The Friedman-Schwartz/Hendry-Ericsson Debate on U.K. Monetary Trends: A 40-Year Retrospective

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April 10, 2024

Abstract

A retrospective is provided on the debate on modeling long-run U.K. money demand launched by David Hendry and Neil Ericsson's 1983 critique of econometric results reported in Milton Friedman and Anna Schwartz's 1982 book on monetary trends. The analysis lays out the evolution of the debate into a 1991 published exchange and evaluates the implications of the debate for monetary economics. It is concluded that Hendry and Ericsson's econometric critique of Friedman and Schwartz was substantially valid, but that their appraisal did not have the implications that opponents of monetarism in the 1980s claimed it had. These commentators attempted to draw from the Hendry-Ericsson study the conclusion that a monetary policy-focused inflation-control strategy was misconceived. In fact, the Hendry-Ericsson appraisal did not have this implication and was quite consistent with U.K. inflation being the result of lagged monetary forces.

* Email: Edward.Nelson@frb.gov. The author is indebted to Oliver Bush, Martin Ellison, Emily Markowitz, Paolo Surico, and Ryland Thomas for their very substantial help in locating archival items used here. The author alone is responsible for errors. In addition, the views expressed in this paper are the author's and should not be attributed to the Federal Reserve System or the Board of Governors.

JEL classification codes: E41; E51; E58.
Key Words: Demand for money, inflation, monetarism, monetary aggregates, nonmonetary view of inflation, general-to-specific methodology.

1. Introduction

David Hendry (1993, p. 115) noted that the topic of the demand for money "became the focus of my interest around 1977." Over the following several years, this shift in focus would bring him prominently into the debate in macroeconomics that had been taking place—both in the U.K. research world and in U.K. public discourse—on monetarism. Hendry's move during the late 1970s and early 1980s toward concentrating on monetary issues in his empirical research would ultimately lead him and his coauthor, Neil Ericsson, into a direct exchange with Milton Friedman and Anna Schwartz on the topics of the demand for money and the analysis of monetary aggregates.

The reason for this outcome was that Hendry's shift into research on U.K. monetary developments occurred at the same time as the completion and publication of Friedman and Schwartz's (1982) final jointly written book on monetary matters, *Monetary Trends in the United States and the United Kingdom: Their Relation to Income, Prices, and Interest Rates, 1867–1975*. As part of the empirical work laid out in that volume, Friedman and Schwartz reported an econometric equation describing the longer-run behavior (over a period spanning approximately a century) of the demand for real broad money in the United Kingdom. Following *Trends*' publication, Hendry and Ericsson were commissioned by the Bank of England to write an analysis of the book's findings concerning the United Kingdom. Their paper provided a critical analysis of the *Monetary Trends* specification of the U.K. demand for money, while offering an alternative approach. The Bank of England symposium session at which their paper was presented took place in London on the afternoon of Friday, October 28, 1983. The Bank published a summary of the event in its December 1983 *Quarterly Bulletin* (Bank of England, 1983) and shortly thereafter issued a compendium of the symposium contributions, including Hendry and Ericsson (1983).

The release and circulation of the Hendry-Ericsson paper during 1983–1984 generated a great deal of comment and controversy in U.K. media and political circles. This amounted to a new, major episode in the long-running debate on monetarism and on Milton Friedman's views in U.K. public discourse.[1] At a slower pace, the corresponding research debate generated by the Hendry-Ericsson critique proceeded over the rest of the 1980s. The profile of the debate in the media and among researchers was sufficient to persuade Friedman to make a new exception to his usual rule of not responding to critics

[1] See Nelson (2009, 2017) for extensive overviews of this longer-term debate.

in print (a rule that was not, in any case, subscribed to by Anna Schwartz—who was much more inclined than her long-time coauthor to pen rebuttals to critiques of research in which she had participated). The outcome was the pair of "co-papers" of Hendry and Ericsson (1991a) and Friedman and Schwartz (1991) that appeared in the March 1991 issue of the *American Economic Review*.

In the analysis provided in this paper, I scrutinize key aspects of the debate between Friedman and Schwartz and Hendry and Ericsson and place the debate in context by considering key monetary-economics developments occurring during the period that covered the emergence, development, and finalization of the key contributions in the debate—in particular, the late 1970s through the very start of the 1990s. In addition to covering the debate that took place in the research literature, I provide an analysis of the public-discourse dimension of the discussion of the debate—in particular, the U.K. and international media coverage, which was concentrated in the initial 1983–1984 period.

The focus is on the implications for *monetary policy issues* of the Friedman-Schwartz/Hendry-Ericsson economic-research debate. The econometric aspects of the debate are covered in detail in Nelson (2024). It is concluded there that Hendry and Ericsson's econometric critique of Friedman and Schwartz was substantially valid.[2] Here, instead, what is under investigation is what the debate implied for the merits of the economic positions with which Friedman and Schwartz were associated and that they advanced in their books and other writings. This, in turn, bears on the question of whether the fact that Hendry and Ericsson were largely vindicated on the econometric points at issue implies that—as U.K. public commentary during the 1980s frequently claimed—the monetary analysis as propounded by Friedman over many years had been decisively undermined.

The conclusion reached in this paper is that the Hendry-Ericsson appraisal did not have the implications that opponents of monetarism in the 1980s claimed for it. These commentators attempted to draw from the Hendry-Ericsson study the conclusion that a monetary policy-focused inflation-control strategy was invalid. In fact, the Hendry-Ericsson appraisal did not carry this implication, and the authors' findings were quite consistent with U.K. inflation being the result of lagged monetary forces.

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[2] One matter on which it is contended that the critique was *not* valid was Hendry and Ericsson's implication that a random-walk representation of velocity was a result contrary to monetarist thinking. This part of the critique, which they articulated in writings other than the 1991 *AER* article, is addressed in Section 4 below.

For their part, Hendry and Ericsson (1985, p. iii) argued that their "paper… is not an 'anti-monetarist' critique."[3] The detailed analysis of the monetary-economics aspects of the Friedman-Schwartz/Hendry-Ericsson debate paper's analysis provided in this paper confirms that the Hendry-Ericsson critique was not fundamentally an "'anti-monetarist' critique." This finding is important, as it helps in obtaining an understanding of why—although Hendry and Ericsson correctly found fault with the econometric work of Friedman and Schwartz (1982)—many of Milton Friedman's propositions concerning monetary economics had, by 1983, become widely accepted in researchers' and practitioners' monetary analysis in the United States and the United Kingdom and why they continue to receive this acceptance 40-plus years later. [4]

In particular, there continues to be widespread adherence to numerous basic positions that Friedman advanced after becoming a monetarist in the early 1950s—with the acceptance of his monetary propositions is especially strong in the case of the large subset of those propositions that did not hinge on focusing monetary policy or monetary analysis on measures of the quantity of money or its rate of growth. Among the strongly-accepted positions are: the desirability of carrying out a monetary policy oriented toward price stability in the context of a floating exchange rate, with the analytical framework underlying such a policy embedding a recognition that there is no long-run inflation/unemployment tradeoff; the distinction between real and nominal interest rates; the dichotomy between the factors determining the respective courses of long-term real and nominal national income; and a realization that incomes policy (that is, direct wage and price interventions) cannot make a useful contribution to the control of inflation. Many modern-day models used for monetary policy analysis incorporate these positions.

A number of further specific analytical positions that Friedman and Schwartz advanced in *Monetary Trends* and that, prior to that, were already key facets of the monetarist side of the Keynesian-monetarist debate also continue to be widely accepted today and are part of the modern mainstream of macroeconomics. These include the following postulates: there is no Keynesian liquidity trap (that is, when nominal interest rates on short-term riskless assets are very low, monetary policy remains capable of stimulating the economy

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[3] This remark, in the abstract of the 1985 Federal Reserve Board working paper version of Hendry and Ericsson's paper, was similar to a passage in the earlier, 1983, Bank of England paper version. Hendry and Ericsson (1983, p. 28) remarked that their "paper… is not an 'anti-monetarist' polemic; rather, it is a pro-econometrics tract."

[4] In view of its importance, this point is elaborated in some detail here in the introduction, rather than left solely to later sections.

by reducing other yields relevant for aggregate spending decisions); nominal and real aggregate economic variables, though determined by different factors in the long run, are each importantly affected by monetary policy in the short and medium runs; and monetary policy (as determined by the reaction function of the central bank) dominates the behavior of inflation over time.

Some of the postulates listed in the preceding two paragraphs are captured, and reflected, in Friedman's famous proposition—one that he made many times in the course of 1963–2003 but that was not, as it happened, explicitly given in *Monetary Trends*: "Inflation is always and everywhere a monetary phenomenon."[5] The empirical findings provided in the Hendry-Ericsson critique of *Monetary Trends* did not overturn this proposition, and the acceptance of the proposition as part of the modern consensus has been consolidated since 1983.

In the course of providing an elaboration of this state of affairs and how it emerged, it is argued in this paper that the Hendry-Ericsson 1983–1991 findings were fully compatible with the notion that inflation is always and everywhere a monetary phenomenon. Most specifically, Hendry and Ericsson's (1991a, p. 30) observation that their "evidence is inconsistent with the hypothesis that, over the period 1878–1970, exogenous money determined prices in the United Kingdom" is fully consistent with the following hypothesis: *Over the century to the mid-1970s, lagged monetary forces—which, over that period, were well summarized by U.K. monetary growth—steered the course of inflation in the United Kingdom in the medium and long run*.[6] It is further argued that this hypothesis is not only consistent with Hendry and Ericsson's findings but also is the consensus account of the historical behavior of U.K. inflation.

The consistency between the two hypotheses just stated helps explain why judgments such as "Milton's monetarism is bunk" (words that appeared as an early newspaper headline reporting the initial Hendry and Ericsson findings) have not been widely taken as flowing from the 1983–1991 Hendry-Ericsson critique of *Monetary Trends*. The soundness, and widespread acceptance among economists, of the basic monetarist or Friedman-associated propositions (set out above) concerning macroeconomic behavior did not at all rest on the validity of the data-transformation and econometric procedures

[5] See Friedman (1963, p. 17 [p. 38 of 1968 reprint]) for his early statement of this proposition.
[6] Some of these influences may become manifest with lags shorter than a year and so may appear as contemporaneous influences when considering relations expressed in terms of annual-average data.

followed in *Monetary Trends*—including that book's problematic use of "phase averaging" and its inadequate allowance for variables' dynamics and simultaneity. There would likely be wide agreement that Hendry and Ericsson came off best in the econometric disputations of 1983–1991. Fully consistent with this outcome, however, is the fact that the notion that inflation is a monetary phenomenon has proved durable and, over the past four decades, has been expounded during their tenures by such central bank heads as Paul Volcker, Alan Greenspan, Ben Bernanke, and Mervyn King.[7]

Consideration of the monetary-economics aspects of the Friedman-Schwartz/Hendry-Ericsson debate affirms the value of Hendry's regular reminders to his readers that a rejection of one hypothesis does not imply acceptance of a specific alternative. As Hendry (1983, p. 194) put it in an article that appeared in print around the time when the Hendry-Ericsson *Trends*-related project got underway: "Rejecting a null hypothesis against an alternative does not entail the validity of the latter." More specifically, Doornik and Hendry (1994, p. 161) noted: "It is a dangerous *non sequitur* to adopt the alternative hypothesis of the test which rejected…" In the case of the U.K. monetary debate, a particular example of such a dangerous *non sequitur* occurred during the mid-1980s, when advocates of nonmonetary approaches to the analysis and control of inflation seized on the Hendry-Ericsson findings and treated it as validating their own *particular* critique of monetarism and of the Thatcher Government's demand-restriction-based approach to controlling inflation.

The most notable development in this vein was the fact that, in the aftermath of the first version of the Hendry-Ericsson paper, critics of Friedman in the United Kingdom such as Nicholas Kaldor pointed to the findings as vindicating their own hardline Keynesian position. This inference on their part was not well founded—indeed, it was seriously mistaken. Consistent with that inference being mistaken, in the years after 1983–1984 the U.K. economic consensus on matters concerning the importance of monetary factors, *vis a vis* nonmonetary factors, in the analysis and control of inflation consolidated *against*, not in favor of, the hardline Keynesian perspective. In fact, hardline Keynesians like Kaldor and Wynne Godley were not well positioned to associate themselves with the econometric criticisms that Hendry and Ericsson had advanced, as Hendry had been publicly critical of Godley's empirical work (see Section 2 below), while Kaldor's own limited ventures into econometric analysis had actually involved heavy reliance of one of

[7] The views of the first three of these individuals on inflation are detailed in Nelson (2023a, 2023b). See also the discussion in Sections 3 and 6 below.

the procedures (rearrangement of estimated regression equations) that Hendry and Ericsson had criticized *Monetary Trends* for deploying (see Nelson, 2024).

Most fundamentally with respect to the economic dimension, the substance of Hendry and Ericsson's critique did not imply an endorsement of the postwar U.K. tradition of adopting a special-factors-oriented, or nonmonetary, approach to analyzing inflation.[8] Kaldor was a strong adherent to that tradition—being an outspoken advocate of using incomes policy as a means of fighting inflation, and strongly opposed to using demand restraint for this purpose. Belief in the basic validity of such a nonmonetary approach to inflation underlay much pre-1979 U.K. macroeconomic policy and also enjoyed heavy support among economists in the U.K. research world over the first three postwar decades. But, as Hendry (1986, p. 201) observed: "The traumas of the 1970s fragmented the existing consensus on how the macroeconomy functioned." Over the post-1979 period, the nonmonetary approach to inflation that Kaldor championed was eschewed by the Thatcher Government and its successors in designing their policy frameworks. These post-1979 frameworks relied on monetary policy as the means of providing the economy with a nominal anchor (successively via monetary-growth targeting over much of the 1980s, a fixed exchange rate *vis a vis* the German mark during the early 1990s, and inflation targeting since 1992). A monetary-policy-focused approach to the analysis and control of inflation came to predominate in U.K. research circles as well—becoming the new consensus—and would be embedded in widely used New Keynesian models.

It should be noted that the retrospective given here does not cover all aspects of the Friedman-Schwartz/Hendry-Ericsson debate on the U.K. findings of *Monetary Trends*. The concentration in this paper is on exchanges appearing through the late 1990s. My analysis will not systematically consider the twenty-first-century follow-ups to the debate, including Ericsson, Hendry, and Hood (2016). In this connection, it is worth mentioning also that the analysis in this paper—like the Friedman-Schwartz/Hendry-Ericsson debate itself—puts aside contesting views of the importance of monetary policy versus fiscal policy. This omission is worth stressing, because the modern version of the monetary-fiscal debate (one centered largely on the fiscal theory of the price level, or FTPL) has become a new front of battle in arguments regarding Friedman and Schwartz's joint work (albeit one very much focused on their *Monetary History* and not *Monetary Trends*). For example, in recent contributions to the FTPL literature, the concentration by

[8] A "nonmonetary" view in this context actually eschews appealing to fiscal policy, and not just monetary policy, as the source of inflation.

both monetarists and New Keynesians on monetary policy in their analyses, and the primary role that both groups have assigned to monetary factors in understanding the historical behavior of output and inflation, have come under challenge (see, for example, Cochrane, 2023, and Jacobson, Leeper, and Preston, 2023). In addition, Bordo, Bush, and Thomas (2022) have argued that monetary policy-focused accounts of the United Kingdom's Great Inflation of the 1970s need to be supplemented by greater consideration of monetary policy/fiscal policy connections and, particularly, of the role played by the monetary accommodation of fiscal deficits in giving rise to periods of high U.K. inflation.

This paper proceeds as follows. Section 2 gives the pre-1983 background to the debate, including the initial reaction to *Monetary Trends* in 1982 and through mid-1983. This section also outlines Friedman and Schwartz's (1982) U.K. money demand equation. Section 3 considers the chronology of the debate from 1983 to 1990. This section covers some aspects of the Hendry-Ericsson 1983 paper and reactions to the emerging debate that appeared in the media and in research contributions. These reactions included attempts to interpret Hendry and Ericsson's findings as bearing on the causes and control of inflation. Section 4 discusses Hendry and Ericsson's *American Economic Review* article, while Section 5 considers the follow-up research of Ericsson, Hendry, and Prestwich (1998a, 1998b)—which, by embracing *Monetary Trends*' measure of the main opportunity cost of holding money, brought a measure of convergence of the two sides of the debate regarding the preferred demand-for-money specification. Section 6 asks what implications for monetary economics flowed from the Friedman-Schwartz/Hendry-Ericsson debate. It is argued that Hendry and Ericsson's results did not have a material bearing on the validity of Friedman's basic propositions in monetary analysis, particularly those concerning inflation. Section 7 provides some concluding remarks.

2. Background to the debate: 1966–1982

Monetary Trends was published in mid-1982. Nevertheless, in order to gain an appreciation of the contours of the Friedman-Schwartz/Hendry-Ericsson debate, it is worth going back to the mid-1960s. Starting the chronology earlier than 1982 sets the scene by providing a means of discussing the research background and activities of each side—Hendry and Ericsson, and Friedman and Schwartz. It deserves underlining that what follows is not an attempt to provide a guide to the development of David Hendry's thinking or a detailed account of his methodology or research output (on these matters,

see, among others, Pagan, 1987; Hendry, 1993; Ericsson, Hendry, and Tran, 1990; and Ericsson, 2004, 2021). Nor is it an exegesis of Friedman's monetary analysis, his career as an economist, his empirical work, the Friedman-Schwartz monetary project, or the Keynesian-monetarist debate—which are all subjects covered in great detail in Nelson (2020a, 2020b), on the period 1932–1972, and in Nelson (2023), on 1973–2006.[9]

The present section will also be mainly concerned with some of the main research activities of each side, while reserving for later sections the coverage of each side's perspectives on certain specific monetary and other economic issues.

2.1 The Hendry-Ericsson side

Hendry was a graduate student, initially for a master's degree, at the London School of Economics over the second half of the 1960s, starting in 1966 (Ericsson, 2004, pp. 743–744, 748; Ericsson, 2021, p. 1) and went on to receive a Ph.D. from the university in 1970 (Ericsson, Campos, and Tran, 1990, p. 98). He was on the LSE's teaching staff of the university over the period from 1969 to 1981 (Hendry, 1993, p. xvii)—in posts successively as a Lecturer (1969–1973), Reader (1973–1977) and Professor of Econometrics (1977–1981) (A&C Black, 1987, p. 798).

Hendry moved to Oxford University at the start of 1982 as Professor of Economics (A&C Black, 1987, p. 798; Ericsson, 2004, p. 753). Oxford University—most specifically, Nuffield College—was Hendry's affiliation throughout the course of the debates from 1983 onward with Friedman and Schwartz (and remains his affiliation today).[10] Neil Ericsson had been one of Hendry's final graduate students at the London School of Economics, receiving his Ph.D. in 1982 (American Economic Association, 1997, p. 164). Ericsson was affiliated with Nuffield College during the period when he and Hendry produced the Bank of England-commissioned paper on *Monetary Trends*, being a research officer at the college in 1983 (American Economic Association, 1997, p. 164). Ericsson and Hendry co-presented their paper on *Trends* at a Bank of England Panel of

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[9] The discussion that follows will also leave out of its coverage most of the wider development occurring in econometric and time-series worlds over this period—some aspects of these relevant to the Friedman-Schwartz/Hendry-Ericsson debate being covered instead in Nelson (2024).

[10] Hendry has had a series of additional affiliations with the university. The principal second affiliations have included the Institute of Economics and Statistics in the 1980s (with Hendry serving as acting director 1982 to 1984: see Blaug, 1986, p. 393), the Department of Economics from its formation in the late 1990s until the mid-2010s; and Climate Econometrics (formed at Nuffield College in 2015).

Academic Consultants session (Bank of England, 1983). This event (held on October 28, 1983, as earlier noted) took place just before Ericsson moved to Washington, D.C., to begin his employment (which is ongoing at the time of writing) as an economist at the Division of International Finance of the Federal Reserve Board.[11]

As indicated in the introduction, in his research applications Hendry had moved to monetary issues in the late 1970s. With coauthors, he produced a series of empirical studies of U.K. money demand, employing the "general to specific" econometric methodology that Hendry and coworkers were developing.[12] Early work in this area included Hendry and Mizon (1978), which covered the demand for real broad money (or M3—currency plus all commercial bank deposits), and it was a critique of a study of the demand for this aggregate published in the *Bank of England Quarterly Bulletin* (Hacche, 1974).[13] Another study, Hendry (1979), was also concerned with the U.K. demand for money but focused on the narrower definition, M1 (currency plus "sight," or demand/checking, deposits). Hendry produced this paper for a London Business School Conference on Econometric Model Building held on July 3–5, 1978 (see Coghlan, 1981, p. 194), and an updated version of the M1 demand specification was provided by Hendry and Richard (1983).

Meanwhile, as Hendry (1993, p. 179) implied, the attention given to his emerging econometric methodology was heightened considerably by the great influence that the study by Davidson, Hendry, Srba, and Yeo (1978) had on U.K. econometric practices. This article applied general-to-specific procedures to the estimation of the U.K.

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[11] Accordingly, when the *Guardian* (December 15, 1983*b*) stated that the Hendry-Ericsson critique came from "Oxford dons," it was correct in the sense that Ericsson was affiliated with Oxford University when Hendry and Ericsson (1983) was drafted. ("A don" is not an official title, being an informal term used in reference to individuals who have an academic affiliation with one of the older U.K. universities.)

[12] The term "general-specific" was used in this context by Hendry (1979, p. 228).

[13] The main title of the paper—"Serial correlation as a convenient simplification, not a nuisance"—was not only somewhat obscure but also did not relay what became a major message of both Hendry's and Mizon's work, namely, that residual serial correlation was usually neither a "nuisance" (if by that term one meant that serial correlation simply implied that generalized least squares, incorporating an autocorrelation correction, should be deployed, instead of ordinary least squares [OLS]) nor a "simplification" (as a key reason why Hendry and Mizon opposed autocorrelation corrections was that they doubted that the implied simplification—that is, the imposition of a "common factor" parameter restriction on the equation's dynamics—was likely to consistent with the data). Rather, Hendry and Mizon viewed residual serial correlation as a symptom of a wider model misspecification. Later work by Hendry would stress this point, while Mizon (1995) would produce a paper on the matter that put his opposition to residual correlation at center stage (including via the paper's title). Residual serial correlation did not figure too heavily in the Friedman-Schwartz/Hendry-Ericsson debate, as it turned out that the *Monetary Trends* money demand equation scrutinized by Hendry and Ericsson did not have a bad Durbin Watson statistic (its value was 1.51—see Hendry and Ericsson, 1991a, p. 13).

consumption function and appeared in the December 1978 issue of the *Economic Journal*.[14] Prior to this, in June 1978 the U.K. Treasury's published guide to its macroeconomic model indicated that the model equation describing the behavior of nondurable consumption spending was based on the Davidson, Hendry, Srba, and Yeo (1978) specification.[15]

Over the second half of the 1970s and continuing into the 1980s, general-to-specific procedures became prevalent in economic modeling as practiced by U.K. policy and policy-related agencies, as well as in much applied macroeconomic research carried out in U.K. academia. The hegemony of general-to-specific procedures in U.K. macroeconometrics (notably in matters concerning consumption demand, portfolio decisions, and housing-sector and labor market behavior) was lightheartedly labeled "Hendrification"—a term that even the U.K. media deployed, a case in point being a piece in the magazine *New Society* (August 12, 1982).[16]

The *New Society* piece noted that Hendry had acquired an image as a critic of Keynesian models, owing to the fact that much of his previous research had pointed to shortcomings of the U.K. macroeconometric models that were prominent in the 1970s. In particular, the article focused on what it characterized as the forecasting arm of the "radical Keynesians"—the Cambridge Economic Policy Group (CEPG), headed by Wynne Godley, which had just missed out on a research grant. Francis Cripps of the CEPG was quoted in the article as saying, "We don't use Hendry's methods," while Hendry himself was quoted remarking of Cripps, "He and Wynne Godley aren't serious modelers—they

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[14] Hendry and Mizon (1978) had been published in the preceding (September) issue of the *EJ*.

[15] See HM Treasury (1978, pp. 11.1 to 11.3). The specific reference cited was a 1976 LSE working paper version of the article, with James Davidson and Hendry given as the authors (p. 11.3). The two-author credit of this early version reflected the fact that the paper was originally written in 1975 (Hendry, 1993, p. 179; see also the citation of the 1975 version in Sims, 1977, p. 220), as a collaboration between Davidson and Hendry as part of the former's master's degree work. Work continued after the two were working in different institutions, and "he got the other authors, Srba and Yeo, to refit the whole thing to an extended sample, which is how they got into the act." (James Davidson, interview, February 12, 2015.)

[16] Mark Taylor (1987) appeared to cite Grice and Bennett (1981) as an early usage of the term "Hendrification." That term was not actually used by Grice and Bennett (1981, 1984), although they certainly cited Hendry and Mizon (1978) as influential on recent years' U.K. money demand work, and "Hendrification" was indeed a widely used term among econometric modelers in the U.K. government by the early 1980s. (Its prevalence led Graham Hacche—whose work had, as noted, generated the Hendry-Mizon critique—to suggest to his Bank of England colleagues that he would prefer the resulting reform of estimated money demand functions to bear his own name and be called "deHacchification" rather than "Hendrification.") Later occasions of the usage of "Hendrification" or "Hendryfication" include Mark Taylor (1991, p. 2) and Keil and Richardson (1990, p. 274), respectively.

look at the world and intuit."[17]

The point that the application of Hendry's approach could provide the basis for an indictment of large amount of extant macroeconometric work, spanning material produced by both the Keynesian and monetarist schools, was underlined by Charles Goodhart (an official at the Bank of England in 1968–1985, who, as well as being heavily involved in internal policy discussions, was the Bank economist of the era who most concerned himself with outside economic research and with academic-central banker interactions on monetary-economics topics).[18] Goodhart noted to the present author in mid-1992, after the main Friedman-Schwartz/Hendry-Ericsson exchanges had appeared in print: "I think that Hendry's primarily concerned with econometric techniques… He's concerned with the question of how you establish reliable inferences, statistical inferences, from regression analysis. And his approach is a methodological approach. And I don't think that he was particularly concerned with the monetarist argumentation—which doesn't, in some respects, necessarily follow from the econometrics. I believe that Hendry's attack on Milton Friedman was not so much an attack on a policy position. It was an attack on a methodological position. He was saying that Friedman's methodological approach was invalid." (Charles Goodhart, interview, July 3, 1992.)

Nevertheless, well before Gilbert (1986, p. 283) noted that Hendry "is widely known… as a critic of Milton Friedman" on account of Hendry and Ericsson (1983), Hendry had increasingly gone on record in being critical of monetarism and of numerous specific aspects of the monetarist research and policy literature. This process had really started in 1978, when Hendry and Mizon (1978, p. 552) had pointed to items in the U.K. press that had questioned monetarism by suggesting that there were higher simple correlations between U.K. inflation and clearly-irrelevant non-economic variables than with prior monetary growth.

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[17] *New Society*, August 12, 1982, p. 251. (Media articles cited in this paper are listed chronologically in Section I of the bibliography.)

[18] Goodhart had positions in U.K. academia, mainly the LSE, during the years on either side of his long spell at the Bank. Over several years starting in 1997, he also served at the Bank in senior part-time capacities: these included three years as a Monetary Policy Committee member (policymaker). Recently (March 2024), Goodhart, still active today in producing work on monetary and banking matters, marked sixty years since his first publication in an economic-research journal. Even before 1964, Goodhart had been among those thanked for comments on *Monetary History* drafts in Friedman and Schwartz (1963a, p. xxii).

Hendry (1980) also cited these items, while adding a further "nonsense" regression of his own, connecting U.K. price behavior to rainfall. Indeed, Hendry (1993, p. 1) named the rainfall regression as the basis for the 1980 paper being titled "Econometrics: Alchemy Or Science?"[19] The fact that this article contained an example critical of money-and-price regressions naturally led Hendry's paper to be perceived in some quarters as a critique of monetarism. An inference that the regression invited was that prices-on-money and monetary-growth/inflation regressions might be no more reliable for analytical and policy purposes than the regression of inflation on rainfall. And although Hendry (1980) did not specifically refer to Friedman, Basu (1982, p. 1781) pointed to the prices-on-rainfall regression in Hendry (1980) as showing that "Friedman's method of establishing monetarism has also come under fire from econometricians." Later, although he did not refer to the later debate with Friedman and Schwartz, Bruce Hansen (1996, p. 1399) pointed to Hendry's "elegant portrayal of rainfall as a better predictor of prices than money (!)" and hinted that the result was a challenge to monetarism.

A caveat should be given regarding the implications of Hendry's (1980) rainfall example. It did provide a durable warning against being overly impressed by high R^2's or t-values. The rainfall example did not, however, amount to—and was not intended to be—a comprehensive indictment of the practice of tracing inflation to monetary forces. The fact that a particular prices-on-money regression may be spurious or have poor statistical properties does mean that prices-on-money or inflation-on-monetary-growth regressions are inherently misspecified or that the relationship between the two series is spurious. As discussed later (Section 6), appropriately specified inflation-on-monetary-growth relations can provide a coherent, if unabashedly reduced-form, way of viewing the behavior of U.K. inflation through the 1980s.

Notwithstanding the discussion of money in his 1980 article, Hendry in his writings before 1983 was not an outspoken critic of Friedman himself (in contrast to other figures in U.K. academia, such as Frank Hahn and Nicholas Kaldor, who from the early 1970s onward had produced detailed criticism of Friedman and monetarism).[20] Hendry also did

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[19] Hendry's (1980) article was an inaugural lecture—that is, marking his becoming a full professor at the LSE (even though this had occurred in 1977). In the previous year's *Economica* volume, another LSE inaugural lecture, this one by George Akerlof, appeared (Akerlof, 1979), and it went on to be discussed in *Monetary Trends* (see Friedman and Schwartz, 1982, p. 415). *Economica* and the LSE would have been entitled to believe that there was a curse associated with the practice of publishing inaugural lectures: Akerlof left the LSE around the time his *Economica* article appeared, while Hendry himself left the LSE at the end of 1981.

[20] With regard to Hahn's activism in the early 1980s as a critic of Friedman, see Nelson (2023, Chapter 1).

not have extensive exchanges in print with U.K. monetarists. As for references to work by Friedman himself, other than favorable citations of Friedman (1956), Hendry through 1982 had rarely mentioned Friedman's research publications in monetary economics. As noted, the aforementioned Hendry (1980) paper, although perceived as critical of Friedman, did not cite any Friedman work. In particular, although that 1980 paper focused on Keynes' (1940) critical review of Tinbergen's (1939) early macroeconometric model, it did not cite the Friedman (1940) critical review of Tinbergen (1939). Hendry may not even have been aware of Friedman's 1940 review at this stage, although he would cite it numerous times after the Friedman-Schwartz/Hendry-Ericsson debate appeared in print, including in Hendry and Clements (1994, p. 270).[21]

Furthermore, both before 1983 and over the rest of the twentieth century, Hendry seldom cited such key monetarist publications as the Friedman-Schwartz *Monetary History*.[22] Furthermore, he does seem to have been steeped in the 1960s and 1970s U.S. literature on the Keynesian-monetarist debates. Certainly, Hendry was a visiting professor of economics at Yale University in 1975 (A&C Black, 1987, p. 798) and so would have attended seminars alongside such eminent critics of Friedman as James Tobin.[23] But he was not, it seems, immersed in the Keynesian-monetarist literature. Tellingly, in a passage of their first paper on *Monetary Trends*, Hendry and Ericsson (1983, p. 52) did not give any references to the pre-*Trends* macroeconomic literature when they remarked, "[Friedman and Schwartz] appear to claim (in some sense) a 'better explanation of economic behavior' than 'Keynesian' theory. It is unclear to us in what sense this claim could possibly hold…" Certainly, items in the prior two decades' literature on the debate between monetarism and Keynesianism could have provided answers about "in what sense" such a claim might hold.

In light of Hendry's eminence in econometrics and the fact that the monetary-economics and econometrics spheres tended to have different sets of participants, the lack of detailed discussion in Hendry's writings over these years of Friedman's work is not surprising.

Nevertheless, by 1983 Hendry had been involved in the policy-oriented aspects of the

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[21] Bacon (1977, p. 1997) was an early example of the citation of Friedman (1940) as a key reference critical of "data mining" or "pretesting."
[22] Hendry and Ericsson (1985, p. 9) cited Friedman and Schwartz (1963a) in passing, when noting that Gould and C.R. Nelson (1974) followed up on the *Monetary History*'s account of U.S. velocity behavior.
[23] Ericsson was also based at Yale University during this period, receiving his bachelor's degree in 1976 (American Economic Association, 1997, p. 164).

debate on U.K. monetarism. Having already had been prominent in U.K. research and U.K. public discourse alike for over a decade, this debate reached a still higher profile after the Thatcher Government came to power. The closing paper in a conference on "Monetarism in the United Kingdom," held in September 1981 at City University (London) and attended by Anna Schwartz, had stated (Bade and Parkin, 1984, p. 241): "Since the election of Mrs. Thatcher in 1979[,] world attention has been focused on United Kingdom economic policy in general and monetary policy in particular." When empirical work and policy simulations were being considered, focus increasingly turned during the early 1980s to different macroeconometric models' results concerning the disinflation proposals of the Thatcher Government, and 1980–1981 Hendry served as a special adviser to an inquiry into that government's conduct of monetary policy conducted by the U.K. House of Commons' Select Committee on the Treasury and Civil Service. His negative perspective on monetarism was clear in his summary—Hendry (1981)—which was printed in one of the committee reports. Subsequently, Hendry noted of the period through 1985 that it was marked by "my frustration with economic policy in the United Kingdom."[24]

Later Hendry remarks made during Friedman's lifetime, such as those in Hendry (1997) and Ericsson (2004), confirmed that he did not think at all highly of monetarism. Hendry's position on monetarism is not, however, a principal concern of the present paper. Instead, the analysis will concentrate on whether the Hendry-Ericsson critique of *Monetary Trends* was inconsistent with the basic elements of Friedman's monetarism.

In that connection, one matter in monetary economics on which Friedman and Schwartz, on the one hand, and Hendry and Ericsson, on the other, shared common ground deserves mention at this stage. Hendry (1979) had already found a stable function in the case of the postwar demand for M1 balances in the United Kingdom, and the Hendry-Ericsson work—although critical of Friedman and Schwartz's claim that *they* had found a stable function describing the demand for U.K. M2—was not opposed in principle to the notion of money demand stability. Laidler (1985a, p. 37) accurately noted that "Hendry and Ericsson (1983)… show themselves quite willing to maintain the hypothesis that, in the long run, and subject to occasional and difficult to explain shifts, the demand for money is essentially proportional to real income and prices, and also varies systematically with [yields]," and on this score shared Friedman and Schwartz's "basic hypothesis about the

[24] Hendry (1993, p. 270).

nature of the long-run demand for money function."[25]

The fact that Hendry and Ericsson subscribed in principle to in the existence of constant monetary relationships was evident in their observation (Hendry and Ericsson, 1985, pp. 16–17): "we seem to share the hypothesis [with Friedman and Schwartz] the hypothesis that there exists a joint data density for the variables" in the *Monetary Trends* U.K. dataset. Although they referred here to the "joint data density"—that is, to the modeling of the whole system of variables—Hendry and Ericsson, in the event, concentrated on an equation describing the demand for U.K. real money balances, rather than considering the behavior of other variables (such as inflation) that were analyzed in the course of the *Trends* study. And on the behavior of real balances, it turned out that Hendry and Ericsson (1991a) did, in fact, share Friedman and Schwartz's view that there was a stable money demand function. Although Hendry and Ericsson (1991a) were severely critical of Friedman and Schwartz's econometric procedures, they did find evidence in favor of a constant-parameter money demand model, and they favorably cited (p. 23) Friedman (1956) as a source of theoretical support for such a demand function.[26]

The emphasis on money demand constancy put Hendry and Ericsson more in line with Friedman's work than with U.K. Keynesian traditions. Ericsson, Hendry, and Hood (2016, p. 114) point to the 1941 Friedman-coauthored wartime memorandum that was brought to light by Nelson (2020a, Chapter 3) as early evidence of Friedman's concern with the constancy of economic relationships. His public writings of the same vintage displayed this interest, too (Nelson, 2020a, pp. 95–96). As of 1943, in fact, Friedman believed that changes in money and income had an *unstable* relationship. But his later project with Schwartz led to a reconsideration—and to their belief in a linkage between nominal money and nominal income, one that largely reflected a stable long-run money demand function. Such a position contrasts with the view (associated in the United Kingdom with vintage versions of Keynesianism and with the Radcliffe Report: that is, Committee on the Working of the Monetary System, 1959) that monetary aggregates tend to have an inherently unstable relationship with other economic aggregates.

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[25] Laidler was writing at a point when Hendry and Ericsson had not arrived at a stable *dynamic* money demand function, as they would in their 1991 paper.

[26] The authors asked readers to "cf" (that is, compare the discussion with that of) Friedman (1956). This is sometimes seen as a means of putting at arms length a reference, as opposed to asking readers to "see" that reference. However, Hendry and Mizon (1978, p. 562) and Hendry (1979, p. 223) did ask readers to "see" Friedman (1956).

2.2 The Friedman-Schwartz side

The catalyst for the Friedman-Schwartz/Hendry-Ericsson debate was, of course, the appearance of their book *Monetary Trends in the United States and the United Kingdom: Their Relation to Income, Prices, and Interest Rates, 1867–1975*. The production of this book proved to be the final stage of the longstanding Friedman-Schwartz project on monetary relations, carried out under the auspices of the National Bureau of Economic Research (NBER).

This closing installment in the Friedman-Schwartz studies departed from the earlier Friedman-Schwartz volumes in two major respects.

First, unlike *A Monetary History of the United States, 1867–1960* and *Monetary Statistics of the United States* (Friedman and Schwartz, 1963a, 1970), it made heavy use of econometric analysis.

Second, the volume concerned both U.S. and U.K. data. This feature reflected a major overhaul of the book resulting from the internal review process. An initial version of *Monetary Trends* was complete by early 1967, but an NBER reading committee urged that the study be extended to include the United Kingdom (see Friedman and Schwartz, 1982, p. xxviii, and Nelson, 2009; 2020b, Chapter 12).[27] The 1967 draft covered both monetary trends and monetary cycles—these terms really meaning, respectively, longer-run connections between the money stock and other economic variables, and the cyclical relationship between money and those series. By the early 1970s, the authors had decided that their third book should be focused on monetary trends alone. In fairly short order, they also dropped their plans for a fourth book, on monetary cycles, altogether.

Previews of the *Trends* empirical work had been given publicly by Friedman on several occasions during the 1970–1976 period (see Nelson, 2009; 2020b, Chapter 15). One of the most detailed presentations of *Trends*' money demand results, however, consisted of Anna Schwartz's (1975) outline of preliminary findings drawn from the two-country study, using 1878–1970 data. The equation describing U.K. money demand reported by

[27] Earlier drafts had been completed earlier still, and the book had been cited as forthcoming in 1963's *Monetary History*, so the passage from initial draft to final version was more than two decades. David Hendry, too, would go through a multi-decade process of producing a major book. In Blaug (1986, p. 393), he indicated that he had been writing his monograph *Dynamic Econometrics* since 1970. It was eventually published 25 years after this start date (see Hendry, 1995).

Schwartz differed substantially from the specification reported by Friedman and Schwartz (1982), even apart from the slightly longer sample period used by the latter. The key differences were threefold. First, no dummy variables were used in the regressions in Schwartz (1975) to represent shifts in money demand, even for the periods of the two world wars. In contrast, Friedman and Schwartz (1982) deployed dummy variables that covered both depression and wartime-related periods, as discussed presently. Second, Schwartz (1975, p. 142) gave estimated income elasticities "consistently above unity" for both countries, while in Friedman and Schwartz (1982) the estimates were close to unity (again, a matter discussed shortly). Third, Schwartz (1975) reported Durbin Watson statistics as part of the published regression output, whereas Friedman and Schwartz (1982) did not.

2.3 Initial reaction in the research literature reaction to the book's U.K. results

Monetary Trends saw print in mid-1982. The reception of the book is discussed in detail in Nelson (2023, Chapter 16). For the purposes of the present paper, it is notable that initial published reviews of Friedman and Schwartz (1982) did anticipate Hendry and Ericsson's critique in some broad ways, by noting questionable aspects of the Friedman-Schwartz econometric modeling and reporting. For example, Mayer (1982) pointed to the aforementioned absence of any Durbin Watson statistics as well as to the authors not having availed themselves of modern time series techniques. However, Mayer (1982, p. 1528) opened with the unnecessary—and blatantly factually incorrect—flourish that in the *Monetary Trends* analysis "all nooks and crannies of the data are explored." It is certainly true that *Trends* documented its data sources in considerable detail and elaborated on how these sources had been the basis for the authors' construction of a century-long U.S. and U.K. dataset. It is also the case that the subsequent chapters had a large amount of graphical and regression analysis of these time series. But the econometric analysis clearly did not amount to a case in which "all nooks and crannies of the data are explored."[28]

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[28] Mayer (1982, p. 1529) also referred to "the amazingly close correlation between money and income over the long run." This was a reasonable, if somewhat excited-sounding, description of the fact that nominal money and nominal income did have an impressive visual match with one another in the United Kingdom and the United States in the century through the mid-1970s. Later in the Mayer review, his being impressed with R^2 may seem overblown (1982, p. 1534, emphasis in original): "For regressions of levels that include yields as well as the dummy variables, the *lowest* R^2 shown is .9977, and for rates of change it is .9534. Even when one makes allowance for the fact that the phase averages wash out year-to-year variations this is extraordinary." An R^2 of 0.9977 (or more) for a multiple regression does not deserve the label "extraordinary" when (as here) the regressor and some right-hand-side variables were integrated of

It is remarks such as this in Mayer's review, as well as Laidler (1982) (who had begun his own review essay, in another journal, by stating, "*Monetary Trends* is a marvelous book…") that likely prompted Hendry (1985, p. 76) to cite these pieces when remarking critically that the book gave rise to "generally favorable reviews"—even though both Mayer and Laidler expressed reservations about specific aspects of the book.

As most of Hendry and Ericsson's discussion was on *Monetary Trends*' estimates (in that book's chapter 6) of a demand function for money, the reviews' coverage of that chapter deserves attention. Proceeding toward that discussion, it is necessary to lay out the equation for the level of U.K. money demand that Friedman and Schwartz reached (see their Table 6.14, p. 282). Friedman and Schwartz's preferred money demand specification had the same basic right-hand-side variables in both its U.K. and U.S. versions. In the case of the United Kingdom—and letting $(m - n)$ denote log nominal per capita money, p the log price level, and $(y - n)$ denote log per capita real income, and following Hendry and Ericsson in letting j subscripts denote "phase" units of time (as opposed to years)—the regression reported for phases spanning from 1878 to 1975 was as follows:

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

$R^2 = 0.970$.

Some aspects of the regression specification will now be discussed, in the course of which the remaining notation used in the preceding equation will be given.

The real income elasticity estimate of 0.88 was numerically not far from unity but—according to the *t*-statistic that Friedman and Schwartz (1982) used—was statistically significantly below one. The U.S. estimated income elasticity was estimated to be above unity (by about the same amount) in the corresponding money demand regression for that country. Some reviews stressed the Friedman and Schwartz (1982) finding of a below-unitary income elasticity (in the U.K. case) as adverse to their analytical framework, either because that framework allegedly required a unitary income elasticity or because the estimate was numerically notably below the U.S. point estimate. One review

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order one ($I(1)$). A mitigating factor here is that Mayer did cite the high R^2 in the case of the rates-of-change regressions, too, and it is indeed unusual for rates-of-change regressions to have R^2 as high as those obtained.

published in continental Europe in June 1983—a lower-profile book review than those already discussed, and highly critical of *Trends*—used a word that would recur in the post-June 1983 critical literature on the book when it suggested that a non-unitary elasticity was an "embarrassment" to Friedman and Schwartz (Tonveronachi, 1983, p. 134).

The finding of a non-unitary elasticity does not, however, have much bearing on the Keynesian-monetarist dispute. It provides some simplifications for monetary analysis, including in theoretical expositions involving quantity theory of money (QTM) based analysis, such as those in Friedman (1970, 1971).[29] But Friedman's money-demand analysis as expounded in 1956 had not laid down a requirement of a unitary income elasticity (see Nelson, 2020a, Chapter 6). In any event, the matter of a deviation of the income elasticity from unity was not a major area of great contention between Friedman and Schwartz and Hendry and Ericsson. Later chapters of Friedman and Schwartz (1982) would use an income elasticity of unity as an approximation, and Hendry and Ericsson (1983, 1991) would impose an income elasticity in their estimate of the U.K. money demand cointegrating vector.

Reflecting the multiple-interest-rate view of money demand in Friedman's monetarist framework, Friedman and Schwartz's money demand equation had two distinct "yields" as opportunity-cost variables. Each of these yields entered the function as decimal fractions (that is, 1 percent became a regressor value of 0.01) and so each essentially appeared in its own, percentage units (that is, the money demand function was semilogarithmic). The first of these yields was *RN*, a short-term interest rate (*RS*) net of (that is, minus) a proxy for the own rate on money. This yield and its calculation are discussed in more detail in Section 8 below. The second was g_Y, the growth rate of nominal income. This variable was used to stand in for the nominal return on physical assets. A physical-assets return was included in the money demand function in order to reflect the notion that the goods and service sectors, in aggregate, are the source of both a scale variable and a cost variable in the money demand function. On this reasoning, money holders, separate from their practice of linking their demand for money balances to income, regard holding an extra unit of real money balances as involving forgoing holding goods—in which case the "yield" on goods is one of the interest rates that matter for the demand for money.

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[29] See also the discussions in Friedman and Schwartz (1982, pp. 238, 367).

Of these two yields, the *RN* term (the use of which gave similar results for Friedman and Schwartz's sample period as the short-term interest rate, *RS*) was evidently much more numerically important and statistically significant, and the g_Y term figured little in the subsequent Friedman-Schwartz/Hendry-Ericsson debate.

We come now to the elements of Friedman and Schwartz's empirical money demand equation that attracted critical comment in numerous reviews, in ways that presaged Hendry and Ericsson's criticisms. These were two items. The first was the money demand equation's inclusion of what Friedman and Schwartz (1982, p. 241) labeled "shift dummy variables." The second was the authors' usage (not only in their money demand estimates but also throughout the main empirical work in *Monetary Trends*) of phase-averaged data.

***The intercept-shift dummy variables*:** The equation given as (1) above contained two intercept-shifting dummy variables. The first of these was a wartime-related dummy (*WD*). The inclusion of this dummy indicated that Friedman and Schwartz's allowance for wartime was more complicated than simply including indicator variables for the two world wars. In light of an examination of postwar movements in rate-of-change data, Friedman and Schwarz introduced an allowance for the "postwar adjustment" by households of their real money balances, rather than a wartime dummy variable *per se.* Essentially, their *WD* variable allowed monetary velocity to decline after World War I after a perceived wartime overshooting of velocity and pick up after WWII after an undershooting of velocity (see Friedman and Schwartz, 1982, pp. 228–230). Some rationale for velocity rising after World War II, as a result of money-holders' rising perceptions of improved economic stability, had been given in Friedman and Schwartz (1963a, Chapter 12). Nonetheless, their 1982 exposition of their decision to include the *WD* dummy variable did not overtly appeal to this earlier rationale. In part, this absence of a detailed rationale for the dummy variable likely reflected the fact that excluding, or allowing for, a shift in behavior arising from the occurrence of war was widespread in empirical work—so some kind of dummy-variable usage in this context was not surprising. A criticism that could be made was that the dummy variable's specification was more complicated than just allowing for wartime observations by putting in a dummy for both, or each of, the world wars (the simpler practice that Hendry and Ericsson, 1991a, would later follow).

Much critical attention in the reviews of *Monetary Trends* focused on the fact that Friedman and Schwartz (1982) included, again for both the United Kingdom and the United States, another shift dummy, called *S*. This dummy, unlike *WD*, did cover the war years explicitly. More controversially, it spanned many other surrounding years as well, in order to represent what Friedman and Schwartz argued was a turbulence-induced upward shift in the demand curve for money in response to interwar economic turmoil and World War II itself. With regard to the United States, this dummy variable covered (that is, was equal to 1.0 in) the period from (phases from) 1929 to 1954. In the case of the United Kingdom, the corresponding period covered was from 1921 to 1955 (Friedman and Schwartz, 1982, p. 239). As their point estimate in their levels regression indicated, Friedman and Schwartz found the upward shift in money demand to have been about 20 percent in the U.K. case.

Although a couple of prominent reviews—Laidler (1982) and Mayer (1982)—seemed to take the inclusion of an intercept dummy like *S* as not being particularly damaging to Friedman and Schwartz's positions, other reviews, including Goodhart (1982), saw the need to allow for such a money demand shift as undermining confidence in monetary relations, and the critical review of Moore (1983, p. 121) stressed Friedman and Schwartz's "extensive use of ad hoc dummies." The inclusion of dummy variables in time-series regressions often tends to be off-putting to consumers of the research, and a one-time member of Friedman's University of Chicago money workshop, William Poole would note (in Darby and others, 1987, p. 1) that in teaching econometrics in university courses, it was often suggested that "only dummies use dummy variables"—though Poole did note instances in empirical practice in which the argument for including dummies was compelling.

Of course, in the case of Friedman and Schwartz's money demand function, the inclusion of the dummy intercept shift necessarily implied an acknowledgment of *some* absence of money demand stability—with Artis (1984, p. 206) in his own review observing of the authors' U.K. money demand equation that because they "employ dummy variables… the claims to temporal stability need substantial qualification."

Hendry and Ericsson (1983, p. 48) focused on the shift dummies as a key indication that Friedman and Schwartz (1982) did not have a stable money demand function: "In what sense is a relationship 'stable' if shift dummy variables constructed after prior analysis of the data must be included in the model to avoid parameter non-constancy?" But in their

later discussion, they allowed more room for differences of opinion on the matter by remarking (Hendry and Ericsson, 1991a, p. 13) that "many investigators would regard the need for the data-based shift dummy *S* spanning one-third of the sample as *prima facie* evidence against the model's constancy." There are, in fact, three key mitigating factors that bring Friedman and Schwartz's (1982) inclusion of *S* less out of step with econometric practice, and more consistent with some notions of money demand stability, than the initial reactions by various commentators in 1982–1983 might have suggested.

First, it is not quite accurate to say, as Struthers and Speight (1986, p. 235) did, that Friedman and Schwartz (1982) "relied on the inclusion of a dummy variable selected after examination of the data sample, and not on theoretical grounds, which is the normal econometric approach." For one thing, it is to give a glamorized picture of the "normal econometric approach" to suggest that decisions to include dummy variables did not in practice involve consultation of the data. Closer in line with practical econometrics was Rasche's (1987, p. 12) suggestion that, though allowing for intercept breaks likely in practice would entail some data inspection, it was better to use formal statistical techniques for ascertaining a shift-point when the date of the shift was unknown.[30]

More to the point, however, Friedman and Schwartz's dummy variable *did* involve theoretical and economic grounding, including, as already implied, analysis that they had published before *Monetary Trends*. Friedman and Schwartz (1982, p. 228) saw the *S* demand shift as reflecting higher money demand arising from "price deflation, uncertainty arising out of serious cyclical fluctuation reinforced by the outbreak of war" as well as continued high money demand, partly forced by the unavailability of consumer goods, during World War II. Although the particular period to which they applied their dummy arose from an examination of the *Trends* data and from initial regressions using those data, the basic argument that Depression-era economic turbulence led to a downward velocity shift had been an important part of Friedman and Schwartz's (1963a) account of the United States in the 1930s (see especially their pp. 353, 673–674).[31]

A departure in *Monetary Trends* from this narrative motivation was that, in the U.K. case, Friedman and Schwartz defined *S* so that the upward demand shift started in the early

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[30] Rasche (1987, p. 14), in particular, argued that, in preference to the "arbitrary selection of a shift point," one could use a having a likelihood ratio test valid in conditions of an unknown shift date.

[31] Friedman had previously indicated that uncertainty raises the demand for money in Friedman (1956, pp. 8–9), in his point 6.

1920s. But having the shift begin earlier in the United Kingdom than in the United States was in keeping with the fact that a state of protracted economic slump had set in "nearly a decade earlier" in the U.K. case (Friedman and Schwartz, 1982, p. 228). That the U.K. economy had a longer, though less steep, interwar depression than the United States did is a widely noted and accepted feature of that period. The United Kingdom's condition in the 1920s and early 1930s has been encapsulated by Ellison, Scott, and Sargent (2019, p. 64) as "falling prices and recurrent recessions."[32]

And with regard to the connection to money demand, Goodhart (1982, p. 1544) observed: "The hypothesis that depression and uncertainty can raise the demand for liquidity has a long tradition." Friedman himself had a long record of expressing the position that uncertainty was a major influence on the demand for money. But he had been frustrated at not being able to operationalize this idea empirically, having failed to find a reliable time series or index that could quantify the degree of uncertainty (see Nelson, 2020a, Chapter 2). Since then, of course, twenty-first-century research has seen vast advances in obtaining indexes of national uncertainty that can be used in applied economic work (see, for example, Baker, Bloom, and Davis, 2016). The same decades have also seen multiple instances of traumatic global events leading to steep increases in money demand: 9/11, the Global Financial Crisis, and the pandemic all led households in the United States and elsewhere to raise their desired quantity of real money balances.

A second mitigating factor regarding the Friedman-Schwartz use of shift dummy variables is that—subject to the important proviso that they need to be accurately isolating intercept shifts—such dummies can facilitate the estimation of the slope parameters that tend to be of most economic interest. Including an intercept dummy that covers a lengthy subsample of the estimation period is econometrically distinct from "partialling out" the observations associated with that subsample: the inclusion of the dummy is not tantamount to preventing those observations from having a material bearing on all the regression parameter estimates. Rather, as Friedman and Schwartz (1982, p. 229) noted—making a basic, but key, point—an intercept shift dummy entails estimation that allows for the intercept, but not regression slopes, to differ across the shift and no-shift periods. They correspondingly regarded money demand parameter stability as being a meaningful concept—one that was applicable even after allowing for intercept shift dummies (p. 249). The basic parameter stability was therefore seen as pertaining

[32] In addition, Johnson (1975, pp. 78–80) argued that the arguments in Keynes' (1936) *General Theory* were largely shaped by the U.K. slump of the 1920s.

primarily to the slope coefficients in the money demand function—its elasticities and semielasticities.

The treatment of slope constancy as the crucial stability concept was consistent with the fact that economics tends to focus on marginal analysis—the incremental influences of certain variables on other variables—and therefore on slopes. With regard to macroeconomics specifically, developments in the 1970s further focused the matter of parameter instability on slope parameters thanks to the appearance of the Lucas (1976) critique, which Gordon (1988, p. 28) characterized as a "formal demonstration that empirically estimated slope parameters were not invariant to policy interventions." In this atmosphere, intercept shifts were perceived as a more admissible form of drift over time in a specification than a change in slope.[33]

To some extent, this attitude already imbued empirical money demand practice before *Monetary Trends*: for example, Teigen (1976) had already included an intercept shift in his money demand function when combining pre- and post-Korean War observations. Around the time *Trends* appeared, Hafer and Hein (1982, p. 15) suggested adding an intercept shift into the quarterly demand function for U.S. M1 while noting that this was consistent with slopes remaining constant before and after the shift: "our analysis suggests that the marginal relationships have remained stable." And later, in his work on U.K. inflation, David Hendry (2001, p. 257) included dummy variables to allow for "different intercepts pre-and post-1945."

The third mitigating factor is that (in contrast to the shift dummy in Hafer and Hein, 1982, for example), the *S* shift variable used by Friedman and Schwartz reverted to a zero value before the end of their sample.[34] This meant that the inclusion in *Trends* of the shift dummy variable was consistent with Artis' (1984, p. 206) contention that, in the United Kingdom, "the demand for M2 is known to be relatively stable over a long period." In

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[33] In monetary analysis, the reliability of various important exercises involving money—including ascertaining the steady-state monetary growth-inflation relationship and establishing the absence of a Keynesian liquidity trap—basically hinges on being able to have reliable estimates of slopes (elasticities or semielastities) that can be taken as being (finite) constants. Such analysis still might be viable in case of occasional, infrequent shifts in the demand function's constant term. Correspondingly, in monetary policy practice, much of the velocity instability that has been most cited as generating disillusionment with the use of monetary aggregates in policymaking has taken the form of breaks in velocity trend—a phenomenon that could not arise solely from an intercept shift in the money demand function.

[34] See Friedman and Schwartz's (1982) definition of the shift variable as well as their remark (p. 229) that the "1957 phase… [corresponds to] the end of high liquidity preferences."

particular, the presence of the *S* variable in the money demand function over a subsample is consistent with the existence of a cointegrating vector in the century to the 1970s involving log real money balances, log real income, interest rates, and a single constant.

Since the 1980s, in light of studies such as Stock and Watson (1993), it has become increasingly commonly accepted to regard "long-run money demand stability" as meaning cointegration.[35] The cointegrating vector—that is, the long-run relationship to which the data reverts—implied by *Monetary Trends*' money demand function did not include *S*. This is because the construction of that dummy variable implied that, after a time, it returned to its original value of zero. It is a persistent "impulse" or "intervention" dummy, not a variable that connotes a permanent shift in the estimated function.

All told, then, the inclusion of dummy variables that permit different intercepts across blocks of the sample period has received more acceptance (including in David Hendry's work) than was the case in 1982–1983. In light of the fact that deployment of such dummies has come to be more common in empirical research, their usage in *Monetary Trends* now seems less out of step with econometric practice.

***Phase averaging*:** But far less conventional, by the standards of economic research both then and now, was Friedman and Schwartz's (1982) use of phase averaging of their annual data before conducting regression, correlation, and graphical analyses.

One of the *Monetary Trends* reviews that Hendry (1985) cited as being favorable, that of Goodhart (1982), strongly criticized Friedman and Schwartz's use of "phase averages" of the data—that is, their decision to carry out their main regressions using a century of annual U.K. observations only after those roughly 100 data points were converted, by a complicated transformation tied to NBER traditions, into 37 observations. Phase averaging was put forward as an attempt to rid the data of certain short-run influences and thereby obtain ostensibly low-frequency observations, each representing one expansion or one economic contraction. Resistance to this approach was clear in Goodhart's (1982, p. 1541) forthright statement—which could have served well as a curtain-raiser to a key part of the Hendry-Ericsson critique—as well as accurately reflecting the verdict of the economics and econometrics worlds on phase averaging: "I cannot accept that the purported advantages of such smoothing processes outweigh their

[35] See Nelson (2024) for further discussion of this interpretation of stability in recent decades' contributions on the demand for money.

disadvantages."

Goodhart later added to the present author: "Essentially, it's an econometric issue, and I don't profess to be a particularly great econometric expert, [but] my understanding is that some of the techniques used by Friedman and Schwartz, according to modern state-of-the-art approaches, aren't really terribly good, so that some of the significance [that] Friedman and Schwartz applied to their own findings was probably exaggerated. And the methods they used—like phase averaging—had certain very significant disadvantages." (Charles Goodhart, interview, July 3, 1992.)

Many writers, including Friedman and Schwartz on occasion, described the intention of phase averaging as being to acquire series from which the influence of the business cycle has been removed. Indeed, Friedman and Schwartz (1982, p. 73) motivated their transformation in terms of "freeing the data so far as possible from the effects of the shorter-term movements we call business cycles." Similarly, other descriptions they gave of phase averaging characterized it as an attempt "to eliminate short-term cyclical perturbations," "[t]o free the data from cyclical fluctuations," and a matter of "averaging out the intra-cycle movement."[36] These statements might lead one to think that phase averaging was as an effort to obtain the trend value of a series, along the lines of the HP-trend output of the Hodrick-Prescott (1980) filter. This was not the case, however, as none of the shorthand descriptions accurately captured what phase averaging attempted to do. The cycle consists of the whole experience of a contraction (or recession/depression) plus expansion combined, whereas a phase is defined as a particular contraction or expansion. As Friedman (1980b, p. 499) remarked—in a more accurate condensed description of the *Trends* procedure: "we used as the unit of observation not a year but half of a cycle, an expansion phase or a contraction phase."

Therefore, by its very nature, phase averaging was neither an attempt to take out cyclical fluctuations (such an effort would mean *amalgamating*, not separating, expansions and contractions) nor an attempt to eliminate all intracyclical fluctuations (as expansions or contractions are inherently "intracyclical" phenomena). Phase averaging was consciously designed to generate a time series that still contained a notable imprint of cyclical fluctuations. Phase averaging was not an attempt, along the lines later pursued by Hodrick and Prescott (1980), to obtain a distinct estimate of trend, decoupled altogether

[36] The quotations are respectively from Friedman (1980a, p. 60 [p. 62 of 1991 reprint]), Friedman and Schwartz (1982b, p. 212), and Friedman (1980b, p. 500 (see also Friedman and Schwartz, 1991, p. 40).

from cyclical variations. But neither, of course, was it an attempt to *detrend* the data altogether: the annual-data nominal and real income and money series used in *Monetary Trends* that possessed upward trends retained those upward trends after the phase-averaging transformation (see, for example, Friedman and Schwartz's, 1982, p. 154, plot of their phase-average money series).[37]

Phase averaging did have a research literature behind it in the form of Burns and Mitchell (1946) and later NBER product, as well as studies by some Friedman-trained participants in his University of Chicago money workshop. But it had failed to gain traction more widely.[38] Phase averaging did not become accepted among macroeconomic investigators as a standard, off-the-shelf data-transformation tool in the period between 1946 and 1982. Instead, it was associated with older-vintage NBER work and not a wider body of research. This fact was brought home to Anna Schwartz when an early project in which she was involved—on the U.K. business cycle—though not an NBER-commissioned enterprise, used NBER methods. The resulting two-volume work (Gayer, Rostow, and Schwartz, 1953a, 1953b), was the focus of what Schwartz called a "just scathing" article by R.C.O. Matthews (1954).[39] These volumes later acquired an improved reputation. Nevertheless, Schwartz acknowledged that "the National Bureau methodology hasn't been in any way, you know, revived, resurrected, or in any way gotten wider approval" (interview, April 21, 2003).

This outcome in part reflected the reality that phase averaging possessed the double disadvantage of being complicated yet non-rigorous: it involved highly laborious formulas, but it lacked solid analytical backing in mainstream macroeconomics and econometrics as practiced in U.S. universities (let alone institutions abroad). By 1977–1980, when Martin Feldstein was making the NBER a nationwide network of researchers located at various universities and other research institutions, Friedman and Schwartz were practically the only researchers, NBER-affiliated or otherwise, who were

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[37] Prior to their estimation of U.S. money demand, the authors detrended U.S. monetary velocity for the period before 1903 (see Friedman and Schwartz, 1982, pp. 216–221). As I discussed in Nelson (2020b, Chapter 12), this adjustment was motivated by reactions in the economic literature to the *Monetary History*'s discussion of velocity behavior. The fact that the *Monetary Trends* U.S. velocity adjustment was a response to suggestions made by monetary economists on that earlier work does not seem to receive adequate recognition in Ericsson, Hendry, and Hood's (2016) critical discussion of the adjustment.

[38] Consequently, Britton's (1991, p. 109) characterization of *Monetary Trends*' work as having been "in many ways dated" when it appeared in print did not really capture the point that a key aspect of *Trends*' approach—phase averaging—had never become part of the mainstream approach to empirical economic research in the first place.

[39] The Schwartz remark appears in Nelson (2004, p. 400).

still using phase averaging.[40] Other leading researchers investigating the business cycle tended to use more standard, modern, and more highly vetted data-analysis methods—or they developed new filters, including the already-noted Hodrick and Prescott (1980) filter.

Friedman and Schwartz's use of phase averaging would be a major ground on which Hendry and Ericsson took them to task. And in developing a detailed and systematic critique of phase averaging, Hendry and Ericsson were taking, and providing a rigorous grounding for, a stand that would be shared widely by macroeconomists and econometricians. Even though Friedman and Schwartz (1991) would defend phase averaging, it seems fair to say that researchers who might express reservations about aspects of the general-to-specific methodology or would point to the merits of various parts of Friedman and Schwartz's writings would not see phase averaging as a valuable practice in empirical work. It is likely that they would agree with the point made by Hendry and Ericsson (1983, p. 57) (though possibly not with their way of expressing it) that "modern econometric techniques are vastly superior in practice to the maze of prior transformations in which FS [Friedman and Schwartz] indulge."

3. Chronology of the debate, 1983 to 1990

The Bank of England decided to have its October 1983 Panel of Academic Consultants meeting (part of a roughly annual series of events at which academics met with Bank staff) to have as its subject matter the U.K. findings reported in *Monetary Trends*. Two papers were commissioned to provide the basis for the day's discussion: Arthur Brown (Leeds University), who was working on a National Institute of Economic and Social Research study of industrial-country inflation since 1950; and David Hendry. Charles Goodhart later recalled (1997, pp. 1598–1599): "I was probably responsible for suggesting David Hendry's name to the Bank; Hendry had just completed several major studies on the demand for money, including a critique of a [Bank] study… Hendry was the obvious person here to explore Friedman's methodology." After Hendry had been contacted and had confirmed his participation, Goodhart gave written confirmation of the arrangement in a letter to Hendry dated May 6, 1983 (Ericsson, 2024).

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[40] Charles Schultze, newly returned to the Brookings Institution from the Carter Administration, did produce a study in 1981 that used phase-average data, though mostly not in a regression context. Accordingly, Stock (1987, p. 1241) was able to cite both Schultze (1981) and Friedman and Schwartz (1982) when noting that notwithstanding its "lack of associated statistical theory, phase averaging is still occasionally employed as a research tool."

By early September, a draft of Hendry's paper, jointly written with Ericsson, was ready. Goodhart (1997, p. 1599) observed: "Christopher Dow and I had no idea what to expect from Hendry in advance, and we were both surprised, and somewhat taken aback, by the scope, scale, and substance of that paper." Dow's reaction was expressed at the time in the invitation letter that he sent to Friedman. The Bank, he explained in a letter dated September 9, 1983, received "a preliminary draft of Hendry's paper, which subjects your statistical methods to considerable criticism and does so on a scale which is considerably more massive and thorough than we originally envisaged."[41] He added: "given the weight of Hendry's criticism we thought you might like to know in advance, in part so that you might have a chance to consider whether you would care to attend the meeting."[42]

If Dow's letter did little to hold back his relish at relaying the news to Friedman, this was understandable. Dow was an adherent to the hardline Keynesian stand that had long been prevalent in U.K. academia and that had heavily shaped pre-1979 U.K. economic policy. Dow's body of work certainly showed that he was willing to dissent from the Keynesian consensus on particular issues, but he strongly lined up against monetarism and particularly disliked the Friedman-Schwartz *Monetary History*.[43] Friedman, for his part, had in 1980 all but called for Dow to be ejected from his position as chief economist of Bank of England, when he had told *Newsweek International* that the Thatcher Government should shake up the Bank's hierarchy (see Nelson, 2017, p. 27).

Dow's letter was written in a manner that hardly increased the likelihood of Friedman accepting the invitation, but Dow would have known that there was, in any event, virtually no chance that Friedman would attend. Goodhart had been at a conference with Friedman as recently as June 1983, but that occasion (in Tokyo) had also highlighted the fact that Friedman's appearances at research events outside the U.S. Bay Area were now largely restricted to prestigious guest talks (in the Tokyo case, as a Bank of Japan conference keynote speaker). Predictably, Friedman declined Dow's invitation.

Dow's letter further indicated that Anna Schwartz would be invited. "I'm sure I was invited to the Bank of England conference, now that you mention it," Schwartz recalled to the present author (personal communication, April 24, 2008). "… But there was no

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[41] Dow letter to Friedman (in Friedman papers and the Bank of England archives), p. 1. Also quoted in Hammond (1996, p. 194).
[42] Dow letter to Friedman, September 9, 1983, available in Friedman papers (Hoover Institution), p. 1.
[43] See Dow (1998, pp. 211–216).

point in going… I'm not an econometrician… [So] I just declined. And Milton would *never* have gone."[44]

The Arthur Brown paper (Brown, 1983) was discussed in the morning session of the October 28 event. It was later printed alongside the Hendry-Ericsson paper in the proceedings document. But it would be overshadowed by that paper and, much later, would not be covered even in a book specifically on Brown's contributions to economics (see Button, 2017).[45]

During the 1980s, the infrequent discussions that appeared of Brown (1983) included two diametrically opposed assessments. Both of these appeared in 1989. In one, Alec Cairncross, a former senior U.K. government economic official who had attended the 1983 panel, suggested: "not long ago… Arthur Brown and David Hendry both gave papers to a Bank of England panel critical of Milton Friedman's monetary history [sic] of the United Kingdom. In my judgment, although Hendry's econometric treatment was masterly, the more conventional demolition job by Brown was quite as effective." In contrast, Capie and Wood (1989, pp. 95–96) regarded Brown's paper as exemplifying the kind of extreme Keynesianism that the monetarist revolution had rendered untenable.

Of these verdicts, that by Capie and Wood seems the one most consistent with the judgment of posterity. Anna Schwartz (1986, p. 671) would regard Brown's project on world inflation (see Brown and Darby, 1985) as showing that "Prof. Brown is caught in a time warp of Keynesian views that no longer command the assent of economists," and Cairncross' words of praise for Brown likely reflected Cairncross' own adherence to that older mindset (including a strongly cost-push view of inflation). Capie and Wood (1989) highlighted the fact that a dated aspect of Brown's analysis was its routine treatment, in keeping with the cost-push tradition, of import price rises as necessarily leading to higher aggregate inflation (see Brown, 1983, p. 35). As discussed later, this manner of inflation analysis, already considerably out of vogue in the United Kingdom by 1983, would become even more so among U.K. economists within just a few years.

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[44] Although the absence of Friedman and Schwartz was not surprising, it did put Dow and the other organizers in a dilemma, as it made it harder to create an atmosphere of debate at the symposium. The perception that the event had little in the way of prominent defenders of the Friedman-Schwartz position was hardly dissipated by the presence of Dow, an ardent Keynesian, nor by the prominent role of chair of the proceedings being given to R.C.O. Matthews, who had a record dating back to 1954 of critical remarks on Anna Schwartz's work.

[45] Button (2017) also does not cite *Monetary Trends*.

Another dated facet of Brown's work—one also tending in keeping with cost-push views—was his implication that substantial excess capacity (negative output gaps) did not lower inflation (p. 42). The plots of the gap series in Brown and Darby (1985) revealed clearly that Brown was not allowing for the productivity slowdown of the 1970s—and so he made use of gap series that possessed large biases (in the direction of erroneously suggesting a severe shortfall of demand) very much along the lines of those documented in the U.S. case by Orphanides (2003).

Notwithstanding his own sympathy with Brown's way of looking at inflation, Dow himself would suggest that, although Brown's paper had been commissioned as "the main paper" at the panel discussion, it had not turned out that way: "Hendry stole the show," while Brown produced "what I thought a shoddy paper, not [on a par with] Hendry's demolition."[46] It was therefore the Hendry-Ericsson paper that was the main output of the conference, and it was on their critique that later outside attention overwhelmingly focused.

3.1 The 1983 Hendry-Ericsson critique

The title of the Hendry-Ericsson paper was "Assertion Without Empirical Basis: An Econometric Appraisal of *Monetary Trends in… the United Kingdom* by Milton Friedman and Anna Schwartz." This title, along with one of the paper's opening quotations (attributed by the authors to John Converse), "In the social sciences there is no such thing as refutation; only embarrassment," accurately relayed the very critical tone and content of the paper. In view, however, of the negative perspective of the paper, a point that may be overlooked is that Hendry and Ericsson specifically contended that *Trends* lacked empirical *basis*—meaning in this case that it, for example, imposed parameter restrictions that Hendry and Ericsson suggested did not hold—not that Friedman and Schwartz had not provided empirical *evidence*, in the form of regression and other analysis. Hendry and Ericsson strongly disputed the strength of that evidence, and they disagreed with many of Friedman and Schwartz's interpretations of the data. But they themselves stated that *Monetary Trends* had included "painstaking statistical analyses" (Hendry and Ericsson, 1983, p. 47).

The Hendry and Ericsson (1983) critique was, of course, made from the perspective of

[46] Dow, writing in mid-1984, quoted in Hacche and C. Taylor (2013, p. 249).

the general-to-specific approach to econometric modeling. But, as already stressed, the authors' criticisms of *Monetary Trends* included numerous econometric points that subscribers to VAR-based approaches would also be likely to endorse, in part because these approaches also stressed the shortcomings of empirical economic modeling that imposed explicit or implicit strict restrictions on data dynamics.

Much of Hendry and Ericsson's (1983) analysis of the phase-average data took the form of demonstrations that hypothesis tests (for example, exclusion of log prices as a regressor in a real-balances equation) not carried out by Friedman and Schwartz or carried out only in simpler regressions were not found to be valid when applied to Friedman and Schwartz's multiple regression describing the demand for money. They also rejected the worth of phase averaging itself: "we cannot accept the procedures FS adopt in preprocessing the data to 'phase averages'" (p. 54). Furthermore, Hendry and Ericsson cast doubt on the very strategy that the use of phase averaging was intended as a means of pursuing—concentrating on longer-run relations and not on shorter-run variable dynamics: "We find nothing to recommended in this practice as against analyzing the annual data and encompassing both 'trend' and 'cycle'" in the modeling (p. 82).

Hendry and Ericsson (1983) focused, as did later versions of their paper, on Friedman and Schwartz's money demand equation. Hendry and Ericsson sought to build their own alternative specification, estimated using the annual data that Friedman and Schwartz (1982) tabulated, covering the period through 1970. As discussed below, their preferred alternative specification evolved over time, with several modifications evident in Hendry and Ericsson (1991a). A major point of their 1983 paper was that their application of the general-to-specific procedure obtained a better-fitting equation (a much lower residual standard error) than that obtained in *Monetary Trends*. Friedman and Schwartz's equation, Hendry and Ericsson's findings suggested, was non-constant on the criterion of formal testing, even when the interwar shift dummy was included.

Hendry and Ericsson reported also that their own equation, including dynamics and estimated on annual data, exhibited parameter instability, too—for example, in the interest semielasticity being lower in later subsamples of the 1878–1970 period. Another reason they concluded that their own estimated specification did not deliver a constant function was that the annual-data counterpart of Friedman and Schwartz's interwar shift dummy entered their regression significantly. Hendry and Ericsson (1983, p. 65) conceded that, via this dummy, Friedman and Schwartz had had "some success" in

isolating a period of shift. But they saw the statistical significance of the dummy as itself evidence against the postulate of stability of the demand for money—a postulate that they considered had been Friedman and Schwartz's (1982) "most important single claim" (Hendry and Ericsson, 1983, p. 67).

3.2 Spillovers of the debate into U.K. public discourse, 1983–1984

In mid-December 1983, the Hendry-Ericsson critique of *Monetary Trends* led to about three weeks of media attention—not saturation coverage, but cumulating to a sizable number of U.K. press pieces. This began on December 15, 1983, with the appearance that day of a couple of articles that day that outraged Friedman and that he would later describe as "personal attacks on me by the Manchester *Guardian*," adding of the articles: "Essentially they accused me of faking data." (Milton Friedman, interview, January 22, 1992.)

The December 1983 *Bank of England Quarterly Bulletin* had a one-page article registering the fact of the October 1983 panel and indicating that a compendium collecting the panel paper would be released shortly.[47] A paragraph in the article stated of Hendry and Ericsson, "They conclude from their analysis that little can be learnt from the results in the book, since the evidence fails to support the claims Friedman and Schwartz make from it." A more accurate statement would have stated "data, on being reexamined" rather than "evidence"—as both sets of authors provided evidence.

Nevertheless, the Bank's summary was nondescript compared with the front-page article by Christopher Huhne in *The Guardian* of December 15, 1983, "Monetarism's Guru 'Distorts His Evidence.'"

In Ericsson (2004, p. 774), Ericsson and Hendry discussed respects in which the transformations of the data in *Monetary Trends* or the chosen regression specification could be described as "distorting" the data or evidence. And, of course, it is a truism that data transformation or filtering, including not only phase averaging but also such operations as taking natural logarithms or obtaining growth rates, or using multiple

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[47] Specifically, the Bank's compendium was released in the week of December 19–23, 1983 *(Financial Weekly* (London), December 16, 1983). But the Hendry-Ericsson paper originated outside the Bank and was, of course, circulated even before December 19 as a manuscript. December 15's articles in the *Guardian* pieces were based on that manuscript.

regressions of any kind, are distortions in the sense that they change the focus of the study to time series possessing different moments or conditional moments from those of the original variables. With regard to *Monetary Trends* specifically, Adrian Pagan observed of the intention of phase averaging (interview, January 8, 2015): "in a way, they *were* distorting the data—but in the sense of you're getting a permanent component out of it." Friedman himself remarked in an early public reaction to the controversy, "Of course I 'manipulated' the data. I don't doubt that I manipulated the data. Every statistician manipulates the data!"[48] Friedman went on to note that routine seasonal adjustment of series can be labeled a "data manipulation." Hendry and Mizon (1985) would lightheartedly note that multiple regression analysis itself consisted of, or was perceived as, "stretching and squeezing data."

But putting constructions on the words "distort" or "manipulate" in these ways, although valid in many contexts, does not capture fully the way in which the words were used in the *Guardian* pieces, as these constructions would imply more benign and routine usages of those words than the sense in which Huhne's articles employed them. Huhne clearly used the word "distorts" in a manner implying that the *Trends* study had violated professional standards of integrity, ethics, and practice—and so carried an allegation of an instance of dishonesty and disreputable behavior. It is on this score that Hammond (1996, p. 198) accurately noted that the *Guardian* articles were themselves "highly distorted" accounts of the matters at issue. Huhne's accusation could not be validly supported by the Hendry-Ericsson critique—which had instead argued that the Friedman-Schwartz study was far behind best econometric practice (an important criticism, but one centered on the book's degree of technical advance) and was, consequently, unreliable.

The specifics of the Huhne accusations began when his front-page *Guardian* article opened: "Nobel prize-winner, Milton Friedman, guru of the monetarist counter-revolution in economics, has effectively been accused of distorting evidence for his theories in a devastating critique to be published by the Bank of England." A later paragraph stated: "In an attempt to prove that inflation is 'always and everywhere' due to too much money chasing too few goods, Professor Friedman manipulates official statistics to reduce the money supply between 1921 and 1955, and to increase prices after the Second World War."[49] This was an oblique reference to the shift dummy variables,

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[48] Iceland state television, September 1984.

[49] The flourish about "official statistics" was itself an inaccuracy. The U.K. government did not publish money-stock data until the 1960s—itself testament to the authorities' neglect of monetary aggregates over

particularly the *S* term. But it was a description that elided the difference between *Monetary Trends*' use of a dummy variable and Friedman and Schwartz's assembling of data, with the article implying that something untoward had been done in the process of data collection. In fact, of course, this was not the case. Friedman and Schwartz's book had laid out the tabulation and sources of their annual and phase-average data well before the *S* variable had ever been included in a multiple regression.

The brief quotation from Hendry in the article had him delivering a verdict of "simply incredible" on *Monetary Trends*. Again, however, a problem of interpretation arises, as there is a divergence between economists' and journalists' usage of the word "incredible." "Incredible" is routinely used in econometrics, most famously by Sims (1980), to describe prior empirical work that imposes dynamic or variable-exclusion restrictions on equations that one judges to be implausible or as unlikely to pass formal tests. In this context, it does not attribute to the researchers under discussion disreputable behavior, whereas in journalistic usage the word often does carry this imputation.

A longer, interior article by Huhne in the same December 15 issue of the *Guardian* was headed: "Why Milton's Monetarism Is Bunk."[50] It again presented the interwar dummy variable as though it had been introduced surreptitiously, rather than being part of a reported regression. Huhne declared, "Friedman arbitrarily reduces the money stock by some 20 percent for a period of no less than 34 years." Huhne also seemed surprised that the *Monetary Trends* analysis, which spanned two world wars, would make allowances for "price controls and rationing" in war periods (as Friedman and Schwartz did via their dummy variables and by an attempt to approximate how prices would have behaved absent price controls). Huhne remarked of Friedman (Schwartz being mentioned only in passing): "He then uses the manipulated data to prove the link between money and prices. This is very circular, and very naughty."

Hendry and Ericsson had voiced strong disagreements with *Monetary Trends*' approach to empirical analysis, including on phase averaging and on the preferred specification.

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this period. As Friedman and Schwartz (1982, p. 111) observed, historical data on money had (largely under the impetus of their own U.S. research) been produced in the United Kingdom by researchers, including Sheppard (1971).

[50] As these descriptions indicated, the *Guardian* articles' concern with *Monetary Trends* involved an essentially exclusive focus on Friedman. It is not, however, correct to state, as Hammond (1996, p. 199) did, that "the articles made no mention of Schwartz." Huhne had stated (*The Guardian* (London), December 15, 1983*b*): "At issue is Friedman and Anna Schwartz's magnum opus, the 660-page culmination of the Nobel Laureate's life work in monetary theory [sic]…"

But Huhne made a distinct charge that was not part of, or implied by, that critique: that Friedman or Friedman-Schwartz had contrived a money/income or money/prices relationship by manipulating the U.K. data series.

It is the present author's conjecture that the background of U.K. money/income and monetary-growth/inflation relations in recent years through 1983, together with an enmity toward monetarism, prompted Huhne to make this charge and to believe in his charge's validity. Inflation and the growth rate of broad money had indeed had a weak relationship with one another (even after allowing for lags) in the United Kingdom period from 1976 through 1983. These years were outside Friedman and Schwartz's sample, and the corresponding relationship was actually strong (registered in a high correlation) in their period of study, including in the case of annual data (see, for example, Berry, Harrison, Thomas, and de Weymar, 2007).[51] Having covered, as a journalist, the recent period in which M3-type monetary aggregates and the economy (including inflation) had had a loose relationship, Huhne seems to have presumed that such looseness was the norm and that a strong correlation between monetary growth and nominal income growth would not emerge from a straight read of the data—that is, taking it as it is and plotting it, or calculating simple correlations.

Huhne likely combined his own belief that the monetary-growth/nominal income growth and monetary-growth/inflation relations had been weak—that is, featured low correlations—over *U.K. history* (not just since 1976) with a negative judgment concerning Friedman's ethics as a researcher. Hence his accusations against Friedman.

The *Guardian* article led to a syndicated Reuter piece, saying that Huhne had quoted Hendry and Ericsson's critique (it had not) and compressing the Huhne allegation to: "Mr. Friedman arbitrarily reduced official figures for [the] money stock by about 20 percent for a period of no less than 34 years between 1921 and 1955" (*The Globe and Mail* (Toronto), December 17, 1983). Elsewhere in North America, however, the critique received little press attention, although the longer "Monetarism Is Bunk" piece was reproduced in the globally distributed newspaper *Guardian Weekly* (December 25, 1983). In Australia, the controversy received considerable newspaper coverage in the days after

[51] These authors used the annual-average data of Capie and Webber (1985). In Friedman and Schwartz's (1982) annual dataset (including the correction given on their p. 116), for the period 1878–1970 the correlation between the log-change in prices (not incorporating adjustments for price controls) and the log-change in money is 0.74 using same-year monetary growth and 0.60 using the prior-year's monetary growth. The corresponding correlations for 1878–1975 are 0.71 and 0.67.

December 15, including a *Sydney Morning Herald* syndication of the first Huhne piece (December 16, 1983) as well an abridged version of the Reuter article in the Sydney afternoon newspaper, the *Daily Mirror* (December 16, 1983).[52]

When I remarked to Friedman—not on the basis of Huhne's articles, which I had not seen at the time, but on the basis of a summary by another journalist, David Smith (1987)—that the media rendition of the controversy made the *Trends* authors seem "exposed [of] some sort of academic fraud, some sort of cooking the books," Friedman replied, "That's right—exactly. And that's the part to which I really object." He distinguished between the Hendry-Ericsson critique itself—"If they disagree with what we did, fine"—and what he considered the inappropriate construction put on the critique by the media coverage: "to accuse the integrity is a very different thing" (Milton Friedman, interview, January 22, 1992).

Friedman had once referred to "[m]y statistical conscience" in a letter in the *Wall Street Journal* (August 28, 1978). On that occasion, he had taken that newspaper to task for what he judged to be a misleading graph—what he termed "'lying with statistics' with a vengeance," in failing to plot two series in logarithmic units and so giving the impression of a stronger relationship than that existed. But, in 1983, a different newspaper was allowing readers to infer that Friedman in his empirical work was involved in a more literal form of lying. He was naturally affronted. Anna Schwartz, too, had been trained in a tradition that made the accusations embedded in the *Guardian* pieces anathema. She had joined the NBER in the early 1940s and would recall of "the Bureau in those days": "People were collecting data, and there was great emphasis on making sure that the data were accurate… This was the way I operated, too." (Anna Schwartz, interview, April 21, 2003.)

In Australia, the perception generated in the local press and in the *Guardian Weekly* of ethical malpractice on Friedman's part prompted Tom Valentine, head at the time of Macquarie University's Centre for Studies in Money, Banking and Finance, to write an article in the business press. Adrian Pagan recalled: "Tom Valentine once wrote a piece for the [*Australian*] *Financial Review*—I think it was around when the Hendry thing came out—[dealing with] you know, 'Is the great man [Friedman] cheating with the data?' (*laughter*) … Tom actually had quite a good analysis of what David basically had

[52] Essentially the same Reuter piece appeared in the national daily newspaper *The Australian* the next day (December 17, 1983) with the over-the-top headline, "Guru Accused of Rigging Data."

done [in the Hendry-Ericsson study] and how to interpret it, rather than this 'flash' stuff that somehow Friedman's been cheating."

"Some of the [news] reports on the [Hendry-Ericsson] paper give the impression that the great man has been detected in some data manipulations which had the objective of producing the final results he wanted," Valentine observed. But, he noted, although the Hendry and Ericsson study did challenge the *Trends* results, the press allegation was not justified by that challenge: "There is, however, no implication that Friedman and Schwartz 'fixed' the results—only that the procedures they used are not sufficiently powerful." Valentine further noted with regard to Hendry and Ericsson, "Their results will cause Milton Friedman considerable embarrassment," but he added that this was primarily "because they emphasize the unsophisticated nature of the statistical techniques which he uses" (*Australian Financial Review*, January 19, 1984, p. 10).

In a piece that had appeared in the *AFR* earlier in the month, London *Financial Times* writer Samuel Brittan had referred to the "latest Friedman-Hendry controversy" (*Australian Financial Review*, January 10, 1984), and he considered that controversy—focusing on the economic issues involved—alongside other recent work by researchers on U.K. monetary issues in his regular *Financial Times* column (January 19, 1984). Brittan made what would prove to be an important observation regarding these wider debates—that they obscured the fact that centering inflation control on monetary policy was becoming a less controversial position than might be inferred from the strident U.K. debate on monetarism. "Traveling abroad, one feels that there is more support for the fundamentals (if not the details and practice) of 'Thatcherite' financial policy among the economic establishments of countries other than the U.K."

Brittan also noted that the Bank of England was trying to refrain from substantive public comment on the dispute. A difficulty that the Bank encountered was that, although it had commissioned and released the Hendry-Ericsson study and summarized it in its *Quarterly Bulletin*, the study was an outside piece of research, not a document endorsed by Bank officials. The Bank did release a public statement making this distinction on the day when the Huhne articles appeared (*Financial Times* (London), December 16, 1983). This distinction was missed in much press coverage, including a later statement by a journalist (*The Scotsman* (Edinburgh), July 6, 1984): "The Bank of England's own theoretical journal may have destroyed the intellectual foundations of Friedmanite economics."

L.A. Dicks-Mireaux, an academic turned Bank official, reiterated the status of the Hendry-Ericsson study in a letter (of January 25, 1984) to an inquirer. Dicks-Mireaux went on to criticize the press coverage: "if you look at Friedman and Schwartz's book, I think you will agree that expressions such as… 'cheat' or 'fiddle'… are unjustified. Hendry and Ericsson criticize the way that the authors adjust their data; but the authors [Friedman and Schwartz] are painstakingly clear in describing their adjustments, and give the reasons for what they have done."[53]

In a rare direct interaction, Friedman wrote to Hendry in May 1984, on the subject of what Friedman called "a spate of libelous and slanderous newspaper articles," and asked that Hendry issue a public statement to the effect that the media's suggestion that Friedman and Schwartz had been accused of unethical distortion of the data was mistaken.[54] Hendry replied in a letter of July 13, 1984, understandably indicating that the media's interpretation of the debate was not his responsibility. He pointed Friedman to the option of suing the *Guardian*—something Friedman was not likely to do, especially as the implication of wrongdoing in those articles had been relayed via choices of words and phrases.[55]

For their part, Hendry and Ericsson (1991a, p. 34) did state that Friedman and Schwartz "carefully document" their data sources and construction, and both authors used the *Monetary Trends* annual-data series in many other studies, including Banerjee, Dolado, Hendry, and Galbraith (1993).[56]

3.3 The interim years of the debate, 1984–1990

Ericsson presented the Hendry-Ericsson paper at the NBER Macroeconomics Conference

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[53] In Anna Schwartz papers, inspected 2007.
[54] In Friedman's letter to Hendry of May 14, 1984, held in Anna Schwartz's papers, inspected by the author in 2007.
[55] See Hammond (1996, p. 199) for the quotation of the Hendry letter.
[56] See, in particular, Banerjee, Dolado, Hendry, and Galbraith's (1993, p. 31) plot of annual U.K. real output growth. This has the distinctive feature of Friedman and Schwartz's series of being positive in 1975. Reported U.K. output growth was negative in this year, but Friedman and Schwartz's adjustment for price controls and their removal reshuffled real growth from pre-1975 observations to 1975. They noted that data on various U.K. real variables did not altogether support this adjustment (1982, pp. 119–120). In Nelson (2016), I argue that Friedman and Schwartz's procedure for adjusting price and output series for the effect of price controls works well for wartime episodes but that the U.K. data in the mid-1970s. Their procedure requires viewing nominal spending growth as reflecting aggregate demand settings. But the U.K. three-day-week of 1974 held down total nominal spending growth, making it difficult to interpret that series as demand-driven in this period.

held on July 12–13, 1984, in Cambridge, Massachusetts. The discussants were: Schwartz's longtime coauthor and Friedman's one-time dissertation student, Michael Bordo; and John Huizinga. The summary of the occasion (*NBER Reporter*, Fall 1984, p. 22) indicated that one point made by Huizinga was: "Ericsson and Hendry's proposed money demand model seems to be a reduced form, in which case it is not surprising that it displays lower residual variance than Friedman and Schwartz's structural equation." One way of putting this argument that Huizinga made is: Because Friedman and Schwartz advanced their money demand equation as a long-run relationship, it should not be a surprise that the equation fit less well than an equation like Hendry and Ericsson's that included explicit dynamics.

This argument would prove a durable one. The notion that a long-run money demand function could be resilient over time—and might well be of interest even in its own right, irrespective of whether the deviations from the long-run relationship could be explained well—was a key tenet of *Monetary Trends*. It was also emphasized in an intervention that Alan Walters, formerly a researcher in monetary economics but more recently economic adviser to Prime Minister Thatcher in her first term, made on the Hendry-Ericsson critique. In Walters (1986, pp. 110–111), he pointed to an annual-data plot of U.K. M2 velocity and the long-term interest rate, given in Artis and Lewis (1984), and suggested that the plot conveyed graphically a concept of money demand stability—stability of the long-run function—that Hendry and Ericsson's (1983) were overlooking by focusing on the criterion of constancy of *dynamic* money demand equations.

Walters' (1986) discussion appeared in a semi-popular book, rather than the research literature. As discussed later, however, on account of the cointegration literature and the key Lucas (1988) study, empirical money demand analysis after 1986 would increasingly view long-run parameter stability, not constant short-run dynamics, as the crucial condition to be met.

The fact that Walters felt the need to reply to Hendry and Ericsson in his book reflected the high profile that their work had acquired in the United Kingdom, and Ericsson's 1984 NBER presentation also aired the critique in a major U.S. academic forum. Nevertheless, there was little scope for a broad research debate to develop. *Monetary Trends* had not

caught a very wide readership among active researchers in the United States.[57] And in the United Kingdom, notwithstanding the attention given to Hendry and Ericsson (1983), a logjam encountered by third parties who wanted to join in was that research journals were unwilling to publish follow-up entries in a debate that had so far not itself taken place in journals. This was the problem faced by the London Business School's Andrew Longbottom and Sean Holly, who issued two papers on the debate as working papers in 1985 after initially producing a draft of one of them in 1984. Using the general-to-specific methodology, the authors were critical of Friedman and Schwartz's specification but found support for the *Trends*' usage of an opportunity-cost variable that allowed for the own rate on money (whereas Hendry and Ericsson had simply used the short-term rate *RS*) and found a more stable-parameter dynamic money demand equation than had Hendry and Ericsson (1983).[58] Their papers were never published.

Hendry, too, was experiencing a logjam. He was encountering resistance to the general-to-specific methodology in monetary-economics and general-interest research journals, and he would recall problems getting his work on the demand for money (probably here referring mainly to the 1984 version of Baba, Hendry, and Starr, 1992) in print: "at the time [1985] I was experiencing great difficulty in publishing empirical studies in the journals whose readership I arrogantly thought needed to know about these developments."[59] Hendry did, however, have an opportunity to put his views on money demand estimates in a contribution to the first issue of the *Oxford Review of Economic Policy* (Hendry, 1985) that included a brief section on a Hendry and Ericsson (1983).

Meanwhile, the authors continued to work on that paper. They presented revisions of Hendry and Ericsson (1983) were presented at the year's European Meeting of the Econometric Society on September 6, 1984 (Econometric Society, 1985a, p. 717), the North American Winter Meeting of the Econometric Society in Dallas on December 28, 1984 (Econometric Society, 1985b, p. 732), and the Fifth World Congress of the

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[57] Romer and Romer (1989, pp. 123–124) observed that most prominent research originating in the United States in the 1980s that had discussed Friedman and Schwartz's work had focused on the *Monetary History*.

[58] Longbottom and Holly also explicitly underlined the message that there was no inconsistency between using general-to-specific procedures and ECM specifications, on the one hand, and arriving at a model that reflected many of Friedman's macroeconomic positions, on the other. This message was later evident in Brayton, Levin, Tryon, and Williams' (1997, p. 49) discussion of the dynamic price equation in the Federal Reserve Board's new FRB/US model. The authors mentioned the fact that this equation had both an "error-correction coefficient" and an imposition of a "vertical long-run Phillips curve" condition.

[59] Hendry (1993, p. 118). He may also have been referring to Hendry and Mizon (1985).

Econometric Society on August 22, 1985 (Econometric Society, 1986, p. 487). In December 1985, a version of the paper, incorporating abridgments and revisions to the 1983 version of the paper but still very similar to the original in text and content, was issued in the Federal Reserve Board's International Finance Discussion Paper series (see Hendry and Ericsson, 1985).

Hendry also presented the Hendry-Ericsson work at Duke University in around 1987.[60] George Tauchen recalled of the seminar that "some of my colleagues, the senior colleagues here, who had been to Chicago, expected me to defend Friedman." Instead, "I was on David's side." Tauchen was critical of *Monetary Trends*' use of intercept dummy variables and also its taking of *t*-ratios and related statistics largely at face value. "I'd give Friedman an *F* in statistics and econometrics. He was not attentive to some of the standard issues of specification, and serial correlation, and, you know, heteroskedasticity and all that kind of stuff. He just wanted to run the regressions and interpret the *t*-statistics. And you can't do that." Of his colleagues, most specifically Friedman's former student Dudley Wallace, Tauchen remarked, "they were probably irritated with me for not sticking up for Friedman, when I thought David was dead right." Tauchen's view was that "there were lots of problems with Friedman's statistics and econometrics, and Hendry put his finger on them." (George Tauchen, interview, November 13, 2014.) Tauchen's reaction exhibited a wider pattern: even researchers steeped in econometric methodologies other than the general-to-specific approach that Hendry and Ericsson propounded found much common ground with the authors on their criticisms of Friedman and Schwartz.

In September 1984, Friedman had given a public, informal reply, to the Hendry-Ericsson critique, when asked about it in a long television appearance—although, as the appearance was on Iceland state television, few would see it at the time. He anticipated the start of the eventual Friedman-Schwartz (1991) reply by stressing the different vintages of the econometrics in the two studies: "What Hendry has done is to criticize my colleague Anna Schwartz and myself for not applying, in a work published in 1982, statistical techniques that he published in 1983. Now, I plead guilty to that charge—absolutely." Friedman also pointed repeatedly to the fact that Hendry and Ericsson had been concerned with modeling the short run and *Monetary Trends* had not: "What he did was to say we attacked the wrong problem [and] we should have attacked a different

[60] Hendry had a visiting-research-professor affiliation with Duke University from 1987 to 1991 (Blaug, 1999, p. 523).

problem—and that our results are not valid for the different problem."[61]
Friedman's 1984 television remarks also indicated his stand at that time about whether there would be a reply: "The answer to whether Hendry's criticism is valid or not will not be given by me. It will be given by other econometricians who analyze his procedures, his methods, his work." Of econometrics, he observed: "That's not my field of specialization. And there are other people who are far more competent in that area. In the course of time, they will examine it [the dispute]."

Shortly afterward, the course of events proceeded in a manner that might well have altogether precluded a Friedman-Schwartz reply. In mid-October 1984, Friedman had a heart attack. He was largely out of action over the following five months, which included cardiac surgery in January 1985. As for Schwartz, in 1985—the year in which she would be turning 70—she was told by the NBER's head, Martin Feldstein, that he was moving her research-associate position to emerita status. It was not difficult to discern that Feldstein, who had done much in his tenure to rejuvenate and expand the NBER, was implying that, after 44 years at the Bureau, it was now time for Schwartz to begin a long and happy retirement. But Schwartz resisted the encouragement to move on. From 1985 onward, she simply switched from working full-time at the New York City office of the NBER as a salaried employee to working full-time at the New York City office of the NBER on no salary.[62] What Schwartz called "my so-called 'official' retirement" (interview, April 21, 2003) was, in practice, not a retirement at all.

In the event that, in the mid-1980s, Friedman had died and Schwartz had retired, it is hard to know how the discourse involving the Hendry-Ericsson critique would have proceeded. Would it have run out of steam, or would proxies for the Friedman and Schwartz side emerged and kept the debate going? As things turned out, Friedman and Schwartz both weathered the events of 1984 and 1985 that might have brought their activities to an end. And in 1985–1986, the engagements that each made at research events that underlined the fact that they were both still around. They also undertook new joint work, including an extremely intemperate reply, appearing in the journal

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[61] As the 1983, but not the 1991, study, had stressed there not being able to find a stable demand function for money, Friedman's 1984 interviewers treated this as a major part of the Hendry-Ericsson critique. In response, Friedman again made the short-run/long-run distinction: "When he asserts that we do not have a stable demand function for money, what he is saying is that [in] his investigation, using the data continuously, not breaking it down as we did, he was unable to devise, derive from that a function that would apply both within the cycles and between the cycles."
[62] Or, more precisely, treating her pension as a *de facto* salary.

Explorations in Economic History, to a critique that had appeared of part of their *Monetary History* account of the Depression. That response (Friedman and Schwartz, 1986) was a watershed, as it represented a deviation from Friedman's default position of refraining from specific replies in print to others' critiques, including of his work with Schwartz. Its appearance was a signal that a reply by both authors to Hendry and Ericsson's critique might well appear after all.

3.4 Moving in to fill the void

As of the mid-1980s, Hendry's general-to-specific procedure was associated with analyzing specific equations that were *part* of the economic system, rather than specifying a complete macroeconomic model—with Laidler (1985b, p. 113) noting that one "group, associated mainly with Dr. David Hendry, argues that specifying such a complete system is so difficult."[63] Hendry and Ericsson did initially plan, nevertheless, to produce a system of equations that modeled all the *Monetary Trends* variables, including inflation. They initially intended that their 1983 paper would have a second section titled "Reconstruction" that provided their own system of estimated equations that provided a model of the whole annual U.K. dataset given in *Monetary Trends*. They ran out of time to do this before the 1983 presentation. Subsequently, they made only limited progress on this in the 1980s, and what work they had done (Hendry and Ericsson, 1986) was made obsolete by advances in the cointegration and VAR literatures.[64] These advances meant that they would have to start a system-wide analysis from scratch, and the authors found that they did not have time to pursue such a project.

Therefore, the Hendry-Ericsson critique remained centered on Friedman and Schwartz's (1982) analysis of money demand. The limited scope of their critique left room for others to draw implications for the validity of Friedman's broader views on monetary economics. During the 1980s, numerous critics of monetarism in the United Kingdom offered the conclusion that the Hendry-Ericsson econometric appraisal was a definite repudiation of the views on monetary theory and policy associated with Friedman. As

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[63] Increasingly from the late 1980s, Hendry was using system methods that used a vector autoregression (VAR) as a starting point. But this change in approach took time to be manifested in his published work, and in the year in which Hendry and Ericsson (1991a) appeared, Phillips and Loretan's (1991, p. 408) classification of different econometric approaches included: "Single-equation ECM (SEECM) methods [as] advocated by Hendry (1987)." Hendry (1993, p. 3) himself summarized his approach in the 1974–1985 period in these terms: "few of the component equations [in a macroeconometric system] were trustworthy, and so without any conscious decision my attention gradually became focused on single-equation models."
[64] Nelson (2024) provides further discussion.

will now be discussed, however, this conclusion was not warranted.

It was clear that the Hendry-Ericsson work was a challenge to Friedman and Schwartz's modeling of money demand as reported in Chapter 6 of *Monetary Trends*. Hendry and Ericsson (1983) could, in turn, be viewed as part of a wider critique laid out in Hendry's writings of the *status quo* of the empirical literature on the demand for money: as Swamy and Tavlas (1989, p. 65) observed, "Hendry... has been extremely critical of existing methods of money demand estimation." But, as has already been indicated, the criticism that Hendry and Ericsson made of *Monetary Trends* acquired special attention—due to Friedman's high international profile, the fact that Friedman and Schwartz's earlier work had been such a large part of the monetarist literature, and the influence of monetarism on U.K. economic policy, especially over the period beginning with the election of the Thatcher Government in 1979.

In view of the fact that Hendry and Ericsson's work was critical of research by leaders of the monetarist movement, a key question was therefore: Did the Hendry-Ericsson critique itself lead to (or have as a corollary) the conclusion that seeking to control inflation through monetary policy—reducing monetary growth, in the monetarist prescription—amounted to an invalid strategy?

The answer was no. This answer was implied by Hendry and Ericsson's concentration on the money-demand portion of the results reported in *Monetary Trends*. It could be safely said that they had strongly challenged Friedman and Schwartz's approach to econometric estimation of money demand functions. Equally, however, In the absence of the system analysis implied by Hendry and Ericsson's proposed "Reconstruction," the existing critique was not—and did not claim to be—a thoroughgoing inquiry into or alternative to the whole of the *Trends* book, including its analysis of nominal income and inflation, or of the account of inflation's behavior given in Friedman's wider body of writings.
To be sure, Hendry and Ericsson (1983) had little good to say about *Monetary Trends*, and Hendry (1993, p. 118) later noted that his own write-up of that critique in Hendry (1985) was "rather polemical." But Hendry (1985, p. 81) had also made the important point—which can be taken as applying to both monetarists and their critics—that one could not make conclusions about the monetary-expansion/inflation link from a money demand equation alone. One instead needed a full model, as well as confidence that that model could be used to contemplate alternative monetary policy strategies.

It is also true that Hendry was quoted in the course of the press coverage of the Hendry-Ericsson critique as saying: "One of the most amazing things about our study is that we have not been able to find any evidence that money supply creates either income growth or inflation" (*The Guardian*, December 15, 1983*b*). But this was apparently merely a reference to the system-of-equations work in progress, mentioned above, that was never completed. And when he later turned, in what was primarily single-equation analysis, to analyzing inflation, Hendry would actually find that monetary growth or real money balances did importantly enter inflation equations (see, for example, Hendry, 2001).[65]

During the 1980s, however, in the absence of a system analysis of the U.K. dataset in *Monetary Trends*, longstanding opponents of monetarism in the United Kingdom moved in to fill the void. They provided accounts that implied that the existing (1983) Hendry-Ericsson critique was a repudiation of the monetarist account of price and income behavior and acted as if their own nonmonetary view of inflation had been supported by the contents of the critique. The *Financial Times*' Samuel Brittan noted that this tendency was evident in public commentary as soon as the Hendry-Ericsson critique became national news. This pattern, he implied, was a reflection of the fact that "a large proportion of the most senior and vocal British economists" were subscribers to the "traditional postwar approach to economic policy" prevalent (in both research and policymaking) before the Thatcher Government's election in 1979 (*Financial Times* (London), January 19, 1984).

Brittan called this pre-1979 consensus "mainstream Keynesianism." It certainly was mainstream in the United Kingdom. But, by the standards of pre-1979 U.S. Keynesianism, and even more so by the standards of the twenty-first century New Keynesian economic consensus prevailing in both the United Kingdom and the United States, it was not mainstream. On the contrary, it was highly extreme. It largely saw inflation in terms of autonomous increases in particular prices or costs and discounted the role that aggregate demand restraint could play in controlling inflation. This view of inflation was largely adhered to by governments in the United Kingdom in the postwar period to 1979 and was reflected in their focus on incomes-policy-based control of inflation.

Brittan sensed that hardline Keynesians, no longer influential on U.K. economic policy,

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[65] See also the Hendry-coauthored work cited in Section 6 below.

were turning the Hendry-Ericsson debate into a "fundamentalist dispute" in which they were trying to restore the preeminence of their positions on the appropriate means of fighting inflation in the United Kingdom (*Financial Times* (London), January 19, 1984). Consistent with Brittan's interpretation, the 1983–1984 period and the rest of the decade saw numerous U.K. critics of Friedman put their own construction on Hendry and Ericsson's (1983) results and interpret the study as an indictment of monetary-policy-centered approaches to the control of inflation.

In this vein, much popular coverage of the Hendry-Ericsson critique in the 1980s portrayed it as a full-scale empirical challenge to monetarism, with the commentary frequently making the claim that the work had been concerned with the determination of inflation and that it had refuted the monetarist account of inflation. The Christopher Huhne account (*The Guardian* (London), December 15, 1983*b*) had started this pattern by asserting that the Hendry-Ericsson article "so effectively undermined the monetarists' conventional wisdom that inflation is always caused by 'too much money chasing too few goods.'" In the *New Statesman*, business economist Gavyn Davies contended that the Hendry-Ericsson study had shown that the United Kingdom did not conform to Friedman's "basic idea—that inflation is always and everywhere a monetary phenomenon" (*New Statesman* (London), March 9, 1984, p. 8).

A book titled *The Conservatives' Economic Policy* (Thompson, 1986, p. 41) claimed that Hendry and Ericsson's work had "seriously challenged" the existence of "a connection between UK money supply growth and inflation."[66] Another volume on the Thatcher Government contained the statement (Holmes, 1985, p. 17): "The supposedly definitive monetarist interpretation of the British inflation experience by Milton Friedman and Nancy [sic] Schwartz has recently been effectively demolished in two studies [that is, Brown's and Hendry-Ericsson's] commissioned by the Bank of England." And a book by a U.K. economics journalist titled *The Rise and Fall of Monetarism* claimed that the Hendry-Ericsson critique had "knocked holes in many of the empirical planks of Friedman's monetarism" (Smith, 1987, p. 150). This contrasted with Hendry and Ericsson's (1986, p. 2) own description of Hendry and Ericsson (1983): "the many other variables of interest in their [Friedman and Schwartz's] dataset [that is, output, prices, exchange rates, and short- and long-term interest rates]… were not investigated."

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[66] The same passage of Thompson's book stated that the Hendry-Ericsson work was a critique of "Freidman [sic] and Schwartz."

In the research world, M.J.C. Surrey (1989, p. 231) acknowledged that what he called Hendry and Ericsson's (1983) "devastating and otherwise comprehensive critique" had not actually studied the "broader questions" of the determination of inflation. Surrey proposed to fill this gap in his own study, which concluded strongly in favor of the nonmonetary view of inflation associated with older U.K. Keynesian traditions (p. 235): "by comparison with the effects of changes in commodity prices, money simply 'does not matter' for inflation in the open industrial economies of the United States and the United Kingdom."

By this time, one of the most prominent U.K. exponents of a nonmonetary perspective toward the analysis and control of inflation—Cambridge University's Nicholas Kaldor—was deceased. Before his death, however, Kaldor had seized on the Hendry-Ericsson study as validating his own position. He had missed the October 28, 1983, presentation at the Bank of England because of a guest-lecturing commitment at Harvard University (Targetti, 1996, p. 212). Kaldor did, however, discuss Hendry and Ericsson (1983) briefly in an introduction written in February 1984 to a new edition of his book, *The Scourge of Monetarism*. After referring to this "devastating critique," Kaldor (1986, p. xix) declared Friedman and Schwartz's books (both *Monetary History* and *Monetary Trends*) "elaborate but worthless" and cast aspersions on Friedman and Schwartz's ethics by expressing doubt on whether they had been "intellectually honest in the pursuit of truth."

These were fighting words with which Kaldor essentially wrapped up his long contributions to the Keynesian-monetarist debate: he died shortly after the new edition of *The Scourge of Monetarism* reached print in 1986. But the contents of that revised edition—which, the introduction aside, was essentially a reproduction of the first edition, itself mostly written in 1980—must have cast doubt on whether Kaldor was really in a good position to claim vindication. The book included his July 1980 statement calling for a "statutory freeze on all wages, salaries, dividends, and rents and all prices of home-made goods" (p. 62), comprehensive import controls (or, alternatively, exchange rate controls to introduce different exchange rates applying to different classes of goods) (pp. 62–63), and a "progressive reduction of interest rates in successive steps" (p. 63). With regard to the control of commercial banks' balance sheets, Kaldor called (p. 107) for an "improved and more comprehensive version of the lending controls abandoned in 1971."

At the time when Kaldor made his recommendations, U.K. inflation was around 20 percent, and the economy was in recession. But in 1986 the U.K. economy had had output growth since 1981, single-digit inflation since 1982, manufacturing recovery since 1982, and employment growth since 1983. In late 1984, Joel Barnett, a member of the Labour Opposition and previously member of the Callaghan Government, was one of the speakers alongside Kaldor in a House of Lords debate on economic policy. Barnett (1984, p. 227) remarked: "high levels of unemployment are going to continue to rise.... If inflation is held down, we are constantly told, everything else will come right. Unfortunately for that theory, we are now well into the sixth year of the policy, and we are still waiting for unemployment to come down; for what we have seen in practice has simply not borne out the theory." But developments instead proceeded in such a way that, a decade later, Doornik and Hendry (1994, p. 213) suggested that the U.K. unemployment rate seemed to be an $I(0)$ series.

Notably, the Thatcher Government had continued in the years after 1980 to pursue macroeconomic policies very different from those Kaldor had recommended: credit and exchange controls had not been reimposed, there had been no incomes policy at all (let alone Kaldor's extreme version, an across-the-board freeze), the budget deficit in actual and structural terms had been reduced, and nominal income growth had stepped down considerably. Consistent with that last outcome, U.K. monetary policy, though no longer pursuing the broad-money targets that the Thatcher Government had emphasized early in its administration, continued to be the economic tool assigned control of inflation. When accounts like Britton (1991, p. 109) referred to a "retreat from monetarism" in the United Kingdom or remarked that "monetarists were no longer making policy" even in 1981, they overlooked the fact that the post-1980 adjustments to monetary policy arrangements made by the Thatcher Government continued to be predicated on a monetary view of inflation.[67] Subsequent debates within the government (for example, on whether to join the European Community's Exchange Rate Mechanism) were premised on agreement that inflation control should be delegated to monetary policy.[68]

Correspondingly, outside the government, although many economists were critical of the

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[67] Britton was, however, correct, in the sense that broad money targets did not heavily shape U.K. economic policy by early 1981, even though they were maintained until 1985. See Batini and Nelson (2005, p. 34), as well as Goodhart's (1997) observation that, on account of declines in the velocity of the Sterling M3 aggregate, U.K. officialdom had become disillusioned with broad money well before October 1983.
[68] See Batini and Nelson (2005), Nelson (2017), James (2020), and Bordo, Bush, and Taylor (2023) for detailed discussions of these issues.

Thatcher Government's record, few of them were likely to have believed in 1986 that implementation in 1980 of the package that Kaldor had recommended would have produced desirable longer-term results—or better outcomes than those actually prevailing in 1986.[69] The fact was that, from 1979 to 1986, Kaldor's views on monetary policy and inflation—once close to the consensus in U.K. academia—had lost favor in U.K. economic opinion generally, not just in policy circles. Writing in late 1980, Robert Gordon (1982, p. 33) had stated: "Although a band of monetarists fights a rearguard action, nowhere as in Britain is the view so entrenched that inflation results from an autonomous struggle over income shares." By 1986, this view that Gordon described, although it was reasserted in "New Cambridge" treatments like Godley and Cripps' (1983) textbook, had ceased to be a mainstream view in U.K. economics. Compared with the situation prevailing a decade earlier, there was now much less willingness to accept the idea that the government should try to control inflation by direct intervention in wage- and price-setting and there was greater adherence to the view that aggregate demand settings could control inflation. In this connection, models of the U.K. economy had been adjusted during the 1980s to enhance the importance on monetary policy, with Wallis and Whitley (1991, p. 126) observing: "Monetary policy is more powerful than in models of the 1970s."

It is against this background that perceptions of the implications of the Friedman-Schwartz/Hendry-Ericsson debate were likely formed by many economists. Hendry and Ericsson's work was largely accepted as establishing econometric weaknesses of Friedman and Schwartz's *Monetary Trends*. Contrary to Kaldor's (1986) suggestion, however, the validity of their criticisms was not widely seen as implying the wholesale invalidity of Friedman's contributions to monetary analysis. There was heavy support for much greater econometric rigor than that in which Friedman and Schwartz had engaged. But just as Friedman and Schwartz in the 1980s were badly out of step with modern, best-practice econometrics, Kaldor in the 1980s was badly out of step with modern, best-practice economic analysis of monetary policy and inflation.

Along these lines, Kenneth Wallis, who had been at the October 1983 Bank symposium and produced a book with Hendry (a tribute to Denis Sargan) around the same time (Hendry and Wallis, 1984), remarked of the debate (interview, January 29, 2015): "I didn't get involved particularly. I could see where both sides were coming from."

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[69] Hendry (1993, p. 270) himself suggested that "by the second half of the 1980s" he was more satisfied with the conduct of macroeconomic policy in the United Kingdom than he had been in 1981.

Likewise, Hendry's former student James Davidson regarded the Hendry-Ericsson criticisms as substantially valid but had mixed reactions to the course of the debate, "not least because of the huge respect for Friedman that I have," including on account of Friedman's work on the consumption function (James Davidson, interview, February 12, 2015.)

An illustration of how the Hendry-Ericsson critique was recognized as revealing key shortcomings of the Friedman-Schwartz *Trends* econometric work, but the message that inflation was a monetary phenomenon incorporated greater—not less—acceptance as the 1980s progressed, is provided by considering the case of Australia. Although this is an example that comes from outside the U.K. context, it likely sheds light on the evolving views of the "median" U.K. economist over the same period.

Australia's media had, as already indicated, given the Hendry-Ericsson critique considerable coverage, and the general-to-specific methodology had also received wide acceptance among Australian applied macroeconomists. Against this backdrop, the Reserve Bank of Australia's Glenn Stevens noted in Stevens and Thorp (1989, p. 87) that *Monetary Trends* "came in for trenchant criticism from Hendry and Ericsson (1983)" and considered this "an example of how far econometric methodology had advanced in two decades."[70] This statement left no doubt of Stevens' belief in the econometric weaknesses of *Monetary Trends*. Yet Stevens, who would subsequently be an RBA deputy governor and governor, would also later write that Australian monetary policy after the 1980s had moved to being guided by the proposition that "monetary policy ultimately determined inflation" (Gruen and Stevens, 2000, p. 52).

As already indicated, this view had prevailed in U.K. policymaking for a longer period—since 1979. It would be cemented in the policy of inflation targeting that started in 1992. After that arrangement had been put in place, Mervyn King—who had successive senior positions at the Bank of England starting in 1991—observed that a country's choice of "monetary policy regime," and not other arms of policy, was what was decisive in determining that country's effective choice of its "target inflation rate" (King, 1996, p. 57).

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[70] This statement was accurate in the sense that Friedman and Schwartz had not drawn on recent decades' econometric advances. However, as already indicated, their 1982 work was not representative even of 1960s-vintage mainstream econometrics, not least because of *Monetary Trends*' employment of phase averaging. (Stevens and Thorp, 1989, also wrote that *Monetary Trends* was an application to the United Kingdom of Friedman and Schwartz's [1963b] work on timing relationships. In fact, it was not.)

Policymakers and researchers in the United Kingdom, the United States, Australia, and elsewhere would maintain the view that monetary policy ultimately determined inflation. They would adapt that view in light of empirical problems with monetary aggregates, such as in the large declines in broad money velocity seen in the United Kingdom in the 1980s. But there would be no return to what had been the economic mainstream to the nonmonetary view of inflation. Although this outlook toward the analysis and control of inflation had been a consensus position in the United Kingdom up to 1979, it would come to be regarded as having been fundamentally misguided.

4. The Hendry and Ericsson *AER* article

In 1987, the years of logjam—in which much of the discussion in print of the Friedman-Schwartz/Hendry-Ericsson debate consisted of references by third parties to working-paper versions of the Hendry-Ericsson study—began to end. In March of that year, Hendry and Ericsson issued a new version of their "Assertion Without Empirical Basis" paper as an Oxford University Applied Economics Discussion Paper. Hendry and Ericsson (1987) was then submitted to the *American Economic Review*.

The referees assigned by editor John Taylor included Bennett McCallum, Anna Schwartz (who confined her direct feedback to the *AER* to a summary letter to Taylor, rather than a report), and Friedman. Taylor (2001, p. 112) later recalled to Friedman of "referee work you once did for me when I was an editor at the *American Economic Review*. You signed your 'anonymous' referee report!" Friedman did, indeed, disclose that he was a referee: Although his name was not actually on his referee report, its text referred to Friedman and Schwartz's book as "our" work, and in his cover letter Friedman told Taylor that the editor should be free to identify him to Hendry and Ericsson as the author of the report.[71] The Friedman referee report covered much of the same ground as in the eventual Friedman and Schwartz (1991) reply (including a number of its misstatements).

"Friedman has yet to offer a response to it," Smith (1987, p. 150) stated at around this time, in referring to Hendry and Ericsson (1983). Friedman's cover letter to Taylor indicated his change in view, already signaled by his behavior in 1986 (see above), about

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[71] Friedman letter to Taylor, September 1, 1987, copy provided to the author from Friedman office files by Gloria Valentine in October 2007. (The letter is now available in the Hoover Institution's archives.) That Friedman cover letter was the source of much of the information on his reactions (to the Hendry-Ericsson critique and to its media coverage) that was relayed in Hammond (1996, pp. 199–202).

the merits of writing a response. Friedman confirmed to Taylor his willingness and desire to write a rebuttal in the event that the Hendry-Ericsson paper was accepted by the *AER*. The *AER* subsequently invited a revision of the manuscript.[72] The revision led to a major overhaul of the text of the paper, although some wording used in earlier versions—such as the remark that Friedman and Schwartz's econometric approach, including phase averaging, had led to "rather badly fitting equations"—was retained[73] There was also a title change, which made clear the paper's focus: "An Econometric Analysis of U.K. Money Demand in *Monetary Trends in the United States and the United Kingdom* by Milton Friedman and Anna J. Schwartz."

The revision was complete by mid-1989.[74] This Hendry-Ericsson paper was accepted in 1990, Friedman and Schwartz produced a reply by midyear, and the two contributions were published in the first issue of the following year's volume of the *American Economic Review*. The two sets of authors were therefore finally directly arrayed against one another, in adjacent articles in the March 1991 *AER*.

4.1 Evolution in Hendry and Ericsson's appraisal, 1983 to 1991

A major change in the content of the Hendry-Ericsson appraisal of *Monetary Trends* between 1983 and 1991 is evident in the fact that many of the commentators in the intervening years attributed to Hendry and Ericsson a finding that contradicted the claim of a stable demand for money. For example, Smith (1987, p. 149) stated, "The results rejected the claim that there existed 'stability of the demand for money as a function of a small number of arguments.'"[75] In their final paper, however, Hendry and Ericsson (1991a) *did* find such a stable function in their modeling of U.K. money demand, and they focused on the contrast in fit and stability of their own dynamic demand-for-money specification and that of *Monetary Trends*' phase-average regression.

Another change, discussed now, between 1983 and 1991 is that the pre-1991 versions of the Hendry-Ericsson paper suggested that the data were inconsistent with Friedman and Schwartz's position that U.K. monetary velocity was *not* a 'will-o'-the-wisp." Hendry and Ericsson suggested that "a random walk is one statistical equivalent of a will-o'-the-

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[72] Bennett McCallum was the managing editor principally involved on the *AER* side in this process.
[73] See Hendry and Ericsson (1983, p. 82; 1985, p. 36; 1991a, p. 33).
[74] See Hendry and Ericsson (1989).
[75] Dow and Saville (1988, p. 208) took a similar message from the Hendry-Ericsson (1983) findings.

wisp" (1983, p. 65; see also pp. 69, 71) and, finding that a random walk described well the log-level of annual M2 velocity, they took their result as evidence against Friedman and Schwartz's rejection of the will-o'-the-wisp characterization of velocity. This, however, amounted to a clear case of a terminological clash between the econometrics and monetary-economics literatures. A random walk may be considered synonymous with a "will-o'-the wisp" in some statistical and econometric work. But a random walk does not at all correspond to the view of velocity behavior that Friedman and Schwartz labeled "will-o'-the wisp." This was clear in their subsection titled "Velocity: A Will-o'-the-Wisp?" (Friedman and Schwartz, 1982, Section 6.1, pp. 207–208)—a part of *Monetary Trends* that none of the discussions of will-o'-the-wisp velocity in Hendry and Ericsson (1983, 1985) and Ericsson, Hendry, and Hood (2016) actually referenced. That subsection of *Monetary Trends* makes it conclusively clear that the will-o'-the-wisp hypothesis concerning velocity, as outlined by Friedman and Schwartz, was not—and is inconsistent with—a random-walk view of velocity behavior.

The key reason why the will-o'-the-wisp view of velocity does not correspond to a random-walk representation of velocity is that when (log) velocity is a random walk, the difference between nominal income growth and monetary growth is *random*—in particular, white noise. In contrast, the will-o'-the-wisp view of velocity that Friedman and Schwartz described implies that the difference between the two series is *systematic*: When monetary growth rises by X percent, nominal income growth is (on this hypothesis) typically unchanged. and so velocity growth falls by a roughly compensating amount of $\frac{1}{k}X$ percent (k being close to 1.0), and the associated fall in velocity is not retraced.[76] As Friedman and Schwartz noted, under this hypothesis "nominal income [is] determined by forces largely independent of the quantity of money, so that velocity adjusts passively."[77]

Friedman and Schwartz (1982) cited such hardline U.K. Keynesian literature contributions as the Radcliffe Report and Sayers (1960) as having expounded this will-o'-the-wisp view of velocity. They also noted: "Carried to its limits, this interpretation makes velocity—and its reciprocal—the ratio of two statistically independent magnitudes."[78] This remark demonstrated the contrast with the random-walk case. A

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[76] A symmetric statement holds for declines in monetary velocity.
[77] Friedman and Schwartz (1982, p. 207). Note that this view of velocity can be applied both to cases in which monetary growth is determined by the central bank exogenously and in which it follows a reaction function that makes money endogenous. The common prediction across these cases is that monetary policy actions that tend to increase or decrease monetary growth will have little effect on nominal income growth.
[78] Friedman and Schwartz (1982, p. 207).

random-walk view of log velocity implies that log nominal income growth equals monetary growth plus white noise: $\Delta(p_t + y_t) = \Delta m_t + e_t^v$. Such a one-for-one relationship between nominal income growth holding on annual data is highly amenable to the QTM, although the QTM could also hold if velocity is stationary rather than a random walk.[79]

As it happened, although Hendry and Ericsson (1983, 1985) found the random walk to be a reasonable univariate representation of U.K. log M2 velocity, Campos, Ericsson, and Hendry (1990, p. 288), using the same *Monetary Trends* annual dataset, concluded that this velocity process was actually "'just barely' stationary."[80] As just indicated, however, a verdict in favor of the random-walk case would not imply that velocity is a will-o'-the-wisp in the manner defined by *Monetary Trends*. Detailed consideration of the random-walk case was dropped from the 1991 version of Hendry and Ericsson's paper.

4.2 Hendry and Ericsson's preferred equation

Nelson (2024) has a detailed analysis of Hendry and Ericsson's (1991a) critique of *Monetary Trends* equation and how Hendry and Ericsson arrived at their preferred specification. In view of the coverage provided in that paper, this subsection can concentrate on Hendry and Ericsson's preferred error-correction equation for real money balances over 1878–1970.[81]

The authors estimated cointegrating equation over 1873–1970 by OLS (as in the first stage of the Engle and Granger, 1987, procedure),

$$(m - p - y)_t = -0.309 - 7.00RS_t \, . \qquad (3)$$

The cointegrating vector estimated was the basis for the residuals, $\hat{u}_t$, that served as an error-correction term in a dynamic money demand equation, which Hendry and Ericsson simplified on the basis of exclusion, simplification, and specification tests to:

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[79] The consistency of a random-walk log velocity process with the QTM was reflected in Rasche's (1987, p. 9) interpretation of an ARIMA(0,1,0) representation of log velocity as confirming that velocity was a "valid and useful" concept.

[80] The fact that Hendry and Ericsson's (1991a) Engle-Granger (1987) regression, which has the log of (the inverse of) velocity as its dependent variable, has an R^2 of only 0.56 might be indicative of velocity being a stationary or $I(0)$ process, rather than $I(1)$.

[81] This equation can be replicated exactly using Friedman and Schwartz's (1982) tabulated data (see Nelson, 2024).

$$
\begin{aligned}
(m-p)_t = {} & 0.45\Delta(m-p)_{t-1} - 0.10\Delta^2(m-p)_{t-2} - 0.60\Delta p_t + 0.39\Delta p_{t-1} - 0.021\Delta rs_t \\
& \quad (0.06) \qquad\quad (0.04) \qquad\quad (0.04) \qquad (0.05) \qquad (0.006) \\
& -0.062\ \Delta_2 rl_t - 2.55[(\hat{u}_t - 0.2)\ \hat{u}^2_{t-1}] + 0.005 + 3.7\ (DWW1 + DWW2)_t \qquad (3) \\
& \quad (0.021) \qquad (0.59) \qquad\quad (0.002) \quad (0.6)
\end{aligned}
$$

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

In equation (3), heteroskedasticity-adjusted standard errors are in parentheses, *DWW1* and *DWW2* are intercept dummies associated with each world war, Δ_2 is the two-period difference operator, and rs_t and rl_t are logarithms of RS_t and the U.K. long-term interest rate, respectively. Commentary on the economics of these long-run and short-run equations is now in order.

With respect to the long-run equation (3), although Hendry and Ericsson (1991a) criticized Friedman and Schwartz's means of reaching the conclusion that the long-run demand for money was homogeneous with respect to prices, they imposed unit elasticity in their corresponding long-run equation (2). In addition, the values for the long-run income elasticity and interest semielasticity of money demand that closely resemble those in Friedman and Schwartz (1982).[82] The income elasticity is near unity in *Monetary Trends* and imposed as unitary by Hendry and Ericsson, while the interest-rate slope is within the ballpark of about −10 in both equations (1) and (3). In addition, provided that one regards long-run money demand stability as corresponding to cointegration, one may infer that both sets of authors evidently found essentially the same cointegrating vector, and so both sets of authors obtained a stable demand function for money.

Friedman and Schwartz (1991) correctly stressed the similarity of estimates of long-run parameters, although the validity of this point was overshadowed by numerous incorrect comparisons that Friedman and Schwartz made in the same reply.[83]

With regard to the short-run money demand equation, Hendry and Ericsson (1991a) highlighted the constancy of equation (3) through 1970. The notion that the short-run demand function is stable contrasts with the stress laid by some researchers on the particular grounds for believing that the *long-run* money demand equation should be

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[82] The similarity of the long-run money demand estimates of the two sides had often been stressed by David Laidler, including in Laidler (1985a) and in his remarks at a session on the demand for money held on December 28, 1990, at the American Economic Association meetings in Washington, D.C.

[83] The flaws in the econometric part of their rebuttal are discussed in Ericsson, Hendry, and Prestwich (1998a) and Nelson (2024).

constant. Friedman and Schwartz themselves, via their attempt to isolate longer-term movements, implicitly took a constant short-run money demand function as less likely to be recoverable from the data than a short-run equation. In more recent work, Lucas (1988) and Linde (2001) have implied that, because money demand shocks might follow a changing time series process or because of forward-looking terms in the money demand function, it is unlikely that a dynamic money-demand function that uses only observed data will be constant. Hendry and Ericsson's result apparently contradicts such positions. It is also worth noting, however, that, in equation (3), Hendry and Ericsson relied on unusual nonlinearity (the squared and cubed error-correction terms) in obtaining a constant short-run money demand function. Their findings are therefore consistent with the notion that more standard linear-in-variables short-run money demand functions might exhibit parameter instability.

5. Postscript to the debate, 1998

Two follow-ups to the debate appeared in 1998 in the form of Ericsson, Hendry, and Prestwich (1998a, 1998b). These articles provided further comparisons of phase-average and annual-data representations of U.K. money demand and presented results that extended the sample period to 1993.

In the course of their analysis, Ericsson, Hendry, and Prestwich made allowance for increased interest payments on U.K. bank deposits in the quarter-century to 1993 by changing the opportunity-cost variable in their demand-for-money equation. In particular, whereas Hendry and Ericsson had treated money as, in effect, "a non-interest-bearing asset" (Hendry and Ericsson, 1985, p. 35) and in their 1991 paper simply used the short-term interest rate RS_t to represent the main opportunity cost of holding money, the 1998 studies adopted Friedman and Schwartz's (1982) procedure (first advanced by Benjamin Klein: see Klein, 1974) of assuming a substantial own-rate on money by using RN_t as the opportunity cost of money.

In effect, this series was RS_t rescaled by a function of the money multiplier, $(1-\frac{H}{M})$, H and M being the unlogged levels of high-powered money and broad money, respectively. RN's construction embedded the assumption that bank deposits had implicit or explicit interest rates that were linked to RS_t, the spread of RS_t over bank deposit rates being

determined by the volume of reserve balances (the difference between currency and *H*) that commercial banks held in relation to their deposit issuance.[84]

The RN_t variable seemed to work well in Ericsson, Hendry, and Prestwich's extensions, and their switch to this variable instead of RS_t represented a degree of convergence in the Friedman-Schwartz/Hendry-Ericsson debate. It also seemed an appropriate application of Klein's RN_t concept, as the U.K. banking system has tended historically to have fewer legal restriction on interest payments on bank deposits than its U.S. counterpart.[85] It further tallied well with Campos, Ericsson, and Hendry's (1990) suggestion that U.K. velocity might be stationary because, being a spread, an RN_t series might well be $I(0)$, so it could drive an $I(0)$ velocity series.[86] Two caveats should be made, however, that apply to the treatment of banking developments from the 1960s onward and are relevant to the post-1970 results in both *Monetary Trends* and Ericsson, Hendry, and Prestwich (1998a, 1998b).

First, the *H* variable that should be used in defining RN_t should include only noninterest-bearing reserves. From 1960 to 1980, one category of U.K. commercial banks' reserve balances—special deposits—did bear close to market-bearing interest rates, and so it should not have been included in the RN_t calculations. Nevertheless, both sets of authors included these special deposits in their *H* definition (Friedman and Schwartz, 1982, p. 137; Ericsson, Hendry, and Prestwich, 1998b, p. 301). It follows that their calculations of *RN* used a *H* definition that was not conformable with the *RN* concept.

It is also true, however, that from 1973 to 1980 a new category of U.K. required reserves, so-called supplementary special deposits, existed and did *not* bear interest. This was not

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[84] Earlier, M. Taylor (1993, p. 112) had regarded the case against the Klein (1974), or *RN*, measure as settled. But one item he cited as evidence against the Klein measure—its assumption of perfect competition among banks—can be relaxed without necessarily preventing *RN* from being a valid opportunity-cost variable—for example, *RN* might still be equal to the opportunity cost of holding money other than an additive constant. Further defenses of the *RN* concept against criticisms appeared in Friedman and Schwartz, 1982, pp. 270–271, 274).

[85] Ericsson, Hendry, and Prestwich (1998a, p. 305) stated: "Financial deregulation in 1984 permitted retail sight deposits (checking accounts) to bear interest." Sight deposits had actually been permitted long before 1984 to receive interest payments: there was no longstanding U.K. counterpart to U.S. law's prohibition of interest on demand deposits (see, for example, Friedman and Schwartz, 1982, p. 260). But increasing competitive forces in the U.K. banking system over the first half of the 1980s led to the advent of explicit interest payment on those deposits.

[86] One could then still contend that log velocity and RN_t in the United Kingdom over 1878–1970 were so close to $I(1)$ series that cointegration analysis intended for $I(1)$ variables was approximately applicable (as Meese and Rogoff (1988) argued in considering highly persistent but stationary time series).

included in the monetary base series later constructed by the Bank of England and used by Ericsson, Hendry, and Prestwich (1998a, 1998b) to extend the *H* series after 1975. From 1976 to 1978, this exclusion made little difference, as banks reacted to the marginal reserve requirement by holding deposit expansion down to rates that implied little need to lodge supplementary special deposit balances. But in 1979–1980, these balances became substantial. In July 1980, just prior to the requirement being abolished, supplementary special deposits were $456 million—a number that would imply that *H* would be 4 percent higher in this period if these balances were included in *H*.[87]

That said, the discussion and figures in Ericsson, Hendry, and Prestwich (1998b), as well as other work on U.K. money, are consistent with the expansion in U.K. commercial bank deposits in the 1970s dominating the behavior of $\frac{H}{M}$ and leading to a large decline in the ratio. This result suggests that the correct calculation of H, while important, would not make a decisive difference to the role that the $(1-\frac{H}{M})$ term to play an appropriate role in the calculation of RN_t. As banks' reliance on reserves diminished in the 1980s, they were able to pay interest rates on customer deposits closer to those prevailing in short-term securities markets. The upshot is that, although H_t is not correctly measured by either set of authors in their *RN* calculations, the quantitative effect of not using the appropriate *H* series may have been only minor.

A more serious measurement issue applies to the nominal money stock, *M*. This issue bears on both the RN definition and the construction of real and nominal balances in the Friedman-Schwartz and Ericsson-Hendry-Prestwich regressions. The key point is that, in order to be comparable to the M2 series used by Friedman and Schwartz in their U.S. studies, the series used to measure U.K. *M* (and hence log real balances, $m - p$) should include, alongside currency, retail deposits only. Since 1971, the official U.S. definitions of M2 since 1971 have followed Friedman and Schwartz (1970) in largely excluding wholesale deposits from the aggregate. But no such retail/wholesale distinction in the official definition of monetary aggregates was made in the United Kingdom until 1982, when it initiated an M2 series (later known, once the main official U.K. broad money became M4, as "retail M4").[88] This series arrived too late for Friedman and Schwartz's (1982) study and, in any event, was not backdated before 1982—that is, no historical

[87] These calculations are based on Bank of England (1982, p. 85) and Capie and Webber (1985, p. 107).
[88] See Nelson (2019) for a discussion. Note that this is not a change in monetary concept that is capable of being addressed by different approaches to splicing the monetary series (the matter stressed by Ericsson, Hendry, and Prestwich, 1998a, 1998b). Rather, it implies fundamentally different specifications of the demand functions for M2 and wholesale deposits (different parameters and right-hand-side variables).

series on U.K. retail money or M2 exists (to this author's knowledge).[89]

Friedman and Schwartz (1982) appear to have misconstrued the various "M2" series assembled by researchers and the M2 series briefly reported by officialdom in the early 1970s as corresponding to their own U.S. M2 concept, that is, one excluding wholesale deposits. For example, they stated—incorrectly—that they "use the same basic definition of money for both countries and for the whole period" (p. 216). In fact, unlike their U.S. series, their U.K. money series both incorporates whatever monetary growth prior to 1968 was due to banks' issuance of wholesale deposits.[90] And they simply extended their U.K. money series from 1968 to 1975 by splicing in those years' data on M3—a series that includes all wholesale deposits.[91] As noted by Artis (1984, p. 206), this decision meant that their *M* series was very heavily boosted by the large wholesale deposit expansion that took place in the United Kingdom from 1971 to 1973.

Both Friedman and Schwartz's (1982) and Hendry and Ericsson's (1991a) money demand equations registered parameter shifts or large prediction errors over the 1971–1975 period.[92] When Hendry and Ericsson (1991a) considered the matter of whether a constant-parameter U.K. money demand function was obtainable, they focused on 1878–1970. This reflected problems of instability in U.K. broad money demand after 1970 that were reported by many researchers, including Hendry and Mizon (1978), Grice and Bennett (1984), and Lubrano, Pierse, and Richard (1986). Part of this instability must have been due simply to the fact that the character of deposits included in U.K. broad money series changed drastically, as the series (M3, Sterling M3, or M4) that tend to be the U.K. broad-money series available over the 1970s include wholesale deposits, and wholesale deposits rose as a proportion of these aggregates. The interpretation of the results on long-run demand for money in Ericsson, Hendry, and Prestwich (1998a,

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[89] Although they did not explicitly state that this was the case, Artis and Lewis (1984) in their study of U.K. M2 apparently used a series that included banks' wholesale deposits and not just retail deposits. This can be gleaned from the fact that wholesale deposits were believed to be about 40 percent of M3 by the early 1970s (Congdon, 1982, p. 11), whereas Artis and Lewis suggested that the M2 series they were studying included about 86 percent of M3 or Sterling M3. Evidently, a more clear-cut M2 series was unavailable.

[90] Friedman and Schwartz (1982, p. 114 stated: "From 1963 to September 1971, the Bank of England published estimates of a number of monetary aggregates, including M2…" In fact, although it published a time series on its M2 series back to 1963, the Bank of England introduced the M2 aggregate only in the third quarter of 1970 (Bank of England, 1970; Capie and Webber, 1985, p. 14). As Friedman and Schwartz noted (see also Congdon, 1982, p. 11), it then abolished this series in September 1971. Even during the brief period in which M2 was an official series in the early 1970s, it was not intended to be, or defined as, a retail-money-only aggregate, in contrast to the modern U.K. M2 series introduced in 1982.

[91] See Friedman and Schwartz (1982, p. 114).

[92] See Hendry and Ericsson (1991a, p. 32) and Ericsson, Hendry, and Prestwich (1998b, pp. 410–411).

1998b) consequently needs to be conditioned by the fact that they, like many other researchers—including Friedman and Schwartz—used a series that in effect, changed in definition, over time: an M2-type (retail deposit) series until the 1960s, but not an M2-like aggregate thereafter.

6. Implications of the debate for monetary economics

As Section 3 documented, the initial Hendry and Ericsson (1983) critique attracted considerable attention in public debate in the United Kingdom. In the wake of this attention, Chancellor of the Exchequer Nigel Lawson was asked about the controversy at a parliamentary committee hearing held on March 28, 1984. Lawson contended that the Hendry-Ericsson paper "did not give monetarism a drubbing at all" and suggested of the government's aim of slowing monetary growth to achieve price stability, "The basic concept is very clear."[93]

Although these comments were defensive—the Hendry-Ericsson paper being treated by Lawson's interlocutor as bearing adversely on the Thatcher Government's policy strategy—they did accurately reflect the reality that the Friedman notion that inflation is always and everywhere a monetary phenomenon had considerable support among economists.

This situation continued to prevail over the four decades spanning from Lawson's March 1984 remarks to the present day. Early in the course of this period, for example, Frederic S. Mishkin of Columbia University, delivering a paper in the first session (on August 2, 1984) of a Federal Reserve Bank of Kansas City symposium "Price Stability and Public Policy," observed at the outset of his discussion: "As long as inflation is appropriately defined to be a sustained inflation, macroeconomic analysis, whether of the monetarist or Keynesian persuasion, leads to agreement with Milton Friedman's famous dictum, 'Inflation is always and everywhere a monetary phenomenon."[94] Already the consensus view among U.S. economists in 1984, this position acquired much more widespread support in the United Kingdom over the 1980s, as has already been indicated.

Over these four decades, in both the United States and the United Kingdom, the view of inflation as a monetary phenomenon has evolved into one expressed in measurements of

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[93] In Treasury and Civil Service Committee (1984, p. 73).
[94] Mishkin (1984, p. 1).

and conceptions of monetary policy that do not involve monetary aggregates. This change has largely reflected the fact that, from the 1970s onward, the variations in many monetary aggregates—both narrow and broad series—have been heavily driven by factors other than changes in monetary policy stance. Such factors have included major reclassifications of deposit categories; the development of new nonbank financial institutions and instruments; and expanded interest payments on bank deposits and on reserve balances. Even this post-1984 rethinking of the appropriate interpretation of "inflation is always and everywhere a monetary phenomenon," however, has focused on the appropriate formulation of that proposition in *modern-day* conditions. It does *not* carry with it a retrospective judgment that a money-focused analysis of the course of monetary policy and inflation in past decades is invalid. In particular, according to this rethinking, it likely remains valid to regard the behavior of monetary growth in advanced economies until the 1970s as largely reflecting the stance of monetary policy in those economies.

The notions that inflation is a monetary phenomenon and that monetary growth provided a good representation of monetary policy stance through the 1970s turn out to be fully consistent with Hendry and Ericsson's (1991a, p. 27) statement that, over their 1878–1970 sample period, "the money stock appears to be endogenously determined by the decisions of the private sector." A situation in which the private sector views itself as making decisions on their money holding is completely in accord with a state of affairs in which monetary policy decisions are decisive in steering the course of monetary growth. To see this, note that even in the reference—Barro (1984)—that Hendry and Ericsson cited as an archetypal example of money being treated as exogenously supplied—there was a specification of "the condition that money be willingly held" (Barro, 1984, p. 141). That the money stock can be driven by monetary policy actions yet be willingly held by the private sector is possible because monetary policy actions in period t have important effects on the variables that enter the money demand decision in period t—such as asset yields and, in the case of a forward-looking money demand equation, the expected future paths of output and prices. The fact that the outstanding stock of money is the amount demanded by the private sector is fully consistent with a situation in which different monetary policy choices imply different money-stock paths. Consequently, the fact of money being demanded and held willingly does not prevent the money stock or monetary growth from potentially serving as a valid representation of monetary policy stance.[95]

[95] The reality that central banks use a short-term interest rate as a policy instrument likewise does not, by itself, invalidate monetary growth or the money stock as an indicator of monetary policy stance. When

On the interpretation just given, money was endogenous in the United Kingdom over 1878–1970. But the type of endogeneity present was similar to that considered in the sticky-price analysis of Ireland (2003) and present in other New Keynesian models: monetary policy made the money stock a function of the economy but, nevertheless, monetary policy decisions had a crucial bearing on the course of money and prices over various time horizons, as well as over output in the short and medium runs.

Hendry and Ericsson's position that, over the century of U.K. data studied, the nominal money stock was in practice made a function of the economic state was therefore valid—and it carried the implication that money was endogenous and should properly be so treated in econometric work. It is also important, however, to stress that a finding of this kind does not support specific prominent anti-monetarist positions prevalent during the heyday of the U.K. Keynesian-monetarist debate and expounded by such figures as Godley and Kaldor. In particular, the endogeneity of money, in the sense of the monetary authorities supplying money on demand (or, more precisely, following practices in the markets for bank reserves and other short-term assets that allow commercial banks to issue retail deposits on demand) in any period at a *given* short-term interest rate, does not invalidate viewing observed annual data on monetary growth during Friedman and Schwartz's (1982) sample period (that is, the century to the mid-1970s) as broadly resulting from policy decisions made by the U.K. monetary authorities.[96] This interpretation of the course of U.K. money (or monetary growth) in the century to the mid-1970s parallels how Friedman and Schwartz (1963a) viewed the U.S. money stock during the first five decades of the Federal Reserve's existence, and it can render money or its growth rate a valid indicator of monetary policy stance even when money is not exogenously supplied.

The endogeneity of the money stock in the sense described is different from the passivity

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monetary policy is enacted via an interest-rate reaction function, money may well be a better indicator of policy stance than the policy rate itself. For example, under some circumstances a constant or rising policy rate may be consistent with monetary policy becoming more expansionary, and high monetary growth may accurately convey the change in policy stance. As Allan Meltzer once put it: "For most issues we want to address (as opposed to estimation), it matters very little whether I say: 'The central bank created too much money growth,' or 'The central bank held the interest rate too low for too long and this induced excess money growth.'" (Quoted in Nelson, 2003, p. 1044.)

[96] In contrast, Kaldor advanced money's endogeneity (more specifically, a particular *way* in which the money stock was endogenous) as grounds for doubting that central banks could exercise monetary control and for suggesting that monetary-growth/inflation correlations were simply a reflection of the money stock passively adapting to an autonomously-generated inflation process. See especially Kaldor (1986), as well as the discussion of the Friedman/Kaldor debates in Nelson (2009).

or uncontrollability of money hypothesized in Kaldor's work—as well as his position (see Kaldor, 1986, p. 28) that consumption and investment spending are insensitive to interest rates. Also in contrast with Kaldor's position, such endogeneity is completely consistent with the centrality of monetary policy for the determination and control of inflation.

Putting this interpretation of money's behavior together with some other propositions concerning monetary analysis noted above, it seems to the present author that a reasonable representation of the modern consensus on the conduct of monetary policy and of anti-inflation policy—a consensus that is heavily informed by contributions that Friedman made in the twentieth century—includes a number of key components relevant to the endogenous-money question. In particular, the consensus argues: that sustained inflation reflects excess nominal spending; that the only cure for sustained inflation is via restriction of aggregate demand; that *temporary* resource slack generated by such demand restriction can contribute to *permanently* reducing inflation; that the cost-push and incomes-policy oriented approach to handling inflation, prevalent in U.K. and U.S. economics through 1979, was misconceived; and that monetary growth, even when the central bank does not attempt to control it, can be—and typically *was*, until the past four decades—a good reflection of the monetary policy stance put in place by the authorities.

A corollary of these arguments is that the endogeneity of money in the sense described above does *not* back up the vision of money's role propounded by hardline U.K. Keynesian critics of Friedman in the 1970s and 1980s, including their associated nonmonetary view of inflation's determination. Nor, it should also be stressed, did Hendry and Ericsson (1991a) claim that their own findings provided such support.

Certainly, monetary aggregates have faded in monetary analysis and policy since the mid-1980s. But the decoupling of the money stock from monetary policy analysis does not reflect an embrace of pre-1979 nonmonetary views of inflation. It instead flowed mainly from doubt that money or its growth rate can serve as a good indicator of monetary conditions to the same extent that it did over the periods studied by Friedman and Schwartz (1963a, 1982). Researchers and practitioners have concluded that financial developments have importantly undermined monetary aggregates' value as a measure of monetary policy, compared with the situation prevailing until the 1980s. But they have not embraced the critiques of Friedman's views on inflation that were made on the

Keynesian side of the U.K. Keynesian-monetarist debate that took place in the 1960s and 1970s.

The fact that the underlying behavior of monetary aggregates over time is largely driven by monetary policy actions, even under arrangements in which the central bank does not attempt to fix money exogenously, was reflected in Alan Greenspan's (2002) remark that in the postwar decades, "Monetary policy… allowed a persistent overissuance of money" (which he also calls "excessive monetary growth"), as well as Bernanke's (2010, p. 4) observation that monetary policy analyses that focus on governments' "power to issue money" deal with issues that are "far from being purely theoretical."

With regard to the United Kingdom specifically, the importance in practice of monetary policy for the historical behavior of the money stock is underscored by the linkage between budget deficits and monetary growth that prevailed until the 1970s. In the postwar period up to 1979, monetary base growth had a strong relationship with the U.K. budget deficit—reflecting the accommodative monetary policy reaction functions in force until 1979. After 1979, less accommodative reaction functions ended this linkage (see Nelson, 2019). Ericsson, Hendry, and Prestwich (1998a, p. 304) observed that $\frac{H}{M}$ "changes little over the period prior to 1970." That being the case, the important policy-driven influence on *H* due to budget deficits implied that the monetary authorities (inclusive of the central government) heavily shaped U.K. monetary growth in the period to 1970.

Hendry and Ericsson (1991a, pp. 27–31) did regard some of their key findings as inconsistent with Friedman and Schwartz's theoretical framework. They suggested that their findings established weak exogeneity of prices in the money demand function. This result seems to rule out equations in which the U.K. price level depends on current money.[97] The finding that contemporaneous money cannot be used as a regressor in price-level equations may not be crucial to monetarist claims, however, as much of Friedman's work emphasized that money's impact on prices typically occurs with a *lag* of one to two years (see for example, Friedman and Schwartz, 1982, p. 403; Batini and

[97] In the error-correction/cointegration framework, the principal condition for weak exogeneity is that the error-correction term enter the conditional model only. Otherwise, equations describing the behavior of the regressors contain information about the parameters of interest, specifically, the coefficients in the cointegrating vector. See, for example, Hendry and Ericsson (1991b, pp. 841, 857), Baba, Hendry, and Starr (1992, pp. 32, 33, 39), Banerjee, Dolado, Galbraith and Hendry (1993, pp. 261, 288-291), and P.C.B. Phillips (1991, p. 296).

Nelson, 2001; and Nelson, 2020b, Chapter 15).

Some more specific words on the linkage between prices and money are in order. First, with regard to weak exogeneity, post-1991 research by Hendry and coauthors (Ericsson, Hendry and Tran, 1994, and Emerson and Hendry, 1996) would cast doubt on the firmness of the finding that prices can be treated as weakly exogenous regressors in money demand equations.

Second, it should be emphasized that although weak exogeneity of prices for money demand implies that the *level* of money does not enter price level equations; nothing precludes lagged monetary *growth* from being a significant driver of inflation.[98] If, as argued by McCallum (1993), the QTM should be regarded as pertaining to rates of change or first differences of variables, then Hendry and Ericsson's (1991a) results do not appear particularly damaging to QTM claims about inflation/monetary-growth connections. Banerjee, Dolado, Galbraith, and Hendry (1993, p. 139) suggested with respect to regressions linking money and nominal variables, "the first-differenced regression appears to be testing a different hypothesis." But this statement needs to be qualified, as the QTM is a hypothesis that can be applied to both levels and growth rates. Even if, in fact, the logs of prices and money are not cointegrated, the QTM may be useful in the analysis of both log-levels and log-differences of prices and money.

Specifically, in the case of log-levels, even if an $I(1)$ money demand shock is present and rules out cointegration, one can still have a *ceteris paribus* proposition about the proportionality of money and prices—for example, in impulse response analysis indicating that these series respond by the same ultimate percentage to shocks to nominal aggregate demand.[99] The QTM may, furthermore, be of interest in the analysis of inflation even when money and prices are not cointegrated. This is because, although it is different from a money/prices relationship, a monetary-growth/inflation relationship is

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[98] Using quarterly U.K. data beginning in 1964, Hendry and Mizon (1993) and Hendry and Doornik (1994) found significant positive coefficients on lagged monetary growth (M1) in their inflation equations.

[99] In light of this reasoning, McCallum (1993, pp. 35–36) argued that if the logs of money and prices are "DS [difference stationary] but not cointegrated… it is highly misleading to conclude that in any practical sense long-run relationships are therefore nonexistent." Engle and Granger (1987) appeared to acknowledge this point in parts of their paper, as they veered between describing the lack of cointegration as implying "no relationship" (p. 261) and stating that in the $I(1)$-shock case "the [two levels] series are correlated random walks but are no longer cointegrated" (p. 264). Similarly, although Hendry (1995, p. 249) seemed to suggest that an $I(1)$ error term deprived a theory of meaning, in Hendry (1989, p. 149) he granted that lack of cointegration can be consistent with "a clear relationship in *differences*."

a natural variation on the former: it amounts to assuming a permanent component in shocks to long-run money demand rather than only persistent. As Bennett McCallum remarked (in Backhouse and Salanti, 2000, p. 239), "[e]ven the most extreme monetarists" contemplate *I*(1) money demand shocks.

Hendry (1980) legitimately highlighted the possible fragility of price-on-money regressions that fail to allow adequately for dynamics, especially when the regressions are specified in levels. But that warning does not imply, of course, that regressions of inflation on lagged monetary growth are automatically invalid. Furthermore, as already indicated, the endogeneity of money is likewise consistent with monetary growth being a sound indicator of monetary policy stance under certain circumstances.[100] In this connection, inflation/monetary-growth regressions can be valid ways of understanding inflation's behavior even in periods when the central bank to a large extent follows practices involving considerable accommodation of the private sector's demand for money. Some studies that do allow for dynamics have found that lags of monetary growth enter significantly in U.K. inflation regressions. Darby and Lothian (1983), for example, found that this was the case with regard to the monetary base growth in an equation covering the 1960s and 1970s.[101]

Inflation-on-monetary-growth regressions of this kind are reduced forms. They are not meant to compete with attempts to characterize price-setting behavior through structural Phillips-curve or markup equations. Those structural equations should, in turn, be regarded as endeavoring to capture how monetary policy actions are transmitted to inflation.[102] Although usually not appropriate to include in a multiple-equation system, the reduced forms can serve a useful role in shedding light on the overall monetary growth/inflation relationship generated by the interaction of the economy's various structural relationships.

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[100] The position can be stated in monetary-growth terms, as it implied a view of inflation being always and everywhere a monetary phenomenon in the sense that a 1 percentage point rise in inflation required, other things equal, a 1 percentage point increase in monetary growth, with the prevention of such a rise being within the power of monetary policy.

[101] Darby and Lothian's estimated inflation equations had no lags of the dependent variables. Other evidence for the United Kingdom through the 1970s suggests, however, that prior monetary growth enters inflation regressions significantly even if lagged inflation is also included. For example, if one uses the Friedman-Schwartz (1982) dataset, the lagged log-difference in money enters an inflation equation for 1878–1970 that also has lagged inflation (and an intercept) positively and significantly (p-value = 0.037).

[102] Budd, Holly, Longbottom, and Smith (1984, p. 116) was an early example of a discussion in a U.K. study that explicitly recognized the problem of including money (or other monetary policy variables) in an inflation equation that also included *variables through which* monetary policy might be transmitted to inflation.

7. Conclusion

This paper has considered the monetary-economics implications of the Hendry and Ericsson (1991a) econometric critique of *Monetary Trends*, a critique that first appeared in 1983−1984. It has been important in this analysis to separate work special to *Monetary Trends* and broader propositions about monetary analysis that appeared in not oly that book but also the *Monetary History* and Friedman's wider body of writings.

With regard to the *Trends*-specific work, Friedman and Schwartz (1991) could accurately contend that Hendry and Ericsson (1991a) had, using different econometric methods, reached money demand estimates similar to their own. In particular, the long-run U.K. M2 demand equation arrived at by both sides was similar, with price homogeneity, an income elasticity near or equal to 1.0, and a sizable semielasticity with respect to the short-term interest rate. There is no doubt, however, that Hendry and Ericsson's estimates were reached using more modern methods and did not rely on practices—such as phase averaging—that they correctly criticized in their appraisal of Friedman and Schwartz (1982). Consequently, the Hendry-Ericsson indictment of *Monetary Trends*' econometrics was largely valid, as discussed in detail in Nelson (2024).

With regard to Friedman's broader work on monetarism, it has been argued in this paper that the validity of Hendry and Ericsson's critique of *Monetary Trends* is not inconsistent with the fact that there have been enduring influences of various propositions associated with Friedman's monetary economics. Their critique did not undercut those propositions. Many elements of the economic policy framework seen in the United States and the United Kingdom in the modern day can be regarded as consistent with—and influenced by—tenets of Friedman's monetary analysis. These elements include the permanent system of floating exchange rates; acceptance of no long-run inflation/unemployment tradeoff; a monetary policy oriented toward price stability; emphasis on the distinction between real and nominal interest rates; a multiple-yield-based view of monetary policy transmission; and a permanent-income view of consumer behavior. There are numerous deviations, too, including: the use of an interest rate as the key monetary policy instrument; reliance on measures of real utilization in the setting of stabilization policy; and the lack of prominence (and often, lack of discussion) of the money stock (and its growth rate) in monetary policy analysis and practice.

With regard to the United Kingdom, perhaps the most important change over time in monetary economics in the postwar period is a change that reflects the influence of Friedman's views. This change has been the acceptance in research and policy circles of the view of inflation as a monetary phenomenon and of the accompanying position that no other governmental tool other than monetary policy can control inflation. This view was not challenged by the Hendry-Ericsson critique—which was explicitly a study of money demand, not of inflation or of the system-wide properties of the U.K. economy.

The monetary view of inflation that Friedman helped instill has also come in the past 40 years to be accepted as something that is separable from—and that *has*, in practice, been separated from—Friedman's longstanding emphasis on monetary aggregates and monetary growth. As Goodhart (1997, p. 1598) observed, U.K. policymakers were disillusioned with broad money as a guide to monetary policy well before the symposium that commissioned the Hendry-Ericsson critique of *Monetary Trends*—and, for that matter, well before the publication of *Trends* itself. But, over the rest of the 1980s and beyond, the acceptance of the monetary nature of inflation, instead of dissipating, crystalized among policymakers and the wider U.K. economics profession. Notably, early in his tenure as Chancellor of the Exchequer, Norman Lamont was reported saying, "the truth is that there is no painless way to reduce inflation, and there is no method known to man that does not involve reducing demand." (*Yorkshire Post* (Leeds), December 13, 1990.) This was a virtual echo of Friedman's (1983, p. 11) words, "Do not suppose there is a painless way to end inflation…," and "The only cure for inflation is to reduce the rate at which total spending is growing." (*Newsweek*, November 12, 1979.) Later, in October 1992, Lamont and the Bank of England co-founded the United Kingdom's regime of inflation targeting. That arrangement, still in force today and since 1997 carried out by the Bank's Monetary Policy Committee, institutionalizes the responsibility of monetary policy for price stability.

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