Learning from Lucas

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This paper recollects meetings with Robert E. Lucas, Jr. over many years. It describes how, through personal interactions and studying his work, Lucas taught me to think about economics.

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1. Introduction

Starting in 1966, Robert E. Lucas, Jr. and other friends generously taught me about macroeconomics. This paper tells how in the early 1970s, together with Neil Wallace, I had hoped to construct, estimate, and optimally control a 1960s-style Keynesian macroeconomic model; how in 1973 Neil and I came to appreciate the way Lucas (1972a) affected our project; and how Chris Sims, Neil, Lars Hansen, and I struggled to respond constructively to Lucas's insights by building, estimating, and evaluating rational expectations macro models. My story is full of starts and stops and accounts of once-promising dead ends. Let me summarize what might be worthwhile messages.

Recollecting parts of my intellectual journey with Bob starts in Section 2 with the story of our first meeting and my early exposure to the professional milieu around him at Carnegie, and how these interactions opened my bumpy road to rational expectations macroeconomics. In Section 3, I describe how in 1970, nine years after Muth (1961) had defined it, I was still unsure about how to define a rational expectation equilibrium, and how a conversation with Ed Prescott helped set me straight. In Section 4, I describe a large obsolescence shock, triggered by the neutrality paper (Lucas, 1972a), that hit me when I was 30 years old—actually, it was an aggregate obsolescence shock that hit the entire macro community. Section 5 provides a short story about my contribution to the creative process that led to the Lucas (1976) critique. I often encountered conflicts between evidence and theories, i.e., between empirical findings and simple models. Thus, in Section 6, I tell how in 1975, contrary to what I had gathered from talking to Neil Wallace, Lucas endorsed my estimation of an ad hoc demand function for money by saying that if theorizing to build deep foundations did not imply a demand function for money that looked much like Cagan's, then it should be ignored. Section 7 is a story about how Bob's idea about two factors underlying US business cycle facts, a nominal and a real one, inspired my paper on index models with Chris Sims, and why Bob didn't publish his comment on our paper. In Section 8, I describe how Bob inspired me to apply recursive methods in a paper of mine on Tobin's q in a general equilibrium.

The mid-1970s was the period when the Lucas critique and the theoretical and empirical work it elicited started reshaping econometric practice. After the dust had settled, macroeconometric practice was no longer what it had been before. Section 9 offers a look into this transformation process by showing that the exchange of ideas between adherents of the new approach and monetary policy was often very direct. In Sections 10–12, I describe how initially Bob urged me to pursue work that deployed the method of maximum likelihood to estimate and evaluate rational expectations macro models, how Bob later told me that this approach was rejecting too many good models, and how that led Bob largely to abandon econometrics for more forgiving calibrations in Prescott's style. It was also thinking about the relationship between calibration and econometrics that led Lars Hansen and me to begin working on bringing concerns about robustness and model misspecification into macroeconomics. A message here is that hearing others and being open to new ideas can send you back to the drawing board and back to school. In Section 13, I tell how, late in our research careers, Bob and I revisited the idea that had originally attracted us to rational expectations—the hunch that it would be fruitful to put the model builder and the econometrician on the same footing, as John F. Muth (1961) had advocated. Section 14 denies that there has ever been a 'rational expectations school' that advocates and agreed upon set of policy prescriptions or a unique macroeconomic model. As an additional story, Section 15 illustrates Bob's careful ways of thinking and writing. Section 16 contains some concluding remarks.

For me, research has always involved socializing and listening to and occasionally having the courage to talk back to larger-than-life personalities, wonderful people including Hyman Minsky, Oliver Williamson, Peter Diamond, Leonard Rapping, Neil Wallace, Chris Sims, Ed Prescott, and many others, who have strong and contending views. This adventure put a charge into learning macroeconomics.

Differences in preferences about how to do scientific economics are mainly about personalities and not about intelligence quotients. Personality differences surface in whether it is better to reason mainly in terms of English words or with mathematical expressions (see the story in Section 9 about Hyman Minsky, my mentor at Berkeley), or the primacy of theory versus econometric evidence (see Sections 10–12 for stories about interactions with Bob Lucas about econometrics and calibration; or the story in Section 15 about whether, without really thinking about it, I was behaving as a Bayesian or a frequentist). When differences in preferences do reflect differences in personalities, some disagreements across very smart researchers cannot be resolved from macro data that are too sparse along the dimensions that would be needed to resolve them.

2. First meeting

Oliver Williamson, for whom I had worked as an undergraduate research associate at Berkeley, suggested that I say hello to Richard Cyert, dean of the Carnegie Institute of Technology, when he visited Harvard for a day in November 1966. I did. Cyert invited me to visit Pittsburgh to meet some faculty members. During that visit dean Cyert offered me a job as a 'research associate' and unlimited computer time while I was finishing my PhD thesis. Cyert knew that I was obligated to report to the US Army as a first lieutenant when I completed my PhD thesis. Cyert told me that a Carnegie assistant professor, Leonard Rapping, had connections with the Defense Department and that Leonard would tell his friends in Pentagon about me. Within the year, Leonard would 'come out' as a Marxist, but at that time he was a bona fide Chicago economist. Leonard arranged for the US Army to assign me to the Systems Analysis Division of the Department of Defense where I worked for Alain Enthoven and other 'whiz kids' in the Pentagon starting in January 1968 after a year at Carnegie. From January 1968 to December 1969, I was First Lieutenant and then Captain in the US Army.

I first met Bob Lucas in his Carnegie office during my November 1966 visit. Bob was reading passages of Neil Wallace's PhD thesis about the term structure of interest rates. I had read Neil's term structure papers before that, but that was the first time I had met someone who actually knew Neil personally.

I was 23 years old and wet behind the ears when in January 1967 I joined Carnegie for 12 months before reporting to the Army. What an opportunity! Mike Lovell and Allan Meltzer were in their late 30s and very generous senior colleagues. Mike Lovell told me to read two papers by John F. Muth about rational expectations. Allan Meltzer taught me about the Gibson paradox and Cagan's and Friedman's latest works. Bob Lucas was an assistant professor. So was his best friend Leonard Rapping, along with Nancy Schwartz, Morten Kamien, Richard Roll, John Ledyard, Peter Frost, Mel Hinich, and Toby Davis. Very active senior colleagues included Herbert Simon and W.W. Cooper.

Edward Prescott and Dale Mortensen had just graduated from Carnegie. The junior faculty told admiring stories about both of them and their works. I learned to program in Algol because that was the language Herb Simon recommended. Mike Lovell shared with me a four-line Algol program for matrix inversion that John Muth had written.

Every day about 3:30 PM, Bob Lucas and Leonard Rapping headed to the lounge to drink coffee and discuss economics with passion and intensity. I listened. Bob and Leonard were in the process of creating their model about the aggregate labor supply that featured intertemporal substitution and adaptive expectations (Lucas & Rapping, 1969). I knew enough to appreciate the importance of what they were working on because a couple of my undergraduate professors had told me that the most important deficiency of the then-existing Keynesian macroeconomic models was the absence of a quantitatively credible aggregate supply function. As a sophomore I took macro from a visiting professor, Meredith Clement from Dartmouth. Clement told us that while people like Duesenberry and Friedman had become famous by 'quantifying the aggregate demand curve', it remained wide open for someone else to become famous by 'quantifying an aggregate supply curve'. I did not understand what Clement meant when I heard that as a sophomore, but I remembered it as I listened to Bob and Leonard during those afternoon coffee sessions. I recognized that they were doing something important. The path from Lucas and Rapping's (1969) work to Lucas's (1972a) neutrality paper might seem obvious to many people now, but in 1967 it was not obvious to me.

At Berkeley, where I was an undergraduate and at Harvard and in Cambridge, Massachusetts, where I was a graduate student, my teachers were mostly prejudiced against 'Chicago economics', especially the types practiced by Milton Friedman and George Stigler. I mean 'prejudiced' in a respectable scientific sense—they used different models than Friedman and Stigler did, and often passed on impressions that Friedman and Stigler were using obsolete theories. I had absorbed the 'Cambridge Mass' opinion that Milton Friedman's way of doing macro was obsolete technically. Friedman used no Cowles Commission style simultaneous equations models like those that characterized the Brookings model and models then being built at Penn—just some distributed lag single equation regressions that Friedman overinterpreted. I had accepted those prejudices, but nevertheless I had read some of Friedman's papers as well as ones by his students Phillip Cagan, David Meiselman and James Meigs. I had never actually met a ‘Chicago economist’ in person until I met Bob and Leonard.

Bob and Leonard were serious about using competitive equilibrium analysis as a tool for both micro and macro. Still being a passionate Keynesian who had been taught to want macroeconomic *dis*equilibria, whatever that meant, I was cautious and quiet and learned by listening. Being terrified, I gave no seminars the year I was at Carnegie Tech. I collected papers and books that I hoped to find time to read and think about during those intermittent ‘hurry up and wait’ interludes that army life sometimes brings. I could not understand Muth’s papers after one reading, or even five or ten. I had to start learning something about stochastic processes, linear least squares prediction theory, z-transforms, and some fixed-point theorems before I could approach an understanding of what Muth was up to. That would have to wait until I left the Army in early 1970.

3. Edward C. Prescott, 1970

I joined the economics department at the University of Pennsylvania in January 1970. In the fall of 1970, I wrote a four-page note on testing the accelerationist hypothesis about the Phillips curve. I gave a copy to Ed Prescott who had an office two doors down the hall. Ed returned to my office the next day, set the paper on my desk, and said ‘there are two definitions of rational expectations, yours and Lucas’s’. Then he walked back to his office. That was a long conversation for Ed. I had no idea about what Ed meant, and having little confidence in myself technically, I took it to mean that Ed thought that what I had written was mistaken and that I should learn more before trying to write something. Fair enough. Years later, while jogging, I suddenly understood what Ed had told me: Lucas had defined a rational expectations equilibrium as a fixed point in a space of functions of Markov state vectors, while I had defined it as a fixed point in a space of stochastic processes (i.e., random sequences indexed by time). Both perspectives are useful. Ed saw that immediately and had not meant to insult me, or at least that is what I now think.

4. Destruction

In June 1971, I moved from Penn to Minnesota. In 1971 at the University of Minnesota and the Minneapolis Fed, I worked with Neil Wallace on a two-part project that involved (1) constructing and then econometrically estimating a then-modern Keynesian macroeconomic model that was small enough that we could then (2) apply to control theory to deduce optimal decision rules for monetary policy. In that way we could organize quantitative evidence that would shed light on the issue of whether the Fed should use a money supply rule or an interest rate rule together with details about how the optimal monetary instrument should feedback on the Fed’s information variables. Bill Poole (1970) and Martin Bailey’s (1962) textbook had described how the choice between a money supply or interest rule (or ‘target’) hinged on the magnitudes of variances and covariances of shocks hitting salient demand curves in a Keynesian model. Neil and I were applying simultaneous equations econometrics to a tightly specified Keynesian model of manageable size to infer the variances and covariances that decided the money supply versus interest rate rule question formalized by Bailey and Poole. I had started working on this project with Neil when I was at Penn and continued during my first couple of years at Minnesota. This involved writing Fortran programs to deploy simultaneous equations estimators and reading about stochastic control theory. Key decision rules in our model incorporated distributed lags justified in the then-modern way as partly reflecting agents' forecasts of future prices and quantities via 'adaptive expectations'.

We worked in open carrels at the Minneapolis Fed. Mine was next to Neil's. One morning in early 1973, Neil popped his head over the partition between our offices and said 'Tom, our project is fatally flawed and over.' I said 'Because?' During the next hour Neil told me that he had read Bob's *Expectations and the neutrality of the money* (Lucas, 1972a) carefully; that Bob had done things the way that we should have if we had known more; that Bob had incorporated rational expectations (something that I had been struggling to learn in my own clumsy way); and that Bob had shown that a Friedman *k*-percent money growth rule was, in a reasonable sense, Pareto optimal—a result that did not depend on the variances that Poole had highlighted and that Neil and I had painstakingly been trying to estimate. I agreed that our project was over. That was a big obsolescence shock.

Over the next three or four days I read Bob's paper as well as I could. There were big inadequacies in my mathematics and economic theory, so I had to read around them and temporarily 'fake it'—taking lots of things on faith. For example, I did not know what a complete metric space or a contraction mapping was. But after those three or four days I understood that Neil had been correct and that it was time for me to think about things differently and to learn some of the tools that Bob had deployed with such force. I had to go back to school and retool if I aspired to become a macroeconomist.

5. Ford Hall Conference, 1973

In the spring of 1973, I organized a small conference on rational expectations at the University of Minnesota in Ford Hall on the East Bank campus. Attendees were John Muth, Ned Phelps, Karl Shell, Ed Prescott, Neil Wallace, Bob Lucas, a nineteen-year-old Sandy Grossman, and me. I had convinced Jack Muth to give a paper that he had kept in a drawer until then—Albert Ando had told me about it. The paper described how to estimate a rational expectations model properly. I introduced Jack Muth as the father of rational expectations. An understated person, Jack softly corrected my introduction and said 'Richardson is the father of rational expectations.' (I suspect that Jack was referring to Richardson's arms race model, but I do not know. I wish I had asked him.) Bob and Ed gave an early version of their *Equilibrium search and unemployment* (Lucas & Prescott, 1974). Karl and Ned understood models and issues very quickly and asked sharp questions. What a day!

The conference was on Friday. Everyone flew home Friday night. On Saturday morning, Rita Lucas phoned. Before heading off to play baseball, Bob had asked her to call me. Bob had misplaced a folder with a draft of a paper for an upcoming Carnegie-Rochester conference he was writing at the request of Karl Brunner. Would I return to the site and a search for the folder? I drove to the university and went to the Ford Hall classroom where we had held the conference. The room had not been cleaned. I found a folder and looked inside: yellow pages in Bob's handwriting with the title *'Econometric policy evaluation: A critique'*. I called Rita to tell her I had found the folder. I put the paper in the mail to Bob on Monday, thereby contributing in an important way to the working out of the Lucas critique (Lucas, 1976).

6. Chicago, fall 1975

Milton Friedman and Bob invited me to give a talk at the Money and Banking Workshop. I gave a paper implementing maximum likelihood for a rational expectations version of Phillip Cagan's model of hyperinflation (Sargent, 1977b). My paper brought together Granger–Sims causality and rational expectations in a way that, in a bivariate context, money growth and inflation in this case rationalized Cagan's adaptive expectations scheme in the same way as John F. Muth's (1960) univariate inverse optimum prediction exercise had rationalized Milton Friedman's adaptive expectations formula for permanent income.

Bob invited me to dinner at his house the night before the seminar. At that time, I was watching Neil Wallace rework monetary theory from the ground up and remarked to Bob that I had reservations about working with Cagan's model, even under rational expectations, because the heart of the model was an ad hoc demand for real balances understood as an inverse function of the public's anticipated rate of inflation. Neil had convinced me that empirical work really should wait until the foundations of monetary theory had been properly set forth and provided a deep enough theory of valued fiat money. Bob shot back immediately that 'if theorizing to build deep foundations do not imply a demand function for money that looked much like Cagan's, then it should be ignored for empirical work'.

That conversation said a lot about Bob's and Neil's distinct approaches to theory and empirical work in those days; or at least what each of them then thought about the depth of a theory appropriate for doing enlightening empirical work. William James said that different philosophies of how to do science ultimately reflect personality.

Something else funny happened that night. It was the first time I had been in Bob's house, and he gave me a tour before dinner. We went into Bob's study. Nice room. After a minute or so, Bob looked at his desk and said 'oh no'. A reading light was propped up by a thick typescript in a tell-tale green cover. It was an early draft of a paper of mine (Sargent, 1979) that I had circulated among friends. Bob had put my manuscript to good use.

7. Chris Sims, Minneapolis Fed conference, 1975

Chris Sims organized a conference about new methods for econometrically studying business cycles at the Minneapolis Fed in 1975. A conference volume appeared in 1977 (Sims, 1977). Bob discussed Chris and my paper entitled *Business cycle modeling without pretending to have too much a priori economic theory* (Sargent & Sims, 1977). I viewed the paper as a systematic way of organizing data via a 'factor analysis' model along lines that Koopmans (1947) had suggested in his critical review of Arthur Burns and Wesley Mitchell's (1946) *Measuring business cycles*. Bob taught Burns and Mitchell's methods and findings in the University of Chicago PhD macro that I had audited later, in 1976. Bob used their findings to motivate features to be included in a structural macroeconomic model—i.e., an equilibrium model with artificial people who solve stochastic control problems in a coherent environment. A key point that Bob had made in class was that Burns and Mitchell in effect required two hidden factors to fit US business cycle facts, a real factor and a nominal factor. The idea underlying a factor model is that all correlations and autocorrelations among components of a vector of random variables are intermediated through their common dependence on a small set of factors. My contributions to Sargent and Sims (1977) came from thinking about what Bob's lectures on Burns and Mitchell's work implied about macro modeling strategies and from discussions with John Geweke, then a graduate student at Minnesota, about how to create a frequency domain version of a classic factor analysis model. John, Chris and I went to the frequency domain because we wanted to study the cross-autocovariance structure of a vector time series.

I wrote a note about that and showed it to Chris, who indicated that he had been thinking about related things, and we joined forces to write what would eventually become our aforementioned paper (Sargent & Sims, 1977). At the conference, Bob discussed that. He captured what I thought were its key implications—including the evidence for two factors, one real, the other nominal. The two-factor feature of US data cast doubt on the adequacy, say, of a Brock–Mirman optimal growth model driven solely by a technology shock to explain US business cycles. I thought that, taken together, the first draft of our paper (Sargent & Sims, 1977) and Bob's discussion of it proclaimed that message loud and clear. But then Chris and I revised our paper, again and again, sending each new draft to Bob. Bob said that he would wait for the Chris–Tom revision process to converge before revising his written comments. Convergence never happened. (Bob was applying a convergence criterion appropriate to a monotone increasing sequence.) Bob said that 'every revision of your paper got worse.' I regret that Bob's discussion of the first draft of our paper was not published in the conference volume because it had carried important insights about what Chris and I were trying to do. From the sequence of revisions Bob read, perhaps he sensed, accurately that Chris and I did not completely agree on how congenial our statistical approach was to the structure of the two-factor business cycle model Bob was constructing.

8. Chicago, 1976–1977

I visited the University of Chicago for the academic year. I taught a small section of PhD macro in the fall and Bob taught a large section in the winter. I audited Bob's class—that surprised some of the students who had taken my section of the class. I also audited Bob's advanced class about recursive methods. It covered Bob's early notes that would eventually grow into the textbook *Recursive methods in economic dynamics* (Stokey, Lucas, & Prescott, 1989). I also audited Buz Brock's classes on mathematical economics—a wonderful class on dynamics with continuous time methods.

The next year I returned to Minnesota and taught many things Bob and Buz had taught me. As part of my process of studying the recursive methods Lucas had taught in his second-year class, I wrote a paper on Tobin's *q* in general equilibrium that I thought of as a 'term paper' for Bob's class. In the spring of 1977, I sent the paper to Bob and also to Buz Brock and Jose Scheinkman. I told Bob that I regarded it as a term paper for his class and got up the courage to ask him whether he might want to join me as a coauthor and try to publish it. As soon as Buz and Jose received my paper, they tried to convince Bob to give me a B+ grade for my term paper. I heard about this from Buz and Jose, who were disappointed that Bob would not participate in their prank. Bob eventually wrote a cordial letter saying he did not want to be a coauthor but that did not mean that he thought it was a bad paper. My paper became *'Tobin's q' and the rate of investment in general equilibrium* (Sargent, 1980).

9. Martha's Vineyard

In 1977 I wrote *Is Keynesian economics a dead end?* (Sargent, 1977) and used it as text for a talk at the annual Minnesota Economics Association meeting. Hyman Minsky, my teacher at Cal, attended my talk. Minsky told me how disappointed he was in me. Minsky said that when he had known me as an impoverished and left-leaning student at Berkeley he would never have predicted that I would 'change sides'. He said 'What are you doing? Why?' That saddened me. I saw rational expectations as a technical and non-ideological tool, a step on the path to a longstanding project of providing microfoundations for Keynesian economics—Tobin and Modigliani's and Solow's and Jorgenson's research project, and one with special econometric promise because of how it economized on free parameters.[1]

I learned that Bob had read the paper (Sargent, 1977) when he called me on the phone to tell me that the Boston Federal Reserve Bank research department director had invited him to write for the Bank's annual Martha Vineyard Conference—a paper that would explain in 'plain English' what rational expectations macroeconomics was really about, unencumbered by equations. Bob proposed that we join forces and write a joint paper for the conference, so we wrote *After Keynesian economics* (Lucas & Sargent, 1979) and we both traveled to Martha's Vineyard in June to attend the conference.

I presented our paper at Bob's request. Ben Friedman discussed it. Ben wanted the audience to know that he really disliked the line of research described in the paper. He disliked it so much that he announced to the audience that when he quoted or paraphrased us, he would put on a black hat and that when he said what he himself thought he would wear a white hat. So, Ben switched hats on and off provoking laughter from the audience each time he switched hats. The audience liked that. Bob was not amused, nor was I. But actually, the hat switching was the best part of Ben's discussion. Ben did not have much of value to say other than that he did not like what we were up to.

Mark Willes, the new president of the Minneapolis Fed, also attended the Martha's Vineyard conference. Bob had not met Mark before the conference. After our session, Bob and I had lunch together and Mark joined us. Mark was quite young and looked even younger than he was. Bob asked Mark what he did. He said he worked at the Minneapolis Fed. Bob asked Mark if he were one of my research associates at the Bank. Mark just said no, he had a less important job. I then clued Bob into Mark's being the president of the Minneapolis Fed. After a good laugh about that, Mark told us that he thought that the session about our paper was very disappointing, as was the rest of the conference. That cheered up both Bob and me. Mark asked me at lunch if I could arrange to have a few people who knew what was going on and were actually participating in

[1] When I think about that conversation after all these years, I realize that I had actually attempted to change sides. When I left Cal, I knew very little math and had aspired to be what Leo Hurwicz called a 'literary economist' who avoided equations and wrote about economics with English words, not geometry and calculus. Somehow in graduate school I 'changed sides' by wanting to learn the mathematical language that had enabled Samuelson, Solow, and Tobin to create coherent models; and to learn the econometrics that had empowered Dale Jorgenson, Zvi Griliches, John Meyer and others to check whether their models were consistent with data. Probably, it was those tools that Hyman Minsky disrespected.

modern macroeconomics to come out to Minneapolis and spend a day in seminar room to discuss things and let him watch, listen, and learn. Bob agreed that would be a good idea.

Within a month, John Taylor, Guillermo Calvo, Chris Sims, Neil Wallace, Bob, and I gathered with Mark Willes around a table in a seminar room at the Minneapolis Fed and talked issues that Mark thought should have been talked about at Martha's Vineyard. Mark asked some questions and listened and stayed throughout the entire day (unusual behavior for a Fed president then or now). Each of us reported what we were working on and how it bore on foundations of quantitative policy evaluation. We talked about the 'if-then' structures of various 'policy ineffectiveness' propositions that had caused a stir on the East Coast. John and Guillermo talked about how changing some of the 'ifs' by some choice-timing decisions would alter the 'thens' in ways that would change both quantitative implications and optimal policies. I am almost certain that they talked about some staggered wage setting models and some Poisson-arrival price-setting-choice opportunity models. John emphasized that rational expectations econometrics provided the tools for implementing such models quantitatively. Chris Sims talked about his way of extending T.C. Liu's (1960) skepticism about sources of econometric identification brought by economic theory, and argued that we were kidding ourselves in thinking that doubts like his could be eased just by doing rational expectations and general equilibrium theorizing. Neil described his road map for redoing theories about the interactions between monetary and fiscal policy—a route that would very soon turn up in the Modigliani–Miller theorems that I view as completing a line of work that Tobin and Brainard did but with appropriate general equilibrium tools. It was a wonderful intellectual event—things were discussed at a more serious and fruitful level as compared to Bob and my 'plain English' paper (Lucas & Sargent, 1979).

10. Econometrics

Although Lucas (1972b) and Sargent (1971) had described how to test the natural rate hypothesis while incorporating pertinent cross-equation restrictions brought by imposing rational expectations hypothesis, in 1971 and 1972 I had not yet digested how subversive the rational expectations hypothesis was of the classic Cowles Commission rank, order and exclusion restrictions that Neil Wallace and I, along with other Keynesian macroeconometricians, were then routinely using. But I got the message immediately when in the fall of 1973 I carefully read a typescript of the later Lucas critique (Lucas, 1976). Franklin Fisher's (1966) book on identification summarized his and other leading econometricians' view in a passage pointing out that economic theories typically provided exclusion restrictions but only rarely provided cross-equation restrictions. Lucas (1976) added investment tax and consumption function examples to the Phillips curve of Lucas (1972b) and Sargent (1971). These examples illustrate how decision rules that Keynesian macroeconomic models required to be fixed under the policy interventions studied in applications of optimal control theory to designing monetary and fiscal policies would instead respond systematically to those policy interventions in ways described by cross-equation restrictions brought by rational expectations. To be useful for policy evaluation, macroeconometrics had to be reconstructed.

As I understood it, Lucas (1976) not only criticized existing practices but sketched a way to do econometric policy evaluation properly. When decision makers face dynamic decision problems and have incentives to forecast taxes and other government policy actions, their optimal decision rules depend on the government's decision rules. To understand that dependence requires the knowledge of the parameters of preferences and technologies that shape private agents' environments. In this way, the cross-equation restrictions implied by private agents' optimum problems became the hallmarks of rational expectations econometrics that is adequate to support macroeconomic policy evaluation.

Bob thus handed me a gift in the form of a research program that was to occupy me throughout the 1970s and 1980s. Bob enthusiastically and generously encouraged me from the beginning—I say generously because it really was his research program and he had indicated the essential components. I learned by doing. I attempted to apply rational expectations econometrics with its hallmark cross-equation restrictions to a string of classic macroeconomic decision rules or equilibrium outcomes: dynamic labor demand, the demand for money, the consumption function, the term structure of interest rates, the Laffer curve for inflationary finance, and so on. For me, these were ideal laboratories for learning and testing alternative estimation strategies.

A single idea unified these superficially different applications. A rational expectations model is a joint probability distribution over a vector sequence of observable variables indexed by free parameters that restrict the preferences, technologies, information flows, and government policies confronting the people who live inside the model. The direct problem was to compute and sample from that joint probability distribution given a parameter vector (i.e., to simulate the model). My job as an econometrician was to solve the *inverse problem*, which was to assemble a time series of observables, and from them to make inferences about the parameter vector. In all of my applications from the 1970s and early 1980s, my tool for solving the inverse problem was to use the method of maximum likelihood.

Bob encouraged this research program in important ways. For example, Bob published several of my papers on rational expectations econometrics in the Journal of Political Economy**.** He also arranged for me to give the Mary Elizabeth Morgan prize lecture (Sargent, 1981) about rational expectations econometrics at the University of Chicago in the spring of 1980. And Bob and I collaborated to write *After Keynesian macroeconomics* (Lucas & Sargent, 1979) and the introduction to the compilation *Rational expectations and econometric practice* (Lucas & Sargent, 1981), both of which are mainly devoted to advocating rational expectations econometrics.

My work on rational expectations econometrics got a huge boost when Lars Hansen teamed up with me in the late 1970s to write our *Formulating and estimating dynamic linear rational expectations models* (Hansen & Sargent, 1980a) and a sister paper about multivariable factor demands (Hansen & Sargent, 1980b) in Lucas and Sargent (1981). Those papers presented details about how to construct and interpret maximum likelihood estimates of rational expectations models.

11. Many macro specifications work well?

In applied macroeconomic circles before rational expectations, conventional wisdom suggested that aggregate US time series contained insufficient evidence to discriminate among alternative contemporary specifications of macroeconomic models. Those specifications incorporated profligately parameterized decision rules that included distributed lags often justified informally as reflecting adjustment costs and backward-looking expectations formation rules. Early rational expectations models markedly reduced the dimensions of spaces of free parameters by eliminating all free parameters indexing peoples' expectations of future variables.[2]

Peoples' personal conditional probability distributions over variables that they have an incentive to forecast are the outcomes and not the inputs of a rational expectations model. From the perspective of an econometrician who is interested in parameter parsimony, a rational expectations model has the virtue of eliminating *all* free parameters measuring peoples' expectations. A by-product of this feature of rational expectations models was to sharply restrict observed time series.

Maximum likelihood estimation of rational expectations models automatically invited us to conduct likelihood ratio tests of the rational expectations restrictions used to identify the free parameters that would be essential for constructing those functionals required to do econometric policy evaluation in a way that would be immune from Bob's *Critique* (Lucas, 1976). I regularly conducted such tests in a number of papers from the 1970s and early 1980s. Likelihood ratio tests spoke against the models.[3]

Those negative tests did not prevent me publishing those findings. Lars Hansen also reported rejections of some rational expectations models in the early 1980s. Lars and I both regarded these rejections as reflecting a *half-empty, half-full* situation. Sure, sometimes a rejection disappointed us because we had liked the model and thought it was elegant. But we thought that there were gains to be reaped from drilling down and studying how the data conflicted with our models. That could give us hints about fruitful dimensions along which our specifications could be improved. This was a recipe for using evidence to improve usable theories.

12. Too many rejections?

I was surprised to learn that Bob was not completely on board. In a conversation at the Econometric Society meetings in the spring of 1982 at Cornell, Bob told that 'those likelihood ratio models of yours are rejecting too many good models.' Bob was convinced that a quantitative strategy illustrated in early drafts of Finn Kydland and Ed Prescott's (1982) paper about real business cycles was a more promising approach than using rational expectations econometrics. Finn and Ed's idea was to import some parameters from sources extraneous to the model under study, to use a two-sided filter so as to remove low-frequency patterns not under study, and then to calibrate the remaining parameters by invoking the law of large numbers to match some sample means.

A good start for an argument in favor of Kydland and Prescott's 'calibration approach' rather than maximum likelihood was in the air. The method of maximum likelihood provides a good estimator if a model is correctly specified. But if you regard your model only as an approximation to a better model that you cannot decline to describe, you should not use maximum likelihood; for if you do, you will make errors of inference of a kind that Christopher Sims (1971; 1972; 1974) had described formally in a string of papers in the early 1970s.

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[2] Jorgenson (1967) called for unifying disparate price sequences in a model of money and growth.

[3] Linear rational expectations imply restrictions on vector autoregressions that can readily be subjected to likelihood ratio tests.

Although they did not, Finn and Ed could have appealed to Chris's analysis to justify parts of their calibration procedures.

13. Specification doubts?

Inspired partly by Chris Sims' early work on econometric consequences of mis-specification and by Bob's endorsement of Ed and Finn's advocacy not to use inference methods entailing likelihood functions, Lars Hansen and I had embarked on a research program to refine rational expectations theorizing and inference by importing tools from robust control theory that start by acknowledging up front a decision maker's concerns that his or her model is mis-specified (Hansen & Sargent, 2008). We presented parts of this research to Bob over the years. At a seminar at the Minneapolis Fed around 2005, Bob asked me privately after the seminar, 'Why should the agents inside our models be like us?' in the sense of worrying about model misspecification. My immediate reaction was that as early followers of John F. Muth (1961), Bob and I did not get to ask that question. Muth advocated putting the theorist and the econometrician on the same footing as the agents inside his model. Lars and I thought that we were remaining faithful to Muth's recommendation, maybe even more than when we had been doing 'pure' rational expectations econometrics.

14. Real bills doctrine and quantitative easing

In Spring 1981, Bob invited me to speak at the Money and Banking Workshop at the University of Chicago about my paper on the real-bills doctrine with Neil Wallace (Sargent & Wallace, 1982). The paper can be viewed partly as formalizing an argument from Chapter V of Book I of Adam Smith's *Wealth of nations*. In that chapter, Smith described what later came to be called the 'real-bills doctrine' and made a qualified argument for free banking in the context of a gold standard. Smith conducted a mental experiment in which a private bank issues paper notes that it promises to convert into gold coins on demand. The bank would back those notes with assets that consist mostly of safe commercial loans and just enough gold coins to be able to honor calls to convert bank notes into gold coins on demand (it is a fractional reserves arrangement). Smith used the term 'real bill' as synonymous with 'safe commercial loan'.

Smith analyzed the consequences of a financial deregulation in a small open economy that had always prohibited banks from issuing small denomination IOUs (bank notes) that might compete with gold coins. The initial condition for Smith's experiment was a precise version of the 'narrow banking' or 100% reserves regime that had been advocated in the Chicago plan of Henry Simons, a plan later recommended, albeit with modifications, qualifications, and hesitations, by Milton Friedman in his 1960 book *A program for monetary stability* (Friedman, 1960). Smith argued that removing the prohibition and allowing banks to issue low-denomination bank notes backed by real bills would (1) have no effect on the domestic price level (the notes would be as good as gold coins as a medium of exchange), and (2) lead to a one-time boost in consumption in the form of imports financed by exporting the gold coins crowded out by paper bank notes.

In the paper Neil and I (Sargent & Wallace, 1982) compared quantity, price, and welfare outcomes in a free-banking system with a 100% reserve regime in a heterogeneous-agent overlapping generations model in the tradition of Samuelson's (1958) classic 'social contrivance' model of a valued unbacked money. To Neil and me, this model seemed a natural environment in which to study how a currency's backing, the asset side of a bank or central bank's balance sheet, affected its equilibrium value. The quality of backing was the focus of Smith's theory. Quality of backing also seems relevant for assessing contemporary 'quantitative easing' experiments. In 1981, there were attractive alternative theories of valued fiat money that deemphasized backing and instead emphasized a money's role in relaxing liquidity constraints. Liquidity constraints and cash-in-advance models were clearly on the minds of Bob and other seminar participants as attractive alternatives to Neil and my model, which had completely ignored liquidity constraints and downplayed the transactions role of money, while instead emphasizing its store of value role.

For me, the seminar was an enlightening event that illustrated, yet again, that there was no such thing as a 'rational expectations school'. All of the contending models on the table at that seminar used a rational expectations equilibrium concept. And all struggled with how to introduce an unbacked fiat money into a somewhat standard general equilibrium model with enough monetary and fiscal tools present to analyze classic macroeconomic policy questions.

Milton Friedman was not physically present at that seminar, but I felt his presence. In his *A program for monetary stability*, Friedman (1960) mentioned that Gary Becker had almost persuaded him to endorse free banking rather than the 100% reserves, narrow banking proposal that Friedman instead advocated. Friedman modified Simons' original narrow banking proposal by requiring that the central bank pay interest on reserves at the nominal rate on safe bonds. Friedman pointed out that those interest payments would have to be financed, and he mentioned alternative ways of financing them. Depending on which financing method is adopted, a narrow banking regime becomes virtually indistinguishable from a free-banking regime. Friedman revisited some of these issues in one of his last published papers written jointly with Anna Schwartz.

15. Neyman–Pearson and Bayesians

In fall of 1981, I gave a literary (no equations, no econometric estimates) paper about Poincaré and Thatcher at an NBER conference in Chicago that eventually became my *Stopping moderate inflations—The methods of Poincaré and Thatcher* (Sargent, 2013). In the public conference discussion, Herschel Grossman asked me whether, if were in charge, would I execute a policy like that of Thatcher or Poincaré. I responded 'it doesn't matter what I think', but after the conference I was dissatisfied with that answer. Bob soon wrote me a letter about this. Bob said that Herschel was taking a Bayesian line that Neil Wallace had also urged on me, namely, that a well-formulated research project should take the form of a decision problem. Bob contrasted that with the Neyman–Pearson statistical hypothesis testing tradition. Bob interpreted my answer to Herschel as reflecting my adherence to the Neyman–Pearson approach. After elegantly comparing the two approaches, Bob said that he preferred the Neyman–Pearson approach. Bob had listened to the exchange between Herschel and me and transformed it into a sharp statement of an enduring issue about how to organize research.

16. Concluding remarks

A referee of an earlier draft told me that a reader could come away thinking that doing macroeconomics is not fun. That is not what I had hoped to convey. To the contrary, for me it has been a joy to work with remarkable people devoted to the noble goal of taming business cycles and improving macroeconomic outcomes.

I end this essay with another story about Bob Lucas. At the University of Chicago in the early 1990s, some administrative people had arranged a surprise lunch time birthday party for Jose Scheinkman, completed with cake and candles. Jose's administrative assistant was supposed to lure Jose into the seminar room where we had gathered to surprise Jose and sing happy birthday. She could not get Jose to leave his office because he was busy working. Bob volunteered to try to retrieve Jose. Within a few minutes, Jose and Bob entered the seminar room and we all shouted 'surprise'. How had Bob done it? He had told Jose that he had put some formulas on the black board of the seminar room that had finally let Bob understand the martingale convergence theorem. If you think this story is funny, then maybe you will also appreciate why I think that doing macroeconomics in the age of Lucas is so much fun.

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