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

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1 Introduction

This survey paper is based on my retirement lecture 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: which were the questions I wished to answer? how did I go about aswering them? stones I stumbled on? and the most important one: did I achieve my goals?

In my early formative years David Hendry and Clive Granger were both very influential for my thinking. I read almost everything they published and found a lot of inspiration from the "general-to-specific" error-correction approach developed by David and the numerous time-series issues offered 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 very intrigued by the concept of cointegration and how it related to error-correction and 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 in theory 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 goldmine of relevant problems to be solved.

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 worked out 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. While I presented the estimates, 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 model. In spite of this, to be right on the research frontier was a hilarious feeling.

Maximum likelihood cointegration immediately received a lot of attention and was subject to immense interest. Fortunately it turned out that the cointegration trace test was the only nonstandard distribution. After the rank was found, the non-stationary data were transformed to stationarity using differencing and cointegration. Hence, standard statistical theory applied in 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 cointegration analysis to a variety of important 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 it was therefore an excellent data set for the illustrative purpose at hand. To develop ideas and questions from a data set where things actually work is much easier than to

work with more challenging data sets, 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 where 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. We successfully obtained funding to organize 2-3 annual workshops starting in 1989. In the first place the funding was for three years, 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 young Ph.D. students. But the 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 the scholarly discussions of cointegration.

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 subset of (1) defined by reduced rank restrictions on Π and the I(2) model by further imposing restrictions on $\Gamma = I - \Gamma_1 - ... - \Gamma_k$. In the first years we did not fully understand the role of all the different components in the model and richness it offered in terms of addressing interesting economic questions.

As mentioned the first defining moment was when Søren solved the reduced rank problems and we were able to address problems in the I(1) world. But after having restricted Π to $\alpha\beta'$, the model was still 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 and we were able to formally answer these questions.

Søren's productivity in this period was remarkable. This is evidenced by Johansen (1989) and subsequently by Johansen and Juselius (1990) in which the mathematical results were applied to monetary transmission mechanisms in Denmark and Finland. The working paper versions appeared already in 1986, showing that in just a few years 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, the 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 it was the first realistic application of a CVAR model to macroeconomic data. This probably 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 which was therefore shown to be a submodel of the more general CVAR model. Even though 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 and

[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.

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, the paper Juselius (1995) turned out to be a very influential for two different reasons, one econometric the other economic. It represents an early work on international parity conditions and combines the purchasing power parity and the uncovered interest rate parity. The data vector contains 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, to subsequently be replaced by the ML procedure. Thus, the I(2) modelling approach 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.

But Juselius (1995) was also very influential for a different reason. The empirical results clearly showed that the PPP needed the UIP to become stationary. This was against the REH approach that assumed stationarity for each of them, but consistent with early work on Imperfect Knowledge Expectations by Roman Frydman and Michael Goldberg. As a consequence, this became the beginning of a long collaboration between Roman and Michael on one hand and the econometrics group in Copenhagen on the other.

The mathematical results needed for the probability/statistical analysis 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 time-series. In 1999 the Energy Journal asked David Hendry and me wether we

could explain the concepts 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. But, the CVAR methodology extended into new directions 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 func-

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

tions, 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 had 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 economists would be able to go back to their theoretical model and properly test the 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, hence, that the economic conclusions using that assumption were not robust, I thought it would be an important signal to the decision maker. Using this framework, allows you to do sensitivity analyses - seeing how the answer might change if the economic model is modified in an empirically more relevant direction. The approach is Popperian in this sense that the fundamental principle is 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.

While I had not expected the empirical results to perfectly support standard theory I never expected them to deviate so much and the empirically relevant conclusions to 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 in terms of 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 more intuitive than based on a formal mathematical definition. 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 was desperately needed. The paper Juselius (1993) was my first attempt to discuss this dichotomy in terms of a monetary problem, but it did not yet offer a bridging principle.

The *ceteris paribus* assumption was another issue I was concerned about. In a theoretical model the assumption allows you to keep certain variables fixed and then focus on those of specific interest. In an empirical model you have to bring these *ceteris paribus* variables into the analysis by conditioning. 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 make myself adopting the REH as an empirical modelling device. This was partly because I considered the underlying assumptions defining so called rational economic behavior to be too restrictive and 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 (???).

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 begun 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 the theory-consistent expectations' formation. See Juselius (2017a and 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 at the expense of empirical relevance.[3] 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 standard neoclassical model assumptions, they nonetheless seemed to make perfectly sense in terms of more old-fashioned Keynesian type of macromodels. Since the macro variables are aggregates of millions of totally different micro units, from the outset I thought it was highly surprising to find many plausible relationships in the data. To my relief, I came across a paper by Clive (Granger, ???) that proposed a plausible explanation for why this was the case. The paper, Juselius and Beyer (2009), was an econometric attempt to study how sensitive outcomes are 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 in particular 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

[3] Bohr: one has to simplify as much as possible but no more.

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. To develop a bridging principle exclusively for the long-term part of the model, started to look more and more appealing. 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 for inflation (Fridman, 1970 and Rohmer, 1996). It was the perfect place to present this idea which was subsequently labelled a *theory-consistent CVAR scenario*. Kevin Hoover was my official opponent 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 hypotheses about the purchasing power parity (PPP) and the uncovered interest rate parity (UIP) to be tested on the cointegration structure. To my own surprise the results were neither straightforward, nor trivial. But, due to other demanding commitments, it took roughly 10 years until the ideas were worked through and published as a chapter of the Handbook of Econometrics (Juselius, 2009). Given the integration properties of the data, the paper demonstrated the impossibility of a stationary PPP, a result that was in accordance with the theory of imperfect knowledge economics (Frydman et al. 200? and 2011?). Subsequent applied papers demonstrated that the PPP needs the UIP to become stationary (Johansen et al., 2010, Juselius, 2017a,b, Juselius and Assenmacher, 2017, Juselius and Stillwagon, 2018).

Over the next many years I continued to develop the principles behind a theory-consistent CVAR scenario. The main task was to develop 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 hy-

potheses was supposed to describe a set of testable 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 Rohmer'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 (200?) 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. Over this period I supervised numerous students and their empirical results were almost without exception equally disappointing regarding the basic assumptions underlying their chosen economic theories. It became increasingly obvious to me that a change in the research paradigm for empirical macroeconomics was very much needed. At this background I wrote Juselius (2011, 2012).

5 Haavelmo's probability approach and the CVAR

In all this time, 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 not flawed. In the second case, valid inference on the structural parameters of economic models is far from granted. 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, valid experiments in macroeconomics are unfortunately not an option. 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 introduced 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 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 much more elaborate discussion of the ideas initiated in Juselius (1993). In this paper, using the concept of "a theory-consistent CVAR scenario" I demonstrated that 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 Fridman's claim that "inflation is always and everywhere a monetary problem". The consequence of this was that money stock control should be used for inflation control. 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 of 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 to excess money, implying that real money stock was purely adjusting and 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 problem?

One problem with Johansen and Juselius (1990) was that inflation rate was not part of the VAR system. This was because at that time we were not yet aware of the econometrics of the nominal-to-real transformation implying that inflation rate should have been included in the data vector to prevent the

loss of some, potentially important, information.[4] Thus, the possibility that the puzzling results were due to the missing inflation rate in the VAR system had to be checked. 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 was however not a very surprising. But the remaining results were 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 shocks to the long-term bond rate and the real GDP were the exogenous forces of the system and money stock, the short-term interest rate and inflation rate were adjusting. 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 the Danish National Bank. As Denmark is a small open economy and Germany is a strong and dominant neighbor, it seemed worth looking into. This was done in the paper Juselius (1996) in which a CVAR analysis based on German monetary data is discussed. The results were quite interesting: First of all, parameter constancy tests suggested a fundamental structural break around 1983, so I had to split the sample in two. In the first part, 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, I started 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

[4]For example,nominal money, m^n income, y^n, and prices, p, transformed to real money, $m^n - p$, and real income, $y^n - p$, with all variables in logs.

three more detailed country analyses: Juselius (1998a) discusses the Danish case, Juselius (2001) the Italian case, and Juselius and Toro (2005) discusses the effect of joining the EMS for Spain.

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.[5]

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 a near I(1) process, whereas the UIP condition was assumed to be a market clearing condition. Empirically, the conditions did not seem to provide support for these theoretical assumptions. The deviations from both the PPP and the UIP were found to exhibit 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. As already mentioned, this was precisely what a monetary model for exchange rate determination based on imperfect knowledge expectations assumed would be the case. See Frydman and Goldberg (2007, 2011).

The PPP and UIP results for Denmark versus Germany were published in Journal of Econometrics (Juselius, 1995) and subsequently - based on an extended information set and an extended sample period - in Juselius (2006, Chapter 21). While working with 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

[5] 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 monetary policy.

inference. The paper Juselius (1995) therefore 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. 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 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 sector as a crucial determinant of the foreign exchange market. That the German bond rate was a dominant exogenous force behind Danish prices, indicated also that they were strongly affected by German conditions.

In 1996, Søren and I moved to the European University Institute in Italy for five years. Ronald McDonald was also visiting for a period and thought the findings in Juselius (1995) were interesting enough to deserve a closer scrutiny. We started a project now based on monthly data for USA-Germany and USA-Japan where also the short-term interest rates were included as system variables. The advantage was that 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 disadvantage 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 data information.[6] 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 in the big system, hence statistically significant adjustment of the real exchange rate required the short rates to be included in the long-run relation. 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 think the results completely plausible. They are essentially consistent with 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 price movements. They also showed that movements in interest rates do affect prices but in a cost-push fashion. This led me to the investigate of how much excessive wage costs additionally had meant for cost-push inflation in this period.

6.3 CPI inflation and excessive wages

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

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

assuming that the bargaining power of the unions is negatively affected by unemployment. Thus, 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 whichon one hand meant improved profit possibilities but, on the other, also stronger competition. For Danish enterprises, facing relatively high wage costs, the latter could be a serious problem. The almost fixed krona/DMark rate after 1983 - a consequence of financial deregulation and the increasingly free capital movements - meant that an uncompetitive 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 stionary 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. As 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. The estimated coefficient of the price wedge is a measure of the relative bargaining power of employers and employees. Also, the price wedge is assumed to be affected by the degree of *product market competition* which - if high - is likely to generate *pricing-to-market* behavior (Krugman, 19??).

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.

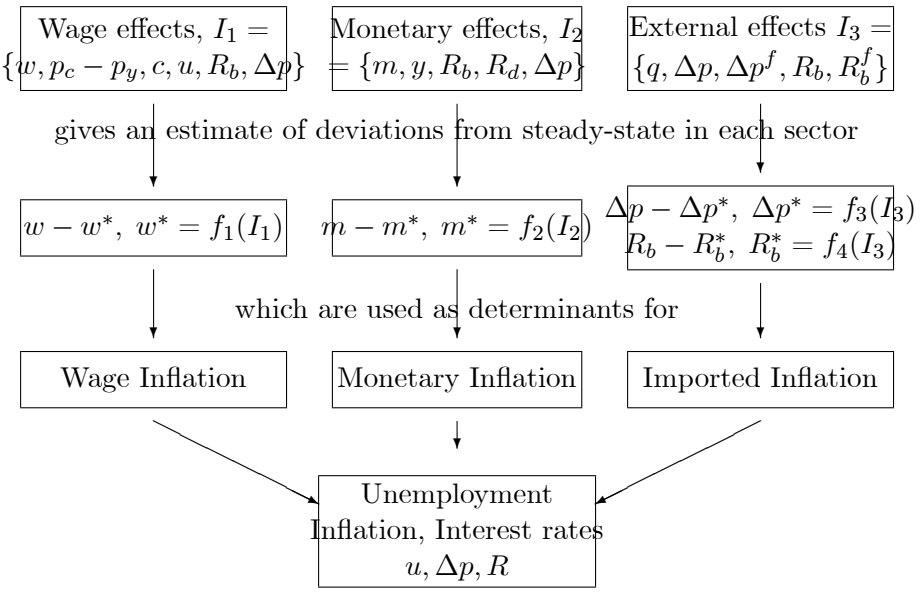
The narrative of the first regime was about strong labor unions, rigid institutions, devaluations and realignments, whereas the one of the second regime was about increasingly weak labor unions, improvements of labor productivity by laying off the least productive part of the labor force. Excessive wage 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 significant 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 also that some of the results reflected institutional differences between the two countries. A variety of still unpublished papers and student works have also found support for the above mechanisms.

6.4 Combining the results

The CVAR model is based on the "general-to-specific" approach, i.e. starting from a general and in most cases overparametrized. statistical description of the mechanisms in the data 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 above diagram 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 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 would potentially have a crucial impact on the rest of the economy.

In the paper Juselius (1992), later reprinted in Juselius (19..) 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, while wage inflation was the dominant cause. The decline of the Danish CPI inflation - starting with capital deregulations in the mid-eighties - was primarily explained by small, but significant, wage increases that only covered a small part of the productivity growth. The cost of globalization and financial deregulation seemed to have been increasingly weak labor unions.

I was excited about the results: the method seems to have produced the empirically richer and more realistic estimates of the mechanisms governing price, wage and unemployment dynamics. Using the "specific-to-general" in the choice of information combined with "general-to-specific" in the final modelling seemed to work well. While this idea, at this stage, does not seem to have caught the interest by policy makers, I believe the approach might potentially be able to give the Cowles Commission type of macro models a

much needed face lift.[7] The variables defining a single behavioral relationship - among numerous other ones - would be subject to an individual 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 are testable and ML estimates of the parameters can be obtained. 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, such as output growth, unemployment, wage inflation, interest rate, CPI inflation, house price inflation, stock price inflation, real exchange rate and many, many more.

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

7 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 the 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 for ever as 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

[7] They consist of a large set of behavioral relations - in which endogeneity, exogeneity and *ceteris paribus* are assumed - tied together in a big structure.

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 so relevant. Nonetheless, it is probably more appropriate to classify economic variables/relations 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, I discuss a case, $\Delta x_t = \omega_t + \varepsilon_{x,t}$ and $\omega_t = \omega_{t-1} + \varepsilon_{\omega,t}$, where the shocks, $\varepsilon_{\omega,t}$, are small compared to the shocks, $\varepsilon_{x,t}$, i.e. where the signal-to-noise ratio is small (Juselius, 2014). Simulations show that univariate D-F tests have serious difficulties detecting 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 - often the case with 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 as it facilitates a classification of 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 and, thus, improve 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, 2012).

The finding 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 ideas of imperfect knowledge economics and inspired me to focus on the role of financial markets for the real economy.

At that time, macroeconomic models rarely included financial behavior as - somewhat simplistically - a fully rational financial actor was assumed to know whether 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. So there was no reason to explicitly include the behavior of the financial market in the macroeconomic models. The reasoning relied on the efficient market hypothesis combined with 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.

Frydman and Goldberg (2007, 2011) offered a plausible answer to the frequent structural breaks and the slow adjustment. Based on an international monetary model for the PPP and UIP, they argued that the persistence of the PPP gap is likely to be the outcome of myopic and loss-averse financial agents who are forecasting in an imperfect knowledge world. Other answers have also been proposed. For example, Hommes (2005) and Hommes *et al.* (2005a, 2005b) 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.

An illustration based on the determination of nominal exchange rate explains the basic idea: If the dominant actors in the market consider the currency to be undervalued, then the majority of the market will act accordinly and the price of the currency will rise. These actors will be confirmed that their expectations were right and the price will continue to rise until the currency is clearly overvalued. The dominant actors in the market begins to fear heavy losses and requires a large risk premium for holding the currency. *Ceteris paribus,* this will cause a reversal of the movement of the exchange rate now towards the equilibrium price. The majority of market actors will expect a depreciation and act accordingly. The currency will continue to depreciate until it is clearly undervalued, after which a new swing starts. Although the picture is more blurred in reality, it reflects the logic of self-reinforcing imperfect knowledge 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 of the PPP and the UIP in a *constant parameter* CVAR model was crucial to me. Inspired by Frydman and Goldberg (2007, 2011) and Tabor (2017), I proposed to interpret the long swings in the real exchange rate in the context of a simple data-generating model with *time-varying* coefficients (Juselius and Assenmacher, 2017). Based on Tabor (20??) this helped me to understand the near I(2) properties typically found in analyses of the CVAR. The arguments are as follows:

A financial actor is assumed to know that, in the long run, the nominal exchange rate follows the relative price of the two countries but that, in the short and medium run, it reacts on a number of other determinants, z_t, which may include, for example, changes in interest rates, relative incomes and consumption, etc. Accordingly, financial actors 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. 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 frequently found in constant-parameter CVAR models can potentially be a result of time-varying coefficients due to forecasting under imperfect knowledge by heterogeneous actors. 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 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 long-term 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 got together producing two papers (Frydman et al. 2007, 2011) where we addressed the PPP puzzle and the long swings puzzle theoretically as well as empirically. But the market for a new economic theory was not yet ripe neither paper was accepted for publication in a top journal.

As already mentioned Juselius (2009) showed that it did not make sense to discuss the PPP parity without jointly including 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 detailed study of the long persistent swings in the real exchange rate between the Swiss franc and the US dollar. The paper showed that the $I(2)$ CVAR model was able to describe equilibrium error-increasing and error-correcting adjustment behavior and to identify the channels through which such feedback mechanisms work. The results showed that speculative trend-following behavior plays a significant role for the determination of exchange rates, interest rates and prices. Also, by interpreting persistent movements in the short-term interest rate differential as a proxy for the uncertainty premium in the foreign exchange market - proposed by Frydman and Goldberg (2007) - the results provided strong empirical support for UIP being stationary once an adjustment for uncertainty is allowed for. Thus, much of the excess return puzzle disappeared when accounting for an uncertainty premium in the foreign exchange market.

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

8 Financial market behavior and persistent cycles in the real economy

By introducing 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 clearly 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 the first attempt to explain this two-way interdependence between the real economy and financial behavior in the foreign currency exchange sector. The latter is of particular importance as the nominal exchange rate is foremost determined by financial market expectations and much less by trade in exports and imports.[8] 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. Somewhat simplistically pricing-to-market (Krugman, 19??) works as follows: Over a prolonged period of currency appreciation, say, firms will struggle to remain price competitive. In such a situation, raising the price is not feasible so 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 their 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.

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). Since, in a global world, consumer goods are subject to fierce competition, the mechanisms can explain why the CPI index has not exhibited the same persistent swings as asset prices and, consequently why *real* asset prices exhibit similar persistent swings as the nominal ones, i.e. why real exchange rates, real interest rates, and real house 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 nonstationary ppp and the interest rate spread implies that the real exchange rate can

[8]This is because a large part - sometimes close to 100 per cent - of transactions in foreign currency are associated with financial speculation.

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. In the paper Juselius and Stillwagon (2018) we investigated the role of interest rate expectations by professional forecasters for 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. The results also showed that it has been the shocks to the US consensus forecast - rather than the UK ones - that have significantly caused the long persistent swings of the dollar/pound market. Finally, over the medium run, the results showed that changes in the nominal exchange rate have been pushing the foreign currency market - consistent with behavioral models of extrapolative expectations - while interest rates have followed suit. By contrast, the nominal exchange rate has been equilibrium-error-correcting in the long-run while interest rate expectations have been 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 I have rarely found cointegration between CPI inflation and nominal interest rate - as the Fisher parity would suggest - while instead cointegration between inflation and the short-long interest rate spread - both near I(1). As the spread can be considered a measure of inflationary expectations, the finding means - perhaps not so surprising - that inflation and inflationary expectations have been co-moving. But, the results also show that inflation has been positively affected - though with a small coefficient - by an increase in the short-long spread, presumably a result of a monetary policy reaction. This is of course not the intended effect but consistent with the results in Section 6 that CPI inflation in Western economies over the last three decades has primarily been affected by cost push rather than demand pull factors.

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, even this 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 an exceptionally low level for several decades. While this resulted in increased liquidity and, hence, credit financed consumption and excess aggregate demand, CPI inflation has, nonetheless, remained low. But exceptionally low levels of short-term interest rates are prone to trigger increases in stocks and house prices. This is exactly what happended in the mid-nineties when these prices started soaring until the bubble burst in 2008.[9] Thus, the low central bank interest rates may primarily have given rise to house and stock price inflation. By deregulating capital markets politicians have to a large extent deposited the power to influence the domestic economy in the hands of the financial market.

To summarize the results of the previous section: A persistently low CPI inflation can be associated with persistent imbalances in the real exchange rate. A low inflation tends to keep interest rates at a low level, that tends to increase credit financed consumption and financial investment. The latter is likely to generate unsustainably high debt as well as house price and asset price inflation. To some extent, these imbalances may counterbalance each other, but 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 the paper Colander et al. (2008) that discuss the role of financial models and

[9]For an excellent CVAR analysis of the Danish housing market, see Hetland and Hetland (2017).

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.

9 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 overheated 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 question whether the Finnish experience could be understood as a balance sheet recession[10], whether the unemployment dynamics could be understood in the context of Phelps' Structural Slumps theory (Phelps, 1994), and whether the theory of Imperfect Knowledge Economics (2007, 2011) could be used to used to understand the persistent movements in the data. To address 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 our 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

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

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. It was therefore straightforward to ask 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.

These questions motivated Juselius and Dimelis (2018) to address the mechanisms behind the Greek depression, the most serious and destructive of all European crises. In many ways the underlying causes 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 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) 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 we therefore had to include 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 increasing 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 inherent crisis 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. All this time, the confidence in the Greek economy continued to drop which in 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.

A similar principle was followed in Juselius et al. (2014) in which the effectiveness of foreign aid was studied and compared for 36 South Saharan African countries, and in the follow-up, Juselius et al. (2017) in which the transmission mechanisms of foreign aid were studied in more detail for Ghana and Tanzania. In the first paper, we were able to classify 29 out of the 36 countries into four more homogeneous groups w.r.t aid effectiveness.[11] Within these groups we performed more detailed analyses of aid effectiveness. The results showed that, while the overall *qualitative* conclusions were rather similar for the vast majority of South Saharan African countries, they were quite different w.r.t. the dynamic transmission of aid onto the macro-variables. Considering that aid is often given for different purposes in different countries, this may not be very surprising as such. Econometrically, our results however imply that panel data studies cannot be used as a basis

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

for policy advice in individual 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, the above conclusion is important.

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

10 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 over the last decades and the one that characterizes most empirical research in economics: "Searching for a data that fits the theory". No doubt, the difference reflects what is considered most important, the empirical reality or the theory supposed to explain it. For me, the choice was never to be questioned: to understand more of the empirical reality was the main reason for why I chose a university career in economics in the first hand.

But, to say that the economics profession embraced my empirically based approach would certainly be an overstatement, possibly because it was technically difficult and fundamentally foreign to most economists, possibly because my empirical findings - as demonstrated above - often contradicted the standard way of reasoning. Thus, not many economists shared my conviction that puzzling but empirically and econometrically well founded results signal the need for new theory and therefore deserved to be taken seriously. Nevertheless, my urge to achieve a better understanding of our complex empirical reality was always stronger than the urge to give up. To develop an empirical methodology based on the CVAR that potentially could improve economic policy decisions has been an important goal in all these years of extremely hard work.

While numerous published papers report all kind of VAR model results, 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 absolutely nothing to do with pressing the VAR button. It is certainly not a method that can be applied routinely, 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 sufficient broad to answer the questions of interest, (3) the most important institutional changes have been controlled for, (4) the parameters of interest are reasonably stable over time, (4) 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.[12] 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 (??) 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 theoretical 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

[12] 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.

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 should data be allowed to 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 revealed by a well-specified CVAR analysis.

Such features are typically unit root and break nonstationarity, 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 tend to disagree. As demonstrated in Section 9, crisis periods are not outside the range of serious CVAR analyses and important lessons affecting ordinary people's live can be learnt from them. Therefore, the complexity of our economic reality must be taken more seriously also by the theorists, or many theoretical models in economics will run the risk of illustrating incorrect beliefs and fail to predict, explain and prevent the next economic crisis.

To summarize and conclude: what I found in the data by carefully structuring them in short-run and long-run components of the pulling and pushing forces was often quite different from what one finds in standard economic text books and totally different from what was 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 Keynesian macro models. However, the pronounced persistence away from equilibrium values was difficult reconcile with any of the two. But by allowing for uncertainty 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 the above framework should contribute to the credibility of my guess.

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