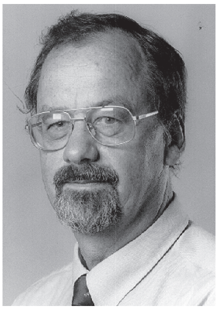
8. Søren Johansen and Katarina Juselius



This interview took place at Copenhagen University in October, 2005

How did you get into statistics and economics? Søren, let's start with you.

Søren: That is a long story. Both my parents were trained actuaries. When I finished high school in 1958, I knew that the actuarial study would be split into statistics and actuarial science. I decided to do statistics, which started with courses in mathematics, and later statistics. I became one of the first graduates of the program in 1964. So I guess it was basically the background of my family that made me aware of statistics and want to study it.

Katarina: My background was just the opposite; I was the first one to get a university degree in my family, they were farmers. As a child I was a 'reading horse'; at the age of 12 I had already read all the books in our local library, many of them two or three times. I just loved literature and after high school started studying literature and humanities. But I quickly realized it wasn't really for me; you had to read huge volumes on the history of literature and learn it by heart. I wasn't very good at memorizing and decided to study something more analytical, which was economics. One of the more influential professors in business administration had a strong influence on me and he encouraged me to continue my studies after my B.Sc. and M.Sc. exams.

I chose to specialize in econometrics because it had a logical structure

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that appealed to me. By understanding the logic, I didn't need to memorize the details; that was crucial to me. I visited London School of Economics (LSE) as a research student in the spring of 1979. This was clearly an eye opener for me. I realized there was a whole world outside the narrow circles of my own university, ~~where~~ scientific criteria were completely different from ~~what~~ I was used to. I was fascinated by this. In Helsinki I had studied statistics without any guidance and I could easily have stopped after my Licentiate degree without getting a PhD. I was not sufficiently focused, but London School of Economics provided me with the focus that I needed.

Who was there at that time?

Katarina: David Hendry had been there and left, but his spirit was still there. I read many of his papers and was excited about them. Andrew Harvey and Jim Durbin were there, and Rob Engle visited the LSE for a year. There was a lot of theoretical time series work going on, but I guess I felt most positive towards David Hendry's ideas, partly because they seemed to represent a more novel approach to do time series econometrics. It also appealed to me that his paper conveyed an urge to go behind the theoretical model and understand the mechanisms that had generated the data.

Let's go back to you, Søren.

Søren: As I said before, from 1958 to 1964, I developed a strong background in mathematics, probability and statistics.

You did very technical proofs, right?

Søren: Yes, you are quite right; I was very much a mathematician. Around that time, the Professor of Statistics was Anders Hald, who introduced R.A. Fisher's ideas of statistics in Denmark. The idea of creating a statistics curriculum in the 1960s was a new idea. It involved trying to combine the British interest in applied statistics with the formal mathematical training that we had. So throughout our studies, we had this combination of a genuine interest in applications with a genuine interest in model formulation and analysis of the model. I did a lot of work in mathematics. ~~I went to Berkeley after my studies for a year in 1965.~~ I remember I took a course with Loeve who was a professor of probability, and I was presenting some complicated work in Choquet theory. So I spent a lot of time doing mathematics. My primary interest was, however, in statistics and the analysis of statistical models. I pursued my interest in probability theory for ten years, and then started collaborating with medical doctors on modeling the liver and analyzing their data.

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What led to that collaboration? That is quite a shift from statistical and probability theory work.

Søren: We were always taking in applied work throughout my education and subsequent ~~teaching, so we~~ would contact people in biology and medicine and talk to them about the problems they were analyzing. We would then introduce the data and their statistical analysis in our teaching. Our ambition was that no practical exercise was ever based on simulated data; it was all real data. Sometimes ~~the real data was~~ chosen to fit what we were teaching in the course, but basically the data analysis we taught was always inspired by problems from the natural sciences.

One of the conacts was the medical doctor Susanne Keiding, and that turned into a long and very interesting collaboration. The medical doctors were doing research on the liver because they wanted to prepare for the day when Danish hospitals could transplant a liver. So they set up this ~~whole~~ system of experiments on pigs in order to see if they could make quantitative measurements of the function of the liver. I found that very interesting. Apart from the medical collaboration, I kept my interest in pure mathematics, so I was still publishing a lot in statistical and mathematical journals.

That takes me up through 1982 when I gave a lecture at the Nordic Statistics Meeting entitled "Some topics in regression". I used the medical data as an illustration. Katarina was there and she introduced me to statistical work done in economics; in particular she showed me Clive Granger's discussion paper (1983) on the duality between error correction and co-integration which contained the first analysis of the so-called Granger representation theorem. Katarina asked me to explain the mathematical and statistical problems surrounding Granger's work and it became clear to me that one could probably build a beautiful statistical theory on these ideas. I took it as a challenge and began working on the topic. This has continued now for 25 years. In the beginning, most of the work was about understanding the mathematics of the structures that Granger had suggested, not the statistics. That led to a completely incomprehensible paper that I wrote in 1985, entitled "On the mathematical structure of cointegration models", which was published some years later. More importantly, it led to the paper, "Statistical analysis of cointegration vectors" in the *Journal of Economic Dynamics and Control* (1988b).

Is that the paper you did for the Econometrics Society in Copenhagen that you submitted late and it was almost not accepted?

Søren: That's right; the Econometrics Society Meeting was in Copenhagen in 1987. In 1985, I had done the mathematical structure, and I had started thinking about how to do the statistics. I was, of course, used to working with analysis of variance and regression, but I'd never worked with time series before. However, it was pretty obvious that the mathematics of this reduced rank formulation was similar to an additive decomposition in the analysis of variance. I'd seen papers in *Biometrika* on multiplicative decompositions, which lead~~s~~ to eigenvalue solutions. So the whole idea of working with the multiplicative formulation of reduced rank was very natural. I was also familiar with multivariate analysis, and the multivariate normal distribution. But even though I knew a lot of what Ted Anderson had written, I did not know what is now called reduced rank regression. In fact that was pointed out to me by Helmut Lütkepohl at the meeting in Copenhagen.

Nonetheless, I managed to produce the formula for the maximum likelihood estimator and the asymptotic distribution of the rank test, just after the deadline for submitting papers to the meetings. But Timo Teräsvirta on the program committee was kind enough to accept it.

At the meeting, the introductory lecture on co-integration was given by Rob Engle, and he kindly included some of my results in his survey. So there was a lot of interest in the results, already from the very beginning. Many people found it exciting to work on inference for co-integration. Of course it happened at roughly the same time as Peter Phillips was working on his triangular system representation for estimating co-integration, and Ahn and Reinsel were actually working on exactly the same model. So many people at different places in the world came up with ~~the same type of~~ ideas at the same time. It often happens. So that takes us to about 1987.

The two of you met in 1982? Did you start collaborating immediately?
Søren: No, I think that we started collaborating in 1983–1984. It must have been around 1982 or 83 when Katarina asked me to present a paper on Clive's ideas. I wish I had that first copy of Clive's discussion paper because it was never published. But, one of the papers that really led me to understand the ~~formulations~~ was a paper by Mark Salmon. He has not worked on co-integration since then, but he had some mathematical ideas that were very useful for thinking about such ~~systems. How you~~ reduce the order of integration by differencing. It was a very nice paper.

Katarina: At the same time as Søren was developing the statistical theory, I decided to apply the co-integrated VAR to the ~~Danish Money Demand Relation~~. Because it turned out to be incredibly stable over time (probably one of the most stable relations ever seen in macroeconomics), it was an excellent data set for the illustrative purpose at hand. I can think of many, many other applications which would have driven me to despair, and probably never would have led to anything. To develop ideas and

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questions from a data set where things actually work is much easier than to work with some of these terrible data sets.

So, Katarina, were you teaching in Helsinki at that time?
Katarina: Yes, until 1985 when I moved to Copenhagen and started teaching there.

And Søren, by 1986 you had left your work on medicine and moved to economics?
Søren: Actually, that happened a couple of years before. Susanne Keiding moved to Aarhus and got a job where research was not her main responsibility, so our collaboration died out after a while. Her move coincided with my interests changing from one type of application to another.

What's the difference between the two applications?
Søren: Well the economic application involves time series analysis, which, initially, I really didn't know anything about. I had to learn it by myself. The medical application was basically (non-linear) regression of the velocity of the metabolism of sugar or alcohol as a function of concentration. The regressor was extremely accurately measured. The measurements were gathered under experimental conditions. The biological models for these metabolisms were only partly understood, and I actually ended up writing a paper on a mathematical extension of a theoretical biological model. Otherwise the group produced many papers, where I was the last author, ~~and~~ I only did the statistical appendix.
It was very interesting to work with medical people. First of all, they are extremely competent people and they really appreciated the collaboration. They were not afraid of asking questions or exposing their need for statistical help. They accepted that I was an expert, but at something else. That was a very positive attitude.

You have not found that as much in economics?
Søren: I found that my work with Katarina was immediately accepted by many econometricians, and I have had many interested and competent collaborators in the econometrics profession. I have not collaborated so much with theoretical economists, probably because my background is too different and their interest in analyzing data is not so pronounced.

You went to San Diego in 1985 and then to Baltimore. Who was there, and what role did those visits play in your thinking about co-integration?
Søren: In San Diego there were Clive Granger, Rob Engle and Hal White. They were the main people we interacted with. It was a wonderful place,

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and I went back for three months in 1989. It was at that time I wrote the two papers mentioned above. After San Diego Katarina and I wrote three additional papers together – one on the basic model where we worked out the rank tests for different deterministic terms as an extension of the statistical modeling paper, the next one on hypothesis testing for the co-integrating vectors and the third one on the identification of the long-run structure. This was a fruitful collaboration because the theory was developed in close contact with the applications. It is the reason why the results are being used a lot.

Katarina: A good illustration of how empirical analysis can guide the theoretical work is an early work on purchasing power parity, and uncovered interest rate parity. It was based on two price levels, exchange rates and two interest rates for Germany and Denmark. The results were very puzzling. The test suggested a rank of three, implying three stationary co-integration relationships. But the graphs of these relations clearly indicated they were highly non-stationary. However, the graphs of the cointegration relations when the short-run effects had been concentrated out were completely stationary. This suggested that the nominal variables were I(2), that co-integration in such a model is from I(2) to I(1), and finally that stationarity can be achieved by combining the I(1) co-integration relations with a linear combination of the differences. It also suggested a straightforward way of analyzing I(2) models using the two-step procedure. Later on the two-step procedure was replaced by the ML procedure. But, the whole I(2) approach was initiated by first looking at the empirical results, and then discussing why they looked so peculiar.

The approach you're following is used more in Europe than in the United States. Would you agree? And if so, why?
Katarina: I think it is correct in the sense that the number of people who press the J button when applying the co-integration method is much larger in the United States than in Europe. Having said this, there are generally too many papers applying our approach without really understanding the methodology and they also come from Europe.

Søren: That's a good question. There was a time some five, ten years ago where any decent econometric conference would have six or seven sessions on co-integration. That has died out. The novelty is gone.

Katarina: I believe this is a natural progression. In the beginning papers were accepted just because they had applied the novel co-integration approach. Now a paper is not accepted just because it is a co-integration analysis, it has to be something more.

Søren: I think there was a time when all the theoretical econometricians were working on topics related to this methodology and many were applying it. Now many econometricians work on other topics – panel data, financial econometrics, factor models, analysis of large data sets, and so on. But the people who make a living on analyzing the usual monthly or quarterly data sets – they routinely apply co-integration methods, and that is possible because the programs are there.

Katarina: Unfortunately, this is not a method that can be applied routinely using standard co-integration software; it requires interaction between the analyst and the data; it is a powerful tool for an expert to use, not a tool for someone who doesn't understand the methodology.

What do you mean, it is not a method that can be used routinely?
Katarina: A good co-integration analysis is when you structure the information in the data, so that the complexity of the empirical reality can be grasped and better understood. I strongly doubt that one will ever get a program that does such an analysis for you. Serious data analysis is a long process that requires a very systematic study; continually working with the data, trying different specifications until reaching the point where one can say: now I understand the basic features of the data (statistically as well as economically). ~~This is~~ a baseline model that it makes sense to continue working ~~with. You~~ have to carefully check for misspecifications – whether the sample period is correctly chosen, whether there have been intuitional changes that need to be corrected for, and so on. These steps are enormously important. If you sidestep them, believing that you just need to press the button and out come some useful results, you will get nonsense. Always!

Søren: In other words, the reason it is a useful tool is that it is based on the analysis of the model. Before you use the results from this particular model you have to check the assumptions behind it. This is not understood in the econometric literature. So there is now something called the Johansen Procedure, and it is completely misleading to believe it can be applied to data that are fractionally integrated or heteroskedastic, or whatever. The Johansen procedure consists of checking the assumptions and then once you know the model is reasonably OK, you go and apply it. It is not just pressing the J button – that is certainly completely inappropriate – but this is unfortunately how it has often been used. It may look like you are doing sophisticated econometric work, but what you are doing is probably close to worthless. My contribution to co-integration analysis was simply to analyze the maximum likelihood estimator and the likelihood ratio test in the Gaussian model. But before you use maximum likelihood, you have to be sure that you have the right model, otherwise the estimator and test do not have the optimal properties you think they have.

Most econometrics is still taught as methods – almost like ~~cookbooks~~ where you have recipes for method 1, method 2, and method 3. That's not the way Katarina and I approach the data. We first choose the method that fits the circumstances. It needs a lot more careful thinking than is usually associated with writing an applied paper in econometrics. Of course, this has nothing to do with co-integration, but it has everything to do with carefully applying statistical methods to data. With modern computers, it is getting easier to do, but it is also getting easier to do ~~the wrong way~~.

Now let us move to 1994 – 1995. Søren, that's the time you moved from Copenhagen to Florence.

Søren: In 1996, I took a position at the European University Institute in Florence.

How was that different?

Søren: It was like night and day. I was suddenly there representing mathematics and statistics. The students I got there – and I had many PhD students – were completely different from the students I had in Copenhagen, which were from a much smaller and more homogeneous group. ~~They~~ had five years of hard training in mathematics and statistics, and after that they would continue for three years as PhD students, often going to United States for a year. They would choose a new direction in which to go, and then follow it. Their dissertation was directed toward producing new theory. Of course, they may not have done everything in three years, but after having defended their thesis, they would continue to develop theory. Many of the statisticians I trained in Denmark have been very successful.

In Florence the idea was completely different. Students came to Florence with an interest in some economic phenomenon, and picked up whatever they needed to do a PhD. It sometimes looks as if ~~one can start doing~~ a PhD in economics if ~~one~~ has a good background in philosophy, mathematics or physics. I often felt that there was very little background knowledge I could rely on the students having. On the other hand, they were extremely engaged students – but supervision was quite different from what I was used to. It was, however, an interesting experience. I ended up being chairman of the economics department, which is something I'm very proud of.

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Do you see yourself as an economist?
Søren: No. I'm not an economist; I don't really understand economics.

What do you mean you don't understand it?
Søren: Because I don't understand the economic motivation for acting in real life and I do not understand how economists think when they model an economic problem. I have no tools to discuss whether something is good or bad economics. It is in that sense I am not an economist. I hope I have never given any other impression. I have, of course, been a co-author on papers which have economic content.

Katarina, do you see yourself as an economist or a statistician?
Katarina: There have been periods when I preferred not to be called an economist. I think it was because there was an enormous gap between the issues I thought were important and the ones my colleagues were discussing. What I found in the data by structuring them in short-run and long-run components was often quite different from what one finds in standard economic text books and totally different from what I saw in the most influential research papers. I became increasingly concerned about that, in particular as I found that many theoretical economists did not seem to care. I am quite convinced that if the economic reality had been more in accordance with standard theory models, the co-integrated VAR model would probably have been embraced with much enthusiasm. As the economic reality *is* very different from the empirical results we find in many high-ranking journals, I cannot help suspecting that these might have been obtained by torturing the data until they confessed. I believe this is what the present incentive system does to economics: it encourages scholars to maximize A and B journal publications, rather than to search for "the truth". The reluctance among economists to take the empirical reality seriously has convinced me that economics is too important to be left to the theorists alone.

What are the central results that differ from your statistical analysis from those in economics?
Katarina: I would first of all say that most theoretical models seem to make sense in a stationary world, but not necessarily in a non-stationary world. This is a very important point as most time-series data can be shown to be non-stationary. Another point is the *ceteris paribus* assumption which allows you to keep certain variables fixed in a theory model. In an empirical model you have to bring these *ceteris paribus* variables into the analysis by conditioning. If these *ceteris paribus* variables are stationary, the conclusions are more likely to remain robust, but if they are non-stationary,

the conclusions often change completely. The third point concerns expectations. From the early outset it became obvious that structuring the data using our approach would never provide anything that even vaguely resembled model-consistent rational expectations behavior, not even as an equilibrium condition. So I think these three elements are the central points of difference. Since most theory models contain all three elements, I often felt I had no ground to stand on. How can you go on doing anything when the most fundamental building bricks are not there? It was a difficult period until I gradually began to see how one should re-organize the information in the data so that they started to make economic sense again. Still, many economists would not accept that it makes sense, because it does not necessarily do so in a stationary world. However, I believe it does make sense in a non-stationary world.

But, admittedly, I would never have expected the empirical results to deviate so significantly from standard theory. In the beginning I thought our procedure would provide an efficient way of getting parameter estimates which had previously been difficult to estimate properly. I never expected that the results and the conclusions would be so different. Discovering that the empirical results often suggested that some very fundamental relationships, based on which most theory was relying, were lacking support in the data was a real shock. For a long time I simply didn't know what to do about it.

How would you say that the approach that you follow differs from that of Chris Sims, for example?
Katarina: Chris Sims, because he is a Bayesian, would not, for example, use likelihood inference on the orders of integration and co-integration of the data as a structuring device. We argue, for example, that the order of integration of a variable is extremely useful because one can tell from the outset that a non-stationary variable cannot be related to a stationary variable. In this case you need the non-stationary variables to be co-integrated before you can associate them with a stationary variable. I don't think Chris would accept that, by exploiting the information in the data given by the integration/co-integration properties of the variables, he could improve the specification of his economic model. Perhaps, one difference is that with the Bayesian analysis it is not easy to know when you are wrong, whereas with our approach the whole idea is to find out when, where and why you are wrong.

Could you expand on your criticism of the Bayesian approach?
Søren: Some people use Bayesian analysis in order to reach a compromise between a VAR model that is a good statistical description of the data,

and the theory model which is a bad description of the data. So they put a prior on the parameters of the VAR model which emphasizes the theory model. This is a compromise between theory and reality, which is very strange to me. One way of expressing my concern is that such an analysis can not tell ~~one~~ if the theoretical model needs modifications. There are lots of situations where Bayesian analysis is the reasonable thing to do, such as monitoring of patients in medicine, analyzing pedigrees, or constructing optimal sampling inspection plans. But when you use it as a cover-up for a discrepancy between model and data, then I can't go along.

What is a model?

Søren: You'd have to say what type of a model, for example, probability model, statistical model or economic model. I think the model is an important tool for formulating your understanding of the phenomenon you are talking about. There is a quotation by Mark Katz that I put in my book, which states that although models have to be used for forecasting, the real function of a model is that it allows you to pose sharp questions. But precise questions are not enough. You still need criteria for rejecting models. In the natural sciences the criteria for rejecting models are more clear-cut. You do experiments to bring the model to the empirical evidence.

What I'm missing in some of the macro work is the idea ~~that one should use~~ empirical evidence to ~~see if one needs~~ to modify the theory. That idea does not seem to be around a lot. The reason why, I suspect, is that it is extremely difficult to model economic phenomena.

So does your new book build economic models?

Katarina: No, it does not. I'm just trying to present a framework in which economists would be able to go back to their theory model and properly test their assumptions – bringing those assumptions to the data. If the outcome of the empirical testing is that a particular assumption isn't in the data and that the economic conclusions using that assumption are not robust, it is an important signal to the decision maker. Using this framework allows one to do sensitivity analyses – seeing how the answer might change if one modifies the economic model in an empirically more relevant direction.

One important example is that most theory models are based either explicitly or implicitly on the *ceteris paribus* assumption – real exchange rates, real interest rates, and interest rate spreads are all stationary. For example, such stationarity assumptions are consistent with essentially all Rational Expectations (RE) models. When you relax these assumptions, allowing for non-stationarity you get into a world of imperfect knowledge economics (IKE), where agents are behaving rationally but the outcome is very different from a world of model consistent RE economics. Based on numerous applications (small, large, open, closed economies) I have found that the stationarity of the parity assumptions does not hold – the persistent movements away from the parities are simply inconsistent with the RE assumption. The policy implication this has for our economies is serious and requires a complete rethinking of our policy models.

What is one of your robust findings?
Katarina: One of the most robust and interesting findings is that the deviations from some of the basic parities – the Fisher parity, the term spread, the purchasing power parity, the uncovered interest rate parity – exhibited a persistence that is empirically close to unit root behavior and clearly untenable with standard rational expectations' theories. Instead, we have found that the domestic–foreign long-term interest rate spread is co-integrated with the PPP (the real exchange rate). This finding is extremely robust; I have found it everywhere. It turned out that exactly this empirical regularity was one of the predictions from the IKE theory developed by Frydman and Goldberg. It led us to start working together and the results look extremely promising. Another robust finding is that inflation and the short-long interest rate spread – both approximately I(1) – are often co-integrated. If the latter is considered a measure of inflationary expectations then it says that inflation and inflationary expectations move together (which is not so surprising). A very robust finding is, however, that it is inflation that adjusts upward (though with a tiny coefficient) when the short-term interest rate increases more than the long-term (presumably as a result of a monetary policy reaction). This of course is not the intended effect and suggests that CPI inflation has primarily been influenced by aggregate supply (cost push pressure) rather than by aggregate demand (demand pull pressure).

What implication for policy do these findings have?
Katarina: The fact that equilibrium in the goods market is not necessarily associated with purchasing power parity but with a relation between PPP and the interest rate spread, in which both can be non-stationary, implies that the real exchange rate can persistently appreciate, say, as long as the domestic interest rate increases more than the corresponding foreign rate. These persistent movements are essentially due to speculative behavior in the market for foreign exchange – when agents' forecasts are based on imperfect knowledge, that is not knowing the right model, nor the right variables – and hence, are basically outside domestic control. So, one implication is that by deregulating capital markets politicians have to a large extent deposited the power to influence the domestic economy into the hands of the financial market.

Today's 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 though the inflation rate, admittedly, has been low in the period of inflation targeting. However, my claim (backed up by numerous empirical results) is that that it has been so for other reasons, primarily global competition, and would have been so independently of monetary policy interest rate changes. When this is said Central Bank interest rate control is definitely extremely important for real growth and employment in the domestic economy. One recent and still preliminary result is that the low interest rates we have seen over the last decades have increased excess liquidity, and this seems to have caused the recent increase in house price inflation and stock price inflation, while not CPI inflation.

What is the future of co-integrated VAR models over the next ten years?
Søren: The model has found its way into many textbooks, and it will stay as a useful methodology. You can then say that as long as these four, five or six macro variables need to be analyzed, it needs to be there. But there are lots of extensions now. One is to combine with volatility models; another is to combine with non-linear models, but the data requirements of these extensions are larger than in the usual macro models. I think that co-integration will be around as long as we are analyzing non-stationary time series.

Katarina: Though I agree with Søren, I have a somewhat different view of the future of co-integration. I believe that the future will see more applications of larger and more realistic VAR models at the expense of these very small, partial models which seldom can say something interesting about the economy. By gradually increasing the information analyzed by the VAR model it should be possible to obtain a more realistic picture of the sensitivity of the *ceteris paribus* assumptions for empirical conclusions. In the end I believe this is a powerful way of obtaining an empirically relevant understanding of our complicated economic reality. I also believe the VAR model will be further developed towards an expert system. These extensions are necessary for the models to become really useful.

Would these expanded models replace standard macro econometric models?
Katarina: To start with let's consider the big Cowles Commission type of macro models everyone likes to hate. They consist of a set of behavioral

relations, assuming endogeneity and exogeneity, tied together into a big structure. One could take these blocks of behavioral relationships and do a co-integrated VAR analysis of each of them. Based on such an analysis one can get Maximum Likelihood estimates of the parameters of the behavioral relations, possibly identifying new relations, but more importantly, one can also learn about the dynamics in this part of the economy – what are the pushing forces and what are the pulling forces. In the end, you can combine these partial dynamical models into a much bigger model resembling something that may come close to a more general long-run equilibrium model.

What are some of the challenges that you faced along the way of developing your approach?

Katrina: The first challenge came when realizing that most economic series were in fact non-stationary but the statistical theory available for analyzing them was developed for stationary processes. The question was whether it implied that all results based on stationary processes were more or less useless. Luckily, it turned out that the only non-standard distribution that had to be found was the one for the trace test which was needed to determine the co-integration rank. But after the rank was found, it was straightforward to transform your non-stationary data using differencing and co-integration and you were back in a stationary world again. It was a great relief to realize that standard statistical theory could be used again – that is one could test hypotheses using Student's t-tests, chi-squared tests, and F-tests.

Then the second challenge occurred when I started to analyze economic data more seriously. Nothing seemed to fit with what I had expected. Then it was the same type of doubt knocking on the door; what to do with the theory models after I had empirically rejected many underlying assumptions. This phase took much longer. I felt I was in no man's land. It is only in the last ten years that I have found a way to bring the old theories back. Now I can see that they are useful, provided they can be reformulated to fit the features I've learned from the data analysis. For example, the non-stationarity of the data implies that the model can contain a static long-run relationship, but it has to be combined with a dynamic adjustment relationship. But the most crucial thing was to get to know the theory behind Imperfect Knowledge Economics. I am convinced that this theory will eventually bring empirical evidence and theory together in a relevant framework.

Let's now talk a bit about the changes going on in the European economics profession. What is your view of these changes?

Katarina: I have been quite critical of the ongoing attempts to make European Economics a "carbon copy" (if I may use your illustrative analogy) of American Economics. In my view it has had the (unintended?) effect of streamlining the way we think, make research, and make policy, which I find extremely unfortunate. In many ways I believe many things have gone in the wrong direction over the last decades. We have all these fantastic electronic tools, fast computers, sophisticated software, the internet, etc. that should help us improve our research productivity. But it often seems to me that we are pressing the buttons and producing results at an ever increasing speed, without allowing for the fact that novel, genuine thinking is a painfully slow process for most of us. While previously European research was allowed to take time (admittedly this also led to waste) it has nowadays been replaced by the "publish or perish" culture with "quality" weighted publication counts adopted from the United States system. With this incentive system there is however an inherent risk that young PhD students, in particular, will choose quick and safe research topics, while more complex topics, of possibly high relevance for our society, might be considered too risky. With regard to my own students, I have to tell them from the outset that I am willing to guide them through an exciting, interesting, frustrating research process, that their empirical results are likely to challenge the standard view on macro and may suggest how to improve policy, but that the results will hardly be publishable in the top United States journals. Just to give an illustration: to help young scholars into a US top journal publication, I have suggested how to rewrite their paper to maximize the chance of getting past the "gate keepers" of the journal. This involves among others: not mentioning in the abstract, introduction and the conclusions any critical results that demonstrate flaws in previously published work of the journal; emphasizing any result that might be interpreted as evidence for the views held by the editors. This, in my view, is one of the unfortunate (and probably unintended) effects of the change in the European incentive system to mimic the US system.

Another less desirable effect is the streamlining of research. Some 10–15 years ago I found it exciting to participate in the Econometric Society Meetings because of the lively debates and the exchange of ideas. Nowadays I tend to find it almost a waste of time: you present your paper to a small audience of people who most likely agree with your ideas (otherwise they would not be there); you might go to one or two of the main invited lectures and that's it. But the debate and the excitement seem somehow to be gone. On the other hand, why should one get excited about yet another twist of a DSGE model when it is quite obvious that its basic assumptions would not pass any serious econometric tests, or about a new epsilon change in a representative agent, infinite horizon rational expectations model, or about yet another twist of an econometric test developed for a very special situation with little generality outside its narrow context? But many of these papers are likely to find their way to good journals.

Søren: I have certainly also seen the changes going on in Europe in the academic world, but I do not have enough knowledge about economics to discuss the consequences for the economics profession.

How could the changes be made so that they work more positively?
Katarina: Before suggesting possible changes it is important to discuss why there have been all these recent changes in the incentive structures of European economics. Few politicians would deny the importance of today's information society and, hence, of the university that provides the information. This, of course, explains why there is this strong political interest in how we manage our universities: whether we do enough research; whether the research is of sufficiently high quality and of relevance to the society. As an illustration, our Danish research minister uses the slogan "from innovation to invoice" to stress that our research should produce jobs! He is also the one who introduced quality weighted publications as a basis for university funding. As the A journals mostly consist of United States top journals, European economists, to be promoted or to get a job in the first case, are more or less forced to direct their research toward these journals. Since United States journals are not necessarily interested in specific European conditions, our economists have instead to engage in the United States economics discussion, which may not be highly relevant for Europe. The numerous objections by the profession that such criteria by themselves are likely to become distortionary ~~with~~ time, that there are infinitely many irrelevant ways of measuring research and probably no relevant ones, ~~have not had much effect~~. So we seem to be stuck with these rules.

Finally, a large part of research funding is now tied to strategic research projects chosen by the government. Furthermore, previously autonomous government research agencies have now been incorporated with our universities. Even though the Danish experience might be somewhat exceptional, I believe similar developments have taken place in other European countries. This in my view is a serious threat to European research diversity, at least in economics, but probably also in other sciences.

You've read our introductory essay; any comments on it? What did we miss?
Katarina: You speak a lot about the diversity of ideas in Europe, but there is another aspect that is important: the strong sense of competition typical

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of the United States system is not at all as pronounced here in Europe. I do not think it is any coincidence that the journal, university, and scholar rankings were invented in the United States. You have a long tradition for asking "who is the best, who has the most original idea, etc". In Europe we did not have this strong feeling of competition: university life was much more about debating, arguing, and working together.

There used to be a lot of generosity among scholars, a willingness to share new ideas rather than hiding them until publication. When the theory of co-integration was worked out over the last decades it was the immense pleasure of working together as a group that made the whole thing worthwhile. That Søren was the top brain in all this was never disputed and therefore not anything we felt we had to compete with. This kind of collective desire to understand more by developing the procedure was an extremely appealing feature of our work that no pay increase or top journal publication ranking could have compensated for. I believe many research oriented professors have chosen to work at the university because they love the intellectual challenge, the stimulating discussions, the exciting ideas, not because of the pay they get. Over the last decade, the Danish government has introduced differentiated salaries for professors as a means to promote research efforts. It has not turned out to be very effective as few professors seem to care about negotiating for higher salaries.

Any final comments?

Katarina: I would like to thank all of you for putting all this effort into providing us with these insightful discussions on European economics, lining up our strengths, and weaknesses, and suggesting how to maximize the former while minimizing the latter. It has given me food for thought which I hope to be able to use in the ongoing public discussion about how to organize our universities.