"Economists, Incentives, Judgment, and the European CVAR Approach to Macroeconometrics"

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Economists, Incentives, Judgment, and the European CVAR Approach to Macroeconometrics[1]

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Abstract: This paper argues that the DSGE approach to macroeconometrics is the dominant approach because it meets the institutional needs of the replicator dynamics of the profession, not because it is necessarily the best way to do macroeconometrics. It further argues that this "DSGE-theory first" approach is inconsistent with the historical approach that economists have advocated in the past and that the alternative European CVAR approach is much more consistent with economist's historically used methodology, correctly understood. However, because the European CVAR approach requires explicit researcher judgment, it does not do well in the replicator dynamics of the profession. The paper concludes with the suggestion that there should be an increase in dialog between the two approaches.

Keywords: methodology, macroeconometrics, general to specific, DSGE, VAR, judgment, incentives

JEL classification: C10. A1

1. Introduction

> To tell an economist that he chooses that type of work and that viewpoint which will maximize what his income is, he will hotly say, a studied insult. Such market-oriented behavior will be characterized not with our customary phrases such as consumer sovereignty, but in terms as harsh as "intellectual prostitution". To adapt one's views to one's audience is hardly to be distinguished from the falsification of evidence and other disreputable behavior.—George Stigler

In this opinion paper I ask a simple question: Why has the European General-to-Specific Approach to Empirical Macro (Hendry, 1995, Johansen, 1976 and Juselius, 2006) had only limited success in the competition for ideas for doing applied macroeconomic policy in the U.S.? As an illustration of the General-to-Specific approach I shall discuss the cointegrated vector auto regression (CVAR) approach, primarily developed in Europe and used by many of the contributors of this special issue, and compare it to the Dynamic Stochastic General Equilibrium (DSGE) approach, primarily developed in the U.S. where it is considered the correct approach to doing macroeconometrics.

The initial reaction to this question by most U.S. economists likely will be: "What approach is he talking about? We haven't heard of the CVAR approach; it must be a minor approach by some out-of-the-mainstream economists." Given the lack of

[1] I would like to thank Peter Kennedy, Katarina Juselius, Lawrence Boland, Kevin Hoover, Casey Rothschild, Aris Spanos, and Tomas Mayer for important suggestions on earlier versions of this paper. I have definitely not taken all their advice so only I am responsible for the arguments here.

familiarity of many economists with this approach, I will discuss in Section 2 what I mean by the European CVAR approach, why I call it European, and how it differs from the DSGE approach. In Section 3 I shall discuss some hypotheses explaining why the European CVAR approach is not winning out in the competitions of ideas and methods and relate these to the notion of a representative researcher and the invisible hand of truth. In Section 4 I take a historical look at the evolution of macroeconomic theory and how that evolution is related to incentives; I argue that there is little historical foundation for the pre-eminence of theory approach as interpreted by DSGE advocates, and in fact, there is a historical evidence suggesting that their approach is an approach that earlier economists would have strongly condemned. In Section 5 I characterize the policy implications of the two approaches and discuss the role of theory and judgment in the CVAR versus the DSGE approach. In section 6 I combine the arguments in the above sections, and argue that given the current replicator dynamics of the academic economics profession, there is a bias against methods, such as the European CVAR method, that explicitly require the use of researcher intuition and judgment in the analysis.

2. The European CVAR and the U.S. DSGE Approach to Econometrics

There are two ways of thinking about the macroeconomy. The first, which I call the Walrasian approach, sees the macro economy as a system that can we can best understand through the lens of formal micro-founded theory, based in carefully specified micro foundations. Most recently, it is an approach associated with the DSGE model. It is the dominant approach taught in U.S. graduate schools and held by U.S. macro economists. It is a formal theory first approach. As Campos, Eriksson and Hendry (2005) point out this approach "insists on a complete theoretical model of the phenomena of interest prior to data analyses." (pg 1)

The alternative approach, which I see the European CVAR approach as consistent with, sees the macro economy as more complex than that and does not see a rigid microeconomicly grounded theory as especially helpful in shedding light on most macroeconomic problems. This approach, which elsewhere I have called the Post Walrasian approach (Colander, 2006), has also been nicely described by Campos, Eriksson and Hendry. It sees the economy as "a complicated, dynamic, nonlinear, simultaneous, high-dimensional, and evolving entity [in which] "social systems alter over time; laws change and technological innovations occur." (pg. 1) This alternative approach can be found in small pockets throughout the world, but tends to be more prevalent in Europe, which until recently did not buy into the DSGE approach anywhere near as completely as did the U.S.

There tends to be a similar divide between the U.S. and Europe in macroeconometrics. The DSGE theory-first approach to macroeconometrics tends to dominate in the U.S. while in Europe there has been, until recently, a more eclectic approach, and it is within these eclectic approaches that one finds the CVAR approach.

2.1. Methodology of the Two Approaches

The two approaches to macroeconometrics differ significantly in their underlying methodology. The Walrasian approach, which underlies the predominant dynamic stochastic general equilibrium (DSGE) approach in macroeconomics, concentrates on carefully developing the theoretical model first. Advocates argue that one must first specify the theoretical model before one can even have a hope of adequately grasping the complex empirical reality. If one doesn't develop, and stay true to, such a carefully specified theoretical model, one will likely be fooled by spurious empirical relationships. This means that a DSGE researcher sees all macroeconomic issues through a DSGE lens. To keep the formal model tractable, this DSGE approach generally requires the researcher to disregard the institutional environment and complex dynamics as possible explanations for why what we observe differs from what theory predicts.

The DSGE approach requires that only a fully pre-specified theoretical model can be brought to the data. It is a "theory-first" methodology, where "theory first" means a carefully specified and fully developed formal theory which may deviate significantly from the characteristics of the economy that intuitively might be important. Only after having fully developed the underlying microeconomic theory in a highly simplified model do advocates of the DSGE approach bring their model to the data. When they do bring it to the data, they generally use calibrated values for some parameters of the model and Bayesian estimation methods to reconcile the information in the data with the theory model. While this DSGE approach allows for some flexibility by representing part of the empirical dynamics with a simple VAR it usually does so without using the VAR to check for misspecification such as parameter non-constancy (parameters are assumed to be constant).

What I am calling the European CVAR approach uses a quite different methodology. Because it sees the outcomes of the economy as data points from a complex system, where by complex I mean a system that involves so many interactions and potential non-linearities that intuitively, one could not hope to fully specify a formal model of the system, the European CVAR approach gives smaller weight to any specific formal theory, and instead uses a broad heuristic theoretical understanding of the economy, which is guided by, but not necessarily dominated by, a formal theory.[2] Thus, for example, the European CVAR approach would address the recent crisis within a system of equations where economic behavior is allowed to persistently deviate from long-run economic equilibrium states. It would provide information on which other variables react on these persistent movements away and where in the system the adjustment takes place. Rather than assuming one correct theory it would be open to theoretical explanations that are consistent with agents who drive prices away from long-run attractors for significant periods of time.

The European CVAR approach does not deny rationality and equilibrium as the foundation of hard core theory; it simply questions the usefulness of an oversimplified theoretical model that, to anyone other than a true believer, intuitively does not

[2] I have discussed these issues further in Colander, (1996, 2006).

correspond to a model that would reasonably explain economic behavior as it manifests itself in observed economic data.[3]

Given the concern about knowledge that can be deduced from formal theory that CVAR advocates have, it is not surprising that the CVAR approach gives more emphasis to data analysis. Advocates of this approach use a carefully constructed econometric methodology designed to extract as much information as one can from the data. (My focus in this paper is the cointegrated vector autoregressive (CVAR) approach advocated by Søren Johansen and Katarina Juselius (Johansen and Juselius, 2006, Juselius, 2006), but the approach is also related to the related general-to-specific approach advocated by David Hendry (Hendry, 2000, 2009).(I see both of these approaches are consistent with the broad archeological approach methodologically advocated by Kevin Hoover (Hoover, 2006, Hoover, Johansen and Juselius, 2008)).

These approaches all share the feature that they view economic reality as a dynamic system of forces that move equilibria (pushing forces, which give rise to stochastic trends) and forces that move equilibria (pulling forces, which give rise to long-run relations) (Hoover et al., 2008). Thus in this European CVAR approach the formal theory of a static economy is adapted to a more heuristic theory that incorporates the researcher's judgment about the effects of institutions, and dynamics on the theoretical results into one's theoretical intuition of what the formal theory is telling one. The European CVAR macroeconometric approach is designed to allow the *complexity* of the economic reality to speak as freely as possible through the lens of the *institutional environment*. The data analysis blends with the theoretical analysis to produce a vision of reality that is not necessarily correct, but is the best that can be arrived at given such a complex system as the macroeconomy.

2.2. The Importance of Judgment in the European CVAR Approach

The important aspect of the European CVAR approach for my argument in this article is that it explicitly requires the researcher to use judgment about the applicability of theoretical, institutional and empirical information to arrive at a conclusion from the analysis. The analysis is as much art as it is science. It is an approach that has a long history in economics and I would argue is consistent with the Marshallian/Keynesian approach that J.M. Keynes summarized as follows:

> Economics is a science of thinking in terms of models joined to the art of choosing models which are relevant to the contemporary world. It is compelled to be this, because, unlike the typical natural science, the material to which it is applied is, in too many respects, not homogeneous through time. The object of a model is to segregate the semi-permanent or relatively constant factors from those which are transitory or fluctuating so as to develop a logical way of thinking about the latter, and of understanding the time sequences to which they give rise in particular

[3] When considering the simplicity of the underlying theory assumptions relative to the complexity of the economy typical of many DSGE models, it may not come as a big surprise that they have been essentially silent about explaining many observed events in particular the more recent ones.

> cases. Good economists are scarce because the gift for using "vigilant observation" to choose good models, although it does not require a highly specialized intellectual technique, appears to be a very rare one. (Keynes, 1938)

I associate the CVAR approach to macroeconometrics with Europe because its use is more prevalent in Europe than in the U.S.[4] But, even in Europe, the CVAR approach is not necessarily winning in the competition with the DSGE models. Instead, it is becoming increasingly accepted by macroeconomists, in particular in Central Banks, that the appropriate approach to empirical macro policy analysis is the "theory-first" DSGE model approach. In particular in US, but also widely in Europe, the DSGE theory first approach is in fact becoming the only allowable way to do macroeconometrics. Chari et al. (2009) summarize this generally accepted methodological view when they write "an aphorism among macroeconomists today is that if you have a coherent story to propose, you can do it in a suitably elaborate DSGE model." Even Michael Woodford's more balanced consideration of the state of macroeconomics (Woodford, 2009) does not cite any of the European work as belonging in the new synthesis in macro. For the majority of top U.S. macroeconomists, it is as if the European method does not exist.

3. Some Hypotheses About why the European CVAR Approach is not Winning Out

Economists unfamiliar with the European CVAR approach to macroeconometrics will likely assume that the reason why this approach is losing out and is not mentioned or discussed in papers on modern macroeconomics is that it is not as good as the DSGE approach. The implicit assumption of most economists is that the cream rises to the top. Since the DSGE approach has risen to the top, it must be the cream. This view would follow from the following implicit assumption about the idea and method selection process in economics which many economists probably would find plausible: Ideas and methods compete, and while the competition is messy, the better ideas and methods (those more likely to represent the truth in an uncertain world) tend to win out in a sufficiently short time to make it reasonable to assume that prevailing ideas and methods are the best ideas and methods.[5] One could say there is what might be called an invisible hand of truth that guides the competition toward the truth. This paper challenges that assumption. It argues:

1. When one uses an economist's lens to analyze the selection mechanism of methods and ideas as it has developed in economics, there is a likely bias in this

[4] Thus, whereas in my interviews with U.S. graduate students, (Colander, 2007) almost none had heard of the CVAR and Hendry approaches, in my interview with European graduate students (Colander, 2009) many more were familiar with it. European and U.S. economics are, of course, intertwined, and there are advocates of both positions in both places. For example, Hoover is a Duke. However, he was trained in England, and as I will discuss below the approach to macroeconometrics that I am referring to is much more prevalent in Europe that in the U.S. Thus, I feel it is appropriate to call it the European approach to econometrics.

[5] Challenging that assumption has been an ongoing theme of my research starting with Colander (1991). Elsewhere (Colander, forthcoming-a) I have called this view the "representative researcher" view of the competition of ideas. In the representative researcher view of methodology the ideas that win out are those ideas that a representative researcher would choose.

selection mechanism, so one would not necessarily expect that the best ideas and methods would rise to the top.

2. Based on casual observation, that bias is likely to favor the DSGE theory-first approach over the European CVAR approach.

The specific aspect of the European CVAR approach that I see biasing the choice against it is its explicit reliance on researcher judgment as part of the analysis. My argument is that any method requiring judgment does not do well in the replicator dynamics of the current U.S. economics profession and increasingly in the European economics profession. By that I mean that, other things equal, research methods that explicitly emphasize the need for explicit judgment lead to fewer publication than do research methods that rely on firm rules and avoid discussions of judgment, or make them implicit in shared assumptions and conventions, such as the acceptance of a formal theory, or a statistical test for significance. The fewer publications reduce the probability of advancement for researchers using that methodology, and thus over time, tends to work against its use.

The bias against judgment is inherent in a blind peer review system. Such systems gravitate toward methodologies that incorporate conventions and implicit judgments that make researcher judgment less important in deciding whether the paper is publishable or not. My suspicion is that the DSGE approach became more prevalent in the U.S. compared to Europe because the U.S. has emphasized a blind journal article peer review system of advancement whereas, until recently Europe had a more eclectic review system that was less phobic about judgment.[6]

3.1. The Representative Researcher and the Invisible Hand of Truth

The essence of my first argument is that, given the existing academic institutions in economics, the dynamic “truth” force pushing for the best idea and method to win out is relatively weak in comparison to other specific institutional forces that have little to do with the truth of the idea or the usefulness of a method in arriving at the truth. Instead of institutional incentives directing researchers to choose the “best method”—the method that a representative researcher is assumed to chose—these institutional forces direct researchers toward “institutionally consistent” methods of analysis that offer the best advancement potential within the existing institutional structure. My point is that institutional consistent methods are not necessarily the methods that are most likely to lead to the truth. While the “appropriateness of the approach or idea” (its contribution toward seeing the truth) clearly plays a role in that process, many other forces do as well, which means that the intricacies of the institutional structure of the economics become central to the understanding of economists’ choice of ideas and methods.[7]

For example, in an institutional structure that requires a certain type of peer review for advancement, some research methods and ideas are more likely to be amenable to that peer-review process than others. My argument is that those

[6] These ideas are discussed in more depth in Rosser, Holt and Colander, (forthcoming) and Colander, 2009)
[7] That is why I have directed much of my work toward understanding those institutional structures.

"institutionally consistent" ideas and methods are likely to be favored by the profession and, therefore, to matter. over others that do not fit so well in the existing particular type of peer review system. That, I argue, may well be the case with the European CVAR method.

The institutional feedback on theory and method choice described above has not previously been considered by economic methodologists because they tend to think of the competition of ideas as occurring within a *representative researcher's* mind.[8] So, unless that representative researcher is ideological or stupid (which few, outside heterodox economists, believe is the case), the representative researcher can be assumed to choose the idea and method that best captures the truth. This leads economists to the implicit conclusion that the "best" methods and ideas win out.

My conclusion is different and follows from my alternative way of thinking about the economics profession.[9] Similarly as George Stigler (1960) in the introductory quotation suggested might be the case, I see economists as motivated by self-interest and incentives. Specifically, I see economics as a complex system in which many models and methods are competing. That competition takes place in a very specific institutional environment and over time the incentives in that environment feeds back on the choices researchers make about model and method. Thus, the current emphasis on economists accumulating quality-weighted journal article publications plays a major role in determining the models and methods that the profession adopts.

My hypothesis is that in the current academic economics institutional environment of 'publish or perish' (in the right journals0 there are very few incentives for top young economists to reflect on the overall economic research process, but there are strong incentives for them to focus on narrow technical issues. The reason is that there are few publishing outlets for broad reflective pieces that would count in the advancement and promotion criteria.[10] It follows that, other things equal, those researchers who think about such issues are much less likely to advance in the field of economics. [11]

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[8] In doing so, they fall subject to the same fallacy of composition that I believe DSGE modelers fall into; they attribute rationality to the system results that would only likely exist if the system were a single individual.

[9] As will be obvious to readers familiar with methodological work, the approach I use has connections to the work of Thomas Kuhn and Imre Lakatos. Since discussing it would involve a long discussion, and I have briefly discussed this work elsewhere (Landreth and Colander, 2001) I will not discuss it here, other than to say that my approach differs from their in that I am describing a complexity field of science (Colander, 200x) rather than a standard field of science. I ague that in such complexity fields of study, where less guidance comes from empirical work, much more focus has to be given to professional advancement incentives in the choice of assumptions and methodologies than they do in the standard natural sciences.

[10] This obviously differs by institution. For example, at a liberal arts colleges, such as where I am at, there is a stronger incentive to do such reflective work, and work that involves judgment, since a wider range of scholarly output is considered than is the case at most universities. But even at institutions that include a wider range of scholarly output in their advancement criteria, there are few outlets for such reflective research that would move a young economist up in the profession.

[11] Elsewhere (Colander, forthcoming-a) I have distinguished a "consumer's knowledge" of theory from a "producer's knowledge" of theory, arguing that to use a theory in policy one needs a "consumer's

Because there are few incentives within the profession to be reflective on the overall rationality of profession's methods and ideas, few economist are reflective. Most are concerned with narrower issues—issues that lead them to success within the existing institutional structure. Because few economists are focused on taking a broad reflective approach, the composite view of all researchers is a composite of views of economists who have few incentives to think deeply about the issues. Hence, there is no justification for assuming that the composite of economists' views will reflect the view of a reflective representative researcher. I will argue below that the "representative researcher" approach is not the way to think about how the profession arrives at its methods or ideas. If one accepts my complex system view of the profession, such a composite "representative researcher searching for the truth" view of the profession's views is incorrect.

4. Incentives and the Evolution of Macroeconomic Theory

Elsewhere, (Colander, 2006a) I have applied this view to the history of macroeconomic theory. In that work I have argued that the path that the Keynesian revolution followed can be best understood within this institutional incentive approach to the competition of ideas. I argued that Keynes had a vague vision of the macroeconomy as a complex system with multiple basins of attraction and complex dynamics. He sensed that such a system could get into trouble and could end up at an undesirable equilibrium. Unfortunately, the mathematics to deal formally with such issues was not fully developed at the time, and most economists were not even close to having the technical expertise needed to formally frame the issue in such a vision.

So while Keynes's initial idea was visionary, it was not an idea that could survive within the then existing institutional framework that advanced economists on the basis of their writings. This worked against highly mathematical economists of the time, such as Richard Strotz (Strotz et al. 1953) and Richard Goodwin (1947), who were developing that complexity vision formally. The problem was that most economists of the time felt uncomfortable dealing with the complex mathematics needed to formally deal with the complexity vision of the macroeconomy that Strotz and Goodwin were putting forward. Their work was beyond the level of many of the economists at the time. It also worked against heuristic economists, such as G.L.S. Shackle (1955) or Hyman Minsky (1986) whose work focused on developing an intuitive understanding of macroeconomics within that complexity vision. Their heuristic work reflected an educated "consumer's understanding" of the complexity approach, but did not offer a clear path forward to advance it. Neither of these approaches did well in the competition of ideas in the post Keynesian period.

In the representative researcher view, the failure of these approaches must have been because they were flawed, and not as good as the ideas or methods that won out. In my complex systems view of the economics profession, the explanation may have been (and I suspect it was) that these methods and ideas did not offer a research path for

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knowledge" but that students are only taught to be producers, and that there are outlets only for producers, not consumers.

students that would allow them to survive and advance in the then-existing replicator dynamics of the profession.

The problem for the students of the highly mathematical economists of the time was that such mathematical research was incomprehensible to most economists. Only few researchers were on the forefront of both mathematics and economics. While these highly mathematical economists were seen as brilliant, they were far ahead of their times, and their students did not do well in the replicator dynamics of the time. The reason was that most of the "peers" doing the reviews of the research did not have the mathematical sophistication to see the contributions of these students as adding significantly to our understanding.[12] Thus, these mathematical economists generated few highly successful students to carry on their complexity views, and the latter faded away. The problem for the students of the heuristic economists was somewhat different. Once their professors had pointed out that the economy is complex, nonergodic, and fundamentally subject to uncertainty, there was not much more to say. This meant that the students did not do well in the replicator dynamics of the profession because they simply repeated the insights of their professors.

The result of these failures was that, instead of becoming a complexity revolution, the "Keynesian revolution" was quickly translated into a rather mundane set of ideas that were more amenable to the peer review replicator dynamics of the time. Keynesian economics, which could have been the beginning of a complexity revolution in economics, evolved into neoKeynesian economics, which modeled the economy as a unique equilibrium, comparative static, multi-market equilibrium system, in which the only problem was institutional rigidities. The current mainstream of modern US macroeconomics argues as if neoKeynesian economic theory is the only alternative to the DSGE modeling approach. This is far from the case. In my view, the serious alternative to the DSGE model is the complex systems model of macroeconomics (Colander, et al, 2008) and the European macroeconometric approach is best seen as the empirical branch of that complex systems approach.

4.1. Macro-econometrics, Incentives, and the Complex Systems Approach

In other papers and books, (Colander, 2006) I have discussed in more depth my complex systems view of the evolution of macroeconomic theory. In this paper, my interest is in on a very small sub-issue of my larger story—the profession's choice of macroeconometric method and its lack of interest in the European approach to macroeconometrics. My argument here is not that the European approach to macroeconometrics is necessarily better than the theory-first DSGE approach. My goal is simply to argue that, when one considers the incentives within the profession that guide model and method choice, that there are strong reasons to believe that, given current incentives, the profession would choose the theory-first DSGE approach not because it is

[12] For a discussion of some of the problems of peer review in economics, see Shepherd (1995)

inherently better in some broader sense, but because it better fits the institutional incentive structure of academic economics.[13]

Aris Spanos (this issue) nicely discusses the issues in debate between the general to specific approach (which is a part of what I am calling the European CVAR approach, and the prevalence of theory approach, which I am calling the theory-first DSGE approach to macroeconometrics. He presents the debate as one in which there are reasonable arguments on both sides. In taking this moderate view, he stands in marked contrast to Robert Solow's condemnation of the DSGE modeling approach (Solow, 2008). Solow, who represents what might be called the recent Neoclassical NeoKeynesian tradition in macroeconomics, sees the DSGE approach as essentially a "rhetorical swindle" that the "macro community has perpetrated on itself, and its students" (Solow, 2007, 235). The CVAR approach, while not sharing Solow's support of the more traditional macroeconomic models, agrees with Solow in that assessment not because the DSGE model is logically incorrect, but because it does not pass the judgment test; it is simply beyond belief that with all the assumptions the DSGE model must make to arrive at a formal model, that that model sheds much light on the type of short run problems that the macro economy often experiences. It simply does not meet the "common sense" test, so unless there are other arguments for using it, it is not an approach to policy that anyone other than someone who has been taught that it is the only correct theory would use as the sole approach for thinking about macroeconomic policy.

Within the "representative researcher" view of the economics profession, Solow's comment has no foundation; he is arguing that the ideas and methods that have won out within the profession are not the best, and are highly flawed. Within my complex system view of the profession, Solow's remark may well make sense; he is arguing that the replicator dynamics in the profession have produced economists who may be good at succeeding within the current academic institutions, but that, in his judgment, those academic institutions are flawed because those institutions have allowed a method that makes little intuitive sense to become a required method for all macroeconomists.[14] The argument of this paper supports Solow's more uncompromising view of the theory-first DSGE approach. While there are certainly arguments for both sides, as Spanos argues, there is, in my view, far less support for the current theory-first DSGE approach than the DSGE modelers have assumed.

4.2. The Lack of Historical Foundations of the "Theory-first" Approach

One of the arguments that supporters of the DSGE modeling approach use, and that Spanos accepts, is that the theory-first approach is simply carrying on a tradition that has long existed in economics: Thus, historically there is some justification for such an approach. I will argue below that this is an incorrect assessment of the history of economics.

[13] As an example of the failure of the European approach, consider that when, in my recent study of graduate economic education in the US I asked graduate students at six top university programs about cointegrated vector auto regression, or the general the specific approach to macroeconometrics few students had heard about it.

[14] I discuss these issues further in Colander. (forthcoming-b.)

Specifically, I agree with those economists who took a strong interest in methodology, such as Nassau Senior (1836) J. N Keynes (1891) or Lionel Robbins (1932) would not agree with the theory-first DSGE methodology. While it is true that Senior, Keynes and Robbins downplayed empirical work, their arguments in support of theory and against empirical work have to be understood in context. At the time Keynes and Robbins were writing, empirical work was rudimentary; the lack of data, statistical tools, and computing power made it almost impossible to derive any sound knowledge from data analysis. Their downplaying of empirical work at the time implies nothing about their views about the role of data or empirical work today. For example, after discussing the problems with empirical work Robbins writes: "Fortunately there is reason to suppose that in the future the alliance between the economy theorist and the statistician will be even closer than it has been in the past." (Robbins, 1930, 21). Thus, all that one can surmise from the lack of support of empirical work of earlier economists is that *given the empirical techniques of the time*, they felt that they could not rely on empirical work to answer questions. So, the *historical connection argument* that economists have taken a "prevalence of theory" over an empirically based approach cannot be seen as providing historical support for the current "prevalence of theory" approach. Methodology is, and should be, dependent on technology; when technology changes, methods should change as well.

4.3. What Earlier Economists Meant by "Theory" was not what DSGE Advocates Mean by "Theory"

A second reason why methodological practices of earlier economists cannot be used as historical justification for the current "DSGE theory-first" approach over the European approach is that for earlier economists "theory" meant something quite different than does "theory" for modern macroeconomists. Specifically, earlier economists distinguished between economic science and political economy.[15] For them, theory in economic science meant something different than theory in political economy.

Nassau Senior, who focused his work on identifying and organizing basic principles in a scientific framework (Schumpeter calls him the first "pure theorist" in economics.) wrote the following:

> (The economist's) premises consist of a very few general propositions, the result of observation, or consciousness, and scarcely requiring proof, or even formal statement, which almost every man, as soon as he hears them, admits as familiar to his thoughts, or at least as included in his previous knowledge: and his inferences are nearly as general, and, if he has reasoned correctly, as certain, as his premises.
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> But his conclusions, whatever be their generality and their truth, do not authorize him in adding a single syllable of advice. That privilege belongs to the writer or statesman who has considered all the causes which may promote or

[15] The discussion here is a summary about which I have written about at length. Since much of modern economics' approach relates to Robbins, who in turn based his approach on the Classical methodological approach, I will concentrate on his approach.

> impede the general welfare of those whom he addresses, not to the theorist who has considered only one, though among the most important of those causes. The business of a Political Economist is neither to recommend nor to dissuade, but to state general principles, which it is fatal to neglect, but neither advisable, nor perhaps practicable, to use as the sole, or even the principle, guides in the actual conduct of affairs. (Nassau Senior, 1836, pg 2-3)

For Senior, economic science was a branch of logic. In the science of economics one did theory, which meant drawing theorems from almost self-evident principles. Economic theory was not meant to directly guide policy, which he saw a much more complicated. To move from the theorems of the science of economics to policy required common sense judgment and institutional which economic theorists did not necessarily possess. The method was further developed by J.N. Keynes (1890) in his famous summary of economist's methodology. Like Senior, J.N. Keynes saw the science of economics as a relatively narrow branch of economics. In this science of economics, theory meant something very similar to the DSGE modelers have in mind. Their scientific theory was a highly formal set of propositions that consisted of primarily deductive reasoning based on first principles. It consisted of this because, given the empirical tools of the time, deductive reasoning was the only branch of economics that could potentially rise to the level of scientific knowledge.[16] *For Keynes and for many Classical and early neoclassical economists, however, that scientific theory had little relevance to policy analysis;* it was only one tool among many to be used by a political economist. Lionel Robbins was quite clear about this. In his review of Hawtrey (Robbins, 1927), a review that included many of the ideas that would later become embodied in his famous 1932 essay, Robbins stated clearly what he thought about using scientific theory to derive precise policy conclusions. He writes:

> What precision economists can claim at this stage is largely a sham precision. In the present state of knowledge, the man who can claim for economic science much exactitude is a quack. (Robbins, 1927, 176)

For both Keynes and Robbins, policy discussions did not belong in the science of economics; they belonged in political economy or in what Keynes called the art of economics. Theory in political economy was a much broader theory than the formal theory of science. It consisted of an understanding of the formal scientific theory, but also an understanding of the limitations of that theory, accepted value judgments of society, as well as knowledge of the institutions of the times. *Political economy theory was a common sense theory that captured the educated common sense of economists of the time.* It was a theory that involved, and had to involve, value judgments.

Robbins made the need to separate the science of economic from political economy clear in his Ely Lecture. He writes:

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[16] Alfred Marshall (1890) downplayed the distinction between political economy and economic science, and started using the term science in a broader sense, but he also argued strongly *against* any use of formal deductive models as part of the analysis. For Marshall, all of economics was what earlier classical economists had called political economy. (See Colander, forthcoming-b)

> My suggestion here, as in the Introduction to my *Political Economy: Past and Present,* is that its (political economy) use should be revived as now covering that part of our sphere of interest which essentially involves judgments of value. Political Economy, thus conceived, is quite unashamedly concerned with the assumptions of policy and the results flowing from them. I may say that this is not (*repeat not*) a recent habit of mine. In the Preface to my *Economic Planning and International Order,* published in 1937, I describe it as "essentially an essay in what may be called Political economy as distinct from Economics in the stricter sense of the word. It depends upon the technical apparatus of analytical Economics; but it applies this apparatus to the examination of schemes for the realization of aims whose formulation lies outside Economics; and it does not abstain from appeal to the probabilities of political practice when such an appeal has seemed relevant. (Robbins, 1981, 8)

For Robbins, the theory of economic science was simply the "technical apparatus" of the theory of political economy. But that theory of political economy went far beyond that technical apparatus, and included a much wider range of argumentation and understanding.

This history sheds a quite different interpretation to the historical antecedents to the theory-first DSGE approach. It is not similar to the approach that Classical economists used. When Classical economists stated that policy was based on theory, they did not mean it was based on a single scientific theory (that was simply a "technical apparatus") as is done by DSGE advocates. Instead, policy was based on a broader sense of theory that included judgments of relevance of the technical apparatus to the problem at hand. What Robbins never would have done is to directly draw policy conclusions from a theoretical model without considering the appropriateness of the theory to the problem at hand. Yet this is precisely what the "theory-first" DSGE model advocates seem to claim: if we do not ground our models in formal theory, we will know nothing.[17] The problem is that when we do ground our policy thinking in formal theory that is not relevant to the problems at hard, we can end up thinking we know something that we don't, which in many ways is worse than knowing that we do not know something.

When one combines these two historical insights about earlier economist's method, arguments, it is clear that rather than being a continuation of economist's method, the "theory-first" DSGE model approach is a significant deviation from earlier economist's method. In fact, I would argue that the European approach to macroeconometrics is much closer to the spirit of the classical approach to policy analysis. No doubt, the European approach differs from the earlier approach in that it gives more focus to empirical data. But that can be explained by the change in empirical technology. Today, much more in the way of data is available; much more in the way of statistical tools are available and much more in the way of computing power is available. These advances have opened up a new way to doing macroeconomic theory and of

[17] The problem with that reasoning was pointed out by Kevin Hoover. He writes: "There is a fundamental problem: How do we come to our a priori knowledge? Most macroeconomists expect empirical evidence to be relevant to our understanding of the world. But if that evidence only can be viewed through totalizing a priori theory, then it cannot be used to revise the theory." (Hoover, 2006)

developing policy-useful macroeconomic theory. This means that today, it may be possible to discover patterns in the data in ways that are fundamentally different than existed in Keynes' and Robbins' time, and only a Luddite would not want to take advantage of those.

5. Characterizing the Debate in Macroeconometrics

With that historical background, let me reconsider the debate between the DSGE theory-first approach and the European approach to macroeconometrics. As my discussion of the history of economics makes clear, the approach being used by the majority of macroeconomists should be called the "preeminence of the DSGE theory" approach, not the "preeminence of theory" approach that characterized Classical economics. The modern DSGE methodology is not an approach that elevates *theory* above empirical work, but instead is an approach that elevates one particular way of using theory -the DSGE modeling approach- above all other ways. The theory-first DSGE approach is best seen as a highly limiting way of doing macroeconometrics, and macroeconomic policy. It is an approach that Senior, Keynes and Robbins would have strongly opposed.

To see the misuse of theory in policy analysis that can occur by users of the theory-first DSGE approach, consider VV Chari and Patrick Kehoe's (2006) discussion of policy relevance of the DSGE model. They write:

> The message of examples like these is that discretionary policy making has only costs and no benefits, so that if government policymakers can be made to commit to a policy rule, society should make them do so. (pp. 7-8)

and:

> Macroeconomists can now tell policymakers that to achieve optimal results, they should design institutions that minimize the time inconsistency problem by promoting a commitment to policy rules. However, to what particular policies should policymakers commit themselves? For many macroeconomists considering this question, quantitative general equilibrium models have become the workhorse model, and they turn out to offer surprisingly sharp answers. (p. 9)

For Robbins, such statements are ones only a quack would make.

5.1 The Role of Theory in the European CVAR Approach

As I understand it, the European CVAR approach to macroeconomics is not anti-theoretical in the broad political economy sense. It is a blend of broad theory disciplined by careful data analysis. The idea is to uncover empirical regularities in the data that can be given a broad interpretation given the underlying theory models. That's why Hoover calls it an archeological approach: carefully excavated results are used to guide theorists as to what theories to use. This, of course, does not exclude the possibility that the empirical results might be masking the true relationships and that one's intuition tells the theorist to disregard the highly imperfect data. Therefore, before making judgment about policy, the European approach requires the economist to carefully consider the relationship between the best available theory and the best available data. *The key to the European approach to macroeconometrics is bringing the data to the theory, and bringing the theory to the data.* To do that is an art that requires researcher judgment, so researcher judgment is integral to the European method. In some ways it is a "wisdom of crowds of specialists" approach, where specialists compare analyses and interpretations, argue about differences in interpretation, and come to a conclusion.

Let me reiterate. The European CVAR approach does not put data ahead of political economy theory; it simply uses data in sorting through the many alternatives that a broad political economy theory may lead to. Thus, the European CVAR approach is totally compatible with what could be called a "prevalence of theory" approach in the political economy context. If one cannot gain any reliable information from the data, then one would have to rely on broad political economy theory combined with a good understanding of institutions. This would be in the tradition of Henry Thornton or Walter Bagehot—a tradition carried on by modern economists such as Charles Goodhart and Perry Mehrling. Their work is theory-first in the European tradition.

Using the European CVAR approach, one takes an agnostic approach to the value of the data and theory analysis. The empirical model analysis may be highly informative, in which case it would be used to provide guidance to policy, or it may be of limited value, in which case one accepts that one has to rely on one's intuition and knowledge of institutions to guide policy. If that's the best we can do, so be it. However, it seems plausible that an empirical methodology that allows the data to speak as freely as possibly about underlying empirical mechanisms is more likely to be able to discriminate between these two cases than a methodology that forces one particular view on the data. Because of limitations of our data and our theories, economic policy will always be based on judgment to some extent. To pretend we know the theory is not sufficient for claiming a "scientific" foundation of our policy.

5.2. Why the CVAR Approach Might Seem Anti-DSGE

The European approach to macroeconometrics is not inherently anti-DSGE theory. However, it may seem to be anti-DSGE theory for two reasons. The first is that the DSGE model, contrary to the European CVAR, does not specifically allow for intuitive judgment to be part of the analysis. It requires researchers to use a model of the macro economy that, in its current state of development, does not include a significant number of heterogeneous agents, the possibility of complex dynamics, multiple equilibria and structural breaks unflinchingly in analyzing the macroeconomy. For most non macroeconomic specialists, it strains credibility that no intuitive judgment is needed to make the DSGE model applicable. But the DSGE theory first approach does not allow such judgment. Somehow, in spite of the large amount of uncertainty that will naturally be associated with such a model, it is supposed to shed significant light on a macroeconomy that includes all those omitted elements and guide us as to how to set fiscal and monetary policy. Were that the case, it would truly be a miracle.

The above argument does not deny that useful theories may well be counterintuitive, nor that the implausibility of the DSGE model alone is not sufficient reason to abandon it. If it could be shown that the DSGE model fits that data better than alternatives, that intuitive implausibility of the DSGE model could be overridden by the empirical results. This leads to the second reason why the European approach seems anti-DSGE. When European researchers put the DSGE model to careful empirical tests, they have found that the DSGE model does not meet these data criteria either.

An example of its failure can be seen by considering a recent paper by Peter Ireland (2004) that purported to take the DSGE model to the data. In his study, Ireland started with the assumption that a simple real business cycle model can explain the US experience in the post Second World War period. He made his theoretical model more 'flexible' by imbedding it in a DSGE model framework in which total factor productivity was assumed to be a stochastic near unit root trend driving the other variables. The paper was impressive, and was high-level cutting edge work to almost all economists who do not specialize in time series econometrics, such as myself, and the large majority of economists, including many DSGE macro theorists. It was published in a good journal.

To test the difference between the European CVAR approach and the theory-first DSGE model approach to macroeconometrics, I asked Johansen and Juselius to consider Ireland's paper for a conference I was organizing. Specifically, I asked them to highlight the difference between the two approaches. I had expected the normal nuanced differences, but that is not what I got; what I got was a blistering critique of the Ireland paper. These can be found in Johansen (2006) and Juselius and Franchi (2007).[18]

For European macroeconometrics advocates, Ireland's paper has two serious problems. The first is that it fails to meet some minimum statistical assumptions. As discussed by Spanos, its failure to meet these is not in debate between DSGE modelers or European macroeconomists modelers. The problem is that Ireland made assumptions about empirical relationships in the data that, if one were not fully committed to the view that the theory is right independent of the data, should have been tested, and if he had tested them, the assumptions would have been seen to be false. But he did not test them.[19] If he was committed to the view that the theory was right independent of the data, then why even bother bringing the model to the data. It would seem more reasonable for him to just state that the model is right, and skip bringing it to the data. That may be the correct way; the information to be gleaned from the data is highly questionable; my point is simply that if you are going to bring a model to the data, then it should be done in a meaningful way.

Juselius and Franchi carried the analysis of Ireland's paper further; they show that when the correct specification tests were done in the Ireland model, essentially all of

[18] While Ireland's work is chosen as an example, it should be seen as representative, and other papers could have been chosen to represent the US theory comes first approach.

[19] The failure of Ireland's paper to meet the statistical assumptions should have meant that the paper never should have made it through the peer review process, and the fact that is did should raise serious concerns about that peer review process. When, at my suggestion, in private correspondence, Johansen raised these issues with Ireland, Ireland seemed unconcerned about them.

Ireland's results are rejected! Moreover, when the model is reformulated based on the European approach, the conclusions are reversed! Despite Juselius and Franchi's negative findings, the DSGE model is not necessarily wrong, and some other theory right. All their findings mean is that Ireland's paper, which seemed to be providing empirical support for the DSGE model, did not provide that support. If one believes that the DSGE model is the model economists should use. The justification must lie in one's intuition that the DSGE model is the correct model, not because the DSGE model fits the data.

The question macroeconomic empirical researchers have to ask is whether Ireland's paper is an anomaly, or whether it is an example of the disregard for the data that the "DSGE model first" approach encourages. I am not enough of an econometrician to make a conclusive judgment on this issue, but the sense that I get from my interviews with economists, and from my studies of U.S. graduate economic education (Colander, 2007) is that Ireland's cavalier approach to empirically testing the model is representative of the more general "DSGE model first" macro approach to data analysis that most U.S. graduate students are taught, and that they consequently practice.

6. The Bias Against Methods based on Intuition and Judgment in the Economics Profession

Let me now combine the two arguments of the paper—the bias in the replicator dynamics of the economic academic institutions, and the lack of success of the European CVAR approach. My claim is that it is likely that the success of the DSGE model approach as compared to the European approach is in large part due to a bias in the replicator dynamics of the profession against methods such as the CVAR approach the explicitly requires researcher judgment. The problem is that the European approach requires macroeconomists to explicitly base their arguments on intuition and judgment, both about the data, the institutions and the theory. Such judgments are difficult to assess, and almost impossible to access in blind peer review journals. Who does the analysis matters. This means that papers using the European CVAR approach do not have a ready outlet in journals and thus the method does not do well in the replicator dynamics of the profession.

The bias in the current replicator dynamics of the economics profession against analysis which emphasizes the need for explicit judgment is in my view a key explanation for the success of the theory-first DSGE-model approach and the lack of success of the European approach. The DSGE theory-first approach allows one to proceed as if one needs no intuition and judgment. It revels in the counter intuitiveness of the theory, seeing counter intuitiveness as strength rather than a weakness, and thus allows all sorts of models that do not pass a minimum intuitive smell test. And then it does not require researchers to bring the model to the data in a reasonable way.

I suspect that Ireland did not test whether the basic underlying assumptions in his model were true because the publishing incentive system he faced, and his commitment to "theory comes first" macroeconomics, did not guide him to do so. Instead, it guided him to get a published paper. He was successful; the paper was published and widely cited because it used high-level econometric techniques, and because it brought a "DSGE model to the data." In the current academic economics incentive structure, publishing has almost become an end in itself, and there is little cost associated with a mistake or taking a less than careful approach.[20]

It is that same focus on publishing that biases the economics profession against the European method of econometrics. The problem is that the European approach does not offer an unambiguous alternative model to replace the "theory-first DSGE model" with. It is a method, not a model. Thus, it requires one to be a specialist in both statistics, in the history of institutions and in macroeconomic theory. It does not allow a separation between the three. Moreover, to choose among alternative theories, a researcher using the European approach must make numerous substantive judgments about the appropriateness of the assumptions. Those substantive judgments must be made on the basis on intuition, one's understanding of theory, and one's understanding of institutions.[21]

The importance of judgment in the European approach can be seen in the following comment from Søren Johansen (Johansen and Juselius, forthcoming). In it he stated:

> So there is now something called the Johansen Procedure, and it is completely misleading to believe it can as such be applied to data that are fractionally integrated or heteroscedastic, or whatever. The Johansen procedure consists of checking the assumptions and then once you know the model is reasonably OK, you go and apply it. It is not just pressing the J button – that is certainly completely inappropriate - but this is unfortunately how it has often been used. It may look like you are doing sophisticated econometric work, but what you are doing is probably close to worthless. My contribution to cointegration analysis was simply to analyze the maximum likelihood estimator and the likelihood ratio test in the Gaussian model. But before you use maximum likelihood, you have to be sure that you have the right model, otherwise the estimator and test do not have the optimal properties you think they have.
>
> Most econometrics is still taught as methods - almost like cookbooks where you have receipts for method 1, method 2, and method 3. That's not the way Katarina [Juselius] and I approach the data. We first choose the method that fits the circumstances. It needs a lot more careful thinking than is usually

[20] Now I am certainly not claiming that all US macroeconometrics involves sloppiness, or that it is only macroeconometrics that involves sloppiness. What I am suggesting is that the incentive system in academic economics encourages researchers to hide judgment. This sloppiness has been pointed out by a number of researchers, Including, Edward Leamer (1983), Lawrence Summers' (1991), Deirdre McCloskey (McCloskey and Ziliak, 1996), and (Dewald et al. 1986), Peter Swan (2006) among others. The assessment that was held by many economists was that the informational content of many aspects of empirical research in macro was close to zero. (Cooley and Leroy 1981). Despite the concerns expressed about the informational content of the econometric studies, thousands of such studies were published in the US.

[21] It is these substantive judgments that Classical economists saw as part of the "theory" when talking about political economy. Political economy theory was the technical apparatus of scientific theory modified by educated common sense and institutional knowledge.

> associated with writing an applied paper in econometrics. Of course, this has nothing to do with cointegration, but it has everything to do with carefully applying statistical methods to data. With modern computers, it is getting easier to do, but it is also getting easier to do wrongly.

The DSGE model allows separation of theory from empirical work. That reduces the judgment researchers must make: Accept that the DSGE model is the right one, and get on with one's work. That allows individuals to specialize—some can specialize in theory—even though intuitively one might have a hard time justifying that further work on the theoretical model is helping us understand the problems we face in the economy. It doesn't require one to be simultaneously a theorist, a macro econometrician, and a institutionally knowledgeable practitioner to publish. Judgments are still there, but they are implicit; they are hidden in the consensus about method and model, which makes them undebatable, even though they should be at the center of the debate.

The correct use of the European CVAR approach is much more demanding than just pushing the J-button. It requires a researcher to be a simultaneous expert in theory, macroeconometrics and institutions, and to use that judgment in coming to a conclusion. It eschews cookbook methods. This makes it difficult to publish in the economic professional environment that guides researchers to use cookbook methods that can have come to be accepted, and can be blindly refereed. The true Johansen method requires researcher judgment and thus is not easily amenable to advancement systems that are highly dependent on blind referring processes. That is my judgment of why it is not the generally accepted method, and why it will have a difficult time becoming the generally accepted method unless the institutions change.

7. Conclusion

Let me conclude by summarizing my answer the question I posed at the beginning: Why has the European approach to macroeconometrics had only limited success in the competition for ideas? My answer is that a likely reason is that it is not as compatible with the replicator dynamics of the academic economics profession as is the DSGE-model first approach.

I certainly am not claiming that I have proven my argument. The arguments in this paper are laced with judgments and intuition based on informal, not formal, evidence. Ultimately, such judgments play an important role on all economists' arguments. My hope with this paper is not to prove anything, but rather to stimulate discussion and debate among those who have a deeper understanding of the various approaches than I do. Ideally that debate would lead each side to spell out their judgments and intuitions. In my view, such a debate would add much more to our understanding of the macro economy and do more to further macroeconomic thought than would another 100 papers extending the DSGE model or 100 papers applying the cointegrated VAR model to a data set.

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