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Time to reject the privileging of economic theory over empirical evidence? A Reply to Lawson (2009)

Katarina Juselius

Øster Farimagsgade 5, Building 26, DK-1353 Copenhagen K., Denmark
Tel.: +45 35 32 30 01 – Fax: +45 35 32 30 00
http://www.econ.ku.dk

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Time to reject the privileging of economic theory over empirical evidence? A Reply to Lawson (2009)[1]

Katarina Juselius

Unviversity of Copenhagen

Economics Department

Abstract: The present financial and economic crisis has revealed a systemic failure of academic economics and emphasized the need to re-think how to model economic phenomena. Lawson (2009) seems concerned that critics of standard models now will fill academic journals with contributions that make the same methodological mistakes, albeit in slightly different guise. In particular, he is rather sceptical to use of mathematical statistical models, such as the CVAR approach, as a way of learning about economic mechanisms. In this paper I discuss whether this is a relevant claim and argue that it is likely to be based on a misunderstanding of what a proper statistical analysis is and can offer. In particular, I argue that the strong evidence of (near) unit roots and (structural) breaks in economic variables suggests that standard economic models need to be modified or changed to incorporate these strong features of the data. Furthermore, I argue that a strong empirical methodology that allows data to speak freely about economic mechanisms, such as the CVAR, would ensure that important information in the data is not over heard when needed. Adequately applied such models would provide us with an early warnings system signalling that the economy is moving seriously out of equilibrium.

Key words: Economic crisis, Dahlem report, CVAR approach, Theory-first, Reality-first, Imperfect Knowledge Expectations, Non-stationary data.

JEL classification: A1, B4, C3, C5, E0, E1, E2, E6.

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1. Introduction

In a recent paper Tony Lawson (Lawson, 2009) discusses the nature of the current economic crises and how it is related to the course of academic economics. His main argument is that economic policy analysis framed by the properties of mathematical deductivist models has not provided too much insight, and that such analyses are predominantly done by academic economists. This is an old debate in which more sceptical economists often had a hard time making their arguments heard. The seriousness of the present crisis has, however, changed this and given the opponents new wind in the sails. But, even though the diagnosis that the patient is ill is fairly easy to make, a prescription of a cure may be less straightforward. Tony Lawson, while seemingly agreeing with the diagnosis seems less sure about the cure. He is concerned that the opponents now will fill academic journals with contributions that make the same methodological mistakes, i.e. build new formal mathematical deductivist models albeit in a slightly different guise. As an example he mentions the so called Dahlem report (Colander, 2009), in which a group of economists (of which I am one) discuss the 'financial crisis and the systemic failure of academic economics'. In this report we argued among others that

"*few professional economist were able to foresee the present immensely deep and probably long lasting economic crisis. Central banks, governments, and the public were mostly taken by complete surprise and 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."*

We also proposed a number of new approaches to cope with the complexity of the economic reality, which Lawson interpreted as 'more of the same' See Colander et al. (2009b) for a general response to this criticism.

Among others, the Dahlem report advocated also the use of empirical relevance (such as consistency with major empirical facts derived from Cointegrated VAR (hereafter CVAR) analyses as a criterion for discriminating between useful and less useful models. Lawson (2009) strongly questions the usefulness of statistical models, such as the cointegrated VAR model, as a means to learn from data. My purpose here is to address this particular issue.

In his concluding discussion (p.776) Tony Lawson refers to Keynes' well known scepticism regarding the use of econometric models:

"Given the ongoing developments, this seems an opportune moment to recall Keynes' evaluation of the relevance of econometric techniques in particular, resting, as these techniques mostly do, on the methods of multiple correlations."

Let me fist emphasize that multiple correlations, i.e. multiple regression models, were *not* what the Dahlem group had in mind when discussing the usefulness of econometric modelling. I strongly doubt that any one in the group would have supported the use of

such models as an appropriate tool for making inference on our highly complicated, multidimensional, non-constant, dynamical reality, had we discussed this possibility. This is so partly because a multiple regression coefficient is based on a postulated (a priori) causal relationship (*x* causes *y* given some *z*), whereas economic mechanisms are inherently simultaneous, multidimensional and dynamic, partly because standard statistical inference (*t*-, *F*-, and *Chi-square* tests) from a multiple regression model would be incorrect as such inference is based on an assumption of stationarity, whereas most economic data are nonstationary[2].

However, the implications of nonstationarity for economic modelling are more far-reaching than just switching to a different asymptotic distribution (such as a Dickey-Fuller distribution). When variables and relations are nonstationary rather than stationary many standard procedures and assumptions in economics become highly questionable, such as the use of the *ceteris paribus* assumption to simplify the economic model. This is because it is unlikely that conclusions from such a model would remain unchanged when the *ceteris paribus* variables are nonstationary rather than stationary. Also the use of model based rational expectations becomes questionable as it often would be irrational to pretend the future is predictable with any reasonable precision in a non-stationary world characterized by stochastic trends and structural breaks (Clements and Hendry, 1999).

As many economists may not be aware of the full implications of nonstationarity for their empirical and theoretical models, I shall use the opportunity to here discuss this issue at some length. The outline of the subsequent discussion is as follows: Section 2 discusses two different approaches to empirical economics, Section 3 discusses nonstationarity and the CVAR approach, Section 4 nonstationarity and expectations, Section 5 nonstationarity and the ceteris paribus clause, Section 6 nonstationarity and its implication for economic modelling. Section 7 concludes.

2. The Theory-First versus Reality-First Aproach

Before discussing the implications of nonstationarity for empirical economics, it is useful to discuss two different methodological approaches, theory-first versus reality-first, which differ in important aspects of how economists and statisticians approach the data. While the former approach has been and still is the preferred way of doing research in economics, I shall argue here that time is getting ripe for a change towards the latter.

The theory-first approach is to formulate a theoretically well-specified mathematical model and apply statistical methods to 'quantify' its parameters. It presumes that the basic economic mechanisms can be pre-specified, i.e. we know which the exogenous forces are, which variables adjust when the system has been pushed out of equilibrium,

[2] Much empirical work in economics seems unaware that valid statistical inference is based on mathematical statistics and is subject to equally strict scientific rules as a derivation of second order conditions in a mathematical model in economics. Nobody would accept a mathematically incorrect second order condition, but most economists would happily report Student's t-statistics even though the basic assumptions underlying this distribution are flagrantly incorrect.

how interventions have affected the system, etc. Econometrics in this case play the subordinate role of providing estimates of the economic parameters assumed to be empirically relevant from the outset. Such an approach will, almost by construction, be less open to signals in the data suggesting the theory is incorrect or in need for modification. It, therefore, runs the risk of producing empirically irrelevant and misleading results as illustrated by several papers in the special issue of the *E-Journal Economics* "Using econometrics for assessing economic models" such as Bjørnstad and Nymoen (2008), Fanelli (2008) and Juselius (2008) regarding the new Keynesian Phillips Curve model, Mercerau and J. Miniane (2008) regarding Present Value Models of the Current Account, Nielsen (2008) regarding Cagan's model of hyper inflation and money demand, Giese (2008) regarding the expectations' hypothesis of the term structure, and Juselius and Ordonez (2009) regarding the Phillips curve and the constant natural rate hypothesis.

The reality-first approach is to formulate a statistically well-specified model for the data to answer the economic questions of interest by imbedding the economic model within the statistical model. Reality-first advocates the use of strict statistical/econometric principles as a criterion for a good model. The statistical model ties economic theory to the data when it nests both the data-generating process and the theoretical model. Then the parameters of the theoretical model can be read as assertions about parameters of the statistical model, which can then be tested against the data provided that the statistical model characterizes it accurately (Hoover et al., 2007).

The reality-first advocates select the data based on broad economic relationships (demand and supply functions, etc.), without constraining them in a pre-specified direction and interpret the results at the background of not just one but several (possibly competing) economic hypotheses. This also facilitates the discovery of new evidence for which prior hypotheses has not yet been formulated. To achieve scientific objectivity, data cannot be constrained from the outset in a theoretically pre-specified direction, as it then would be impossible to distinguish between results which are due to the assumptions made and results which are genuine empirical facts. The only way the methodology works properly is by allowing the data to speak as freely as possible about empirical regularities[3].

Thus, in the 'theory first' case, statistics is often (ab)used as a tool to procure certain theoretically meaningful estimates [irrespective of their statistical meaningfulness]. This, I believe, is what Tony Lawson has in mind when dismissing the use of formal statistical models. In the 'reality first' case, the statistical model is taken seriously and used actively as a means of learning from data about the underlying mechanism of interest[4].

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

[4] Spanos (1995, 2009) discusses under which conditions it may lead to valid inference about the economic reality and why a 'reality-first' approach to economic modelling seems so foreign to most economists.

Hoover (2006) in a discussion of Walrasian contra Marshallian economics pointed out some methodological problems associated with the Walrasian 'theory first' approach:

"The Walrasian approach is totalizing. Theory comes first. Empirical reality must be theoretically articulated before it can be empirically observed. There is a sense that the Walrasian attitude is that to know anything, one must know everything. " .... "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."..."The Marshallian approach is archaeological. We have some clues that a systematic structure lies behind the complexities of economic reality. The problem is how to lay this structure bare. To dig down to find the foundations, modifying and adapting our theoretical understanding as new facts accumulate, becoming ever more confident in out grasp of the super structure, but never quite sure that we have reached the lowest level of the structure."

It was in the sense of the Marshallian archaeological approach that the Dahlem report discussed the use of econometric models as a means to improve our empirical understanding.

Suppose, as a result of the present financial and economic crisis, economists started looking for a paradigmatic change in their use of economic models as a way of improving their understanding of the empirical reality. How should they get along? Our answer in the Dahlem report was simply to learn from the data in a systematic and structured way. If we do not understand the historical past, there is little hope for understanding the future. Since a well-specified VAR in its unrestricted form is essentially just a convenient reformulation of the information in the data (Hendry and Mizon, 1993, Juselius, 2006, Chapter 3), the VAR approach seems an obvious place from which to start digging. It sees the world as a highly complex dynamic system, the properties of which must be inferred from data reflecting a single (nonreplicable) realization of a multivariate, path-dependent process. A key element of the VAR approach is, first, to get the probability model right (which is judged by ruthless application of diagnostic testing) to allow us to judge theories in relation to the testable restrictions that they imply for the probability model. Naturally, this implies that the data-generating process must be approximated by simpler relationships, which characterize the data accurately enough for our particular purpose (Hoover et al., 2007). Because theory models often are nonlinear and the VAR is a linear model, the latter works well when a first order Taylor expansion of the nonlinear theory model provides a good approximation of the empirical reality. For example, a VAR model that worked well up to the beginning of the present crisis may gradually show evidence of predictive failure as the crisis unfolds. However, this feature is valuable in itself as it signals that the market is significantly moving away from equilibrium.

Thus, a well-specified VAR can be a convenient summary of basic empirical facts (not stylized!) that a theoretical model should be able to explain during normal times to claim empirical relevance. In this sense, the VAR model can be thought of as providing confidence bands (broadly defined) within which the theory model should fall. Correctly done it is 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.

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: Without the mathematical logic of the theory model, one opens up for possibilities of quackery[5]. Is VAR modelling 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 being used to ask sharp and precise questions about the economy. This might be due to a lack of understanding of what 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, and 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 could be compared to an archaeologist using a shovel instead of a fine brush when removing dust to lay the underlying structure bare in an ancient site.

Another frequent argument is that the quality of economic data is too low for learning anything from them. While we agree that economic time series data seldom correspond very closely to the theoretical concepts of a theory model (prices, income, money, etc. in a theory model versus the multitude of different measurements CPI, PPI, PY, GDP, GNE, DI, M1 M2, M3, etc. that can be chosen in an empirical analysis), it is unfortunately the case that theoretically correct measurements do not exist and, hence, cannot be used by politicians and decision makers to react on. The forecasts, plans and expectations that agents base their decisions on are the observed data, however imperfect they are.

Even though macro data are contaminated with measurement errors, the latter may not be of great concern unless they are systematic and cumulate to nonstationarity. As a matter of fact, if one takes macroeconomic data seriously, it comes often as a surprise how informative they are. 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. To start from the idea that we know what the empirical model should tell us and then insisting that the reality should behave accordingly is not just a receipt for deep frustration, it also often leads the researcher to begin 'torturing' the data until they confess.

3. Nonstationarity and the CVAR

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

In the previous section I argued that the unrestricted VAR was just a convenient parameterization of the covariances of the data and, therefore, had to be subject to simplification testing to be useful. As statistical evidence indicates that nonstationarity is pervasive in economics, the first (and most crucial) step in the simplification search is to test the order of integration and cointegration. Accounting for (near) unit roots (i.e. persistence) in the model provides a powerful tool to make the statistical inferences robust. As a matter of fact, such inference would be misleading unless nonstationarity is adequately modelled, One way to account for a unit root is to transform the data to stationarity by differencing. But differencing throws away all the long-run information in the data. Fortunately, when data share a stochastic trend, a particular linear combination of the levels of the variables will also be stationary. Such variables are said to be *cointegrated*. A stationary cointegration relationship among nonstationary variables can frequently be interpreted as defining a long-run equilibrium toward which variables are adjusting.

By combining differenced and cointegrated data, the CVAR model is a natural way of analyzing economic data as short-run variations around moving longer run equilibria. As many theories offer testable implications about persistent shocks and steady-state behaviour, such dynamic properties are central to the CVAR approach. Longer run forces are themselves divided into the forces that move the equilibria (*pushing forces*, which give rise to stochastic trends) and forces that correct deviations from equilibrium (*pulling forces*, which give rise cointegrating relations). Interpreted in this way, the CVAR has a good chance of nesting a multivariate, path-dependent data-generating process and relevant dynamic macroeconomic theories. Unlike approaches in which the data are silenced by prior restrictions, the CVAR model gives the data a rich context in which the data are allowed to speak freely (Hoover et al., 2007). . See also Framroze-Møller (2008) for a detailed exposition.

Accurately characterizing the *persistence* of the data is a vital aspect of the CVAR approach as it allows us to classify and analyze data in terms of 'pulling and pushing' forces in the economy, thereby focussing on the long run. It allows us to answer questions such as: Can we identify the autonomous stochastic shocks that cumulate to stochastic trends? and: Which are the stable long-run relations? How do the system adjust after being hit by a shock?

4. Nonstationarity and expectations.

The strong evidence of (near) unit roots and (structural) breaks in economic variables and relations suggest that economic behaviour is often characterized by a pronounced persistence away from long-run steady states. Similarly, the strong evidence of shifts in equilibrium means and/or steady-state growth rates, suggest that forecasts from constant parameter theory models, assumed to be correct from the outset, are likely to perform poorly[6]. Since rational expectations models are inherently consistent with economic actors that are able to recursively foresee future outcome with known

[6] The model that usually wins in forecasting competitions is the simple second order difference model, which is totally void of any economic content. Sophisticated economic models usually come last. See Clements and Hendry (1999), Hendry (2006).

probabilities, the pronounced nonstationarity of the data (unit roots and breaks) seem to be untenable with these basic assumptions (Hendry and Mizon, 2009).

But, if rational expectations have to go, what should replace them? Frydman and Goldberg (2007, 2009) demonstrate that the assumption heterogeneous agents making forecasts based on imperfect knowledge fundamentally change the economic implications of rational behaviour in the financial markets. Instead of assuming that economists are able to pre-specify the correct economic model and that agents make decision knowing this correct model and, hence, make forecasts based on the correct variable set, Imperfect Knowledge Economics (IKE) assumes 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 risk averse and myopic, then we will end up with a theoretical framework that has proven to be empirically relevant, even remarkably so. Where the RE approach forces the observed data into the straightjacket of a highly stylized model, describing agents that know the equilibrium values of prices, say, the IKE approach tells us that agents do not know what these values are, that buyers and seller may have different views on what they are, that they change their mind on what information to use to forecast these values.

One important implication of the IKE framework is that speculative behaviour in a world of imperfect knowledge, while fully rational, has a tendency to drive prices away from long-run equilibrium values for extended periods of time generating a pronounced persistence in the data that is difficult (impossible) to reconcile with the RE based models. What makes the IKE theory interesting for a CVAR modeller is that these persistent equilibrium errors should be compensated by similar persistent behaviour somewhere else in the economy. Identifying, estimating and testing such relationships are exactly what cointegration analysis is about.

For example, in the foreign exchange market, IKE tells us that the frequently observed persistent movements in real exchange rates (deviations from purchasing power parity) should be counteracted by similar persistent movements in the real interest rate spreads (deviations from the international Fisher parity). Frydman et al. (2009) showed analytically that (speculative) IKE behavior is likely to generate persistent movements around long-run benchmark values that can approximately be described by near unit root processes. Hence, we would expect to find more stochastic trends (persistence) in an IKE than an RE based economy, 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, Frydman 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. It shows that under imperfect knowledge expectations, (1) unregulated financial markets do not necessarily contribute to an efficient allocation of resources (as the present crisis has amply

demonstrated) and (2) persistent (nonstationary) movements away from one parity are likely to generate similar compensating movements in another parity but that a combination of two persistent parity errors can be 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 fact that IKE provides a formal framework for thinking about economic behaviour without forcing our thinking into too stylized (and implausible) structures is in my view an important empirical advantage. The IKE implications are qualitative rather than quantitative and therefore needs the partnership of a strong empirical methodology. The cointegrated VAR methodology is designed to describe the complicated empirical reality, not a pre-specified theoretical model, and hence needs to be combined with a theory that can guide in the search for empirically relevant mechanisms without hampering the search. Thus, the CVAR approach combined with IKE seems to provide a strong partnership for testing hypotheses and learning about our complicated reality.

5. Nonstationarity and the ceteris paribus assumption

It is a common practice 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 economy no variables can be kept artificially fixed, the empirical problem needs 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.

Thus, by embedding the theory model in a broader empirical framework, sensitivity analyses using the CVAR approach can be used to check whether the conclusions from an economic model are fragile with respect to the ceteris paribus clause and, thus, whether they 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 these parity conditions are too persistent to be considered stationary. 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 this seems to be the case is illustrated for example by the above mentioned papers in the special issue of the *E-journal Economics.* See Juselius (2009). Thus, the nonstationarity of economic data is likely to have serious policy implications and requires a rethinking of our policy models.

6. Non-stationarity and economic modelling

Traditionally macroeconomic fluctuations in inflation, real growth, unemployment, etc. were studied using well-established long-run regularities in the data such as the consumption-income ratio, consumption function, (inverse) money-income velocity, demand-for-money functions, the Phillips curve relation, etc. These were derived from a general framework building on the IS-LM model explaining fluctuations in aggregate demand – aggregate supply in the economy. This paradigm was replaced by a micro-based macro-theory starting in the fifties and the sixties. The argument being that since the macro-economy after all is the aggregate of the micro units we need to have a macro-theory that can be derived from a micro-theory. But, to be able to arrive at a constant parameter model describing a few structural parameters, we have to rely on such unrealistic assumptions that most results can be deemed empirically irrelevant from the outset.

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). Such models are inherently consistent with the idea that unanticipated shocks cumulate over time to generate stochastic trends in the variables.

As discussed above, basic long-run relationships such as purchasing power parity, real interest rates, uncovered interest rate parity, the term spread, the consumption-income ratio, the capital-income ratio, the labour-income, the natural rate of the Phillips curve, etc. are mostly found to exhibit a pronounced persistence which is untenable with the hypothesis of stationary static steady states. In such a situation we can use cointegration analysis to find out which variables or relations exhibit a similar compensating persistence, i.e. we can search for empirically stable combinations of persistent (equilibrium) errors. When deviations from (static) equilibrium relations exhibit pronounced persistence, the mathematical logic of the CVAR model tells us that such compensating persistence is likely to be found in variables measuring nominal growth rates (Kongsted, 2005, Juselius, 2006). The implication for economic modelling is that we need to move from a framework of static to dynamic equilibrium relationships. Thus, the fact that we find static equilibrium errors to be unstable (nonstationary) does not preclude the existence of stable relationships, as already mentioned it just moves the discussion of stability to a higher level. In this sense, a standard static long-run relation can be seen as a testable special case of a more general dynamical equilibrium relation.

Many Dynamic Stochastic General Equilibrium (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 CVAR model rather than imposed from the outset. As the general structure of a DSGE and a CVAR model is similar, the former can in principle be formulated as a sub-model within the CVAR and is therefore a testable hypothesis. For example, Juselius and Franchi (2007) show how to translate the basic assumptions underlying the real business cycle DSGE model in Ireland (2004) into a set of testable assumptions on cointegration relationships and stochastic trends in a CVAR. Structuring the data in this way offers a number of empirical facts (for example that it was shocks to consumption that have generated the long business cycles rather than shocks to capital as in Ireland's RCB model), that a theory model should replicate in order to claim empirical relevance. In the Ireland case most assumptions underlying the model were rejected when properly tested, illustrating Tony Lawson's point that models that get the 'right results' and 'address interesting questions' may nevertheless turn out to be misleading and empirically irrelevant.

Would CVAR modelling be useful for a better understanding of financial and economic crisis? Take the recent crisis that was allowed to build up over a fairly long period without a proper warning system which, at an early stage, would have signalled that the system was moving seriously out of equilibrium. Already many years before the bubble burst, the relative house-consumption price index exhibited very pronounced nonstationary behaviour. This pronounced movement of house prices away from ordinary consumer prices was (primarily) facilitated by low price inflation and interest rates. Prior to the bursting of the bubble, the house – consumption price ratio increased almost exponentially signalling that house prices were moving far away from their sustainable level, given by the level of inflation and interest rates. This would have been the absolutely last moment for the authorities to react to prevent the subsequent disaster. But, the empirical result that the extraordinary high level of house prices were sustainable only by the extraordinary low levels of nominal interest rates and inflation rates should have been reason for concern much earlier. It could, for example, have resulted in a detailed market analysis as the one provided by Lawson in his article.

My point here is that without a proper understanding of the disequilibrium forces at the macro level, the informal (and highly valuable) market analysis in Lawson (2009) will often come too late to prevent the next crisis. Markets are infinitely inventive and it would be almost impossible to foresee what can generate the next crisis. Unless we build up a good macroeconomic understanding and a reliable signalling system (based on scientifically valid formal mathematical-statistical models) it is very likely that the next crisis again will come as big surprise.

The thought experiment 'what would Keynes have said to the CVAR approach' were he still alive is intriguing. As Keynes was a scholar with a deep respect for the complexity of economic life, he would probably have been convinced that econometric models, when adequately used, are indispensable as tools for improving our grasp of the complicated economic reality.

7. Concluding remarks

Tony Lawson argued in his article that "current academic research practices need to be transformed before real insight can be achieved". He cautions "*against the substitution of yet more formalistic models, albeit reflecting alternative economic hypotheses, in place of those that have hitherto been dominant*" and "*suggests that the formalistic modeling endeavour mostly gets in the way of understanding*". Provided that this can be interpreted to mean that the use of formalistic mathematical deductivist theory-first models has mostly failed to improve our understanding of the complex economic reality and that the use of econometrics as a means of illustrating these models has not provided much additional insight, then there is no disagreement. Where our opinions diverge is in the alternative use of econometric and economic models.

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. I believe 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 failing to learn what we really need to know. It can be compared to an archaeologist who knows before he starts digging what he will find. The answer to this crisis is, in my view, not to force the theory models onto the data, suppressing all signals indicating lack of empirical relevance, but to insist on building economic models that include these data features. This, by no means precludes the type of insightful analysis of the underlying causes of the present financial and economic crisis such as given by Tony Lawson and Axel Lejonhufvud in the 33rd issue of the Cambridge Journal of Economics of 2009. Both types of analyses are valuable and needed.

I have argued here that to understand the economic mechanisms that eventually caused the crisis we have to understand the mechanisms having generated the observed data. But, to take economic models to the data allowing for complex features such as interactions, dynamics, heterogeneity, etc. is technically (as well as intellectually) extremely demanding and would have been unthinkable without today's access to high speed computers. Now that we have the tools to compute even utterly complex models, we need to discuss how we can use the tools to efficiently learn from empirical evidence. I believe this is the direction to go as it seems unlikely that today's most pressing economic problems, such as the present financial crises, the effect of globalization on domestic economic policy, international governance and regulation, the effect of capital deregulation on domestic economies, how to guide and assist under-developed countries (Stiglitz, 2002), etc. can be adequately addressed by further elaborating the economists' standard tool kits including representative agents and model based rational expectations.

In the Dahlem report we argued that the nonstationarity of economic data has important implications for how we associate our economic theory models with our economic reality. We also suggested that an empirically relevant theory model should be able to explain the basic empirical facts as formulated in terms of the pulling and pushing forces estimated from a correctly specified and adequately structured CVAR model. By doing so, the CVAR approach will switch the role of theory and statistical analysis in the sense of rejecting the privileging of a priori economic theory over empirical evidence (Hoover et al., 2008). I believe it is time to allow this switch, which to be

successful will have to imply a change in the academic incentive system (Colander, 2009).

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