

Agents as Empirical Macroeconomists: Thomas J. Sargent's Contribution to Economics*

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Abstract

Thomas J. Sargent has fundamentally changed and, in the words of Art Rolnick, who interviewed him in 2010, “irrevocably transformed” the field of macroeconomics—whether as sole author, as co-author with collaborators, as an author influenced by others, or as a teacher of the profession, influencing others. This paper is about his contribution to our field. The Nobel Memorial prize to Thomas J. Sargent and Christopher A. Sims has been awarded for the “empirical research on cause and effect in the macroeconomy” or, more broadly, for macroeconometrics. One often likes to distinguish between theorists, on the one hand, and empiricists and econometricians, on the other. Thomas J. Sargent holds a unique position in between. A key theme in a large part of his work has been to put the agents in his model on equal footing with the econometrician who is observing data from the model (i.e., to assume that agents are themselves empirical macroeconomists or macroeconometricians). In this paper, I use this theme to examine his work and his contributions to the study of economics.

Keywords: Learning; macroeconometrics; macroeconomics; monetary economics; Nobel laureates; rational expectations; Thomas J. Sargent

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I. Introduction

“A rational expectations equilibrium is a likelihood function. Maximize it.”

Thomas J. Sargent (in Evans and Honkapohja, 2005).

In 2011, Thomas J. Sargent and Christopher A. Sims were awarded the Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel “for their empirical research on cause and effect in the macroeconomy”. This paper is about Sargent’s contribution. It might be hard to describe

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their contributions in full separation. While their list of co-authored papers is short, and it appears to contain only Sargent and Sims (1977), it is clear that Sargent has been deeply influenced by Sims, and greatly admires him. In the interview by Evans and Honkapohja (2005), and referring to his work with Lars Hansen on robustness (e.g., Hansen and Sargent, 2008), he has stated “that Lars’s and my readings of Chris’s early work on approximation of distributed lags were important inspirations for our work on robustness. Chris authored a beautiful approximation error formula and showed how to use it to guide the choice of appropriate data filters that would minimize approximation errors. . . . One version of Chris’s min–max analysis originated in a message that Chris wrote to me about a comment in which I had argued that a rational expectations econometrician should never use seasonally adjusted data. . . . Chris both read my comment and wrote his memo on a Minneapolis bus going home from the U in 1976—that’s how fast Chris is. . . . Lars and I wrote a paper that went a long way towards accepting Sims’s bus memo argument.” Many of Sargent’s contributions are co-authored. The list is long and includes illustrious names such as Lars Peter Hansen, Lars Ljungqvist, Timothy Cogley, Noah Williams, Albert Marcet, Tao Zha, and Neil Wallace, to mention a few. He appears to hold them in high esteem. For example, about Neil Wallace, one of his earliest co-authors, he has said that “when I think of Neil, one word comes to mind: integrity. . . . Except for our paper on commodity money, . . . Neil asked me to remove his name from every paper that he and I wrote together. He thought the papers should not be published. After he read the introduction to one of our *JPE* papers, Bob Lucas told me that no referee could possibly say anything more derogatory about our paper than what we had written about it ourselves. Neil wrote those critical words” (see Evans and Honkapohja, 2005). Luckily, Wallace did not always prevail in preventing publication or having his name removed: the collaboration between Sargent and Wallace (Sargent and Wallace, 1971, 1973a, 1973b, 1974, 1975, 1976, 1981, 1982, 1983, 1985, 1987; Beers *et al.*, 1983) has resulted in some of the most iconoclastic and now iconic, fundamental, and path-breaking contributions to the study of economics.

Sargent has fundamentally changed and “irrevocably transformed” (Rolnick, 2010) the field of macroeconomics—whether as sole author, as co-author with collaborators, as an author influenced by others, or as a teacher of the profession, influencing others. Thus, this paper is about his contribution to our field. His contributions are vast, his co-authors are many, and this paper is short. There is absolutely no way to do justice to his opus within the confines of this text, and I sincerely apologize for the omissions of important contributions and key co-authors. However, they are well known and easy to find. May the reader thus take these omissions (as

well as the non-omissions) as encouragement to read the originals for him- or herself.

The Nobel Memorial prize to Sargent and Sims has been awarded for the “empirical research on cause and effect in the macroeconomy” or, more broadly, for macroeconometrics. One often likes to distinguish between theorists, on the one hand, and empiricists and econometricians, on the other. Theorists postulate a model framework and behavioral assumptions for the economic agents in these models, in order to deduce regularities that might hold in observations or that explain observed phenomena. Empiricists and econometricians take data, perhaps armed with an *a priori* theoretical perspective, to induce statistical regularities and empirical observations, which theorists might wish to explain or which confirm regularities deduced by theorists. Economists often describe themselves as one or the other.

Sargent holds a unique position in between. A key theme in a large part of his work has been to put the agents in his model on equal footing with the econometrician observing data from the model (i.e., to assume that agents are themselves empirical macroeconomists or macroeconometricians). As a theorist, he suggests that agents behave as potentially excellent empiricists. As an empiricist, he suggests that agents are potentially excellent theorists. Thus, Sargent is an empirical macroeconomist, but in a sense that runs considerably deeper: the agents in his theoretical models are also empirical macroeconomists. In turn, this has profound implications for thinking about cause and effect in the macroeconomy, and therefore for macroeconomic policy.

There is a beautiful symmetry here. Together with his co-authors, Sargent has devoted considerable intellectual effort to exploring that symmetry and connection, and the resulting implications. It is the task of this paper to follow these explorations in more detail. This symmetry also points to an exciting research agenda for the future, for the rest of us, to a powerful theme and paradigm, which we are still only beginning to fully explore and understand.

With this, Sargent provided the important connection and glue to the revolution in macroeconomics, which occurred at the beginning of the 1970s under the banner of rational expectations, and with key participants, such as Robert E. Lucas, Jr., and the “four horsemen of Minnesota”,¹ that is, Neil Wallace, Edward C. Prescott, Christopher A. Sims, and Thomas J. Sargent himself. He connected the theoretical and empirical approaches, and he provided deep and significant insights on his own to thereby create a unified and truly revolutionary whole.

¹ See <http://www.startribune.com/local/131488728.html?refer=y>.

II. A New Macroeconometrics: Rational Expectations and Cross-Equation Restrictions

A key battle that emerged in macroeconomics in the late 1960s and early 1970s concerned the natural-rate hypothesis (see Friedman, 1968; Phelps, 1968; Lucas, 1972, 1973). Friedman (1968) has written that “the ‘natural rate of unemployment’... is the level that would be ground out by the Walrasian system of general equilibrium equations, provided there is imbedded in them the actual structural characteristics of the labor and commodity markets, including market imperfections, stochastic variability in demands and supplies, the cost of gathering information about job vacancies and labor availabilities, the costs of mobility, and so on.” It is the level of unemployment emerging as supply equals demand and at the equilibrium real wage. Deviations occur when real wages calculated from existing nominal wage levels and the current price level are out of synch with that equilibrium real wage. If nominal wages are bargained ahead of time, then surprises in the current price level or surprises in the inflation rate will give rise to such deviations. This logic gave rise to the celebrated expectations-augmented Phillips curve formulation by Lucas (1973), represented by equation (1.1) of Sargent (1976a):

$$Un_t = -\gamma (p_t - p_t^e) + \sum_{i=1}^{n_1} \lambda_i Un_{t-i} + u_{1t}. \quad (1)$$

Here, Un_t denotes the unemployment rate at date t , $\gamma > 0$ is a parameter, p_t is the log of the price level, p_t^e is the expectation of that price level at date $t - 1$, λ_i are weights on past unemployment characterizing the unemployment dynamics, and u_{1t} is an error term (with the subscript 1 copied from his multi-equation system). Thus, unemployment will deviate from the level expected, based on its own past evolution, only because of the surprise in the log of the price level ($p_t - p_t^e$) or, equivalently, the surprise in the inflation rate ($\pi_t - \pi_t^e$), where $\pi_t = p_t - p_{t-1}$ and with p_{t-1} known at $t - 1$. Anticipated inflation will not have an effect on unemployment, if it is fully incorporated in the pre-agreed nominal wage level w_t . That is, if $\alpha = 1$ in equation (1) of Sargent (1971):

$$\frac{\Delta w_t}{w_{t-1}} = \alpha \pi_t^e + f(Z_{t-1}) + \epsilon_t. \quad (2)$$

Here, I have modified this equation by explicitly indicating that an expectation of inflation is taken, and I have not included variables, except for a random term dated t on the right-hand side for the sake of exposition. Furthermore, expected inflation here should be read to be defined by the change of the level divided by the previous level rather than as the log-difference, a subtle distinction that I ignore hereafter.

Thus, the empirical controversy has centered on the value for α in equation (2). Sargent (1971) has stated: “An estimated α of unity (or close to unity) is taken to confirm the Phelps–Friedman ‘accelerationist’ position, while an α markedly lower than unity is taken to imply that there is a meaningful long-run trade-off between wage inflation and the unemployment rate. Empirical investigations of the accelerationist hypothesis have focused on estimating α .”

Closely related to this is the Fisher hypothesis regarding the relationship between the natural rate of interest, the market rate of interest, and inflation. Friedman (1968) has written that the “monetary authority can make the market rate less than the natural rate only by inflation. It can make the market rate higher than the natural rate only by deflation.” Appealing to the presidential address of Friedman (1968), Sargent (1973b, 1973c) has provided the updated or “alternative statement of Fisher’s theory . . . that the Phelps–Friedman hypothesis of a natural rate of unemployment is true, and thus that no (systematic) monetary or fiscal policies can produce a permanent effect on the unemployment rate. Given these two hypotheses . . . it follows that the real rate of interest is independent of the systematic, or foreseen, part of the money supply, which therefore can influence the nominal rate only through effects on expected inflation. The notion that the real rate of interest is independent of the systematic part of the money supply embodies the key aspect of Fisher’s theory appealed to by Friedman in his presidential address.” These statements already embodied what has come to be viewed as a hallmark implication of the early rational expectations literature: the ineffectiveness of systematic monetary policy to move unemployment rates and real interest rates. In other words, systematic monetary policy cannot cause unemployment to move. This crucial insight is at the center of the Nobel citation for Sargent.

Therefore, the empirical evaluation and test of these theories amounted to obtaining estimates of expected inflation and the resulting coefficients γ and α in the equations above. In a series of papers, Sargent (1971, 1972a, 1972b, 1973a, 1973b, 1973c, 1976a) upended the traditional empirical approaches, instead creating a new macroeconometric approach based on rational expectations. At the core is the question of how to think about the expectations regarding future inflation in equations (1) and (2). Rational expectations theory answers this question by equating it with the mathematical expectation inside a fully specified dynamic model of the economy,

$$\pi_t^e = E_{t-1}[\pi_t] \quad (3)$$

where the specification includes the specification of the exogenous stochastic processes. This answer of Sargent (together with Lucas and Wallace and originally provided by Muth, 1961) has so profoundly shifted the way we

now look at these equations that it seems to hard to remember it as anything but the most natural way to proceed. However, this was not always so. Indeed, a theme of the lifetime work of Sargent himself is that he never viewed this as the complete answer.

In 1971, Sargent approached the task of estimating the expected inflation as follows (Sargent, 1971). He noted that, traditionally, expected inflation was obtained as a distributed lag on past inflation using the Fisher–Cagan equation:

$$\pi_t^e = \sum_{i=0}^m v_i \frac{\Delta P_{t-i}}{P_{t-i-1}} \quad (4)$$

(see Fisher, 1930; Cagan, 1956; Sargent, 1971, equation (2)). Here, P_t denotes the price level at date t . If the coefficients v_i are given, then this amounts to reformulating equation (2) as

$$\frac{\Delta w_t}{w_{t-1}} = \alpha \left(\sum_{i=0}^m v_i \frac{\Delta P_{t-i}}{P_{t-i-1}} \right) + f(Z_{t-1}) + \epsilon_t. \quad (5)$$

Next, Sargent (1971) noted that “almost always, the constraint that has been imposed is that the distributed lag weights in equation (4) sum to unity”:

$$\sum_{i=0}^m v_i = 1. \quad (6)$$

This is an important constraint, because (as noted in Sargent, 1971) “the quality of the estimates of α obtained is obviously predicated on the adequacy of this restriction. Apparently, the restriction has been justified by the following type of argument. Suppose that the rate of inflation increases from zero, which has been its value for a long time (more than $m + 1$ periods), to one percent per annum, staying there indefinitely. Then, it is reasonable to expect that eventually people will catch on and expect inflation to continue at one percent per annum. This is taken to imply that the weights in equation (4) sum to unity. It is certainly reasonable to expect that a sustained, constant inflation sooner or later would be fully anticipated. However, this consideration is of very little use in producing a reasonable restriction to impose on the v_i ’s of equation (5) for the purposes of empirical estimation; for, during the periods used in estimation, inflation was never as sustained as is imagined in the mental experiment described above. That experiment leads us to deduce a restriction on the weights in equation (5) by assuming a time path for the inflation rate that bears little resemblance to the path that inflation has actually followed in the past.” From a current perspective, it is important to note that there is a subtly stated assumption (or observation) here, namely that inflation is stationary and not integrated. This has been a topic of considerable debate over the

last two decades (e.g., Ng and Perron, 2001). At the time, this was less contentious, given the data available then. Sargent (1971) has stated that “even the most casual glance at the price history of the United States makes it clear that the inflation rate has not been a strongly drifting variable.”

Sargent (1971) also postulated that “when searching for an identifying restriction on the sum of the weights in equation (5) to be used in empirical work, it therefore seems most appropriate to ask what sort of expectations-generating scheme would be reasonable in light of the actual behavior of the inflation rate during the period being studied. In doing this, it seems natural to adopt assumptions about the evolution of the rate of inflation that are compatible with equation (4) being a ‘rational’ (more precisely, a minimum-mean-squared error) generator of expectations.” Therefore, Sargent (1971) has turned equation (4) into a regression equation, with the weights obtained by least squares. If inflation is stationary (as Sargent has inferred), then this “require[s] that the weights in equation (5) sum to considerably less than unity”, “that imposing restriction (6) amounts to supposing that the public’s method of forming expectations of inflation was very irrational in the sense of being widely inconsistent with the actual inflation process”, and “that imposing restriction (6) on US data is likely to lead to serious overestimation of the individual distributed lag weights, the ν_i ’s, and to serious underestimation of α .” In other words, by imposing the restriction that the weights in equation (4) are obtained from a least-squares regression on a stationary time series, the weights must sum to something less than one, and then in turn α must be closer to unity than if restriction (6) was imposed. Hence, the data end up being more consistent with the natural-rate hypothesis and the ensuing radical policy implications than when equation (6) was imposed.

Here, a careful reader might have noted two rather different definitions of the term rational expectations. In the first, and now common, definition, rational expectations is equated with the mathematical expectation inside a fully specified dynamic model of the economy. However, according to the quote from Sargent (1971) above, he understands it there as a minimum-mean-squared error estimator and of “forming expectations of inflation . . . [as] consistent with the actual inflation process”. Inside a fully specified theory, these two might, and often do, coincide. However, as a matter of practice and given data for the actual inflation process, but not a completely specified model of the economy, this comparison is considerably more challenging. Thus, Sargent has postulated that agents can be turned into macroeconometricians, and they can be asked to perform an inflation forecast to the best of their abilities and knowledge, given the available data and given available insights into the economy and the political process. This is a subtle distinction. It is a distinction that lies at the heart of much of Sargent’s later work.

The key insight above (i.e., that forecasts for inflation should be formed rationally, in the sense of the agent as a smart macroeconometrician) has been applied in various forms in Sargent (1971, 1972a, 1972b, 1973a, 1973b, 1973c, 1976a). For example, in Sargent (1973b, 1973c), he has turned to the estimation of the Fisher relationship, often postulated as

$$r_t = \rho + \pi_{t+1}^e + u_t$$

$$\pi_{t+1}^e = \sum_{i=0}^m v_i (p_t - p_{t-i-1}),$$

where ρ is the natural real interest rate, r_t is the market nominal interest rate between t and $t + 1$, and the second equation reformulates equation (4) in logs (note that Sargent has written π_{t+1}^e as π_t). If we combine these equations, we obtain

$$r_t = \rho + \sum_{i=0}^m v_i (p_t - p_{t-i-1}) + u_t. \quad (7)$$

Sargent (1973b, 1973c) has examined this equation in the context of a fully specified model, in which nominal interest rates are determined on a money market. He has compared the implications of adaptive expectations to those of rational expectations (here, in the model-theoretic sense), and he has shown them to be radically different. He has observed that the “usual way of implementing Fisher’s theory about interest and expected inflation has been to regress nominal interest rates on current and lagged values of the logarithm of the price level, interpreting the coefficients as estimates of the distributed lag by which the public seems to form its expectations about inflation. The implausibility of those distributed lags as devices for forming predictions of inflation has weakened the appeal of Fisher’s doctrine. However, according to the version of the model with rational expectations described... [in Sargent (1973b, 1973c)], these regressions are not a valid test.”

His model is given by the following three equations, the first for aggregate supply

$$y_t = k_t + \gamma (p_t - p_t^e) + U_t, \quad (8)$$

the second for aggregate demand

$$y_t = k_t + c (r_t - p_{t+1}^e - p_t) + dZ_t + \epsilon_t, \quad (9)$$

and the third for portfolio balance

$$m_t = p_t + y_t + br_t + \eta_t, \quad (10)$$

and by a specification of the money supply process m_t , which is assumed to be set as a function of past variables (i.e., dated $t - 1$ or earlier). Here,

$\gamma > 0$, $c < 0$, and $b \leq 0$ are parameters, y_t is the log of output, k_t is an exogenously given stochastic trend of normal productive capacity (so that $y_t - k_t$ is the output gap), Z_t is a list of exogenous variables, representing fiscal policy in particular, and U_t, ϵ_t, η_t is a sequence of mutually uncorrelated, but possibly serially correlated and normally distributed random variables. Imposing rational expectations

$$p_{t+1}^e = E_t[p_{t+1}] = E[p_{t+1} | \theta_t] \tag{11}$$

implies that

$$r_t = \frac{1}{c}(y_t - k_t) - \frac{d}{c}Z_t + E[p_{t+1} | \theta_t] - p_t - \frac{1}{c}\epsilon_t, \tag{12}$$

where θ_t is the list of all variables known at date t , and d is some constant.

Following this, Sargent demonstrated two key propositions. He showed that a natural-rate hypothesis holds “in the sense that the deviation of output from its normal level is statistically independent of the systematic parts of monetary and fiscal policies; that is, . . . [changes known in advance in monetary or fiscal policy] have no effects on the expected value of $(y - k)$. Second, the real rate of interest is independent of the systematic part of the money supply; that is, alterations . . . of the feedback rule . . . [for the systematic part of the money supply] have no effects on the expected value of the real rate.” Thereby, he has established, again in the context of this model, that the predictable part of policy cannot cause movements in the output gap or the real rate.

Furthermore, and with the first equation (8) of the model, he has shown that equation (12) implies

$$E[r_t | \{p_t\}] = E[(p_{t+1} - p_t) | \{p_t\}] + \frac{1}{c}E[U_t | \{p_t\}] - \frac{d}{c}E[Z_t | \{p_t\}] + \frac{\gamma}{c}E[p_t - p_t^e | \{p_t\}] - \frac{1}{c}E[\epsilon_t | \{p_t\}].$$

Here, $\{p_t\}$ is an abbreviation for the list (or σ -algebra generated by) $(p_t, p_{t-1}, p_{t-2}, \dots)$. Because there is generally no reason to expect the list of expressions beyond the first term on the right-hand side (i.e., the expressions on the second line) to sum to zero, a regression of r_t on present and past prices will contain terms that include more than the Fisher correction for expected inflation represented by the first term. This is the basis for Sargent’s argument that traditional tests of the Fisher hypothesis are incorrect.

Instead, Sargent has proposed a different, proper test, relying on the natural-rate hypothesis, which he views as tightly linked to the Fisher hypothesis. Interpreting unemployment as proportional to the negative of the output gap $y_t - k_t$, he has shown that the rational expectations version of his model gives rise to equation (1). This equation, in turn, can be checked by testing whether only lagged unemployment, but not the other

lagged variables, is helpful in predicting current unemployment. In other words, past inflation but also past policy choices should not Granger-cause current unemployment rates. Sargent has tested this proposition in a variety of ways, which has not been an unequivocal success; some tests have resulted in a rejection of the non-causality proposition. None the less, with some discussion, he has concluded that the natural-rate hypothesis and the non-causality of the predictable part of macroeconomic policy on future unemployment is a good description of the data.

Sargent has picked up a rather similar model in Sargent (1976a), and likewise he has tested its implications there, with the aim “to test how emphatically the data reject a model incorporating rather severe classical hypotheses”. While “some evidence for rejecting the model has been turned up, it is far from being overwhelming and decisive”. Most importantly, “the tests have turned up little evidence requiring us to reject the key hypothesis of the model that government monetary and fiscal-policy variables do not cause unemployment or the interest rate. The fact that such evidence has been hard to turn up ought to be disconcerting to users of the existing macroeconomic models, since as usually manipulated those models all imply that monetary and fiscal policy do help cause unemployment and the interest rate. Models of the kind presented in this paper imply that there is no scope for the government to engage in activist countercyclical policy, so that it might as well employ rules without feedback for fiscal and monetary policy, for example, Friedman’s x percent growth rule for the money supply.” Thus, with his macroeconomic work, Sargent has provided crucial empirically solid ground for these key tenets of the new rational expectations macroeconomics of the time. This has indeed been a revolution. Macroeconomics has been changed irrevocably.

Rational expectations amounts to assuming that agents forecast future as well as possible variables, given past and present data. Thus, in a rational expectations model, all past and present variables can, in principle, be useful for forecasting future variables, and thus they can show up in any equation involving expectations, a point that has been forcefully raised by Sims (1980). This implies that it will often be hard or impossible to find convenient exclusion restrictions for estimating macroeconomic relationships, which is the approach favored before the rational expectations revolution and outlined by the Cowles commission. However, this does not imply that all variables can enter all equations in an arbitrary fashion. On the contrary, a specific rational expectations model gives rise to cross-equation restrictions. Sargent has called cross-equation restrictions the hallmark of rational expectations econometrics (see Evans and Honkapohja, 2005). The most forceful expositions and benchmark contributions are those of Sargent (1978) and Hansen and Sargent (1980); these had been described earlier in his studies of hyperinflations in Sargent and

Wallace (1973a) and Sargent (1977), and they are also described in Sargent (1981) and Hansen and Sargent (1991).

An excellent exposition is given in the advanced information provided by the Nobel Committee for Economics (2011). I highly recommend reading this, but there might be little point in repeating these explanations here. For yet a different perspective, let me draw on the notation and results in Uhlig (1999), so that I can expose the ideas as follows. Linear-quadratic rational expectations models or rational expectations models solved by first-order approximations to the equations characterizing the equilibrium often give rise to a system of second-order stochastic equations

$$0 = E_t[Fx_{t+1}] + Gx_t + Hx_{t-1} + Dz_t. \tag{13}$$

Here, x_t is a vector of endogenous variables, F , G , H , and D are coefficient matrices, and z_t is a vector of exogenous stochastic processes, following a (vector) AR(1) process,

$$z_t = Nz_{t-1} + \epsilon_t, \quad 0 = E_t[\epsilon_{t+1}], \quad \Sigma = E[\epsilon_t \epsilon_t'], \tag{14}$$

for matrices N and Σ . Hansen and Sargent (1980) have considered and emphasized the case when one part of the vector z_t (i.e., z_{1t}) is observable, while the other part, z_{2t} , is not. To keep matters tractable (and in line with their exposition), two separate (vector) AR(1) processes can be assumed for each part, but potential correlation in their innovation must be allowed for. The system above then becomes

$$0 = E_t[Fx_{t+1}] + Gx_t + Hx_{t-1} + D_1z_{1t} + D_2z_{2t}, \tag{15}$$

where x_t is a vector of endogenous variables, F , G , H , and D are coefficient matrices, and z_t is a vector of exogenous stochastic processes, following a (vector) AR(1) process,

$$z_{jt} = N_j z_{j,t-1} + \epsilon_{jt}, \quad 0 = E_t[\epsilon_{j,t+1}], \tag{16}$$

and Σ is the variance-covariance matrix of the stacked vector $[\epsilon'_{1t}, \epsilon'_{2t}]'$. If it helps, it might be best to suppose that everything is univariate, that is, to assume that $F, G, H, D_1, D_2, N_1, N_2, x_t, z_{1t}, z_{2t} \in \mathbb{R}$, but equations (17) to (27) work in the general case, unless stated otherwise. Conversely, note that cases with additional leads and lags can be written in the above form by stacking, because a univariate AR(k) process can be rewritten as a k -dimensional VAR(1) process. In any case, first it is supposed that $F, G, H, D_1, D_2, N_1, N_2$, and Σ are known, that N_1 and N_2 have eigenvalues strictly below unity in absolute value, and that we seek to solve for a stationary stochastic process x_t solving equation (15).

Given equations (15) and (16), it appears to be a reasonable conjecture that the solution will be given by a recursive law of motion

$$x_t = Px_{t-1} + Q_1z_{1t} + Q_2z_{2t}, \tag{17}$$

where the eigenvalues of P are strictly below unity in absolute value to ensure stationarity. Indeed, substituting equation (17) into equation (15) twice and comparing coefficients² yields the equations

$$0 = FP^2 + GP + H \tag{18}$$

$$0 = FQ_jN_j + (FP + G)Q_j + D_j. \tag{19}$$

The first equation is a matrix quadratic equation in P . In the univariate case, P is simply one of the two solutions to a univariate quadratic equation—the standard equations apply. For the multivariate case, we have to work slightly harder. Suppose that x is an eigenvector of P and λ is the eigenvalue $\lambda x = Px$. By stacking the endogenous part of equation (15) into a first-order system or direct verification, we can see that x and λ also satisfy the generalized eigenvalue equation

$$\lambda \begin{bmatrix} F & 0 \\ 0 & I \end{bmatrix} \begin{bmatrix} \lambda x \\ x \end{bmatrix} = \begin{bmatrix} -G & -H \\ I & 0 \end{bmatrix} \begin{bmatrix} \lambda x \\ x \end{bmatrix}, \tag{20}$$

and that, conversely, any eigenvector and eigenvalue solving the latter provides an eigenvector and eigenvalue of the former (see Uhlig, 1999). If the generalized eigenvalue equation (20) has exactly as many stable eigenvalues as the dimensionality of x , then the solution for P is unique. This condition is satisfied in many economic models, but see Farmer (1993) for interesting exceptions. Given P , equations (19) are now linear equations in the matrices Q_j , which can be solved after vectorization with standard linear algebra tools (see Uhlig, 1999). In the univariate case, these are linear equations in the real variables Q_j .

With this, a recursive law of motion has been obtained, but it cannot immediately be used for estimation, because the second component of z_t is assumed to be unobserved. Because the innovations might be correlated, we run a regression of ϵ_{2t} on ϵ_{1t} to obtain

$$\epsilon_{2t} = V\epsilon_{1t} + \xi_t \tag{21}$$

for some matrix $V = \Sigma_{21}\Sigma_{11}^{-1}$, so that ξ_t is uncorrelated with ϵ_{1t} . With lag operator notation, equation (16) can be written as

$$(1 - N_jL)z_{jt} = \epsilon_{jt} \quad \text{as well as} \quad z_{jt} = (1 - N_jL)^{-1}\epsilon_{jt}. \tag{22}$$

In particular,

$$\begin{aligned} z_{2t} &= (1 - N_2L)^{-1}\epsilon_{2t} \\ &= (1 - N_2L)^{-1}(V\epsilon_{1t} + \xi_t) \\ &= (1 - N_2L)^{-1}V(1 - N_1L)z_{1t} + (1 - N_2L)^{-1}\xi_t, \end{aligned}$$

² The comparison of coefficients is not always correct, but it is practically nearly always correct.

so that equation (17) can be rewritten as

$$x_t = (1 - PL)^{-1} [Q_1 + Q_2(1 - N_2L)^{-1}V(1 - N_1L)]z_{1t} + (1 - PL)^{-1}Q_2(1 - N_2L)^{-1}\xi_t. \tag{23}$$

If we wish to expose the lag structure more explicitly, we might wish to rewrite this, in turn, as

$$x_t = (1 - PL)^{-1} \left[(Q_1 + Q_2V)z_{1t} + Q_2 \sum_{s=0}^{\infty} N_2^s(N_2V - VN_1)z_{1,t-1-s} \right] + (1 - PL)^{-1}Q_2 \sum_{s=0}^{\infty} N_2^s\xi_{t-s}. \tag{24}$$

It would now be easy to expand and multiply with $(1 - PL)^{-1} = \sum_{s=0}^{\infty} P^sL^s$ as well.

Equation (23) is a version of equation (22) in Hansen and Sargent (1980). It should be compared to the unrestricted regression of x_t on z_{1t} , allowing for serial correlation in the error term,

$$x_t = A(L)z_{1t} + B(L)\tilde{\xi}_t, \tag{25}$$

for some lag polynomials $A(L)$ and $B(L)$ and some independently and identically distributed term, $\tilde{\xi}_t$. The comparison shows that $A(L)$ and $B(L)$ are not arbitrary but must instead satisfy

$$A(L) = (1 - PL)^{-1} [Q_1 + Q_2(1 - N_2L)^{-1}V(1 - N_1L)] \tag{26}$$

$$B(L) = (1 - PL)^{-1}Q_2(1 - N_2L)^{-1}\Psi, \tag{27}$$

for some matrix Ψ to equate $\xi_t = \Psi\tilde{\xi}_t$, and where P , Q_1 , and Q_2 must solve equations (18) and (19). Thus, there are restrictions across these equations or cross-equation restrictions. Some restrictions have already arisen when ignoring the restrictions provided by equation (18), as seems to have been done in Hansen and Sargent (1980), or even when also ignoring equation (19); there is already considerable structure in equations (26) and (27). As an example, suppose everything is univariate. Equation (27) then implies that the serial correlation of the error term in equation (25) takes the form of an AR(2) with two roots (which we can call λ_1 and λ_2), one of which must be P and the other N_2 , although we might not yet be able to assign which is which. The root N_1 of the observable process z_{1t} can be estimated. Suppose it happens to equal to one of the two roots λ_1 and λ_2 . Then, it follows that $A(L)$ must be a linear combination of the lag polynomials $(1 - \lambda_1L)^{-1}$ and $(1 - \lambda_2L)^{-1}$; there is no other choice. The restrictions run even deeper, if equations (18) and (19) are taken into account and, furthermore, if the parameter matrices are, in turn, functions

$F = F(\theta)$, $G = G(\theta)$, etc., of some lower-dimensional fundamental parameter vector θ . In this case, subtle dependences on θ arise across all these equations.

A slightly different perspective is to note that both the data and the recursive law of motion solving the (linearized) system of equations that characterize the equilibrium can be represented with a VAR. The structure of the model and its parameters impose restrictions on the theory VAR as well as the data VAR coefficients. Variables that are assumed to be observable to agents in the model might not be observable to the econometrician who seeks to estimate the parameters of the model. Mapping the theory VAR to the data VAR might take some work (see Fernández-Villaverde *et al.*, 2007). Tracing out how the data VAR is ultimately restricted by the deep parameters of the model embodies the cross-equation restrictions.

If we postulate that ϵ_t has a specific distribution, then this leads to a non-linear maximum likelihood estimation problem for θ , obeying these cross-equation restrictions. This gives rise to the quote by Sargent that is given at the beginning of this paper: “A rational expectations equilibrium is a likelihood function. Maximize it” (see Evans and Honkapohja, 2005). With the likelihood function at hand, the door is open to Bayesian estimation as well. Bayesian estimation has the additional advantage of constraining parameters to remain in *a priori* reasonable ranges or to impose *a priori* views on the parameters on which the data will not speak loudly. Therefore, building on the advances and insights of Sargent, the recent body of literature has moved towards estimating dynamic stochastic general equilibrium (DSGE) models with Bayesian methods. The fact that software, such as DYNARE,³ is readily available has made this approach feasible for larger parts of the profession, and this software has become part of the standard toolkit of current macroeconomic research. These developments owe much to the path-breaking work by Sargent.

III. Monetary Policy, Fiscal Policy, and Policy Irrelevance

Sargent and Wallace (1971, 1973a, 1973b, 1974, 1975, 1976, 1981) have carefully developed and examined the theoretical implications of rational expectations, often in connection with the natural-rate hypothesis, as formulated by Lucas (1972, 1973). They have examined the role of monetary and fiscal policy in these models. In particular, and along with Sargent (1973b, 1973c, 1976a), they have examined the role of various assumptions on monetary policy in the context of a version of the model stated in equations (8)–(10), when monetary policy sets the money supply or the nominal interest rate in response to variables dated $t - 1$. They have

³ See <http://www.dynare.org>.

derived results for optimal monetary policy given an objective function for the government, thus turning the government itself into an agent participating in the economy, and with choices endogenously derived within the model.

At the heart of the examination is the role of expectations. They have called p_t^e “the public’s psychological expectation of the log of the price level to prevail at t , the expectation being held as of the end of period $t - 1$ ” (Sargent and Wallace, 1975). They have examined the profoundly different implications of assuming that these expectations are formed in an adaptive way rather than rationally (i.e., as the mathematical expectation, plus perhaps an extra error, inside the model). For the case of rational expectations, they have forcefully stated the conclusion that “the distribution of real output is independent of the systematic money supply rule” (Sargent and Wallace, 1975). Along with their result, they have delivered the explanation in the clearest of terms, as follows. “In order for the monetary authority to induce fluctuations in real output, it must induce unexpected movements in the price level by virtue of the aggregate supply curve (8). But, by virtue of the assumption that expectations about the price level are rational, the unexpected part of price movements is independent of the systematic part of the money supply, as long as the authority and the public share the same information. There is no systematic rule that the authority can follow that permits it to affect the unexpected part of the price level” (Sargent and Wallace, 1975).

This has raised the issue of why the monetary authority should set the money supply or the nominal interest rate as a function of past data only, or what happens when the policy is changed entirely. Sargent and Wallace (1975) have written: “Of course, the authority could add an unpredictable random term to the systematic part of the money supply. Then the distribution of unexpected price movements and of real output will depend on the distribution of... [the unpredictable random term]. But clearly, there is no way the authority can base a countercyclical policy on this particular non-neutrality, since there is no way the authority can regularly choose... [it] in response to the state of economic affairs in order to offset other disturbances in the system,” unless the monetary authority has some informational advantage over the public. If there is such an informational advantage, they have conceded that “this shows that the monetary authority can do as well given an information discrepancy as it can do if there is none. But can it do better? Can it, as it were, take advantage of the presence of an information discrepancy? We are not sure. But within our structure, the answer seems to be that it can take advantage of a discrepancy, although necessarily in a limited and rather subtle way... But this should not be taken to mean that we are back in the setting produced by the assumption that expectations are formed on the basis of fixed

autoregressive schemes. The information discrepancy assumption does not produce any simple trade-off between the means of output and the price level” (Sargent and Wallace, 1975). They have concluded that their particular optimal monetary policy rule, given by equation (31) in Sargent and Wallace (1975), is perhaps the best way to proceed.

As for a change in policy, Sargent and Wallace (1976) have provided the appropriate analysis. There, they “take note of a very general implication of rationality that seems to present a dilemma. Dynamic models that invoke rational expectations can be solved only by attributing to the agents whose behavior is being described a way of forming views about the dynamic processes governing the policy variables. Might it not be reasonable at times to attribute to them a systematically incorrect view? Thus suppose an economy has been operating under one rule for a long time when secretly a new rule is adopted. It would seem that people would learn the new rule only gradually as they acquired data and that they would for some time make what from the viewpoint of the policy-maker are forecastable prediction errors. During this time, a new rule could be affecting real variables. A telling objection to this line of argument is that new rules are not adopted in a vacuum. Something would cause the change—a change in administrations, new appointments, and so on. Moreover, if rational agents live in a world in which rules can be and are changed, their behavior should take into account such possibilities and should depend on the process generating the rule changes. But invoking this kind of complete rationality seems to rule out normative economics completely by, in effect, ruling out freedom for the policy-maker. . . . The persons on the committee and staffs that constitute the authority matter in the sense that they influence the prospects about policy But the authority has no freedom to influence the parameters of the final form . . . since the objective prospects that it will act wisely or foolishly are known to the public. . . . The conundrum facing the economist can be put as follows. In order for a model to have normative implications, it must contain some parameters whose values can be chosen by the policy-maker. But if these can be chosen, rational agents will not view them as fixed and will make use of schemes for predicting their values. If the economist models the economy taking these schemes into account, then those parameters become endogenous variables and no longer appear in the reduced-form equations for the other endogenous variables. If he models the economy without taking the schemes into account, he is not imposing rationality.” We could call this the Sargent–Wallace paradox of free will in politics. Inside these models, there is no choice of policy, as we go along; rather, what appear to be policy choices are really just random draws from a stochastic process.

Sargent and Wallace (1975) have pointed out and reaffirmed the known result that the price level is undetermined, if the monetary authority follows

a rule that sets interest rates rather than a rule that sets money supplies. In their iconic paper (Sargent and Wallace, 1981), they have pointed out that one should not watch monetary policy alone in order to determine what will happen to inflation, but rather they have pointed to the important interaction between monetary and fiscal policy. If monetary policy later finances fiscal deficits via printing money, then this will imply inflation early on, even if the monetary authority tries to take a tough stance initially, because forward-looking agents rationally foresee the expansion of the money supply down the road. A more radical version of this argument combines the assumption of a passive monetary authority with the assumption of a government that does not adjust taxes in response to rising debt levels in the future, resulting in the fiscal policy being the active player in determining the current price level. This perspective is known as the fiscal theory of the price level (see Leeper, 1991; Sims, 1994; Woodford, 1994). One might wish to see this as a cousin to the analysis of Sargent and Wallace, but the underlying economics is different in important dimensions.

Rational expectations and the irrelevance of systematic monetary policy were so intertwined in the early rational expectations literature that the two were then regarded as synonymous. Of course, we now know that richer models with rational expectations and, for example, sticky wages and/or sticky prices, with policy-induced wedges, with financial frictions, or with lagged effects in various places, are not necessarily in conflict with the view that anticipated macroeconomic policies have real effects. Indeed, while rational expectations impose cross-equation restrictions on the estimation of a fully specified model, typically this does not imply policy irrelevance of the systematic part of monetary policy, let alone fiscal policy. However, the intellectual breakthrough of Sargent and Wallace was to point out that hoping for positive effects of monetary or fiscal policy by counting on fooling and tricking the public is doomed to failure. Policies that seek to be effective need to fish considerably deeper. This has profound practical implications. Politicians all too often seem tempted to talk the public into believing certain outcomes or certain actions, so that they can then afford to do something very different. However, the public is skeptical and does not naïvely believe what politicians promise, much to the chagrin of the politicians in charge. Politics that is all talk but no action will achieve little. Put in this way, this might seem like a patently obvious and commonsensical insight. However, it is important to make sure that our economic models also reflect this insight, in order to render them useful when providing policy analysis and policy advice. Along with Neil Wallace, Robert E. Lucas, Jr., and others at the heart of the rational expectations revolution, Sargent has forced us to clean up our macroeconomic models, and to achieve this consistency with common sense.

IV. Agents as Macroeconometricians: Learning, Self-Confirming Equilibria, and Robust Control

A further insight has emerged from the analysis of policy changes in Sargent and Wallace (1976). The agents in these models should not be naïve macroeconometricians, simply extrapolating a finite past into the indefinite future. If policy changes are afoot, then this information should be taken into account in forming appropriate forecasts. This requires thinking about the economic nature and interaction of macroeconomic variables rather than merely forecasting from a (finite) past. Put differently, past data of finite length alone might not be enough to inform the agent what will happen if certain policy choices occur.

There is a version of this argument which applies to the government agent, as forcefully stated by Sargent (1976b). He has pointed out that “estimates of reduced forms alone will not permit one to settle the difference between Friedman and advocates of rules with feedback. Given any set of reduced-form estimates, there is an invariance assumption that will permit a member of either camp to make his point. . . . for any estimated reduced form, there is an invariance assumption which if imposed delivers the conclusion that one deterministic rule is equivalent with any other. . . . To rule on the policy issue thus requires bringing to bear theoretical considerations or doing empirical work of a kind considerably more subtle than that directed solely at estimating reduced forms. In effect, it is necessary to get some evidence on what sort of invariance assumption is the most realistic one to impose. How one does that is a delicate, though not entirely intractable, task.”

There is a conundrum here. Rational expectations inside a fully specified model is a precisely defined concept, imparting on the agents not only full knowledge of the exact specification of the world they live in, but also an exact knowledge of the value of all parameters and the stochastic evolution of all processes, including the process from which policy choices are drawn. In such a world, then, we can calculate the mathematical expectation of objects and we can equate them to the expectations of agents, be they households, firms, or governments. Agents in these models should not and will not disagree about the key facts. Sargent has called this a “communism of models” (see Evans and Honkapohja, 2005).

Thus, Sargent has said that the optimistic route is to take a fully specified rational expectations model at face value, and to maximize the implied likelihood function (see Evans and Honkapohja, 2005). A less optimistic route is to accept only certain parts of the model as correct, and to find appropriate ways of proceeding. In the same interview, Sargent has stated that “calibration is less optimistic about what your theory can accomplish because you’d only use it if you didn’t fully trust your entire model, meaning

that you think your model is partly misspecified or incompletely specified, or if you trusted someone else's model and data set more than your own. . . . Somehow, calibration was intended as a balanced response to professing that your model, though not correct, is still worthy as a vehicle for quantitative policy analysis. . . . Other people, for example, Larry Christiano and Marty Eichenbaum, . . . [use] GMM estimates using a subset of the moment conditions for the model and data set at hand. Presumably, they impose only a subset of the moment conditions because they trust some aspects of their model more than others."

The inherent conundrum has been part of the criticism of rational expectations macroeconomics from the beginning. Agents do not appear to face the problem that we, as outside macroeconometricians, face, even if we were to agree on the exact structure. Given finite data only, we would be uncertain about the exact value of the underlying parameters. Moreover, economists disagree about which model is appropriate, while the agents inside these models do not seem to face the same dilemma. If our models are meant to capture actual macroeconomic relationships and the actual "psychological expectations" (Sargent and Wallace, 1975) of agents, then it would seem to be only appropriate to assume that the agents in our models are just as confused about these matters as we are, as observing economists and econometricians.

This is a genuine challenge. Sargent has made a number of profound contributions facing up to this challenge, and thus he has created a fascinating research agenda that can reasonably be expected to endure for considerable time. Sargent and Sims (1977) sought to model business cycles "without pretending to have too much *a priori* economic theory", although their criticism then was directed at the prevalent large-scale macroeconomic models rather than the fully specified dynamic stochastic general equilibrium models as used contemporaneously. "Enlisting in Margaret Bray's and David Kreps's research program" (see Evans and Honkapohja, 2005), Sargent analyzed what happens when agents learn about the key parameters of their environment, using versions of least-squares learning or adaptive learning (e.g., Marcet and Sargent, 1989a, 1989b, 1989c). If the true model is within the set of models that are learnable, then more often than not, agents will be capable of learning at least one or several of the rational expectations equilibria, although perhaps not all of them, depending on stability conditions (see Evans and Honkapohja, 2001).

If the true model is not within the set of models that are under consideration by the agent as macroeconometrician, or if a government agent has practically ruled it out, then convergence takes place to a self-confirming equilibrium (i.e., an equilibrium in which the time series generated by the equilibrium reconfirms the estimates used by the agent in the set of models under serious consideration, even if these models do not include that

equilibrium itself; see Cho and Sargent, 2008). Cho and Sargent (2008) have stated that “self-confirming equilibria interest macroeconomists because they connect to an influential 1970s argument made by Sims that advocated rational expectations as a sensible equilibrium concept. This argument defended rational expectations equilibria against the criticism that they require that agents ‘know too much’ by showing that we do not have to assume that agents start out ‘knowing the model’. If agents simply average past data, perhaps conditioning by grouping observations, their forecasts will eventually become unimprovable.” While the absence of the true model within the set of models under serious consideration might already induce small agents to converge to overall mistaken beliefs, the issue is more serious for large agents, such as governments. These agents “can influence the market outcome... [and thus] cannot expect to learn everything that they need to know to make good decisions: in a self-confirming equilibrium, large agents may base their decisions on conjectures about off-equilibrium-path behaviors, which turn out to be incorrect. Thus, a rational expectations equilibrium is a self-confirming equilibrium, but not vice versa” (see Cho and Sargent, 2008). Thus, policy-makers might become stuck with some wrong beliefs about the economy, because past data reconfirm their wrong beliefs. Obtaining data that could contradict these beliefs would require experimenting in directions that do not seem to be advisable, given these beliefs.

However, circumstances can give rise to such experimentation. Policies that work well in normal times can cease to work well when times change. The idea that policy-makers might be unprepared for turbulent times, if they have not occurred in the past, has been pinpointed by Ljungqvist and Sargent (1998) as explaining the dilemma concerning European unemployment. Because self-confirming beliefs are rather stable, such escapes by natural experimentation are rare. As it turns out, among those rare deviations, some are more likely than others. Drawing on the mathematical theory of large deviations, Cho *et al.* (2002) have described how a monetary policy-maker stuck in the bad Nash equilibrium of high inflation might escape it when seeing a string of unusual data.

In his path-breaking Marshall lectures, Sargent (1999) has drawn on and presented the culmination and synthesis of the vast range of his lifetime of exploration into the central role of expectation formations and learning from data. He has applied it to the central issue of macroeconomic policy.⁴ In Sargent (1999), he has explored the implications of rational expectations and policy games, recursive learning algorithms, self-confirming equilibria, and escape dynamics in order to explain how inflation was conquered

⁴ Sargent has taken up the issue of policy experimentation, learning, and the ensuing dynamics in a number of subsequent papers (e.g., Cogley and Sargent, 2002, 2005; Cogley *et al.*, 2007).

in the 1980s. He has juxtaposed two views of that conquest. The first view is “the triumph of the natural-rate theory” (Sargent, 1999). The Friedman–Phelps–Lucas development of the natural-rate hypothesis and the insights into policy irrelevance and short-run trade-offs of the rational expectations revolution won the day, convincing academics and policy-makers alike to refrain from tinkering with the real side of the economy using monetary policy. The second view is “the vindication of econometric policy evaluation[:]. . . policy-makers accepted Samuelson–Solow’s 1960 Phillips curve as an exploitable trade-off [but] . . . also adapted their methods for learning from data and for deducing policy recommendations. . . . That method revealed an adversely shifting Phillips curve, which when interpreted mechanically, led policy-makers to pursue lower inflation” (Sargent, 1999). To analyze the vindication perspective, Sargent (1999) has taken “an important idea from Sims (1988): an adaptive model allows a government to learn from past attempts to exploit the Phillips curve and to discover a version of the natural-rate hypothesis that instructs it to reduce inflation. . . . Escape route dynamics . . . can spontaneously generate regime shifts.” While one might have expected Sargent to show that ideas win, and that the conquest of inflation was indeed a triumph of the Lucas–Sargent–Wallace insights into natural-rate theory and rational expectations macroeconomics, instead he has concluded that the vindication of econometric policy evaluation is the more appropriate perspective. Put differently, government agents might not be agents endowed with rational expectations, but rather they are empirical macroeconomists. It might suffice to conclude that the road taken does not matter because the outcome is really not all that different—after all, inflation was conquered, one way or the other. However, Sargent (1999) has concluded with a dire warning. “Though our simulations and econometric evidence bolster the vindication of the econometric policy evaluation story, we hope that this is the wrong story. We hope instead that policy-makers somehow have learned a correct rational expectations version of the natural-rate hypothesis and have found devices to commit themselves to low inflation. Otherwise, the mean dynamics governing adaption threaten eventually to rekindle inflation.” Put differently, it is now all too easy to forget the hard-earned lessons of the 1970s, to be relaxed about the evil of high inflation in light of the persistently low and stable inflation rates in the US and the European Monetary Union, and therefore once again to turn to monetary policy to tinker with the real side of the economy, stimulating economic activity and solving borrowing problems of fiscal authorities in difficulties. In effect, Sargent (1999) has warned us that if we forget, we shall learn again—the hard way.

The Marshall lectures and related papers presume that there is a choice between several clearly formulated alternative views of the world, such as an exploitable Phillips curve trade-off of the Samuelson–Solow variety

or a natural-rate hypothesis as formulated by Lucas. However, when a benchmark model exists, what are more fuzzy than potential alternative models? How should decision-makers behave, if they wish to ensure that no disasters occur, if the world happens to be somewhat different than they imagined it to be, and if indeed the benchmark model is misspecified? What are good decisions to take that are robust against alternative views of the world that are hard to differentiate from the benchmark model, given the finite data available? These are the issues taken up in a series of papers by Hansen and Sargent (2001a, 2001b, 2003, 2005), and in their book on robustness (Hansen and Sargent, 2008). One of the key insights of this research is that decision-makers can end up behaving as if they were considerably more risk averse, when viewed from the perspective of the benchmark model (see Hansen *et al.*, 1999).

What about entirely different learning schemes, which build on the advances made in psychology and in artificial intelligence of modeling brain learning, such as classifier systems, and imposing bounded rationality? In Marimon *et al.* (1990) and Sargent (1993), Sargent has taken up this perspective and characterized the behavior that emerges when embedding such artificially intelligent agents in an otherwise standard economic setting. This classifier learning scheme has, in turn, been reinterpreted as rule-of-thumb behavior in Lettau and Uhlig (1999) and it has been juxtaposed to dynamic programming. Now, learning might no longer achieve the rational expectations outcome, because of the restricted domain of classifier applicability.

Which agents do we assume to be smarter in the end, those with rational expectations or those who allow for misspecification or learning of some kind? Agents with rational expectations inside fully specified dynamic stochastic general equilibrium models are entirely sure about their world, and they cannot conceive of it any other way. Agents who allow for misspecification and learning are more humble, and perhaps more human. They might concede, as Socrates is once claimed to have done, that “I know that I know nothing”, or that one cannot know anything with certainty. It is debatable whether this is smart or not, but it is certainly wise. Thus, Sargent has taught us how the agents in our macroeconomic models have grown up since the beginnings of the rational expectations revolution in macroeconomics. They are now empirical macroeconomists in an uncertain and ever changing world. This, no doubt, is progress.

V. Sargent as Teacher

It is impossible to think of Sargent without thinking of him as a central teacher of modern macroeconomics to the profession. His textbooks (Sargent, 1987a, 1987b), the exercise companion of Manuelli and

Sargent (1987), and the textbook of Ljungqvist and Sargent (2004) are truly required texts for all subsequent generations of graduate students in economics. Quite literally, modern macroeconomics is what Sargent has taught us. He has shaped how economists now think about and teach macroeconomics, considerably beyond the impact he has had on his own graduate students and the readers of his papers. Aside from his own original and pathbreaking contributions, this has left a profound and lasting impact on our profession. May I speak for all thus affected that we are deeply grateful for his service.

VI. Conclusions

Sargent has fundamentally changed and “irrevocably transformed” (Rolnick, 2010) the field of macroeconomics—whether as sole author, as co-author with collaborators, as an author influenced by others, or as a teacher of the profession, influencing others. This paper is about his contribution to the study of economics.

The Nobel Memorial prize to Sargent and Sims has been awarded for the “empirical research on cause and effect in the macroeconomy” or, more broadly, for macroeconometrics. One often likes to distinguish between theorists, on the one hand, and empiricists and econometricians, on the other.

Sargent holds a unique position in between. A key theme in a large part of his work has been to put the agents in his model on equal footing with the econometrician observing data from the model (i.e., to assume that the agents are themselves empirical macroeconomists or macroeconometricians). As a theorist, he suggests that agents behave as potentially excellent empiricists. As an empiricist, he suggests that agents are potentially excellent theorists. Thus, Sargent is an empirical macroeconomist, but in a sense that runs considerably deeper—the agents in his theoretical models are also empirical macroeconomists. In turn, this has profound implications for thinking about cause and effect in the macroeconomy, and therefore in macroeconomic policy.

In his earlier work, Sargent has explored the implications of rational expectations and the natural-rate hypothesis as formulated by Lucas (1972, 1973) for macroeconometrics. He has derived key policy irrelevance propositions and he has demonstrated that they are empirically relevant. These are benchmark results of the early rational expectations literature. More generally, he has shown how rational expectations models generate cross-equation restrictions, and he has called them “hallmarks” of rational expectations econometrics. This upended the standard macroeconometric practice of the time, and it paved the way for the current and future macroeconometric practice of estimating dynamic stochastic general equilibrium models.

In his later work, Sargent has turned to thinking of households, as well as governments, as agents learning from data in an uncertain world, and he has explored such issues as learning dynamics and robustness. He has shown that the conquest of inflation in the 1980s might have been a result of the continuous updating of conventional macroeconomic models, rather than a triumph of the natural-rate hypothesis and the power of the ideas of the rational expectations revolution. However, if we have indeed arrived at an island of good macroeconomic policy, it can be lost again just as easily, when previous lessons disappear in the fog of the distant past and “the mean dynamics governing adaption threaten eventually to rekindle inflation” (Sargent, 1990). Sargent has taught us how the agents in our models have grown up since the rational expectations revolution. They are thus now responsible for their own mistakes.

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