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A Nobel Prize for Empirical Macroeconomics: Assessing the Contributions of Thomas Sargent and Christopher Sims

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ABSTRACT *This paper provides an assessment of the contributions of the 2011 Nobel Prize winners, Thomas Sargent and Christopher Sims. They received the prize 'for their empirical research on cause and effect in the macroeconomy'. The paper illustrates that Sargent entertained different interpretations of rational expectations during distinct phases of his research. And it shows that Sims shifted the focus from theoretical identification restrictions to identifying the main characteristics of the time series data, a shift of focus from theory to time series.*

1. Introduction

In 2011, the *Sveriges RiksBank Prize in Economic Sciences in Memory of Alfred Nobel* was awarded to Thomas J. Sargent, William R. Berkley Professor of Economics and Business at New York University, and Christopher A. Sims, Harold H. Helm '20 Professor of Economics and Banking at Princeton University, 'for their empirical research on cause and effect in the macroeconomy.'

There are various ways to interpret for what subject or field the 2011 Nobel Prize was awarded. Focusing mainly on the work of Sargent, one could say that this award is the second to recognize rational expectations economics, with Robert Lucas having received the prize in 1995 'for having developed and applied the hypothesis of rational expectations, and thereby having transformed macroeconomic analysis and deepened our understanding of economic policy.' Focusing mainly on Sims' work, one could say that this award is the second to recognize the analysis of time series and its relevance for economic theory,

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with Robert Engle and Clive Granger having received the prize in 2003 'for methods of analyzing economic time series with time-varying volatility (ARCH).' But taking their work together, the award appears to have been given for the contributions of Sargent and Sims to advancing a methodology for empirical macroeconomics. That is, their work taken together covers a broader area of research than cause and effect only.[1]

Macroeconomic questions about the effects of fiscal or financial policies cannot be answered by conducting controlled experiments. Instead we have to work with 'passive observations,' that is, time series data provided by history. To study cause-and-effect or impulse-and-propagation problems, the early econometricians, Jan Tinbergen and Ragnar Frisch, invented the idea of a macroeconom(etr)ic model to investigate these problems empirically.[2] Their early designs led to structural-equations modeling, that is, the Cowles Commission (CC) approach to modeling, which became the dominant approach throughout the 1950s and 1960s.

During the 1970s, this approach saw a growing number of critiques, of which Lucas' (1976) critique was the most devastating. It eventually led to a different kind of modeling, namely modeling based on rational expectations. If one wishes to build models for evaluating policy measures, one would like to have models that indicate what remains invariant under policy interventions. Since the 1970s, economists started to doubt whether the so-called structural equations were indeed capturing invariance, as the CC researchers initially claimed. The Lucas Critique not only changed macroeconomics, but equally changed econometrics. In the early 1980s, the CC approach was no longer the dominant approach; competing methodologies had been developed, such as the general-to-specific approach developed by David F. Hendry, the Bayesian approach by Edward Leamer, and the VAR approach by Sims.[3] In sum, the Lucas Critique changed macroeconomic modeling.

Only a year after the publication of the Lucas Critique, Sargent & Sims (1977) published their (only) jointly written paper 'Business cycle modeling without pretending to have too much *a priori* economic theory,' in which they illustrated the application of time series models to macroeconomics. According to Qin (2011, p. 161), this paper was the 'blueprint of the VAR approach.' It was presented as a reply to Tjalling Koopmans' (1947) critique on the National Bureau of Economic Research (NBER) studies of business cycles, and Koopmans' subsequent debate with Rutledge Vining, better known as the 'Measurement without Theory' debate. According to Sargent and Sims, the CC macroeconometric models used too much *a priori* theory. Economic theory was needed to

[1]The article that provides the 'scientific background' on the 2011 prize is entitled 'Empirical Macroeconomics', which covers more accurately their 'complementary' contributions (Economic Sciences Prize Committee, 2011).

[2]Tinbergen and Frisch were awarded the first Nobel Prize in Economics, 'for having developed and applied dynamic models for the analysis of economic processes.'

[3]At the Fifth World Congress of the Econometric Society (August 19, 1985), for the first time, an Invited Symposium on Econometric Methodology was held. The invited speakers were Hendry, Leamer, and Sims.

identify a model, but following the critique of Liu (1960), many *a priori* restrictions were not 'really reliable' and should be 'mistrusted.' However, instead of Liu's solution that one ought to estimate directly the reduced forms of these models, Sargent and Sims concluded that one should represent the behavior of the variables by a VAR model.

VAR stands for vector auto-regression. A VAR model is a statistical model used to capture the linear interdependences among multiple time series. It describes the evolution of a set of k endogenous variables over a same sample period ($t = 1, \ldots, T$) as a linear function of only their past evolution:

$$y_t = A_0 + A_1 y_{t-1} + A_2 y_{t-2} + \ldots + A_p y_{t-p} + e_t \quad (1)$$

where A_0 is a $k \times 1$ vector of constants, A_i is a $k \times k$ matrix (for every $i = 1, \ldots, p$) and e_t is a $k \times 1$ vector of error terms.

According to Qin (2011, p. 162), this paper attracted more criticisms than approval at the conference where this paper was presented, which led to contradictory responses by both authors. Sims emphasized that the new approach was developed more for hypothesis testing and evaluation than forecasting, whereas Sargent acknowledged that the VAR approach was more for prediction than policy evaluation. It reflects, according to Qin, the difference between the economic theorist Sargent and the econometrician Sims: 'For a macro theorist, it is an opportunity to seek a systematic route of producing empirically operational theories, whereas for an econometrician, it is one to strengthen the theoretical underpinning of empirical models' (Qin 2011, p. 162).

It is to these differences that we now turn, briefly touching on the intellectual lives of Sargent and Sims in Section 2. Next, Section 3 discusses the methods and ideas of Sargent, whereas Section 4 does so for Sims. Finally, Section 5 offers several concluding comments.

2. Intellectual Life

2.1. Thomas Sargent

Thomas Sargent was born in Pasadena, California on July 19, 1943 as the son of an insurance salesman and a social worker. He grew up in Monrovia, California, east of Pasadena and attended college at the University of California at Berkeley. He finished his BA degree with the title of University Medalist, the Most Distinguished Scholar in the Class of 1964. Upon the recommendation of his Berkeley professor Hyman Minsky, he went on to Harvard to pursue a PhD degree, which Sargent received in 1968. During the last year of his PhD program, he also served as a Research Associate at the Carnegie Institute of Technology, where he met John Muth, Robert Lucas, and Herbert Simon.

After obtaining his doctorate, Sargent served for almost two years as First Lieutenant and Captain in the US Army. This was followed by an appointment as Associate Professor of Economics at the University of Pennsylvania. After having been denied tenure, he moved to the University of Minnesota. From there, he went on to the University of Chicago, Stanford University, and since 2002 he has served as the William R. Berkley Professor of Economics and Business at New York University. Sargent became a Fellow of the Econometric Society in 1976 and of the National Academy of Sciences and American Academy of Arts and Sciences in 1983. He has been senior fellow of the Hoover Institute at Stanford University since 1987 and is past president of the Econometric Society (2005), the American Economic Association, and the Society of Economic Dynamics and Control.

2.2. Christopher Sims

Christopher Sims was born in Washington, DC, on October 21, 1942 and attended college at Harvard University, where he took a graduate course in econometrics from Henk Houthakker, who later became his dissertation advisor. He finished his BA degree magna cum laude in mathematics in 1963, and went to the University of California at Berkeley to graduate in economics, where he had first-year econometrics from Dale Jorgenson and first-year economic theory from Daniel McFadden. After one year, for personal reasons, he returned to Harvard where he, like Sargent, received his PhD in Economics in 1968. He stayed at Harvard for another two years as Assistant Professor of Economics; then he moved to the University of Minnesota for a period of 20 years, the first four years as Associate Professor of Economics and thereafter as Professor of Economics. Sargent, who was at Minnesota at that time, was instrumental in recruiting Sims. In 1990 Sims was appointed as Henry Ford II Professor of Economics at Yale University. Since 1999, he has been at Princeton University, first as Professor in Economics and since 2004, as Harold H. Helm '20 Professor of Economics and Banking.

Sims became a Fellow of the Econometric Society in 1974, was co-editor of *Econometrica* for the period 1977–1981 and past president of the Econometric Society (1995). In 1989, he became member of the National Academy of Sciences for having 'inspired a new generation of young econometricians in the building of parsimonious systems to explain the workings of the aggregate economy,' and from 2006 till 2009, he chaired the Economic Sciences section of the National Academy of Sciences.

3. Sargent: Methods and Ideas

Sargent has been instrumental in the development of rational expectations economics, mostly due to his efforts to connect economic theories and econometric tests of those theories. In the process, he collaborated with Sims and Lucas during different phases of his career, as elaborated in subsequent sections.

Economics experienced the so-called rational expectations revolution during the 1960s (Begg, 1982; Guzzardi, 1978; Kim, 1988; Klamer, 1983). Its central idea was that individuals should not make systematic mistakes. Economic agents are not stupid, they learn from their mistakes, and draw intelligent inferences about the future from what is happening around them. While the adaptive expectations hypothesis had the disturbing implication that it allowed individuals to make systematic forecasting errors period after period, the rational expectations hypothesis asserted that people learned from their mistakes. People with rational expectations did still commit errors, but not the same ones each time. Individuals could differ from one another in their expectations and still be rational if they were using different information. But when all these individual expectations were added together, errors tended to cancel out—producing an aggregate view of the future that reflected all the available information.

According to Sargent (1993, p. 21), '[t]he idea of rational expectations is . . . said to embody the idea that economists and the agents they are modeling should be placed on an equal footing: the agents in the model should be able to forecast and profit-maximize and utility-maximize as well as the economist—or should we say econometrician—who constructed the model.' In other words, Sargent saw no reason for superiority of one category of individuals over another group of people.

Sargent gave the rough notion of symmetry a more precise formulation by embedding it in different contexts during different phases of his research, four of which are elaborated below (Sent, 1998). In the first phase, Sargent came to the idea of rational expectations as an econometric concept. During the second phase, Sargent attempted to interpret rational expectations as both an econometric and a theoretic construct. The third phase involved incorporating general equilibrium theory into the symmetry structure. The fourth phase concerned Sargent's eventual interpretation of rational expectations as the final outcome in a learning process.

3.1. Phase 1: Rational Expectations as an Econometric Concept

This phase in Sargent's rational expectations research took place in the late 1960s and early 1970s. At that time, the concept of adaptive expectations was under severe attack in economics for fitting econometric models that forecast better than economic agents (Muth, 1960, 1961). Moreover, theories developed by neoclassical economists were deterministic while models employed by econometricians were random. This was an obstacle in Sargent's search for symmetry.

In response, Sargent attempted to re-establish symmetry by starting from time-series econometrics, in particular distributed lags, and the term structure of the interest rates (Sargent, 1968, 1969). That is, through his analysis of distributed lags for interest rates, Sargent became aware of the role of expectations, because orthodox neoclassical theory stated that they influence the relationship between spot and forward rates, nominal and real rates, and short and long-term rates. Furthermore, rational expectations provided Sargent with an answer to how symmetry might be achieved, as they allowed him to introduce probabilistic ideas in economic theory as well. He considered rational expectations a more elegant way to resolve the separation between the randomness of distributed lags in econometrics and the determinism of neoclassical models. That is, the agents populating the theoretical models were presumed to have rational expectations by employing econometric techniques. Since Sargent initially started from the viewpoint of econometrics, an econometrically motivated interpretation of the concept of rational expectations emerged, which involved treating the econometrician and the agents in the model in a symmetric fashion.

Since he focused on interest rates, Sargent also encountered the importance of Lévy stable distributions with infinite variance and the associated problem of constructing statistical estimators (Glauber & Meyer, 1964; Mirowski, 1990; Roll, 1970). An important characteristic of Lévy stable distributions is that they are stable or invariant under addition. Although their theoretical properties are well established, the lack of analytical closed-form expressions for most stable density functions has been a major source of difficulty in applications. There is not a general estimation method for the parameters of stable laws and there is not much theory of statistical inference for stable laws.

Sargent had established with his student Robert Blattberg at Carnegie Mellon University that with Lévy stable distributions almost every technique of modern econometrics is useless and would have to be discarded (Blattberg & Sargent, 1971). As a result, almost all references to stable Lévy distributions in economic variates disappeared by the mid-1970s and many of the earlier enthusiasts recanted with regard to stable Lévy distributions (Mirowski, 1990). The threat of Lévy stable distributions was averted by ignoring them, without a direct critique of the earlier findings of infinite variance. Randomness, therefore, was tamed by assuming that variances are finite.

Sargent was especially troubled by this, for he sought to establish symmetry between techniques used by economic agents and the models developed by econometricians. In particular, the emergence of an econometrically motivated interpretation of rational expectations required the availability of statistical estimators. Whereas Lévy stable distributions previously only threatened neoclassical econometrics, they could now also compromise economic theory based on rational expectations. When stable laws enter the stage, econometricians and agents would run into difficulties with the construction of statistical estimators. Rather than relinquishing the econometrically motivated interpretation of rational expectations through distributed lags, Sargent somewhat silently gave up Lévy stable distributions with infinite variance.

In this first phase of his quest for symmetry among agents, economists, and econometricians, therefore, Sargent encountered the problem that Lévy stable distributions lack an algorithm for estimating the parameters. This obstructed his attempts to connect the randomness in the models used by econometricians and agents with the determinism in the models developed by economists. Therefore, relinquishing Lévy stable distributions served Sargent well in his attempts to establish symmetry among agents, economists, and econometricians. Yet, this led him to adopt the 'unrealistic' assumption that data previously shown to have exhibited infinite variance now followed a distribution with finite variance.

3.2. Phase 2: Rational Expectations as both an Econometric and a Theoretic Construct

While econometricians were the first promoters of rational expectations, their initial focus changed from lag distributions to vector autoregressions during the late 1970s and early 1980s. While the desire to establish symmetry among agents, economists, and econometricians continued to guide Sargent, this change led him to advocate a different connection. That is, while he continued to start from the perspective of econometrics during this phase of his work, he now tried to establish symmetry by incorporating vector autoregressions.

In this period, Sims was developing his VAR methodology for macroeconomic modeling. Inspired by Sims's econometric approach, Sargent concentrated on restricting vector autoregressions on the econometric side of symmetry. As before, Sargent ended up with an econometrically motivated interpretation of rational expectations. Rather than handling distributed lags, symmetry between agents and econometricians now required that agents with rational expectations fit vector autoregressions. The inclusion of economists in Sargent's symmetry picture further mandated using the acquired econometric information to construct a theoretical model. However, a major disciplinary difficulty in the form of observational equivalence (Sargent, 1976) emerged and hindered Sargent. That is, contradictory theoretical models could both generate the very same vector autoregressive relations. Since the vector autoregressive model relied on observed regularities not traced to underlying behavior and the structure of the complete system was not taken into consideration, the reliability of the model was unknown and vector autoregressions were liable to produce misleading forecasts. To incorporate economic theory in his symmetry structure, Sargent felt he needed to overcome the problem of observational equivalence by establishing a stronger connection between vector autoregressions and economic theory.

With the aid of his colleague Lars Hansen, Sargent responded to the problem of observational equivalence by showing that vector autoregressive models were not necessarily atheoretical (Hansen & Sargent, 1981a, 1981b, 1990, 1991b). In particular, Hansen & Sargent (1991b, p. 1) argued that their 'goal has been to create a class of models that makes contact with good dynamic economic theory and with good dynamic econometric theory.' Having grounded 'good dynamic econometric theory' in the engineering tools of vector autoregressions, Hansen and Sargent searched for 'good dynamic economic theory' in the engineering theory of recursive dynamics and linear optimal control. Although the combination of vector autoregressions, recursive dynamics or linear optimal control, and rational expectations helped Sargent to establish symmetry in this phase, new difficulties arose because this combination was technically not terribly successful, difficult to implement, and based on controversial assumptions.

In addition, Sargent became aware of the fact that his analysis relied on outdated engineering techniques that require much stability. He had largely avoided questions about the way in which economic agents make choices when confronted by a perpetually novel and evolving world. This was so, despite the importance of the questions, because his tools and formal models were ill suited for answering such questions. Changes in his environment and the appearance of a few extra difficulties were necessary for Sargent to move in this direction. These additional difficulties emerged during the third phase.

3.3. Phase 3: Rational Expectations with General Equilibrium Theory

This phase is centered on Sargent's eventual interpretation of rational expectations as individual rationality and mutual consistency of perceptions. It is this phase that is singled out in the Nobel Prize award.

From roughly the early to mid-1980s, Sargent focused on incorporating general equilibrium theory in his framework of rational expectations and vector autoregressions (Lucas, 1987). The general equilibrium framework imposed full theoretical restrictions on the coefficients in the vector autoregression and therefore avoided the problem of observational equivalence encountered before. Whereas the previous two phases in Sargent's work started with the conception of agents as econometricians while economists were added as somewhat of an afterthought, the present phase started with the concept of agents as little economists while econometricians were added as somewhat of an afterthought.

Although rational expectations pioneer Robert Lucas had used general equilibrium theory from the start, it took Sargent until the late 1970s to move in this direction. During that time, he spent a year as a visiting professor at the University of Chicago and took two courses from Lucas. Sargent sought to establish symmetry by linking the vector autoregressions employed by econometricians and the general equilibrium theory developed by economists through the concept of rational expectations. Hence, in the interpretation of the concept that emerged, agents have expectations that are rational when these depend, in the proper way, on the same things that economic theory says actually determine that variable. A collection of agents is solving the same optimization problems by using the relevant economic theory and the solution of each agent is consistent with the solution of other agents. Econometric methods can then be used to estimate the vector autoregressions that result from this economic model. For Sargent, establishing symmetry among agents, economists, and econometricians with this set-up was facilitated by the fact that general equilibrium theory involved an *a priori* bias towards symmetry among agents.

Although Sargent had finally achieved symmetry, his search does not have a happy ending here. Instead, Sargent encountered new difficulties. First, if there is symmetry among the agents, then there is no reason for them to trade with each other, even if they possess different information. Instead of there being a hive of activity and exchange, Tirole (1982) proved that a sharp no-trade theorem characterizes rational expectations equilibria (Sargent, 1993, p. 113). Second, agents and econometricians have to be different in order to justify the error term. When implemented numerically or econometrically, rational expectations models need to impute more knowledge to the agents within the model, who use the equilibrium probability distributions, than is possessed by an econometrician, who faces estimation and inference problems that the agents in the model have somehow solved (Sargent, 1987, p. 79). Third, there is a need for asymmetric actors in rational expectations economics for the concept of policy recommendations to make sense. In particular, making recommendations for improving policy amounts to assuming that in the historical period the system was not really in a rational expectations equilibrium, having attributed to agents expectations about government policy that did not properly take into account the policy advice (Sargent, 1984, p. 413). A fourth problem concerns the issue of conceptualizing learning if agents are thought to behave like econometricians. In particular, econometric metaphors of reasoning possess a blind spot for the process of information search and errors made in information collecting, because econometric theories of inference and hypothesis testing are applied after the data have been collected; they do not start until the variables and numbers needed for the formulas are available (Sargent, 1993, p. 23).

In the present phase, therefore, Sargent was yet again unable to maintain symmetry within the set-up he had developed. These difficulties, combined with the ones outlined in the previous phase, eventually jointly transformed Sargent's entire program. In the subsequent phase, Sargent tried to reimpose symmetry among agents, economists, and econometricians by making them all boundedly rational.

3.4. Phase 4: Rational Expectations as the Learning Outcome

Sargent changed his attitude towards rational expectations in response to developments in the late 1980s. During this period, Sargent became involved with the Santa Fe Institute. Complexity, intractable unpredictability, spontaneous self-organization, adaptation, nonlinear dynamics, computational theory, upheavals at the edge of chaos, inductive strategies, new developments in computer and cognitive science—these were some of the themes taken up by researchers at the Santa Fe Institute. Begun by a number of distinguished physicists at the Los Alamos National Laboratories, the Santa Fe Institute originally had nothing to do with economics. This changed with a workshop on 'Evolutionary Paths of the Global Economy' from 8–18 September 1987 at the Institute campus in Santa Fe (Anderson *et al.*, 1988; Pool, 1989). The gathering was successful enough to continue the economics program at the institute, which focuses on the economy as a complex, constantly evolving system in which learning and adaptation play a major role.

One area that received a great deal of attention during the workshop was the specific question of how economic agents take the future into account when making decisions. The axiom of rational expectations seemed patently untrue to the physical scientists, who were acutely aware of the difficulties inherent in predicting the future. The problem in developing a more 'realistic' model was that if economic agents were assumed to be able to anticipate the future, but not perfectly, then it is hard to know just how imperfect rationality should be. One suggestion was to develop theoretical economic agents that learned in the way actual economic agents did, which was in line with Sargent's desire to restore symmetry. Before embracing a Santa Fe-type approach, however, Sargent tried to deal with the problems encountered during the previous phases in different ways.

The asymmetry among agents, economists, and econometricians that emerged within the setting of rational expectations, general equilibrium theory, and vector autoregressions bothered Sargent. In response, he was led to revise part of his framework in the mid-1980s. Instead of starting from rational expectations, Sargent considered adaptive expectations in work, mostly co-authored with Albert Marcet, who was a graduate student at the University of Minnesota during Sargent's tenure there and who subsequently followed in Sargent's footsteps by accepting an Assistant Professor position at Carnegie Mellon University (Marcet & Sargent, 1988, 1989a,b,c, 1992). The models they developed were adaptive in the sense in which that term is used in the control literature (but not in the macroeconomics literature). That is, the agents were assumed to behave as if they know with certainty that the true law of motion is time invariant. Because the agents operate under the continually falsified assumption that the law of motion is time invariant and known for sure, the models do not incorporate fully optimal behavior or rational expectations.

The appeals to the control literature also returned Sargent to his earlier collaboration with Lars Hansen (Hansen & Sargent, 2008). Together they explored a class of mechanisms for expectations formation based on robust control, which starts from the perspective that the agent has an imperfect understanding of how the economy works. That is, agents are not only averse to risk, but also do not know the true stochastic process that generates uncertainty. Hansen and Sargent's research on robustness, was in fact inspired by Sims (1971a, 1972a). At the same time, it has been criticized by Sims (2001) for leaving the Bayesian one-model framework of Savage (Evans & Honkapohja, 2005, p. 577).

Unwilling to relinquish rational expectations entirely, Sargent did not see learning as anything really new in economics. Instead, he saw it as a way of strengthening the standard ideas and dealing with their problems—as a way of understanding how economic agents will grope their way toward neoclassical behavior even when they are not perfectly rational (Sargent, 1993, p. 23). In particular, he tried to reinforce rational expectations by focusing on convergence to this equilibrium (Marcet & Sargent, 1992, p. 140). He also tried to use learning with adaptive expectations to deal with some of the problems associated with rational expectations (Sargent, 1993, p. 25). Finally, incorporating learning could assist in the computation of equilibria (Marcet & Sargent, 1992, p. 161).

This new framework, however, did not fully allow Sargent to satisfy his interests due to the emergence of new difficulties. That is, the resulting representation was 'unrealistic' in the sense that agents were assumed to have already formed a more-or-less correct model of the situation which they were in, and learning was just a matter of sharpening up the model a bit by adjusting a few knobs. Since Sargent had moved towards picturing economists and econometricians as being far from rational and knowledgeable about the system they analyze, this 'unrealistic' picture still left him with a rather weak attempt at establishing symmetry.

Unhappy with the 'unrealistic' interpretation of learning under adaptive expectations that had emerged, Sargent wanted something closer to the way economists and econometricians learn. The Santa Fe meeting inspired Sargent to appeal to artificial intelligence. Instead of assuming that agents were perfectly rational, they could be modeled as being artificially intelligent and they learn from experience, like real economic agents. Rather than modeling the economy as a general equilibrium, societies of interacting artificially intelligent agents could be organized into an economy. Reluctant to abandon his earlier contributions completely, Sargent did not go all the way with Santa Fe. Instead of relinquishing the neoclassical notion of an equilibrium, he focused on convergence to equilibrium (Marimon et al., 1990).

Sargent saw what he called his bounded rationality program as an effort to restore symmetry among agents, economists, and econometricians. He now moved to picturing agents, economists, and econometricians alike as being boundedly rational but converging to rational expectations. Ironically, however, the move to artificial intelligence left Sargent with a new asymmetry that emerged between him and the agents in his models. Specifically, when Sargent made agents more bounded in their rationality, he had to be smarter because his models became larger and more demanding econometrically. Furthermore, artificial intelligence did not allow Sargent to fully establish symmetry, because the proliferation of free parameters in the bounded rationality program left him with an asymmetry between economists and econometricians:

> Bounded rationality is a movement to make model agents behave more like econometricians. Despite the compliment thereby made to their kind, macroeconometricians have shown very little interest in applying models of bounded rationality to data. Within the economics profession, the impulse to build models populated by econometricians has come primarily from theorists with different things on their minds than most econometricians. (Sargent, 1993, pp. 167–168)

This phase illustrates how Sargent's attempts at establishing symmetry continued to be frustrated. Sargent himself acknowledged that neither learning through adaptive expectations nor learning through artificial intelligence established the symmetry he sought. Whereas adaptive expectations excluded agents from the symmetry structure, artificial intelligence continued to exclude agents from the symmetry structure and further left Sargent with an asymmetry between economists and econometricians.

3.5. Applied Research on Rational Expectations

In addition to the theoretical research outlined above, Sargent has also contributed to the literature on economic history, including influential work on monetary standards and international episodes of inflation. Although less technical, this research clearly starts from a rational expectations perspective.

In *The Conquest of American Inflation*, Sargent (2001) analyzed the rise and fall of US inflation after 1960. Considering the behavior of inflation and unemployment in this period, Sargent explains of how American policymakers increased inflation in the early 1960s by following erroneous assumptions about the exploitability of the Phillips curve—the inverse relationship between inflation and unemployment. Extending both adaptive expectations and rational expectations theory, Sargent describes postwar inflation in terms of drifting coefficients. He interprets his results in favor of adaptive expectations as the relevant mechanism affecting inflation policy.

Lars Ljungqvist and Sargent modeled the European job market using a rational expectations framework with human capital dynamics and labor market frictions that impede the ability of displaced workers to find new jobs (Ljungqvist & Sargent, 1998, 2003, 2008). This model is able to capture the interaction of changes in the microeconomic environment confronting individual workers and Europe's generous unemployment compensation system. This interaction generates lower unemployment during periods of low turbulence such as in the 1950s and 1960s, but it generates persistently high unemployment during periods of high turbulence such as in the 1970s and 1980s.

Along with Francois Velde, Sargent (& Velde, 2002) analyzed government debt, defaults, and the subsequent inflation around the time of the French Revolution. Their book, *The Big Problem of Small Change*, explains why French policies led to fiscal imbalances, defaults, and inflation along with government instability. The *Big Problem* the book addresses is the observation that governments are hard-pressed to provide a steady supply of *Small Change* because of its high costs of production. In the nineteenth century, governments replaced the small change in use until then with fiat money, thereby ensuring a secure flow of small change. By solving this problem, Sargent and Velde argue, modern European states laid the intellectual and practical basis for the diverse forms of money that comprise the modern monetary system.

4. Sims: Methods and Ideas

Sims' (1967) first publication, 'Evaluating short-term macro-economic forecasts: the Dutch performance' is a good starting point to discuss his work. It deals with a theme that runs through most of his work: what kind of macroeconomic model is most useful for evaluating economic policy. The paper is about the Netherlands Central Planning Bureau (CPB), the only economic policy institute in the Netherlands at that time, and was based on Jan Tinbergen's idea that policy evaluations should be based on large structural-equations models.[4]

The common view at that time was that models for policy evaluation should be validated on their predictive performance, which was directly linked to their performance as tools for policy evaluations. This involved comparing the predictive performance of models with those of the naive models. A naive projection is a projection that is based on 'naive' models such as $y_{t+1} = y_t + \varepsilon_t$ and $y_{t+1} - y_t = y_t - y_{t-1} + \varepsilon_t$, where ε_t represents white noise.

So, the larger question of 'how useful a large econometric model may be in short-term macroeconomic planning' (Sims, 1967, p. 235) became the issue of: 'how the Dutch system has performed purely as a forecasting system, relative to naive projections or to less econometrically elaborate systems in other European countries' (Sims, 1967, p. 225). Sims showed that 'the Dutch forecasts have in fact been significantly better than naive projections' (Sims, 1967, p. 225).

[4]Actually, Tinbergen built the very first two macroeconom(etr)ic models. The first was of the Dutch economy, published in a Dutch report in 1936, and the second was of the US economy (Tinbergen, 1939). After the Second World War, the CPB was founded and Tinbergen became its first director. His ideas concerning economic policy that he developed at the CPB were published in Tinbergen (1952) and Tinbergen (1956). This specific type of model-based economic policy was the main target of Lucas' (1976) critique on economic policy.

Although Sims mentions ‘other European countries’, Dutch performance was compared only to Norway. The Norwegian performance was highly influenced by Ragnar Frisch’s ideas on economic policy. Hence, we have an empirical comparison between the ideas of economic policy of the two pioneers of economic policy based on econometric models. The Dutch forecasts had been based on a ‘fairly large’ econometric model, the Norwegian forecasts not. It appeared that the Norwegian forecasts were not significantly better than the naive projections.

There was, however, another interesting difference between the Netherlands and Norway due to the different views of Tinbergen and Frisch, namely the dissimilar political functions and responsibilities between the two ‘planning agencies.’ In the Netherlands, the CPB functions ‘as an advisory organ which renders assistance in the coordination of economic policy’ (Sims, 1967, p. 235), while in Norway the government was held responsible for the achievement of the Norwegian forecasts. Hence, policy-makers had a stronger interest in the size of the figures than in the Netherlands.

> Since the responsibility of policy-executors for the plan figures necessarily brought with it greater participation by them in its preparation, they had at the same time more direct channels of influence on it than are likely to have existed in the Netherlands. In short, forecast accuracy *per se* is likely to have been a primary objective for the Dutch planners, an intermediate one for the Norwegians. (Sims, 1967, p. 235)

According to Sims (1967, p. 235), it was unlikely that this political factor could entirely explain the better Dutch forecasts, ‘but it should be borne in mind as a qualification to the intercountry comparison’. From then on, the relation between modeling and policy remained a central interest of Sims.

As a spin-off from his dissertation work with Houthakker, Sims’ early research considered a variety of problems connected to statistical approximation. This work included the study of discrete-time approximation of continuous-time models (Sims, 1971a), the approximation of finite parameter distributed-lag models to more general dynamic economic models (Sims, 1972a), and the general problem of statistical approximation in rich or high (infinite) dimensional parameter spaces (Sims, 1971b).

The first of his applied papers that attracted considerable interest from macroeconomists in the 1970s, including Friedman, was his paper ‘Money, income and causality’ (Sims, 1972b). It attracted a large amount of interest because it came out in the peak of the monetarist-Keynesian controversy, and a lot of macroeconomics research was centered on this controversy. Its main empirical finding is that the hypothesis that causality is unidirectional from money to income agrees with the postwar US data, whereas the hypothesis that causality is unidirectional from income to money is rejected. Although the paper seems to say that Friedman was right and that the Keynesians were wrong, its real message was more sophisticated: if you accept money as exogenous in the regression equation that explains income and interpret this regression equation as characterizing the response of the economy to the money stock, the estimated equation still implies that only a fairly small fraction of all output variation was explained by the money stock.

In other words, this was not a simple black-and-white controversy. Both sides told a part of the causal story.

> What was true then and is still true now is that it's very hard to get evidence that monetary policy is as important as most people seem to think it is, and certainly as Friedman seemed to think it was, at the time, in generating business cycles. Tobin saw that this result really didn't undermine the view that there was a lot else going on in the economy and possibly a lot of other policies would be important. (Sims interviewed by Hansen, 2004, p. 278)

This dispute about the causal role on the money stock led to a few papers on the concept of causality. The methodological novelty of the 1972 paper was the use of Granger's test of causal ordering (Granger, 1969).

Although money was originally investigated as an exogenous variable, subsequent research showed, however, that money was predictable by the interest rate, and this predictable part of money was most strongly associated with output. Moreover, when monetary authorities smooth interest rates, the money stock starts moving in line with asset prices. While, strictly speaking, money is therefore statistically endogenous, it nevertheless may look as if it is causally prior in a Granger sense ('a historically reliable pattern of dynamic statistical relations, which look like causal relations ought to look'), so the seeming Granger causality of monetary aggregates to other macroeconomic variables is not a true causal relationship (Sims, 1980a).

4.1. VAR modeling

Sims' VAR approach 'dissented vigorously from the Cowles Commission tradition' (Pagan, 1987, p. 14).[5] The appeal of VAR models is based in part on skepticism of the empirical validity of tightly parameterized models. 'I think the most reliable way to do empirical research in macroeconomics is to use assumptions drawn from "theory," which actually means intuition in most cases, as lightly as possible and still develop conclusions' (Sims interviewed by Hansen, 2004, p. 282).

The 'manifesto' of Sims' VAR approach is his paper 'Macroeconomics and reality' (Sims, 1980b; see Qin, 2011, p. 162). Its specific attack was on ad hoc dynamic restrictions used too frequently for identification purposes: 'the identifi-

[5]Pagan (1987) summarizes three alternative methodologies—Hendry, Leamer and Sims. His (Pagan, 1987, pp. 15–16) critical evaluation of Sims' methodology summarizes this methodology in four steps:

1. Transform data to such a form that a VAR (see equation (1)) can be fitted to it.
2. Choose as large a value of p and the number of variables in y_t as is compatible with the size of data set available and then fit the resulting VAR.
3. Try to simplify the VAR by reducing p or by imposing some arbitrary 'smoothness' restrictions, such as monotonicity, upon the coefficients.
4. Use a recursive decomposition of the estimated residuals in order to 'identify' the impulse response functions.

cation claimed for existing large-scale models is incredible' (Sims, 1980b, p. 2). It was not aiming at getting rid of large-scale macro-models, because 'they are useful tools in forecasting and policy analysis' (Sims, 1980b, p. 11), but the restrictions imposed in the usual style of identification are neither essential to constructing a model that can perform these functions nor innocuous:

> How can the assertion that macroeconomic models are identified using false assumptions be reconciled with the claim that they are useful tools? The answer is that for forecasting and policy analysis, structural identification is not ordinarily needed and that false restrictions may not hurt, may even help a model to function in these capacities. (Sims, 1980b, p. 11)

To keep the models useful for policy analysis and forecasting, the VAR approach proposes to identify the model on the dynamic characteristics of the time series, the 'empirical regularities.'

The VAR approach tends to lead to experimentation with different kinds of models and different restrictions, and essentially informally or formally averaging across the results. Initially, this implied the use of models as black boxes. However, Sims later acknowledged that for getting people to use a model, they should be able to tell stories about what is going on inside the model.

> We also can investigate any desired aspect of the discrepancy between our model's implications and the behavior of the data, because we can simulate solutions of it. Any use of a model to trace out the impacts of policy interventions will require use of its full set of dynamic implications. (Leeper & Sims, 1994, p. 82)

They used a dynamic stochastic general equilibrium (DSGE) model to fit the data. DSGE models lend themselves to simulations more easily because they are much simpler representations than the more complex large-scale macro-economic models. Therefore, they are more useful to trace the impacts of policy interventions (Boumans, 2006). 'We also can investigate any desired aspect of the discrepancy between our model's implications and the behavior of the data, because we can simulate solutions of it. Any use of a model to trace out the impacts of policy interventions will require use of its full set of dynamic implications' (Leeper & Sims, 1994, p. 82).

DSGE modelers such as Kydland and Prescott, however, argue strongly against using econometric tools, and instead prescribe calibration to validate their models. Contrary to this position, Sims (1996) argues that DSGE modeling and econometrics should 'converge.' In this period, he also opted for a Bayesian style of econometrics instead of a hypothesis-testing style of econometrics. After Arnold Zellner's (1971) attempt in the 1960s and 1970s, and Edward Leamer's in the 1980s, Bayesian econometrics has found its third prominent missionary.

5. Summary and Conclusion

While large national econometrics models were successful in the 1950s and 1960s, they performed much worse in the 1970s. They did not successfully predict and could not explain the simultaneous high inflation and unemployment rates. Together with the Lucas Critique, these developments changed macroeconomic modeling. One change led away from the Cowles Commission econometric methodology to the calibration methodology. The other change aimed at a new convergence—DSGE modeling with a new econometric methodology, namely the VAR approach. Both Sargent and Sims contributed to the latter approach.

As this paper has illustrated, Sargent entertained different interpretations of rational expectations in four phases of his research. Furthermore, Sargent's choices were partly inspired by his social environment—Blattberg in the late 1960s through early 1970s, Sims and Hansen in the late 1970s through early 1980s, Lucas in the early to mid-1980s, and Santa Fe and Marcet in the late 1980s through early 1990s.

In the late 1960s and early 1970s, Sargent used an econometrically motivated interpretation of rational expectations with a focus on distributed lags. In the late 1970s and early 1980s, this emphasis changed to vector autoregressions. During both these phases, Sargent started with the conception of agents as econometricians while economists were added to the symmetry picture as somewhat of an afterthought. In the early to mid-1980s, Sargent focused on how rational expectations in a general equilibrium framework could lead to vector autoregressions. During this phase, he started with the conception of agents as economists while econometricians were added as somewhat of an afterthought. In the late 1980s and early 1990s, Sargent tried to show convergence to rational expectations through learning by agents, economists, and econometricians alike through the use of adaptive expectations or artificial intelligence.

Sims' main contribution to this development was to show how macro-econometric modeling should be revised in order to counter the Lucas Critique. This did not imply abandoning theory, but involving theory as little as possible. It shifted the focus from theoretical identification restrictions to identifying the main characteristics of the time series data—a shift of focus from theory to time series.

Whereas Sargent & Sims (1977) published one paper together on the application of time series models to macroeconomics, their interpretation of its results were different. That is, whereas econometrician Sims viewed it as an attempt to strengthen the theoretical underpinnings of empirical models, economic theorist Sargent viewed it as an opportunity to seek a systematic route of producing empirically operational theories.

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