Points of Departure

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1 Introduction

Macroeconomics and econometrics are tools for recognizing patterns in data and interpreting them in ways that distinguish cause from coincidence. What attracted me to macroeconomics were its noble goals of identifying the causes of economic depressions and of inventing government policies to promote prosperity. In school, I learned that modern macroeconomics required more math than I knew. Therefore, for years at Minnesota I audited math classes. It was easy to select classes. If I sought to understand economics papers about X, then I wanted math Z. During math classes, I'd get ideas for papers and recognize how to solve economic problems that had stumped me.

2 Macroeconomics from 1930 to 1968

Macroeconomics began in the 1930s with a catastrophic depression, the construction of national income accounts by Colin Clark, Simon Kuznets, and Richard Stone, and a 1936 *General Theory of Employment, Interest, and Money* by John Maynard Keynes. Keynes combined assumptions about the main national product components and their interactions into a theory of the level of national output. Depression data had convinced Keynes that drops in demand, not supply, had driven output down and unemployment up. Intuition led Keynes to postulate special demand functions for consumption and investment. He guessed a demand for aggregate consumption that is an affine function of aggregate income. He posited that the aggregate demand for investment is a decreasing function of an interest rate. An interest rate was also a key variable in Keynes's demand function for money.

For many years, people debated 'what Keynes meant'. (No one asked 'what did Robert Lucas or David Kreps mean?') Its mostly non mathematical style is one feature that made the *General Theory* difficult to understand, but not the only one. Keynes could write clearly (read his *Tract on Monetary Reform* or *Indian Currency and Finance*), but he didn't in much of the *General Theory.* A list of insights and conjectures doesn't add up to a coherent model. Labels like 'fundamental psychological law of consumption' are bluffs that don't describe what motivates people. Keynes's *General Theory* is confusing because Keynes was probably confused.

Keynes's literary style reflected his equipment. Important tools that now serve modern macroeconomics hadn't been invented. Keynes stressed people's expectations about future outcomes as volatile determinants of aggregate investment, but because he had no theory of expectations, he treated them as exogenous variables. Wiener and Kolmogorov created a statistical theory of prediction during World War II. Kalman invented a recursive version of that theory in 1960. Wald and Bellman invented dynamic programming in the 1940s and 1950s. Von Neumann, Morgenstern, and Savage completed their theories of expected utility only in the 1940s and 1950s.

Keynes meant his *Theory* to be *General* in the sense that it explained big transient declines of output and employment that could not be accounted for by the classical economics that Keynes said continued to be relevant near full employment. Keynes was both conservative and optimistic in advocating that aggregate demand be manipulated to put the economy in positions where classical microeconomics principles apply. Faith in that 'neoclassical synthesis' continues to influence macroeconomics.

Because they thought that Keynes's *General Theory* was obscure, John R. Hicks in 1937, Trygve Haavelmo in 1939, and Franco Modigliani in 1944 wrote down what they inferred was Keynes's theory as sets of simultaneous equations (n unknowns in n equations) comprising a static macroeconomic model. These equations became foundations of a three-front research program called Keynesian economics that dominated scientific macroeconomics until the early 1970s. First, adding explicit sources of dynamics and random errors created stochastic models that could be compared with time series data. Properly formulating and estimating Keynesian econometric models were principal goals of the research of Marschak, Koopmans, Hurwicz, and others at the Cowles Commission. Second, deeper and more realistic foundations were sought for each of the demand functions comprising a Keynesian model: deeper by providing an explicit theory of the motives and constraints underlying that decision rule, and better in fitting both aggregate time series and cross-section micro data. Finding micro foundations meant deducing a decision rule for a component of aggregate demand or for the demand for money from a constrained optimization problem. A third research line opened by the Haavelmo-Hicks-Modigliani contributions and their econometrics-ready successors was the Tinbergen-Theil research program of computing optimal government policies for an analogue economy.

Using the notion of econometric identification to disentangle cause from effect was an essential part of this three-front program. Key discoveries about identification were made by researchers at the Cowles Commission who married econometrics to policy analysis by distinguishing structural parameters from reduced form parameters. Structural parameters are objects that an economic theory asserts remain fixed when government polices change. Reduced form parameters characterize population regressions of endogenous variables on exogenous and lagged endogenous variables. Reduced form parameters are tangles of structural parameters. An economic theory asserts how reduced form parameters change when government policies change. An economic model's structural parameters are said to be identified if they can be uniquely disentangled from reduced form parameters.[1] Identification requires that a theory assert that some things remain constant while others vary. To get identification, the Cowles Commission extended the idea that forces affecting quantities and prices can be sorted into two disjoint categories – one operating exclusively through demand, another exclusively through supply. Supply curves are supposed to remain fixed when demand shocks arrive; demand curves are supposed to remain fixed when supply shocks arrive. Such "exclusion conditions" for identification were immediately and widely applied to Keynesian macroeconometric models.

In addition to its stress on identifying structural parameters, Keynesian econometrics made two enduring practical contributions. One is the idea that a model is a joint probability distribution over a sequence indexed by pertinent free parameters. A second is a scientific rule that it takes a model to beat a model.

Now on to micro foundations. Friedman, Modigliani, and Tobin improved the fits of aggregate consumption functions by adding dynamics. Baumol and Tobin derived demand functions for money from constrained optimization problems. But microeconomic underpinnings for a Keynesian investment function proved elusive. Reflecting that, in 1955 Tobin published a "Dynamic Aggregative Model" that differs significantly from Hicks's 1937 IS-LM model because Tobin's model has no demand function for investment and no IS curve. Instead it has a demand function for capital.[2] Haavelmo's 1960 *Study in the Theory of Investment* elaborates the failure of a Keynesian investment function to emerge from an intertemporal optimization problem for a repre-

[1]Or equivalently, they can be uniquely recovered from a likelihood function.

[2]Significantly, the interest rate in Tobin's demand function for capital is *real* interest rate, while the interest rate in his portfolio balance equation is a *nominal* interest rate. Tobin's instantaneous nominal interest rate equals his real interest rate plus an exogenous expected rate of inflation. Similarly, Hicks wanted a real interest rate in his IS curve and a nominal rate in his LM curve.

sentative firm. Haavelmo showed how that theory implies a demand function for capital like the one Tobin used in his "Dynamic Aggregative Model," not a demand curve for investment. Finding micro foundations for a Keynesian investment function was at the top of the agenda of 1960s macro, one that attracted the best talent. Accepting Haavelmo's challenge, Jorgenson created a widely admired theory of investment by augmenting Haavelmo's theory of a demand for capital with a distributed lag delaying responses. Complete success occurred only in the late 1960s when Treadway, Gould, and Lucas added adjustment costs to Haavelmo's specification of the firm's problem and thereby derived a Keynesian demand function for investment – i.e., a decision rule for investment rates driven by expectations of future interest rates.

Early static Keynesian models took the aggregate money wage as exogenous. A time series econometric incarnation of a Keynesian model required a dynamic statistical model of the money wage. Keynes had offered fruitful hints about this important missing component when he said that the fall in the wage rate that "classical economics" had relied on would occur too slowly to arrest unemployment. Inspired by a 1958 paper about UK data by A.W. Phillips, in 1960 Samuelson and Solow published a scatter plot of a rate of change of an aggregate wage index against a US unemployment rate. The scatter plot traced a negatively sloped Phillips curve that confirmed Keynes's hunch. Samuelson and Solow gave no formal micro foundations for that negative slope, but they offered insightful remarks about possible sources of such a slope together with important caveats, including guesses about how expectations about future inflation might position that scatter graph.

In addition to its stress on econometric identification of structural parameters, the Tinbergen-Theil program of using econometric models to make quantitative statements about optimal government policies brought optimal control theory and dynamic programming into macroeconomics. Early applications recommended that a government apply dynamic programming to an econometric model that had not in any consistent way imputed dynamic programming to the agents living inside it. The "Lucas Critique" asserted that models with good micro foundations would let private agents apply dynamic programming too. Early responses to the Lucas Critique reversed the situation by constructing economies in which private agents use dynamic programming but governments don't. Eventually macroeconomists would construct structures in which private and government agents both use dynamic programming. But in the late 1960s, those advances lay in the future. Accomplishing them required a new way of thinking about peoples' expectations and a new equilibrium concept for macroeconomics.

Micro founded theories of the dynamics of consumption, investment, and the demand for money assigned important roles to decision makers' expectations about future outcomes. Incorporating expectations in a workable macroeconometric model requires either measuring them or treating them as hidden variables whose laws of motion can be estimated. Researchers at the Carnegie Institute of Technology and elsewhere had tried to measure them and study their properties before modeling them. But data on expectations were scarce, so econometricians including Nerlove, Jorgenson, and Griliches modeled expectations as hidden variables governed by distributed lags. The most popular theory was the "adaptive expectations" model that Friedman had applied in the 1950s. It assumed that expectations about a future value of a scalar variable x are a geometric sum of past values of x. Imposing that the weights on lagged x's sum to one makes this a one-parameter model.

Edmund Phelps's 1967 model of the dynamics of inflation and unemployment imposes adaptive expectations and has people's expectations about inflation position the Phillips curve. With a particular definition of 'long run', Phelps's model implies that there is no long-run trade off between inflation and unemployment. But there is a temporary trade-off whose duration depends on the rate at which adaptive expectations discount past data. Phelps solved a Tinbergen style optimal control problem for a best inflationstabilization policy. Starting from initial conditions of sustained inflation, the best thing to do is to lower inflation gradually. The rate at which inflation should be reduced depends sensitively on the geometric rate at which adaptive expectations discount past data.

In addition to Phelps's Phillips curve analysis, other fruitful uses of adaptive expectations in macroeconomics included Friedman's application to an aggregate consumption function and Cagan's to the monetary dynamics of hyperinflation.

But what about micro foundations? Why should people form expectations about a variable by exponentially smoothing past values of the same variable? Why use exponential smoothing and why not consult other variables? John F. Muth asked and answered these two questions in ways that would revolutionize applied economics. For Muth, finding micro foundations for expectations meant interpreting them as outcomes of least squares problems. Least squares forecasts are conditional mathematical expectations. In a typical macroeconomic model, how the people inside a model forecast future x's influences how future x's should be forecast. Muth advocated assuming that the artificial people inside a model have personal subjective expectations that equal the objective conditional mathematical expectations implied by the model.

In his 1960 "Optimal Properties of Exponentially Weighted Forecasts," Muth posed an "inverse optimal prediction problem" having the form "I tell Muth an arbitrary formula for forming expectations about future outcomes of a random variable x; then Muth's job is to find a joint probability distribution for sequences of x that make my formula optimal in the linear least squares sense." This is an "inverse problem" because it takes the form "I tell you the answer, you tell me the question." Muth posed and solved this problem when Friedman's adaptive expectations scheme is the answer. Muth proved that if the first difference of x is a first-order moving average of a serially uncorrelated mean zero shock, then the optimal predictor of x over any horizon is a geometric average of current and lagged x's and that the decay parameter in the geometric sum equals the coefficient defining the moving average component of x. Muth's answer showed that optimal forecasting functions have no free parameters: all are functions of parameters describing the stochastic process being forecast.

The message of John Muth's 1961 "Rational Expectations and the Theory of Price Movements" is that when expectations are about endogenous variables, "rational expectations" is a "fixed point" property. For my purposes here, Phelps's model is a good macroeconomic laboratory for illustrating the rational expectations equilibrium concept of Muth's 1961 paper.[3] A conditional expectation of future wage inflation implied by Phelps's model differs from the exponentially smoothing expectations rule that Phelps imputed to the people inside his model. Phelps's model makes better forecasts than do the people inside his model. As an equilibrium condition, rational expectations eradicates any such systematic gap between the personal expectations of agents inside a model and the best forecasts implied by the model. Phelps's model induces a mapping from an arbitrary sequence of expectations about wage growth to a sequence of best forecasts of wage growth. Phelps had evaluated this mapping only at the one point associated with adaptive expectations. A rational expectations equilibrium is a fixed point of the mapping. Imposing the rational expectations equilibrium concept on Phelps's 1967 model would substantially alter the model's policy implications in ways that I shall describe.

3 Rational Expectations Econometrics

My work in the 1970s sought to put rational expectations to work on each of the three fronts of the post WWII Keynesian program of (1) constructing

[3]In his 1961 paper, Muth illustrated his equilibrium concept by applying it to a model of prices and quantities in what, with adaptive expectations, was called a cobweb model.

an econometric theory appropriate for macroeconomics; (2) providing micro foundations that would improve econometric fit and stability; and (3) using macroeconometric models to make quantitative statements about good government policies. We had to rethink sources of parameter identification in econometrics; rework the micro foundations of virtually every component of the Keynesian structure; and reformulate quantitative optimal policy analysis. Their internal logic tied the three problems together, so I worked on all three.

A stroke of good luck was learning Clive Granger's definition of causality for a covariance stationary vector time series stochastic process. Granger's definition is about properties of optimal forecasts of one variable as a function of the history of all other variables. Consequently, Granger's "causality" criterion shapes answers to the two questions about the micro foundations of expectations that we met earlier: what current and past variables should a decision maker use to forecast? And what function of those past data should be used?

It helped me to read Christopher Sims's papers that connected Granger causality to an econometrically useful definition of exogeneity. I was fortunate to be with Chris at Minnesota. Sims's papers clarified Granger's work by deepening its mathematical foundations.

Granger and Sims adopted the same mathematical setting that Muth had used in his 1960 and 1961 papers about rational expectations. This framework naturally accepts economic models in which decision makers have quadratic objective functions and random disturbances have finite second moments. This neatly separates the problem of forecasting from the problem of making the best decisions given a set of forecasts, a special 'certainty equivalence' property that substantially simplifies economic models. It was widely used in early rational expectations models.

In these ways, many people's hard work had determined for me a tightly focused research agenda: (1) put decision makers in a setting in which uncertain exogenous variables are described by linear stochastic processes in the style of Granger and Sims, make assumptions that validate that "certainty equivalence" applies, then find a rational expectations equilibrium random process for prices and quantities; (2) extract econometric implications of that rational expectations equilibrium; (3) estimate that econometric model and then use it to find consequences of alternative government policies. My early papers applied parts of this program to study the natural unemployment rate hypothesis, the term structure of interest rates, the monetary dynamics of hyperinflation, the Gibson paradox, and the coordination of monetary and fiscal policies.

An early application was my 1971 "Note on the Accelerationist Controversy" criticizes Solow and Tobin's popular test of the Phelps-Friedman natural unemployment rate hypothesis for not properly imposing rational expectations and then suggests a test consistent with rational expectations. The test that I criticized had used adaptive expectations in the form of exponential smoothing of past inflation rates. Relying on Friedman's one-parameter model, Solow and Tobin had imposed a unit sum on the exponential coefficients describing how people forecast inflation. I pointed out that for a unit sum to be a property of optimal univariate forecasts of inflation, inflation itself would have to be a very persistent process (today we would say that it has a 'unit root'.) But until 1970, post WWII inflation had been only weakly serially correlated. This simple application of univariate optimal prediction theory let me conclude that Solow and Tobin's test could give false rejections of the natural unemployment rate hypothesis and also indicated a more appropriate test of cross-equation restrictions. After I had written my paper, I discovered that Bob Lucas had written a paper that also described how to test the natural unemployment rate hypothesis in a rational expectations equilibrium.

Hicks's 1937 *Value and Capital* had set forth an 'expectations theory' of the term structure of interest rates according to which the yield on a risk-free zero-coupon n period bond should be a geometric average of one-period yields expected to prevail over the next n periods. I had read a number of good papers that supplemented Hicks's expectations theory with something like Friedman's adaptive expectations model for the one period rate to construct an econometric model of the term structure. I responded to the challenge of formulating and estimating a rational expectations version of the expectations theory of the term structure by writing three papers, each a little better than its predecessor. The challenge was to use optimal prediction theory to construct a theory of expectations about the short rate and then to deduce what that implied for the evolution of the long rate. My first attempt published in the *Journal of Money Credit and Banking* in 1972, followed my procedure in a "Note on the Accelerationist Controversy" and simply posited that the short rate followed a univariate linear stochastic process, that traders used this process to forecast short rates, and then to apply Hicks's geometric averaging formula to get long rates. In this way, I deduced from the dependence of short rates on lagged values of the short rate how longer rates should also depend on lagged values of the short rate. This yielded a cross-equation restriction: a long rate equation inherited all of its parameters from the short rate equation.

Thanks to Sims, vector autoregressions were in the air at Minnesota in the mid 1970s. The term structure of interest rates is a vector. What restrictions does a rational expectations version of the expectations theory of the term structure put on a vector autoregression? My 1972 paper didn't answer that question because it assumed that traders forecast the short rate by using only the history of the short rate. If they observe the entire term structure, shouldn't they use the entire vector of interest rates (the term structure) to forecast all rates? My 1979 "Note on Maximum Likelihood Estimation of the Rational Expectations Model of The Term Structure" used all rates and deduced a subset of the restrictions that rational expectations imposes on a vector autoregression for the term structure. That subset heavily over identifies the free parameters of the model and opens a maximum likelihood estimation strategy.

Angelo Melino's 1981 Harvard PhD thesis succeeded in imposing all of the restrictions that the theory imposes on a vector autoregression. I admired Melino's work but wanted to approach the problem from a different angle. To do that, technical reinforcements arrived in the form of a research assistant at Minnesota, Lars Peter Hansen. We reformulated the problem by building on Sims's extensions of Granger's paper on causality. Sims had used vector Wold moving average representations to study implications of the causality structure Granger had originally defined in terms of restrictions on a vector autoregression. Lars and I reformulated the rational expectations version of the expectations theory of the term structure as a set of restrictions on a vector moving average. To preserve covariance stationary and ergodicity of the key objects to be estimated,[4] we imposed a restriction that Engle and Granger subsequently called "co-integration". In addition to letting us completely characterize the restrictions, we recognized that our model belonged to a broader class that proved to be good laboratories for discovering useful econometric properties of linear rational expectations models. Key issues involve possible differences in the information contained in histories of moving average errors and innovations to a vector autoregression.

Phillip Cagan's 1956 "Monetary Dynamics of Hyperinflation" had posited that the demand for money is a semilogarithmic function of the expected rate of inflation, modeled via a single-parameter Friedman style adaptive expectations scheme. Cagan's model presented the following multivariate counterpart of the question that Muth had posed in his 1960 paper: for what bivariate inflation-money creation process would Cagan's adaptive expectations scheme for forecasting inflation be an implication of rational expectations? In my 1977 "Demand for Money During Hyperinflation Under Rational Ex-

[4]Stationarity and ergodicity are technical conditions that make it possible to learn from large samples of time series data.

pectations", I constructed that bivariate model and applied a maximum likelihood estimation strategy. The resulting rational expectations model has the sharp implication that money creation fails to help predict inflation, an implication that is broadly confirmed in Cagan's data. This finding provokes one to think about relationships between Granger causality and other senses of causality. The economic sense of the model is that sustained high rates of money creation are responsible for high inflation outcomes.

Common to those early papers were sources of econometric identification coming from rational expectations. These differ in their mathematical form and economic content from the "exclusion restrictions" that had been employed in most earlier macroeconometric work. Exclusion restrictions identify parameters by assuming that sources of variation that affect some decisions (e.g., demand) don't affect others (e.g., supply). That they move demand and not supply is what generates variation along a fixed supply curve.

The logic of rational expectations pulls the rug out from under exclusion restrictions because dynamic demand and supply functions depend on expectations of future paths of many of the same variables. The rational expectations equilibrium concept implies that decision makers use histories of all variables to forecast those paths. Instead of exclusion restrictions, rational expectations brings cross-equation restrictions that recognize that demand and supply decisions depend in different ways on the same forecast paths. That in turn makes these decisions depend on histories in different ways. By disentangling these dependencies, the theory identifies structural parameters describing preferences, technologies, and information sets. In applications, these cross-equation restrictions have substantial bite and become tighter when more variables appear in decision makers' information sets.

My 1978 *JPE* paper exploited such restrictions in the context of a dynamic model of the demand for labor in the presence of adjustment costs. Two joint 1980 papers with Lars Peter Hansen substantially generalized that work. In these papers, we derived a set of formulas expressing the cross-equation restrictions associated with a class of linear rational expectations models of interrelated factor demand.

Neil Wallace and I studied identification in a model of inflation whose structural equations consist of a Cagan style demand for money and a government budget constraint determining the growth rate of money. The model has a continuum of rational expectations equilibria, some of them being driven by "sunspots", and many of them displaying "rational bubbles". Nevertheless, data on money creation and inflation together with the rational expectations cross-equation restrictions strongly identify parameters and uniquely pin down a single equilibrium. This paper tells a useful story about a logically distinct pair of possible multiplicities, one pertaining to the number of equilibria consistent with a given vector of parameters, and the other pertaining to uniqueness of parameters characterizing bivariate observations.

In 1978, I published a note answering "no" to the question: should you use seasonally adjusted data to estimate rational expectations models? Seasonal adjusted data are long symmetric two-sided filters of the original data, meaning that seasonally adjusted data at t are functions of data at *future* dates. For that reason, seasonal adjustment alters the information in a history of a series by letting the filtered data peek at the future.[5] Imposing the cross-equation rational expectations restrictions on the seasonally adjusted data leads to false restrictions if decision makers are actually using seasonally unadjusted data. My dynamic demand for labor model was a good laboratory for illustrating these claims.

Chris Sims told me that my conclusion would be modified if the rational expectations model being estimated is misspecified more along some dimensions than others. Sims conjectured that by distorting the cross-equation restrictions implied by a false rational expectations model, seasonal filtering could improve maximum likelihood estimates of a subset of parameters capturing more trusted parts of the model. To investigate that idea, Lars

[5] Technically, the data are smoothed, not filtered.

Hansen and I constructed examples in which an econometrician estimated parameters of one rational expectations model while the data were actually governed by another. Some of our examples confirmed Sims's conjecture. While doing that research, we investigated several alternative ways of modeling seasonality in rational expectations models, building on earlier work by Tiao and Grupe and by Osborne about models with hidden periodicity. (I view Lars and my work on seasonality as an early part of our current research program on robustness to model misspecification.)

Suppose that a theoretical model is formulated at a fine time interval, e.g., weekly or in continuous time, while data are available only at coarser time intervals. What restrictions does a continuous time model impose on the available discrete time data? What features of the continuous time model can be identified from the discrete time data? Sims had analyzed these questions in the context of distributed lag models. But he had not posed them for rational expectations models. Lars Hansen and I wrote several papers showing how the hallmark rational expectations cross-equations restrictions are especially powerful in identifying continuous time models from discrete time data.

Bob Lucas presented a problem to me at a conference at the Minneapolis Fed in the mid 1980s when sustained U.S. Federal deficits were being widely discussed. Bob asked what restrictions the assumption of present value budget balance would impose on a vector autoregression for total tax collections and total government expenditures. I produced a preliminary answer describing a sense in which the observable implications are very weak and published it in *Macroeconomic Theory, Second edition.* The weakness of the restriction ultimately comes from the freedom to promise big surpluses in the distant future. Subsequently Lars Hansen, William Roberds, and I studied the problem in greater depth. We discovered that even with a constant risk-free interest rate, present value budget balance imposes no restrictions on a bivariate vector autoregression for government expenditures and tax collections. The present value budget balance hypothesis acquires empirical bite only when supplemented by additional principles guiding either tax collections or expenditures. We used a class of permanent income models to illustrate the role of such restrictions in providing identification.

Since the 1980s, macroeconomists have routinely estimated vector autoregressions, computed impulse response functions, and then applied procedures to associate rotated VAR innovations with shocks purportedly impinging on some incompletely specified deeper economic model. Lars and I analyzed "Two Difficulties in Interpreting Vector Autoregressions." The first arises in situations in which VAR innovations contain less information than the shocks hitting agents' information and budget sets. The second arises when aggregation over time conceals agents' information from the econometrician. Lars and I we used the rational expectations equilibrium concept to extend Chris Sims's earlier work on related time-aggregation issues. We showed how exploiting the cross-equations restrictions of rational expectations econometrics overcomes both kinds of difficulties and also identifies shocks hitting a decision maker's information set.

To reform the third branch of the Keynesian quantitative research program, we sought optimal government policies in settings that impose the rational expectations equilibrium concept. Since expectations of endogenous variables depend on government decision rules, there is a distinct rational expectations equilibrium for each government policy. The optimal policy problem is to find the best rational expectations equilibrium. One of my first efforts along these lines involved finding optimal policy in a rational expectations version of a Phelps-Friedman natural unemployment rate model. In a 1973 paper "Rational Expectations, the Real Rate of Interest, and the Natural Rate of Unemployment," I cast an econometric version of what I had decoded from Milton Friedman's 1968 AEA presidential address. Imposing a rational expectations equilibrium on that structure produced some striking results. Friedman's original model with adaptive expectations had contained a transient trade off between inflation and unemployment that could be optimally exploited along the lines that Phelps had analyzed in his 1967 paper. That exploitable trade off evaporates under rational expectations. There still exists a Phillips curve, but it can't be exploited by any systematic government policy. Using rational expectations also changed the relevant analytic concepts from "long-run versus short-run" to "systematic versus random".

My 1973 Brookings paper originally had used the "systematic versus random" policy classification to obtain a stronger irrelevance result for systematic monetary policy. The editors of the Brookings Papers declined to publish that result. So Neil Wallace and I refined and published it as " Rational Expectations, the Optimal Monetary Instrument, and the Optimal Money Supply Rule" in the *JPE* in 1976. The point of our irrelevance examples was not that monetary policy is irrelevant in all imaginable rational expectations models. It was that to make monetary policy matter requires more frictions than were present in the particular models on which which Neil and I had imposed rational expectations.

In terms of the evolution of twentieth century macroeconomics, my two papers about a natural unemployment rate theory were transitional creatures because they retained many features of earlier Keynesian models, including IS-LM curves. Their micro foundations weren't coherent. Neil and my papers didn't close what Kenneth Arrow in 1967 had called a 'scandalous' gap between macroeconomics and general equilibrium theory. But a paper by Bob Lucas had. In his 1972 "Expectations and the Neutrality of Money," Bob completed a general equilibrium investigation of how to interpret and exploit the Phillips curve. Bob's paper stated a sharp policy ineffectiveness result, that like a similar one in Neil and my papers, hinged sensitively on rational expectations.

After the mid 1970s, Neil and I emerged from the Keynesian waters in which we had evolved and thereafter mostly used general equilibrium models that incorporated the rational expectations assumption. The rational expectations assumption had not been a necessary piece of earlier general equilibrium theories. The general equilibrium models that have come to dominate applied work in finance, macroeconomics, and public finance are special instances of Arrow-Debreu models. That is because Arrow-Debreu models assume general representations of preferences that allow agents to have diverse beliefs about the future. The special general equilibrium models used today in macroeconomics typically (1) restrict preferences of each agent to be some version of discounted expected utility; and (2) impose an extensive 'communism' that precludes any heterogeneity of beliefs across agents inside a model, as well as any differences between these and either nature's probability distribution or the econometrician's. This communism of beliefs is heavily exploited in the rational expectations fixed point equilibrium concept and also in all empirical implementations that rely on a law of large numbers. A compelling scientific justification for assuming rational expectations is that it drastically reduces the number of objects and parameters comprising an econometric model. Pure general equilibrium theorists don't have to be in the 'dimension reduction' business, but econometricians do.

In the same ways that it made us rethink econometric identification, the dimension reduction brought by rational expectations reshaped our understandings about the channels through which fundamental economic forces operate. An irony is that while the rational expectations equilibrium concept emphasizes people's expectations, it makes those expectations disappear as exogenous variables. They are outcomes.

Economic forces that shape equilibrium expectations are today a focus of applied analysis throughout economics. The hallmark cross-equation identifying restrictions brought by rational expectations are routinely used to study runs on banks and sovereign bonds; effects of employment protection and social safety nets on labor supply decisions; enforcement and information constraints in incentive compatible social insurance and financial arrangements; how deposit insurance and lender of last resort facilities alter equilibrium prices and risk-taking and through them tax payers' implicit insurance obligations; and alternative arrangements for coordinating monetary and fiscal policies.[6]

4 Concluding remarks

I have followed instructions to write about my education as an economist and the scientific challenges it presented. I am indebted to the giant macroeconomists from my youth for their idealism and purpose, the high scientific standards that they set, and also the scientific challenges they presented to my generation. Rational expectations econometrics has refined old problems and opened new ones. Lars Hansen's Nobel lecture describes how studying these improves our understandings of equilibrium expectations and macroeconomic outcomes.

[6]The high rates of money creation in the hyperinflations studied by Cagan had fiscal origins. Thinking about the economic forces that shape equilibrium inflation led Neil Wallace and me to our 1981 "Unpleasant Monetarist Arithmetic." We combined a rational expectations version of Cagan's demand for money with a specification of the money supply process that depends on how "fiscal" and "monetary" policies are coordinated. Fiscal policy partly determines the rate at which total government debt grows while monetary (a.k.a. debt management) policies use "open market operations" to determine what fractions of the public's holdings of government debt do and don't bear interest, fractions that also influence the rate at which government debt grows. The arithmetic of a consolidated government budget constraint entangles monetary and fiscal policies. Equilibrium paths of the price level depend on how monetary and fiscal policies are coordinated. Our 1981 paper is sometimes accurately called a fiscal theory of equilibrium price paths.