A Pro-Econometrics Tract:
Modern Time Series Methods and the Friedman-Schwartz/Hendry-Ericsson Debate

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Abstract

This paper considers David Hendry and Neil Ericsson's 1991 critique of the econometric approach of Milton Friedman and Anna Schwartz's 1982 book on monetary trends. Hendry and Ericsson advanced their critique from the perspective of the general-to-specific methodology. But a major message that arises out of this paper's analysis of their critique is that the key matters on which they took issue with the Friedman-Schwartz study—including the book's poorly justified technique of phase averaging as a means of filtering data and its use of specific-to-general estimation procedures—were "cross-methodological" points that would likely be widely shared among adherents to modern (by 1980s standards) time-series econometrics. Hendry and Ericsson's description of their work as "a pro-econometrics tract" was therefore appropriate.

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1. Introduction

Hoover (2003, pp. 421–422) observed that in a paper whose first release was "long before its publication, David Hendry and Neil Ericsson (1991[a]) launched a stinging attack on the empirical adequacy of Friedman and Schwartz's [1982a] *Monetary Trends*." That Friedman-Schwartz/Hendry-Ericsson exchange is the subject of this paper. An extensive analysis is provided of Hendry and Ericsson's (1991a) appraisal of *Monetary Trends in the United States and the United Kingdom*—an appraisal that concentrated on an equation in that book describing the demand for U.K. broad money—as well as Friedman and Schwartz's reply, which appeared alongside the Hendry-Ericsson article in the March 1991 issue of the *American Economic Review* (*AER*). The focus is on econometric aspects of the debate. The analysis puts the debate in the context of Friedman and Schwartz's and Hendry and Ericsson's respective approaches to empirical work as well as developments taking place in the field of time-series econometrics during the 1970s and 1980s.

A key message that emerges from the analysis given in this paper is that the econometric criticisms put forward by Hendry and Ericsson (1991a) were largely vindicated. The defiant, and seemingly thoroughly unpersuaded, posture taken by Friedman and Schwartz (1991) did not prove to be well grounded. Importantly, posterity has not validated the econometric aspects of the 1991 Friedman-Schwartz rebuttal. As Hendry and Ericsson (1990) noted soon after Friedman and Schwartz penned and made public their reply, and as Ericsson, Hendry, and Prestwich (1998a, 1998b) detailed in print some years after the 1991 *AER* exchange, many of the factual and analytical positions taken by Friedman and Schwartz in their 1991 piece were incorrect. To take some of these (which, together with others, will be discussed in detail later in this paper): Friedman and Schwartz (1991) claimed that the rate-of-change money demand equation that they had reported in 1982 fit better than Hendry and Ericsson's (1991a) error-correction equation; that the residual standard deviations of the levels regressions under discussion were not in units comparable to those of the estimated error-correction equations that were also under discussion; that having inflation on the right-hand-side of any real-balances equation, including in Hendry and Ericsson's (1991a) preferred specification, put into question the equation's status as a money demand function; and that their own 1982 levels equation (describing the demand for U.K. real broad money balances) fit the historical data better than had Hendry and Ericsson's levels equation. All of these contentions were incorrect.

Friedman and Schwartz's (1991) rebuttal made further claims whose validity rested on statistical and empirical approaches that had been used in the economic-research literature to some extent in the past but that, by the 1980s, had been superseded and had definitely become outmoded. Specifically, their positions on temporal data transformations and the econometric handling of two-way interactions between economic series reflected 1930s and 1940s National Bureau of Economic Research (NBER) and pre-World War II statistical traditions in which Friedman and Schwartz were steeped but that had failed to become accepted as part of the modern macroeconomic and econometric consensus.

In particular, Friedman and Schwartz maintained that phase averaging (averaging annual data in a manner intended to capture particular time series' behavior during a recession or expansion as a single observation) provided a useful means of obtaining data on longer-term values of economic variables. They also contended that alternating the regressor and dependent variable in successive estimated regressions provided a means of allowing for simultaneity between the economic series in question—and, drawing on the same (tenuous) line of reasoning, they suggested that valid parameter estimates (or reasonable upper and lower bounds of these) could be obtained, via these successive regressions, in the presence of simultaneous determination of the variables. Friedman and Schwartz further postulated that the usual calculation (as embedded in regression software, and as given in econometrics textbooks) pertaining to the residual standard deviation (that is, an ordinary least squares regression's standard error of estimate [SEE]) became incorrect once the regression included lags of the dependent variables—and that, in the presence of such regressors, the usually-calculated SEE number needed to be scaled up by what was, typically, a large factor. As this paper will discuss, these suggestions were out of step with modern research, and they had not become assimilated into standard empirical macroeconomic analysis and time series econometrics. Consequently, not only Friedman and Schwartz's book but also their 1991 reply underscored Hendry and Ericsson's (1983, p. 48) warning about "the practical dangers of seeking to analyze complex stochastic processes while eschewing modern econometric methods."

This paper's analysis confirms the point made by Hendry and Ericsson (1983, p. 28; 1985, p. iii) that their "paper… is a pro-econometrics tract." It is argued at length below that the Hendry-Ericsson 1983–1991 critiques were "pro-econometrics" not just on the dimension of applying much more rigorous econometric procedures than those deployed by Friedman and Schwartz (1982a) but also, importantly, by virtue of the fact that the

Hendry-Ericsson papers articulated many criticisms, notably of phase-averaging, that reflected positions on empirical analysis that would be shared by numerous econometricians of the 1980s and later decades—including many who would not be regarded as subscribers to the general-to-specific methodology advocated by Hendry. One way of articulating this point is to suggest that, in terms of the perceived debate underway during the mid-1980s on macroeconometric methodology between David Hendry (located then, as now, at Oxford University) and Christopher Sims (then based at the University of Minnesota), the Hendry-Ericsson criticisms of *Monetary Trends* were. in good part, *cross*-methodology points: matters on which the different, but both "pro-econometrics," Hendry and Sims outlooks on time series analysis would be in agreement.

In this connection, the context in which Hendry and Ericsson (1985, p. 11) cited Sims (1980) is notable and appropriate.[1] They pointed to Sims (1980) and, by implication, the ensuing vector autoregression (VAR) literature as supportive of the broad approach to time series—in particular, one that did not impose severe restrictions on dynamics at the start of the estimation exercise—that shaped their own critique of *Monetary Trends*' empirical work on the demand for money.

This paper proceeds as follows. Section 2 outlines Friedman and Schwartz's (1982a) U.K. money demand equation that was the center of the debate. Section 3 discusses in detail two key areas of common ground shared by the two sides of the debate. Section 4 analyzes some key developments in econometrics that helped shape subsequent developments and that stalled Hendry and Ericsson's efforts to develop a system of equations that could be an alternative to the analysis of the U.K. dataset in *Monetary Trends*. Section 5 outlines the key econometric issues raised in Hendry and Ericsson's critique, focusing on their 1991 *American Economic Review* article. Section 6 considers Friedman and Schwartz's reply in the same issue of the *AER*. It is contended that many of Friedman and Schwartz's statements in rebuttal were incorrect or did not strengthen their case. Section 7 concludes.

Two limits on the scope of this paper deserve note. The first is that the implications for

[1] Further discussion of the contrasts and differences in the Hendry and Sims approaches will be made in the course of this paper when they shed light on the Friedman-Schwartz/Hendry-Ericsson debate. Studies that contrasted Hendry's approach to econometrics with other methodologies included Pagan (1987) (who included consideration of a third approach, that of Edward Leamer) and Faust and Whiteman (1997) (who considered the Edward C. Prescott or real business cycle [RBC] school as offering the third leading alternative among current approaches to empirical macroeconomics).

monetary economics of the debate are touched on only in the conclusion. These implications are considered in detail in Nelson (2024). That paper also considers pre-1991 and post-1991 developments in the debate not examined in detail here.

A second limitation that needs to be elaborated upon at the outset is that the discussion in this paper does not consider in detail the econometric concepts of exogeneity and the implications drawn by proponents of the general-to-specific procedure for econometric identification, valid conditioning (which, roughly speaking, refers to the appropriate circumstances in which variables dated *t* can appear on the right-hand-side of an equation estimated by ordinary least squares [OLS]), and the Lucas critique. Restricting the coverage in this paper in this way keeps the scope of this paper manageable. In addition, the arguments about exogeneity, valid conditioning, and the Lucas critique have been subject to considerable discussion in prior research, including the challenges by Leeper (1995), Bruce Hansen (1996), Faust and Whiteman (1997), and Linde (2001).[2] The existence of these contributions reduces the need for coverage here. Those prior discussions also highlight the fact that the issues covered in this literature are largely separate from those that were at stake in the Friedman-Schwartz/Hendry-Ericsson debate. Although Ericsson and Irons (1995) used Hendry and Ericsson's (1991a) money demand model and Friedman and Schwartz's (1982a) annual data in a paper focused on exogeneity and Lucas-critique matters, other contributions in this area—including those just cited, as well as Favero and Hendry (1992) and many of the contributions collected in Ericsson and Irons (1994)—concentrated on other empirical applications and tended to mention the Friedman-Schwartz/Hendry-Ericsson debate only in passing.[3] The coverage of exogeneity in this paper is therefore limited.

2. The equation under debate

Milton Friedman and Anna Schwartz's book *Monetary Trends in the United States and the United Kingdom: Their Relation to Income, Prices, and Interest Rates, 1867–1975*

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[2] See also Lubik and Surico (2010) for more recent work in this area.

[3] The omission of coverage of issues concerning super exogeneity, in particular, is consistent with the focus in this paper on the numerous aspects of the Hendry-Ericsson critique that would be endorsed by econometricians who were not subscribers to the general-to-specific methodology. The fact that matters concerning exogeneity are not a strong area of agreement across econometric methodologies is brought out by the fact that, whereas Ericsson and Irons' (1994) book was titled *Testing Exogeneity,* Stock and Watson (2003, p. 352) contended that "it is impossible to develop a statistical test of the hypothesis that the instruments are in fact exogenous." This suggests that, although the general-to-specific methodology's concept of weak exogeneity has received acceptance across methodologies (see, for example, Watson, 1994, p. 2887), there has been more resistance to the concept of super exogeneity.

covered both the United States and the United Kingdom. However, the Hendry-Ericsson study—the background for which is extensively discussed in Nelson (2024)—had its origin in a Bank of England request to Hendry to examine only the U.K. part of the work. In 1983, Hendry and Ericsson planned a thoroughgoing "econometric appraisal" of *Monetary Trends* that would seek to model all the U.K. variables in the Friedman-Schwartz annual U.K. dataset. As discussed in Section 4 below, however, Hendry and Ericsson's plans to carry out such a system analysis were scuttled by advances in the econometric analysis of time series that took place in the 1980s. In the end, both in 1983 and later, the authors focused on *Monetary Trends*' equation describing the demand function for U.K. broad money (roughly speaking, an M2-type aggregate). This focus was reflected in the eventual title of the published Hendry and Ericsson (1991a) paper indicating that the paper consisted of an "econometric analysis of U.K. money demand."

In order to set the scene for the later discussion, this section lays out the *Monetary Trends* U.K. money demand discussion that was at the center of the 1991 debate. Estimated on Friedman and Schwartz's "phase-average" data, this equation was (see Friedman and Schwartz, 1982a, Table 6.14, p. 282):

$$m_j - n_j - p_j \;=\; \underset{(t\,=\,0.08)}{0.16} \;+\; \underset{(t\,=\,18.13)}{0.88}\,(y_j - n_j) \;\underset{(t\,=\,3.42)}{-11.16}\,RN_j \;\underset{(t\,=\,0.74)}{-0.22}\,g_{Yj} \;+\; \underset{(t\,=\,2.38)}{0.014}\,WD_j \;+\; \underset{(t\,=\,7.56)}{0.21}\,S_j \quad (1)$$

R^2 = 0.970, 36 phase-average observations (drawn from 1873–1975 annual data).

Table 1 gives notation used in this equation and other acronyms used later.

Equation (1) is in per capita terms, expresses the interest-rate opportunity-cost variable (RN) in terms of a short-term interest rate adjusted for an own rate on money, uses nominal income growth (g_Y) as a proxy for nonfinancial alternatives to money, and allows for world wars by a dummy variable (WD) that focuses on the postwar restoration to normal levels of real balances, rather than on the wartime behavior of money holdings.

Hendry and Ericsson would differ from Friedman and Schwartz on these specification choices. They would not include population and nominal income growth in the money demand specification, would associate the war-related dummies with the specific years of combat, and would use the raw short-term interest rate, RS, as the financial opportunity cost of money. Nevertheless, none of these differences would be the focus of the Hendry and Ericsson (1991a) critique, and so they are not considered in detail in what follows.

Table 1. Notation and acronyms	
M	Level of U.K. broad money (M2)
m	Log of U.K. broad money
p	Log of U.K. price level
y	Log of U.K. real national income
RS	Level of U.K. short-term interest rate
RN	Friedman and Schwartz (1982a) first opportunity-cost variable (RS adjusted indirectly for own-rate on money balances)
g_Y	Friedman and Schwartz (1982a) second opportunity-cost variable (growth rate of U.K. nominal income—proxying the nominal return on physical assets)
n	Log of U.K. population
Δ	First-difference operator
Δ^2	Second-difference operator
$I(0)$, $I(1)$	Integrated of order 0, 1 (respectively)
j (subscript)	Time subscript for phase-average observation
t (subscript)	Time subscript for annual observation
S	Intercept dummy variable covering interwar and early post-World War II observations (spanning years or phases over 1921 to 1955)
WD	Intercept dummy variable, intended to capture restoration of velocity to values prevailing before each world war
L	Lag (backshift) operator
rs	Log of RS
rl	Log of U.K. long-term interest rate
ADL	Autoregressive distributed lag
ECM	Error-correction mechanism
QTM	Quantity theory of money

Equation (1) also has an intercept shift S, associated mainly with the interwar observations. Friedman and Schwartz's conception of the long-run money demand function being constant was predicated on the inclusion of this shift dummy. This means that the constancy that they envisioned was conditional on a postulated shift in money demand associated with interwar economic uncertainty. The inclusion of the shift term in Friedman and Schwartz's U.S. and U.K. regressions generated critical remarks in some of the initial 1982–1983 reviews of *Monetary Trends* and in Hendry and Ericsson (1983). However, as discussed in Nelson (2024), it is likely that the backlash generated by a term like *S* would be less today than was the case in the mid-1980s. In the intervening years, the inclusion of intercept dummies has become less controversial, owing to greater acceptance in applied macroeconomics that infrequent intercept shifts represent a more contained, and so more benign, form of parameter non-constancy than shifts in slopes. This change in view is reflected in Bai, Lumsdaine, and Stock's (1998) contrast between permanent shocks and what they call a "full structural change" (p. 401) and their remark (p. 395): "Because the permanent shocks occur so rarely, it is useful to model them as one-time events."

In fact, Friedman and Schwartz's *S* variable is a less pervasive source of shift than those that Bai, Lumsdaine, and Stock (1998) contemplated: the *S* variable implies a temporary level shift (and so does not enter the cointegrating vector) whereas the "permanent shock" that Bai, Lumsdaine, and Stock contemplate enters the cointegrating vector and might lastingly raise the level and even growth rates of series. The *S* variable is discussed further below, but it deserves underlining that Hendry and Ericsson's critique of the *Monetary Trends* specification (1) was primarily focused on points other than whether *S* should have been included in the equation.

To get to the key issues in the Hendry-Ericsson critique of equation (1), it will be useful to specify a couple of areas of apparent agreement on econometric matters between Friedman and Schwartz and Hendry and Ericsson. Isolating those areas of agreement helps spotlight the major differences, because the proposed strategy for dealing with what was agreed to be a problem differed across the two sets of authors. The next section documents these points.

3. Common ground of the two sides

Even if one puts aside their glaringly flawed procedure, discussed in detail in Section 5, of phase-averaging the data, it is straightforward to see reasons why the way in which Friedman and Schwartz arrived at equation (1) above in *Monetary Trends*, as well as the specification itself, would come under criticism from the vantage point of the general-to-specific methodology expounded by Hendry and Ericsson (1983, 1991). For example, Friedman and Schwartz undertook specific-to-general modeling; made no attempt to model short-run dynamics other than by trying to abstract from it through phase averaging; presented few diagnostic test results regarding their estimated specifications; and, in their discussion of exogeneity, did not dwell on the econometric requirements for a variable to be classified as exogenous.[4]

But an important point that may be obscured by the sharp conflict between their approaches is that, on empirical work, there were two notable areas of *agreement* between Friedman and Schwartz and Hendry and Ericsson. The two key matters—the importance of empirical evidence; and disdain for methods, such as prewhitening, associated with the Box-Jenkins approach—on which they broadly concurred can be discerned from examining each side's body of writings. These two points of agreement are now discussed in the context of the research backgrounds of the debate participants. As this analysis will make clear, the broad agreement on the two matters considered did not invariably extend to details, with each side advancing different answers on what was the appropriate response to what was agreed to be a problem.

3.1 Emphasis on evaluating theories by empirical testing

Hendry and Ericsson (1983, p. 47; 1985, p. 1) quoted with approval Friedman's (1953a, pp. 8–9) remark that "the only relevant test of the *validity* of a hypothesis is comparison of its predictions with experience. The hypothesis is rejected if its predictions are contradicted…"

The essay on research methodology in which Friedman made this statement covered both microeconomics and macroeconomics. The essay's writing, however, took place when Friedman was producing his earliest monetarist work, while he was beginning his NBER project on money with Anna Schwartz.[5]

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[4] They also did not provide comprehensive testing of parameter restrictions. However, Friedman and Schwartz (1982a, Chapter 5) did test some restrictions and, as discussed in Section 5 below, in one instance they also provided constancy tests. Consequently, Hendry's (1985, pp. 76–77) statement, "A remarkable feature of their book is that none of the claims was actually subjected to *test*," requires qualification, even though the statement was treated as an unconditional fact by Smith (1987, p. 150). Hendry and Ericsson's (1991, p. 15) position was the more qualified one that "Friedman and Schwartz tested for trends and price homogeneity in more restrictive models" than the general unrestricted framework that would have been a more appropriate venue for such testing.

[5] More precisely, Friedman joined Anna Schwartz's preexisting project.

In Friedman's monetarist work, the important role of empirical testing of hypotheses was reaffirmed. The desire to pit the predictions of Keynesian theory and of the QTM against one another in an empirical analysis motivated the launch of Friedman's project with David Meiselman at the University of Chicago in the mid-1950s. The same motivation permeated the resulting Friedman-Meiselman (1963) study and was manifested in the critical remarks that appeared in that paper on the prior dearth of testing of Keynesian models' predictions concerning fiscal policy against the evidence (see Nelson, 2020a, p. 358; see also Nelson, 2020b, Chapter 12).[6] Another manifestation of this perspective—this time on the part of Anna Schwartz—came in Schwartz (1984, p. 134). In a comment (given at a September 1981 conference on monetarism, held in the United Kingdom) on Budd, Holly, Longbottom, and Smith (1984), Schwartz remarked: "I do not know why 'views on the relative sizes of the interest elasticity of demand and the income elasticity of demand for money provide an important distinction between monetarism and non-monetarism.' There should not be 'views.' There should be evidence."

Similar sentiments appeared in Hendry and Ericsson's work. In the case of Hendry, this included his famous conclusion in Hendry (1980, p. 403): "The three golden rules of econometrics are test, test and test."

A basic belief in the value of empirical evidence was therefore subscribed to by Friedman and Schwartz and by Hendry and Ericsson. But this agreement between the pairs of authors did not extend to a shared position on how empirical research should be conducted and, notably, on the primary basis on which a model should be considered "tested." In the case of David Hendry, as is well known, the testing steps that were regarded as vital included not only ensuring that the parameter restrictions needed to obtain the preferred specification were valid, but also making sure that the chosen equation passed a set of statistical tests. This perspective, laid out in detail in Campos, Ericsson, and Tran (1990) and Campos, Ericsson, and Hendry (2005), was encapsulated in Hendry's (1987, p. 38) statement: "The evaluation is the justification."

With regard to Friedman's own position on testing, some comments on his posture toward econometrics and his attitude toward deciding between hypotheses are in order here, in order to provide adequate context. Beyond what follows, further discussion of these matters can be found in Nelson (2020a, Chapters 3 and 9).

[6] See Tavlas (2023) for a further detailed study of the Friedman-Meiselman project.

A useful jumping-off point is Hendry's (2002, p. 596) remark: "Of course, Milton Friedman would place little faith in significance tests—they systematically reject his models." In making this statement, Hendry was responding to a misguided attempt to treat Friedman as an authority on how econometrics should be used in applied work. Friedman certainly was not such an authority—so a sharp response on Hendry's part was understandable. Nevertheless, Hendry's implication (perhaps advanced somewhat facetiously) that Friedman's downplaying of test statistics was a response to adverse findings regarding monetarist modeling does not line up with the timing of Friedman's disaffection with formal macroeconometrics. Indeed, as will be seen, despite his own immersion in statistical work during the 1930s and 1940s, Friedman's reservations regarding macroeconometric modeling and test statistics that focused on in-sample performance of models were on record as early as 1940—before he became a monetarist.

Hirsch and De Marchi (1990, p. 230) suggest that the focus on historical episodes in the *Monetary History* research (which was largely carried out in the 1950s) by Friedman and Schwartz moved Friedman "away… from the notion that a conclusive statistical (numerical) test is possible" in ascertaining what theoretical framework best characterized the data. This chronology, too, is misplaced, as Friedman's skepticism about whether statistical tests were the crucial means of distinguishing between macroeconomic theories dated back to the start of the 1940s—when, as already noted, he was still heavily working in statistics.

It should be stressed that this skepticism was especially directed toward *macroeconomic* applications of statistics and econometrics. So when one considers Friedman and Kuznets' (1945, p. 426) statement, "These conclusions should be tested empirically," and the fact that the book in which this statement appeared (*Income from Independent Professional Practice*) did extensively use statistical tests, one must keep in mind that the Friedman-Kuznets study was *microeconomic* in its content. Starting especially in the mid-1940s, Friedman had much more confidence in microeconomics or price theory as containing a settled body of analysis than he had in macroeconomics (see Nelson, 2020a, Chapter 9).[7] Correspondingly, Friedman's continued heavy usage of statistical testing in his microeconomic work in the 1940s went alongside doubts on his part about testing and estimation in a macroeconomic context—doubts that had set in by the start of the decade.

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[7] This attitude was consistent with the confidence in QTM propositions that Friedman had acquired by the end of the 1940s, as that confidence went hand in hand with a negative verdict on the extent to which economists had solid knowledge about short-run dynamics.

In the colorful account that he gave in Friedman (1986) and in the solo-authored addendum to Friedman and Schwartz (1991) of his becoming disillusioned with statistical tests and acquiring a more circumspect view about econometrically estimated parameters, Friedman dated his disaffection to an experience in 1943–1944, when a regression that he used when working in metallurgy failed to provide the basis in practice for a reliable alloy. This incident may well have significantly undermined Friedman's confidence in the employment of regression analysis in microeconomic and experimental contexts. But, as stressed in Nelson (2020a, Chapter 3), it cannot account for his corresponding skeptical attitude toward regressions that used macroeconomic data. That attitude certainly was in force earlier. After an initial enthusiasm for the possibilities of macroeconometric work, he put his skepticism on record in his review of Tinbergen (1939). That review (Friedman, 1940) anticipated the remarks in Friedman and Schwartz (1991) in suggesting that an econometrician working on a specific data sample would often likely be capable of arriving at a successful in-sample specification, only to find that it fit poorly outside the estimation period.[8] This sentiment was echoed in his observation at the 1950 NBER conference on macroeconomic modeling (Friedman, 1951, p. 108): "the fact that the equations fit the data from which they are derived is a test primarily of the skill and patience of the analyst; it is not a test of the validity of the equations for any broader body of data."

Those conference remarks were certainly no strong endorsement of macroeconometric modeling. But—reflecting the fact that Friedman was really pointing toward the limitations, not the futility, of regression analysis—neither were they a repudiation of such modeling. And they were not out of step with the views of many applied economists—a situation reflected in Roose's (1952, p. 413) approving quotation of Friedman's (1951) words.[9]

The notions that out-of-sample performance was an important part of the evaluation of a model, and that in-sample success should be discounted considerably, would be reflected in the title of the classic study of Meese and Rogoff (1983), "Empirical Exchange Rate

[8] For example, Friedman (1940, p. 659) stated that Tinbergen's equations were "selected after an extensive process of trial and error *because* they yield high coefficients of correlation" and "Tinbergen makes no attempt to determine whether his equations agree with data other than those which they translate."
[9] The fact that Friedman's sentiments were widely shared has, of course, the corollary that Friedman's (1951) observations in this connection were not especially novel ones to articulate. They were, and continued to be, recurrently expressed by many economists. Friedman (1940) himself had quoted Wesley Mitchell (1927) criticizing overfitting but did so in a manner that, in turn, implied that the stand taken by Mitchell on this matter was a commonplace one.

Models of the Seventies: Do They Fit Out of Sample?" Nor were these notions really very out of line with much of David Hendry's work. Although Hendry did, of course, put considerable emphasis on in-sample adequacy (as judged by diagnostic tests) of an econometric specification, he also noted in Hendry (1995, p. 557) that "whether design was deliberate or inadvertent, the only genuine test of a model is against evidence that was not available when the model was developed."

Friedman's suggestion in the 1951 quotation given above that the "validity of the equations for any broader body of data" amounted to the crucial test of the estimated equations was consistent with the fact that, notwithstanding his doubts about macroeconometrics, he continued during the 1950s to estimate econometric equations and oversee and encourage others' empirical econometric work. He also viewed himself as working in tandem with specialized econometricians in the sense that, despite his reputation as a critic of the multi-equation modeling approach of the Cowles Commission (later the Cowles Foundation), he saw NBER research, such as his and Schwartz's money, as contributing to a profession-wide effort to build an appropriate structural model, by adding to the volume of information available on, and economic analysis of, particular sectors of the economy.[10] And via the University of Chicago's money workshop, Friedman oversaw much econometric work by students that was intended to test various key monetary propositions on different countries' datasets.

In pursuing his own research program in monetary economics, Friedman over the course of the 1950s and into the early 1960s had a two-track agenda of historical-episode analysis and regression analysis. The work with Schwartz involved both aspects, though the bulk of their research in the 1950s consisted of the data-oriented, but not regression-focused, "analytical narrative" of the monetary-history work. In contrast, in his collaboration with David Meiselman starting in the mid-1950s, Friedman had an econometrics-centered investigation of the merits of the QTM and of Keynesian fiscal-multiplier theory.

The Friedman-Meiselman work also exhibited three further features of Friedman's econometrics that interacted with his conception of testing. The first of these was the low-parameter-number character, or parsimony, of the regressions considered. Friedman

[10] Friedman's attitude here paralleled Mizon's (1995, p. 108) later motivation for the general-to-specific procedure's traditional focus on single equations in terms of "discover[ing] incrementally parts of the underlying structure."

had very mixed feelings about all-in-one regressions that nested different hypotheses (see Nelson, 2020a, p. 185). Friedman and Meiselman focused on simple regressions that had spending as the dependent variable and a monetary or fiscal variable as the regressor. They did not regard the underlying whole macroeconomic *model* as consisting of one equation: they were amenable to viewing their regression specifications as the reduced form of a multiple-equation system. But they evaluated Keynesian and quantity-theory models using single-equation, parsimonious regressions.

Friedman and Meiselman did report equations in which two regressors (a fiscal variable and a monetary variable) appeared (along with a constant term).[11] But their interest in this multiple regression was partly motivated by the possibility that a model could imply that *both* fiscal and monetary variables were relevant for aggregate spending—or, conceivably, that the relevance of a regressor might depend on whether one was considering real or nominal series (Friedman and Meiselman, 1963, pp. 177, 205; Friedman, 1971, p. 330). That is, even in this case, the multiple regression was not necessarily seen as being of interest because it allowed scope for the nesting of different hypotheses: instead, it was perceived as possibly representing a wider, single model. And in any event, multiple regressions did not take center stage in their article. As indicated by the title of the Friedman-Meiselman study, the focus was on the properties of the simple regressions estimated (specifically, its slope parameter and the correlation coefficient—the latter statistic, of course, being the square-root of R^2 in the simple-regression case).[12]

This desire for parsimony was manifested not only in the Friedman-Meiselman research but also in Friedman's empirical implementation of his work on money demand. Friedman's (1956) money demand specification actually implied a larger number of arguments in the demand function than in most existing specifications. In empirical implementation, however, Friedman and Schwartz (1970, p. 139) favored a "fairly

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[11] They also reported regressions that used the price level as a third regressor.

[12] Notably, SEEs (the $\hat{\sigma}$'s in the notation of many Hendry and Ericsson papers) were not reported in Friedman and Meiselman (1963). Their regressions' SEEs became an area of contention when Ando and Modigliani's (1965) challenge called for the (squared) SEE to be the criterion used in comparing the fit of rival equations. Friedman and Meiselman (1965, p. 765), in reply, made the elementary but important point that, for a given left-hand-side variable and sample period, the adjusted R^2 and SEE had a direct, continuous relationship. (One of the compelling reasons for using the SEE invoked by Hendry [1983, p. 200; 1993, p. 450], and Hendry and Ericsson [1991a, p. 12]—that, if the dependent variable is a log or log-change variable, the SEE can be interpreted directly as being in percentage units—did not apply in the 1965 debate, as the authors on both sides were using unlogged levels in their regressions.)

simple" specification, citing concerns about overfitting: "there is always a stable function for anything, if the number of variables included can be indefinitely large." In *Monetary Trends*, as earlier noted, this desire for parsimony was reflected in condensing Friedman's multiple-yield money demand function into a two-yield empirical money demand equation.

The second facet of the Friedman-Meiselman research relevant here was the limited role given, in discriminating between hypotheses to, statistical tests. Despite the use of nesting as part of the study, and so of regressions that might center the contest between hypotheses on a *t*-test, Friedman and Meiselman mostly concentrated on other regression output. This was consistent with their regarding their task as primarily one of making a bilateral comparison of two distinct, separately estimated specifications of the behavior of aggregate spending. Similarly, ahead of comparing his own theory of consumption with older Keynesian theories in *A Theory of the Consumption Function*, Friedman indicated to readers that his book was characterized by an "almost complete absence of statistical tests of significance" (Friedman, 1957, p. ix). The consumption-function study instead favored an approach that was also prevalent in Friedman's other work. This was one of applying the same preferred regression to a different sample or different classes of data: for example, ascertaining the agreement of macroeconomic models' predictions with profits data, as in Friedman (1940), or the consistency of time-series and cross-sectional results, as in Friedman (1957).[13]

It was in the context of the evaluation of the same specification against successive, different datasets that Friedman often used the term "test." He took that perspective with regard to *Monetary Trends* even before he and Schwartz introduced the United Kingdom into the coverage of the study. "We do not rely very extensively on formal tests of significance, but on the consilience of results for a variety of different series or groups of data," Friedman explained to an NBER colleague after the latter had read the 1967 draft of *Monetary Trends*.[14] Correspondingly, in a paper spun off from the final version of *Trends*, Friedman and Schwartz (1982b) used the term "test" in this context. Referring to

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[13] Ericsson, Hendry, and Hood (2016, p. 134) suggest that Friedman's approach was to "fit simple models that are checked on subsamples" and cite Friedman (1940) in that light. Subsample analysis was a yardstick that Friedman used. But so was evaluating equations out of sample and by use of different types of data from those used in estimation. The cross-check that Friedman (1940, p. 660) carried out was an example of this attitude: he used an out-of-sample test and used data centered in the corporate sector as a means of evaluating an economy-wide model.

[14] Letter to Solomon Fabricant, February 2, 1967, p. 1, Anna Schwartz files, inspected by the author in 2007.

the fact that they were going to follow up an existing study of quarterly money demand, which had used new term-structure data, by using the corresponding data as regressors to their own phase-average specification, the authors stated (p. 201): "The success of that test [the regressions in the earlier work] induced us to make a similar test."

It is clear, therefore, that Friedman's preferred usage of the term "test" was in the context of asking whether one observed the same phenomenon in different samples and so could, for example, obtain similar estimates of econometric equations across countries or in the same country over time. Along these lines, in a 1962 letter to colleagues overseas, he observed regarding the findings of monetary studies in which he had been involved: "Although these were derived from experience in the United States, it has seemed to me that one of the most effective ways of testing them would be to see whether hold also for other countries and particularly for countries with a different monetary background."[15] He alluded to this broad conception of testing by occasionally using the specific term "statistical test" in reference to formal econometric testing of a parameter restriction in a regression equation (for example, Friedman and Meiselman, 1963, p. 177).

Despite, therefore, their common interest in empirical testing, Friedman's conception of what was a test, and the procedure by which alternative hypotheses, obviously differed from Hendry's. It was not really as extreme a difference as Friedman and Schwartz (1991, p. 39) made it out to be in their statement that Hendry and Ericsson favored centering the analysis on "a single hypothesis (equation), however complex, supposedly 'encompassing'… all subhypotheses." Hendry and Ericsson did view including all variables implied by various rival economic hypotheses in a single initial regression as undesirable in some circumstances. In this connection, Hendry (1986, p. 207) spoke out against "vacuous 'monster models' which embed all existing relationships in a mindless formulation," while Ericsson's research as a graduate student of Hendry's had concerned non-nested models (see Ericsson, 1983). Nevertheless, the gulf between Friedman and Schwartz, on the one hand, and Hendry and Ericsson, was large on these matters—as the discussion of specific-to-general methods in Section 5 below will highlight.

A third notable aspect of the Friedman-Meiselman project that was prevalent in Friedman's other monetary work was the focus on OLS.[16] The OLS orientation partly

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[15] From Friedman's 1962 letter to overseas academics, quoted in Friedman and Friedman (1998, p. 279).
[16] Or, more precisely, OLS and weighted least squares—both of which imply that all the right-hand-side variables in the regression "instrument for themselves."

reflected the fact that Friedman was out of step with the state of the art in econometrics from a very early stage in his career. Accounts like Bhagwati (1977, p. 225) actually identified Friedman by name with the perceived lag in the technical rigor of teaching at the University of Chicago in the late 1950s and 1960s behind that of other institutions.[17] It is unlikely, however, that Friedman had a dominant influence on the tone of the economics department—able to enforce his will on colleagues' teaching and research—in the manner implied (see Nelson, 2020b, Chapter 11).

It is not disputed, however, that, by the standards of other leading universities in the late 1950s, econometrics at the University of Chicago in that period was of a low level of technical advance. The perception that the economics department was deficient in the area of econometrics was underlined when Franklin Fisher—who would become a leading authority on simultaneous equation estimation—and J. Denis Sagan, later Hendry's dissertation adviser, visited the department in 1959/1960. Fisher, who shared an office with Sargan during the months when both were visiting department members, remembered that the default situation that they encountered on arriving was that "the graduate students were not taught anything beyond least squares, [though] Sargan certainly would have gone beyond that… About that, [Carl] Christ said, 'Well, you know, the students are really not up to [grasping] that.' A weakness."[18] Fisher did not indict Friedman specifically for this situation. But he suggested that Friedman's attitude "didn't help" and faulted him along with colleagues for not putting more effort into hiring econometricians, including Zwi Griliches, when the opportunities to do so arose (Franklin Fisher, interview, January 16, 2015).[19]

In *Monetary Trends*, Friedman and Schwartz themselves concentrated on OLS-related methods. This aspect of their work would not in itself be a major impetus for Hendry and Ericsson's challenge, as the latter authors' critique likewise primarily used OLS. But the focus on OLS in *Trends* in part reflected Friedman's lack of background in simultaneous equation econometrics—as well as his having become acquainted with regression

[17] Bhagwati focused on the lack of mathematical economics but implied that this spilled over into the treatment of econometrics in the department.
[18] Significantly, Sargan valued the experience partly because of the presence of other visitors, like Fisher and Zwi Griliches, who were "interested in talking to about econometrics" (quoted in Phillips, 1985, p. 125).
[19] Fisher added: "Now, I should explain that I was there at the nadir of the department. In a couple of years, it was getting considerably better. Both in econometrics, which they were beginning to actually teach—teach something beyond least squares—and also in theory." (Franklin Fisher, interview, January 16, 2015.)

analysis largely through a non-economics-oriented, statistical tradition, whose manner of dealing with identification problems was dissimilar to that of much postwar econometrics.[20] This perspective likely led him to be overconfident about the degree to which two-way interactions between variables could be handled within an OLS framework. That overconfidence, in turn, would be reflected in Friedman and Schwartz's (1991) strong claims, discussed later, about what could be accomplished by running regressions "both ways."

3.2 Critical attitude toward the application of Box-Jenkins methods

Hendry (1985, p. 73) contrasted econometric modeling with "purely extrapolative forecasting devices (such as the well-known Box-Jenkins methods)" and noted that those methods provided a baseline match with the behavior of economic series that econometric models had to surpass if they were to endure. By the time he wrote these words, Hendry had a long background of participation in the controversies on these matters in research circles—dating back to when these controversies had sprung up after the appearance of Box and Jenkins (1970) and the advent of commercially available software specializing in the estimation of the autoregressive moving average (ARMA) univariate models that Box and Jenkins advocated. With the reliance on Box-Jenkins procedures proliferating in economic research and forecasting in both the United Kingdom and the United States during the 1970s, Hendry was involved in the arguments on what were the implications for the future of structural econometric models. As he recalled in Hendry (1993, p. 117), "I became inadvertently embroiled in the 'time series versus econometrics' debate."

The Box-Jenkins procedures also became prevalent in monetary economics over this same period—to the chagrin of Milton Friedman. In January 1977, shortly after he had left for Northern California, Friedman complained (see Friedman and Modigliani, 1977,

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[20] This is not to say that pre-World War II statisticians who ran regressions were unaware that the orthogonality of the error term and the regressor was a necessary condition for the validity of their regression estimates. But they often had the benefit of an *a priori* basis for believing that the orthogonality condition indeed held (as might be the case in some instances of the deployment of experimental data). With respect to econometrics, Wonnacott and Wonnacott (1979, p. 8) argued: "When this 'simultaneous equation problem' was first recognized, it raised statistical problems that could not be solved even with the most advanced techniques used by mathematical statisticians." (Notably, Friedman would describe himself as working in mathematical statistics, along with economics, through 1946.) Wonnacott and Wonnacott went on to state: "So econometricians, for the most part, had to develop their own theory." See Stock and Trebbi (2003) for an important account of aspects of these developments.

p. 13) about the content of seminars held over the closing years at his previous workplace: "in our Workshop on Money and Banking at the University of Chicago, my major problem has been battling with the proponents of Box-Jenkins."

The ubiquity of Box-Jenkins procedures in the later years of Friedman's stewardship of the money workshop was reflected in the presentation, by regular workshop attendee Arnold Zellner, of "Time Series and Structural Analysis of Monetary Models" (a version of his paper with Franz Palm published later in the year in the *Journal of Econometrics*, "Time Series Analysis and Simultaneous Equation Econometric Models") on January 8, 1974. The Zellner-Palm paper made abundant use of the terminology "transfer function" (a formulation that Box and Jenkins, 1970, had used in reference to their limited extension of ARMA procedures to a multivariate setting)—something sure to grate on Friedman, in view of his aversion to introducing new jargon into economic research. The hard sell of Box-Jenkins procedures in the workshop series really came, however, from another workshop participant, Thomas Heagy, who was a graduate student in the business school of the University of Chicago. Heagy repeatedly presented his work at the money workshop, both during Friedman's tenure as head of it and for a little while afterward.[21] For example, on March 11, 1975, Heagy presented "The Effect of Monetary Innovations on National Income, The Price Level, and the Interest Rate." Then, on November 9, 1976—near the end of Friedman's time running the workshop—Heagy presented "The Long Term Demand for Money in the U.S.—A Time Series Approach." In a "Note to Reader" at the start of the paper (Heagy, 1976) associated with this talk, Heagy stated: "Throughout this paper, it is assumed that the reader is familiar with the basic concepts and notation of linear stochastic processes and multiple-input transfer functions *a la* Box-Jenkins."

Friedman had reason to be pleased by one aspect of the Box-Jenkins revolution: ARMA and ARIMA methods were being deployed to reveal the forecasting shortcomings of 1960s-vintage Keynesian macroeconomic models of which Friedman had been critical (see C.R. Nelson, 1972). Friedman felt, nevertheless, that Box-Jenkins methods were far too mechanical and that their widespread application was occurring at the expense of concern about the economic hypotheses under consideration.

In taking this stand, Friedman was on the same side of the "time series versus

[21] Very little of Heagy's copious research output ended up seeing print.

econometrics debate" as David Hendry. And Hendry was vastly more engaged in that debate than Friedman was. Whereas Friedman was involved, as he put it, in "battling" Box-Jenkins in his workshop in the mid-1970s, Hendry took the fight in a host of U.K. and U.S. research forums in the 1970s, doing so in print and in national and international research conferences. As will be seen, a pivotal contribution, focusing on statistical considerations, that he coauthored in 1972 helped set the stage for Box-Jenkins procedures to be superseded by vector autoregressions in empirical macroeconomic analysis. And, in the years after 1972, he also strongly argued against Box-Jenkins procedures for another reason: the need to retain structural economic relationships in dynamic modeling.

Although he would later be subject to frequent criticism that he embedded an inadequate amount of economic theory into his econometric work (see, for example, Svensson and others, 2002), the posture that Hendry took during the 1970s in the wake of the Box-Jenkins challenge was essentially one of making the case for the merits of letting economic theory inform empirical modeling. On this score, he was taking a position in the debate that was representative not only of the "median econometrician" but also that of many in the general economic-research field, including Friedman. Along these lines, Phillips and Loretan (1991, p. 407) credited "the methodology advocated by David Hendry" in the 1970s, especially its "key elements" of retaining levels terms as right-hand-side variables and ensuring that long-run economic relationships were implied by the equations, as an important part of the fightback against Box-Jenkins techniques.

In fact, as will now be discussed, the Hendry position in response to pure time-series approaches contained three key elements that macroeconomists embraced or would later come to embrace: objecting to the outright discarding of the use of economic theory in model specification; arguing that the modeling of series' dynamics did not, in practice, really require the inclusion of moving-average (MA) terms; and objecting to procedures that prewhitened the data considered in the analysis.

With regard to incorporating economic theory into empirical work, Hendry's activity advocating this practice, and so defending econometrics against the pure-time-series approach, included talks given during his 1975 spell in the United States. Most notably, he made remarks on the matter at the Federal Reserve Bank of Minneapolis conference "New Methods in Business Cycle Research," held on November 13–14, 1975, and was part of a panel discussion, alongside Tjalling C. Koopmans, and Guy Orcutt, at Yale

University's Cowles Foundation seminar on December 3, 1975.[22] The written record of his remarks at the November 1975 event, Hendry (1977), included an encapsulation of what he saw as the path forward: "the desirability of incorporating the best features of time series analysis into econometric practice."[23] That is—and reflecting the situation that Hendry was taking a position taken widely by economists and not just specialist econometricians—he felt that that the forecasting success enjoyed by pure time-series approaches pointed to the need for structural economic modeling to reflect empirical dynamics more comprehensively—and should not be interpreted as a reason for abandoning models that incorporated economic structure.

Most of Hendry's involvement in the time-series-versus-econometrics controversy naturally took place in his permanent base of the United Kingdom. The prevalence in that country of Box-Jenkins techniques among firms and academia had turned those techniques into a mini-industry, and those in research circles favoring the techniques included proponents outside the economics profession who were aggressively advancing the notion that econometrics needed not to be modernized but, instead, replaced. Often stridently, these Box-Jenkins advocates saw the techniques not only as a source of benchmark forecasts but also as a reason why structural econometric modeling could and should be put to rest. Hendry deplored the situation when he appeared at a meeting of statisticians and economists organized by the Royal Statistical Society on the evening of June 23, 1976: "Unfortunately, some researchers have reacted by interpreting Box-Jenkins methods as an *alternative* to econometric estimation."[24]

Gwilym Jenkins, of the Box-Jenkins team, was himself a prime example of a non-economist who was presenting ARMA methods as something that could replace the preceding body of econometric approaches rather than be assimilated into it. In lectures given at a statisticians' conference at Cambridge University in July 1976, and published in 1979 as part of a "Time Series Library" book series, Jenkins (1979, p. 63) blasted even the use of seasonally adjusted data as creating "confusion in the minds of both technical people and lay people" and in a chapter titled "Comparisons with econometric modeling" suggested that such modeling led to "many *spurious explanatory variables* to be introduced" (p. 91, emphasis in original).[25]

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[22] See https://cowles.yale.edu/cf-seminars.
[23] Hendry (1977, p. 183).
[24] Hendry (1976, p. 494, emphasis in original).
[25] See Jenkins (1979, pp. vii–ix) on the origins of his book. Jenkins dedicated his 1979 study to statistician R.A. (Sir Ronald) Fisher and clearly suggested that the Box-Jenkins endeavors were in keeping with the

An even more ardent proponent of the position that Box-Jenkins users should not concentrate on building bridges with econometric approaches but instead, on transcending econometrics was Oliver D. Anderson, who worked in various academic and government statistical posts in the 1970s and who was, like Hendry, a participant at the June 1976 London event. The basis for that event was Kenneth Wallis co-presenting a paper (Prothero and Wallis, 1976) that argued for a synthesis of time series and structural econometric approaches. Hendry and Anderson were among Prothero and Wallis' multiple discussants. Wallis later summarized Anderson's 1970s research trajectory as: "He picked up the book [Box and Jenkins, 1970], and that was it." (Kenneth Wallis, interview, January 29, 2015.) Anderson became a highly prolific writer on Box-Jenkins methods and producer, in the 1970s and 1980s, of a series of interdisciplinary conference volumes on their application (organizing so many conferences, in fact, that one of the final entries in the book series bore the forbidding title *Time Series Analysis: Theory and Practice 7*—see O.D. Anderson, 1985).

In his June 1976 comments, Anderson was, in part, superficially conciliatory toward the economics world—claiming that he sought "not confrontation but co-operation and communication." In fact, however, he actually sought to relegate the future role of econometricians to one of joining in estimation of Box-Jenkins equations and helping interpret equations already generated by ARMA procedures (O.D. Anderson, 1976a, p. 488). In the same year, Anderson produced his own book expounding Box-Jenkins methods in which he indicated unhappiness with the fact that, in economics, "models based on theoretical considerations are favored [over] purely statistical fits," a situation that he suggested was not justified, as the economy was very "incompletely understood" (O.D. Anderson, 1976b, p. 134).

Also in those June 1976 remarks, Anderson saw time series as likely to win the perceived contest with econometrics: "what econometric modeling will have to compete with very soon is multivariate time series analysis. I do not see how it can hope to cope then…

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sort of work that Fisher had encouraged (Jenkins, 1979, pp. vii–viii). Notably, Friedman—no adherent to Box-Jenkins methods—was also a longtime admirer of R.A. Fisher's work. Reflecting this fact, Friedman and Kuznets (1945, p. 339) had cited two R.A. Fisher books, including his *Statistical Methods for Research Workers* (R.A. Fisher, 1925). Evidently, different research workers who considered themselves adherents to the R.A. Fisher tradition ended up subscribing to approaches to statistics that were quite divergent from one another—just as, within the economics profession, Friedman and other major economists regarded themselves as following in *Irving* Fisher's footsteps but differed sharply from one another on the elements of Irving Fisher's body of work on which they put emphasis.

Have the econometric modelers any comparable advance up *their* sleeves?"[26] As it happened, the answer was "yes," and econometrics met the challenge. The subsequent decade would see the multiple time-series econometrics revolutions of the widespread usage of error-correction equations, cointegration, and vector autoregressions. Hendry would play a pioneering role in promulgating the first of these and was also deeply involved in cointegration research.[27] But research in which Hendry had participated some time earlier, during the 1970s time-series-versus-econometrics debate, also in effect provided foundations for a position implicitly or explicitly taken by the proponents of the third of the key 1980s econometric innovations—vector autoregressions.

The specific important work by Hendry in this connection consisted of his holding the line against the ARMA/Box-Jenkins movement by making the case that MA terms were largely unnecessary in practical econometrics. Hendry and Trivedi (1972) argued, on the basis of simulation evidence, that in the univariate modeling of a time series, AR(p+q) processes provided a sound representation of a series when the variable was actually produced by ARMA(p, q) dynamics.[28] To be sure, this result was advanced only as a demonstration that modest-lag-length autoregressive (AR) specifications served as a good approximation in this case—it was not an analytical finding.[29] In principle, the presence of any MA term in an ARMA equation implied an infinite-order AR process—so it was the case that a modest-lag-order AR process was necessarily less general than an ARMA process. It was against the background of this analytical result that Jenkins (1979, p. 53) implied that ARMA specifications were essentially unavoidable, as he maintained that, in practical applications, one MA term could only be well approximated by "several" AR terms. Hendry and Trivedi, however, argued the opposite—that it was a good approximation that an AR representation of an ARMA series required no more lags than the total of the dynamic parameters of the ARMA process.

This position—later reaffirmed by Hendry (1995, pp. 358, 566)—implied analytical and

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[26] Anderson (1976a, p. 488).

[27] Bruce Hansen (1996, p. 1399) pointed to Hendry (1980) as showcasing a "remarkable understanding of late-1980s time-series theory, such as the implications of unit roots [and] spurious regressions." Some of these later developments are considered in Section 4 below.

[28] p, of course, here denotes the autoregressive lag in the ARMA process. In other sections of this paper, however, the same notation is used in reference to the log price level.

[29] This was true also in the case in which a VAR provided an approximation for a VARMA. Consequently, Hendry and Mizon (1993, p. 276) described a VARMA as "more general processes" than a VAR, even though Hendry's position in empirical applications, in parallel with the univariate case, was that the dynamics implied by MA terms (at least in the case of invertible MA processes) could be well approximated by means of a manageable increase in the VAR lag length.

computational simplifications that were of considerable benefit to macroeconomists. As Mizon and Hendry (1980, p. 23) noted, "moving average error processes pose identification, estimation and testing problems." If the approximation offered by Hendry and Trivedi was valid, those problems could be sidestepped, because MA parameters did not need to be used at all. Most notably, in the event that autoregressive specifications were all that was needed, OLS could be used, rather than the nonlinear least squares (or other non-OLS techniques) implied by ARMA estimation. In a multivariate context, the Hendry-Trivedi suggestion also provided support for later key developments—including economists using VARs rather than VARMAs, and the focus in dynamic rational expectations models on autoregressive, rather than ARMA, dynamics in the specification of the model and forcing processes (for example, King and Watson, 1998).

Although researchers rarely cited Hendry and Trivedi (1972) in doing so, over the 1970s and 1980s the macroeconomics and applied-econometrics worlds in the United Kingdom and the United States crab-walked toward the Hendry-Trivedi position that autoregressive dynamics provided a good proxy for ARMA dynamics. For example, Campbell and Perron (1991, p. 154) stated that "the more general case [of] ARMA (p,q) … can be approximated by a high-order autoregressive process." And, in the practical applications of such approximations, prominent instances appeared in which the autoregressive process is indeed in practice p+q, just as Hendry and Trivedi suggested. Along these lines, Shapiro and Watson (1988, p. 135) noted that "it is difficult to distinguish the trend-stationary AR(2) model from the ARIMA(1,1,1)," and Campbell and Deaton (1989, p. 365) found that, in part because of overparameterization, their estimated "ARMA(2,1) processes are very close to being AR(1) processes." The same perspective provided, as already indicated, a foundation for VARs, rather than VARMAs, serving as the baseline specification for modeling macroeconomic dynamics.[30]

The simplification inherent in using VARs rather than VARMAs helped contribute to a considerable streamlining of the treatment of dynamics in empirical work in macroeconomic research over the course of the 1980s. This streamlining, which much of Hendry's work had helped foster, contrasted with the situation prevailing at the start of the decade, when multiple approaches to modeling dynamics were used even in the same study. The chaotic set of practices present in empirical work as of the early 1980s were

[30] Pesaran and Smith (1992, p. 13) see Sims (1980) as advancing VARs as "a simplification of the VARMA model." Sims' (1980) discussion did not in fact refer to VARMAs as all. But the notion that VARs could approximate VARMA dynamics was implicitly the position of VAR advocates, and, as indicated already, Hendry's research had provided some basis for the emergence of this position.

brought out by Bade and Parkin's (1984, p. 253) description of various "different procedures for testing for the causality relation between money and prices… [including] the cross-correlation analysis of innovations… the estimation of a multivariate transfer function… [and] the estimation of a one-sided AR model using Almon lags…"

The reference to "innovations" in the preceding quotation reflected another dimension of Box-Jenkins procedures. Negative reaction to this particular dimension formed the third key element of Hendry's response in the time-series/econometrics debate—and on this issue, too, Hendry and Friedman were on the same side. Box and Jenkins (1970, Chapters 10–11) had put forward a means of extending ARMA models to include right-hand-side variables—that is regressors, or, as Box and Jenkins called them, "inputs." As the means of making an input stationary before supplementing the ARMA equation with it, the authors argued for "prewhitening" the series—a univariate transformation to make the input white noise (pp. 379–380). Jenkins (1979, p. 91) stuck to this recommendation when urging "adequate identification techniques, such as prewhitening."

Correspondingly, in the course of the widespread adoption of new time-series methods into econometrics during the 1970s, some studies analyzed relationships between series only after "prewhitening" all series under study. For example, Sims (1972), using quarterly U.S. data, multiplied both log nominal income and log nominal M1 by the quasi-differencing filter $(1 - 1.5L + .5625L^2)$ prior to running bivariate regressions.[31] In a related vein, the autoregressive integrated moving average (ARIMA) modeling approach that became perceived as best practice in many economists in the 1970s led researchers to remove all unit roots in each data series prior to estimation. Box-Jenkins proponents also expressed interest in more exotic and nonlinear transformations of the data, such as raising it to the power, in order to deliver an allegedly better-behaved filtered series. Friedman and Schwartz (1982a, p. 552) questioned the worth of such specifications in economic applications, remarking that, at least for the monetary relations with which they were concerned, "for the most part the important question... has to do so with the longer-term systematic movements that are expressly abstracted from in the ARIMA models."

[31] Later, of course, Sims became a strong proponent of VAR analysis in which series entered in their original levels or log levels. Sims noted (interview, March 15, 2013): "Nowadays, I just use a multivariate time series model with enough lags and, if I feel I need a lot of lags, I use a prior. In those days, there were much stronger computational limits on what you could do, and [that motivation underlay] the prewhitening—this was something that bothered a lot of people about that paper... [But] I still think that's true: If you're limited to that range of [1972-vintage] techniques, prewhitening would make sense."

This quotation puts in perspective a passage of Ericsson, Hendry, and Hood (2016, p. 114) that makes the attribution of "Friedman regarding measurement error as dominating stochastic shocks." This was not, in fact, Friedman's stance. He actually believed that economic shocks dominated the observed permanent component of economic series.[32] His position, as stated explicitly in an item they do not cite, was that "purely random measurement errors may have a far larger impact on the transitory component than on the permanent component" of time series (Friedman, 1988, p. 232). On this ground, he was poorly disposed toward both differencing and quasi-differencing of data.

Schwartz (1984, p. 135) took exception specifically to prewhitening as the preferred filter: when applying modern techniques to economic time series, she suggested, "it does not mean that the series should be whitened." Friedman buttressed this position when he remarked that a focus on prewhitened series "throws out the baby with the bath" and that the associated examination of the relationships between prewhitened series amounted to a case of "correlating noise" (letter to Charles Goodhart of May 10, 1982, quoted in Hammond, 1996, p. 191).

On this matter, Friedman and Schwartz were four-square with Hendry and Ericsson, who regarded prewhitening as an unwise procedure. They rejected it as a valid pre-estimation step and, at the estimation stage proper, did not favor the implied common factor (quasi-differencing) restrictions on the dynamics unless formal tests supported these restrictions. This perspective also implied that, like Friedman and Schwartz, they also had deep reservations about ARIMA procedures that routinely applied the first-difference operation, $\Delta = 1 - L$.[33]

Again, however, the Friedman-Schwartz/Hendry-Ericsson agreement was on the problem—that prewhitening or similar quasi-differencing and differencing operations took away important information regarding long-run relations—not on the solution. Friedman and Schwartz advocated phase averaging as an attempt to obtain series that contained only longer-run information, while Hendry and Ericsson rejected this move and sought to obtain longer-run information by examining short- and longer-run dynamics jointly. This would be a key part of their critique of *Monetary Trends* (see Section 5).

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[32] He also believed in a strong distinction between permanent and transitory component of the series, but as the quotation given presently indicates, he did not treat transitory variation and measurement error as identical.

[33] In particular, the analysis later of matters concerning unit roots and cointegration will highlight Hendry's advocacy of error-correction equations instead of outright first-difference specifications.

4. The cointegration and vector autoregression revolutions of the 1980s

The Hendry and Ericsson (1983) critique was initially presented at an October 1983 Bank of England symposium and issued by the Bank in printed form in December 1983. A detailed account of how the paper developed from the 1983 version to its 1991 counterpart appears in Nelson (2024). That discussion concentrates on the reaction to the Hendry-Ericsson analysis in the monetary economics sphere. In contrast, in this section the focus is on how two econometric revolutions of the 1980s—cointegration and vector autoregressions—shaped the final product.

In a September 1984 television appearance during which he spoke about the Hendry-Ericsson econometric appraisal, Milton Friedman pointed to the fact that the critique concerned *Monetary Trends*' empirical demand-for-money specification and had largely put aside the other chapters, including those that were more directly concerned with the determination of nominal income, interest rates, and prices: "let me emphasize that [in discussing the critique] we're talking about a very small part of a very big book."[34] Anna Schwartz, too, would observe nearly twenty years later: "The fact that the initial reviews concentrated on the demand functions for money probably gave the impression that that was what the whole book was about, but that was only one chapter. And when the econometrics was regarded as vulnerable with the Hendry and Ericsson criticism, the profession was less willing to spend time with the book."[35]

In their original paper, Hendry and Ericsson (1983) had recognized that they were concentrating on Friedman and Schwartz's U.K. money demand equation but promised that the next step would be developing a multi-equation system providing a full model of the system of variables listed in *Trends*' subtitle: income, prices, and interest rates. "We appreciate the risks involved in our postulating an alternative picture of how the system operates, but… we intend to take that risk in Part 2."[36] The contents page of the paper accordingly included "Part I Critique" and "Part II Reconstruction." In the event, the authors ran out of time. In the December Bank of England printing of the proceedings, the paper was simply issued with the contents page indicating a Section II but with no actual Section II appearing.[37]

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[34] Iceland state television, September 1984.
[35] In Nelson (2004, p. 406).
[36] Hendry and Ericsson (1983, p. 80).
[37] The October 20 cover letter by Bank official Leslie Dicks-Mireaux to panel participants stated: "The paper by Hendry and Ericsson is part of a longer paper they are in the process of producing. Part II is

With the money demand work already more than accounting for a paper in itself, Hendry and Ericsson soon decided that their multi-equation structural modeling of the *Trends* annual dataset would be the basis for a separate paper. But although they went on, in 1984–1986, to produce and present a write-up of some of their early results, it would turn out during the 1980s and 1990s that they did not produce the hoped-for "reconstruction" modeling the whole *Trends* U.K. dataset. Rather, they continued to concentrate on the narrower subject of U.K. money demand.

Crucially, two revolutions in econometrics in the 1990s intervened, which rendered the "reconstruction" task different from that envisioned in 1983–1984 and made Hendry and Ericsson's provisional work on the U.K. system largely obsolescent. These were the cointegration revolution—Engle and Granger's (1987) research on cointegration and its linkage with ECMs, as well as successor papers on cointegration; and the VAR revolution—Sims' (1980) launch of vector autoregressions (VARs) as the standard baseline in the econometric modeling of time series. The impacts of each of these revolutions on the Hendry-Ericsson analysis of *Monetary Trends* will now be considered.

4.1 The cointegration breakthrough and the linkage with error-correction equations

A key precursor to the Engle-Granger (1987) paper, "Spurious Regressions in Econometrics," written by Clive Granger and Paul Newbold and appearing in the *Journal of Econometrics* in 1974, was partly prompted by econometric results that appeared as part of the Keynesian-monetarist debate. David Sheppard (1971) had estimated the Friedman-Meiselman multiple regressions (that is, the equations, noted above, relating expenditure to fiscal and monetary totals) on U.K. data. "The regression on levels yielded a corrected R^2 of 0.99," Granger and Newbold (1974, p. 118) noted of Sheppard's postwar estimate of this multiple regression—but, glaringly, the accompanying Durbin-Watson statistic in Sheppard's regression was only 0.59.[38] Furthermore, the corresponding first-difference specification—which Sheppard had also estimated—had no residual serial correlation but also no explanatory power: an adjusted R^2 of minus 0.03 and a Durbin Watson of 2.21. This combination led Granger and Newbold to suggest: "This provides an indication of just how one can be misled by regressions involving

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intended to be shorter and to contain some tentative suggestions as to how a small model dealing with the major macroeconomic variables studied by F&S [Friedman and Schwartz] might be constructed. It will not be presented formally to the Panel, though it is hoped it might be made available on the day of the meeting." (Arthur Brown papers, Leeds University.)

[38] The regression in question, and the next one referred to, appeared in Sheppard (1971, Table 5.2, p. 76).

levels if the message of the [Durbin Watson] statistic is unheeded."[39] Granger and Newbold noted with respect to pairings of a very high R^2 and a very low Durbin Watson statistic, "this phenomenon crops up so frequently in well-respected applied work," and they suggested that it reflected the existence of upward trends in the explanatory variable(s) and the left-hand-side variable—of the kind that aggregate economic time series like nominal money, real income, nominal income, and prices certainly had in the United States, the United Kingdom, and elsewhere.

It was already well known in econometrics that upward-trending variables amounted to a common characteristic of economic time series, was a feature that breached the conditions under which ordinary regression analysis was applicable to time series, and could generate unreliable parameter estimates that were spuriously significant. For example, in an analysis of the production function, Knowles (1960, p. 16) observed: "Measures of total inputs and output for a dynamic growing economy like ours are likely to exhibit, as a common characteristic, highly correlated growth trends…. These common time trends complicate the statistical problem of determining quantitatively the functional relationships between inputs and output." The Granger-Newbold paper, however, offered a specific means of diagnosing problems of this kind: the low Durbin Watson/high R^2 combination.[40]

The Granger-Newbold paper also advanced a proposed solution. And here the authors' sympathy with and linkages to the Box-Jenkins-inspired critique of econometrics became evident. As already indicated, Box and Jenkins advocated considerable transformation of the data before running regressions. As part of their own procedures, they recommended the extreme step of prewhitening. But short of that, in Box and Jenkins (1973, p. 342), they strongly endorsed first-differencing: "the popular assumption that errors are independent and identically distributed is much more likely to be approximately true for the first difference than for the original series. It is the *failing* to difference rather than the differencing which has sometimes led to error…." Granger and Newbold, too, largely

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[39] Without citing the reference in question, Granger and Newbold (1974, p. 111) had noted of high R^2/low Durbin Watson statistic combinations: "The most extreme example we have met is an equation for which $R^2 = 0.99$ and [Durbin Watson] = 0.093." Actually, another follow-up to the Friedman-Meiselman study, that by Meiselman and Simpson (1971), had almost matched this feat, as one table of results included a regression that had an adjusted R^2 of 0.98 and a Durbin Watson statistic of 0.08 (Meiselman and Simpson, 1971, Table 4, p. 241).

[40] Granger and Newbold (1974) emphasized at the outset of their discussion that a low Durbin Watson statistic was already indicated in econometrics textbooks as corresponding to a serious problem. Their message was, in part, that the widespread awareness of this problem had not prevented much research in applied economics being published that used regressions that had low values of the statistic.

endorsed first differencing, on the grounds that regressions on levels may deliver sizable slope coefficient yet "may well be entirely spurious" (1974, p. 111). They added (p. 118): "Until a really satisfactory procedure is available, we recommend taking first differences of all variables that appear to be highly autocorrelated. Once more, this may not completely remove the problem but should considerably improve the interpretability of the coefficients." The words "may well be" and the proviso that the recommendation was pending a "really satisfactory procedure" left the door open to returning to levels specifications in the future. But for now, the authors were aligning themselves with the time-series camp in advocating outright differencing.

Although Granger and Newbold were calling for the practice of first-differencing to become more prevalent, it was already widespread in empirical work by 1974. To a considerable degree, their proposed solution of first-differencing was already (along with generalized least squares) the extant method taken in response to the perceived unreliability of levels regressions. Even in cases when the statistical implications of using trending levels in regressions were far from fully recognized, much applied work as of 1974 had sought to abstract from rising trends or prevent those trends from dominating the results. Friedman and Schwartz had themselves remarked in 1963 in describing their monetary analysis at the time: "The method we have used is to take logarithmic first differences of the money stock, which is equivalent to using the percentage rate of change from one time unit to the next… It is clear that this device effectively eliminates trend."[41]

In Friedman and Schwartz's case in 1963, this device was primarily used in the context of graphical and correlation analysis. But in the regression analysis of Friedman and Meiselman (1963, 1965) both levels and first-difference regressions were reported, and similarly in *Monetary Trends* (mainly reflecting the influence of practices in place before the 1970s) levels and growth-rate specifications were typically reported in the regressions describing real money balances, nominal income, and real income. This practice, like a considerable amount of published work in the 1960s and early 1970s, treated the first-difference specification mainly as a *robustness check* on the levels specification, and so a mark of success was "consistency of results [using] levels and rates of change"

[41] Friedman and Schwartz (1963b, p. 36); see also their page 40. Meuriot (2015) states: "Sargan (1964) *introduced* the use of first differences of the log of variables so that the variables were stationary." (Emphasis added.) The Friedman-Schwartz example just given is one of many that could be cited to show that the usage of the logarithmic first difference in applied macroeconomics preceded Sargan (1964). The latter's innovation lay instead with connecting these to ECMs.

(Friedman and Schwartz, 1982a, p. 281).[42] Granger and Newbold's recommendation, however, represented a break from using first-difference equations as a check and instead amounted to making them a (provisional) replacement for the econometric analysis of levels. And it was a recommendation that proved influential on practical work—with an early example of its influence on the monetary literature coming in Auerbach and Rutner (1975), who cited Granger and Newbold (1974) and stated (p. 12), "before relationships between the variables with a trend can be estimated properly, the effect of the trend must be removed from each variable."

This was a matter on which, as part of his participation in the "time series versus econometrics" debate of the 1970s, Hendry took issue with the emerging consensus. He objected to the embrace of first-difference specifications, seeing them neither as a valid check on, nor as an appropriate replacement for, levels specifications. As Hendry (1999, p. 357) noted: "I have strongly argued against blanket differencing since 1975..."[43] This attitude was manifested in the appearance during the 1975/1976 academic year of what became the Davidson-Hendry-Srba-Yeo (1978) paper, which was in effect a successful relaunch of ECMs, previously used in applied macroeconomics by Sargan (1964).[44] It

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[42] As Hendry, Pagan, and Sargan (1984, p. 1044) implied, the view that first-difference specifications should be expected to deliver slope estimates similar to a levels specification, or served as a check on levels estimates, rested strongly on the intuition of the nonstochastic or steady-state case in which $y = \beta_1 z$ implies $\Delta y = \beta_1 \Delta z$. Friedman and Schwartz did in fact recognize verbally that, if two levels series are linked to one another, part of the dynamics of the behavior of the corresponding first differences must involve forces that enforce the long-run levels relationship: "there must be some mechanism to bring the various series back to the trend lines" (Friedman and Schwartz, 1982a, p. 208). However, their general practice in *Monetary Trends* of estimating symmetric specifications for levels and growth rates of series implied that they did not empirically allow for ECM-type terms.

[43] Hendry's opposition to growth-rate equations was stressed not only in Hendry (1993) but also in Meuriot (2015), who points to Hendry's advocacy of ECMs as a stimulus to Clive Granger's development of cointegration and the Granger representation theorem. Meuriot makes a number of important points but, as discussed below, it should also be kept in mind that the *economic* idea of cointegration—a relationship between levels series, including upward-trending series—was prevalent in the wider economic literature, well before the distribution theory addressing unit-root series had really been developed. (Meuriot also states unequivocally that Granger and Hendry first met in November 1975. This is possible but is very unlikely. Meuriot does not cite Renton [1975]—the record of a conference on economic modeling held at the London Graduate School of Business Studies in July 1972 to which both Hendry and Granger—in the latter's case with coauthor Paul Newbold—contributed papers. This conference was held when Clive Granger was still based in the United Kingdom.)

[44] Sargan (1964) would be referred to frequently as "the Colston paper," as it was produced for the sixteenth symposium of the Colston Research Society (held in the University of Bristol, April 6–9, 1964) and published in the resulting proceedings volume. Meuriot (2015) contends that, on the basis of his stay in the United States in 1975, Hendry "realized that Sargan's works were unknown outside [the United Kingdom], particularly those concerning error correction models." This is not correct as written, though a statement that "Sargan's work on ECMs was unknown," might well be. Sargan's published research was certainly not unknown in the United States, but the most well-known work was that on simultaneous equation estimation (for example, Sargan, 1958) and was separate from his applied work on ECMs such as

was also evident in Hendry's remarks at the November 1975 Federal Reserve Bank of Minneapolis conference, referred to above. When discussing a new paper at that conference by Granger and Newbold (1977), Hendry (1977, p. 196) cited the Davidson-Hendry 1975 version of the Davidson-Hendry-Srba-Yeo (1978) article, while also indicating that an error-correction equation, $\Delta yt = \gamma_1 + \gamma_2\Delta x_t + \gamma_3 (y_{t-1} - x_{t-1}) + \omega_t$ (with ω_t white noise and $\gamma_3 < 0$), had the virtue of maintaining the levels relationship (in this case, a unitary loglinear relationship) even when the dependent variable was a first difference.

Hendry had already had considerable success in having this case accepted by U.K. applied macroeconomists, with the "Hendrification" of applied work in that country being under way even before the 1978 appearance of his two coauthored *Economic Journal* articles on ECMs.[45] In effect, by the late 1970s in the United Kingdom ECMs had been "reintroduced to econometrics" (Pagan, 1989, p. 340), in a more durable manner than had been the case in in the 1950s and 1960s. Adrian Pagan observed (interview, January 8, 2015) that although the ECM concept had been in A.W. Phillips and J.D. Sargan's work, it was the Hendry-coauthored work of the 1970s that "basically operationalized it." He remarked of Davidson, Hendry, Srba, and Yeo (1978): "They saw this very good application in terms of consumption and income. And it was the paper that really had the impact, actually… Showing how the thing works, really—that was what the consumption/income paper did." Applications of ECMs to the demand for real money balances followed, especially in the 1980s, and Swamy and Tavlas (1989, p. 65) characterized matters in the money demand literature as: "Hendry has pioneered the error-correction estimation procedure."

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the "Colston paper." The well-known character of Sargan's research on simultaneous equations analysis, including among the new generation of applied U.S. macroeconometricians, was reflected in Goldfeld and Quandt's (1972) and Fair's (1970) citation of that work. Hendry's dissertation research under Sargan was closer to these better-known Sargan contributions than it was to those on ECMs. The legacy of Hendry's early, instrumental-variables-related work was felt in what became Hendry's PC–GIVE (later PcGive) econometric package, which got its name from the acronym "Generalized Instrumental Variable Estimators." (See, in particular, Hendry, 1989: this reference gave the acronym and cited Sargan, 1958, as a key reference.) This econometric package first appeared commercially in 1984 but had been available in academic circles for more than a decade before that. For example, the 1972 version of the mimeographed guide to the package was titled "GIVE: Users' Manual for General Instrumental Variable Estimation of Linear Equations with Lagged Dependent Variables and First Order Autoregressive Errors," mimeographed, London School of Economics, 1972." See Hendry (1975, p. 320).

[45] Although the Hendry and Mizon (1978) study was concerned to a large degree with the preferability of equations incorporating ECMs (via a general specification) over the "common factor restrictions" associated with generalized least squares or with quasi-differencing of the data, it was also motivated by the author's opposition to specifications that were written entirely in growth-rate terms. In this connection, Hendry (1993, p. 129) recalled of the background of the Hendry-Mizon (1978) project: "we were unhappy about the use of differenced data in econometric modeling."

Hendry's position on the matter of ECMs *vis a vis* first-difference equations would eventually prevail widely in applied work in both the United States and the United Kingdom. But in the late 1970s and early 1980s, the first-difference movement remained strong in the United States. It seemed to get a boost from the initial reaction in applied macroeconomics to the statistical literature on unit roots. C.R. Nelson and C.I. Plosser's (1982) pioneering study applying the Dickey-Fuller (1979, 1981) tests documented the prevalence of unit roots in macroeconomic time series (including Friedman and Schwartz's annual U.S. monetary data, produced for *Monetary Trends*). Initially, in U.S. macroeconometric work, the resulting increased awareness of unit-root issues reinforced the tendency to regard outright first-difference specifications as being appropriate. For example, Rasche (1987, p. 10) stated: "the data are consistent with the 'unit root' hypothesis, so first-difference, rather than level, specifications are appropriate."[46]

What Hendry (1993, p. 120) called a "countercritique" was, however, at hand. In the 1980s, Granger largely repudiated his prior advocacy of fully-differenced specifications. Instead, he would develop the concept of cointegration—linear linkages between the levels of $I(1)$ series—with an estimation procedure in the case of pairs of cointegrated series being developed by Engle and Granger (1987). This became a research phenomenon, with Pagan (1989, p. 343) observing: "The concept of cointegration has already had an enormous impact upon econometric theory and practice."

The cointegration literature, in suggesting that two series could share a "common trend"—in which case a regression relationship between the levels might have a non-trending (an $I(0)$, or, more specifically, covariance stationary) error term—largely dovetailed with what much of macroeconomic theory had to say. Macroeconomic analysis pointed to stable relationships involving trending real economic magnitudes, including connections between output and labor market or spending variables (see, for example, King, Plosser, Stock, and Watson, 1991).[47] In discussions of the

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[46] As already indicated, Sims (1980) used levels in vector autoregressions from the outset, and that was a position that he reaffirmed in the late 1980s. But in the empirical literature using VARs through the late 1980s, the Nelson-Plosser work was sometimes seen as pointing toward a differenced specification. For example, Evans (1989, p. 44) argued for a VAR specification in first differences. on the basis of "the finding of Nelson and Plosser (1982) that U.S. macroeconomic time series are typically well characterized" as $I(1)$.

[47] King, Plosser, Stock, and Watson (1991) also suggested that a money demand function would be a cointegrating relation. Such a position was, essentially, common ground of the sides of the Friedman-Schwartz and Hendry-Ericsson debate. There is, however, as discussed some below, a significant amount of monetary analysis that contends that quantity-theory-type relationships do not require that the error in

macroeconomics-cointegration connection, Friedman was brought into the picture, thanks to both Engle and Granger (1987) and Stock (1988) retrospectively viewing Friedman's (1957) consumption function as a depiction of a cointegrating relationship.

The cointegration revolution also largely supported Hendry's own work, which had kept levels terms in estimated relationships.[48] It specifically endorsed his use of ECMs, thanks to Engle and Granger's (1987) deriving the result (the Granger Representation Theorem) that cointegration between two variables implied that at least one of the series was described by an error-correction specification (rather than an equation consisting only of first differences).[49] As Ericsson (1992, p. 252) observed, the advent of the cointegration notion provided "a firmer statistical and economic basis for… error-correction models," while also rendering dead letter the time-series-versus-econometrics contest of the 1970s because it essentially ended "the 'debate' on whether to use levels or differences" (see also Cochrane, 2018).

Hendry embraced the work on cointegration, which he considered "about the most important recent time-series development" (Hendry, 1999, p. 343). He participated in the emerging literature, including by organizing and contributing to a special issue of the *Oxford Bulletin of Economics and Statistics* in 1986, ahead of Engle and Granger's (1987) publication.[50] In two important respects, however, the Engle-Granger work was somewhat at odds with the practices followed in Hendry and Ericsson (1983) and led them to a rethinking of their project.

First, Hendry (1978, p. 178) indicated that, prior to the emergence of the cointegration literature, he had been confident that the availability of error-correction specifications resolved problems associated with the econometric treatment of levels series. On this view, the regression analysis could begin with a dynamic equation estimated by OLS, with the first difference of a series being on the left-hand side, and with the right-hand-side variables typically being in first differences, other than one lag of various levels terms. Significance tests could then be applied to the regressors in this unrestricted

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the money demand equation be $I(0)$. (See McCallum, 1993, and the discussion in Nelson, 2024, of the implications for empirical work on the QTM.)

[48] Engle and Granger (1987, p. 251) led off their acknowledgments with thanks to Hendry for interactions.

[49] Here, cointegration will refer to a relationship between $I(1)$ series. According to some usages of cointegration terminology, $I(0)$ series enjoy "trivial cointegration" with one another, because they satisfy automatically the condition that a linear combination of them is $I(0)$.

[50] He had unsuccessfully sought to persuade Engle and Granger to publish their paper in this special issue.

equation, with simplifications (deletions of some variables, and taking of linear combinations of others) implied by the test results leading to an eventual, preferred error-correction equation. This preferred equation would, typically, include an ECM variable composed of a linear combination of the lagged levels series that had appeared as separate regressors in the original, general specification.

The cointegrating literature largely disabused Hendry of this view. It suggested that, owing to the different statistical distributions and faster convergence properties of the coefficients that were estimated as the weights in particular cointegrating relationships, it was desirable and appropriate that these coefficients should be estimated, and the ECM term then defined using these coefficients, prior to the estimation of the dynamic error-correction equation (or, in Engle and Granger's system-based framework, equations).

Second, Engle and Granger (1987) in formalizing ECMs used a different formulation of the ECM equation from what Hendry had espoused. As noted above, in the 1970s he had given an example of such an equation $\Delta y_t = \gamma_1 + \gamma_2\Delta x_t + \gamma_3(y_{t-1} - x_{t-1}) + \omega_t$. This could easily be generalized to allow for further lags of Δy_t and Δx_t and to a nonunitary coefficient (cointegrating coefficient) on x_{t-1}. But the Engle-Granger version instead specified a "vector error correction mechanism" (VECM) system in which no contemporaneous right-hand-side regressors appeared. A system of x_t and y_t that allowed for one lag of the difference terms in this case would consist of the equations $\Delta x_t = \gamma_{10} + \gamma_{12}\Delta x_{t-1} + \gamma_{13}\Delta y_{t-1} + \gamma_{24}(y_{t-1} - \beta x_{t-1} - \kappa) + \omega_{1,t}$ and $\Delta y_t = \gamma_{20} + \gamma_{22}\Delta x_{t-1} + \gamma_{23}\Delta y_{t-1} + \gamma_{24}(y_{t-1} - \beta x_{t-1} - \kappa) + \omega_{2,t}$ (the γ_{ij}'s being estimated by OLS, as κ and β could be taken as known constants, estimated in step 1 of the procedure described below). The element of the definition to refer to vectors of equations was a generalization of the previous concept—and would become highly influential, as reflected in Bruce Hansen's (2018) statement that "the vector error correction model (VECM) of Engle and Granger (1987) is one of the most widely used time-series models in empirical practice." As part of the influence of VECMs, Engle and Granger's definition of ECM equations to exclude current-dated terms was largely to become the standard one. Econometric textbooks would often take error-correction equations as, by definition, excluding contemporaneous variables.[51]

[51] For example, in his ECM discussion, Hamilton (1994)—though he initially cited (see p. 571) Davidson, Hendry, Srba, and Yeo (1978) (a study that did include contemporaneous regressors)—defined the error-correction system in VECM form, containing only lags as regressors (p. 580). So did Stock and Watson (2003, p. 554). The Stock-Watson textbook also defined the ADL specification as containing only lagged regressors (p. 446).

The exclusion of right-hand-side variables dated *t* had appeal on the grounds that it implied a symmetric treatment of all the variables in the vector system and did not embed an assumption about same-period interaction among time series. But as Alogoskoufis and Smith (1995, pp. 139, 145) stressed, Engle and Granger's omission of contemporaneous regressors from the error-correction equations represented a change in the character of those equations that made them more susceptible to being viewed as reduced-form characterizations of economic dynamics.[52] In particular, this specification of the right-hand-side of error-correction equations represented a break from Hendry's practice. His—predominantly single-equation—work in this area starting in the mid-1970s had typically conditioned on (treating as econometrically exogenous) some variables, by having their current first differences appear as regressors, and interpreted the resulting error-correction equation, estimated by OLS, as a structural relationship. As Bruce Hansen (1996, p. 1405) observed, "in practice, Hendry routinely works with linear equations with contemporaneous variables on the right-hand-side."

The Engle-Granger redefinition of ECM-related equations was not something that Hendry fully embraced. Reflecting his attitude, Hendry and Ericsson (1991a) adopted only partially Engle and Granger's recommended means of proceeding in the presence of a bivariate cointegrating relationship. Engle and Granger called for a so-called "two-step" procedure: first, estimate the cointegrating regressor by static bivariate regression and then, in step 2, estimate the pair of VECM equations, using the residuals from the static regression as the ECM term. As will now be discussed, the successive versions of the Hendry-Ericsson paper reflected the incorporation of Engle-Granger cointegration revolution. But they did go all the way to an embrace of the two-step procedure.

In Hendry and Ericsson (1983), in line with the emerging cointegration literature, the authors estimated a static money demand equation—an OLS regression on annual data of the log real balances-real income ratio on a constant and the level of the interest rate (p. 71). But they also estimated an ADL levels equation. In ascertaining long-run behavior, they focused on the long-run solution of that equation, rather than the previously estimated cointegrating regression (p. 74).[53] In Hendry and Ericsson (1985), although

[52] Of course, insofar as the structural equations describing the economy involves forward-looking terms, an error-correction equation that omits these terms will not be structural even if it includes period-*t* right-hand-side variables.
[53] This ADL estimates delivered a considerably lower long-run interest semielasticity of long-run U.K. money demand than did the Engle and Granger (1987) procedure. This contrast seems at odds with the result in Banerjee, Dolado, Hendry, and G.W. Smith (1986) that Engle-Granger regressions tend to deliver

they cited the now-expanded cointegration literature, they largely went back to the pre-cointegration ECM literature's practice of arriving (after deleting and consolidating some regressors) at a preferred error-correction equation and having a single lag of various levels series on the right-hand-side, rather than estimating a cointegrating vector beforehand (Hendry and Ericsson, 1985, p. 32). And in their next step, the "'preferred' error-correction specification" did not group the levels terms into a single error-correction term in estimation, in contrast to much cointegration work.[54] In contrast, in the final version of their work, Hendry and Ericsson (1991a) did use the static cointegrating regression method of Engle and Granger (1987), thereby following the first step of the two-step method. This equation, using Friedman and Schwartz's (1982a) annual data and estimated over 1873–1970 was (their equation (9), p. 24):

$$(m - p - y)_t = -0.309 - 7.00RS_t \,, \qquad (2)$$

with residuals, $\hat{u}_t$. Hendry and Ericsson (1991a) then broke from the Engle-Granger procedure, or its single-equation analogue, as they did not proceed to a second-step ECM equation that consisted only of lagged right-hand-side terms. Instead, they estimated, for the log-level of real money balances over 1878–1970, what was largely an ADL

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downward-biased, and hence lower (in absolute value), estimates of the cointegrating parameters than methods that better allow for dynamics. A possible reconciliation of the findings is that Hendry and Ericsson (1985, 1991) did not allow for an own rate on money and simply put the actual short-term interest rate into the long-run money demand function. The dependence of real balances on the actual rate (when the own rate is not included, and so not held constant, in the analysis) may be larger in the short run than in the long run.

[54] The long-run solution was obtained by solving the equation, in which individual lagged levels appeared freely, for the steady state (p. 32). In other work over this period and in later years, Hendry and Ericsson, largely in research conducted with other coauthors, used a further alternative approach to estimating a cointegrating vector. In the non-system case (that is, when only a single series was being treated as a left-hand-side variable), this consisted of estimating by OLS an unrestricted ADL levels equation in which one of the variables in the cointegrating vector was the left-hand-side variable, solving the estimated OLS equation for its long-run expression, and having the long-run solution be the cointegrating vector, the deviations from which defined the error-correction right-hand-side variable to be used in subsequent ECM equation estimation. They found in numerous simulation and applied exercises that this procedure tended to give more reliable parameter estimates than the Engle-Granger procedure, and that the significance (as judged against appropriate critical values) of the associated ECM term in the error-correction equation estimation provided stronger evidence of cointegration than might be suggested by tests that used the residuals obtained from a static regression. On these matters, see especially Banerjee, Dolado, Hendry, and Smith (1986) and Kremers, Ericsson, and Dolado (1992), as well as Baba, Hendry, and Starr (1992, p. 40), Campos, Ericsson, and Hendry (1996, p. 194), and Hendry (1997, p. 183; 1999, p. 343). See also the related remark in Hendry (1992, p. 378) regarding a case in which he suggested that "the unrestricted model allows significant rejection of the null of no cointegration." Other means of improving on the Engle-Granger procedure to allow for dynamics included those proposed by P.C.B. Phillips and B.E. Hansen (1990) and Stock and Watson (1993). The advent of the Johansen procedure in the late 1980s and its incorporation into software packages meant that it largely, but by no means entirely, displaced the other approaches as a means of allowing for dynamics when estimating cointegrating vectors.

specification—one that, as Friedman and Schwartz (1991, p. 24) would note, was an equation "containing 37 parameters": the constant term, the *S* dummy variable and separate dummies for each world war, the error-correction term ($\hat{u}_{t-1}$), that term raised to powers (as discussed in the next section), and lags 0 to 5 of the natural logs of: real income, real money, the short-term interest rate, and the long-term interest rate.[55] Having estimated this general specification, Hendry and Ericsson then deleted many regressors, recast others as difference terms, and arrived at a preferred ECM equation (with $\Delta(m - p)_t$ as the dependent variable) that is discussed in the next section.

The procedure therefore consisted of a mixture of cointegration-literature and pre-cointegration practice: although a cointegrating regression was estimated, an intermediate step was included, more like the pre-cointegration approach to the derivation of ECM equations, of having lagged $I(1)$ variables as regressors and then ascertaining whether testing deletions and the taking of linear combination of those variables, in conjunction with the recasting of the dependent variable as a first difference, could lead to an error-correction specification.[56] And, consistent with other work by the authors on ECMs, contemporaneous regressors appeared in both estimated dynamic equations: the general ADL regression, and the preferred error-correction specification.

A look at what might have emerged if step 2 of Engle and Granger's (1987) procedure had been pursued in this instance is given in Table 2, in the case of the error-correction term as defined above and the system being $\Delta(m - p)_t$, Δp_t, Δy_t, and RS_t. For simplicity, the wartime dummy variables and the shift dummy *S* are not included, so the sole right-hand-side variables are a constant, $\hat{u}_{t-1}$, and a lag of each first difference. With regard to the $\Delta(m - p)$ equation in the VECM system, it is clear that its fit was considerably poorer than Hendry and Ericsson's preferred equation. The $\Delta(m - p)_t$ equation has an R^2 of0.2042 and a SEE of 3.48 percent. If the dummy variables had been included, these statistics improve only to an R^2 of 0.270 and SEE of 3.4 percent. In contrast, Hendry and Ericsson got to an R^2 of about 0.87 and a SEE of about 1.42 percent, as discussed below.

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[55] This was not an ADL of the kind estimated in Hendry and Ericsson (1983, 1985) and elsewhere because the lagged ECM term(s) were included as well as lagged levels of the series indicated. This did not produce exact collinearity because *RS* entered the ECM term in levels, while its freely estimated lags were in logs. Consequently, $\hat{u}_{t-1}$ was not an exact linear combination of other regressors in the equation.

[56] The critical values for these test statistics in the $I(0)$ case did not necessarily carry through to the $I(1)$ case. But insofar as an inference problem associated with $I(1)$ variables is that tend to *appear* statistically significant using conventional critical values when they were not, in fact, statistically significant, there might not be a great problem involved in deleting $I(1)$ variables when they are *not* significant according to conventional critical values.

Table 2. First-order vector error correction system, annual U.K. data, 1878–1970

	Log-change in real money balances, $\Delta(m-p)_t$	Inflation (log-change in prices), Δp_t	Log-change in real output, Δy_t	First difference of short-term interest rate, ΔR_t
$\Delta(m-p)_{t-1}$	0.4661 (0.1271)	0.2445 (0.1474)	–0.1461 (0.1228)	0.0038 (0.0332)
Δp_{t-1}	0.0081 (0.0844)	0.8174 (0.0980)	–0.2054 (0.0816)	0.0255 (0.0214)
Δy_{t-1}	–0.1733 (0.1133)	0.3419 (0.1314)	0.2148 (0.1095)	0.0478 (0.0288)
ΔR_{t-1}	0.0220 (0.4609)	–0.3865 (0.5348)	–0.1499 (0.4455)	–0.1051 (0.1169)
$\hat{u}_{t-1}$	–0.0139 (0.0355)	–0.1900 (0.0412)	0.0054 (0.0344)	–0.0192 (0.0090)
Constant	0.0103 (0.0052)	–0.0046 (0.0061)	0.0205 (0.0050)	–0.0007 (0.0013)
R^2	0.2042	0.5044	0.1381	0.1312
SEE	0.0348	0.0405	0.0337	0.0088
No. obsns.	93	93	93	93

Fit aside, there are other reasons a $\Delta(m-p)_t$ equation whose regressors included period-*t* series might be preferable to a VECM equation describing real balances. VECM equations are always likely to be only reduced forms, but there is a special reason for *not* viewing a VECM real-balances equation obtained via the Engle-Granger two-step method as a money demand function. As Table 1 indicates, such a specification allows only the *lagged* interest rate in the real-balances equation. It is therefore not very amenable to being viewed as representing the demand for money. This is because of the strong presumption in monetary analysis that portfolio decisions depend on the current interest rate. Lagged interest rates, on this view, are bygones—and should not matter for period-*t* decisions about real money holdings (see McCallum, 1985, pp. 583–584).

Even in theoretical frameworks that allow for considerable dynamics in money demand, it is considered nonstandard to exclude the current short-term interest rate from the money demand function, as it is usually assumed that at least some scope exists to adjust real balances in response to current interest rates. And in a setting in which real balances are decided in advance or are subject to portfolio adjustment costs, past period's interest rates should stand in for the opportunity cost of holding balances in period *t* only insofar as they are judged helpful in predicting interest rates dated *t* or later. Hendry and Ericsson's (1991a) inclusion of contemporaneous interest rates in their dynamic money demand function is therefore understandable. Their putting interest rates and other variables dated *t* in an equation *estimated by OLS* is more controversial—and Bruce Hansen (1996, p. 1405) criticized this practice's "obvious endogeneity problem"—but was in line with much empirical money demand practice in the 1970s and 1980s.

4.2 Vector autoregressions

Hendry (1997, p. 183) judged that the *Federal Reserve Bank of Minneapolis Quarterly Review* was not "a mainstream journal." From a longer-term perspective, this may prove to have been an overly harsh judgment. Very notable contributions appeared on macroeconomics and applied econometrics in the *Quarterly Review* during the 1970s and 1980s. On the econometric side, the *Quarterly Review* deserves note especially for having published a special issue as early as 1979 on VARs—at a time when the term "vector autoregression" had barely appeared in print in other economic-research journals. A major impetus for this special issue was, of course, Christopher Sims' paper "Macroeconomics and Reality," which was cited in its 1977 working-paper form in the *Quarterly Review* (see Sargent, 1979). The Sims paper, of course, went on to appear in *Econometrica* at the start of the 1980s.[57] About a decade further along, it was clear that the VARs had created a revolution in empirical work in economics. In contrast to the

[57] Of course, just as Box and Jenkins (1970) did not invent ARIMA models and Hendry did not create ECMs (although he coined the terminology), the concept of vector autoregressions (as opposed to their operationalization in macroeconomics) as a concept did not start with Sims (1980) or with the working-paper versions of that paper. Lutkepohl (2007) gives Quenouille (1957) as an early reference on VARs in the time-series literature. Analytical work on VARs also appeared the work of some time-series statisticians who tended to interact heavily with economists: see, for example, Hannan (1970, p. 326) and T.W. Anderson and J.B. Taylor (1979). It should also be noted that vector autoregressions had also been considered in econometrics in the context of the residual process, as a generalization of modeling univariate error processes. For example, Guilkey and Schmidt (1973) had an article titled "Estimation of Seemingly Unrelated Regressions With Vector Autoregressive Errors." Goldfeld and Quandt (1972, p. 187) indicated that Hendry himself presented a paper, "The Estimation of Complete Models With Errors Generated by a Vector Autoregressive Process," at the Econometric Society's September 1969 meeting in Brussels. This paper, published in 1971, was part of Hendry's Ph.D. dissertation work (see Hendry, 1971, p. 257).

situation in 1979 when the *Minneapolis Quarterly Review* was one of very few outlets in economics promoting them, by the early 1990s Campbell and Perron (1991, p. 167) noted that in the wake of Sims (1980), VARs had "become an increasingly popular device to model the stochastic properties of a multivariate system."

"Early in my career, when I started doing VARs, I thought Hendry and I were on the same research program," Christopher Sims observed (interview, March 15, 2013). "Because I had read papers he had written criticizing large macro models for pretending that there was no [residual] serial dependence left after they estimated their equations, and then showing that there was. And I thought that was, basically, the same thing I was saying." Sims added: "But then we went off in very different directions." An example of the difference, Sims noted, was that "I kept trying to estimate multivariate models," whereas Hendry, through the mid-1980s, tended to focus on particular ECM equations.

Consequently, by the mid-1980s VARs and the general-to-specific methodology were seen as rival approaches—a perception that informed a session of the Econometric Society's congress held at Cambridge, Massachusetts, in August 1985. "At the 1985 World Congress, Chris and David gave talks," George Tauchen recalled (interview, November 13, 2014). "And Angus Deaton introduced it. He said, "OK, we're now going to have the *Star Wars* session"—meaning that it was the war between the two stars, Chris and David."[58]

The contrast between the two methodologies as of 1985 should not be overemphasized. As noted in the introduction, Hendry and Ericsson (1985) accurately portrayed both themselves and Sims (1980) as concerned with starting from general dynamics and so as being both inclined against econometric analysis such as that of *Monetary Trends* that began with a restrictive specification.

Nevertheless, Hendry in his writings through the late 1980s did himself often stress the *differences* between his perspective and Sims' and was critical of VARs as a modeling strategy. For example, Hendry (1985, p. 73) stated: "His [Sims'] proposed solution of allowing the data to play a direct and much larger role in empirical analysis than it does

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[58] The panel (held on August 19, 1985: see Econometric Society, 1986, pp. 474–475) also had Edward Leamer as the representative of his little-used variant of Bayesian econometrics. Adrian Pagan (January 8, 2015) recalled of the session: "I was the discussant at the end. I decided I wouldn't participate in the debate, but I discussed them all"—in the form of remarks on the other participants' methodologies. These remarks formed the basis of Pagan (1987).

in the straitjacket of the 'conventional' approach has much merit, but in my view swings too far. In particular, his unrestricted equations inextricably mix features which are relatively variable with those which are potentially more constant, rendering the whole non-constant."[59] He elaborated in Hendry (1987), his World Congress lecture, that he believed that it was a shortcoming that VARs had only lagged regressors on the right-hand-side, in contrast to his own typical ECM specifications which, as noted above, tended to include period-*t*-dated regressors. The omission of contemporaneous variables was a design feature of VARs—one that was intended to allow the analyst to prevent assumptions about same-period interaction of variables from having a bearing in the estimated dynamics.[60] Hendry argued, however, that the omission of current-dated variables was liable to generate equations that did not have temporally stable coefficients. At the end of the 1980s, he repeated this criticism in the 1989 PC-GIVE manual: here, Hendry (1989, p. 13) cited VARs as an example of what Hendry, Pagan, and Sargan (1984) had called "dead-start models"—that is, equations in which the only regressors allowed were lags of the dependent variable and of other series. The "exclusion of contemporaneous information" from an equation in a VAR system, Hendry (1989, p. 13) suggested, made the equation's "parameters… susceptible to change" in a manner that might be avoided in a procedure that included period-*t* values of regressors.

Matters changed tremendously, however, when advances in the cointegration and error-

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[59] This quotation indicates that Hendry readily acknowledged that Sims (1980) did provide an alternative macroeconomic modeling framework to his own. In contrast, another U.K. commentary, that of Britton (1991, p. 117) was more ambivalent, claiming: "Modelbuilding continued to flourish in Britain during the 1980s, although it was somewhat in eclipse in North America." Britton noted the "pioneering work on time-series econometrics… by David Hendry and others" as contributing to the U.K. outcome. Hendry's work was indeed pioneering. But Britton's formulation seemed to imply that Sims' 1980s contributions in time-series econometrics were neither pioneering nor even modelbuilding—judgments out of kilter with Sims later receiving a Nobel award for his research on VARs. Britton may have been taking VAR systems as being advanced for forecasting but not other types of modeling. But that was not the message of the VAR literature that followed Sims (1980), and as Sims stressed in L.P. Hansen (2004, p. 282), he himself believed in identifying models (including by imposing structure in the form of assumptions about same-period interaction among variables) and had done so in Sims (1980) to the VAR estimated in that paper.

Even if the large amount of work using VARs is set aside, Britton's position that economy-wide models were "somewhat in eclipse" in the 1980s was an overstatement. Although work at academic institutions in the area was likely less than earlier and later, macroeconometric modeling by specialized commercial firms remained sizable. In addition, the Federal Reserve Board staff maintained a macroeconomic model over the period. In large part reflecting Neil Ericsson helping increase awareness among colleagues of the properties of error-correction equations, the 1980s generation of the Board models incorporated ECM dynamics, with Brayton, Levin, Tryon, and Williams (1997, p. 49) noting that in this period the model was "reestimated, and many equations respecified, using some of the error-correction specification and methods of residual analysis suggested by David Hendry and other LSE econometricians."

[60] Or, as Watson (1994, p. 2844) put it, "VARs… don't dichotomize variables into 'endogenous' and 'exogenous.'"

correction literature put VARs into a central position. A turning point came when Soren Johansen's paper "Statistical Analysis of Cointegration Vectors," presented at the University of Copenhagen in 1987 and published in the *Journal of Economic Dynamics and Control* in 1988. As already indicated, Engle and Granger's (1987) cointegration analysis had really covered comprehensively only the case of a single cointegrating vector governing two $I(1)$ variables, and Granger (1986, p. 226) acknowledged, "Testing for cointegration in general situations is still at an early stage of development." Johansen (1988), however, took large steps in generalizing cointegration analysis to heavily multivariate cases: he proposed tests for estimating the number of cointegrating vectors and procedures for estimating them; his proposed estimator of cointegrating parameters was amenable to standard χ^2 and t-testing; and he produced a means by which the estimation of cointegrating parameters used information on dynamics. Hendry had been highly critical of the Engle-Granger static regression procedure regarding its weakness on the last point—see Banerjee, Dolado, Hendry, and G.W. Smith (1986)—and he would praise the Johansen procedure for this reason and also because it was "the most general" of the "many possible tests for cointegration" (Doornik and Hendry, 2006, p. 112).

Johansen made these strides via "asymptotic analysis of the CVAR" (Søren Johansen, personal communication, September 5, 2019)—"CVAR" meaning cointegrated vector autoregression. Whereas Hendry in the 1980s had suggested VARs might not be a reliable setup, Johansen (1995, p. 4) was an advocate of them, viewing the VAR system as something that "allows the embedding of interesting economic hypotheses in a general statistical framework." P. Hansen and S. Johansen (1998, p. 4) later added: "What is fascinating about the cointegrated vector autoregression is the very rich structure it has."

As a result of this linkup between multiple cointegrating vectors and VAR analysis, Hendry became a convert to the routine use of VARs, in conjunction with the Johansen procedure. His new recommendation was accordingly "system analysis at the start" (Hendry, 1999, p. 358), and he would note that his practice since 1988 had been system, rather than single-equation, analysis.[61] The negative reference to VARs in the discussion of "dead-start models" was retained in the 1992 PcGive manual (Doornik and Hendry, 1992, p. 12), as well as in the excerpting of the 1989 manual in Hendry (1993, p. 453), but it was dropped from later editions of the manual starting with Doornik and Hendry (1994a). And that PcGive manual came alongside the PcFiml manual, which heavily

[61] David Hendry, remarks at Carnegie-Rochester Conference, Pittsburgh, November 1996.

featured VAR estimation, especially in the context of deploying the Johansen procedure (see Doornik and Hendry, 1994b). Correspondingly, when Ericsson (2003, p. 254) noted that a VAR equation was equivalent to a dead-start model of the variable in question, he implied that such a restriction on the specification of dynamics might be acceptable in a "system context." Hendry and Mizon (1993, p. 272) likewise indicated that, in view of cointegration developments, they now considered VAR estimation to be "an essential first stage in a multistage modeling strategy."

In the Johansen procedure, the stages succeeding the first stage of estimating VARs involved ascertaining and estimating the cointegrating vectors in the VAR and proceeding to estimate a VECM system. Hendry's work now included this stage. But, in the spirit of their earlier practice, Hendry and Mizon (1993) indicated that they regarded it as desirable to go to a further stage, in which particular variables in the system were represented as error-correction equations that had as right-hand-side contemporaneous regressors some of the other system variables. Their preference was therefore to specialize the modeling of dynamics further after taking the usual steps of CVAR/VECM estimation. Nevertheless, they and other adherents to the general-to-specific methodology acknowledged that the earlier steps, associated with the Johansen procedure, were appropriate whether the further step was taken: "Analysis of the system as a whole is valid in any case," Ericsson (1994, p. 6) observed. The rift that had been perceived during most of the 1980s between the VAR and general-to-specific methodologies was now substantially narrowed, on account of the embrace by Hendry and others of the notion that VAR estimation constituted the appropriate first step in dynamic analysis.[62]

This development in the literature fed back onto the Hendry-Ericsson project involving the *Monetary Trends* dataset. The greater degree of common ground of the VAR and general-to-specific essentially scuttled the work that Hendry and Ericsson had done in following up their money demand work with the promised "Reconstruction"—that is, their hoped-for analysis of the determination of the whole system of variables that had

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[62] This linkup was with the VAR *literature*, particularly that using classical methods, rather than with Sims' own application of VARs. Sims was not well disposed toward the multi-stage approach to VAR or VECM estimation used in the Johansen and similar approaches, arguing: "It is not necessary to preprocess our models and data and decide how much integration and cointegration there is before we proceed…" (In Backhouse and Salanti, 2000, p. 237. See also his remarks in Sims, 1988, 1991.) Much VAR analysis did, however, use the cointegration-testing and VECM-estimation framework. See, for example, Kurmann and Mertens (2014).

been examined in *Monetary Trends*. They actually produced a short working paper in 1984, revised in 1986, that presented early results in their modeling of *Monetary Trends* variables other than real balances: real income, prices, short- and long-term interest rates, and the exchange rate. But the title of the paper—"Prolegomenon [that, is, a prelude] to a Reconstruction" (Hendry and Ericsson, 1986)—was an admission that they had a long way to go. In this research report, the authors acknowledged that they were having trouble in their annual-data modeling in characterizing the behavior of a key variable—real output growth—that Friedman and Schwartz (1982a), too, had found difficult to relate to other economic series.

Even this preliminary work was, however, rendered out of date by the cointegration revolution and by the Hendry and Ericsson acceptance of the Johansen system approach. Their work on the *Trends* variables in 1984–1986 version had applied general-to-specific procedures on an equation-by-equation basis. In doing so, it had mostly imposed cointegrating relations *a priori* or had implicitly estimated cointegrating vectors via including unrestrictedly levels series on the right-hand-sides of equations. But the CVAR literature implied that this was not an appropriate starting point, that having unrestricted levels in the dynamic equations produced inferential problems, and that cointegrating vectors should be estimated and then included as ECM terms in the VECM system. Ericsson (1994, p. 2) observed that "conditional ECMs are almost invariably much simpler to model than the whole system."[63] But the literature developments of the later 1980s implied that the "Reconstruction" that the authors had promised in 1983 in effect would need to involve carrying out the ambitious task of modeling "the whole system" using CVAR procedures. Reflecting the authors' numerous other commitments, this "Reconstruction" did not appear.[64]

5. The econometric findings of the Hendry and Ericsson article

This section discusses five key criticisms made of *Monetary Trends* by Hendry and Ericsson (1991a). These concerned: (*i*) specific-to-general modeling; (*ii*) parameter

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[63] "Conditional" here meaning that the equation included period-*t* regressors. Of course, even a VAR equation is "conditional" in the sense that each regression in the VAR has lagged data in the right-hand-side variable list (for example, Andrews, 1994, p. 1222, refers to an equation like that in a VAR as a conditional density).
[64] The modeling of the *Monetary Trends* variables in Ericsson and Irons (1995) was not advanced as a system analysis. The authors concentrated on single-equation univariate equations (so-called marginal models) in characterizing series other than real money balances.

constancy; (*iii*) testing and evaluation; (*iv*) phase averaging; and (*v*) reverse regressions. Remaining subsections then discuss Hendry and Ericsson's preferred specification.

5.1. Specific-to-general modeling

Friedman and Schwartz (1982a, p. 215) expressed a preference for a specific-to-general modeling strategy. In following this approach, they initially reported money demand equations that were simple regressions of log real money on log real income (1982a, section 6.2), before adding explanatory variables one at a time. Their ultimate specification was, as already indicated, a multiple regression. But along the way to reaching that preferred equation, they decided to omit some variables (such as the long-term interest rate). Consequently, their investigation did not involve estimating a general equation incorporating all the explanatory variables considered in the money demand analysis.

As Hendry and Ericsson (1991a) emphasized, this specific-to-general strategy is plagued by the problem of how to interpret regression results when a statistic indicating a test rejection is encountered at some point during the process.[65] This problem manifested itself in Friedman and Schwartz's judgments about what variables to include in their preferred specification. Occasionally, they decided to exclude a variable from their money demand model because it was not significant when added to a regression that includes the explanatory variables considered up to that point in the analysis, and so they foreclosed the possibility of ascertaining whether that explanatory variable should appear in the most general specification that they contemplate. The decision just discussed, involving the long-term interest rate's omission from the demand function for money, was a case in point (see Friedman and Schwartz, 1982a, pp. 264–269). Another case occurred when they tested for the price homogeneity of money demand before introducing interest rates into their analysis (1982a, pp. 253–259). That is, their test consisted of adding the log price level (p_t) to a money demand equation that included log

[65] In addition to their testing (discussed presently) of the Friedman-Schwartz equation's exclusion of variables, Hendry and Ericsson (1991a, p. 15) found significant residual nonnormality in that equation. Testing for residual normality was a regular practice in Hendry's work, but on this occasion the authors elaborated on the justification for such testing. This elaboration may have been a response to referee feedback. That feedback may have included the point later made by Bruce Hansen (1999) that residual nonnormality does not render invalid time-series least-squares regression. (Indeed, B. Hansen [1999, p. 195] went so far as to say: "Normality is neither a good nor bad property of a regression error.") Hendry and Ericsson suggested that residual nonnormality could signify underlying specification error. See also Harvey (1990, p. 3) for another argument regarding the desirability of residual normality.

real income (and their shift dummies) but did not include interest rates. When, as occurred in some reported equations, the estimated coefficient on p_t differed numerically from unity—it cannot be said whether it differs *significantly* from unity because Friedman and Schwartz reported no *t*-statistics on this occasion—they attributed this to the effect of omitting interest rates from the equation—and decided to impose price homogeneity throughout the remainder of the analysis (1982a, p. 254).

Economically, this was not a very controversial decision, because price homogeneity is such a basic postulate in monetary analysis. But it was problematic from an econometric-testing standpoint. And, from the general-to-specific perspective, the question of whether (as Friedman and Schwartz postulated) a finding of nonunitary influence of p_t on real money demand is an artefact of not including interest rates in the estimated equation is something that can be straightforwardly tested econometrically. The manner of proceeding would seem to be to include log prices and interest rates in an unrestricted specification. Friedman and Schwartz (1982a) did not carry out a test along these lines. For their part, Hendry and Ericsson (1991a, p. 14) found that p_t entered significantly when added to the *Monetary Trends* preferred model of U.K. money demand, rejecting price homogeneity and contradicting Friedman and Schwartz's conjecture that p_t would become insignificant if the effect of interest rates was accounted for.[66]

Hendry and Ericsson's own analysis *did* ultimately come out in favor of price homogeneity—a restriction that, as noted, has considerable analytical appeal. But their analysis of Friedman and Schwartz's own econometric treatment of the problem brought home the basic message that the specific-to-general approach is problematic because the specific, or lower-parameter, equation, may imply invalid parameter restrictions. In particular, and reflecting standard "omitted variables" results, a specialized version of a general specification that excludes significant elements of the general version will typically give rise to inconsistent estimates of the parameters of interest.[67] Hendry and

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[66] Hendry and Ericsson (1991a, p. 15) also found that a trend term entered notably if added to equation (1). Its significance seemed to emerge mostly at the expense of the real-income term.

[67] The qualification "typically" is required for two reasons. First, parameter estimates do not lose consistency when important variables are omitted, if the excluded variables are orthogonal to the regressors. Second, in describing as inconsistent (or as biased and inconsistent) the estimates obtained from an estimated specification that has imposed invalid restrictions, one implicitly takes the parameters of interest to be the parameters associated with the more general specification. Otherwise, if the moments in the data are constant over time, the parameters of most equations estimated by ordinary least squares can trivially be described as consistent because, as Engle, Hendry, and Richard (1983, p. 286) remarked, "it is always possible to redefine the 'parameters' such that any chosen convergent estimation method yields

Ericsson (1991a, p. 16) summarized these difficulties by remarking: “The crucial issue is that a reject outcome at any stage of modeling invalidates *all previous inferences*, so that decision-taking during model generalizations is ill-founded.”[68]

Hendry and Ericsson suggested that *Monetary Trends*’ employment of a specific-to-general means of settling on a specification was the reason why the authors eventually ran “literally hundreds of regressions for different periods and different sets of independent variables” (as Friedman and Schwartz, 1982a, p. 266, described matters) before arriving at their preferred specification. In contrast, in the general-to-specific approach, the preferred equation can be regarded as a linear restriction of the initial general specification. Those specifications that can be reached only by imposing invalid parameter restrictions on the general version are ruled out as candidates as the preferred ultimate equation. This criterion alone has the potential to rule out a good deal of the regressions that Friedman and Schwartz entertained in their money demand analysis.[69]

5.2. Testing for a constant money demand function

Hendry and Ericsson (1991a, p. 13) applied the Chow (1960) test to Friedman and Schwartz’s money demand equation, obtaining a rejection ($F(18, 12) = 6.3$).[70]

They then stated that Friedman and Schwartz (1982a) described their money demand functions—including the U.K. money demand equation on which the 1991 debate would be focused—as stable. Yet, Hendry and Ericsson contended, Friedman and Schwartz “do not formally *test* for constancy.” In fact, Friedman and Schwartz (1982a, p. 232) did report constancy tests, including *F*-test statistics regarding the equality of residual

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consistent estimates thereof.” See also Hendry, Pagan, and Sargan (1984, p. 1039) and Hendry (1995, p. 351) for related statements.

[68] Hendry (1979, p. 226) earlier expressed the same point. See also Hendry and Mizon (1993, p. 275), Doornik and Hendry (1994a, p. 243), and Hendry (1993, p. 30; 1995, pp. 207, 269–270).

[69] It would not rule out running regressions on subsamples, however. Such subsample regressions were among the “hundreds of regressions” to which Friedman and Schwartz were referring.

[70] That is, if conventional critical values are used. In light of Hendry and Ericsson’s subsequent treatment of log real money, log real income, and the short-term interest rate as $I(1)$ processes, reliance on standard critical values may not be appropriate. However, Hendry and Ericsson are justified in using standard critical values in testing *Monetary Trends*’ U.K. money demand equation, as the Friedman-Schwartz book treated the equations as susceptible to econometric analysis designed for stationary data, and diagnostic test statistics might have standard distributions if this assumption had been valid (see Hendry and Ericsson, 1991a, p. 13). Similar comments apply if one contends, as Hendry and Ericsson later indicated, that the *t*-tests and related statistics given in connection with the phase-average regressions in Friedman and Schwartz (1982a) and Hendry and Ericsson (1991a) need to be adjusted to allow for the conversion of the SEEs from phase-average to annual percentage units.

variances across subperiods. As it happened, however, those tests *rejected* the stability of the preferred U.K. money demand multiple regression—and so supported Hendry and Ericsson's own subsequent finding of parameter nonconstancy regarding the equation in question.[71] It should also be noted that most of these tests of money demand constancy were conducted on regressions that already allowed for some parameter instability via the inclusion of the interwar shift dummy variable *S* (discussed in Section 2 above), as well as the war-related term.

5.3. Phase averaging and filtering

As already stressed, Friedman and Schwartz (1982a) transformed the annual data that they collected into phase-average data, which were used in their regression specifications. Friedman and Schwartz's key money demand regressions lacked explicit lags of any variable.[72] But because the phase averages combined, in a complicated fashion, about two to three annual observations, static regressions applied to phased-averaged data implicitly amounted to dynamic regressions on annual data. In effect, therefore, phase averaging introduced, in a restricted manner, lags of the regressors (including lagged dependent variables).

A very thorough analysis of phase averaging would be given in Campos, Ericsson, and Hendry (1990), a study that was a spinoff of the Hendry-Ericsson analysis of *Monetary Trends*. The same group of authors later provided, in Campos, Ericsson, and Hendry (1997), an extension and capsulization of their analysis of phase-averaging procedures. In order to gain a heuristic impression of the repercussions of these procedures for Friedman and Schwartz's work, a simple example, considering a less detailed case than those examined in the rigorous Campos-Ericsson-Hendry analyses, can be sketched.

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[71] In particular, stability of residual variances was found in the case of the U.S. money demand specifications (both the final multiple regression and a simpler version) but not with regard to the corresponding U.K. equations. Friedman and Schwartz may have regarded the rejection of constant residual variance in the U.K. case as reflecting the fact that statisticians' construction of older U.K. annual income data apparently involved extensive interpolation (see, for example, Friedman and Schwartz, 1982a, pp. 175, 177, 233, 601)—a practice that they believed tended to hold down unconditional data variation and also conditional second moments like residual variances. In their 1991 reply (pp. 40–41), they cited this consideration as a basis for not regarding increased residual variance across periods as evidence against the stability of their U.K. money demand equation.

[72] As equation (1) indicates, nominal income growth was used as a regressor, but to represent a yield, not lags.

If annual log real money balances $(m_t - p_t)$ is replaced by a weighted average of the current year's and the previous year's annual values, $(1 - \gamma)(m_t - p_t) + \gamma(m_{t-1} - p_{t-1})$, the result may well be a smoother series. But a static regression that used the smoothed series as the dependent variable, as well as similarly smoothed regressors, would be analogous to a dynamic annual regression in which $(m_t - p_t)$ was the dependent variable and the lagged dependent variable $(m_{t-1} - p_{t-1})$ had an imposed coefficient of $-\gamma/(1 - \gamma) < 0$. Phase-averaged data were essentially two-to-three-year declining moving averages, so the logic of this example carries over to the analysis of such data. In particular, it is evident that, upon being re-expressed in the form of the annual series, a phase-average equation can be interpreted as a dynamic specification that imposes restrictions on the lags of the dependent variable, the other right-hand-side variables, and the annual-data error term.

To those experienced with regression analysis of annual macroeconomic time series, the implied dynamics associated with these restrictions are unusual, especially because the coefficients on the lagged dependent variable are negative. As Hendry and Morgan (1995, p. 48) noted, this is a typical consequence of taking moving averages of the variables—and, as they further observed, seems a "peculiar" restriction to impose in view of the large and significantly positive coefficients on lagged dependent variables frequently found in empirical representations of levels (or log levels) of economic data.[73] As the discussion of SEEs in the next subsection will indicate, it turns out to be the case that, in light of phase-averaging's peculiar restrictions on dynamics, Friedman and Schwartz's U.K. money demand specification, if estimated as a *static* regression on *annual* data, fits considerably better than when estimated as a phase-average regression.[74]

As stressed in preceding sections, in speaking out against phase-averaging as a sound procedure, Hendry and Ericsson (1991a) were making a case that would be shared widely by applied macroeconomists. The same goes for the more detailed postmortem on phase averaging given in the Campos, Ericsson, and Hendry (1990, 1997) accounts.

Building on earlier analysis of phase averaging in the 1980s Hendry-Ericsson papers, the

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[73] The comparison with the equation just given is complicated by the fact that it has a moving-average error term, whereas many of the empirical time-series equations that include lagged dependent variables freely may not have much residual autocorrelation.

[74] This result, reported by Longbottom and Holly (1985), was consistent with Hendry and Ericsson's (1983, p. 52; 1985, p. 24) conjecture that the *Monetary Trends* money demand equation was misspecified "in part because of the time aggregation."

Campos-Ericsson-Hendry developed a key point—that what may be lagged feedback in annual data can turn into contemporaneous interaction among dependent and explanatory variables. Accordingly, even when some right-hand-side variables may qualify by econometric criteria (such as weak exogeneity) to be valid regressors in equations estimated by OLS on annual data, a least-squares regression using the phase-average counterparts of these regressors may not deliver consistent estimates.

One could take this result as implying that, even if one were inclined—in violation of good econometric practice—to use a phase-average model of U.K. monetary relations, one should estimate the model as a system of equations by simultaneous-equation methods, thereby recognizing feedback among money, price, real income, and interest-rate series. In contrast, as will now be discussed, Friedman and Schwartz seemed to have an exaggerated assessment of the degree to which simultaneity matters could be handled while continuing to rely on OLS-type estimation.

5.4. Reverse regressions and rearranging estimated regressions

Friedman and Schwartz (1991, p. 43) seemed to endorse Hendry and Ericsson's characterization of themselves when they observed that "they [in Hendry and Ericsson, 1991a, p. 34] note near the end that we 'emphasize an "errors-in-variables" paradigm.'"[75]

This paradigm was an econometric tradition in which Friedman had become steeped in the 1930s, via his exposure to the older statistical literature and to such figures as Harold Hotelling. As *practiced* by him in his work with Schwartz and elsewhere, this approach to regression analysis did not entail a belief that measurement error was the predominant form of *shock* to economic time series. On the contrary, Friedman's strong view, voiced as early as 1940, was that specification error was pervasive in actual time-series regressions. This position, in combination with the judgment that he and Schwartz came to that monetary forces were a key medium through which shocks were transmitted to national income, gave him ample basis for believing that there were many sources of error in regression and of disturbances to time series. Friedman nevertheless evidently believed that much of the intuition about regression analysis present in the errors-in-variables framework—a framework centered on measurement error—carried through to a

[75] Hendry and Ericsson had previously made this attribution in Hendry and Ericsson (1983, p. 61; 1985, p. 4).

general environment in which time series were driven by many sources of shocks.[76]

In particular, running regressions both ways played a prominent role in Friedman and Schwartz (1982a). Friedman and Schwartz (1991) affirmed the importance of this practice and usefulness, while their merits and status were, in contrast, questioned by Hendry and Ericsson (1990, 1991a, 1991b).[77]

In discussing the issues involved in this argument, it is important to distinguish between two means by which the left- and right-hand side variables in a simple regression might be transposed. These are (*a*) *rearranging an estimated equation* and (*b*) running regressions both ways, that is, *reverse regression*. Both of these procedures were discussed by Ericsson, Hendry, and Mizon (1998, pp. 376–378)—who called (*a*) "inversion after estimation," and (*b*) "inversion prior to estimation.".[78] It will be argued in what follows that *Monetary Trends* deployed (*b*) but not (*a*) extensively. The reasoning behind Friedman and Schwartz's use of (*b*)—reverse regressions—will be critically analyzed.

In order to emphasize the difference between (*a*) and (*b*), and as a means of bringing out the alleged virtues, as well as the criticisms, of reverse regressions, it is useful to consider a case in which the data are in logs and are $I(0)$, and in which the analyst wishes to estimate the following static equation relating nominal money to prices:[79]

$$m_t = \beta_{11} p_t + e_t\ , \qquad \mathrm{E}(e_t) = 0,\ \mathrm{E}(e_t e_t) = \sigma_e^2. \qquad (3)$$

with a constant included in the regression but suppressed here.

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[76] That is, his position differed from that taken in the strict or literal errors-in-variables approach to regression. In this approach, the existence of measurement errors is the only empirically important ground for believing that a regression will have an error term (see Hendry and Morgan, 1995, p. 35). Notably, Cooley and Leroy (1981, pp. 839–840) suggested that a (misguided) belief of the kind attributed above to Friedman—a belief that errors-in-variable intuition carried over to the more general environment of econometric analysis of stochastic time series—was quite widespread among macroeconomic researchers during the 1970s.

[77] Running regressions both ways had also been practiced in Friedman and Kuznets (1945, p. 172). They were also considered in Friedman (1957, pp. 200–206), albeit after most of the empirical analysis in the book had already been provided.

[78] Hendry and Ericsson (1991a, p. 16) also referred to (*a*) as "renormalizations." The "inversion" terminology refers to the fact that, when viewed in terms of the underlying levels series (rather than the logs that appear in the equations), the linear rearrangement of the equations corresponds to inverting the functions that connect the series.

[79] For illustrative purposes, the discussion here is confined to the bivariate relationships. See Engle and Hendry (1994) for generalizations to the multiple-regression case.

Rearranging an estimated equation: This procedure ((a) above) would amount to rearrangement of the above equation *after* it had been estimated. That is, one would obtain an OLS regression applied to the above equation and then purport to draw inferences about the influence of m_t on p_t by simply rearranging ("inverting") the equation. This procedure would lead, in the case of a regression that has been estimated on a very large sample, to the formulation:

$$p_t = \beta_{11}^{-1} m_t - \beta_{11}^{-1} e_t . \quad (4)$$

On the basis of (3), an analyst might claim that the influence of m_t on p_t is β_{11}^{-1}. Correspondingly, if equation (2) supported the absence of money illusion (and so the existence of price homogeneity) in money demand (corresponding to $\beta_{11} = 1$—really, the only economically acceptable restriction, as already suggested), the investigator might claim that via the manipulation in (4), equation (3) also supported the hypothesis that the log price level responds one-for-one to the log-level of nominal money.[80]

Reverse regression: Reverse regression—(*b*) above)—occurs when the analyst rearranges regression (3) in such a way that p_t becomes the left-hand-side variable, but—instead of algebraically rearranging the parameter estimates obtained from the *m*-on-*p* regression—actually estimates the newly-rearranged equation by an OLS-type procedure—that is, runs a regression in which p_t is the dependent variable and m_t is the regressor:[81]

$$p_t = \beta_{21} m_t + v_t , \quad E(v_t) = 0, E(v_t v_t) = \sigma_v^2. \quad (5)$$

Friedman and Schwartz (1982a, Chapters 6, 8) perform reverse regression extensively. For example, in their Chapter 6, in ascertaining the income elasticity of money demand, they regressed log real balances on log real income (which provides a first estimate of the

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[80] This step of rearranging, or inverting, an equation estimated by OLS is not the only means by which one can represent prices in terms of money. For example, the case of Barro (1984, pp. 139–141), which is related to that cited by Hendry and Ericsson (1991a, p. 30), can be defended as one that derived a representation of, or solution for, *p* in terms of *m* using a theoretical *simultaneous equation system*, rather than an OLS equation, as the starting point. In that case, inversion or rearrangement of equations is admissible, as the initial estimation, or the validity of the original equation, was not predicated on there being only a one-way connection between the variables. Another valid case, considered below and in Nelson (2024), is representing price behavior in terms of *lagged* money. But the economic-research literature certainly contains clear-cut cases of invalid inversion of estimated OLS equations—see the discussions below of the prominent work of Kaldor and Okun.
[81] The distribution of the *m* and *p* data is assumed to be jointly normal, in which case the regression specifications run both ways are linear in form.

income elasticity) as well as the reverse regression, whose estimates they renormalized in order to glean a supposed second estimate of the income elasticity (pp. 225–227). In addition, in Friedman and Schwartz (1982a, Chapter 8), they rearranged their estimated preferred money demand equations by moving the dependent variable to the right-hand side and a regressor to the left-hand-side and then reestimated them. The reverse-regression analyses just described are tantamount to a practice of treating the OLS estimate of β_{21} in equation (5) as though it is an estimate of β_{11}^{-1}.

We are now in a position to discuss equation inversion and reverse regression in more detail. That OLS estimates of β_{21} do not converge to β_{11}^{-1} was a key message of Engle and Hendry (1994) and Hendry and Ericsson (1983, 1985), as well as a number of older textbook treatments. The derivations provided in these discussions imply that there must be an exact (that is, one holding without any error for all t), loglinear relationship between money and prices in order for the coefficient of an OLS regression on m_t on p_t to be the inverse of the coefficient of an OLS regression of p_t on m_t. Unless a nonstochastic conditional relationship prevails, the probability limit of an OLS estimate of β_{21} will differ from β_{11}^{-1}.

The derivations provided in these analyses, as well as the discussion in Hendry (1979, p. 227; 1985, p. 80), also make it clear that *equation rearrangement* is not a practice supported by OLS-type procedures. Having estimated a money demand equation, an analyst is not free to draw economic conclusions concerning the response of p_t to m_t (or the response of other previously right-hand-side variables to m_t) simply by rearranging ("inverting" in the terminology used by Hendry and Ericsson) the equation to generate an expression in a manner that puts m_t on the left-hand-side and one of the regressors on the left-hand-side. Admittedly, the degree to which the *Monetary Trends* analysis endorsed this fallacious procedure is questionable: Friedman and Schwartz's usage of equation inversion—as opposed to their extensive deployment, and endorsement, of reverse regression—was limited and not very clear-cut. In particular, Friedman and Schwartz's (1982a) discussion did not unequivocally suggest that inverting equations in the manner just described was valid. Instead, they implied that it was a reasonable approximation because the equations being inverted had R^2's close to unity (1982a, p. 344). This is one example of a pervasive aspect of the analysis in *Monetary Trends*: the authors regarded the variables under study as essentially generated by a simultaneous-equation system but evidently viewed OLS or OLS-type estimation as a reasonable means of estimating the equations.

Two other points should be stressed specifically about equation rearrangement, in particular, in order to put the matter in perspective. First, although running regressions both ways was a recurrent practice in Friedman's econometric work, the step of equation rearrangement was not particularly associated with him. Indeed, in the past, two major Keynesian economists had been taken to task for engaging, in policy-relevant research produced in the 1960s, in the practice of estimating an OLS regression and then rearranging (inverting) it. These critical discussions of Keynesian work had highlighted the fallacy involved in this practice of rearranging a conditional-expectation relationship estimated by least squares as a purported means of ascertaining the response of a regressor to the left-hand-side variable. One writer noted that Kaldor (1966) had attempted to rearrange an OLS projection of Y on X. The latter projection, the writer noted, "is a statistical regression equation. It gives the most probable value of Y for a particular value of X. It cannot be transposed like an ordinary algebraical equation." (*Financial Times* (London), November 10, 1966.)

Perhaps most notably, Plosser and Schwert (1979, pp. 179–181) argued that the original, and well-known, Okun's law estimates (Okun, 1962), linking variations in output and unemployment, suffered from this shortcoming—as Okun had regressed the first difference of unemployment on real GNP growth and then inferred a partial derivative of real growth with respect to unemployment changes from the regression. "Unfortunately, regression relationships cannot be manipulated this way," Plosser and Schwert (1979, p. 180) observed.[82]

Second, the criticism of inverting money demand equations is not particularly applicable to the econometric approach in much of the monetarist literature of viewing nominal income or its growth rate as a function of monetary variables. Much of this literature consists of using a reduced-form equation linking the nominal variable in question to *lagged* monetary variables (or monetary and fiscal variables). This does not require, or imply, starting with a money demand function (whether estimated by OLS or other methods) and rearranging it. One can instead regard the inflation or nominal income equation as an equation in its own right, a reduced form, appropriately estimated by OLS,

[82] Their remark therefore dovetailed with Ericsson's (1992, p. 265) observation, "Inversion does not obtain the correct parameter for the inverted equation." (See also Engle and Hendry, 1994, p. 112.) More problematically, Plosser and Schwert went on to suggest (1979, p. 181) that a correct estimate of the Okun's-law derivative in question might be discernible from an OLS regression of output growth on the change in unemployment. A more generally valid procedure would surely be to use a non-OLS estimator that could treat both unemployment and output as endogenous variables.

and view it as resulting from the operation of money demand and other structural relationships in the economy-wide model. Whether it is a useful reduced form will largely depend on whether the monetary aggregate (or its growth rate) provides a good summary of monetary conditions over the sample period. But the procedure does not require treating the money stock or monetary growth as exogenous, as one can simply appeal to the fact that, being lagged, the monetary variables are predetermined. Indeed, one disadvantage of *Monetary Trends*' use of phase averages is that it precluded allowing for a discrete lag (say, a quarter or more) between monetary growth and spending (or prices), precluding estimates of reduced-form equations of this kind.[83]

On the arguments just given, equation inversion was not particularly associated with monetarism or with the emphasis that Friedman and other monetarists put on a linkage between money and nominal variables. Friedman did, however, align himself with the practice, now discussed in more detail, of running regressions both ways, and particularly with the notion that this procedure was a helpful means of addressing simultaneity.

Running regressions both ways and simultaneity: A more serious shortcoming of Friedman and Schwartz's approach to reverse regressions is one that truly shows how they were a generation behind econometric practice. This was their overconfidence in the notion that running regressions "both ways" provided a means of obtaining structural parameter estimates in conditions in which the variables under study are simultaneously determined.

Friedman and Schwartz followed an early statistical literature that has largely been eschewed in economic research in the postwar period (one twenty-first-century exception being Bollinger, 2001). In keeping with that prior literature, Friedman and Schwartz, in both their book and their 1991 paper, argued that reverse regressions, estimated by least squares, had value because they generated genuine upper and lower bounds on estimates of the true, unknown parameter. Friedman and Schwartz's procedure (employed heavily in *Trends*) would be applied to the above setup as follows: estimate both regressions (3) and (5); and report, as the upper and lower limits (respectively) of the estimates of the unknown parameter β_{11}, the OLS estimate of β_{11} associated with regression (2), and the

[83] See Fischer and Nicoletti (1993) for another warning against drawing inferences by inverting an estimated model. Again, however, one should be vigilant in jumping to the conclusion that monetarist conclusions about money/prices relationship rest on such equation inversions.

inverse of the estimated coefficient obtained from the reverse regression, (5).[84]

Friedman and Schwartz (1991) argued in this connection that their heavy usage of reverse regressions protected them against the accusation that they neglected matters related to endogeneity and exogeneity. Their suggestion evidently was that running regressions "both ways" provided two estimates of β_{11} that together served to bracket the true parameter β_{11} even if one, or at least one, of the estimated regressions imposed invalid exogeneity assumptions regarding the left-hand-side variable.

This suggestion was partly inspired by the "errors in variables" approach to least-squares regression. That approach has a long history, and it was prominent in Leamer's (1984) empirical work. Apart from Leamer, however, very few prominent econometricians endorsed the approach by the 1980s—and certainly not as a means of justifying OLS-type estimation in the context of a simultaneous system.

Hendry and Ericsson's (1990, p. 3) comment on Friedman and Schwartz's usage of reverse regressions is worth quoting: "Although this procedure is valid in the linear, bivariate, constant parameter, properly specified, *i.i.d.* example of [Friedman and Schwartz (1991, pp. 42–43)], it is not applicable to time-series data such as those that [Friedman and Schwartz, 1982a] analyze. Violations of the noted assumptions are immediately implied by the following: some of the bivariate regressions involved exhibit autocorrelated residuals; the [SEE] for their money-demand equation is non-constant, which [Friedman and Schwartz, 1991, pp. 40–41] acknowledge, attributing it to interpolation of the earlier data; and their final models include several additional explanatory variables (i.e., the models are not bivariate)."[85]

Similarly, Hendry (1995, p. 229) noted that when there are structural changes observed in the sample period, "the direction of regression is unique here, and not open to arbitrary choice as in constant-parameter models (such as errors in variables...)."[86] Further cautions

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[84] See, for example, Friedman and Schwartz's (1982a) usage, already noted, of this procedure in estimating the income elasticity of the demand for real balances.

[85] Friedman (1992, p. 2129) himself acknowledged that the conditions for the validity of the reverse-regression approach included a homoscedastic measurement-error process.

[86] See Schultz (1938, pp. 147–149, 186, 221, 264, 531, 590) for examples of the use of the errors-in-variables argument of the kind described—specifically, Schultz motivated the direction of regression on the basis of postulates regarding which series is measured (more) correctly. Friedman was involved in helping produce the Schultz book. As already stressed, however, he also evidently acquired the view that reverse regressions were a valuable tool in a general stochastic setting.

about the applicability of reverse-regression analysis to the multivariate time-series context would appear in Ericsson, Hendry, and Hood (2016).

Friedman and Schwartz (1991) made it clear that they regarded running regressions both ways as a valuable tool for obtaining parameter estimates when variables are simultaneously determined. Evidently, however, in some of the results in *Monetary Trends*, and certainly in the arguments made in their 1991 reply, Friedman and Schwartz were applying intuition and results found in the statistical literature on regression (most particularly, that on errors in variables) and putting it in what was a largely different context—that of multivariate time-series regression.

It would seem that next to no one in modern macroeconometrics has taken the stand on identification and handling simultaneity that is implied by Friedman and Schwartz's argument for the usefulness of reverse regressions.[87] Therefore, although Hendry and Ericsson (1991a) took some positions on identification that would not be shared by adherents to other methodologies, their rejection of reverse regression as a means of addressing variable simultaneity would likely be widely endorsed by researchers.

5.5. Selection of the dynamic specification

Let us now consider the dynamic equation at which Hendry and Ericsson (1991a) arrived. This was their equation (10) (Hendry and Ericsson, 1991a, p. 25). This regression can be replicated exactly using their data source, the tables of Friedman and Schwartz (1982a). The replication (with conventionally-calculated standard errors) is given as equation (6):

$$\begin{aligned}(m-p)_t = {} & \underset{(0.06)}{0.45}\Delta(m-p)_{t-1} \underset{(0.04)}{-0.10}\Delta^2(m-p)_{t-2} \underset{(0.04)}{-0.60}\Delta p_t + \underset{(0.05)}{0.39}\Delta p_{t-1} \underset{(0.005)}{-0.021}\Delta rs_t \\ & \underset{(0.018)}{-0.062}\,\Delta_2 rl_t \underset{(0.55)}{-2.55}[(\hat{u}_t - 0.2)\,\hat{u}^2_{t-1}] + \underset{(0.002)}{0.005} + \underset{(0.5)}{3.7}\,(DWWI + DWW2)_t \qquad (6)\end{aligned}$$

$R^2 = 0.87$, SEE = 0.0142.

In equation (6), *DWW1* and *DWW2* are dummy variables for the world wars.[88]

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[87] As already indicated, Cooley and LeRoy (1981) actually did find a few examples of this practice when considering the literature that had appeared through 1980.
[88] The dummy variables' coefficients are multiplied by 100.

Friedman and Schwartz (1982a, p. 116) reported that they had found, as they were sending their book to press, that their procedure for obtaining price-controls-adjusted price series had not been correctly applied in the case of the United Kingdom in World War II. They reported corrected values in a footnote. Hendry and Ericsson's regressions used the uncorrected, tabulated data. As part of the replication exercise undertaken in this paper, it emerges that estimates of their preferred specification are very similar when one uses the corrected series. In particular, the estimates are:

$$\begin{aligned}(m-p)_t = {} & \underset{(0.06)}{0.46}\Delta(m-p)_{t-1} \underset{(0.04)}{-0.10}\Delta^2(m-p)_{t-2} \underset{(0.04)}{-0.60}\Delta p_t + \underset{(0.05)}{0.40}\Delta p_{t-1} \underset{(0.006)}{-0.021}\Delta rs_t \\ & \underset{(0.018)}{-0.060}\,\Delta_2 rl_t \underset{(0.55)}{-2.56}[(\hat{u}_t - 0.2)\,\hat{u}^2_{t-1}] + \underset{(0.002)}{0.005} + \underset{(0.5)}{3.7}\,(DWWI + DWW2)_t \qquad (7)\end{aligned}$$

$R^2 = 0.87$, SEE = 0.0143.

Some features of this error-correction specification will be discussed later in this section. At this point, some consideration of how this preferred equation was derived from the general specification is worthwhile.

Hendry and Ericsson (1991a, pp. 22–23, 25) provide the following information about how their preferred model was derived from the general specification: "The conditional model is simplified to an ECM and evaluated in light of the model design criteria... The [general ADL] representation in Table 3 was simplified to the error-correction model (10) using the approach described in Hendry (1983) of first transforming the model to an interpretable and near-orthogonal specification ... and then eliminating insignificant effects..."

Laying out more concrete, mechanical decision rules that an investigator could follow in arriving at a specific preferred specification does risk reducing the flexibility of the general-to-specific approach. Nevertheless, the absence of a more specific outline does open up the approach to the criticisms of Sims (1991, p. 931) that it "makes so much of the estimation process informal." Pagan (1987, pp. 7–8) made a similar observation. Hendry's own reduction criteria differed across studies over the 1980s and 1990s: for example, in Baba, Hendry, and Starr (1992, p. 33) variables with a *t*-statistic below 3.46 are deleted; in Hendry (1992, p. 380), the *t*-statistic cutoff is 1.0.

It should be acknowledged, however, that over the past quarter century Hendry's approach in this area (along with his prescriptions to others) has become much more systematized, via the PcGets, and later Autometrics, automatic model selection software packages (see Hendry and Krolzig, 2001, and Doornik, 2009). The development of these packages flowed from Hoover and Perez's (1999) scrutiny of, and efforts at automation of, the general-to-specific procedure. The appearance of the packages represented a major change from the prior situation: Hendry and Ericsson (1986, p. 2) had observed of reaching a satisfactory specific equation using their methodology, "The work involved is labor intensive and as yet is not (and perhaps never will be) susceptible to 'automation.'" The later software largely saw this automation brought to fruition. In fact, using Autometrics, Ericsson (2010) finds that, starting from Hendry and Ericsson's (1991a) general specification, these automated procedures delivered a preferred specification very much like equation (6). Let us therefore turn to particular aspects of that equation.

5.6. Nonlinearities

Faust and Whiteman (1997) argued that the general-to-specific approach has delivered the most convincing results when the preferred specification is truly simple, in the sense of being parsimonious and lacking elaborate dynamics. And it is on this dimension that Hendry and Ericsson's (1991a) specification is likely less convincing than some of the other equations that have arisen from the general-to-specific procedure.[89] At the heart of the matter is Hendry and Ericsson's (1991a) usage, both in their general specification and preferred simplification, of what Mark Taylor (1993, p. 116) aptly called "a highly nonlinear error correction model."

Hendry and Ericsson used nonlinear error-correction terms: the generalized ADL equation that the authors first estimated contained the level, square, and cube of the error-correction term, and the preferred equation contained a restricted combination of these terms. Such nonlinearities were apparently an attempt, using a linear-in-parameters model, to capture the features found in the same dataset by Escribano. Escribano, a graduate student at the University of California, San Diego, did so in a paper ultimately published in 2004 but that Escribano drafted at an early point in the 1984–1990 period of stasis in the Friedman-Schwartz/Hendry Ericsson debate. The nonlinear terms evidently

[89] Faust and Whiteman (1997) pointed to the case of Baba, Hendry, and Starr's (1992) money demand equation as one that was nonstandard and exhibited substantial breakdown out of sample. On this breakdown, see Hess, Jones, and Porter (1994).

aided in obtaining a constant-parameter specification—something not quite achieved in the initial, 1983, Hendry-Ericsson study.

The result, however, is a final equation that is very hard to interpret, despite its parsimony. Because the error-correction term, being measured empirically by OLS residuals, takes both positive and negative values by construction, it is difficult to find a sound justification for including a *squared* error-correction term. As Hendry has noted elsewhere (in Hendry, Leamer, and Poirier, 1990, p. 238), "cubics... [are] essential because of the property that they preserve sign, whereas quadratics don't preserve sign." Even if the presence of the cubic term compensates for the sign problems associated with including the squared term, the latter's presence seems problematic.

The nonlinear error-correction terms were part of a disarming assortment of ways in which interest rates are allowed to bear on the demand for money in Hendry and Ericsson's (1991a, p 24) unrestricted autoregressive distributed-lag equation:

(*i*) the log short-term interest rate rs_{t-i} enters, at lags $i = 0$ to 5;

(*ii*) the log long-term interest rate rl_{t-i} enters at lags $i = 0$ to 5;

(*iii*) the lagged level of the short-term interest rate, RS_t, enters via the ECM term lagged one year, $\hat{u}_{t-1}$ (that is, $m_{t-1} - y_{t-1} - p_{t-1} + .309 + 7.00RS_{t-1}$);

(*iv*) the lagged square of the short-term interest rate, $[RS_{t-1}]^2$ and various cross-product terms, enter via the squared ECM term lagged one year, $\hat{u}_{t-1}^2$;

(*v*) the lagged cube of the short-term interest rate, $[RS_{t-1}]^3$ and numerous cross-product terms, enter via the cubed ECM term lagged one year, $\hat{u}_{t-1}^3$.

The nonlinear ECM terms in (*iv*) and (*v*) complicate very considerably the interpretation of the equation dynamics. In particular, they imply that the usual straightforward interpretation of error-correction coefficients—that is, that they show the speed of adjustment of the left-hand-side variable's level to the values implied by the equation's long-run solution—is not available.[90] On grounds of interpretability, therefore, the inclusion of nonlinear transformations of the error-correction term is questionable. It is correspondingly not surprising that nonlinear error-correction terms have not been widely adopted in the 30-plus years since the appearance of Hendry and Ericsson (1991a).

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[90] The error-correction term is, however, highly statistically significant, with a *t*-statistic of –6.05 that might be viewed as adding strongly to the evidence that equation (2) is a cointegrating relationship.

5.7 Lagged terms in the preferred equations

Other than the error-correction terms and the lags of the left-hand-side variable, Hendry and Ericsson's (1991a) preferred equation—(6) above—has one other explicit lagged term, Δp_{t-1}. They interpreted this regressor's place in the equation as being in conjunction with period-*t* inflation, Δp_t, with a linear combination of the two terms seen as standing in for expected future inflation.

As in separate work by Hendry and others (see the Appendix), the presence of Δp_t term was interpreted as *partly* arising from a further source: a "nominal-adjustment" specification of the behavior of money-holders in their short-run decision-making. According to such formulations, the purchasing power of nominal holdings of currency and bank deposits are allowed to diminish on impact if prices rise by more than expected, with portfolio decisions failing to make real money balances altogether insensitive to current inflation.[91]

As noted, however, aside from the nominal-adjustment rationale for including Δp_t, Hendry and Ericsson also suggested a role for Δp_t and Δp_{t-1} in standing in for next-period expected inflation: the combination of the two, they argued (1991a, p. 25), "is a predictor of next period's inflation, optimal if prices vary quadratically." Although they referred to this postulated forecasting as reflecting "optimal" and "forward-looking" behavior, it is clear that Hendry and Ericsson (1991a) did not suggest forward-looking or optimizing behavior in the rational-expectations, general-equilibrium model sense. They apparently postulated adaptive inflation expectations on the part of money-holders, with Δp_{t+1} being predicted using a fixed linear combination of Δp_t and Δp_{t-1}.

The other lagged variable in the equation is not obvious but is implied by the presence of $\Delta_2 rl_t$, the log-difference between the period-*t* and *t*–2 values of the U.K. long-term interest rate. Although Hendry and Ericsson (1991a, p. 25) interpreted this term as part of "large immediate responses to changes in inflation and interest rates," $\Delta_2 rl_t$ is equal to $[\Delta rl_t + \Delta rl_{t-1}]$—and so includes a lagged-interest-rate term. Its presence in the equation therefore raises the problem of how to interpret period-*t* responses of real money balances to old interest-rate movements. One possible way of interpreting this term arises from the absence in Hendry and Ericsson's equation of any current or future real-income-

[91] For further discussion, see the Appendix.

growth terms. It is possible that lagged movements in nominal long-term interest rates could be correlated strongly with real long-term interest rates or other real asset yields and, through that linkage, are standing in for expectations of future movements in real income.

6. Friedman and Schwartz's (1991) reply

In this section, several aspects of Friedman and Schwartz's reply to Hendry and Ericsson (1991a) are considered.[92]

The title of Friedman and Schwartz's reply, "Alternative Approaches to Analyzing Economic Data," held the promise that they would concentrate on differences in the approach and scope of *Monetary Trends* with that of Hendry and Ericsson (1991a) and not attempt an econometric rebuttal. In the event, their paper did not altogether live up to its title and did not maintain a high-level perspective on the topics at issue. Although they would have been better advised to maintain a 20,000-feet view, the authors tried to get into the nitty-gritty of comparing their 1982 regressions with those produced by Hendry and Ericsson. In so doing, Friedman and Schwartz were emboldened to make what turned out to be an ill-considered and erroneous quantitative and analytical rebuttal to the time-series and other econometric points made by Hendry and Ericsson (1991a). These erroneous positions on Friedman and Schwartz's part focused on fit and on the status of short-run money demand functions. They appeared mainly in Sections III and IV of their reply and are discussed in Sections 6.1 to 6.4 below, before Section 6.5 considers how a more effective reply might have looked like.

Ahead of this analysis, it is worth stressing that Friedman and Schwartz's (1991) text did contain, along with errors, elements of what would have been a tenable reply. They were justified in pointing out that a focus on the long-run money demand equation was a distinct and legitimate enterprise—that is, that they had not been concerned with the short run. It was coherent to think of a long-run function, although phase averaging was a misguided way to isolate it, of the function being stable even if the short-run dynamics of money demand changed over time. Their reply was also effective in pointing out that Hendry and Ericsson's long-run money demand estimates differed little from those in *Monetary Trends*.

[92] For further analysis of this reply, see Nelson (2020a, Chapter 3; 2023, Chapter 16).

Another, more negative, point, that Friedman and Schwartz made reasonably effectively was that there was a potential problem of overfitting in exercises that relied on general-to-specific-type procedures. By 1991, it was clear from Friedman's writings dating back fifty years that he was skeptical of regressions that had large amounts of explanatory variables and discounted the end result that came out of comparing various alternative specifications. It was therefore consistent with his past positions that Friedman and Schwartz (1991, p. 47) noted that Hendry and Ericsson had been "trying a large number of alternative hypotheses on a single body of data."[93] It was also likely a position shared by many econometricians who do not subscribe to the general-to-specific approach, such as Bruce Hansen (1996, p. 1402), who argued that "it is unclear how to interpret conventional tests used at the end of a specification search," while Sims (1991, p. 931) went further and suggested of the general-to-specific approach that "the *t*-statistics and standard errors presented for the final model have little meaning."

It should be acknowledged that, even by 1991, there had been some rebuttals on the part of Hendry and Ericsson to criticisms of this kind. For example, Hendry's contribution to Hendry, Leamer, and Poirier (1990) and Campos and Ericsson (1988) advanced arguments that the standard errors reported in equations that result from general-to-specific modeling are approximately valid measures of uncertainty. As the 1990s proceeded, the attention that Hendry gave to this criticism in his writings increased—a fact evident in the increase in coverage of "data mining" in the 2000 edition of his collection *Econometrics: Alchemy Or Science*? over that in the 1993 edition.

Friedman and Schwartz probably gave too much space in their reply to the issue of overfitting, particularly via their inclusion of a far-from-novel addendum on the matter in which Friedman (writing solo) recounted his own disillusionment with multiple regressions. In any event, they likely were aware that they could not rely on this argument too heavily: having produced a book themselves on U.K. and U.S. monetary trends, they could hardly take a stand that research on this subject was now ended or that they had produced the last word on the matter. Consequently, they could not really

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[93] They refrained from using the terms "data mining" and "overfitting," but they used other formulations that were familiar parts of past critical discussion of specification searching. For example, Friedman and Schwartz (1991, pp. 47–48) stated: "Put differently, it is impossible to specify how many 'degrees of freedom' have been used up in the process of reaching the final equations presented." This was, in fact, not putting the point very differently from how it had been stated in the past. For example, Hebden (1983, p. 11) had remarked: "the critics go on to say that you may have lost 'degrees of freedom' by trying many models[,] so the statistical tests of your final model should be adjusted to take account of that loss."

object to others reexamining their dataset (even if, as Friedman and Schwartz, 1991, recommended, true new tests required moving on to other datasets).

Having, however, evidently recognized that their rebuttal needed to consider Hendry and Ericsson's (1991a) results in detail, Friedman and Schwartz in their 1991 reply made a number of key analytical errors in advancing alleged points of rebuttal.

6.1 Friedman and Schwartz's (1991) reply: the role of inflation

Friedman and Schwartz (1991) objected to Hendry and Ericsson's (1991a) employment of same-period inflation as a regressor. Friedman and Schwartz (1991, p. 46) argued that Hendry and Ericsson's inclusion of the inflation rate as a regressor in their money demand regression makes it difficult to interpret as a money demand function. In reporting on their attempt to replicate the Hendry-Ericsson preferred equation, they stated that dropping Δp_t from the equation raised the SEE from 1.5 percent to 2.83 percent.[94]

Even Friedman and Schwartz's discussion in 1991, however, suggested (p. 41) that they *were* open to entertaining the lack of short-run price homogeneity as a valid property of the money demand function. This, however, was precisely a manner in which one can interpret Hendry and Ericsson's having Δp_t in equation (6) above.[95] It follows that Friedman and Schwartz's objection to including Δp_t rested on a misinterpretation. One can argue, of course, about the validity of putting contemporaneous right-hand-side variables into a least-squares regression—but this is not the ground on which Friedman and Schwartz objected to the Hendry-Ericsson inclusion of Δp_t.

6.2 Friedman and Schwartz's reply: error-correction equations as levels equations

Friedman and Schwartz (1991) made a number of erroneous points in trying to establish that Hendry and Ericsson's preferred equation was not comparable with their own U.K. money demand levels equation. In doing so, Friedman and Schwartz showed little

[94] Friedman and Schwartz (1991, p. 46, fn. 10) stated that their attempted replication of Hendry and Ericsson's model is 1.5 percent, compared with the 1.424 percent reported by Hendry and Ericsson (1991a). The equation standard error given by Hendry and Ericsson, and the accompanying regression are, in fact, exactly replicable using the *Monetary Trends* data tables. See the previous section.
[95] Hendry and Ericsson also interpreted this term as representing a cost of holding money (see the discussion above). The Appendix considers different rationales for the appearance of inflation in a dynamic money demand equation.

appreciation of the capability of an error-correction equation to serve as a means of representing both short- and long-run dynamics. They correctly noted that Hendry and Ericsson's error-correction equation "has a first difference or a rate of change as a dependent variable" (1991, p. 46). On that basis, Friedman and Schwartz made an incorrect jump to the conclusion that it was not, in effect, a levels equation. They rejected the notion, expressed in the Hendry and Ericsson (1991a, p. 27) passage that they quoted, that "direct comparison is valid"—that is, that the Hendry-Ericsson error-correction specification—equation (6) above—and the 1982 *Trends* levels specification—equation (1) above—were describing the same, levels, variable.

The Hendry-Ericsson position was correct: it is, of course, the case that the dependent variable of an equation that contains an error-correction term can be written interchangeably in levels or differences.[96] A simple and very familiar demonstration of this comes from the one-lag ADL equation (suppressing the constant):

$$y_t = b_1 x_t + b_2 x_{t-1} + b_3 y_{t-1} + e_t \,,$$

which can be rewritten:

$$\begin{aligned} \Delta y_t &= b_1 x_t + b_2 x_{t-1} + (b_3 - 1) y_{t-1} + e_t \\ &= b_1 \Delta x_t + (b_1 + b_2) x_{t-1} + (b_3 - 1) y_{t-1} + e_t \\ &= b_1 \Delta x_t + (b_3 - 1)[y_{t-1} - b' x_{t-1}] + e_t \,, \end{aligned}$$

where $b' = -(b_1 + b_2)/(b_3 - 1)$.[97]

It is surprising that Friedman and Schwartz (1991) were under a misapprehension regarding the status of error-correction equations. True, such equations had become prevalent mainly after Friedman's research activities tailed off. The error-correction aspect of older studies like Phillips (1954)—with which Friedman would have been familiar—primarily acquired prominence only in the 1980s. Nevertheless, in order to appreciate the point that a first-difference or growth-rate equation can govern the level of an equation, one does not need to be steeped in error-correction equations or in the

[96] As Hendry and Ericsson (1991b, p. 859) observed, error-correction equations, "while they have a differenced variable as the regressand, are actually expressible in levels due to the error-correction term."
[97] See also Banerjee, Dolado, Galbraith, and Hendry (1993, pp. 48–52) and Hendry (1995, pp. 213–214) on the various means by which ADL levels equations can be cast into error-correction form.

integral-derivative aspects of Phillips' error-correction equation that were stressed in such later studies as Salmon (1982).[98] Instead, one needs only to see that the growth-rate/levels linkage in ECM equations is very similar to that embedded in the theory of ordinary differential equations (with which Friedman, again, would certainly have been familiar). In that theory, the expression $\dot{x} = f(t, x)$—where $\dot{x}$ is the time derivative of x (that is, $\frac{dx}{dt}$)—is an equation "*solved explicitly for the derivative*" but is still an equation governing the level of x (Pontryagin, 1962, p. 6, emphasis in original). An equation of this kind had been used to describe the evolution of inflation in Stein's (1970) analysis of Friedman's (1969) study of real money balances.[99]

Notably, in contrast to Friedman and Schwartz's failure to appreciate that error-correction equations are tantamount to levels equations, a later paper that Schwartz coauthored, Bordo, Chouri, and Schwartz (1995), discussed this feature of error-correction dynamics explicitly and used error-correction equations extensively.

6.3 Friedman and Schwartz's reply: the SEEs of *Trends*' growth-rate equations

Friedman and Schwartz (1991, p. 46) noted that their U.K. money demand results including a levels regression but also "a parallel regression in rates of change." They immediately suggested that this *was*, other than a slight difference in sample period, comparable to Hendry and Ericsson's error-correction equation. This statement was incorrect—and so was the associated direct comparison that Friedman and Schwartz made with their rates-of-change regression SEE of 1.34 percent (given in Friedman and Schwartz, 1982a, Table 6.14, p. 283) with the 1.42 percent of Hendry and Ericsson's preferred error-correction equation.

It is clear that, in composing their 1991 reply, Friedman and Schwartz had not adequately

[98] Although there is no doubt that ECM equations did not become prevalent until Hendry's papers of 1975 onward, the 1980s literature repeatedly reminded readers that A.W. Phillips was an originator of the concept. In the wake of the many near-ritualized citations of Phillips, Yoshida (1990, p. 19) cautiously opened a discussion titled "History of ECM" with the words, "The basic premise of ECM—that people act to correct their past errors—is said to have been introduced by Phillips (1954, 1957), the well-known originator of the Phillips curve." Slightly later, P.C.B. Phillips and M. Loretan (1991, p. 407) made the more authoritative statement that A.W. Phillips provided an "early stimulus" to ECM work but that Sargan (1964) had been a "more direct stimulus" on account of his having a discrete-time ECM equation and estimating the equation using least-squares methods.

[99] See, for example, Stein's page 416.

refreshed their memory regarding what they had written in *Monetary Trends*.[100] Friedman and Schwartz (1982a, p. 281) observed, "The standard errors of estimate for level and rate-of-change regressions are not readily comparable."[101] This fact actually highlighted a disadvantage of phase-averaging: because, under phase-averaging procedures, the rates-of-change calculations refer to differences between unequally spaced observations, phase averaging of data loses the attractive properties of natural logarithms that levels and rates of change are both directly in the same percentage units.

Ericsson, Hendry, and Prestwich (1998a, pp. 408–409) found that, upon being rescaled into annual percentage units, the U.K. rate-of-change demand-for-money equation estimated by Friedman and Schwartz (1982a) had a SEE of 10.72 percent, not 1.34 percent.

6.4 Friedman and Schwartz's reply: the SEEs of *Monetary Trends*' levels equation

The Hendry-Ericsson papers of 1991 and earlier, as well as Campos, Ericsson, and Hendry (1990), were premised on a common acceptance that the SEE of 5.54 percent, given by Friedman and Schwartz (1982a) as part of the regression output associated with equation (1) and essentially replicated by Hendry and Ericsson (1991a) should be taken at face value. That is, it was taken for granted that this phase-average regression output was *in the same units* as the SEEs of annual-data regressions describing U.K. real money balances, such as those estimated by Hendry and Ericsson (1991a). Hendry and Ericsson (1983, p. 57) and Campos, Ericsson, and Hendry (1990, p. 288) both considered this a high SEE by the standards of money demand equations, and Hendry and Ericsson (1991a) compared favorably their final regression, with its 1.42 percent SEE, with *Monetary Trends*' reported SEE of 5.54 percent (or 5.66 percent in Hendry and Ericsson's 1991 replication).

After the finalization of their 1991 paper, however, Hendry and Ericsson (1990, p. 2) established that the phase-average units embedded in the Friedman-Schwartz equation meant that its SEE needed rescaling (by the average phase length) to be put into annual

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[100] An example of this, in addition to the one given presently, was Friedman and Schwartz's (1991, p. 41) apparent, and incorrect, acceptance of the statement that *Monetary Trends* had not provided formal constancy tests.

[101] Friedman and Schwartz (1982a) also noted that levels and growth-rates regression SEEs were not in the same units on their page 253. (Hendry and Ericsson, 1983, p. 57, cited pages 249–253 of *Monetary Trends* as having made this caveat. But the main expression of it was on page 253.)

units. After this adjustment, the SEE of Friedman and Schwartz's (1982a) levels equation (equation (1) above) was 10.12 percent, not 5.66 percent (a point that the authors put in print in Ericsson, Hendry, and Prestwich, 1998a, p. 409).[102]

Two points should be noted in connection with this rescaling. First, Longbottom and Holly (1985, p. 6), using the annual dataset given in Friedman and Schwartz (1982a), report similar parameter estimates similar to those found in Friedman and Schwartz's phase-average U.K. money demand specification, and a standard error of estimate for the specification on U.K. data for 1878–1970 of 5.5 percent.[103] This is similar to Friedman and Schwartz's phase-average regression's reported SEE. But the similarity in numbers do not imply that the latter equation was in annual units. Rather, it implies that the unusual restrictions on dynamics implied by phase averaging worsened the equation's fit, adding about 4.6 percentage points to the SEE.[104]

Second, like other participants in the debate through 1991, Friedman and Schwartz (1991) neglected to allow for this adjustment in comparing their levels equation's SEE with those of Hendry and Ericsson's (1991a) equations. In particular, this marred what could have been a sound comparison: that of their own levels equation, which they considered a long-run equation, with Hendry and Ericsson's (1991a, p. 24) cointegrating regression—their equation (9), given as equation (2) above. The Hendry-Ericsson cointegrating regression had a SEE of 10.86 percent. By invalidly comparing this with their phase-average equation's 5.54 percent reported value, Friedman and Schwartz

[102] Hendry (*Financial Times* (London), April 26, 2013) observed that, as Friedman and Schwartz did not appropriately adjust their SEE estimates to allow for the fact that phase-averaged data were used in the analysis, their regressions' *t*-statistics (whose calculation depends on the SEE) were also seriously unreported. This is a valid criticism. Nevertheless, it would appear that the variables in Friedman and Schwartz's U.K. money demand regression would likely prove to be significant by conventional significance criteria even if this factor's influence on *t*-statistics were taken into account. Making this result likely is the fact that, in Longbottom and Holly's (1985) annual-data version, discussed presently, of Friedman and Schwartz's U.K. phase-average specification, the scale and opportunity-cost variables had *t*-values above 2.0 (as well as estimated coefficients similar to those obtained in the phase-average case). Friedman (1988, p. 237, fn. 16) found the same result with regard to the corresponding U.S. specification.
[103] The Durbin Watson statistic obtained for the annual model is only 0.6, however.
[104] In light of this, one can put forward a different interpretation from that made by Ericsson, Hendry, and Prestwich (1988b, p. 406) of the effect of phase averaging. They suggested that the high SEE of phase-average regressions should be interpreted as "implying that phase averaging failed entirely in its ostensible purpose—to remove the short-run fluctuations in the data." Phase averaging was not designed to remove all short-run fluctuations but was certainly intended to remove some of them. Phase averaging appears to have reduced the variation of individual series somewhat: see Hendry and Ericsson's (1991a, p. 17) plot of velocity. This is perfectly consistent with phase-average *regressions* having larger residual standard deviations than their annual-data counterparts, as those regressions imposed the restrictions on dynamics described here.

(1991, p. 46) concluded: “The standard error of estimate for regression 9 is nearly twice the standard error of estimate of our regression.” Comparing the long-run equations was a legitimate exercise, but the numerical contrast was invalid for the reasons given above. In fact, the fits of the Hendry-Ericsson cointegrating regression and the Friedman-Schwartz long-run equation equations were quite close to one another—being, as already indicated, 10.86 percent and 10.12 percent respectively, both very large values.

6.5 Friedman and Schwartz’s reply: equation standard errors under dynamics

Hendry and Ericsson’s (1991a, p. 24) unrestricted levels equation (almost an ADL specification but, as already noted, including terms involving $\hat{u}_{t-1}$) had a SEE of 1.55 percent. Friedman and Schwartz contended that, as the estimated sum of coefficients on the lagged dependent variable (log money) was 0.90, the SEE of Hendry and Ericsson’s equation “is 15.5 percent.” Elaborating on this claim, Friedman and Schwartz argued, “it [the SEE] is given as 1.55 [percent], but that has to be multiplied by 10 to be comparable to [Friedman and Schwartz’s (1982a) equation, which had no explicit lagged dependent variable]... since the long-run dependent variable is 0.1*m*.”

According to Friedman and Schwartz (1991), then, the appropriate measure of the SEE of a dynamic regression model amounted to:

$$SEE_{FS} = SEE \cdot \{|\text{Sum of Coefficients on the Lagged Dependent Variable} - 1|\}^{-1}.$$

So any specification that excludes the lagged dependent variable from the list of regressors will have:

$$SEE_{FS} = SEE.$$

And any dynamically convergent ADL equation will have:

$$SEE_{FS} \geq SEE,$$

with strict inequality prevailing whenever (as is basically inevitable when, as in Hendry and Ericsson, 1991a, the coefficients on lags of the dependent variable are positive and estimated freely) the coefficients on the lagged dependent variable do not sum exactly to zero.

If Friedman and Schwartz's measure of the standard error of estimate is accepted, it leads to a strong preference for equations of the distributed-lag or static type that exclude lags of the dependent variable. This likely helps explains Friedman's longstanding aversion to including lagged dependent variables in regressions (see Nelson, 2020a, pp. 443, 554) and his favoring their omission even in his quarterly-data study, Friedman (1988). It is also the case that in his analytical work on monetary matters, Friedman tended to stress the unconditional variance of shocks rather than their (often much smaller) conditional variance conditional on the period-(t–1) information set.[105]

Distributed-lag and static specifications—although they meet the criterion of omitting lagged dependent variables—are frequently empirically inadequate, as was discussed by Hendry, Pagan, and Sargan (1984, pp. 1043–1044). In any case, the explosion of empirical work using Box-Jenkins and later VAR methods made the use of lagged dependent variables in dynamic modeling routine, and in those settings Friedman and Schwartz's (1991) proposed adjustment to the SEE is not, typically, made.

Hendry and Ericsson (1990) rejected Friedman and Schwartz's proposed adjustment—but implied that the adjustment likely had merit when constructing confidence intervals for forecasts that applied to periods many years ahead. Several vintages of the PcGive package from the 1990s onward actually included in their reported regression output "Long-run sigma," which essentially corresponds to what is called SEE_{FS} above.

6.6 How should Friedman and Schwartz have replied?

In light of the fact that Friedman and Schwartz's (1991) reply was undercut by numerous analytical errors and mistaken numerical comparisons on their part, the question arises: Could a more successful reply have been delivered—one that gave a sounder defense of some of the money demand work in *Monetary Trends*? The answer is yes. A more modest reply, focused on *long-run* money demand estimation, would have been more effective. Some elements of that hypothetical reply are now sketched.

First, whereas, in Friedman and Schwartz (1991), the authors appeared to suggest that they would still do phase averaging of the data if they had to start their project from scratch, they could instead have pointed to the NBER origins of the work and concurred

[105] McCallum (1998) argued that Friedman's (1953b) stress on the unconditional, rather than conditional variance, set that study apart from most later formal analysis of stabilization policy.

that phase averaging had, in the event, not become a widely accepted practice. This admission would have been a realistic concession—and would still have left Friedman and Schwartz able to state their agreement with Hendry and Ericsson (1991a) that the basic trajectories of the phase-average data very much resemble those of the annual data.

Second, Friedman and Schwartz could have acknowledged, as they did in the text of *Monetary Trends*, that their dataset consisted of simultaneously determined variables.[106] In light of that, their use of OLS-type estimators should be regarded as a process of generating approximate versions of equations that should more validly have been obtained by simultaneous methods. The Friedman and Schwartz (1991) reply does have some articulation of this point. But its expression is marred by the overconfident statements made in that reply about the prospect that running regressions "both ways" serves as a means of allowing for simultaneity. A further point that should be noted here is that, in light of the cointegration literature, it could also have been possible to argue that, as the levels variables under consideration in *Monetary Trends* were likely predominantly $I(1)$ series, OLS or OLS-type estimators actually might be asymptotically valid in the presence of simultaneous determination of the variables.

Third, Friedman and Schwartz could have retained the valid point that Hendry and Ericsson's estimates of the income elasticity and interest semielasticity of money demand were similar to theirs: respectively about 1.0 and close to –10.0 in both studies. This point was stressed in Laidler's (1989, p. 1157; 2006, p. 65) discussion of the debate, but in Friedman and Schwartz (1991), the statement of it was overshadowed by the authors' insistence on emphasizing the supposed value of reverse regressions.

Fourth, Friedman and Schwartz should have built much more on their point (p. 47), "Their [Hendry and Ericsson's] regressions are designed to explain the short-term adjustment process as well as the long-term relation. We had no such ambitious aim." The cointegration literature has provided retrospective validation to the concept of a constant-parameter money demand function being a *long-run relationship*—though, of course, it has not at all vindicated Friedman and Schwartz's implementation of that concept using phase averages. In particular, what the cointegration literature implies is that, provided that Friedman and Schwartz's (1982a) U.K. levels money demand equation is viewed as a cointegrating relationship and the variables in that equation truly do

[106] Friedman and Schwartz (1982a, p. 343) stated: "In principle, all the variables are simultaneously determined…"

cointegrate (at the coefficients estimated), then their equation is, indeed, a stable long-run money demand relationship.

In their 1991 reply, Friedman and Schwartz undercut the making of this point by deciding to compare the SEEs of their levels equations with the dynamic equations estimated by Hendry and Ericsson (1991a). Despite this misguided part of their 1991 reply, the basic notion that a long-run equation like Friedman and Schwartz's might be viewed as a stable demand-for-money function is a tenable one. Crucially, this can be the case *even if* deviations from the long-run relationship have a large, and even fluctuating, variance. In the classic econometric studies of long-run U.S. money demand by Lucas (1988) and Stock and Watson (1993), the authors made clear that they did not regard the existence of large, highly serially correlated, and heteroskedastic deviations of velocity from their long-run velocity equation as discrediting, or undermine the validity of, that estimated velocity function.[107]

Lucas and Stock-Watson therefore viewed money demand stability as a matter of the long-run function possessing constant parameters. This corresponds to a narrower definition of parameter constancy than that in Hendry and Ericsson's exercises—which emphasized formal econometric stability, on the criterion of the Chow and other tests, of the money demand equation as a regression function, including constancy of its residual variance.[108] But the concept of long-run parameter stability as the appropriate definition of money demand constancy has come to be accepted in the research literature over the past 35 years. For example, in U.S. data, Belongia and Ireland (2019, p. 3) take a cointegrating vector whose coefficients are interpretable as money demand parameters as implying "evidence of stable money demand relationships."

If this view of money demand constancy—one in which constancy corresponds closely to the existence of a cointegrating relationship—is accepted, it also puts in a new light the comparisons that Hendry and Ericsson (1985) made between Friedman and Schwartz's

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[107] Specifically, Stock and Watson (1993, p. 807) described their cointegration finding as indicating that "long-run M1 demand has been stable" and did not regard the fact that deviations from the relation were large in the postwar period as evidence against stability or as making a decisive case against using the long-run relation as the preferred low-frequency specification (p. 804). Earlier, Lucas (1988, p. 146) treated stable long-run money demand as implying stable values of the income elasticity and interest semielasticity of money demand and stated that he did not expect the deviations from the long-run function to exhibit an unchanged "structure over the entire period."

[108] Nevertheless, they acknowledged the possibility of situations in which the long-run relationship between money, prices, and real income continued to hold in the face of shifts in the short-run relationship. See Hendry and Ericsson (1991a, p. 32).

(1982a) money demand equation and a random-walk model of U.K. log velocity. The *Trends* equation, they stated, was "variance-dominated by"—that is, had a lower SEE than—the annual equation $\Delta v_t = c_0 + \varepsilon_t^v$—"and *hence they* [Friedman and Schwartz] *cannot even encompass that elementary hypothesis*."[109] Δv_t certainly has a lower standard deviation than the residuals of Friedman and Schwartz's money demand equation. But that may not be an economically germane benchmark against which a long-run equation should be judged.

Here, a parallel with another familiar long-run relationship may be useful. The error term ε_t^κ in a cointegrating term-structure relationship $RL_t = \kappa + RS_t + \varepsilon_t^\kappa$ may well have a higher standard deviation than the first difference series ΔRL_t and ΔRS_t. But a finding of a lower standard deviation either of either ΔRL_t or ΔRS_t, or both, than that of ε_t^κ in no way refutes or undermines the status of the term-structure relationship as an economically important long-run function—and does not imply that that function has been found wanting and should be replaced or discarded. Put differently: There is no onus on a long-run equation describing the level (or log level) of a series to have a SEE that is lower than the standard deviation of the first difference of that series or of a regression describing the behavior of that first difference.[110]

7. Conclusion

The econometric criticisms that Hendry and Ericsson (1991a) made of Friedman and Schwartz's 1982 book *Monetary Trends* had considerable merit. Few, if any, researchers today would argue for the use of phase-averaged data instead of the original data series, and Hendry and Ericsson pointed up many key disadvantages of the process of phase averaging. Furthermore, Friedman and Schwartz's approach to dynamic econometric modeling appears was unsophisticated and made further unacceptable by developments in empirical and theoretical time-series work in the late 1970s and the 1980s, including work on ECMs (in the development of which Hendry played an especially key role), cointegration, unit roots, and VARs. The emphasis in *Monetary Trends* and Friedman and Schwartz's 1991 reply on the value of reverse regressions as a means of addressing endogeneity was old-fashioned and was jarringly ill-suited to a time series context. On

[109] Hendry and Ericsson (1985, p. 24; emphasis in original). A similar comparison appears in Ericsson, Hendry, and Hood (2016).

[110] To emphasize: It is not disputed that a random-walk time-series representation of log velocity has a lower SEE than that of Friedman and Schwartz's implied long-run velocity equation—only that such a better fit does not provide a valid economic criterion on which to dismiss that long-run velocity equation.

all these counts, Hendry and Ericsson's critique—although it was grounded specifically on the general-to-specific methodology that they advocate—largely consisted of standing up for modern time-series practices that were used across econometric methodologies.

The implications of monetary economics of the critique of *Monetary Trends* are considered in detail in Nelson (2024). Those implications can only be briefly touched on here, with the emphasis on money demand estimates. Friedman and Schwartz could feel justified that many of their qualitative results withstood the critique of Hendry and Ericsson (1991a). For example, as Friedman and Schwartz stressed (and as earlier highlighted by Laidler, 1989, and as mentioned in Section 6 above), Hendry and Ericsson's estimates implied values of the long-run income elasticity and interest semielasticity of money demand that closely resembled those in Friedman and Schwartz (1982a).[111] Furthermore, although Hendry and Ericsson criticized Friedman and Schwartz's means of reaching the conclusion that the demand for money was homogeneous with respect to prices, they reached the same conclusion using their own approach.

It has been emphasized, however, that this defense of the economic results in *Monetary Trends*, although present to some extent in Friedman and Schwartz (1991), was overshadowed by their efforts to establish a victory on the econometric points. Friedman and Schwartz's position that their own money demand equations had fits better than, or comparable with, those of Hendry and Ericsson (1991a) was mistaken, and their interpretation of the status of error-correction equations was misplaced. Hendry and Ericsson (1991a) had highlighted genuine shortcomings of the Friedman-Schwartz regression work. In so doing they bore out the description that they had given to their appraisal when they first drafted it in 1983: "a pro-econometrics tract."

[111] In addition, Friedman and Schwartz's (1982) estimate of a 20 percent money demand shift during the interwar years was supported by Hendry and Ericsson's (1983) equation, and although Hendry and Ericsson's 1991 preferred equation excluded this dummy variable, they indicated that there may be some merit in including such a shift term: see Hendry and Ericsson (1991a, p. 26).

Appendix. Inflation and the dynamics of the demand for money

There are two leading ways, not mutually exclusive, in which inflation has been advanced as an influence on the short-run dynamics of the demand for money.

***Inflation as an opportunity cost*:** In Friedman (1956), inflation, Δp_t, is presented as an opportunity-cost variable. If the other opportunity-cost variable is RS_t, the long-run money demand function might look like

$$m - p = c_0 + c_1 y + c_2 RS + c_3 \Delta p.$$

An error-correction equation that allows the opportunity-cost variables to matter for period-*t* decisions regarding real money balances might look like:

$$\Delta(m - p)_t = a_{10} + a_{11}\Delta RS_t + a_{13}\Delta^2 p_t - a_{12}(m_{t-1} - p_{t-1} - c_0 - c_1 y_{t-1} - c_2 RS_{t-1} - c_3 \Delta p_{t-1}).$$

A white-noise period-*t* error term would also be included in the above expression but is suppressed here. This expression can be rewritten:

$$\begin{aligned}\Delta(m - p)_t &= a_{10} + a_{11}\Delta RS_t + a_{13}\Delta^2 p_t + a_{12} c_3 \Delta p_{t-1} - a_{12}(m_{t-1} - p_{t-1} - c_0 - c_1 y_{t-1} - c_2 RS_{t-1}) \\ &= a_{10} + a_{11}\Delta RS_t + a_{13}\Delta p_t - (a_{13} - a_{12} c_3)\Delta p_{t-1} \\ &\qquad - a_{12}(m_{t-1} - p_{t-1} - c_0 - c_1 y_{t-1} - c_2 RS_{t-1}).\end{aligned}$$

This reformulation shows that the appearance of Δp_t on the right-hand-side of an error-correction equation for $\Delta(m - p)_t$ can be rationalized by the dependence of long-run real money demand on the inflation rate.

Nominal adjustment specification: Goldfeld (1973, p. 611) suggested that, perhaps on account of costs faced by money-holders in adjusting their nominal balances, the money demand function might not have short-run homogeneity with regard to prices. The resulting "nominal adjustment" specification can be written, if one ignores the error term, in a partial-adjustment framework as (see Rasche, 1990, p. 50):

$$(m - p)_t = a + b_1 y_t - b_2 RS_t + b_3 (m - p)_{t-1} + b_4 \Delta p_t,$$

or

$$\begin{aligned}\Delta(m-p)_t &= a + b_1 y_t - b_2 RS_t + (b_3 - 1)(m-p)_{t-1} + b_4 \Delta p_t \\ &= a + b_1 \Delta y_t - b_2 \Delta RS_t + (b_3 - 1)(m-p)_{t-1} + b_1 y_{t-1} - b_2 RS_{t-1} + b_4 \Delta p_t \\ &= a + b_1 \Delta y_t - b_2 \Delta RS_t + (b_3 - 1)[(m-p)_{t-1} - c_1 y_{t-1} - c_2 RS_{t-1}] + b_4 \Delta p_t\end{aligned}$$

in which $c_1 = b_1/[b_3 - 1]$, $c_2 = -b_2/[b_3 - 1]$. Again, when written in error-correction form, this specification delivers a term in Δp_t in the real-balances equation.

The two together: Hendry's work on money demand rested on both these rationales for including inflation in the real-balances ECM equation (see Hendry and Doornik, 1994, p. 31). Hendry and Ericsson (1991a) appealed to Friedman's (1956) opportunity-cost argument, while Hendry (1985, p. 80) also suggested that "the empirical evidence here and in the USA points directly to agents planning in *nominal* terms in the short run" (emphasis in original; see also Hendry, 1979, pp. 238–239).

As stressed by Goldfeld and Sichel (1987), it is difficult to distinguish econometrically between these two rationales for the appearance of current inflation in a dynamic equation describing the behavior of real money balances. What is clear, however, is that *either* rationale is consistent with the equation being a demand-for-money equation. This consistency implies that Friedman and Schwartz's (1991, p. 46) were misplaced in suggesting that Hendry and Ericsson's (1991a) inclusion of inflation in their equation describing $\Delta(m-p)_t$ rendered that specification incapable of being interpreted as a money demand function.

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