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# Unit Roots and Cointegration: A Panel Discussion with David Hendry, Peter Phillips, Katarina Juselius, and Søren Johansen

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**Abstract:** In April 2025, the Department of Economics at the University of Oxford hosted the “Workshop to Celebrate Forty Years of Unit Roots and Cointegration”, which commemorated the 40<sup>th</sup> anniversary of the *Oxford Bulletin of Economics and Statistics*’s 1986 special issue “Economic Modelling With Cointegrated Variables”. The current article summarizes that workshop’s panel discussion with major contributors to the literature on cointegration—David Hendry, Peter Phillips, Katarina Juselius, and Søren Johansen—and includes additional remarks by Martin Ellison and the conference’s audience. The discussion highlights key roles that the panelists and the *Bulletin* have played in advancing the literature on cointegration.

**Keywords:** cointegration, equilibrium correction, error correction, spurious regression, structural breaks, unit roots.

**JEL Classifications:** C10, N1.

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# 1. Introduction

**Neil R. Ericsson (NRE):** I am delighted to chair this panel discussion by four major contributors to the literature on cointegration: David Hendry, Peter Phillips, Katarina Juselius, and Søren Johansen. My introduction begins with precursors to the cointegration literature, including on spurious regressions, unit roots, and error correction. I'll review the *Bulletin's* fundamental role in advancing developments in cointegration, and also Oxford's role—in particular, Oxford University Press and the OUP editor Andrew Schuller, who was pivotal in promoting cointegration. I'll then summarize some major contributions to cointegration analysis by the panelists. The panelists themselves will follow, and Martin Ellison will follow them as discussant. We'll then open discussion to the audience and wrap up with final remarks by the panelists. Panelists, the discussant, and I are denoted by our initials (**DFH, PCBP, KJ, SJ, ME, and NRE**). Other speakers are indicated by their full names. A selected bibliography on the literature follows the text.

**Precursors.** Udney Yule's 1926 paper on nonsense regressions posed a key puzzle to the profession. In the same year, Bradford Bixby Smith published a paper in *JASA* that combines difference methods and deviation-from-trend methods for correlated time series, resulting in an error correction model (ECM). So, even at the start, we have two distinct threads, and those threads continue post-war. In econometrics, Bill Phillips in 1954 formally develops error correction from a control-theoretic perspective, and Denis Sargan in his 1964 Colston paper empirically implements an ECM for wages and prices, with novel economic innovations. In a parallel track for time series models, George Box and Gwilym Jenkins's 1970 book and Clive Granger and Paul Newbold's 1974 paper address spurious regressions by differencing.

In November 1975, Chris Sims hosted a conference that included a lively discussion between Clive and David on modelling of time series. That discussion led to Clive's 1981 paper linking cointegration and error correction, which resolved those two strands of the literature from a modelling perspective. Leading to that resolution, James Davidson, David Hendry, Frank Srba, and Stephen Yeo ("DHSY") in 1978 had formulated an empirical UK consumption function embedding a long-run expenditure-income relationship, and David in 1979 had framed spurious regressions as "Alchemy or Science". Peter's 1986 pathbreaking paper sorted out distributional issues for spurious regressions. Rob Engle and Clive's 1987 paper followed, and we owe a great deal of thanks to both Clive and Rob for that.

**The *Bulletin*.** Before Rob and Clive's paper was published, however, the *Bulletin* published its 1986 special issue on cointegration entitled "Economic Modelling With Cointegrated Variables". It was edited by David, and with the lead article by Clive. That special issue set the path for the *Bulletin's* role in cointegration. Four years later, the *Bulletin* published Søren and Katarina's paper on estimation and inference on cointegration. I'll return to that paper, which sets the stage for subsequent cointegration analysis; and it is very extensively cited.

In 1992, Anindya Banerjee and David edited a special issue of the *Bulletin* entitled "Testing Integration and Cointegration", and it doesn't stop there. The *Bulletin* publishes a whole series of special issues. While they don't all include the words integration or cointegration in their titles, they do include many papers about cointegration. These special issues include:

- David's 1986 "Economic Modelling With Cointegrated Variables",
- Anindya and David's 1992 "Testing Integration and Cointegration",
- Anindya and David's 1996 "The Econometrics of Economic Policy",
- Anindya's 1999 "Panel Data, Unit Roots, and Cointegration",
- Mike Clements and David's 2005 "Information in Economic Forecasting",
- David, Massimiliano Marcellino, and Grayham Mizon's 2008 "Encompassing", and
- the forthcoming 40<sup>th</sup> anniversary issue.

Not surprisingly, many economists nicknamed the *Bulletin* the "Oxford Bulletin of Cointegration".

The OUP editor Andrew Schuller also played a central role in publishing many books on and about cointegration, including:

- Clive's 1990 *Readings on Econometric Methodology*,
- Rob and Clive's 1991 *Readings on Cointegration*,
- Svend Hylleberg's 1992 *Modelling Seasonality*,
- Anindya, Juan Dolado, John Galbraith, and David's 1993 *Co-integration, Error Correction, and the Econometric Analysis of Nonstationary Data*,
- Clive and Timo Teräsvirta's 1993 *Modelling Nonlinear Economic Relationships*,
- Neil Ericsson and John Irons's 1994 *Testing Exogeneity* (originally titled *Cointegration, Exogeneity, and Policy Analysis*), and
- Colin Hargreaves's 1994 *Nonstationary Time Series Analysis and Cointegration*.

Peter, David, and I (NRE) participated in the conference that led to that publication. Subsequent related books published by OUP include:

- David's 1995 *Dynamic Econometrics*,
- Søren's 1995 *Likelihood-based Inference in Cointegrated Vector Autoregressive Models*,
- Michio Hatanaka's 1996 *Unit Roots and Cointegration*,
- Peter Reinhard Hansen and Søren's 1998 *Workbook on Cointegration*,
- Rob and Hal White's 1999 *Cointegration, Causality, and Forecasting*,
- Gunnar Bårdsen, Øyvind Eitrheim, Eilev Jansen, and Ragnar Nymoen's 2005 *The Econometrics of Macroeconomic Modelling*,
- Katarina's 2006 *The Cointegrated VAR Model*, and
- Jennie Castle and Neil Shephard's 2009 *The Methodology and Practice of Econometrics*.

While OUP was the center of publications on cointegration, Blackwell Publishing (also in Oxford) played an important role. It published (and continues to publish as Wiley-Blackwell) the *Bulletin*. It also published David and Ken Wallis's 1984 *Econometrics and Quantitative Economics*, David's 1993 *Econometrics: Alchemy or Science?*, and Peter's 1993 *Models, Methods, and Applications of Econometrics*.

**The panelists.** I have selected five publications by each panelist as key contributions that pushed the literature forward.

For David, I'll begin with Anindya, Juan, David, and Gregor Smith's article in the *Bulletin's* 1986 special issue—Anindya's first publication. That article provides analytical and Monte Carlo evidence on the strengths and limitations of the Engle-Granger procedure, even before Rob and Clive's paper was published. Anindya, Juan, John Galbraith, and David's 1993 book looks at the properties of cointegration analysis. Mike and David's 1999 book on forecasting nonstationary economic time series involves much analysis about cointegrated systems. David and Katarina's 2000/2001 joint work explains cointegration to a more general and nontechnical audience. Finally, and very importantly, David and Jurgen Doornik implemented these techniques into software, initially as PcGive, then in OxMetrics. Unless econometric techniques are implemented in software, they're pretty much useless, in effect remaining on the drawing board. *Early* implementation is also beneficial, as occurred with Søren's procedure for cointegrated VAR modelling. Søren's procedure was programmed into OxMetrics a year before his paper appeared in print, and his paper came out quickly.

Peter's 1986 paper on understanding spurious regressions solves key distributions for integrated variables. At the 1985 World Congress of the Econometric Society in Boston, Peter was scheduled to present a paper on distributions of test statistics of models. Peter entered his session and said: that's a really interesting paper and I'll talk briefly about it, but I have a much more interesting paper to present—his spurious regressions paper. We were in awe of Peter's work. Soon after, in 1988, Peter and Pierre Perron published their results on unit-root tests, a paper with over 30,000 citations. Peter and Mico Loretan's 1991 paper solves key system inferences, along with Peter's 1991 *Econometrica* paper on optimal inference. Peter's 1992 *Bulletin* paper with Peter Schmidt develops LM tests for unit roots. That gives a sense of some of Peter's contributions, how rapidly he was able to produce them, and how widespread and important his work is.

Katarina's 1990 *Bulletin* paper with Søren lays the formal framework for inference in cointegrated VARs and shows how that framework can be applied empirically. Their paper has over 20,000 citations and contributed to the *Bulletin* becoming a hub for cointegration. Katarina's 1992 paper—in a special issue that I edited for the *Journal of Policy Modeling*—proposes and implements an ingenious solution for dealing with multiple cointegrating vectors when there aren't enough observations to estimate all the cointegration vectors in one system. Katarina proposes analyzing subsystem by subsystem and then combining the information from those subsystems. Henrik Hansen and Katarina's 1995 CATS in RATS implemented cointegration procedures, for which she and others were solving distributional properties. Katarina's 2006 book systematically lays out the framework for modelling cointegrated VARs and illustrates the approach with substantive empirical analyses. Her 2015 paper shows the link between Haavelmo's approach and cointegrated VARs.

Søren's 1988 paper, with over 30,000 citations, was formative; and it rapidly moved the profession to jointly modelling long- and short-run properties in a VAR. Søren's 1991 *Econometrica* paper provides the general inferential framework for cointegration; his 1992 *Bulletin* paper adds a linear trend to the VAR and solves the inferential properties from having a trend. His 1995 OUP book further develops the cointegration framework for a VAR, all in one place, laid out systematically. Søren's 1995 *Econometric Theory* paper develops the framework

for the cointegration of  $I(2)$  variables. Many thanks go to Peter, who founded *Econometric Theory* and was its editor until January 2026.

Let's now turn to the panelists themselves. Anindya circulated a dozen potential questions for the panelists, focusing on early developments in integration and cointegration, their importance in retrospect, Yule's influence, the relevance of asymptotic theory, American versus European approaches for implementing cointegration practices, the continued relevance of this literature, and future prospects. I'll begin by posing Anindya's questions to David. Peter, Katarina, and Søren will follow with their perspectives on these and other topics.

## 2. David F. Hendry

**NRE:** What was your engagement with the cointegration literature early on? How did that come about?

**DFH:** When I completed my PhD in econometrics, there was an ongoing debate between economists and statistical time series analysts—the latter exemplified by Box and Jenkins, who advocated differencing data before modelling. We knew about Lawrence Klein's "great ratios" from his textbook. While the underlying data often appear non-stationary, some ratios are relatively stationary—the savings ratio, the capital-output ratio, etc. Moreover, Denis Sargan had demonstrated the advantages of relating differences of variables to feedbacks in functions of their levels, so it seemed odd to throw away that information on levels by differencing. The solution was simple: include lagged values of all the levels variables, rather than differencing the data before modelling. Even so, we weren't sure about unit roots.

At the Summer 1975 World Congress of the Econometric Society, Gordon Anderson and I presented the economic theory and an empirical application of a Sargan-type system in which we modelled UK Building Societies—the UK equivalent of US savings and loans associations. Our model worked well as an equilibrium correction system. A couple of months later, at a conference organized by Chris Sims, I criticized Clive Granger and Paul Newbold's presentation on spurious regressions when estimating static equations, which had led them to recommend differencing à la Box and Jenkins. I explained that you could get around the problem of nonsense regressions—which had high  $R^2$ s and low Durbin-Watson statistics—by putting in lags. Denis had shown that lagged levels of the variables could help explain the differences. At Chris's conference, Clive said that it was invalid to add levels of regressors to equations explaining differences. Clive then tried to disprove that approach. My challenge to Clive eventually proved fruitful, as it led him to develop the concept of cointegration.

**NRE:** Yule's 1926 paper was formative in the early literature but then disappeared from view. What do you think is the importance of his work, whose centenary we celebrate in 2026?

**DFH:** Understanding nonsense correlations remains essential but had little prominence in economics until Clive's presentation at Sims's 1975 conference. At LSE, Terence Gorman knew about Yule's paper and told me that the paper itself had been forgotten because economics was ahistorical, even while Yule's ideas on autoregressive models were widely known and used. Similarly, Bradford Bixby Smith, who published on error correction models in 1926, was completely forgotten until Terence Mills re-discovered his work.

**NRE:** What issues were you thinking about at the time?

**DFH:** The econometric challenge was to show that there were no problems in statistical inference with keeping log levels and their lags in econometric models. About the same time as Chris's conference, Grayham Mizon and I had critiqued Graham Hacche (then at the Bank of England) for differencing the data before modelling. To demonstrate that differencing wasn't needed, Grayham and I simulated a setting with unit roots and found little impact relative to stationary data, although in retrospect we were misled by incidentally having a large intercept that induced a deterministic linear trend. My 1980 paper on "Alchemy or Science" suggested that we understood nonsense regressions because we could create them—as with cumulative rainfall explaining UK inflation better than money—and also detect them as spurious. That spurious regression even showed up in the popular press and was mentioned in Parliament. All that said, Peter later revealed that there is far more to spurious regressions than what we thought at the time.

On the empirical side, DHSY—the UK consumption function developed by James Davidson, myself, Frank Srba, and Stephen Yeo, and published in 1978—showed that there was no problem in retaining lagged levels of consumers' expenditure and disposable income in our model. Indeed, those lags played a fundamental role in stabilizing the model through what we (erroneously) called error correction. At the time, we didn't appreciate the distinction between within-regime and across-regime feedbacks. The then so-called error correction was only within regime, not across regimes.

**NRE:** How did you come to think about integrated processes and cointegration?

**DFH:** We were puzzled by David Dickey and Wayne Fuller's 1979 findings because we had not found substantively different estimator distributions. I wondered: what was the benefit of sorting out all these cases for unit-root processes when it didn't seem to matter whether the data had unit roots or was stationary? For the variables that we analyzed, we could construct ratios (or log-ratios) that appeared well-behaved. Then came Clive's 1981 paper, in which he began unravelling that puzzle. Even though he had not yet fully formulated the structure of cointegration, we realized that there was more to this than we had been thinking.

**NRE:** How was the econometrics literature influenced by a whole new asymptotic theory?

**DFH:** Econometricians—including many attendees at the current conference—focused on developing the distributional theory for estimation and for testing with new critical values, including extensions for deterministic terms, systems, near unit roots, and structural breaks.

**NRE:** How have those theoretical developments on cointegration influenced empirical economic modelling and forecasting?

**DFH:** Empirical modelling has benefited from cointegration formulations, which facilitates economic interpretation and can achieve a more orthogonal representation. Forecasting is a more mixed bag. Cointegration helps when there are no shifts over the forecast horizon. When shifts do occur, systematic forecast errors can result because the forecasts are reverting to the old equilibrium.

**NRE:** What roles did the *Bulletin* play in these developments?

**DFH:** The *Bulletin* published many key papers on theoretical developments of cointegration and

its empirical implementation, beginning with its 1986 special issue on cointegration, which I edited. At the time, I knew of several excellent papers on cointegration. Clive had his “Developments” paper. My research with Anindya, Juan Dolado, and Gregor Smith was underway as I felt cointegration was an important advance. So, it was a small step to invite other work, including Rob and Clive’s paper. I did fail to get their paper, as it was subsequently accepted for *Econometrica*.

After the *Bulletin*’s 1992 special issue, the *Bulletin* became one of the most-cited journals in statistics. During the 1996 UK research assessment exercise, I received puzzled emails from Oxford statisticians who had never heard of the *Bulletin*. Cointegration had not yet permeated the statistics literature. For years, statistics appeared to lag behind econometrics on cointegration, breaks, and model selection. As you mentioned above, the *Bulletin* also published many regular articles on cointegration, and it had subsequent special issues that included cointegration as a theme.

**NRE:** What were the first problems that you tackled in this framework?

**DFH:** In the 1980s, I and many others were thoroughly engaged in sorting out the distributional consequences of integrated and cointegrated processes, and in implementing these new techniques empirically.

In the 1990s, Mike Clements and I started developing an economic theory of forecasting that accounted for cointegration and structural breaks. The statistics literature had a well-developed theory of forecasting, but that theory didn’t incorporate cointegration and had little on breaks. Mike’s and my work was spurred by my advising the Treasury and Civil Service Select Committee in 1991. Many economists had submitted evidence to the Committee about how their models were mis-forecasting, what they thought was missing from their models, and why they thought those omissions resulted in large forecast errors. However, they didn’t formally analyze the sources of the forecast errors. Moreover, there was no econometric theory of forecasting that analyzed forecast errors from models for cointegrated economic data with breaks. My report to the Committee (just recently published in the *Bulletin*) attempted to fill that gap.

Let’s put this in an economic context. In the late 1980s, the then Chancellor of the Exchequer Nigel Lawson introduced new economic policies that altered the equilibrium. Consumption fell more than income over 1989-1991. DHSY imposed an equilibrium with consumption proportional to income and so forecast poorly, subsequent to that shift in equilibrium. This fooled Norman Lamont, a subsequent Chancellor, who kept saying that “the green shoots of economic spring” were about to appear. After the Committee’s 1991 report, staff at HM Treasury reported to me that DHSY had gone off track. That led me to realize the key distinction between equilibrium correction mechanisms (EqCMs) such as DHSY, which only correct within regimes, and error correction mechanisms (ECMs), which could correct across different regimes. Equilibrium correction is a disaster when forecasting in the presence of a structural break, not only because the model goes off track but because its forecasts revert to the old equilibrium, increasing the forecast errors. This feature is characteristic of equilibrium correction models. Dropping the equilibrium correction term induces a unit root, bringing the forecasts back on track and robustifying the forecast. In 1989, John Muellbauer and Anthony Murphy showed that major changes occurred in many of the key determinants of consumption that were omitted from DHSY; and subsequent

research confirmed their findings. Hence, DHSY's equilibrium shifted; but those determinants were also probably difficult to forecast.

Relatedly, Rob Engle observed that DHSY had equilibrium correction in the consumption equation but not the income equation. However, having the same equilibrium correction in both equations induces a unit root in the process. That is, non-stationarity in economic data is a natural outcome of reduced rank in the cointegrating system.

**NRE:** Why do you think the prominence of cointegration has faded?

**DFH:** With or without cointegration, the many forecast failures from macroeconomic models were discouraging. Typical macro models—including dynamic stochastic general equilibrium models (DSGEs)—are equilibrium correction. So, when there's a shift in equilibrium, such models systematically mis-forecast. Shifts happen all the time: China's entry into the world economy, Y2K, the dot-com boom and bust, SARS, the great recession, quantitative easing, Brexit, Covid-19, Russia's invasion of Ukraine, energy crises, and massive disruptions to trade. Shifts cause forecast failures in EqCMs, whereas differenced-data models appear more robust, at least until calculating their forecasts of levels. Working in an area plagued by recurrent forecast problems seemed unattractive. Moreover, DSGEs, which ignored cointegration and claimed micro-economic foundations, became popular for publishing in top economic journals.

**NRE:** Why has the profession in the United States been less keen on the theory and practice of cointegration than in Europe?

**DFH:** Empirically, cointegration requires looking at the data, which often results in accusations of measurement without theory. When I was at Yale in 1975, I presented an early version of DHSY and was called a "data miner", even though Friedman's, Duesenberry's, and others' consumption functions were all special cases of DHSY.

**NRE:** Why does the analysis of time series still matter in a world of mostly harmless econometrics?

**DFH:** Do you mean "harmful econometrics"? Many challenges persist. Random sampling and independence are often conflated, even though Fairfield Smith in 1938 demonstrated substantial under-estimation of estimated variances from hidden dependence. Relatedly, unmodelled heterogeneity may reflect subgroup nonconstancies rather than individual variation. Those nonconstancies could shift cross-sectional relations and lead to forecast failure. Without addressing these issues, it is unclear how viable or stable the resulting inferences are; see recent critiques by Nancy Cartwright and Angus Deaton. Analysis of time series can deal with such issues through sequential factorization to obtain the equivalent of independence.

**NRE:** What is the continued relevance of cointegration?

**DFH:** Many empirical applications remain to be done. Time series are often "naturally" cointegrated because of economic behavior, as with logs of consumers' expenditure and disposable income, where their differential is approximately the savings rate. If series are cointegrated, you don't need to impose it. Even so, detecting underlying long-run relationships may require incorporating short-run behavior in the model. Machine learning techniques may have difficulty finding cointegrating relations unless explicitly taught to look for them.

**NRE:** Where do we go from here?

**DFH:** Empirical modelling of climate change! The stochastic trend from economic activity consuming fossil fuels drives a stochastic trend in greenhouse gas (GHG) emissions and hence in global air and sea temperatures. Cointegration has played an important role in econometric models of climate change, dating back to work by David Stern, Robert Kaufmann, James Stock, Katarina, and Søren. Analysis of climate change needs to incorporate sudden shifts—as in work by Rocco Mosconi, Bent Nielsen, and Taka Kurita—and address measurement changes, which Felix Pretis and I showed matter greatly. Felix has also provided a theoretical framework that maps an energy-balance model of global temperature, ocean heat content, and radiative forcing to a cointegrated system. Cointegrated relations are crucial for long-run forecasting (say, 100 periods ahead)—unlike models in differenced data—and are invaluable in climate modelling, where they can correspond to physical links that are unlikely to be subject to shifts, as my conference paper with Jennifer Castle, Jurgen, and Luke Jackson demonstrates. Luke, Katarina, Andrew Martinez, and Felix recently linked the melting of the two polar ice caps in a multi-cointegrated model.

Editors of the two top scientific journals *Science* and *Nature* have recognized that econometricians have statistical methods of general interest. Jennie Castle, Felix, and I lead a team of researchers at Oxford's Climate Econometrics project, most recently funded by Calleva. Several of those researchers co-authored an August 2024 article in *Science* that already has over 100,000 downloads. The journal *Nature* published a follow-up article commending their approach.

Finally, as seen at this conference, applications abound.

### **3. Peter C.B. Phillips**

**PCBP:** A very good morning to everyone in Oxford from Auckland, New Zealand. Thank you to the organizers, particularly Anindya, for inviting me to be part of this conference and sharing in a panel discussion and giving a presentation tomorrow. I'm sorry not to be there and be with many old colleagues and former students and friends.

I'll focus on some key points of the questions that Neil raised. This conference recognizes a revolution in the mid-1980s that focused mainly on time series econometrics, but also on persistence more generally. Persistence occurs not just in time series but also spatially. Climate is one such application. That revolution in econometrics opened the eyes of scientists working in various different fields, although not immediately. That is important to bear in mind when discussing the future of the subject, which is founded in empirical discovery along the lines that David discussed, and also in the methodological underpinnings that give validity to the procedures that we use in practice.

Persistence and linkages amongst persistent series are prevalent, not only in economics but also outside of economics. The data typically have non-stationary characteristics but, until the 1980s, there was a near vacuum of high-level technical research on this pervasive problem. People typically had run regressions on time polynomials, which certainly worked for reasons that I've explained in subsequent papers. Or, they differenced the data before regression. David referred to this, Yule discussed this, and there were earlier papers as well. Differencing is clearly a convenient way of eliminating the type of nonstationarity evident in prices and interest rates and other data that display random-walk characteristics, what we now think of as stochastic trends.

Those observations ultimately led to Box and Jenkins's procedures.

Let's turn to error correction modelling and Denis Sargan. Denis had an inimitable role that drew on his brilliance as a scholar, his extraordinary intuition, and his technical skills. He was First Wrangler in mathematics at Cambridge, which no other economist has ever equaled. The mathematician G. H. Hardy got only a fourth. Denis had a real commitment to understanding economic behavior and to advancing methodology that would help in empirical work. He strongly believed in error correction modelling and stated many times that it helped avoid what I call the "marshland of spurious regression".

Denis was my PhD thesis advisor at LSE. However, I was introduced to ECMs before going to LSE while writing my master's thesis at the University of Auckland under Rex Bergstrom in 1969-1970. I formulated a three-equation continuous-time trade-cycle model, developed the asymptotic theory for it, and sorted out identification issues to avoid the aliasing problem. I knew that trade cycle models involved aggregate data, so I had a stationary ECM.

I said to Rex that maybe I can develop the finite sample theory. Rex replied that the world's best mathematicians had worked on this for ten years, and so what are your chances? I thought that I'd be able to do an Edgeworth expansion. After my LSE thesis, I worked on the Edgeworth expansion for the AR(1) model at the University of Essex. There was a very clear implication: the  $1/\sqrt{n}$  correction term in the expansion exploded as the autoregressive parameter approached unity. That also explained why the bootstrap broke down for unit root processes. Immediately after I published that paper in 1977 in *Econometrica*, I received a letter from John White. I had read his paper and knew that he was working on Edgeworth expansions. I asked him whether anyone had proved his speculation about the Wiener process functional for unit root theory, and he replied no, that no one seems to know any way of doing this.

When I arrived at Yale the following year, I met David Pollard—one of the world experts on weak convergence—and asked him whether he knew anyone who was working on problems like this, but he didn't. Subsequently, on a sabbatical, I worked out the unit root limit theory myself and derived its implications for spurious regressions. As Neil mentioned, I presented those results at the 1985 World Congress.

Spurious regression is a complicated subject. David's 1979 LSE inaugural lecture highlighted how problematic spurious regressions are in empirical work by regressing UK price series on a quadratic function of cumulative rainfall. By construction, cumulative rainfall is at least  $I(1)$ ; and prices are probably a long-memory process with memory parameter of around 1.4. I think that's one of the reasons why the quadratic was successful in David's example, including comparison relative to a monetary explanation of prices. David's regression passed a host of tests, following his mantra of "test, test, and test", which is tremendously important. There remained methodological issues concerning what constitutes a valid test, especially for multiple testing and sequential testing, which require special controls on size. Linkages amongst highly persistent economic series compounded the challenges in ECM modelling. A methodological gap needed to be filled.

I proposed an alternative interpretation of spurious regressions that involves a coordinate representation of one persistent series in terms of other, possibly unrelated, series. That

formulation is not causal and not explanatory, but it can “explain” the type of fascinating results that David obtained.

Three key developments drove the 1980s cointegration revolution. First, ECM modelling already had a long and distinguished history, particularly at the LSE, with key contributions by Bill Phillips, Denis Sargan, and Rex Bergstrom. Second, Ted Anderson had worked on reduced rank regression, which was central to the algebra used much later in reduced rank regression for cointegration. Third, understanding and being able to detect spurious regressions was critical, as David’s inaugural lecture showed. These methodological contributions provided a foundation for the edifice of work in cointegration, which has influenced not only economics but more generally the social and business sciences and now the natural sciences. However, none of that early work derived a limit theory for nonstationary data, although John White’s 1958 *AMS* paper hints that an invariance principle might apply.

As an aside, the Royal Economic Society held a conference in the mid-1970s in Sheffield, I believe. Several econometricians—including David, Clive, Meghnad Desai, Grayham Mizon, and myself—were having a lunchtime discussion, which was all about ECMs. How could an ECM have a differenced variable on the left-hand side, and on the right-hand side a stationary error and a linear combination of persistent variables? From the property of “balance”, we knew that that linear combination must be stationary. But how could that happen? Denis believed that it was valid to run these ECM regressions, but the statistical underpinnings hadn’t been developed yet. That lunchtime discussion didn’t resolve the econometric problem, but it led several of us, including Clive, to think much more about it. Rob and Clive’s 1987 paper proved consistency. They didn’t solve the limit distribution but speculated that it might be normal under certain conditions. In fact, under exogeneity it turns out to be mixed normal.

The *Bulletin*’s 1986 special issue focused on how the new research provided some solutions that were immediately useful in applied work, and that special issue predated many key papers that were subsequently published in leading journals, including *Econometrica*, *Review of Economic Studies*, *Journal of Econometrics*, and *Econometric Theory*. My own unit root paper was delayed being published in *Econometrica* until 1987 because the editor wanted both my paper and Rob and Clive’s paper to run together as lead articles.

Many economists have critiqued empirical macro models. As early as the 1970s, Robert Basman and others were disenchanted with the very large “structural” models of the time, highlighting how difficult it was to understand those models because of their high dimensionality. Machine learning and AI in modelling pose similar risks, as their methodological underpinnings are typically derived for IID variants but remain to be worked out for highly persistent data.

Relatedly, Sims’s 1980 article on VARs criticized the “incredible restrictions” imposed in many models. His critique motivated the development of so-called structural VARs (SVARs)—or what I would call theory-empty VARs—and impulse response analysis. Decades earlier, however, Henry Mann and Abraham Wald had analyzed the properties and limitations of VARs and SVARs in their pathbreaking 1943 *Econometrica* article. Regrettably, early articles such as theirs are now rarely read, reflecting concerns about our profession being ahistorical. That can lead to a proliferation of errors and misunderstandings that percolate through the profession.

As another example, consider the notable contributions of David Dickey, Wayne Fuller, and David Hasza that led to the ADF procedures commonly used in testing for unit roots. Their articles have no functional central limit theory at all. Instead, they assume NID errors, hoping that some invariance principle might apply. Modern textbooks typically attribute Dickey and Fuller to developing the whole functional central limit theory that led to the standard unit root limit distribution that we all know and love.

#### 4. Katarina Juselius

**NRE:** What was your engagement with the cointegration literature early on? How did that come about?

**KJ:** I chose to study economics because I was interested in understanding why unemployment went up and down, what determined inflation, and so on. I also was interested in an empirical approach. At the time, the static regression model was the dominant empirical approach and was taken for granted. I learned how to use it, but I wasn't satisfied with it. The underlying assumptions seemed simplistic and not credible. I have a skeptical mind and I wanted to find out the results for myself. I came across the so-called error correction models by David and others. I found them vastly more realistic and intuitive, and they opened up a completely new world for me, where doing empirical research produces results that actually matter.

These tools weren't easy to implement because we only had a static regression package. I needed David's software code, so I called Frank Srba, who was managing David's programs at LSE. Because the programs were complicated and not well-documented, Frank was reluctant to share them. The programs were then called GIVE, RALS, FIML, ARFIML, ARRF, RAML, and GENRAM; and they were mainframe only. After some back and forth, Frank relented and sent them to me on magnetic tape, which I had to pick up from customs. Running the programs required input on punch cards and, after many hours, I became expert at running and modifying those programs and did lots of empirical modelling with them.

I then discovered Clive Granger's paper on error correction and cointegration. I was completely intrigued by it and read it over and over. I realized it was very important theoretically but couldn't understand how to implement cointegration in practice. Clive had formulated cointegrated models in a moving average representation, and such models were very complicated to estimate at the time.

To better understand cointegration, I wrote Søren—then a young promising mathematical statistician—to discuss Clive's paper at a Nordic Meeting for mathematical statisticians. Søren saw the potential of cointegration and how it could address the previously unsolved problem of analyzing non-stationary processes. Søren started working on the mathematical and statistical issues to translate Clive's vector moving average representation to a vector autoregressive one, formulate the likelihood-based trace and max statistics, and derive their nonstandard asymptotic distributions. It was very exciting to see Søren solve those issues.

I didn't have Søren's statistical competency, but I could complement it with my empirical expertise. We started working together, and I subsequently moved to Copenhagen, where Søren was. I applied Søren's framework to two data sets on money demand and its impact on inflation. One data set was for Denmark and the other for Finland. The empirical results also helped check

whether the theoretical results make sense. That was a fruitful way to develop both theoretical and empirical results. It was about 1986, when everything started falling into place.

In 1987, the European Meetings of the Econometric Society were in Copenhagen. Søren presented his theoretical results, and I presented my empirical money demand models, including calculated eigenvalues, eigenvectors, and trace statistics. The distributions of the test statistics were a problem. Søren had derived the statistics, but their distributions had to be simulated. Computers at that time were not very powerful. To simulate the distribution of a test statistic for a model with no constant and no trend took several weeks. I presented my paper and concluded by saying: unfortunately, the critical values for the test statistics aren't available yet. Then the door opened, and in came one of Søren's research assistants with the simulated critical values. That was fortuitous. Later on, one of my PhD students Michael Osterwald-Lenum simulated and tabulated different sets of critical values, which were subsequently published in the *Bulletin*.

The paper that I presented in Copenhagen evolved into Søren's and my 1990 *Bulletin* paper. That paper was initially under review at *Econometrica* for about three years. David, Søren, and I were then visiting San Diego, where we and the other San Diego econometricians had been discussing our paper at great length. When a letter arrived from *Econometrica* rejecting the paper, David offered to publish it in the *Bulletin*, which we happily accepted. After having waited such a long time, getting it published immediately was very rewarding.

**NRE:** How was the econometrics literature influenced by a whole new asymptotic theory?

**KJ:** It allowed us to distinguish between the long run, medium run, and short run. Doing so is especially important in applied economics. It was also very important for understanding equilibrium correction and common stochastic trends, and it helped us distinguish between the (endogenous) pulling forces and the (exogenous) pushing forces.

**NRE:** Why do you think cointegration's prominence has faded?

**KJ:** I think there are several explanations. One is that, unfortunately, many of the early empirical applications of cointegration were not very good, and that led to the approach losing some credibility. I refereed and rejected many applied cointegration papers because of serious flaws in their analyses, but many other similar papers were published.

Also, separate macro-based and micro-based strands had emerged in empirical macro-modelling. The former is strongly influenced by Keynesianism and treats the macroeconomy as an entity as such, whereas the latter derives the macroeconomy from individual agent behavior. The former has been out of fashion for more than half a century (particularly in the United States) but often explains the data vastly better than the micro-based models.

Relatedly, many empirical economists are used to starting with a theoretical model, estimating its parameters, and expecting those estimates to have a direct economic interpretation. A cointegrated VAR study often finds unexpected results, such as wrong-signed or insignificant coefficients, and thus a common critique is that this empirical approach doesn't produce the "right" results. However, there are many reasons for such "failures", with one common one being that the preferred theory model does not explain the data sufficiently well. By contrast, a careful cointegrated VAR analysis may be very informative by telling a different, more relevant story. That approach typically involves discovery, rather than straightjacketing the data to match a given

theory.

Finally, the cointegrated VAR model represents a very precise yardstick with which to measure economic behavior. It may therefore be a very bad method to apply if you want to illustrate that your preferred theory model is empirically correct.

**NRE:** What is the continued relevance of this literature?

**KJ:** Its relevance depends to a large extent on the world around us and whether it settles down or not. Cointegration models need fairly long periods of reasonably constant structures. Over the last fifty years, we have experienced many serious breaks, including the collapse of Bretton Woods, the rise of the US dollar as a global currency, financial deregulation, increased globalization, the financial crisis, Covid-19, the Russian invasion of Ukraine, and recent trade wars. David has discussed how such breaks can be modelled econometrically as shifts in the equilibrium mean. However, I think that some of these changes are so severe that they alter fundamental features, such as the number of common trends and hence the number of cointegration relations. It's hard to say whether this is the case except with a reasonably long period of observations. Currently, it seems that the next major crisis starts as soon as the previous one has ended, so I'm worried about just how frequent these structural breaks are occurring. We need to be able to extract constant economic behavior deeply buried under the debris of crisis behavior. Climate change may be especially amenable to these methods because climate has not experienced as many abrupt shifts as other parts of the economy.

## 5. Søren Johansen

**NRE:** Søren's presentation will be in the form of an interview. My first question concerns your 1988 paper, which built on the Granger representation. How did you realize that the likelihood framework was the right approach, and that reduced rank was the solution? I recall that insomnia contributed to this discovery.

**SJ:** Let's start with the insomnia. Yes, it did play a role. There was the 1987 Econometric Society conference at the Technical University of Denmark, which is in Lyngby, the village where we live—so it was natural to submit a paper. Unfortunately, I didn't have a paper to submit. I remember lying in bed shortly after the submission deadline, awake, looking at the shape of the light in our bedroom and the roof, and thinking: how on earth do we solve these distributional problems about reduced rank? I suddenly solved it and wrote down a little summary of the solution. Even though the deadline had passed, I sent my results to Timo Teräsvirta, who was co-chairing the program committee. I thought Timo would help me, and he did. Just prior to my presentation, I received the biggest compliment in my life when Rob Engle spent most of his invited lecture talking about my results, which he had already seen in an abstract. With that introduction, I was extremely well received by my audience.

Helmut Lütkepohl saved my reputation at that conference. I presented these fantastic derivations, and Helmut said: Isn't that just reduced rank regression? As a statistician, I of course knew what rank-based statistics are: replace the ordered data with numbers 1, 2, 3, 4, 5, ..., and run a regression with them. My immediate answer to Helmut would have been that that's nonsense. However, I realized that I may have misunderstood Helmut, so I said, what an interesting idea, and I'll look into it. So, thank you very much to Helmut for pointing out to me that Ted Anderson

had solved the reduced rank regression problem in 1945 for his thesis. Since then, I have always referred to Ted's results.

Also, Katarina presented her cointegration results on money demand. It was a nice occasion. We had dinner somewhere, and I was walking next to Masanao Aoki, who was then the editor of the *Journal of Economic Dynamics and Control*, and he offered to publish my paper. It came out very fast, within a year. Reading it now, I realize that it is not as well expository as I would have liked.

**NRE:** Although your initial career was as a mathematical statistician, you've coauthored many papers with econometricians, including Katarina and Bent. What benefits have you found in coauthoring?

**SJ:** I think coauthoring is very natural. If you're interested in a problem and someone else says that they're also interested, then you collaborate with them. Alternatively, you could have an attitude that this is my problem to solve. My attitude is that, if we can solve it together, then that's fine. I don't see a systematic approach to collaboration. One should be open minded and talk about problems to other people, and that's been very useful for me.

**DFH:** On the topic of coauthorship, in December 2003 I met with Søren and Katarina in Stockholm when Rob and Clive were getting the Nobel Prize. While we were walking over a bridge, I suggested to Søren that one could include a dummy variable for every observation in a regression model. Søren said, you're nuts. I responded, saying, no no Søren, I've done it. Just don't do it all at once; do it in big blocks. Eventually, I persuaded Søren to coauthor on this topic, which we did multiple times. Sometimes, coauthorship needs persuasion. Also, it was very cold, which may have motivated us to move on and encouraged collaboration.

**SJ:** I remember another thing. David and I were walking down the street, and suddenly we both stopped, walked back five meters, and found this shop where you could buy chocolate. I knew that that was what he really wanted.

**NRE:** You also worked out distributional properties for cointegration tests when there is a structural break in the DGP. That established a framework for the saturation methods that you, David, and Bent have worked on. How did that originate?

**Bent Nielsen:** Søren and I were at the ESEM 2006 conference in Vienna. Søren had been working on the paper for impulse indicator saturation in a model with location and scale. He was wondering about how to generalize to the regression case. I had been working on a paper about the empirical processes of regression residuals, and we found out how to bring those ideas together.

**NRE:** Søren, you also generalized the cointegrated VAR by including a trend. Dickey and Fuller had included a trend in their unit root test, but the trend has a more subtle role in the cointegration framework. Why were you interested in this particular problem?

**SJ:** Suppose you start with a VAR, add an intercept, and ask how that constant term alters the process. Using the Granger representation, the intercept implies a linear trend in the underlying data. If the VAR includes a linear trend as well as an intercept, the data then have a quadratic trend unless the VAR's linear trend is restricted to the cointegrating vector. Regardless, deterministic variables such as an intercept and linear trend affect statistical properties, so you

have to be careful.

**NRE:** Let's now turn to Martin Ellison, who will offer a few comments and perspectives on cointegration.

## 6. Martin Ellison

**ME:** I'm a macro economist and a user of cointegration. My involvement in cointegration goes back thirty years to when I was a PhD student at the European University Institute in Florence, walking into the Sala Rossa to start my advanced econometrics training. Søren was the lecturer and emphasized that there are two ways to teach a course: very deeply on one topic and understanding it properly, or superficially and with some understanding of several topics. Søren taught an entire course on only cointegration, delving deeply into it. Søren is a fantastic lecturer, and I learned a lot.

An assignment for that course provides a humorous anecdote. The assignment was well crafted and creative, aiming to help us understand cointegration. Søren emailed us the assignment and was so helpful that he also attached the solution. Søren didn't seem too happy when I pointed this out, but I really enjoyed the course.

From my perspective as a macro economist, cointegration has featured in a few of my favorite papers. For example, Olivier Blanchard and Danny Quah use cointegration to distinguish between permanent and temporary shocks to output and to identify them. Similarly, Jim Stock and Mark Watson's approach to cointegration helps identify how a shock influences consumption, output, and investment. Paul Beaudry and Franck Portier show that a news shock has an immediate effect on the stock market and a permanent effect on TFP, but no immediate effect on TFP. These papers build on a proper understanding of unit roots and cointegration.

Cointegration helps show how to do time series econometrics properly: take the order of integration of variables seriously, and look at the cointegrating vectors, especially in light of the Kaldor facts and the great ratios. Cointegration clarifies the distinction between short run and long run, and it highlights how problems arise if analysis confounds short and long run.

Why don't we all do time series econometrics like that? There are several reasons. Contemporary empirical macro analysis has been influenced by other developments as well, starting with the Lucas critique, which highlights how reduced-form models break down when policy rules alter. As David taught me in Oxford, weak and super exogeneity are a fundamental response to the Lucas critique, but these exogeneity concepts haven't influenced empirical macro in the way that we thought they would. Another response to the Lucas critique has been Andrew Harvey's structural time series models. Subsequently, Frank Smets and Rafael Wouters built off of structural time series models using a Bayesian approach that has become hugely popular. More recently, Alejandro Justiniano, Giorgio Primiceri, and Andrea Tambalotti developed time-varying structural VARs with stochastic volatility. In Google Scholar, Smets and Wouters's top two papers together have just over 14,000 cites, whereas Johansen's 1988 paper has over 30,000 cites. That's a two-one win for Johansen. When I go to a football match, if my team wins two-one, I'm quite happy, but it's not a walkover, and the other team is pretty decent.

Mainstream empirical macro has never really bought into cointegration, and neither have policy-

making institutions as a whole. Although Norges Bank has been very keen on cointegration for a long time, the Bank of England has been in and out, and the US Federal Reserve System generally hasn't adopted an explicit cointegrated framework in their macro models.

A key reason is that other approaches provide superior storytelling. Especially in central banks, storytelling is ubiquitous and gives a comfort blanket of perceived causality, as when describing what transmission channels matter in the economy. What happens if we shut down or alter a particular channel? The structural approach provides a richer way of thinking about these issues. For instance, consider the recent changes in tariff policies. A pure time series approach could assess the effects of the United States' 1930 enactment of the Smoot-Hawley tariffs and derive current scenarios from that. Alternatively, it may be easier to tell a story from a structural time series model with calibrated or estimated key elasticities, such as for the substitution between imports and exports.

That advantage of structural modelling may be less for forecasting if it's assumed that the underlying policy regime doesn't alter. Even so, a story is typically needed. For example, one might say that GDP growth is going to be weak because of a problem with labor market participation. Creating such a story is more of a challenge when working with atheoretical non-identified time series models.

I'll finish with two thoughts. First, we are all doing the same thing, just with different approaches. As Tom Sargent would say, a model is a sequence of probability distributions over endogenous variables. We all agree with that. Second, as George Box observed, all models are wrong, but some are useful. I build and estimate theoretical models, and they're wrong. Cointegrated VAR models are wrong too. We need to acknowledge that both types of models are wrong, and also that we can learn from them both.

**NRE:** One promising direction, which you allude to, is to develop economic models that have cointegrating implications and then to implement empirical formulations that embody the economic theory in a dynamic cointegrating framework. Robert King, Charles Plosser, Jim Stock, and Mark Watson's 1991 article in *AER* is a good example of that approach.

## **7. General Discussion and Final Remarks**

**NRE:** Let's open up for questions and discussion from the audience.

**Sophocles Mavroidis:** What topic in cointegration would you advise a young researcher to focus on?

**PCBP:** I would encourage them to work on curved time series. Linkages occur over time and explain non-stationarity that is eliminated by taking appropriate linear transforms of the data. Linkages also occur cross-sectionally with persistent dependence. Both types of linkage are possible in long wide panels, and also in long short panels and in some long narrow panels.

**DFH:** As both Katarina and I have mentioned, economies have experienced many disruptions. In modelling, account for such structural breaks, particularly in a cointegrated framework. Likewise, analyze how a cointegrated model performs when forecasting in a world whose structure alters.

**KJ:** We have learned from David the importance of modelling general to specific. I have also

found it useful to progress simple to general by expanding the information set, although David has not always agreed with me. For instance, start with a small set of variables and see how the model changes when you add variables. That may provide insights on ceteris paribus assumptions and on how good a small model is compared to a bigger model.

**John Muellbauer:** Interest in cointegration declined with the rise of new classical economics and the new Keynesian DSGE approach, which have become so fashionable. I've been working with the Czech central bank, where they've decided to design a semi-structural model to run in tandem to their DSGE model, with a view to replacing the latter. Olivier Blanchard calls these semi-structural models *structural* because they tell stories about the *structure* of the economy. The Czech DSGE model is ridiculous for telling stories because (e.g.) it assumes that households make investment decisions and own all the firms in the economy. However, in the Czech Republic, half the firms are foreign-owned; ditto, banks. Their DSGE model has no asset prices or balance sheets, but asset prices and balance sheets really matter, as witnessed in the great financial crisis. Also, monetary transmission doesn't work through just intertemporal substitution and the exchange rate, but also through balance sheets, cash-flow channels, and so on, as we have seen in the UK, most strikingly in 1989–1992. Monetary transmission, as understood by the central bank's board, is quite different from what's in their DSGE model.

Cointegration remains important to the profession, but implementing the lessons faces some obstacles. Long-run relationships can be quite complex to model because they may involve four or five key variables. Focusing on only two or three variables may miss a major part of the story. The training of economists is also a fundamental problem. The Czech National Bank is having real difficulty finding people who are capable of working on a new semi-structural policy model because many economists lack time-series training and an understanding of cointegration.

**Álvaro Escribano:** What's the future of cointegration with big data?

**DFH:** You have to be careful when analyzing big data, choosing your tools, and interpreting how they relate to the outcome. Jurgen, Jennie, and I showed that, when modelling cointegration, summaries of big data like principal components give exactly the wrong answer because they pick up maximum variation, whereas direct cointegration analysis of the data is deliberately designed to get minimum variation. If big data have cointegrating relationships, they'll be extremely precise because there's so much information. However, big data may add nothing, as when going from annual data to minute-by-minute data, which may just introduce extra variation. I don't see big data's impact on cointegration analysis being dramatic.

**Grayham Mizon:** In the 1970s and 1980s, Sir John Mason was director general of the Met office and was a Fellow and Treasurer of the Royal Society. He invited David and me to a conference at the Royal Society to present views on cointegration and forecasting. David and I were told very firmly beforehand that meteorologists understand forecasting so much better, and we as economists have nothing to add. Afterwards, Mason recognized that meteorologists and economists could in fact learn from each other, including on data collection and analysis, and on forecast evaluation.

**DFH:** Mason also very insightfully observed that meteorological forecasting doesn't change the weather, whereas economic forecasting can alter people's behavior.

**Jurgen Doornik:** How will the revolution of machine learning and AI influence our discipline? Typically, both techniques ignore time series properties qua time series and are relatively theory-free, contrasting with what econometricians can do.

**DFH:** I'm skeptical about current popular methods of machine learning, which require a designated training sample and then an application of it. If the data's structure is changing, the training sample may have little to do with the holdout sample and so be uninformative. Economic theory may provide a bridge between the training sample and the holdout sample.

**NRE:** Let's finish with summaries from the panelists.

**PCBP:** As Martin pointed out, SVARs provide a richer framework for discussing causality and transmission mechanisms. Not everyone would agree with that, but impulse responses can be useful, and many central banks like that approach. However, such analysis could be conducted in a constrained environment such as reduced rank regression.

The prominence of unit roots and cointegration has faded some, and especially in the United States where most PhD programs don't provide high-level intensive time series education. These topics are typically just part of another course. By contrast, at Yale we had two full-semester courses in time series, starting with proofs of the ergodic theorem. That provided a very strong technical foundation, enabling students to enter a new area and innovate.

Machine learning just handles a very high-dimensional problem by constrained regression, often L1 regression. Analytics on ML are much more difficult. The framework is typically IID, whereas data in the social sciences, business sciences, and natural sciences are often nonstationary with dependence and persistence. We need the technology and a foundation for inference and prediction, especially because the training sample may rapidly become irrelevant with time variation.

Similar criticism applies to the now-popular difference-in-differences and treatment-effects analysis. Not enough attention is given to the counterfactual, which is further complicated by identification issues for the trend. It's tremendously important to take that into account, as we do with partially identified sets and fan charts.

I think the work on cointegration and unit roots will remain influential, as has the work by Hooker, Yule, and others, a century past.

**KJ:** In economics, it is considered important to be able to tell stories. The stories being told need to be empirically relevant, but they are not always so. This discrepancy may occur because there is not just one story but several stories that could be told, and it is not always clear which one to choose. A careful cointegrated VAR (CVAR) analysis can make this choice easier, as it could point out important features in the data that are consistent—or inconsistent—with extant theory models. In this sense, the CVAR model can act as broad “confidence bands” within which empirically relevant stories should fall. Incorporating CVAR analysis into policy modelling could then lead to stories that are more relevant empirically.

**DFH:** The language that we use is important. Scientists have prediction; weathermen have forecasts; builders have precast; and economists have forediction. “Forediction” emphasizes that our so-called forecasts are actually stories. They are fore-diction, not just forecasts. While typical

economic forecasts include numbers, they are also often accompanied by words (i.e., foredition) that interpret those numbers. Illustrating that distinction, Neil has compared numerical forecasts from the Fed's Tealbook with foreditions in the Fed's FOMC minutes, using text analysis of the latter. Similarities between those forecasts and foreditions can be tested, and they don't always match. Also, forecasts may differ from foredition when the latter describes alternative scenarios, as with saying that GDP may increase less than predicted because of possible future interventions.

Finally, I have no problem with SVARs per se, but in current practice they're not adequately tested. Modellers produce an SVAR, report some numbers, and claim victory. When tested, however, SVARs typically aren't constant over time. Their nonconstancy isn't consistent with them being "structural". So, what are they? I don't know. If that literature did "test, test, and test" SVARs, using the right significance levels for the number of tests, then I would have more admiration for SVARs than I currently do.

**NRE:** Let me wrap up. Cointegration has revolutionized our approach to and understanding of the economy. It clarifies the long-standing spurious-regressions problem, integrates the time-series and economic approaches to empirical modelling, and provides a unifying framework for understanding equilibria and disequilibria. The panelists led the way in developing key breakthroughs on cointegration: conceptually; statistically, especially for distributional properties of test statistics; and empirically, with diverse applications including wages and prices, money demand, consumer expenditure and income, imports and exports, the term structure of interest rates, debt, financial markets, housing, the labor market, R&D, production and productivity, monetary policy, forecasting, and more recently climate change. Many fascinating theoretical and applied challenges still await solving. My thanks go to David, Peter, Katarina, Søren, Martin, and the audience for such an informative retrospective and prospective discussion on cointegration.

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