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Economic forecasting in theory and practice: An interview with David F. Hendry

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ABSTRACT

David Hendry has made major contributions to many areas of economic forecasting. He has developed a taxonomy of forecast errors and a theory of unpredictability that have yielded valuable insights into the nature of forecasting. He has also provided new perspectives on many existing forecast techniques, including mean square forecast errors, add factors, leading indicators, pooling of forecasts, and multi-step estimation. In addition, David has developed new forecast tools, such as forecast encompassing; and he has improved existing ones, such as nowcasting and robustification to breaks. This interview for the *International Journal of Forecasting* explores David Hendry's research on forecasting.

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1. Early work on forecasting

NRE: David, you've made major contributions to many areas of economics and econometrics. These include econometric methodology, general-to-specific modeling, Monte Carlo techniques, software implementation, the history of econometric thought, policy analysis, and empirical investigations of consumer expenditure, money demand, inflation, and the housing market. We discussed these topics at length in Ericsson (2004), so let's focus on another important topic—forecasting. Over the last couple of decades, you've made significant contributions to our understanding of economic forecasting. When did you first become interested in forecasting?

1.1. The University of Aberdeen

𝔻𝔽ℍ: It was in 1964. I was an undergraduate at the University of Aberdeen, and I was very much influenced by the empirical economic models of Lawrie Klein (1950) and Jan Tinbergen (1951), who suggested that we might be able to forecast future outcomes. In my undergraduate thesis,

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I estimated a regression model for annual UK consumers' expenditure given current income and lagged expenditure—painstakingly worked out on a mechanical calculator. Using the whole-sample parameter estimates, I calculated a "forecast" of the last observation to see how close it was to the outcome.

NRE: In effect, you were evaluating the last residual of your estimation period. What did you find?

𝔻𝔽ℍ: The forecast and the outcome were reasonably close. That's unsurprising, given how the "forecast" was calculated. Because the forecast was within the estimation period, the corresponding forecast error was included in the sum of squared errors that OLS minimized.

1.2. Macroeconometric models and predictive failure

NRE: When you were writing your PhD thesis under Denis Sargan at the London School of Economics (LSE), you developed a small macro-model of the UK economy that included an equation for consumers' expenditure. How did your forecasts fare?

𝔻𝔽ℍ: Not well! In late 1967, I calculated *ex ante* forecasts of consumers' expenditure for the next two quarters: 1968Q1 and 1968Q2. When actual expenditure was later reported by the Central Statistical Office, I found that my model had massive forecast failure. The parameter constancy test rejected, and its p-value had so many zeros that it was embarrassing. It took me years to understand why such forecast failure is commonplace.

That particular forecast failure arose from a change in economic policy. During 1968Q1, the Chancellor of the Exchequer (that is, the UK finance minister) threatened to increase Purchase Tax—essentially, a sales tax—if consumers didn't "behave themselves" and spend less. Consumers responded by spending more, especially on durable goods. So, in the next quarter, the Chancellor duly increased Purchase Tax, and consumers' expenditure fell. My model did not account for the policy threat, the policy's implementation, or consumers' responses to both. Consequently, my model's forecasts failed badly.

NRE: Your UK model was subsequently published as Hendry (1974), which included a new test for predictive failure. It generalized Gregory Chow's (1960) single-equation predictive failure test to systems, albeit in a χ^2 version rather than the F version that Jan Kiviet (1986) later developed. How did that experience with your small macro-model influence your work on forecasting?

𝔻𝔽ℍ: It motivated me to investigate the nature of predictive failure. Why did models built from the best available economics using the latest econometrics and fairly good data not produce useful forecasts? In Hendry (1979b), I linked predictive failure to poor model formulation, but that explanation subsequently turned out to be unhelpful, or at least incomplete.

NRE: Other economists were also evaluating forecasts from macro-models. In particular, Charles Nelson wrote two influential papers on *ex ante* forecasts: Cooper and Nelson (1975) and Nelson (1972).

𝔻𝔽ℍ: Charles showed that forecasts from univariate time-series models could beat forecasts from large empirical economic models such as the FRB–MIT–PENN model. From an LSE perspective, such large models treated dynamics inadequately, often simply as autocorrelated errors in static equations. Because of that dynamic mis-specification, we suspected that models that included only dynamics could forecast better. I found that simple dynamic models did indeed forecast better than static economic models, even though the latter embedded economic theory whereas the former did not. However, I had misinterpreted the implications of Nelson and Cooper's results. I had not realized that models in differences—such as those in Nelson (1972)—almost invariably forecast better than models in levels if the means of the variables being forecast altered. We now refer to such changes as *location shifts*.

NRE: Nelson and Cooper's forecasts used methods that were proposed by Box and Jenkins (1970). Those methods are robust to location shifts for reasons that we did not appreciate at the time. However, those methods omit information about the long run because they include only variables in their differences.

𝔻𝔽ℍ: Indeed. At a Minneapolis Fed conference in 1975, I criticized Clive Granger for differencing: see Hendry (1977) on Granger and Newbold (1977).

NRE: In his Nobel prize lecture, Clive gives an amusing account of that discussion: "A colleague, David Hendry, stated that the difference between a pair of integrated series could be stationary. My response was that it could be proved that he was wrong, but in attempting to do so, I showed that he was correct, and generalized it to cointegration, and proved the consequences such as the error-correction representation." (Granger, 2004, p. 363).

𝔻𝔽ℍ: Clive's development of cointegration also resolved the debate between modeling in levels and modeling in differences, as I discuss in Hendry (2004).

NRE: We already knew something about working in differences and in levels from the equilibrium correction models in Denis Sargan's (1964) chapter of the *Colston Papers*. A decade prior to Denis's paper, Bill Phillips (1954) had analyzed integral, proportional, and derivative control in formulating policy—also an equilibrium correction framework. An even earlier precedent is Bradford Bixby Smith (1926), a paper re-discovered by Terry Mills (2011).

2. Development of pertinent econometric tools

NRE: In addition to analyzing predictive failure, you developed new econometric tools, focusing on exogeneity, mis-specification analysis, and encompassing. Because these tools helped clarify issues on forecasting, let's look at these tools, starting with exogeneity.

2.1. Exogeneity

NRE: In the 1970s, you, Rob Engle, and Jean-François Richard reinterpreted the concept of exogeneity, later published in Engle, Hendry, and Richard (1983). You subsequently applied that framework to feedback versus

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