a bit wild. Tim Kehoe attacked the findings by pointing out that our theory predicts that the value of the stock market should have been twice as high as it actually was in the 1960s. Tim’s point was well taken. Ellen and I asked each other, What could account for this huge deviation from theory in the 1960s? We spent a week working on the problem. Then Ellen came running into my office with a big grin on her face. She said it was taxes. We, of course, had corporate income taxes and taxes on corporate property in the first version of the neoclassical growth model that we were using. But we had omitted taxes on distributions to owners. Consequently, our estimate of the fundamental value of corporations is a good one only when the tax rate on distributions to households is near zero. At the end of 1999, tax-deferred accounts included pension fund reserves, IRAs, 401(k)s, and the like; taken together they held the majority of corporate equity. Because of these accounts, the
tax rate in 1999 was low, and introducing taxes on distributions (dividends plus part of buybacks) would have decreased the predicted value of the stock market by only about 5 percent. This was not the case in the 1960s, however, when the tax rate on distributions was closer to 50 percent than to 5 percent. With this correction to our model, the value of the stock market in the 1960s is very close to, rather than far lower than, that predicted by the model.

Our paper was rejected by an American journal so we sent it to a British journal, the *Review of Economic Studies*. The editor wanted to see the analysis done for the British economy, which we did. This was a stroke of luck. The United Kingdom had even bigger movements in the value of its stock market relative to GDP than did the United States—a factor of 3 for the United Kingdom versus a factor of 2.5 for the United States. The theory predicted the behavior of the British stock market even better than it predicted the behavior of the U.S. stock market. On this one we owe a debt to the editor for requiring us to use our analysis for the United Kingdom.

Another question that arose at some Minneapolis Fed policy briefings in the late 1990s was why people were working so much. It was not productivity if GDP is used as the measure of output. It was not low tax rates or reforms in labor market policies. We didn’t want to rely on some contagious virus that led people to work more than normal. More seriously, the neoclassical growth model with exogenous population, productivity, and taxes worked well for the U.S. economy up to the 1990s. In the 1990s, observations were greatly at variance with this theory.

People were working more than theory predicted, and accounting profits were not increasing as they did in other booms. We introduced a technology that absent nonneutral technology change, gives the same predictions as the basic neoclassical model. But we permitted nonneutral technology change, which was a reasonable thing to do given the high-tech boom. Many observers said the importance of investment in developing new products and markets for these products was abnormally large. The national accounts do not report this type of investment, and as a result there was an abnormally large amount of this unreported investment in the 1990s. We determined the total factor productivity parameters from both activities, and computed the equilibrium path of the
economy taking as exogenous the paths of these total factor productivity parameters, population, and tax rates. Predicted and actual behavior of the economy matched beautifully. Further theory predicted the abnormally high average capital gains for the period. Theory works. We were a little late in answering Gary Stern’s question about the stock market, but it was answered. Likewise, we were a little late in answering another important question that arose in policy briefings in the late 1990s: Why are Americans working so much?

More on Labor Supply
Teaching was influential in my development as an economist. For political reasons, I never taught the methodology I discuss in this essay at the graduate level. But in the late 1990s, I began teaching it at the undergraduate level. This teaching is time-consuming and challenging. I wanted the students to use the neoclassical growth model to address a question. I had them calibrate the model to the growth facts and use the model to run business cycle experiments. They verified that business cycles were generated by the model if, and only if, people have a high willingness to intertemporally substitute leisure. For the value of this parameter that generated business cycles, I had them evaluate the model using a consumption tax versus an income tax to finance sizable transfer payments to households. The students said there was something wrong with the program, because they could find no income tax rate that would finance the transfer. When I saw this, I knew it was important. The Laffer curve was something of consequence and not an intellectual curiosity, as I had assumed.

This led me to do cross-country analyses using the theory. The finding was that cross-country differences in tax rates account for much of the huge differences in market hours per working-age person. These international observations are well designed to estimate the key parameter because predictions depend on tax rate differences, and these tax rate differences are larger. The marginal effective tax rate includes all factors in the budget constraint that affect the relative price of consumption and leisure. We now know why Europeans work so little in the market sector relative to residents of other advanced industrial countries. Their tax rates are much higher.
The Wise Guy in My Undergraduate Class

There was a wise guy in my undergraduate course—at least he thought he was one. He kept asking why we use one technology for the Malthusian era and another for the modern growth era. I recognized that he was smart and was asking an important question that was not addressed, at least in the quantitative dynamic equilibrium theory I espouse. At the time, I didn’t have an answer and mumbled something about elasticity of substitution. I discussed the problem with a number of colleagues. Gary Hansen, who was visiting the Minneapolis Fed, was one of these colleagues. After extended discussion, I think it was he who had the idea of having two technologies, both of which get better over time. At some point in time, a transition begins from using the traditional land-intensive technology to the neoclassical growth technology. The transition occurs when the neoclassical technology progresses to the point where it is profitable to operate it at the Malthusian wage and rental price of capital. Undergraduates who are smart and not programmed to think one way are valuable inputs into the research process.

Conclusion

In this essay, I reviewed my development as an economist. I see my and Finn’s key contribution as developing a methodology for the study of dynamic economic phenomena. Using this methodology, so many people have contributed so much to advancing our economic understanding. This is the golden age of economics with numerous advancements being made using these tools and extensions of these tools. A rich class of important problems remain that can be addressed using our existing tools of analysis. Another important development is that the data sets available have become richer and have made it possible to address questions quantitatively. I cannot underestimate the importance of measurement in drawing scientific inference.

A number of other valued collaborators were instrumental in my continual development as an economist. They include John H. Boyd and Harold Cole at the Federal Reserve Bank of Minneapolis, and Ayse Imrohoroglu, Javier Díaz-Giménez, Hugo Hopenhayn, and Víctor Rios-Rull, who were my advisees at Minnesota. A list of all those for whom I was a
dissertation adviser or co-adviser, or played a significant advisory role, is at the end of this essay.


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**Selected Books**

*Contractual Arrangements for Intertemporal Trade*, 1985 (ed. with N. Wallace)
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