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Searching for a theory that fits the data: A personal research odyssey*

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Abstract

This survey paper discusses the Cointegrated VAR methodology and how it has evolved over the last 30 years. The first section is a description of major steps in the econometric development of the CVAR model that facilitated serious real world applications. The next three sections are primarily methodological and discuss (i) difficulties and puzzles when confronting theory with the data, (ii) the formulation of a viable link between theory and the data, a so called theory-consistent CVAR scenario, and (iii) how all this was inspired by Trygve Haavelmo and his Nobel prize winning monograph "The Probability Approach to Economics". The next two sections discuss early applications of the Cointegrated VAR model to monetary transmission mechanisms, international transmission mechanisms and wage, price and unemployment dynamics. They report puzzling evidence, discuss the need for new theory, and propose a method for combining partial CVAR analyses into a larger macroeconomic model. The following sections propose a new, empirically-based, approach to macroeconomics in which imperfect knowledge based expectations replace so called rational expectations and in which the financial sector plays a key role for understanding the long persistent movements in the data. The last section argues that the CVAR can act as a "design of experiment for passive observations" and illustrates with several applications including unemployment dynamics under crises periods and aid effectiveness in South Saharan African countries.

Keywords: Cointegrated VAR methodology, Linking theory and evidence, Empirically based macroeconomics.

JEL code: B41, C32, C51, C52

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1 Introduction

This survey paper is based on my retirement lecture given at the Economics Department of the University of Copenhagen in 2014. Since retirement is one of the important dividing lines in a long active life, I used it as an opportunity to slow down for a while to reflect on my professional career. I asked myself: what were the main questions that motivated my research; how did I go about answering them; what stones did I stumbled on; and the most important one: did my research help contribute to useful answers. The present paper is an attempt to answer these questions.

From the outset, my professional interest was primarily in the field of empirical methodology. I was looking for a procedure that would allow me to test theoretical assumptions rather than just believing in them. In this phase of my early formative years David Hendry and Clive Granger were influential for my thinking. I read almost everything they published and found inspiration from the "general-to-specific" error-correction approach developed by David and the numerous time-series issues formulated by Clive. But, while all this was important, it was Clive's 1981 working paper on cointegration and error-correction that changed both my professional career and my personal life. From the outset I was intrigued by the concept of cointegration and how it related to error-correction. I spent hours and hours on the mathematics. The problem was that Clive defined cointegration in a vector MA average model of unobservable errors that was extremely difficult to estimate at that time, whereas error-correction models were based on the autoregressive model in variables and were much more straightforward to estimate. I found it hard to grasp the intuition of a model formulated in errors rather than variables and could not see how to use cointegration in empirical work.

In 1982 I organized a session on econometric time-series analysis at the conference of Nordic Statisticians and asked Søren Johansen to give a prepared comment on Clive's paper. Søren was able to see the beauty of Clive's cointegration idea and gave an insightful presentation in which he envisioned its potential for solving the problem of nonstationarity of time-series processes. As most economic series are approximately nonstationary but the statistical theory used to analyze them was based on stationarity, this was of course extremely important. One can say that we stumbled over a gold mine of relevant problems to be solved. Søren's formal training in mathematical statistics guaranteed scientific rigor - a prerequisite for academic quality. I was thrilled by the numerous possibilities for asking new and relevant questions about our economies that cointegration analysis seemed to offer.

To my great relief, Søren was able to formulate the concept of cointegration in the autoregressive model while still building on Clive's representation theorem. It was an important breakthrough when Søren derived the so called trace test for the cointegration rank and its asymptotic, nonstandard, distribution. The latter was based on Brownian motions instead of Gaussian processes and had to be determined by simulations. At that time computers were much slower than today and simulating the distribution of the trace test for a simple

VAR model of low dimension took several weeks. At the Econometric Society Meeting in Copenhagen 1986 Søren presented the first ML results on cointegration, the trace test and its asymptotic theory and I applied the results to a model for monetary transmission mechanisms in Denmark. In the middle of my presentation, one of Søren's students arrived with the first simulated tables for the trace test still warm from the printer. Unfortunately they were valid for a model without deterministics and, therefore, not appropriate for my empirical results. In spite of this, to be right on the research frontier was a exhilarating feeling.

Maximum likelihood cointegration immediately received a lot of attention and was subject to an overwhelming interest for how to use it in practice. Fortunately it turned out that the cointegration rank test was the only nonstandard distribution that had to be derived. After the rank was found, the nonstationary data was transformed to stationary using differencing and cointegration. Hence, standard statistical theory applied to the transformed model and one could test hypotheses using Students t tests, χ^2 tests, and F tests.

In those first years, Søren worked out the representation theory, the probability theory and the statistical theory that were necessary for applying likelihood based cointegration analysis to empirical problems. I used the results to obtain a ML based estimate of the Danish money demand relation based on a four-dimensional cointegrated VAR model. Fortunately, the relation turned out to be incredibly stable over time - probably one of the most stable relations I have ever seen in macroeconomics - and, because everything seemed to work well, it was an excellent data set to start with. Thus, we were able to develop the main cointegration tools before tackling more challenging problems, where the complexity forces you to rethink both econometrics and economics.

In 1987 I organized a small meeting in Copenhagen for a group of Nordic econometricians. We discussed the idea of applying for a grant from the Nordic Social Science Research Council (NOS-S) to establish a network of Nordic econometricians and successfully obtained funding to organize 2-3 annual workshops starting in 1989. To start with the funding was for a three years period, but we successfully got several extensions and also funding from other sources The last workshop took place in 2001. In the beginning we were approximately 15 participants, most of which were young Ph.D. students. As interest in the network grew steadily also among econometricians and empirical economists outside the Nordic countries, many well-known non-Nordic econometricians joined our workshops and conferences. At the end of the project, 60-80 researchers from universities, public and private research institutes, central banks, etc. took part in our regular meetings on cointegration research.

The Nordic project was highly successful. Most of the results to be discussed in the subsequent six sections were worked out during this period. Part of its success was due to the continuity of the research in the sense that each new workshop built on previously obtained results. It created a feeling of being part of an exciting collaborative endeavor and helped us to maintain focus. But the largest part was due to Søren. Without him sitting among us, listening to our not always well-formulated problems, translating them into clear math-

ematics, and working out a solution for the next workshop, the project would not have been nearly as productive. Søren taught us to become much better econometricians in those years.

In the mid-nineties, most of the econometric tools needed for a full-fledged CVAR analysis were already derived, mostly by Søren. Hence, I could then start focussing on the development of the CVAR as an empirical methodology and the applications to numerous economic problems. In particular, I was eager to work out a procedure for how to associate the rich structures of the CVAR with the concept of a "designed experiment for data by passive observations" as proposed by Trygve Haavelmo in his 1944 monograph "The Probability Approach to Economics". The basic idea was to offer a scientifically viable procedure for how to learn about important issues/problems in the economy using the CVAR as a magnifying glass. This could be used to detect changes in structure, to estimate and compare the structures before and after the shift, to observe similarities and dissimilarities between different economies and to relate these to institutional differences, and of course to check the validity of theoretical assumptions. Using the CVAR in this way revealed that the most dominant features of economic data were a pronounced persistence (i.e. slow adjustment), structural breaks, non-constant parameters, and strong feed-back dynamics. Unfortunately, they were often inconsistent with the basic assumptions underlying standard economic models. It gradually became obvious to me that a change in the research paradigm for empirical macroeconomics was very much needed.

All this is discussed in more detail in the subsequent sections organized as follows. Section 2 is a description of the major steps in the econometrics of the CVAR approach that were necessary for later applications to economic problems. The next three sections are primarily methodological. Section 3 discusses my first attempts to confront economic theories with data and my puzzlement when results did not support basic assumptions, Section 4 reports my long-lasting efforts to formulate a viable link between a theoretical model and the data structured by the Cointegrated VAR (CVAR) model, a so called theory-consistent CVAR scenario. Section 5 relates the proposed methodological approach to Trygve Haavelmo and his Nobel Prize winning monograph. Section 6 discusses early applications to monetary transmission mechanisms, international transmission mechanisms, and wage, price and unemployment dynamics. All sections discuss puzzling evidence convincing me of the need to search for new theory. Section 7 demonstrates how partial CVAR models can be combined into a larger macroeconomic model. Section 8 relates the excessive persistence documented in Section 6 - basically inconsistent with standard rational expectations' models but consistent with imperfect knowledge based models - to speculative financial behavior affecting asset prices but not consumer prices. Section 9 discusses how persistent long swings in real asset prices are likely to generate similar long swings in the real economy, in particular the unemployment rate. Section 10 argues that the CVAR can act as a "design of experiment" in macroeconomics. It illustrates the idea with an application to unemployment dynamics over the Finnish crisis period in the nineties and the recent Greek depression. A comprehensive comparative study of aid effectiveness in 36 South

Saharan African countries is also discussed as further evidence. Section 11 concludes with some personal reflections on what elements an empirically relevant macroeconomics should contain.

2 The first steps

From a statistical point of view the unrestricted VAR is the most general model:

$$\Delta x_t = \Pi x_{t-1} + \Gamma_1 \Delta x_{t-1} + ... + \Gamma_k \Delta x_{t-k} + \mu_0 + \mu_1 t + \Phi_1 D_t + \Phi_2 S_t + \varepsilon_t, \quad (1)$$

where x_t is the data vector, μ_0 is a vector of constant terms μ_1 a vector of trend coefficients, D_t a vector of dummy variables and S_t a vector of seasonal dummies. The $I(1)$ model is a submodel of (1) defined by reduced rank restrictions on Π and the $I(2)$ model a further submodel defined by reduced rank restrictions on (a transformation of) $\Gamma = I - \Gamma_1 - ... - \Gamma_k$.

As mentioned, the first defining moment was when Søren solved the reduced rank problems and we were able to address problems in an $I(1)$ world. But after having restricted Π to $\alpha\beta'$, the model was seriously over-parametrized and had to be simplified by statistical testing to become a model of economic relevance. This became increasingly obvious when we estimated CVAR models for different data set. Numerous questions of economic relevance lined up, all of them prompting for a mathematical solution. Søren delivered asymptotic distributions and maximum likelihood test procedures in a steady stream allowing us to formally answer these questions.

Søren's productivity in this period was remarkable. This is evidenced by examining Johansen (1989) and subsequently Johansen and Juselius (1990) in which the mathematical results of the former were applied to monetary transmission mechanisms in Denmark and Finland. The working paper versions appeared already in 1986, showing that in just a little more than one year Søren had already developed the basic building bricks for how to do likelihood inference on hypotheses involving the cointegration relations $\beta' x_t$ and the adjustment coefficients α.[1] For example, our Oxford Bulletin paper discusses both theoretically and empirically how to test and impose reduced rank on the VAR model, how to test hypotheses on the deterministic components (such as constant and trend), on the cointegration parameters β, and on the adjustment coefficients α, primarily $\alpha_i = 0,\ i = 1, ...p$. Besides the paper offered the first realistic application of a CVAR model to macroeconomic data which may have contributed to its popularity. For the Danish data we found that $r = 1$ and that only money stock was significantly adjusting to $\beta_1' x_t$, i.e. $\alpha_i = 0$ for $i = 2, ...p$. As an extra bonus, this turned out to be the condition for when a CVAR model is equivalent to a single equation error-correction model. Thus, the result showed that the

[1] The working paper version of the Oxford Bulletin paper was first submitted to Econometrica and was lying there for two years only to be rejected. David Hendry was the editor of Oxford Bulletin and was keen on having it. It quickly became one of the most cited papers in economics and Oxford Bulletin became the most cited journal shortly afterwards.

latter is a submodel of the more general CVAR model. While I would approach the empirical analysis somewhat differently today, I still think it is a paper to be proud of.

The next very influential paper, Johansen and Juselius (1992) discusses some additional tests on the cointegration relations $\beta' x_t$ based on an empirical application of the purchasing parity and the uncovered interest rate parity for UK data. The paper shows theoretically and empirically (i) how to test the same restriction on all β vectors that, if accepted, basically implied a transformation of the data vector, (ii) how to test the stationarity of a known vector in β, e.g. the stationarity of the real interest rate or an interest rate spread. The latter test procedure was extended to the case where some of the coefficients of a β vector are known but others have to be estimated, e.g. the test of a stationary real interest rate with an equilibrium mean shift.

A third influential paper (Johansen and Juselius, 1994), discusses the important issue of identification of the long-run cointegration structure in terms of three aspects of an identified structure: formal, empirical, and economic. The paper shows theoretically as well as empirically how to impose and test identifying restrictions on all β vectors and applies the theoretical concepts to an IS-LM model based on Australian data.

Finally, Juselius (1995) turned out to be very influential for two different reasons, one econometric the other economic. The paper represents an early work on purchasing power parity and uncovered interest rate parity based on two price levels, the nominal exchange rates and two interest rates for Germany and Denmark. The trace test suggested a rank of three which I thought would imply three stationary cointegration relationships. But the graphs of $\beta' x_t$ showed that some of them were clearly nonstationary. However, the graphs of the cointegration relations when the short-run effects had been concentrated out, $\beta' R_t$, looked definitely stationary. After the first puzzlement, we realized that this made sense in a model where $x_t \sim I(2)$, $\beta' x_t$ is a $CI(2,1)$ relation - i.e. cointegration is from $I(2)$ to $I(1)$ - and $(\beta' x_t + \omega' \Delta x_t) \sim I(0)$, - i.e. stationarity could be achieved by combining a nonstationary cointegration relation, $\beta' x_t \sim I(1)$, with a linear combination of the nonstationary differences, $\omega' \Delta x_t \sim I(1)$. This suggested a straightforward way of estimating and analyzing $I(2)$ models using the so called two-step procedure, subsequently to be replaced by the ML procedure. Thus, the $I(2)$ analysis was initiated by first looking at the empirical results and then trying to understand why they looked so strange. An illustration of how empirical analysis can positively guide theoretical econometrics.

The mathematical results needed for the probability/statistical analysis of cointegration were summarized by Søren in his 1996 book "Likelihood based inference in Cointegrated Vector Autoregressive Models" and the empirical methodology needed for economic applications by myself ten years later in my book "The Cointegrated VAR model: Methodology and Applications". At this time, cointegration had become the standard way of analyzing economic timeseries. In 1999 the Energy Journal asked whether David Hendry and I would be willing to explain the concepts of unit roots and cointegration for their readers. This we did in two companion papers, the first one in the context of a single

equation *ecm* model and the second in the context of a system CVAR model. The two papers, Hendry and Juselius (2000, 2001), became highly cited also outside the field of energy economics demonstrating the profession's interest in applying cointegration in various branches of economics.

While Søren's work on the mathematics of cointegration was fundamental for the success of the method, its wide-spread use would not have taken place without access to user-friendly software. Henrik Hansen translated our various program codes into a nice menu driven package, CATS in RATS version 1 (Hansen et al., 1994). It was the first software package to contain all the tests discussed above and the demand for it was correspondingly high. However, the CVAR methodology was subject to an ongoing development and the need for an updated version became more and more pressing. In particular, we desperately needed a menu-driven program for the $I(2)$ analysis containing not just the two-step procedure but a full ML analysis of the immensely rich $I(2)$ structure. For two years, Jonathan Dennis worked extremely hard to produce the next version CATS in RATS, Version 2.0. (Dennis et al., 2006). The new version contained not just a full-fledged $I(2)$ analysis, but also a variety of new and improved features. Among others it added an expert system for long-run identification that vastly facilitated the search for empirically meaningful long-run structures in the data. It improved my own productivity enormously, probably by a factor of at least 50. Recently Jurgen Doornik translated the RATS code into OxMetrics and invested an enormous amount of time and effort onto the project.[2] In particular the coding of the $I(2)$ analysis into OxMetrics was a major achievement. CATS, version 3.0 is now available (Doornik and Juselius, 2017).

In the mid-nineties, most of the CVAR theory was developed and all ingredients needed for a successful cointegration analysis were available. The appealing novelty of the CVAR model was that it was tailor-made to study long-run, medium-run and short-run structures in the same model, allowing the complexity of the empirical reality to be grasped and better understood. The autoregressive formulation of the CVAR was designed to describe cointegration and adjustment, the so called the pulling forces whereas the moving average formulation described common trends and impulse response functions, the so called pushing forces. It offered detailed and immensely rich analyses of a variety of economic issues including estimates of the important dynamic responses which had previously been difficult to estimate. I was convinced that the CVAR approach would mean a big step forward toward an improved understanding of our macro economy.

3 Confronting theories with data

From the outset, the idea with the CVAR was to offer a framework in which data would be allowed to speak freely without being silenced by prior restriction,

[2] Andreas Noack Jensen was first hired to make coarse translation of some of the procedures from RATS to Ox code.

in which prior hypotheses could be adequately tested and empirically relevant structures estimated. This would allow economists to properly test their theoretical models and their assumptions - bringing those assumptions to the data. If the outcome of the empirical testing was that a particular assumption wasn't in the data and that the economic conclusions using that assumption were not robust, then I thought this would be an important signal to the decision maker. To my disappointment, not many economists seemed interested in having their models robustified or falsified.

The CVAR approach is Popperian in this sense that the fundamental principle builds on the ability to falsify a hypothesis. In contrast to forcing your preferred theory model onto the data - even though they protest strongly - the idea is to let the statistical analysis be a guide to an empirically relevant structure. If the latter is inconsistent with your prior, the analysis will help you to see where and why your priors were wrong. Thus, using this procedure also allows you to do sensitivity analyses - seeing how the answer might change if the economic model is modified in an empirically more relevant direction.

While I had not expected the empirical results to perfectly support standard theory I never thought that they would deviate so much and that the conclusions would be so different. Discovering that some very fundamental relationships, based on which most macro models were built, were not supported by the data was highly disturbing and forced me to think about methodological issues. After numerous less successful attempts to interpret the CVAR results based on mainstream theories, it dawned on me that economic theories might make sense in a stationary, but not necessarily non-stationary world. As few economic models at that time made an explicit distinction between stationary and nonstationary processes, the idea of stochastic trends as the driving force of a system and of dynamic adjustment to long-run equilibrium as the pulling force was foreign to most economists. Exogeneity played an important role but was differently defined in economics and econometrics. In the former case it was essentially assumed, in the latter defined as weak, strong, and super exogeneity which were formulated in terms of the statistical model and, hence, testable. See Engle, Hendry and Richard (1983).

Since the seminal paper by Sargan (1964), error-correction models had been developed in numerous papers by David Hendry. These were mostly applied as single equation models and the error-correction mechanisms was then formulated intuitively as a measure of an equilibrium error rather than mathematically defined. But not even these relatively simple and economically intuitive error-correction models seemed to exert much influence on standard economic models. A bridging principle that could link theoretical models in economics to the pulling and pushing forces of the CVAR model seemed desperately needed. Juselius (1993) was my first attempt to discuss this dichotomy in terms of a monetary problem, but the paper did not yet offer a bridging principle.

The *ceteris paribus* assumption was another issue I was concerned about. In a theoretical model this assumption allows you to keep certain variables fixed and, therefore, to focus on those of specific interest. In an empirical model you have to bring these *ceteris paribus* variables into the analysis by condi-

tioning. If they are stationary, the conclusions from the theoretical model are more likely to be robust, but if they are non-stationary, the conclusions can and often do change fundamentally. Because of this, it worried me a lot that I frequently found important economic determinants like the real interest rate, the real exchange rate, and the term spread to be empirically indistinguishable from a unit root process. While not all of them enter every macroeconomic model, most of them are - explicitly or implicitly - part of the *ceteris paribus* assumption, everything else constant or, more realistically, "everything else stationary". When stationarity was replaced by nonstationarity, I often found that conclusions changed in a rather fundamental manner. I gradually realized that the theory division of variables into endogenous, exogenous and fixed could not be assumed to hold in the empirical model.

The use of *expectations*, that play such a prominent role in economic models, was also problematic for empirical models formulated in observed variables. Economists solved this problem by making assumptions on how (rational) economic agents would forecast future outcomes, the so called rational expectations' hypothesis (REH). Even though most empirical models in macro were estimated subject to restrictions under the REH, I could not adopt the REH as an empirical modelling device. This was partly because I considered the underlying assumptions defining rational economic behavior to be highly unrealistic in a nonstationary world. But it was also because tests of the REH in a CVAR model showed that it had essentially no empirical support in the data. See for example Johansen and Swensen (1999, 2004).

How to solve the problem of unobserved expectations in a CVAR analysis was an issue that bothered me a lot and for a long time I had no clue of how to solve the problem. After I came across the theory of imperfect knowledge expectations I began to see a way forward, but it took me a long time before I was able to formulate a CVAR scenario that also included testable assumptions on a theory-consistent expectations' formation. See Juselius (2017a, b).

Finally, there was the important issue of *aggregation* from the micro to the macro level. Most theoretical models in macroeconomics were then based on the assumption of a representative agent. This simplifying assumption clearly facilitated a mathematical formulation of the economic problem but often at the expense of empirical relevance. It certainly seemed to be one reason why my empirical CVAR results deviated so strongly from the ones assumed in standard economic models. However, even though the empirical results differed from the standard neoclassical model assumptions, they nonetheless made more sense in terms of more old-fashioned Keynesian type of macroeconomic models. Since the macro variables are aggregates of millions of idiosyncratic micro units, it seemed highly surprising to find so many plausible relationships in the data. To my relief, I came across a paper by Clive (Granger, 1981) that proposed a plausible explanation for why this was the case. Juselius and Beyer (2009), was an econometric attempt to study the sensitivity of outcomes with respect to different aggregation methods and to propose a viable procedure. The practical problem of aggregating the components of a macro variable, e.g. EU-wide GDP, turned out to be surprisingly complex and far from straightforward when data

are nonstationary.

4 Linking theory with evidence: a bridging principle

I gradually acknowledged that a statistically well-specified empirical model and an economically well-specified theoretical model represent two different entities for which there were no direct links. My own experience in empirical modelling indicated that macroeconomic data were primarily informative about long run economic regularities measured by cointegration relationships, $\beta' x_t$, and about the pushing exogenous forces, $\alpha_\perp \sum_{i=1}^{t} \varepsilon_i$. The transitory effects, measured by Γ_i, were often found to be unstable based on recursive constancy tests. Hence, to develop a bridging principle exclusively for the long-term part of the model, seemed both promising and relevant. The idea was to assess the economic model in two steps: first test its long-run structure and, if not rejected, then its short-run structure conditional on the long-run. Econometrically, such a two-step procedure made sense as the long-run parameter estimates are super-consistent contrary to the short-run ones which are ordinary consistent.

In 1999 I was invited to give a presentation at a conference on "Macroeconomics and the Real World" held in Bertinoro. At that time I had been struggling to formulate a complete set of testable long-run hypotheses for a monetary model of inflation (Friedman, 1970 and Romer, 1996). It was a perfect chance for presenting this idea, subsequently labelled a *theory-consistent CVAR scenario*. Kevin Hoover was my official discussant and got interested in the idea. As a result we have been collaborating since then. My Bertinoro paper was published in the conference volume (Juselius, 2000a) but was also selected to appear in the special issue of the Journal of Economic Methodology as Juselius (2000b).

At around the same time, I made some early attempts to formulate a complete set of testable hypotheses about the purchasing power parity (PPP) and the uncovered interest rate parity (UIP), i.e. a theory-consistent CVAR scenario. To my own surprise the results were neither straightforward, nor trivial. But, due to other demanding commitments, it took roughly 10 years until I finalized the ideas in a chapter of the Handbook of Econometrics (Juselius, 2009b). Given the integration properties of the data, the paper demonstrated that a stationary PPP was empirically invalid, a result that was in accordance with the theory of imperfect knowledge economics (Frydman and Goldberg, 2007, 2011). That the PPP needs the UIP to become stationary was subsequently demonstrated in several papers. See for example Johansen et al. (2010), Juselius (2017a, b), Juselius and Assenmacher (2017), Juselius and Stillwagon (2018).

Over the subsequent many years I continued to develop the principles of a theory-consistent CVAR scenario. The main task was to derive a general procedure for how to translate basic assumptions about the shock structure and steady-state behavior of the theoretical model into testable hypotheses on the

pulling and pushing forces of a CVAR model. Such a set of hypotheses was supposed to describe a set of empirical regularities one should find in the data if the basic assumptions of the theoretical model were empirically valid. If a theoretical model passed the first check of its basic properties, then it was a potential candidate for an empirically relevant model. This idea became a guiding principle of my Oxford University Press book (Juselius, 2006) in which I demonstrated that essentially all basic assumptions underlying Romer's theoretical model on monetary inflation were strongly rejected by the data.

Massimo Franchi visited our department in 2006-7 and we decided to take a closer look at a paper by Peter Ireland (2004) titled "A method for taking the model to the data", in which a real business cycle (RBC) theory is formulated as a Dynamic Stochastic General Equilibrium (DSGE) model. Both the code and the data were available online and Massimo replicated all results of the paper. Based on a theory-consistent CVAR scenario for the model most of the assumptions made by Ireland were tested, essentially all of them were rejected, and all main conclusions were reversed (Juselius and Franchi, 2007). Mikael Juselius (2010) did a similar checking of a New Keynesian Phillips curve model and showed that its basic long-run assumptions were inconsistent with the data.

In 2008 I acted as a guest editor of a special issue for the E-journal *Economics* with the title *Using Econometrics for Assessing Economic Models.* Again, most of the submitted papers documented lack of support for basic assumptions of the chosen economic models. I also supervised numerous students over this period and their empirical results were almost without exception equally disappointing. At this background I wrote Juselius (2010, 2011).

5 Haavelmo's probability approach and the CVAR

As already mentioned, my most important inspiration came from the 1944 Nobel Prize winning monograph by Trygve Haavelmo. Based on stringent and insightful discussions, he distinguishes between statistical inference in economic models based on (i) experimental design data artificially isolated from other influences so that the validity of the *ceteris paribus* clause is satisfied; (ii) non-experimental data obtained by "passive" observation for which there is no control of the theory that have generated the data.

In the first case, inference is valid provided the experimental design is valid. In the second case, valid inference on the structural parameters of economic models is far from sure. A true pre-specified model cannot be assumed and any *ceteris paribus* assumptions are prone to be invalid as everything else is likely to have changed. While many abstract economic models would generally require experimental design data to yield valid inference, such experiments are unfortunately not an option in macroeconomics. The question is rather whether it is at all possible to confront stylized economic models with the complex economic reality without compromising high scientific standards? Is it at all possible to learn from the data in a systematic and structured way? Haavelmo's answer was to introduce the concept of a "design of experiment" for data obtained by

passive observations and discuss the validity of inference in that framework.

It occurred to me that a well-structured CVAR model could be a candidate for such a design of experiment. Since there are many economic models but one economic reality, the statistical model should be sufficiently general (broad) to allow the data to speak freely about the empirical content of a variety of potentially relevant economic models. A correctly specified CVAR model satisfies this requirement and is designed to describe dominant features of economic data, e.g. dynamics, interactions, pronounced persistence and structural breaks. The paper Juselius (1993) was an early and incomplete attempt to discuss the CVAR model as a "design of experiment" for data by passive observations. Roughly 20 years later, in connection with the 100th year anniversary of Trygve Haavelmo, Hoover and Juselius (2015) offered a much more well-argued discussion of the concept "design of experiment" for data by passive observations and argued that the CVAR may represent such an experiment. In the same volume Juselius (2015) offered a more elaborate discussion of the ideas initiated in Juselius (1993). In this paper, using the concept of "a theory-consistent CVAR scenario" I demonstrated how to translate one of Haavelmo's own economic models into a set of testable hypotheses on the CVAR model. This, I believe, is the closest I have come to demonstrate that the CVAR could act as a design of experiment for data by passive observations.

6 Early applications

While realistic applications of the CVAR were quite rare in the first years after Søren had solved the mathematics of ML inference in these models, the curiosity and excitement were enormous. This showed up as an overwhelming interest in my first CVAR application of Danish monetary transmission mechanisms. At that time, the discussion in macroeconomics was strongly influenced by Milton Friedman's claim that "inflation is always and everywhere a monetary problem". The consequence of this claim was that money stock control should be used to control inflation. What was needed was a monetary authority that was dedicated to keep money supply aligned to the equilibrium level of a money demand relation.

Most attempts to estimate such a relation had been based on simple regression models, or in some cases on single equation error-correction models. The CVAR approach was therefore considered a big step forward in terms of generality and sophistication. I myself was convinced that the CVAR would produce more efficient and much improved estimates and in many ways it did: I found a completely stable money demand relation for Danish data with a plausible coefficient to the cost-of-holding money (measured by the long-short interest rate spread). From an econometric point of view, the results were simple and straightforward to interpret: the trace test suggested $r = 1$ so there was no need to impose (difficult) identifying restrictions on the long-run structure and the estimated cointegration relation was directly interpretable as a deviation from a plausible money-demand-relation. So far everything looked good! But then the

α coefficients showed that money stock was exclusively adjusting implying that monetary shocks had no exogenous effect on the system. Even more problematic, the results implied that cumulated shocks to the two interest rates acted as exogenous drivers to the system against the expectations' hypothesis. It was a successful econometric example, but many of the results were economically puzzling.

6.1 Is inflation a monetary phenomenon?

One problem with the Johansen and Juselius (1990) results was that inflation rate was not part of the VAR system. This was because at that time we were not yet aware of the econometric consequences of the nominal-to-real transformation (Kongsted, 1999). The latter implied that the inflation rate should have been included in the data vector to prevent the loss of some, potentially important, information.[3] Thus, the possibility that the puzzling results were due to the missing inflation rate in the VAR system had to be checked. However, in an extended CVAR model with inflation as a system variable I identified the same empirically stable demand for money relation. As the cointegration property is invariant to extensions of the information set, this outcome was predicted. But the remaining results were also as puzzling as before: (i) the deviations from the money demand relation did not significantly affect the inflation rate, (ii) money stock was still purely adjusting, (iii) monetary shocks had no exogenous impact on the system and (iv) the short-term interest rate seemed to follow the long-term bond rate instead of the other way around. To sum up, the exogenous forces of the system were given by the shocks to the long-term bond rate and the real GDP and the adjusting forces were given by money stock, the short-term interest rate and inflation rate. The results were subsequently published in Juselius (1998a) and - based on an extended sample - subject to very detailed discussions in Juselius (2006).

Altogether these results were even more puzzling than before and I tried desperately to make sense of them. One of my hypotheses was that inflation in Denmark had been more affected by the actions of the Bundesbank than of the Danish National Bank. As Denmark is a small open economy and Germany is a strong and dominant neighbor, the idea did not seem too far-fetched. Juselius (1996) investigates this idea by analyzing monetary transmission mechanisms based on German data. The results were quite interesting: First, parameter constancy tests suggested a fundamental structural break around 1983, so the sample had to be split in two. In the first period, the results were in accordance with my prior: a plausible monetary policy rule was identified and inflation was significantly adjusting to it. In the second part, the same policy rule was found but inflation was no longer significantly adjusting.

This was the first time I obtained results showing that macroeconomic transmission mechanisms might have changed around mid-eighties. To learn more,

[3] For example under long-run price homogeneity, nominal money, m income, y, and prices, p, can be transformed to real money, $m-p$, real income, $y-p$, and inflation, Δp. All variables are in logs.

I begun to study monetary transmission mechanisms more systematically. In Juselius (1998) I compared the Danish and German results with similar analysis of Spain and Italy and concluded that monetary transmission mechanisms had indeed changed, probably as a consequence of financial deregulation and increased globalization. The comparison was followed by three more detailed country analyses: Juselius (1998a) discusses the Danish case, Juselius (2001) the Italian case, and Juselius and Toro (2005) discusses the effect for Spain of joining the EMS.

After all these attempts to estimate monetary transmission mechanisms combined with similar analyses by my students, I became increasingly sceptical of the theoretical basis for monetary inflation. Instead of (CPI) inflation always and everywhere being a monetary problem, the results indicated almost the opposite that inflation was never and nowhere a monetary problem.[4]

I decided to study the international transmission mechanisms to find out to what extent inflation was primarily imported.

6.2 Is inflation imported? Analyses of the international transmission mechanisms

My applications in this sector of the economy were motivated by the two theoretical cornerstones of international macroeconomics: the purchasing power parity (PPP) and the uncovered interest rate parity (UIP). The PPP condition (i.e. the deviations from PPP) was assumed to hold as a stationary or at most as a near $I(1)$ process, whereas the UIP condition was assumed to be a market clearing condition. The empirical support for these theoretical assumptions was generally weak. The deviations from both the PPP and the UIP conditions exhibited a pronounced persistence that was empirically indistinguishable from a first - or sometimes even second - order nonstationary process, whereas a combination of the two was often found to be stationary. This was precisely what early work on a monetary model for exchange rate determination based on imperfect knowledge expectations assumed would be the case (subsequently published in Frydman and Goldberg, 2007, 2011). As a consequence, it became the beginning of a long collaboration between Roman and Michael on one hand and the econometrics group in Copenhagen on the other.

The PPP and UIP results for Denmark versus Germany were published in the Journal of Econometrics (Juselius, 1995) and subsequently - based on an extended information set and an extended sample period - in Juselius (2006, Chapter 21). During my work on the PPP - UIP problem, it dawned on me that the CVAR model with its informationally rich pulling and pushing structures contained an enormous potential for combining deductive and inductive inference. In this vein, Juselius (1995) reports a large number of tests, not just of the stationarity of the PPP, the UIP and the combined relation, but of basically every possible hypothesis related to the foreign transmission mechanisms.

[4] Many years later I revised my thinking on this: inflation is in fact a monetary problem, but after deregulation of capital movements, it s asset price inflation and house price inflation and not goods price inflation that strongly reacts to excess liquidity.

This detailed analysis offered a wealth of new information, again some of it quite puzzling. For example, the trace test found the data vector to be $I(2)$ and tests of unit vectors in β found prices and the exchange rate to be individually $I(2)$. The test of overall long-run proportionality of the two prices was accepted, whereas proportionality between relative prices and the nominal exchange rate was clearly rejected. It suggested that the determination of prices may have behaved according to theory, whereas not the nominal exchange rate. To shed light on this puzzle I checked the estimates of the stochastic $I(2)$ trend, $\alpha_{\perp 2}$, and its loadings, $\beta_{\perp 2}$. The former showed that the $I(2)$ trend was primarily generated from the twice cumulated shocks to the long-term German bond rate and the latter that the $I(2)$ trend loaded onto both the two prices and the exchange rate. That the stochastic $I(2)$ trend originated from shocks to the German bond rate and that the trend loaded into the nominal exchange rate pointed to the financial market as a crucial player in the foreign exchange market. That the German bond rate was a dominant exogenous force behind Danish prices, indicated also that the latter had been strongly affected by German conditions.

In 1996, Søren and I moved to the European University Institute in Florence, Italy, for five years. Ronald McDonald was also visiting for a period. We initiated a joint collaboration of PPP and UIP for USA-Germany and USA-Japan now based on monthly data. Because we also included the short-term interest rates in the analysis, it allowed us to address the expectation's hypothesis and the term structure of interest rates in addition to the PPP and the UIP conditions. The problem was that the system became very large - seven equations - and, therefore, more difficult to handle. The solution was to first analyze a smaller five-dimensional model - consisting of prices, the long-term interest rates and the nominal exchange rate - and then to use the cointegration results of the smaller model as the starting point for the big model. This procedure - dubbed specific-to-general in the choice of the information set - relies on the invariance of cointegration to expansions of the information set. Since then I have successfully used this principle as a means to manage long-run identification in high-dimensional systems.

Like Juselius (1995), overall long-run proportionality between relative prices and the nominal exchange rate was strongly rejected. But unlike Juselius (1995), we applied the nominal-to-real transformation nonetheless and performed the analysis in the $I(1)$ model framework, acknowledging the loss of some data information.[5] The obtained results were similar to, albeit richer than, the ones reported for Danish-German case. Among others they showed that the adjustment of inflation to the PPP relation was utterly slow with a tiny adjustment coefficient, whereas it was much faster to the combined PPP-UIP relation. Inflation rates were purely adjusting, hence inflationary shocks had no long-run impact on the system. The long-term bond rates were weakly as well as strongly exogenous, hence they were exclusively pushing the system. Interestingly, the real exchange rate was weakly exogenous in the small system but no longer so in

[5] Later Johansen et al. (2010) and Juselius (2017a, b) report the full analysis of the original data based on the I(2) model.

the big system. Thus, statistically significant adjustment of the real exchange rate required a long-run relation in which the short rates were included. It illustrated the peril of the *ceteris paribus* clause for conclusions when data are non-stationary. The results of the two analyses are published in Juselius and McDonald (2004) and (2006).

Many of the above results were puzzling from the point of view of standard theory: inflationary shocks had not affected nominal interest rates whereas interest rate shocks had been pushing the inflation rates, albeit in a cost push manner. The long-term interest rates were exogenous to the system rather than the short rates and the short-long interest spread was nonstationary against the expectations's hypothesis. Today, I find the results to be completely plausible as they are basically consistent with the empirical implications of the theory of Imperfect Knowledge Economics (Frydman and Goldberg, 2007, 2011) as worked out in Juselius (2017a).

In summary, the results showed that international transmission mechanisms are important for understanding movements in domestic prices. They also showed that movements in interest rates do affect prices but in a cost-push fashion. This led me to the investigate the impact of wage costs for cost-push inflation in this period.

6.3 CPI inflation and excessive wage claims

My first study of wage, price, and unemployment dynamics was based on Danish data and is described in Juselius (2006, Chapter 20). The choice of variables was motivated by theoretical models of centralized wage bargaining assuming that the bargaining power of the unions is negatively affected by unemployment. Hence, a proposed pay rise by the labor union would reflect a trade-off between a higher consumption wage against lower employment. Whether the employers' union accepts the pay rise is assumed to be a trade-off between future profits and firm competitiveness against the increased risk of a union strike. Both unions are generally assumed to strive to maximize their share of future productivity increases.

During the sample period (1975:1-2003:1) the European markets became increasingly integrated which on one hand implied improved profit possibilities but, on the other, also stronger competition. For Danish enterprises, facing relatively high wage costs, the latter was potentially a serious problem. The almost fixed krona/DMark rate after 1983 meant that a less competitive Danish enterprise could no longer count on exchange rate realignments to improve its competitiveness. To stay in the market such an enterprise basically had three possibilities: (i) to reduce employment until the marginal cost equalled the competitive price, (ii) to increase labor productivity, or (iii) to outsource production. All three measures were used and all of them affected the unemployment rate.

As a result, unemployment moved in long and persistent swings around long-run average values not just in Denmark but in most European countries. These long and persistent unemployment episodes were puzzling from the point of view of standard theories that assumed unemployment rates to vary in a

stationary way from a constant rate, the natural rate of unemployment. This inspired Edmund Phelps to write the theory of "Structural Slumps" published in 1994 where he argues that - in a customer market economy - the natural rate of unemployment is likely to depend on the real interest rate and/or the real exchange rate.

These considerations motivated my choice of data - manufacturing wages, consumer prices, producer prices, productivity, unemployment, the long-term bond rate and the real exchange rate - altogether seven variables. Because a seven-dimensional VAR system is challenging to analyze, I used the specific-to-general approach to manage the complexity of identifying a plausible long-run structure. In the first step, I analyzed the first five of the above listed variables and, in the second step, I added the interest rate and the real exchange rate. This allowed me to study the effect of the *ceteris paribus* assumption "real interest rate and real exchange rate constant" on wage determination. It also helped me to get an idea of how globalization and financial deregulation had affected the mechanisms of the labor market and, at the same time, to test some of the fundamental hypotheses of Phelps' structural slumps theory.

The results showed that the nominal wage and the two price variables were individually $I(2)$ and that overall long-run homogeneity among them was statistically acceptable. Hence, by using the nominal-to-real transformation, the model could be analyzed in the $I(1)$ framework without loss of information. Thus, the nominal variables were replaced by the real consumer wage, the price wedge between consumer and producer wages, and consumer price inflation, all of them $I(1)$. While the price wedge transformation was econometrically motivated, it is also an important theoretical variable because the estimated coefficient of the price wedge can be seen as a measure of the relative bargaining power of employers and employees. Also, even more importantly, the price wedge is likely to be affected by the degree of *product market competition* which - if high - is likely to generate *pricing-to-market* behavior (Krugman, 1986, 1993).

The empirical results of the Danish wage and price mechanisms are given a detailed discussion in Juselius (2006, Chapter 20). One important finding - revealed by the tests of parameter constancy - was a significant change in the mechanisms around mid-eighties. It was a fundamental change - similar to the German monetary mechanisms in 1983 - that left me with no other options than to split the sample period in two. The first part comprised the seventies up to mid-eighties, the other mid-eighties until 2003.

The narrative of the first regime was about strong labor unions, rigid institutions, devaluations and realignments, whereas the one of the second regime was about increasingly weak labor unions, improvements of labor productivity by laying off the least productive part of the labor force. Excessive wage claims seemed to have caused both price inflation and unemployment in the first regime but foremost unemployment in the second. Competitiveness was largely achieved by producing the same output with less labor as evidenced by unemployment and trend-adjusted productivity moving together in the second regime. While there was evidence of a Phillips curve relationship in both regimes, it was rather small and insignificant in the first, but strong and signif-

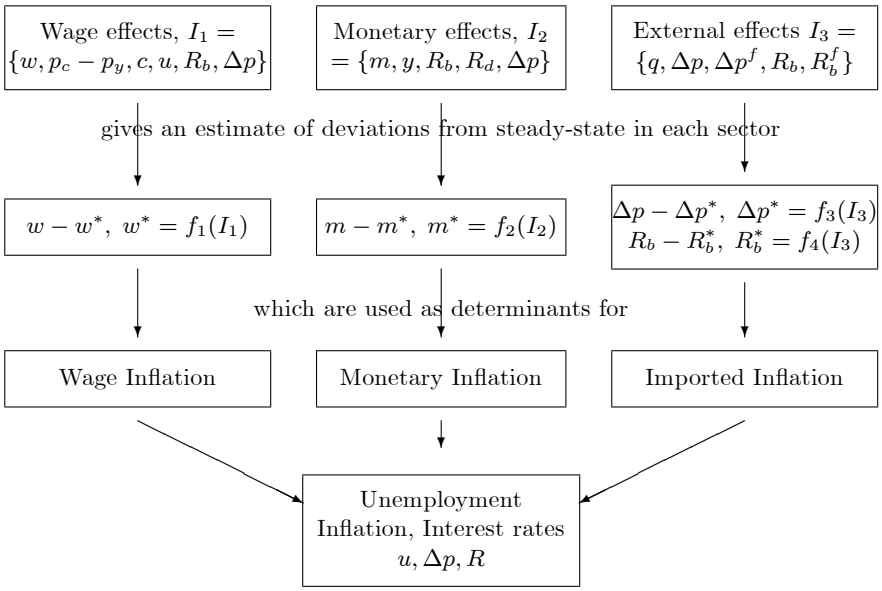
icant in the second. In both regimes inflation was significantly affected by the real exchange rate consistent with the results in the previous section. In the second regime, unemployment and the real bond rate were co-moving consistent with a Phelpsian natural rate. As in previous analyses, inflation and the bond rate were not found to be cointegrated.

While, I found the results exciting, I was also intrigued by them. The question was whether they had any generality outside Denmark. At this time, Javier Ordonez visited our department and we decided to study the wage and price dynamics for Spain using a similar approach. The Spanish results published as Juselius and Ordonez (2009) showed that the basic mechanisms behind the determination of wage, price and unemployment were very similar to the Danish ones, but there were also differences that seemed to reflect institutional differences between the two countries. In addition, support for the above mechanisms have also been found in various still unpublished papers and student works.

7 Combining the results: a proposal for a large scale macro model

The CVAR model is based on the "general-to-specific" approach, i.e. starting from a general statistical model - often highly overparametrized. - and then simplifying the model by imposing more and more (testable) restrictions on the model parameters. The advantage of this approach is that data are allowed to speak freely - no prior theoretical restriction are imposed from the outset - about long-run and short-run structures in the data. The disadvantage is that the number of parameters increases substantially with each included variable. Adding one variable leads to $(2p+1)k$ new parameters, where p is the dimension of the variable vector and k is the autoregressive lag. This can quickly become prohibitive in macroeconomic models, where sample periods seldom are very long.

To circumvent this problem, I proposed a procedure illustrated in the diagram below where economically relevant cointegration relations were first identified based on a subset of smaller VAR models and then combined into a bigger model. The procedure relied on the invariance of the cointegration property to expansions of the information set. If cointegration is found in a smaller model it would also be found in an extended model. It was also based on the assumption that an economically identified cointegration relation - i.e. a deviation from a long-run equilibrium value - could be treated as a summary measure of the most important information from that sector. For example, if wages are on the equilibrium level, then the value of the cointegration relation would be approximately zero and there would be no pressure on the rest of the economy from this sector. But, if there are excess wages - i.e. the absolute value of the cointegration relation is large - then the sector could potentially have a crucial impact on the rest of the economy.



In Juselius (1992), later reprinted as Juselius (1994) I applied this idea to study how CPI inflation was affected by monetary inflation, wage inflation and imported inflation measured by cointegration relations in three partial VAR models. I also used the idea in Part VI of my cointegration book in which I reported much more detailed and extensive analyses of the three sectors (Juselius, 2006: Chapters 19-22). The results of the combined model in Chapter 22 show that monetary inflation had basically had no effect on Danish CPI inflation. The decline of the Danish CPI inflation - starting with capital deregulation in the mid-eighties - was primarily explained by small, but significant, wage increases that only accounted for a small part of the period's productivity growth. Globalization and financial deregulation seemed to have contributed to increasingly weak labor unions. Altogether, the approach seemed to have produced empirically richer and more realistic estimates of the mechanisms governing price, wage and unemployment dynamics.

I was quite excited about the idea of using the "specific-to-general" in the choice of information and the "general-to-specific" in the search for a parsimoniously parametrized model. I was also excited about the potential of combining the cointegration relations from partial models into a big model explaining how key variables have responded to imbalances in important sectors in the economy. I thought this approach would have the potential to give the Keynesian type of macro models - consisting of a large set of behavioral relations where endogeneity, exogeneity and *ceteris paribus* are assumed a priori - a much needed face lift. The variables defining a single behavioral relationship would be subject to a partial cointegrated VAR analysis without the need to make prior assumptions on their endogeneity and exogeneity status. In such smaller VAR systems, the

stationarity of the presumed behavioral relations could be tested and the parameters estimated. But, even more importantly, information about the dynamic transmission effects in each of the sub-sectors of the economy would be readily available.

By combining these partial dynamic models into a much bigger model of the economy one would obtain something resembling a general (dis)equilibrium model. It would be based on the assumption that deviations from equilibrium values - the equilibrium errors - are the most crucial determinants of key variables in the economy, for example output growth, unemployment, wage inflation, interest rate, CPI inflation, house price inflation, stock price inflation, real exchange rate.

This, in my view, would be a powerful way of gaining empirically relevant understanding of our complicated economic reality.

8 Persistent equilibrium errors and financial market behavior

After having applied the CVAR to numerous empirical problems, it became evident that there was more persistence in the data than standard models could explain. I often found data to be indistinguishable from $I(2)$ - not just price variables, like the CPI, but also relative prices, nominal and real exchange rates, even real and nominal interest rates - all of which one would *a priori* expect to be at most be $I(1)$. Even unemployment, another important real economy variable, was often found to be indistinguishable from $I(2)$ and cointegrated with real interest rate and real exchange rate.

Many economists would argue that such findings are implausible as economic variables could not drift away forever the way a true $I(2)$ process can, nor could equilibrium errors be $I(1)$ since economic variables do not move infinitely away from their equilibrium values. But, while this is obviously correct, it does not exclude the possibility that variables over finite samples may exhibit a persistence that is *empirically* indistinguishable from a unit root or a double unit root process. Besides, as economic relationships seldom remain unchanged for very long periods of time, the infinity argument may not be very relevant. Hence, while economic variables/relations are seldom true unit root processes it is, nonetheless, useful to classify them as either stationary, *near* $I(1)$ or *near* $I(2)$.

What makes a near $I(2)$ process extremely interesting is that such a process is able to generate long-lasting swings (Johansen, 1997, 2006a, Paruolo and Rahbek, 1999). In spite of this, applications of the $I(2)$ model are rare in the literature. To understand why, Juselius (2014) discusses a simple case, $\Delta x_t = \omega_t + \varepsilon_{x,t}$ where $\omega_t = \omega_{t-1} + \varepsilon_{\omega,t}$ and the shocks, $\varepsilon_{\omega,t}$, are small compared to the shocks, $\varepsilon_{x,t}$, i.e. the signal-to-noise ratio is small. Simulations show that univariate D-F tests hardly ever detect the second unit root in the drift term, whereas the multivariate tests almost always find it. This is particularly

so when the signal-to-noise-ratio is small - typically the case for asset prices in speculative markets - and a finite-order VAR model is an accurate approximation to the underlying unobserved components model. As most people use univariate rather than multivariate tests to determine the order of integration, the results can explain why so few econometricians actually apply $I(2)$ models.

Why is this important? Knowing the correct order of integration and cointegration among variables is a very important and useful piece of information that can be used to classify the data into more homogeneous groups. For example, an $I(1)$ variable cannot be significantly related to an $I(0)$ variable, neither can an $I(2)$ variable to an $I(1)$ variable, but they can be combined to form a stationary cointegrated relationship. Hence, by exploiting the information in the data given by the integration/cointegration properties of the variables, one can obtain robust estimates of long-run, medium-run and short-run structures in the data, thus improving the specification of the economic model. In the words of Hoover et al. (2008), the CVAR allows the data to speak freely about the mechanisms that have generated them. For a more detailed discussion, see also Juselius (2006, 2013).

At that time, financial behavior was rarely included in macroeconomic models since - somewhat simplistically - a fully rational financial actor was assumed to know whether - and how much - the market price deviated from its equilibrium price and would act accordingly. Rational financial markets would, therefore, drive financial prices back to equilibrium and the equilibrium prices would correctly reflect movements in the real economy. Because financial prices were assumed to be correct, deregulated financial markets would not be harmful to the real economy. Hence, there was no reason to explicitly include the behavior of the financial market in macroeconomic models. The reasoning relied on the efficient market hypothesis, that again relied on the "rational expectations' hypothesis" and the assumption that economic models are known and stable over time. But, all these assumptions seemed at odds with what I constantly saw in the data: the frequent structural breaks, the frequent changes of exogeneity status, the long and persistent swings around equilibrium values indistinguishable from a unit root process establishing itself as tiny but significant α adjustment coefficients.

That the deviations from some of the fundamental economic parities - the Fisher parity, the term spread, the purchasing power parity, the uncovered interest rate parity - were statistically indistinguishable from unit root processes seemed particularly worrisome to me. Where did this additional persistence come from? It seemed inconsistent with standard REH models that assumed much faster adjustment to long-run equilibrium values. Why did the persistent swings not vanish with the nominal-to-real transformation when the nominal deflator was the consumer price index? It gradually dawned on me that the long and persistent swings in real transforms - e.g. real exchange rates - were often associated with financial variables such as nominal exchange rates, interest rates, stock prices, house prices, energy prices, prices for precious metals.

This empirically very strong feature - combined with small signal-to-noise ratios - was consistent with the basic theory of imperfect knowledge economics

(Frydman and Goldberg, 2007, 2011) and inspired me to focus on the role of financial markets for the real economy. The basic idea of their theory is that no one can know - even in probabilistic terms - the true fundamental value of a financial asset (as it is based on future cash flows). Given this Knightian uncertainty, market participants interpret in diverse ways a wide range of news about fundamental factors in forecasting future asset prices (from GDP and inflation rate announcements to political developments and debt crises). Consequently, persistent movements in fund variables can often lead to persistent movements in market participants' exchange rate forecasts in one direction, either away from or back towards benchmark values. This connection between fundamentals and forecasts, therefore, lead to persistent swings that revolve around PPP. In the context of a monetary model of the exchange rate, such behavior leads to persistent fluctuations (near $I(2)$) in the real exchange rate and real interest rates (Frydman et al., 2012).

Another strain was offered by Hommes (2005) and Hommes *et al.* (2005a, 2005b) in which they explain the persistent swings with a financial market populated by fundamentalists using economic fundamentals to forecast future price movements, and by chartists - trend-followers - using technical trading rules to forecast prices. Agents switch endogenously between mean-reverting fundamentalists and trend-following chartists depending on how far away the price is from long-run equilibrium values. Positive feedback prevails when the chartists dominate the market.

Common to the above models is that today's asset price depends on future prices which, in varying degree, are being forecasted under imperfect knowledge and, therefore, deviates from the price derived under the REH. In both models prices can deviate from long-run benchmark values for extended periods of time generating self-reinforcing expectational cycles.

The econometric analysis of such self-reinforcing expectational cycles is, however, far from straightforward. In particular, the issue of how to reconcile such behavior with the persistent fluctuations of the PPP and the UIP in a *constant parameter* CVAR model was crucial to me. Inspired by Frydman and Goldberg (2007, 2011), Juselius and Assenmacher (2017) interpreted the long swings in the real exchange rate in the context of a simple data-generating model with *time-varying* coefficients based on the following assumptions: A financial actor understands that PPP holds in the long run, but that this is not necessarily the case in the short run. Therefore, he/she is likely to react on a number of other determinants, z_t, for example, changes in interest rates, relative incomes and consumption, and many more. In such a world financial actors tend to attach time-varying weights, B_t, to relative prices depending on how far away the nominal exchange rate is from its fundamental PPP value, i.e.,

$$s_t = A + B_t(p_{d,t} - p_{f,t}) + z_t. \tag{2}$$

where s_t is the log of the nominal exchange rate, $p_{d,t} - p_{f,t}$ is the log of relative price between domestic and foreign country, and B_t will fluctuate around 1.0.

The change in the nominal exchange rate can then be expressed as:

$$\Delta s_t = B_t \Delta(p_{d,t} - p_{f,t}) + \Delta B_t(p_{d,t} - p_{f,t}) + \Delta z_t.$$

Frydman and Goldberg (2007) make the assumption that $|\Delta B_t(p_{d,t} - p_{f,t})| \ll |B_t \Delta(p_{d,t} - p_{f,t})|$. This is backed up by simulations showing that a change in ΔB_t has to be implausibly large for $\Delta B_t(p_{d,t} - p_{f,t})$ to have a noticeable effect on Δs_t so that

$$\Delta s_t \simeq B_t \Delta(p_{d,t} - p_{f,t}) + \Delta z_t. \tag{3}$$

To study the properties of this type of time-varying parameter model, Tabor (2017) considers the CVAR model:

$$\begin{aligned} \Delta Y_t &= \alpha(Y_{t-1} - \beta_t X_{t-1}) + \varepsilon_{y,t} \\ \Delta X_t &= \varepsilon_{x,t}. \end{aligned} \tag{4}$$

He generates the data with $\alpha = -1$ and $\beta_t = \beta_0 + \rho\beta_{t-1} + \varepsilon_{\beta,t}$, so that $E(\beta_t) = \frac{\beta_0}{1-\rho} = \beta$ for $\rho = \{0.0, 0.5, 0, 95, 1.0\}$. $\alpha = -1$ implies that the adjustment of Y_t back to $\beta_t' X_t$ is immediate. Instead of estimating a time-varying parameter model, Tabor fits a *constant* parameter CVAR model to the simulated data, so that $(\beta_t - \beta)X_t$ becomes part of the CVAR residual. The simulation results show that the closer ρ is to 1, the more persistent is the estimated gap term, $Y_t - \hat{\beta}' X_t$, and the smaller is the estimated adjustment coefficient α (while still highly significant). As long as $\rho < 1$, the mean of the estimated $\hat{\beta}$ approximately equals its true value β.

Thus, the pronounced persistence away from long-run equilibrium values and the small adjustment coefficients often found in constant-parameter CVAR models can potentially be a result of time-varying coefficients due to forecasting under imperfect knowledge. Juselius (2017b) shows that this may explain the persistence of the PPP gap and the inability to reject $I(2)$ persistence in a constant parameter CVAR model. While in this case the $I(2)$ model is just an approximation to a model with time-varying coefficients, it is likely to be a highly useful approximation. This is because the linear VAR representation gives access to a vast econometric literature on estimation and testing, whereas the complexity of estimating a time-varying parameter VAR model would be daunting except for in very small models.

When analyzing the PPP and UIP conditions for various countries based on near $I(2)$ CVAR models, the results frequently showed that the domestic – foreign interest rate spread was cointegrated with the deviations from the PPP (i.e. the real exchange rate). Since this empirical regularity was one of the main predictions from the IKE-based models, Roman Frydman, Michael Goldberg, Søren and myself started a collaboration where we addressed the PPP puzzle and the long swings puzzle theoretically as well as empirically (Frydman et al. 2008, 2013).

As already mentioned, Juselius (2009b) showed that the assumption in REH models of stationarity of the PPP parity was not valid and that stationarity

required a combination of the PPP and the UIP parity. In a follow-up paper, Johansen et al. (2010) reported a full econometric analysis of all the international parity conditions using German - US data. Also Juselius and Assenmacher (2017) report a similar study based on Swiss-US data. The latter paper also discusses equilibrium error-increasing and error-correcting adjustment behavior as a way of identifying the channels through which self-reinforcing feedback mechanisms work. The results showed that trend-following behavior plays a significant role for exchange rates, interest rates and prices. Also, by interpreting persistent movements in the real exchange rates as a proxy for the uncertainty premium in the foreign exchange market - as proposed by Frydman and Goldberg (2007) - the results provided strong empirical support for uncertainty adjusted UIP being stationary, i.e. once loss-aversion and uncertainty is allowed for. Thus, much of the excess return puzzle disappeared when an uncertainty premium in the foreign exchange market was added to the model.

The above papers focusing on financial behavior convinced me that financial behavior was potentially extremely important for the real economy.

9 Financial market behavior and persistent cycles in the real economy

By allowing for imperfect knowledge and uncertainty as major determinants of agents behavior, many puzzling empirical results started to make sense again. In a world of imperfect knowledge, agents are behaving rationally but the outcomes are very different from the ones in an REH world. This prompted the question whether and how the real economy is affected by the non-stationarity of the above parity conditions.

My paper from 2013, "Imperfect Knowledge, Asset Price Swings and Structural Slumps: A Cointegrated VAR Analysis of Their Interdependence", in (eds.) E. Phelps and R. Frydman, *Rethinking Expectations: The Way Forward for Macroeconomics* was a first attempt to explain a two-way interdependence between the real economy and financial behavior in the foreign currency market. The nominal exchange rate is particularly important in this context as it is foremost determined by market expectations and much less by trade in exports and imports. This is because the transactions in the foreign currency market that are associated with financial speculation are dominant. When the exchange rate is fluctuating in long persistent swings around its fundamental value, export firms have to use 'pricing-to-market' rather than constant 'mark-up pricing' unless they are prepared to lose market shares (Krugman, 1986). For example, over a prolonged period of currency appreciation a business firm will experience a mounting pressure to remain price competitive. As raising the price is not feasible, there are few other options than to improve productivity. This can be achieved, for example, by requiring that workers produce more per hour, by firing the least productive workers, by outsourcing, by introducing new technology (robots) and, to some extent, by adjusting profit. When the exchange rate

finally reverses - now depreciating - the pressure on competitiveness is released but, because companies in competing countries now are experiencing an appreciating exchange rate and act accordingly, prices do not rise much. Thus, in our globalized economies, consumer prices are kept low and stable because of fierce competition and because nominal exchange rates adjust very slowly back to equilibrium, whereas asset prices - being determined by future expectations - tend to fluctuate in long persistent swings.

The findings that unemployment and trend-adjusted productivity have been co-moving and that the natural rate of unemployment has been a function of the real interest rate - rather than a constant - are consistent with the above mechanisms (Juselius, 2006, Chapter 20, Juselius and Ordonez, 2005). They also explain why the CPI index has not exhibited the same persistent swings as asset prices and, consequently, why *real* asset prices are empirically almost indistinguishable from their nominal magnitudes.

That equilibrium in the goods market is not directly associated with purchasing power parity but with a stationary relation between a nonstationary real exchange rate and the interest rate spread implies that the real exchange rate can persistently appreciate/depreciate as long as the domestic interest rate increases/decreases more than the corresponding foreign rate. Since these persistent swings around equilibrium values are caused by speculative behavior in the market for foreign exchange, they are essentially outside domestic policy control. Juselius and Stillwagon (2018) investigated whether interest rate expectations by professional forecasters might be behind the long persistent swings characterizing foreign currency market for the US dollar and the UK pound. The results gave fairly strong support to the hypothesis that it is the interest rate expectations, measured as consensus forecasts by professional forecasters, that have been pushing the interest rates and the exchange rate in the long run. They also showed that it is primarily the shocks to the US consensus forecast - rather than the UK ones - that are behind the long persistent swings in UK and US interest rates as well as the dollar/pound rate. Finally, the results showed that changes in the nominal exchange rate have been pushing the foreign currency market in the medium run - consistent with behavioral models of extrapolative expectations - while interest rates have followed suit. By contrast, the nominal exchange rate was found to be equilibrium-error-correcting in the long run, while interest rate expectations were pushing. This autonomous role for interest rate expectations is congruent with models emphasizing imperfect knowledge.

The above mechanisms can also explain the inflation puzzle, i.e. why it has been low and stable over time - below 2% for several decades - at the same time as the nominal interest rate has moved in long persistent swings. They can also explain why cointegration between CPI inflation and nominal interest rate is almost always rejected - against the Fisher parity - while cointegration between inflation and the short-long interest rate spread - both near $I(1)$ - is often not rejected. As the spread can be considered a proxy for inflationary expectations, the finding means - perhaps not so surprisingly - that inflation and inflationary expectations have been co-moving. Furthermore, the above

papers often find that inflation has been positively affected by an increase in the short-long spread - with a small coefficient - consistent with the results in Section 6 that CPI inflation in Western economies has primarily been affected by cost push rather than demand pull factors over the last three decades.

Monetary policy is mostly based on the assumption that central banks can control CPI inflation by controlling the short-term interest rate. To efficiently do so would among others require that the above parities hold as stationary conditions. When they do not, an important part of the standard transmission mechanism is missing. I have seen little evidence that the short-term interest rate is an efficient instrument for CPI inflation control, albeit acknowledging that inflation rate has been low in periods of inflation targeting. However, my claim - backed up by the above empirical results - is that it has been so for other reasons - primarily global competition. While Central Bank interest rate control is likely to be important for real growth and employment, the inflation in this period would probably have been low independently of the changes in central bank interest rates.

The long period of low inflationary pressure has implied little need on the part of the central banks (foremost the ECB and the US Federal Reserve Bank) to raise the central bank interest rate which has been at exceptionally low levels for decades. While this has resulted in increased liquidity - and, hence, a strong growth of credit financed consumption and excess aggregate demand - CPI inflation has, nonetheless, remained low. However, exceptionally low levels of short-term interest rates are likely to increase the demand for houses and equity as well as their prices. The soaring house and stock prices from mid-nineties until the bubble burst in 2008 are evidence of this effect.

To summarize: The persistently low CPI inflation rates are likely to be associated with persistent imbalances in the real exchange rate. Low inflation rates tend to put a downward pressure on central bank interest rates, which tend to increase credit financed consumption and financial speculation. The latter are likely to generate unsustainable high debt as well as house price and asset price inflation. While, accruing imbalances may counterbalance each other to some extent, a balance that is maintained by several imbalances is a very fragile balance. A large shock somewhere in the system, is sufficient for the whole thing to collapse — as demonstrated in 2008 when the financial crisis hit the world economy with unprecedented force.

Thus, the great recession seems to have grown out of many imbalances allowed to develop over a long time. This was also the conclusion in Colander et al. (2008) that discussed the role of financial models and their effect on real economy. The paper was produced during a one week long intense meeting in 2007 in Dahlem, Germany. Soon after the first version of the paper appeared, the financial crisis hit the world economy.

10 Modelling crises periods: using the CVAR as a design of experiment

While many economists claimed that the Great Recession was a once in a life time event - a black swan - that could not have been foreseen, I vividly remembered a similar crisis at the beginning of the nineties in Finland. The deregulation of the Finnish credit market in 1986 had resulted in an over-heated economy and in strongly increasing real estate prices. When the house price bubble burst, unemployment rates soared and reached approximately 20% - from a starting position of 1.6% - in a very short period of time. In a joint project with my son Mikael Juselius we addressed the questions: (i) whether the Finnish experience could be understood as a balance sheet recession[6], (ii) whether the unemployment dynamics made sense in the context of Phelps' Structural Slumps theory (Phelps, 1994), and (iii) whether the theory of Imperfect Knowledge Economics (2007, 2011) could explain the persistent movements in the data. To answer these questions, we applied the CVAR model to inflation, unemployment, a short-term and a long-term interest rate.

Econometrically, our CVAR model performed surprisingly well - considering the wild fluctuations of the Finnish data. The results - reported in Juselius and Juselius (2013) - gave support to all of three priors: the Phelps' hypothesis that the natural rate of unemployment is a function of the real interest rate; the Frydman and Goldberg Imperfect Knowledge hypothesis of pronounced persistence in the long-term real interest rate; and the Koo hypothesis of the Central Bank interest rate as an impotent instrument during a balance sheet recession. Furthermore, based on a smooth transition model in which the transition variable was designed to capture household sector leverage adjusted for movements in the value of the housing collateral, the paper demonstrated how the strongly increasing house prices had played a crucial role for the depth and the length of the subsequent crisis. As soon as house prices started falling and the housing debt exceed the value of the collateral, the leverage effect was shown to become extremely important.

The Finnish results seemed to be able to shed light on how inflation, unemployment and interest rates are determined in a crisis period. Therefore, the questions whether the historical data for Finland - possibly also for Japan - could have been used to foresee the Great Recession and whether there were lessons to be learnt from the Finnish experience of unemployment dynamics begun to seem increasingly important. It motivated Juselius and Dimelis (2018) to address the mechanisms behind the Greek depression, the most serious and destructive of all the European crises. Many aspects of the Finnish crisis were similar to the ones in Greece: the deregulation of the Finnish credit market in 1986 resulted in a booming housing market and a serious house price bubble; joining the eurozone caused the Greek bond rate to drop to previously unprecedented levels and caused a credit financed boom in aggregate demand. As in

[6] Motivated by the collaps of the Japanese real estate bubble a few years after the Finnish crisis, Richard Koo (2010) published his first book on balance sheet recessions .

Finland, Greek wages and prices - in particular real estate prices - were rising and competitiveness was deteriorating. When the Greek bubble burst, the drop in aggregate income and the rise in unemployment were huge and of similar magnitudes as in Finland. But the Greek crisis, while similar in many respects to the Finnish one, differs strongly in others. For example, the source of the debt (private/public, external/internal), the strong/weak institutional set-up, and in particular the exchange rate regime are defining differences of crucial importance. The fact that Finland was able to devalue its currency while Greece was not is likely to have made all the difference for the length of the crisis. It is one reason why the comparison with Finland is interesting.

Unlike the Greek economy, Finland managed to get out of the crisis in approximately three - admittedly very hard - years by devaluing the Finnish markka by 33%. Also, unlike the Greek experience, the Finnish unemployment came down quite fast though stabilizing at a somewhat higher level compared to the pre-crisis period. One reason why the Greek unemployment was stuck at very high levels was the prolonged period of policy uncertainty following the outbreak of the crisis. Unlike the Finnish analysis, the Greek analysis therefore required a variable measuring confidence as well as two variables measuring the development of the Greek competiviness within and outside the eurozone.

In the Greek analysis, the most striking result was a critical relationship between the bond rate and the unemployment rate: As the crisis erupted, the bond rate increased strongly followed by a strong increase in unemployment, the increase in unemployment rate caused the bond rate to increase further and unemployment to follow suite, and so on. This vicious cycle was orchestrated by a continuous fall in the confidence rate that kept deteriorating until relative producer costs stopped increasing around 2012. The empirical results showed that all variables, except CPI inflation, exhibited error-increasing behavior somewhere in the system. This feature is likely to have aggravated the problems and effectively prevented good policy solutions. As the euro rate was determined by factors mainly outside the Greek control, Greece was stuck in a situation with no feasible options: a dramatic lowering of wage costs was politically impossible; leaving the euro would have been extremely costly due to the large proportion of external debt. At the same time the confidence in the Greek economy continued to drop which by itself added to the depressed state of the economy.

The two papers illustrate an important methodological principle: by using the same design, i.e. the general CVAR model, and controlling for institutional differences by conditioning on appropriately selected variables, one can learn about similarities and dissimilarities in different economies. This is particularly valuable when addressing policy changes and the response to them.

Juselius et al. (2014) followed a similar principle when studying the effectiveness of foreign aid in 36 South Saharan African countries. Among these we were able to classify 29 countries into four more homogeneous groups regarding aid effectiveness[7] and to perform more detailed analyses within the groups. The

[7] The division into groups depended on whether foreign aid and the macro-economy -

results showed that, while the overall *qualitative* conclusions were rather similar for the vast majority of South Saharan African countries, they were quite different regarding the dynamic transmission of aid onto the macro economy. This may not be very surprising as such as aid is often given for different purposes in different countries. But our results suggest that one should be cautious to use panel data analyses as a basis for policy advice in South Saharan African countries. As aid effectiveness has frequently been studied based on panel data analyses which - implicitly or explicitly - assume homogeneous countries across the panel, this is a reason for concern. A few of the countries were difficult to classify, among them Ghana and Tanzania. Based on an extended data set, Juselius et al. (2017) studied the transmission mechanisms of aid in more detail for these two countries.

The above papers illustrate the great potential of the CVAR as a design of experiment for data obtained by passive observations discussed in Hoover and Juselius (2015) and Juselius (2015). As a matter of fact, it might be time to challenge the frequent claim that one cannot apply designed experiments in macroeconomics.

11 Some reflections

The title of this paper "Searching for a theory that fits the data" was chosen to emphasize the distinction between my own empirical approach and the one that underpins most empirical research in economics: "Searching for a data that fits the theory". No doubt, the difference reflects what is considered most important by the scholar, the empirical reality or the theory that is supposed to explain it. For me, the choice was easy: to understand more of the empirical reality was the main reason why, in the first case, I chose a university career in economics. To develop an empirical methodology based on the CVAR that potentially could improve economic policy decisions has been an important driver in all these years of extremely hard work.

While numerous published papers report all kinds of VAR analyses, most of them give the impression of being done by statistical non-experts: data have been read in and the VAR button has been pushed. But, a correct CVAR analysis has nothing to do with pressing the VAR button. It is not a method that can be applied mechanically, it depends upon the researcher's judgement and expertise and requires interaction between the analyst and the data. For example, it does not make sense to work with a VAR model until you have checked whether (1) the sample period is representative for your research questions, (2) the chosen information set is sufficiently broad to answer the questions of interest, (3) the most important institutional changes have been controlled for, (4)

measured by GDP, investment, private consumption and government expenditure - (1) had been unrelated in the long run; (2) whether aid had no long-run effect on the macro-economy - tested as a unit vector in α - but the latter had been influencing aid; (3) whether aid has been exogenous with respect to the macro economy and finally; (4) whether aid and the macro-economy have been tied together in an interdependent relationship.

the parameters of interest are reasonably stable over time, (5) the residual misspecification tests are acceptable, just to mention some of the important steps. If you sidestep them, you will very likely get nonsense.

One common claim is that CVAR models are so general that they can show anything.[8] A similar claim is 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 theoretical model, one opens up for the possibility of quackery. I hold the opposite view. Scientific objectivity can only be achieved provided data are not constrained from the outset in a theoretically pre-specified direction. In the latter case, it is impossible to know which results are due to the assumptions made and which are true empirical findings. This point was amply illustrated by Juselius and Franchi (2007). In this paper we checked the assumptions underlying a DSGE model by Ireland (2004) and found that essentially all of them lacked empirical support in the data. When a well-specified CVAR was fitted to the data the results showed that all conclusions - about a real business cycle model - were reversed. Thus, the conclusions of the Ireland paper reflected the assumptions made rather than true empirical findings.

Another frequent claim is that the quality and the informational content of macroeconomic data are too low. I agree that economic time series data seldom correspond to the concepts of a theoretical model. For example, the representative agent's income, consumption, and hours worked in a DSGE model has little in common with the various measurements of aggregate income, private consumption, and total hours worked that can be found in the publications of the Statistical Office. While, admittedly, macro data are contaminated with measurement errors, such errors may not be of great concern for the more important long-run analysis, unless they are systematic and cumulate to a nonstationary process. Whatever the case, 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 and we better understand them, however imperfect they are. Besides, thirty years of empirical modelling have convinced me that macroeconomic data are surprisingly informative, but only if you let them tell the story they want to tell.

Thus, I believe the CVAR approach has great potential as a scientifically sound empirical methodology but only if data are allowed to speak as freely as possible about empirical regularities. This, of course, does not mean that data should speak by themselves without theory as this would not lead anywhere, nor can data speak without rigor: A statistically adequate VAR analysis should obey equally strict rules as a mathematical analysis of an economic model and should satisfactorily describe all aspects of the data. Consequently, an empirically relevant theory should be able to explain all the dominant features of the data

[8] This, in my view, is a sure proof that the person in question has never performed a proper CVAR analysis. Hundreds of summer school students in the Copenhagen summer schools, who have struggled to make a well-specified CVAR deliver results in accordance with their favorite economic model - often without success - would certainly nodd in agreement.

revealed by a well-specified CVAR analysis.

Such features are typically unit root nonstationarity, structural change, non-constant parameters, dynamic long-run equilibrium relationships, self-reinforcing feedback mechanisms, all of them with strong implications for the theoretical model. For example, $I(2)$ nonstationarity is consistent with static equilibrium relations that deviate persistently - in a near $I(1)$ manner - from their long-run equilibrium values. This is often consistent with complex adjustment dynamics, dynamic long-run relations, and a nonstandard - non REH - expectations formation. Data covering crises periods typically reveal this kind of features. While many economists would consider crisis periods to be aberrations - black swans - outside the range of economic modelling, I disagree. As demonstrated in Section 9, crisis periods are not outside the range of serious CVAR analyses and important lessons affecting ordinary people's lives can be learnt from them. Therefore, I am convinced that the complexity of our economic reality must be taken more seriously also by the theorists. Otherwise, many theoretical models in economics will run the risk of illustrating incorrect beliefs and fail to predict, explain and prevent the next economic crisis.

My research over the past three decades shows that the results are quite different and contradictory to those found in standard models and totally different from what is published in many high ranking journals. From the outset my empirical findings almost always rejected Neoclassical or New Keynesian models but were more consistent with older Keynesian macro models. However, the pronounced persistence away from equilibrium values was difficult to reconcile with any of the two. But by allowing for uncertainty, loss aversion and imperfect knowledge in the formation of agents' expectations, the persistent swings started to make sense again. At this stage, my best guess for an empirically relevant theory in macroeconomics would be Keynesian macroeconomics with a fully incorporated financial sector and with expectations based on uncertainty and imperfect knowledge. That my early empirical findings over the first two decades - while then totally puzzling - would no longer be puzzling in this framework should contribute to the credibility of my guess.

I hope that this overview paper has helped econometricians, economists and non-economists to see that economic policy is likely to be improved if based on empirically relevant information rather than theoretical convictions. My greatest disappointment over these years is the resistance with which most of the economics profession has approached the empirically based CVAR methodology, perhaps because the CVAR analysis doesn't confirm their convictions. Whatever the case, economists on the whole do not share my conviction that theoretically puzzling - but empirically and econometrically well founded results - signal the need for new theory and, therefore, deserves to be taken seriously. There is little doubt that empirically unfounded economic policy is likely to have exacerbated some of the defining problems of our time, such as recurring crises, increasing inequality, growing populism, etc.

On a more optimistic note, I would also like to mention the numerous students that have taken our regular courses in Copenhagen, participated in our summer schools and Ph.D. courses around the world. Their enthusiasm and

willingness to work extremely hard to learn more about empirically relevant economic mechanisms, give hope.

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