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Special Issue on Using Econometrics for Assessing Economic Models—An Introduction

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Abstract

Two methodological approaches to empirical economics which are labelled 'theory first' versus 'reality first' are introduced building the background for the discussion of the individual contributions to this special issue.

Special issue "Using Econometrics for Assessing Economic Models"

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1 Motivational Background

The purpose of this special issue was formulated as:

Econometrics is often used passively to provide the economist with some parameter estimates in a model which from the outset is assumed to be empirically relevant. In this sense, econometrics is used to illustrate what we believe is true rather than to find out whether our chosen model needs to be modified or changed altogether.

The econometric analyses of this special issue should take its departure from the latter more critical approach. We would like to encourage submissions of papers addressing questions like whether a specific economic model is empirically relevant in general or, more specifically, in a more specific context, such as in open, closed, deregulated, underdeveloped, mature economies, etc. For example, are models which were useful in the seventies still relevant in the more globalized world of today? If not, can we use the econometric analysis to find out why this is the case and to suggest modifications of the theory model?

We encourage papers that make a significant contribution to the discussion of macroeconomics and reality, for example, by assessing the empirical relevance of influential papers, or the robustness of policy conclusions to econometric misspecification and the ceteris paribus clause, or by comparing different expectations's schemes, such as the relevance of forward versus backward expectations and of model consistent rational expectations versus imperfect/incomplete knowledge expectations, etc.

One of the great advantages of this journal is that it encourages open discussions and critical debates. In my view, this is something that has been lacking in economics over more recent periods, in which we have seen a streamlining of academic research into the "representative agent with optimizing behavior based on rational expectation" type of Dynamic Stochastic General Equilibrium (DSGE) models. Interpreted positively, this could suggest that economics as a science has finally converged to a state of profound understanding. This, of course, would be a blessing for the billions of people whose welfare depends on an empirically relevant economic policy. Unfortunately, data describing our economic reality tell a very different story. As the present financial and economic crises has amply exemplified, the need to question and debate present dogmas in Economics seems greater than ever (Colander et al. 2009; Katlesky 2009; Lejonhufvud 2009).

With this background, it is a great pleasure to introduce the articles of this special issue, demonstrating scientific curiosity, a critical and constructive view on how to do empirical econometrics, and a willingness to address difficult and relevant problems using adequate econometrics. Many of the papers touch methodological themes that have played a prominent role in my own research: to use a strict econometric methodology based on sound statistical principles; to assess the empirical relevance of influential theory models; to learn from data how to best modify or change economic theory when needed.

As the guest editor of this issue, I take the opportunity to discuss the individual contributions within the context of the following three methodologically motivated themes: (1) principles for how to bridge economic theory and empirical evidence, (2) how to actively use econometrics to improve economics, and (3) incentives, diversity and debate in economics. The idea is to give the reader an overview of an important

ongoing methodological discussion and at the same time show how each of the individual contributions add to this discussion. The ultimate aim is, of course, to inspire the readers of this journal to engage in this important debate.

In Section 2, I first briefly introduce two fundamentally different methodological approaches to empirical economics, which, somewhat loosely, could be called 'theory first' versus 'reality first' using Aris Spanos' paper on 'The pre-eminence of theory' as a motivating article. In Section 3, I discuss the difficult task of linking theory and evidence in a nonstationary world with illustrations from many of the articles in this issue. In Section 4, I discuss institutional incentives, diversity and debate inspired by David Colander's article. Section 5 concludes with a discussion of how to use empirical econometrics as an progressive research paradigm and illustrate with the contributions of this special issue.

2 'Theory First' versus 'Reality First'

The 'theory first' and 'reality first' approaches are in many ways fundamentally different: the former starts from a mathematical (static) formulation of a theoretical model and then *expands* the model by adding stochastic components, the latter from an explicit stochastic formulation of *all* data and then *reduces* the general statistical (dynamic) model by imposing testable restrictions on the parameters. These are also called 'the specific-to-general' and the 'general-to-specific' approaches to economic modelling. For a detailed methodological discussion of the two approaches, see for example Gilbert (1986), Hendry (1995, 2009), Juselius (2006) and Pagan (1987) and Spanos (1986, 1995, 2006) and his article in this issue (Spanos 2009).

Spanos (2009) discusses the historical preference for 'the pre-eminence of economic theory' which is based on the idea that all basic features of the economic model can (and should) be pre-specified before taking the model to the data. In this case econometrics plays the subordinate role of 'quantifying' theoretically meaningful parameters assumed to be empirically relevant and statistically meaningful (to provide a basis for inference) on a priori grounds. Spanos argues that such an approach can be defended, but only if the probabilistic assumptions comprising the underlying statistical model (implicit or explicit are satisfied vis-à-vis the data in question, and that we need a better understanding of the role of data in modern statistical inference to establish a constructive dialogue between the 'theory first' and 'reality first' approach. He elaborates this further in what he calls 'an all-encompassing methodological framework', where he draws a clear distinction between *substantive* and *statistical* assumptions. He argues that this is the key to the 'testing of assumptions' argument as their respective validity has different implications for inference. The substantive assumptions pertain to the centuries old issue of the realism of economic theories, whereas the statistical assumptions pertain to the reliability of the statistical inference.

Colander (2009b) also discusses the role of theory in understanding the economic reality. He argues that there is an important distinction between theory as the technical apparatus (i.e. the axiomatic, highly mathematical analysis of economic problems) and theory as the political economy (i.e. 'the art of economics' in conducting economic policy). He also argues that Classical economists made this

distinction and that it has, to a significant extent, been wiped out in today's academic approach to economics. He suggests that this is partly the case because the present university incentive system is based on quality weighted journal articles. The 'art of economics' requires judgment which is difficult to assess objectively. Since objectivity in the refereeing process is considered desirable, articles based on judgment do not fare well in top economics blind peer reviewed journals, which may explain why the 'technical apparatus' has taken over as the preferred way of doing economics.

Spanos (2009) provides a slightly different take on the issue. He argues that the element of judgment can be minimized if the role of the data—as it relates to statistical inference—in empirical analysis is properly understood. This is because the reliability of any statistically-based inference necessitates the validity (vis-à-vis the data in question) of the underlying probabilistic assumptions. This is not a matter of choice, i.e. select different metrics 'to assess the usefulness of their theories'. If statistical inference is used to pose and answer substantive questions of interest, such statistical model validation is a must. In contrast, if one were to opt for the 'crystal ball' method instead, no such validation will be necessary.

Both views are interesting and inspire debate. Is it the case that proper econometric analysis can replace the element of judgement in 'the art of economics', or is it the case that sophisticated econometrics have diffused the necessary judgement? Historically, it is easy to see why the art of economics was associated with strong elements of judgment. To take economic models to the data allowing for complex features such as interactions, dynamics, heterogeneity, etc. would have been unthinkable without today's access to high speed computers. Now that we have the tools to compute even very complex models, we need to discuss their impact on how we learn from empirical evidence and the role of judgement in this process.

Spanos gives some reasons why a 'general-to-specific' approach to economic modelling seems so foreign to most economists. It is caused by the way economists approach the data, which is strongly influenced by the legacy of the pre-eminence of theory approach, which clearly differs from the way statisticians approach modelling. Economists formulate a theoretically well-specified model and apply statistical methods to 'quantify' its parameters, whereas a statistician formulates a statistically well-specified model for the data and analyzes the statistical model to answer the economic questions of interest. In the 'theory first' case, statistics is often (ab)used as a tool to procure certain theoretically meaningful estimates (irrespective of their statistical meaningfulness), and in the second case, the statistical model is taken seriously and used actively as a means of learning from data about the underlying phenomenon of interest.

In the subsequent discussions I for simplicity use the notion of a VectorAutoRegressive (VAR) model or a Cointegrated VAR (CVAR) to represent 'general-to-specific', 'reality first' approaches, such as Equilibrium Correction Models (ECM), Vector ECM, Panel ECM, etc. Similarly, I use the notion of a DSGE model to represent 'specific-to-general', 'theory-first' approaches such as Rational Expectations, Representative Agents, etc. models.

2.1 Is DSGE Modelling the Answer?

Colander discusses the wide-spread use of DSGE modelling in academic curricula, top economics journals, and central banks as an illustration of the dominant role of 'theory-first' (the technical apparatus) in today's political economy and argues that the DSGE model is not able to satisfactorily address the complexity of today's empirical reality. If this is correct, and many of us are convinced it is (see for example Colander 2006; Colander et al. 2008, 2009; Katlesky 2009; Lejonhufvud 2009). It raises the question: Why did they become so popular and why are they still being used by the profession?

In an attempt to answer this question I first refer to an article by Lawrence Summers in 1991, 'The Scientific Illusion in Empirical Economics', where he discussed the role of applied econometric work (Summers 1991). In that article he argues that empirical economics has exerted little influence on the development of economic theory and provided little new insight on economic mechanisms. As an illustration, he mentioned two different approaches to applied econometric modelling; (i) the representative agent's approach, where the final aim of the empirical analysis is to estimate a few deep parameters characterizing preferences and technology, and (ii) the use of sophisticated statistical techniques exemplified by a VAR model à la Sims, to 'identify' certain parameters on which inference about the underlying economic mechanisms is based. In both cases he argues that these highly simplified empirical models have not been able to discriminate between theories aiming at explaining an infinitely more rich and complicated macroeconomic reality.

Almost twenty years have passed since Summer's critique and, even though the profession did not respond openly to the critique when it appeared, much of the more recent DSGE type of modelling can be seen as a response. Consider, for example, the way Ireland (2004) takes his real business cycle model to the data: he starts with a representative agent's model, allows total factor productivity and capital to be a highly persistent process, adds a VAR process (à la Sims) to the observed data. In the process he has made the model, with its deep structural parameters, more flexible by augmenting it with a VAR process in which the near unit root nonstationarity of the data is accounted for by adding an exogenously given stochastic trend.

It seems, therefore, natural to ask if DSGE modelling can be a resolution to Summer's critique. I argue that the DSGE models still have a long way to go before they are able to properly account for the complexity of the economic reality. The fact that they have now been made more flexible is only an added-on feature needed to account for basic dynamic properties of the data. But the main problem remains: data are not allowed to speak freely about the economic mechanisms. The DSGE/VAR approach does not answer questions such as whether the parameters have changed over the sample period, whether a different theory model is empirically more relevant, or whether there are new features in the data for which theory is not yet formulated.

2.2 Learning from Data?

Many economists would argue that unless the empirical model is constrained by theory from the outset, one would not be able to make sense of the results: With-

out the mathematical logic of the theory model, one opens up for possibilities of quackery. The question is whether this is a relevant claim[1]: Is the VAR approach immune to this critique? Not necessarily. The way VAR models are discussed in the literature gives the impression of them having been applied mechanically (pressing the VAR button) rather than used to ask sharp and precise questions about the economy. This might be due to a lack of understanding of what a likelihood based VAR analysis is. To claim that the statistical analysis is based on full information maximum likelihood, requires that the model satisfactorily describes all aspects of the data. For this to be the case the researcher must carefully check for possible shifts in mean growth rates or in equilibrium means; for the effects of interventions, reforms, and changing policy. He or she must also decide whether the sample period is defining a constant parameter regime; whether the information set is correctly chosen, and many other similar decisions. The accuracy of the results depends on all these assumptions being correct in the model. To make the necessary analysis to develop a satisfactory model is a time consuming and tedious process, that depends upon the researcher's judgement and expertise. It has nothing to do with pressing the VAR button. But without such checking, the results can be (and often are) close to useless and if they are taken seriously by policy makers, even worse than worthless.

It is, therefore, important to emphasize that a statistically adequate VAR analysis has to obey equally strict scientific rules as an analysis of a mathematical model in economics[2]. In principle there is no arbitrariness in such empirical analyses as Spanos points out in his article[3]. However, such objectivity can only be achieved if the data are not constrained from the outset in a theoretically prespecified direction. Otherwise it would be impossible to know which results are due to the assumptions made and which are empirical facts. The only way the methodology works properly is by allowing the data to speak as freely as possible about empirical regularities[4]. As a consequence the interpretation of the results has to be at the background of not just one but several (possibly competing) economic hypotheses.

In its unrestricted form, an adequately specified VAR model is just a convenient reformulation of the covariances of the data (Hendry and Mizon 1993; Juselius 2006, Chapter 3) and, therefore, not very informative as such. Its usefulness is in providing a framework within which one can reduce the number of parameters until further restrictions change the value of the likelihood function and, hence, the information in the data. The final VAR model, if successfully applied, should structure the information in the data as parsimoniously as possible using an economically meaningful parameterization. One could say that it provides a set of sophisticated empirical facts for a theoretical model to explain, in order to claim empirical relevance. In this sense, the VAR model can be seen as providing (broadly defined) confidence bands within which an empirically relevant theory model should fall. Correctly done it is

[1]Lejonhufvud (2009) argues against this claim citing Niels Bohr for saying "But you are not thinking. You are just being logical."

[2]In this case, the rules are given by mathematical statistics rather than mathematical economics.

[3]Of course, there always exist borderline cases when a different choice of p-value may lead to a different choice, but such choices are replicable and can be assessed.

[4]This is, of course, *not* the same as letting the data speak by themselves, which generally would not produce useful results.

a powerful tool that allows the researcher to engage in a logical discussion with the empirical reality and by doing so guides his/her research into empirically relevant directions. But, as with all research, originality and good judgement are likely to help generating genuinely interesting results.

2.3 Does the Approach Matter?

Unfortunately, the DSGE 'theory first' and the VAR 'reality first' approach often produce very different results even when applied to identical data and, hence, could easily lead to different policy advice. For example, Juselius and Franchi (2008), performed a detailed empirical VAR analysis of Ireland's real business cycle data without *assuming* from the outset to know all the necessary foundational information, such as the number of stochastic trends (one in Ireland's DSGE model, 2-3 in the data), where they come from (shocks to TFP and capital in Ireland, shocks to consumption and labor in the data), the lag order of the VAR (one in Ireland, two in the data), which the long-run relations are (trend-stationary steady state variables in Ireland, trend-stationary cointegration relations in the data), that the DSGE structure has been unchanged since the second world war (strong evidence of structural breaks in the data), and so on. Thus, the conclusion reached in Ireland's paper (that the real business cycle theory model is able to explain the long business cycles in the US post war period) was not based on correct statistical inference. The fact that the empirical analysis of Ireland's model at the first glance seemed impressive, despite its strong empirical rejection when the data were allowed to speak freely, is a warning against drawing conclusions from models based on many untested assumptions. Strong economic priors imposed on the data without testing may say more about the faith of the researcher than the state of the economic reality.

Because, the 'theory first' approach is less likely to be open to signals in the data suggesting that the assumed theory is incorrect or needs to be modified, it runs the risk of producing empirically irrelevant or misleading results. This is illustrated by Bjørnstad and Nymoen (2008), Fanelli and Juselius (2008) regarding the new Keynesian Phillips Curve model, by Mercerau and Miniane (2008) regarding Present Value Models of the Current Account, by Nielsen (2008) regarding Cagan's model of hyper inflation and money demand, by Giese (2008) regarding the expectations' hypothesis of the term structure and by Juselius and Ordonez (2009) regarding the constant natural rate hypothesis. Most of the above papers use cointegration techniques to assess the empirical robustness of extant models. The statistical properties of the empirical models have been carefully checked in all of them, though the exploratory potential of a VAR analysis have only been exploited in a few.

3 Bridging Economic Theory and Statistical Evidence

The pronounced lack of empirical support of many popular economic models demonstrated by the articles in this issue will not come as a surprise to many economists, who would argue that their models are not meant to be close approximations to the

economic reality and that by adding new features their models will gradually be improved[5]. I would like to challenge this view and argue that most economic models are intrinsically developed for a stationary world, whereas most data are nonstationary. As illustrated by the articles in this issue, nonstationarity is likely to have strong implications for the role of expectations and the *ceteris paribus* assumption, just to mention two of the more important aspects of economic models.

3.1 The Implications of Nonstationarity on Statistical and Economic Modelling

Many economic theory models have evolved as non-stochastic mathematical entities which have been applied to data by adding a NIID error term (more recently a stationary AR process) and using Students t, χ^2, and F tests[6] to make inference. While such an approach could potentially be appropriate in a stationary world, it becomes problematic in a nonstationary world. For example, reporting p-values from models incorrectly asserting stationarity is meaningless. But not all inference based on stationary processes has become useless when data are nonstationary. This is, because the theory of nonstationary processes shows that unit roots and breaks in the data can be eliminated by cointegration without loss of information (see for example, Engle and Granger 1987; Johansen 1988; Hendry 1987, 2009). Once the cointegration rank is determined[7], it is straightforward to transform the nonstationary data into stationary components using differencing and cointegration so that standard inference applies again.

The nonstationarity of economic data has, however, even more important implications for how we associate our economic theory models with our economic reality. The fact that economic data often are well described by the CVAR models may suggest that empirically relevant economic models need to be formulated as dynamic adjustment models in growth rates and equilibrium errors, the so called equilibrium correction models. See, for example, Hendry (1987, 1995), Juselius (2006). This is the type of model most of the contributors have used and discussed in this issue.

Such models are based on the assumption that data have been subject to permanent shocks that cumulate over time to generate stochastic trends in the variables. Many DSGE models would also allow for such permanent shocks, for example shocks to technology and preferences. But, in this case, the nonstationarity of the data is incorporated in the model by *assuming* an exogenously given stochastic trend. The difference between the two approaches is that the number of stochastic trends is estimated in the CVAR model, not assumed, and the presumption that one of them is a technology trend would be formulated as a testable restriction on the parameters of the model rather than imposed from the outset. But the general structure of a DSGE and a CVAR model is similar and the former could in principle be a submodel within the CVAR. By simplification testing of the CVAR model we might

[5]To cite one anonymous discussant to Spanos article: "I don't think DSGE modelers don't care about the empirical support of their theories; they just have a different metric to assess the usefulness of their theories." If the standard metric rejects the model, just choose a different metric.

[6]These are all based on stationary NIID errors.

[7]Only the test of the number of cointegrated relations, the so called rank test, needs a nonstandard, Dickey-Fuller type of distribution.

(but we seldom do) arrive at a parsimonious model which corresponds closely to the assumed DSGE model.

Because some DSGE models and the CVAR have a similar structure, one would think that the DSGE proponents would be more open to the latter. The fact that most US economists according to Colander have hardly heard about the European General-to-Specific CVAR approach to empirical macro may suggest that we have not promoted our methodology sufficiently well. We have used econometric concepts with strong economic connotations, but with different econometric meanings without an adequate translation between the two. Framroze Møller (2008) provides a thorough discussion of such economic/econometric concepts within the framework of a simple theory model.

3.2 The Role of Expectations in a Nonstationary World

The next topic concerns *expectations*. In a non-stationary world it seems highly unlikely that model based rational expectations could be empirically relevant as way of modeling expectations by economic actors. This is because the prevalence of nonstationarity in economic time series, in itself, is evidence of the fact we do not know in which direction the future is moving. To act as if we do seems highly irrational.[8]

Rather than assuming that we can pre-specify *the* correct economic model and that agents make decision knowing this correct model and, hence, the correct information set, we might instead accept the obvious fact that agents do not know *the* right model, nor *the* right variables, that agents change their view as they learn more. Furthermore, if we add that agents, in view of their imperfect knowledge, are myopic and endogenously risk averse, then we will end up with a different theory, the imperfect knowledge economics (IKE) (Frydman and Goldberg 2007), that has recently shown to be empirically relevant. This theory avoids the pitfall of assuming constant 'structural' parameters, recognizing that with imperfect knowledge economic behavior is likely to alter over time. Nevertheless, IKE provides qualitative predictions which, though testable within the CVAR, are looser than those derived under the Rational Expectations Hypothesis (REH), which are testable but essentially always rejected.

For example, the IKE theory predicts that expectations based on imperfect knowledge are likely to drive financial prices away from long-run benchmark values for extended periods of time until increasing risk premia cause a price reversal towards equilibrium. Because there is no inherent tendency of prices to end up in equilibrium (with imperfect knowledge the equilibrium value is not known) they will cross equilibrium and continue away until next reversal. Such IKE behavior has many testable implications within the VAR model. For example, Frydman et al. (2009) showed analytically that (speculative) IKE behavior is likely to generate persistent movements around long-run benchmark values that can be described by

[8] Even though cointegration of nonstationary variables can be used to improve forecasts (Hendry 2006), the basic difficulty remains. Also, cointegrated relations are often subject to breaks (changes in the equilibrium mean) which are unpredictable *ex ante*, while understandable *ex post*. See Clements and Hendry (1999, 2008).

near unit root processes. Such processes are often not statistically distinguishable from unit root processes. Hence, we would expect to find more stochastic trends (persistence) in an IKE than an RE based model, which is what we mostly find. For example, the movements of real exchange rates away from fundamental benchmark values during periods of currency float have shown to be surprisingly persistent, often dubbed the ppp and the long swings puzzles (Rogoff 1996). Econometric testing within a CVAR has mostly found the real exchange rate to be empirically near I(2) and to cointegrate with the real interest rate differential (see, Juselius and MacDonald 2004, 2007; Johansen et al. 2009). This strong empirical finding seems ultimately to have provided a resolution to the 'PPP puzzle' (Frydman et al. 2008).

The above result is important for two reasons: (1) It shows that, under imperfect knowledge expectations, fully rational behavior in the financial market is likely to drive prices away from equilibrium for extended periods of time, i.e. unregulated financial markets do not necessarily contribute to an efficient allocation of resources (as the present crisis has amply demonstrated), (2) In an economy with no restrictions on the equilibrating forces, persistent movements away from one parity are likely to generate similar compensating movements in another parity. For example, the persistent movements away from Purchasing Power Parity (PPP) typical of periods of currency float would have to be compensated by similar equilibrium errors in the international Fisher parity. This is also what we find: A combination of the two persistent parity errors is often empirically stable. Thus, nonstationarity does not preclude stable relationships, it just moves the discussion of stability to a higher level. In this sense, a standard RE equilibrium relation can be seen as a testable special case of a more general IKE based equilibrium relation.

The implications of the above findings do not, however, stop with the PPP and the real interest rate differential: It can be shown that such persistent IKE behavior has implications also for the Fisher real interest parity, the uncovered interest rate parity and for the term structure and that under IKE, none of them are likely to hold as stationary conditions. This is illustrated by Giese (2008) in a CVAR analysis of the term structure of five US zero coupon bonds providing strong evidence that the term spreads are nonstationary, by Juselius and Ordonez (2009) in a CVAR analysis of Spanish data providing evidence of the strong implications of a nonstationary Fisher parity on the domestic wage, price, and unemployment dynamics.

3.3 The Ceteris Paribus Assumption in a Nonstationary World

It is a common practise to simplify a theory model by using the *ceteris paribus* assumption "everything else unchanged". However, the empirical relevance of the *ceteris paribus* assumption in a theory model is likely to be strongly affected by the order of integration of the *ceteris paribus* variables. If they are stationary, the conclusions are more likely to remain robust than if they are nonstationary. In the latter case conclusions may (and often do) change. Because in the real world no variables can be kept artificially fixed, the empirical problem has to be addressed in the context of "everything else changing" and the impact of the *ceteris paribus* variables are brought into the analysis by conditioning.

By embedding the theory model in a broader empirical framework, sensitivity analyses using the CVAR approach may point out when the conclusions from an economic model are fragile with respect to the *ceteris paribus* clause and, thus, to lead to pitfalls in the economic reasoning. In this sense, a correctly done CVAR analysis can be useful for suggesting modifications of too narrowly specified theoretical models.

For instance, most rational expectations' models are based either explicitly or implicitly on the ceteris paribus assumption—constant (or, at least, stationary) real exchange rates, real interest rates, and interest rate spreads. As mentioned in the previous section, empirical analyses have suggested that the parities do not hold as stationary conditions. The interesting question is whether such a pronounced persistence away from long-run benchmark values has implications for the way we usually think about macro and policy. That it seems to have serious policy implications and requires a rethinking of our policy models is illustrated in many of the present articles. See for example, Giese (2008), Bjørnstad and Nymoen (2008), Fanelli (2008), Juselius (2008), Juselius and Ordonez (2009), Garcia-Solanes and Torrejon-Flores (2009), and Nielsen (2008).

4 Incentives, Diversity and Debate in European Economics

In his article in this volume, Colander (2009b) asks the question: why has the CVAR approach not taken off in US? Is it because the CVAR is not as good as the DSGE to promote the 'truth'? He argues that the reason it hasn't is not that it is not a better approach; the reason is that the economics profession's institutional structure is flawed and that there exists a serious agency dilemma. He argues that, in the present system based on 'publish or perish', there is an incentive for publishing articles rather than searching for the truth in the best possible way, i.e. publication of articles has become an end in itself, and methods that are less conducive to article publication tend not to be chosen.

Today, the majority of academic incentive structures is based on quality weighted journal publications strongly influenced by the US system where such a structure has long been at work. While Europe used to be more eclectic, the present extreme version of the US incentive system forces European economists to focus their research for publication in top US journals. This leaves a small group of US editors in control of the research agenda of European macro economists. As the former generally favour the 'theory-first' type of empirical research, exemplified by the DSGE approach, few of the top journals would accept papers based on the CVAR 'reality first' approach. However, to advance in the US and European university system, economists need publications in US top journals.

The question is why are US top journals in favor of 'theory first' research. Spanos (2009) argues that the historical legacy of the preeminence of theory may explain the editorial preference. The fact that the DSGE model is a familiar way of doing research can explain why the editorial board of the top journals favors it. In many ways, DSGE models can be seen as a refinement and direct continuation of a research

paradigm that has been predominant over at least the last 4–5 decades. This may also explain why the profession has adopted it so widely: not because it is easy (it is often extremely involved to derive and estimate a DSGE model) but because it corresponds to the conventional way of thinking and it corresponds to how economists have been taught economics over the last decades. The mathematical derivations are reasonably straightforward (though not easy) to check by referees which may add credibility to the acceptance process[9]. Nevertheless, it seems plausible that the CVAR approach would have been more widely accepted if its results had been more favorable to the preferred theory view.

While previously European research was allowed to take time (admittedly this also led to unacceptable waste) it has nowadays been replaced by the 'publish or perish' culture where the number of 'quality' weighted publications determines your academic career. By definition, a new journal (such as this journal) cannot immediately be a top journal, but to attract the best and influential papers it has to be. Thus, our best economists are from the outset likely to choose topics and methods favoured by the top US journals and Europe will give up the possibility for scientific debate on home ground. In such a system, there is an inherent risk that young Ph.D. students, in particular, will choose quick and safe research topics, while more complex topics, of possibly high relevance for our society, might be considered too risky. This seems to be a highly unfortunate consequence of the present incentive system and probably not what the tax payers would have liked their money to be used for.

Thus, there might have been some less desirable (unintended?) consequences of moving to the present incentive system with its built-in tendency to make European research a (second rate carbon) copy of US economics (Colander 2008). It also seems to have had the (unintended?) effect of streamlining the way we think about economics, do research, and make policy. But, to change economics into an empirically more relevant direction, the present incentive system of 'publish (in US top journals) or perish' needs to be changed. That is highly unlikely.

Even today, in the middle of a serious financial and economic crises, it is hard to convince an editor of a US top journal to publish empirical results that have been found by a careful CVAR analysis. The immediate reaction to such results is mostly that CVAR results are not consistent with the results of the DSGE theory models, hence they must either be wrong or irrelevant'. Nevertheless, the clues to the present crisis are hidden in the historical data and if we wish a better understanding of the underlying mechanisms of the crises and its influence on our economies, we have to take this data much more seriously than done in approaches where data are silenced by prior restrictions. In contrast, the CVAR approach gives the data a rich context in which to speak freely about a multivariate, path-dependent, dynamic, and reflexive reality at the background of relevant dynamic macroeconomic theories (see Hoover et al. 2008). But to convince proponents of the competing approaches we need to show convincingly that the general-to-specific methodology is able to deliver answers that are considered acceptable by the proponents. This, for example, could be by demonstrating that the CVAR analysis is capable of finding the 'true'

[9]The fact that valid statistical inference is based on equally strict mathematics as mathematical economics seems not equally much recognized by editors or referees.

DSGE structure in a simulated environment, designed so that the DSGE proponents decide about the structure of the model and the CVAR proponents decide about basic persistency properties of the data. If the CVAR approach is shown to efficiently distinguish between differently simulated DSGE models, it would be harder to object to their negative implications in actual applications.

5 Using Empirical Econometrics to Improve Economic Models

The idea of the special issue was to challenge the dominance of the 'theory first' over the 'reality first' approach broadly interpreted. One could say that reality almost surpassed itself in providing us with a relevant example. That the present financial and economic crisis has demonstrated the empirical failure of 'the theory first' approach seems rather obvious. Few professional economist were able to foresee this immensely deep and probably long lasting crisis. Central banks, governments, and the public were mostly taken by complete surprise. Even as the crises unfolded, extant theory models did not provide (much needed) guidance for how to ride out the crises. Policy makers were essentially left to grope in the dark hoping that their costly policy measure would have the intended effect (see Colander et al. 2009). While painful for much of the world, the fact that DSGE models have a hard time addressing the crisis at all—much less explaining it—may, however, be good news for all kinds of economists who think differently and previously have had a hard time making their voices heard.

Let us assume for a moment that economists, say as a result of the present economic crises, will start looking for a paradigmatic change in their theoretical understanding of the empirical reality. How should it come across? My suggestion is to learn from the data in a systematic and structured way. If one takes macroeconomic data seriously, it comes often as a surprise how informative they are. As already discussed, the biggest hurdle in learning from data is the (almost irresistible) urge to impose too many economic priors on the statistical model, in spite of them being against the information in the data. If the outcome of the empirical testing is that a particular assumption isn't in the data and that the economic conclusions using that assumption are not robust, it is an important signal both to the theorist and to the decision maker. The articles in this issue contain many such important signals.

My interpretation of Colander's argument (that the important distinction between 'the technical apparatus' and the 'art of economics' seems to have been wiped out in today's use of DSGE modelling) is that these models, though technically impressive, may need a reality check. Because the CVAR model is by construction 'bigger' than the theory model(s), it may provide such a check. Such empirical checking should address not just one but several (possibly competing) theoretical hypotheses, but it should also be open to evidence that could generate new hypotheses to be subsequently tested on new data. In this sense, the CVAR can provide a framework within which the DSGE proponents should be able to properly test their theoretical assumptions—bringing those assumptions to the data. The articles of this issue illustrate that such an approach is highly relevant.

5.1 The Articles in this Volume

This issue contains altogether 12 articles, of which the ones by Colander (2009b) and Spanos (2009) have a strong methodological orientation. These two articles provide each an insightful discussion of different aspects on economic methodology, Colander from the viewpoint of an eclectic macroeconomist, Spanos from the viewpoint of an econometrician. Any one interested in the methodology of economics will find them exciting reading. Because their main ideas have already been introduced in the previous sections, I not repeat them here. The remaining 10 articles, all of which are addressing various empirical/econometric issues, have only been briefly mentioned in the previous text. I, therefore, give a short description of their main contributions and how they are related to the methodological theme of this special issue.

Framroze Møller (2008) discusses how to bridge the gap between the 'theory first' and the 'reality first' approaches by starting from a simple theoretical model and then showing how this model could be analyzed in a cointegrated VAR model. By doing this he shows that there is a close correspondence between basic economic and econometric concepts: the economic relations (or rather observed deviations from these, the equilibrium errors) correspond to cointegration relations; an economic equilibrium corresponds to the long-run value in a CVAR model; the comparative statistics are given by the long-run impact matrix in the CVAR; the exogenous variables are the common trends in the CVAR. While exogeneity in an economic model has no exact statistical correspondence in the CVAR, it can be given an empirical interpretation in terms of weak and strong exogeneity. Also the adjustment parameters of the CVAR are interpretable in terms of expectation formation, market clearing, nominal rigidities. This article illustrates that it is possible to bridge the gulf between economic models and the CVAR and that it is not even very difficult.

Giese (2008) finds strong evidence that two stochastic trends are driving the term structure of zero coupon bond rates of 1, 3, 18, 48, and 120 months maturities. Consistent with this she finds that the term spreads are nonstationary, but pairwise cointegrated. The economic interpretation is that shocks to the level and the slope of the yield curve are driving the interest rates, while the curvature of the yield curve is stationary. This is yet another illustration that stable relations can be found in a nonstationary world, but on a higher level. The finding of two stochastic forces, one originating from the long end of the term structure and the other from the shorter end, has strong implications for the effectiveness of monetary policy. The latter is usually based on the assumption of one stochastic trend originating from monetary policy shocks, which works their way through the term structure until they affects the very long rates. With two stochastic trends, this transmission mechanism is no longer straightforward.

Many of the articles are addressing inflation determination. Nielsen (2008) is one of them. He addresses the Cagan model of money demand during hyper inflation and discusses how to address it in an empirical VAR model with explosive roots. Using Yugoslav data as an illustration, he shows that the VAR model with double unit roots (the I(2) model), which has previously been used to analyze hyper inflationary episodes, is unsuitable as it does not account for the explosive nature of hyper inflation. But even when explosive roots are properly allowed for in the

VAR analysis, he argues that a linear analysis of prices and money will fail. This is because inflation develops explosively, whereas log real money develops more as a random walk. The solution is to replace the conventional measure of inflation (the change in prices) with a more appropriate measure of the cost of holding money.

In today's world with frequent fears of deflation, this may not seem as a very topical issue[10]. However, deflation is at the other end of hyper inflation and understanding how to analyze hyper inflation may very well be useful for understanding episodes of deflation. Furthermore, to understand inflationary mechanisms in extreme periods may in fact be more useful than to do it in 'normal' periods. The ultimate goal should be to formulate a model that works independently of the regime and Bent Nielsen's inflation model might very well be a good starting point. The analogy to the present crises is obvious: we need to formulate models that are able to handle crises as well as 'normal' periods.

Three articles are explicitly concerned with different aspects of the New Keynesian Phillips Curve (NKPC) model, which has been the preferred model for inflation determination among academics and central bankers for quite some time. All three demonstrate that the assumed theoretical model has serious problems when taken to the data.

Bjørnstad and Nymoen (2008) use a panel of 20 OECD countries to test the NKPC model. They are able to replicate the typical features reported in the literature of such models, but when testing the NKPC model against a dynamic imperfect competition model (ICM) in an encompassing environment a different conclusion is reached. In this set-up they find that the two main explanatory variables in the NKPC model, the expected rate of future inflation and the real marginal cost, have only weak explanatory power in contrast to their typical high significance in the literature. The different result is due to them being correlated with the equilibrium correction term of the ICM model. The conclusion is that the ICM model can encompass the NKPC model, but not vice versa. That the expected inflation term in the NKPC model drops out when the information set is increased raises serious doubt about the theoretical basis of the NKPC model. It also illustrates that in a nonstationary world 'rational expectations' are in fact irrational as discussed in Section 3.2.

Juselius (2008) tests the joint hypothesis of the NKPC and the optimizing IS curve using US and aggregate Euro area data. Because the data are found to be approximately nonstationary, he uses a procedure by Johansen and Swensen (1999) for testing the cross-equation restrictions of exact linear rational expectations within the CVAR model. He finds that the restrictions of the NKPC are rejected regardless of the sample periods and regardless of the choice of proxy for real marginal costs. Because of the super consistency of cointegration coefficients, he is able to focus on the sub-set of restrictions associated with the model's steady states without imposing the remaining cross-equation restrictions. He finds that the cointegration restrictions are rejected in most cases. His article demonstrates that the nonstationarity of economic data does have an effect on the empirical relevance of theoretical models (developed essentially for a stationary world) and that it can be used to increase the power of testing such models.

[10] A Zimbabwean might be of a different view.

Fanelli (2008) asks whether the historically unsatisfactory fit of the NKP models (see references in his article) can be explained by (slow) learning behavior of economic actors. He allows economic actors to update their perceived coefficients as new information arrives using VAR-based learning. His results show that euro inflation and wage share are cointegrated, but the relationship is not stable over time, that the 'hybrid' version of NKPC is sharply rejected under the rational expectations hypothesis, that NKPC also tends to be rejected under adaptive learning but not as strongly. The latter conclusion is however based on a NKPC model with nominal interest rate included, which is not a feature of most such models. However, it is an interesting result as such, as it points to the importance of checking for the robustness of the *ceteris paribus* assumption in standard models. In particular it suggests that the non-stationarity of the fundamental parities discussed in Section 3.2 cannot be ignored when addressing inflation determination.

One important idea of the CVAR approach is that, by using the same methodological approach on different economies (studying the empirical reality through the same glasses) one can learn about similarities and dissimilarities and, hence, about the effect of regulation/deregulation, different economic regimes, etc. This issue is illustrated by Juselius and Ordonez (2009) in an empirical CVAR analysis of the Spanish wage, price and unemployment dynamics in the convergence period to the Euro area purchasing power parity. The idea is that the Spanish experience may contain useful lessons for future joining members. They find that the Balassa-Samuelson effect, product market competition and capital liberalization have been the main driving forces in this convergence period. Inflation has adjusted in the long run to the Balassa-Samuelson corrected purchasing power parity and in the medium run to two medium-run Phillips curve mechanisms; one where the unemployment inflation trade-off is a function of the real long-term interest rate and the real wage cost, the other where it is a function of real exchange rate and the long-term interest rate. The latter relations demonstrate the importance of a nonstationary real interest rate and exchange rate (and therefore of imperfect knowledge) on the domestic economy as discussed in Section 3.2.

However, to fully understand the present crisis (which has hit Spain very seriously) and how to cope with it, it would not be sufficient to model the inflationary mechanisms of consumer and producer prices. As the crises developed from a housing bubble and an inflated financial sector, the CVAR analysis would have to be augmented with an analysis of house price inflation and stock price inflation. While in no way a complete model of the Spanish economy, the results demonstrate that there are lessons to be learnt for possible new member states and that the empirical reality in Spain has been immensely more complicated than the proponents of the NKPC theory would like us to believe.

Garcia-Solanes and Torrejon-Flores (2009) study the Baumol-Bowen (B-S first stage) and the Balassa-Samuelson (B-S second stage) effect in a panel analysis of sixteen OECD countries and sixteen Latin American countries. Applying panel cointegration and bootstrapping techniques they find some evidence that the Balassa-Samuelson effects are empirically relevant for the Latin American countries, but less so for the OECD countries. The second stage of the BS hypothesis is clearly rejected for the more developed OECD group, which seems due to the persistent movements

in nominal exchange rates away from the long-term movements in relative prices. As discussed in Section 3.2, such persistent movements are inconsistent with the rational expectations hypothesis but fully plausible with imperfect knowledge expectations (Frydman et al. 2009). This provides further evidence of the importance of appropriately modelling expectations in a nonstationary world.

Mercereau and Miniane (2008) ask whether the empirical evidence on present value models of current account reported in the literature is robust to the (largely ignored) pronounced persistence in observed current account data. They show that the estimated optimal series are very sensitive to small sample estimation errors, making it almost impossible to determine whether the consumption-smoothing current account tracks the actual current account or not. They conclude that two important 'consensus' results in the literature - that the optimal series is highly correlated with the actual series, but substantially less volatile - are not statistically robust. Thus, the article provides evidence of the importance of properly controlling for (near) unit root behavior when taking the theory model to the data.

Many of the articles demonstrate the implicit importance of the expectations formation/forecasting behavior in the financial market for the real economy. Sucarrat (2009) addresses explicitly the question of forecasting financial variability and how to evaluate a number of different forecasting models. His contribution is to develop a framework for forecast comparisons of variability in models of financial return, to study the finite sample properties of the rankings obtained under different loss functions using Monte Carlo simulations, and to suggest a practical procedure for such comparisons. His results show that an explanatory model of financial variability, i.e. a model that does not treat variability (nor volatility) as a well defined entity, but as something that can be explained provided the right variables are chosen, can outperform other popular prespecified models. In this sense, his approach is similar to the 'reality first' approach, except that it is applied to the conditional variability rather than to the conditional mean.

5.2 Concluding Discussion

As amply illustrated by the articles, the 'reality first' approach indicate that many widely used theory models are empirically inadequate. Section 3 argued that this may not be so surprising considering that most theory models implicitly or explicitly are based on assumptions of stationarity, of *ceteris paribus*, and of model consistent rational expectations. Still, many economists would not accept that the empirical results discussed here make sense, because they do not necessarily do so in a stationary world. But models that make sense in a stationary world may not be very useful for explaining behavior in a nonstationary world, for example the present financial and economic crisis.

The 'theory first' view of how to address this crisis would very likely be to further elaborate the economists' standard tool kits including representative agents and model based rational expectations. The articles here illustrate that this view needs be challenged. To start from the idea that we know what the empirical model should tell us and then insist that the reality should behave accordingly is not just a recipe for deep frustration, but also for not learning what we need to know. It

can be compared with an archeologist who knows before he has started digging what he will find. The answer to this crisis is, in my view, *not* to force the theory models onto data, suppressing all signals indicating lack of empirical relevance, but to insist on building models that include these features. I have argued in this editor's introduction that one way of achieving this goal is to require that an empirically relevant theory model should be able to explain the main economic mechanisms of the data characterized by a correctly specified and adequately structured CVAR model. It is time to switch the role of theory and statistical analysis in the sense of rejecting the privileging of *a priori* economic theory over empirical evidence (Hoover, Johansen and Juselius 2008).

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