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**NEWS-MAGAZINE MONETARISM**

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## ABSTRACT

### News-Magazine Monetarism\*

This Paper examines some recent monetary policy debates, in light of commentary on those issues contained in some of the work of Milton Friedman. The specific aspect of Friedman's work considered here is the commentary on monetary policy in his *Newsweek* magazine columns from 1966 to 1984. My conclusions from this examination include: (1) in contrast to claims made in the VAR literature, the analysis of monetary policy and the business cycle by Friedman and other critics of monetary policy in the 1960s and 1970s did not assume that the money supply was exogenous, or contend that monetary policy shocks were the dominant source of cyclical fluctuations. Rather, the criticism was of the destabilizing tendency of the monetary policy feedback rule followed in those decades. (2) There is support for the argument of Orphanides (2000a) that many monetary policy prescriptions by commentators in the 1970s were based on over-optimistic estimates of the growth rate of productive potential. Friedman's *Newsweek* discussions, like his other work, were unusual for not making policy prescriptions based on output gap estimates.

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## 1. Introduction

Alan Walters (1987, p. 427) observed of Milton Friedman, ‘In effectiveness, breadth and scope, his only rival among the economists of the 20th century is Keynes.’ Similarly, Alan Greenspan (1997) has remarked, ‘His views have had as much, if not more, impact on the way we think about monetary policy... as those of any person in the last half of the twentieth century.’ John Taylor (2001, p. 101) comments that Greenspan’s words ‘are no exaggeration. Many would say they do not go far enough.’<sup>1</sup>

As the above quotations suggest, Friedman’s influence on academic work on monetary policy in the last several decades has been pervasive. Alan Walters, writing in 1965, noted, ‘The last decade has... seen a revival of interest in money... Many of these studies have been produced or stimulated by Professor Milton Friedman...’ (1965, p. 2). Robert Clower, writing in September 1970, observed, ‘Contemporary discussion of monetary policy centres upon the work of Milton Friedman...’ (1971, p. 25). That remained true seven years later, when Lewis (1977, p. 1) opened his Ph.D. dissertation with the words, ‘Much, if not most, of the present controversy about the appropriate role for monetary policy centres around the views of Milton Friedman.’ Meltzer (1969, pp. 25, 29) offered this perspective: ‘I notice that people take various positions. One is that Milton Friedman is completely wrong; another is that Friedman is almost completely wrong. A third is that there is a grain of truth to what Friedman says... If we develop our analysis and concentrate on improving our understanding of money... rather than on the issue of whether Milton Friedman is wholly right or wholly wrong, we will have more progress...’ By the 1980s, some resolution had taken place, with Friedman and Schwartz (1982, p. 70) observing that ‘[t]he climate of professional opinion has changed greatly’ since the 1950s, and that the framework that they had advocated was now ‘more nearly in the mainstream’.

Friedman’s work continues to feature prominently in discussions of current policy issues by central bankers. For example, the archive on the European Central Bank’s (ECB’s) web page indicates<sup>2</sup> that the President, Vice-President, and Chief Economist of the ECB have all given speeches that include publications of Friedman’s in their bibliography, with a variety of articles from 1951 to 1992 cited. The reach of Friedman’s influence on monetary policy discussions ranges from the acceptance by

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<sup>1</sup> Beside Walters’s (1987) entry on Friedman for the *New Palgrave Dictionary*, see Brunner and Meltzer (1993) and Hafer and Wheelock (2001) for discussion of Friedman’s contributions in the context of other monetarist studies.

<sup>2</sup> As of April 2002.

policy-makers of the absence of a long-run inflation/unemployment trade-off,<sup>3</sup> to their use of specific phrases due to Friedman. For example, Otmar Issing, Chief Economist and Member of the Executive Board of the ECB, refers (2001, p. 291) to ‘the validity of Friedman’s famous dictum that monetary policy lags are long and variable’, while Laurence Meyer, member of the Board of Governors of the Federal Reserve System from 1996 to 2002, observes that, ‘Few economists would disagree that inflation is, as Milton Friedman taught us long ago, always and everywhere a monetary phenomenon’ (Meyer, 2001, p. 5).<sup>4</sup> Perhaps the ultimate testament to Friedman’s influence is that the word ‘Friedmanite’ (adjective and noun) appears in the *Oxford English Dictionary*.<sup>5</sup>

It would, however, trivialise Friedman’s contribution—and be no compliment to a person whom the *Economist* magazine once described as ‘able to argue the hind leg off a horse’ (1970, p. 37)—to claim that his views have been so integrated into the mainstream that they are now uncontroversial. On the contrary, debate continues on the extent to which the current monetary policy practice of inflation targeting can be regarded as an evolution from, or a sharp departure from, Friedman’s policy proposals (e.g. Gavin, 1996; Barro, 1998; Leeson, 2000; Meyer, 2001). Similarly, opinion differs on the question of whether models used in today’s macroeconomic analysis include the features emphasised by Friedman, or whether these models, instead, constitute a rejection of Friedman’s views (e.g. Goodfriend and King, 1997; Woodford, 1999; Alvarez, Lucas, and Weber 2001; Nelson, 2002; Svensson, 2002). Moreover, as discussed in Section 4 below, it has been claimed that the findings of the recent vector autoregression (VAR) literature reject Friedman’s interpretation of the post-war data (Sims, 1998).

In this paper, I examine some monetary policy issues discussed in the recent literature—doing so in light of commentary on those issues contained in some of

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<sup>3</sup> For further discussion of Friedman’s views on the trade-off, see Section 5 below.

<sup>4</sup> These speeches also illustrate the limitations of any attempt to quantify the extent of Friedman’s influence on monetary economics and monetary policy: both Issing and Meyer explicitly mention and quote Friedman, but do not include any Friedman paper in their bibliography—such as sources for Friedman’s ‘long and variable lag’ expression (e.g. Friedman, 1961, p. 464) and his ‘always and everywhere a monetary phenomenon’ statement (e.g. Friedman, 1963, p. 17). On many monetary issues, Friedman’s contribution has become so well-known that explicit reference to his work has become almost superfluous.

<sup>5</sup> The dictionary entry (1989, p. 192) also gives the variant ‘Friedmanian’, an adjective that seems to have originated earlier (Pesek and Saving, 1963, p. 353), and that has appeared in Friedman’s work as well as that of Robert Lucas, Paul Samuelson, and Lars Svensson (see Friedman, 1970, p. 325; Lucas, 1972, p. 121; Samuelson, 1973, p. 169; Persson, Persson, and Svensson, 1987, p. 1423). Still further variants have appeared: Kane (1967, p. 432), Bhagwati (1977, p. 225), and Goldfeld (1982, p. 362) use the word ‘Friedmanesque’.

Friedman's work. The specific aspect of Friedman's work on monetary policy that I draw upon is his series of columns in *Newsweek* magazine from 1966 to 1984. The analysis and commentary in these columns clarify Friedman's positions on a number of issues, including the behaviour of velocity (discussed in Section 3) and the role of monetary policy shocks in business cycle fluctuations (Section 4).

Friedman's *Newsweek* columns have themselves been a source of controversy. Walters (1987, p. 426) contends that in Friedman's *Newsweek* columns, 'high professional standards of integrity were maintained'. Nevertheless, other prominent scholars have criticised the *Newsweek* columns, on the grounds that they take more extreme positions on monetary policy than are present in Friedman's scientific work. Tobin (1970, p. 301) claimed that '[i]n his less guarded and more popular expositions' of his views on monetary policy, such as in his *Newsweek* columns, Friedman came 'close to asserting that [changes in money] are the unique cause' of nominal income variations. In 1983, Robert Solow gave a similar characterisation of Friedman's *Newsweek* columns (see Section 3 below). More recently, Paul Krugman has appeared to endorse these criticisms by expressing the opinion that Friedman 'has often been wrong, and... is sometimes willing to cut corners to win an argument' (Krugman, 1994, p. 92).

One aspect of Friedman's *Newsweek* columns, namely their position on the macroeconomic effects of bond-financed tax changes, has been examined in detail previously (Gordon, 1974). Gordon (1976, p. 55) states that, while he once believed that there was 'an inconsistency between "academic journal monetarism" and "news-magazine monetarism"', a close examination indicates that the analysis of the effects of a tax increase in Friedman's *Newsweek* columns is not different in substance from that in his scientific work.<sup>6</sup> This leaves open the possibility that Friedman's discussions of monetary policy in his *Newsweek* columns were indeed—as alleged by Solow and Tobin—inconsistent with, and more extreme than, his scientific work. On the basis of an examination of the *Newsweek* columns, I argue below that these criticisms are unwarranted—i.e., that the positions on monetary policy and the quantity theory of money presented in Friedman's *Newsweek* columns are fully consistent with the positions he presented on those subjects in his scientific writings.

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<sup>6</sup> Beside those mentioned in the text, journal articles and books that have referred to the analysis in Friedman's *Newsweek* columns include Laidler (1982, p. 299), Evans (1984, p. 205), Bruno and Sachs (1985, p. 195), Ball and Mankiw (1995, pp. 1161–1162), and Barnett (1997, p. 1171).

I restrict myself to the *positive* analysis in Friedman’s columns—his discussion of the consequences of, and forces driving, actual US monetary policy actions—and so place normative aspects, such as Friedman’s advocacy of a constant money growth rule, beyond the scope of this paper.<sup>7</sup> This positive analysis, alongside Friedman’s scientific work, establishes that his views on monetary policy as carried out in practice in the post-war United States, were both more eclectic and more realistic than many of his critics have acknowledged. This casts doubt, as I show, on the claims that the findings of the structural VAR literature dramatically undermine Friedman’s empirical positions.

## 2. The columns

The source material for this paper is the close to 300 columns that Milton Friedman contributed to *Newsweek* approximately every three weeks, beginning with the September 26, 1966 edition and finishing with the January 16, 1984 edition; see Friedman and Friedman (1998, pp. 356–364). All but one of the columns published to August 1972 were reprinted in Friedman (1972); 31 of the additional 38 columns published to October 1974 were reprinted in Friedman (1975); and 60 of the additional 137 columns to October 1982 were reprinted in Friedman (1983). In addition to relying on the reprinted material, I obtained copies of all the non-reprinted columns from the original *Newsweek* editions.<sup>8</sup> In total, I judged 189 of Friedman’s columns to cover monetary or macroeconomic policy issues, and these were the ones used for the analysis below.

## 3. Behaviour of velocity

In an interview given in 1983, Robert M. Solow stated,

‘As far as Friedman’s arguments are concerned, I always thought that he sang two tunes. In the economic[s] profession, he was absolutely reasonable. I could find no distinction between his modern quantity theory of money and eclectic Keynesian economics. But in writing for *Newsweek*, he argued a hard monetarism, as against the soft monetarism of the “modern quantity theory”. In hard monetarism, velocity is constant and *nothing* but the money supply

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<sup>7</sup> Some columns where Friedman made policy recommendations nevertheless prove useful for obtaining information about his positive economics, e.g. concerning behaviour of monetary velocity, and the use of output gaps in analysis of the state of the economy (see Sections 3 and 5 below).

<sup>8</sup> I used the US editions, as, from 1976 onward, non-US editions of *Newsweek* frequently replaced Friedman’s columns with local material.



matters for nominal GNP. I thought that was just factually wrong.’ (Quoted in Klammer, 1984, p. 145).

Was the analysis in Friedman’s *Newsweek* columns based on a constant-velocity set-up where ‘*nothing* but the money supply matters for nominal GNP’?<sup>9</sup>

From his earliest columns, Friedman stressed a long and variable time lag between monetary policy changes and the economy (e.g. January 9, 1967).<sup>10</sup> This alone implies an acceptance that velocity is not constant. But the *Newsweek* columns also accepted many other sources of velocity movements. As in Friedman (1956), the columns acknowledged that interest rates affected the cost of holding money balances, and so, the amount of nominal income consistent with a given quantity of money (e.g. January 23, 1967; May 12, 1975). In line with the framework in Friedman (1956) that made the rate of return on physical assets a separate argument in the money demand function, the columns gave declining inflation as a reason why some money growth would be absorbed into cash balances and not reflected in higher spending (October 16, 1972). Friedman also cited ‘the desire of people to hold somewhat more money relative to their income as they become richer’ (June 3, 1968) as grounds for why the long-term money growth rate would exceed the growth in real income under stable prices—consistent with the somewhat higher-than-unity real income elasticity of long-run money demand estimated by Friedman (1959) and Friedman and Schwartz (e.g., 1982) on US data. Another argument of Friedman and Schwartz (1963, 1982), namely that there was a negative relationship between money demand and economic confidence (and so a negative relationship between velocity and uncertainty), is reflected in the columns. Among the events Friedman cited in the columns as triggering uncertainty, and therefore falls in nominal income growth relative to monetary growth, were President Nixon’s 1971 introduction of price controls (February 7, 1972), the Vietnam War (October 16, 1972), and the volatile behaviour of interest rates and the economy during the early 1980s recessions (July 25, 1983).

In addition to these money-demand-based factors, the columns accepted that other events could create divergences of nominal income growth from monetary growth, one example given being the 1970 General Motors strike (January 10, 1972).

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<sup>9</sup> Krugman (2002) similarly characterises Friedman’s position as an ‘insistence that changes in the money supply explain all of the economy’s ups and downs’.

<sup>10</sup> Dates given in parentheses refer to the edition of *Newsweek* that contains the relevant column.

Clearly, the economic analysis in Friedman's *Newsweek* columns was not based on a constant-velocity assumption. Rather, by stressing the long and variable lags in the money-income relationship, and by permitting variables that affect the cost of holding money to produce discrepancies between money supply growth and nominal income growth, the columns are consistent with Friedman's scientific writings on the subject.

#### 4. Monetary policy shocks and Federal Reserve policy

Christopher Sims (1998) argued that:

‘There is a view, which Milton Friedman used to restate regularly some years ago, that erratic variation in monetary policy is the primary source of business cycle fluctuations, with each post-war US business cycle largely explainable via the pattern of monetary policy variations preceding it. Friedman used to defend this view via statistical analysis that took the time path of a monetary aggregate as a sufficient statistic for the time path of monetary policy. The recent VAR literature decisively undercuts this way [of] looking at history... [Its] conclusion [is] that the contribution of policy shocks to business cycle variation is modest...’ (Sims, 1998, p. 934).

Leeper, Sims, and Zha (1996, p. 2) elaborate on the vector autoregression (VAR) findings that they believe contradict Friedman's statements:<sup>11</sup>

‘Another robust conclusion, common across these [structural VAR] models, is that a large fraction of the variation in monetary policy instruments can be attributed to the systematic reaction of policy authorities to the state of the economy.’

By way of contrast, Michael Woodford (1998, p. 393) writes:

‘The VAR evidence... implies that the *unsystematic* component of monetary policy has not been a very important source of disturbances to the economy. That finding might be disquieting to some monetarists, though I actually suspect that it would be cheerfully accepted by Friedman and Schwartz.’

And Bennett McCallum (1998, p. 307) has remarked,

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<sup>11</sup> See also Sims (1980, p. 2; 1996, p. 117).

‘Friedman, Brunner and Meltzer have never contended that typical central bank behaviour does *in fact* feature exogenous money growth rates. On the contrary, these writers have frequently been critical of actual central banks precisely because of their responses (in terms of money growth rates) to cyclical conditions.’

Similarly, Kenneth West (1993, p. 162) observes that the hypothesis ‘that the money supply... is set in total disregard to the state of the economy’ is ‘not a view that Friedman or anyone else has advocated, as far as I know’.

The dispute implicit in the above quotations can be clarified by considering what is the appropriate parameterisation, in describing post-war US data, of the following reaction function for quarterly growth in the nominal money stock ( $\Delta m_t$ ):

$$\Delta m_t = b_0 + b(L)e_{mt} + c(L)\mathbf{x}_t', \quad (1)$$

where  $b_0$  is a constant,  $\{e_{mt}\}$  is a sequence of exogenous policy shocks,  $\mathbf{x}_t'$  is a vector of non-policy shocks, and  $b(L)$  and  $c(L)$  are (possibly infinite-order) polynomials in the lag operator  $L$  (so e.g.  $b(L)e_{mt}$  is a distributed lag of the  $e_{mt}$  series). Equation (1) can be regarded as the money supply function implied by a monetary policy that permits money growth to expand or contract in response to movements in, for example, output, the exchange rate, or inflation. For since these endogenous variables can be written (using the Wold representation) as a function of the history of all the shocks hitting the economy, monetary policy can be regarded as allowing  $\Delta m_t$  to be a function of those shocks (see Christiano, Eichenbaum, and Evans, 1998).<sup>12</sup> Similarly, a version of equation (1) holds if the monetary authorities follow an interest rate rule; in that case, the shock vector  $\mathbf{x}_t$  includes money demand shocks (see Poole, 1970).

At issue is whether, in his discussion of US monetary policy as it operated in practice in the post-war period,<sup>13</sup> Friedman’s arguments implied that setting all elements of the

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<sup>12</sup> As a concrete example, suppose the policy rule implies a money supply reaction function of the form  $\Delta m_t = d un_t + e_{mt}$ , where  $d > 0$  and  $un_t$  is the unemployment rate. Suppose further that in equilibrium, the policy rule and structure of the economy imply that the solution for unemployment in terms of underlying shocks is  $un_t = c_1 e_{mt-1} + c_2 v_t + c_3 v_{t-1}$ , where  $v_t$  is a real shock (assumed to be white noise). Then the equilibrium money relation is  $\Delta m_t = d[c_1 e_{mt-1} + c_2 v_t + c_3 v_{t-1}] + e_{mt}$ , which can be cast in the form of equation (1) by setting  $b_0 = 0$ ,  $b(L) = 1 + dc_1 L$ , and  $\mathbf{x}_t = v_t$ , and giving  $c(L)$  a single row consisting of  $dc_2 + dc_3 L$ . Note that if either the policy shocks or non-policy shocks are serially correlated, it is assumed that they have been re-expressed, by substitution, in terms of underlying, white-noise innovations, with  $e_{mt}$  in equation (1) corresponding to the policy innovations and the  $\mathbf{x}_t$  to the non-policy innovations.

<sup>13</sup> I stress that the Sims, Leeper-Sims-Zha, and Woodford discussions quoted above all refer to the post-war US data. For that reason, Woodford’s characterisation of Friedman and Schwartz is not

coefficient matrix  $c(L)$  to zero in the policy reaction function (1) was a good approximation. Sims's characterisation is that Friedman argued that policy shocks dominated the behaviour of  $\Delta m$ , and that these shocks contributed substantially to observed output variability. In that case, policy responses to non-policy shocks were not empirically important, so all entries of  $c(L)$  could, indeed, be set to zero. On the other hand, the claim by West and others that Friedman accepted that monetary policy in practice reacted to the state of the economy, and so to non-monetary shocks, would imply that there is no presumption that  $c(L)$  has only zero entries; nor that the policy shocks dominate the  $\Delta m_t$  series. In that case, Friedman's position on the importance of monetary policy would be disconnected from any claim about the empirical importance of monetary shocks—either for money growth variation, or for the behaviour of other variables, such as nominal income or physical output. Indeed, as discussed shortly, the Woodford argument quoted above rests on there being such a disconnection.

In a sense, the divergent positions of Sims and West given above can be regarded as different interpretations of Friedman's position on the exogeneity of money. But in discussing Friedman's position on exogeneity, it is important to distinguish two issues. Friedman and Schwartz certainly did regard it as 'appropriate to regard the money stock as exogenous (i.e., determined by the monetary authorities)' (Friedman and Schwartz, 1991, p. 42). That is, for particular paths for variables directly controllable by the central bank, such as open market operations or reserve requirement ratios, there was an implied path for money growth; and alterations in the paths of the control variables would change this money growth path in a predictable direction. That position of Friedman's is not the one principally under dispute by the recent VAR literature,<sup>14</sup> much of which implicitly takes a similar view by positing a money supply reaction function like equation (1) (e.g. Christiano, Eichenbaum, and Evans, 1998). Rather, the disputed question is a *second* exogeneity issue, namely

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necessarily inconsistent with Cagan's (1978, p. 88) statement that Friedman and Schwartz (1963) found 'that money had been the most important source of disturbance to the economy over that [1867–1960] period'. In addition, Hetzel (2001) argues that the key inter-war monetary policy mistakes discussed by Friedman and Schwartz (1963) would not fall into the category of monetary policy shocks.

<sup>14</sup> To be sure, some of the VAR literature, including Leeper, Sims, and Zha (1996), has focussed more on the details of the reserves market than Friedman typically did, and has included reserves-based measures of monetary policy in the analysis, rather than a single broader aggregate such as M1 or M2. But the Sims and Leeper-Sims-Zha papers quoted above do not claim that their crucial difference with Friedman is that money should be defined more narrowly; rather they argue that *all* monetary and reserve aggregates in practice respond to non-policy shocks, and that this contradicts Friedman's position.

whether Friedman's characterisation of actual Fed policy admitted non-zero responses to non-policy shocks in rule (1) above.<sup>15</sup>

Friedman's *Newsweek* columns provide a running commentary on Federal Reserve policy from the mid-1960s to the early 1980s, so they constitute a valuable basis for discriminating between Sims's and West's characterisations of Friedman's position on actual monetary policy. Here are the most pertinent excerpts on the subject from the *Newsweek* columns:

'Throughout the post-war period... the Fed has tended to delay action and then, when it had to act, to go too far.' (October 30, 1967).

'Recent monetary growth partly reflects the Fed's reaction to the stock market crisis in May and to a Federal debt issue...' (July 6, 1970).

'The early stages of the [1960s] inflation produced a sharp overreaction by the Fed that caused a credit crunch in 1966 and a mini-recession in 1967.

Overreaction to that mini-recession set it off on the accelerating inflation of 1967 to 1969. Fine-tuning with a sledgehammer!' (July 26, 1971).

'...any attempt to use monetary policy for fine-tuning is likely simply to introduce additional instability. And this is indeed what has happened.' (February 7, 1972).

'The Fed currently attempts to control the money supply indirectly, by controlling a particular interest rate (the Federal funds rate).' (December 8, 1975).

'[Of the] pressures impinging on the [Federal Reserve] System... the most important are the pressures to create money in order to pay off exploding federal spending and in order to promote the goal of "full employment".' (October 3, 1977).

'...pressures from Congress and the Administration to finance rising government spending and to keep interest rates low are a major reason for high monetary growth...' (April 24, 1978).

'In mid-1982, alarmed at the severity of the recession and at the threat of an international debt crisis, [the Fed] stepped hard on the accelerator.' (January 16, 1984).

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<sup>15</sup> Alan Walters's position on the exogeneity of money in the United Kingdom is also clarified by making this distinction. It can reconcile his statement that 'the aggregate quantity of money is determined by the monetary authorities' (Walters, 1970, p. 42) with his observation that 'it is a fair caricature to suppose that the [UK] authorities fix the interest rate and supply the market with the quantity of money needed to sustain that rate...[T]here was in fact no control over the reserve base...' (Walters, 1970, pp. 43, 62).

As the above quotations indicate, the columns recognised that the Federal Reserve reacted to economic developments, including movements in the stock market, the international economy, inflation, output, and federal deficits. They are also explicit in recognising the Fed's use of an interest rate instrument.<sup>16</sup> They clearly do not imply a monetary policy reaction function in which only policy shocks matter for monetary growth.

What does this imply for Friedman's stress on the importance of monetary policy? There is no doubt that, both in his scientific work and elsewhere, Friedman argued, as he put it in his October 30, 1967 column, that '[i]nstead of offsetting other forces making for economic instability, the Fed has itself been a major source of instability', and that he described US monetary policy as 'erratic', as Sims notes.<sup>17</sup> Sims's characterisation of Friedman's view is that monetary policy generated output volatility in the post-war US by injecting an extra source of disturbance, namely the policy shocks  $e_{mt}$  in equation (1). In the same vein, Yoshikawa (1993, p. 121) claims that Friedman is among the 'monetarists... [who] consider unanticipated changes in the money supply exogenously caused by central banks to be the major shock driving economic fluctuations'.<sup>18</sup>

But monetary policy does not have to generate policy shocks in order to be a contributor to total output volatility. A systematic monetary policy rule, that feeds back on the state of the economy and contributes no extra type of shock, is capable of magnifying cyclical variability. In terms of equation (1), such policies correspond to zero values for the  $b(L)$  coefficients combined with non-zero, but inappropriate, choices for the feedback coefficients that appear in the  $c(L)$  matrix. Such a policy would not introduce policy shocks, but would instead, exacerbate cyclical fluctuations by propagating the effects of non-policy shocks.

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<sup>16</sup> As Goodhart (1989, p. 331) observes, when discussing 'the level of short-term interest rates, [Friedman] had no doubts that these were normally determined by the authorities, and could be changed by them'. In his analysis of the UK situation, Alan Walters also recognised that policy-makers had an interest rate reaction function, observing that '[t]he government increases Bank rate when prices start or are likely to start rising too rapidly' (Walters, 1970, p. 46). In comments on an earlier version of this paper, Allan Meltzer has remarked, 'We all understood that central banks controlled interest rates not money. But looking at interest rates cannot tell me whether money is easier or tighter unless I observe how the stock of money changes relative to the demand for money.'

<sup>17</sup> For example, in his July 5, 1971 column, Friedman asked, 'Why must the Federal Reserve swing so erratically from side to side?'

<sup>18</sup> Similarly, Canova and De Nicoló (2002, p. 1132) claim: 'Friedman and Schwartz (1960) [*sic*]... argued that rates of change in money were good approximations to monetary policy disturbances.'

The evidence from the *Newsweek* columns suggests that Friedman did recognise that there was considerable response of monetary policy to the state of the economy. That Friedman nevertheless blamed the Fed for creating instability is in keeping with the point in Friedman's scientific work that stabilisation policy can be destabilising (e.g. Friedman, 1953).<sup>19</sup> Bad feedback rules, not an emphasis on the importance of monetary policy shocks, are central to this critique.<sup>20</sup> In keeping with this, Congdon (1982, p. 15) observes that 'Friedman has... only contended that [monetary] targets prevent [the effects of] non-monetary disturbances... from being exaggerated'.

To avoid misunderstanding, the present author does not disagree with the finding of the structural VAR literature, such as Leeper, Sims, and Zha (1996), that monetary policy shocks account for a relatively small fraction of the post-war variation in both monetary policy instruments and in output. Rather, my point is that such findings are not a contradiction of Friedman's position. The above quotations from Friedman reinforce West's contention that Friedman accepted the existence of systematic monetary policy responses to the state of the economy.<sup>21</sup> And the relative unimportance of monetary policy shocks certainly does not imply that systematic monetary policy cannot matter very much for cyclical fluctuations, nor that ill-chosen systematic monetary policy rules are not destabilising. As Woodford (1998 p. 393) observes, 'The VAR evidence... in no way implies that the nature of *systematic* monetary policy does not greatly matter for the effects (upon both inflation and output) of *other* kinds of disturbances.' Christiano, Eichenbaum, and Evans (1999, fn. 4) concur that the VAR literature 'is silent' regarding 'the impact of the systematic component of monetary policy on aggregate output and the price level.' Walsh (1998, p. 33) gives an example: 'If policy is completely characterised as a feedback rule on the economy, so that there are no exogenous policy shocks, then the VAR

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<sup>19</sup> LeRoy (1995, p. 238) argues that 'Friedman opposed discretionary policy on substantive grounds: policy-makers can be expected neither to diagnose the problem accurately enough nor to implement a policy response quickly enough to affect the macroeconomic environment in the right direction.' This opposition in principle applies to versions of rule (1) with no policy shock terms. For explicit denials by monetarists that they emphasise only policy shocks, see Friedman and Schwartz (1982, p. 552) and Brunner (1983, p. 50).

<sup>20</sup> The closest to a contradiction of this position that I know of in Friedman's writings is his criticism of the real business cycle (RBC) literature on the grounds that its emphasis on 'technological change as the chief source of disturbances... has exaggerated their importance relative to monetary disturbances' (Friedman, 1993, p. 173). The RBC literature, however, attributes literally *no* output variability to monetary shocks. So RBC work can be criticised for understating the importance of monetary shocks, even if these shocks in practice account for only a modest portion of cyclical variability.

<sup>21</sup> Indeed, Friedman and Schwartz pointed to the fact that US history had featured several different monetary policy feedback rules (different arrangements for the provision of money to the economy), yet considerable consistency in the money/nominal income relation, as evidence of the importance of money for economic behaviour. On this, see e.g. Friedman (1961, p. 450), Friedman and Schwartz (1970, p. 139), Brunner (1986, p. 45), Hammond (1996, p. 97), and Batini and Nelson (2001).

methodology would conclude that monetary policy doesn't matter... [I]t does not follow that policy is unimportant; the response of the economy to non-policy shocks may depend importantly on the way monetary policy endogenously adjusts.' Indeed, as Brunner and Meltzer (1993, p. 24) argue, 'choice of monetary regime can increase stability... by eliminating (or reducing) the induced monetary responses that augment real shocks.'

It is also worth noting that, unlike the present VAR literature, two of Friedman's most prominent contemporary critics—James Tobin in the US and Nicholas Kaldor in the UK—did not interpret Friedman's analysis of the data as resting on the proposition of negligible response of monetary policy to the state of the economy. For example, Tobin (1976, p. 95) observed that 'central banks, according to Friedman's own criticism of them, supplied money to accommodate the economy's demands.' And Kaldor (1985, p. 13) noted that '[i]t was nowhere stated in the writings of Friedman... that the quantity theory of money *only* holds...[when] the monetary authorities are sufficiently "competent" to regulate the money supply.'

All in all, I find that there is considerable support in the *Newsweek* columns for Woodford's conjecture that the VAR findings 'would be cheerfully accepted by Friedman and Schwartz'.

## 5. Comparisons with Paul Samuelson's *Newsweek* columns<sup>22</sup>

For much of Friedman's period as a *Newsweek* columnist, Paul Samuelson also had a *Newsweek* column.<sup>23</sup> Though Samuelson's column often dealt with macroeconomic policy, he rarely covered precisely the same subject matter as Friedman's contemporaneous column, so a systematic comparison of forecasts made in each column is difficult. However, on two key macroeconomic issues, there is a major contrast in the positions advanced by each columnist.

*A. The inflation/unemployment trade-off.* In his academic work, Paul Samuelson was jointly responsible for the proposition that there was a permanent trade-off between unemployment and inflation in the US (Samuelson and Solow, 1960). He continued this theme in his *Newsweek* columns in the late 1960s. In his July 14, 1969, column,

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<sup>22</sup> The exercise reported in this section was independently suggested by Milton Friedman, Athanasios Orphanides, and Anna Schwartz in their comments on an earlier version of this paper.

<sup>23</sup> Approximately 250 columns by Samuelson were published in the editions from September 19, 1966 to May 11, 1981. All but seven of the columns to April 1973 were reprinted in Samuelson (1973); an additional hundred columns to 1981 were reprinted in Samuelson (1983). I examined these reprints and also obtained copies of all the non-reprinted columns from the original *Newsweek* editions.



Samuelson stated that the inflation experienced by the US since 1965 had confirmed his 1960 belief in a Phillips curve. In his October 26, 1970 column, Samuelson again affirmed, ‘The trade-off between full employment and price stability does constitute a cruel dilemma for any Administration...’

Friedman, of course, argued in his scientific work that there was no long-run trade-off: real benefits of inflationary policies would wear off once the new inflation rate was embedded in inflationary expectations (Friedman, 1958, 1966). Macroeconomic stimulus that pushed inflation to a higher rate could not lower unemployment permanently below its natural rate—or, to put the point more positively, full employment and growth at potential were not incompatible with price stability. Friedman expressed these themes in an early *Newsweek* column entitled ‘Inflationary Recession’ (October 17, 1966). There he noted that in recent years, ‘rising prices stimulated economic activity because they were rising faster than people had anticipated... The only way to make an expansion of this kind last is... still more rapid inflation’. Instead, he recommended a monetary and fiscal program consistent licy would ‘prepare the basis for a subsequent non-inflationary expansion’.

The views advanced by Friedman in the 1960s that the long-run Phillips curve was vertical, and that inflation and unemployment could rise together as the short-run trade-off wore off, have proved more durable than Samuelson’s 1960s view that there existed a permanent trade-off. Indeed, in his March 21, 1973 column, Samuelson conceded, ‘Years ago we’d have called you neurotic if you worried about inflation and recession at the same time. Now... [w]e’ve learned about “stagflation”...’

*B. Productive potential and the output gap.* Orphanides (2000a, 2000b) argues that a major source of monetary policy errors in the US in the 1960s and 1970s was inaccurate information on the degree of slack in the economy. A key problem was that ‘[a]s is now evident, real-time estimates of potential output severely overstated the economy’s capacity’ (Orphanides, 2000a, p. 16). Orphanides notes that Friedman was consistently cautious about relying on output gap estimates, but contends that policy-makers and other influential outside economists took the official output gap series seriously. As a result, policy-makers permitted what now appear easy monetary policy settings—a serious mistake in light of the double-digit inflation that resulted. Taylor (2000), by contrast, argues that ‘potential GDP and its growth rate became politicised as early as the late 1960s; serious economic analysts... paid no attention’ to the official figures. Paul Samuelson’s views on the output gap over this period are of interest because they suggest whether some ‘serious’ economists did accept the validity of the official gap estimates.

Samuelson supported the use of the output gap in monetary policy and, in the late 1960s, endorsed the official quantitative estimates of the gap. In his July 14, 1969 column, Samuelson praised the Kennedy Administration's economists for introducing the output gap concept into policy, and declared that their estimate of 'growth of [US] real potential [GDP] at 4-plus per cent a year' had been vindicated.

In his August 2, 1971 column, Samuelson reaffirmed that the US had a 'more than 4 per cent' potential output growth rate, and that 'to get [unemployment] down to... the full-employment level, we need real growth rates of 5 and 6 per cent from now to November 1972'. He emphasised that this was based on a conservative (i.e. high) estimate of the full-employment rate of unemployment. His estimate of the required real growth needed to restore full employment implies an output gap of about  $-2.25\%$  in mid-1971. Yet this estimate, which Samuelson considered if anything biased toward zero, compares to a present (2002) Congressional Budget Office estimate of the 1971 Q2 output gap of only  $-0.4\%$ .<sup>24</sup> Thus even a lower-bound estimate by Samuelson of the gap appears in retrospect to have overestimated the amount of slack in the economy in 1971 by nearly 2%. Later, in his February 18, 1974, column, Samuelson described 5.5 to 6 per cent unemployment as not 'remotely near' full employment.

Errors in real-time estimates of the output gap became larger in the mid-1970s due to failure to incorporate the effects of the slowdown in productivity growth from 1973 (Orphanides, 2000a). Taylor (2000) argues that while this slowdown was not incorporated into published output gap estimates until 1977, and then only partially, it was recognised by practitioners and observers much earlier, so that the official series—which gave a double-digit negative gap in 1975—was not taken seriously.

The evidence suggests, however, that Samuelson, while not as erroneous in his views on the output gap as the official estimates, did seriously overstate the degree of excess capacity in the economy in the mid-1970s. For example, in his January 12, 1976, column, Samuelson wrote, 'What we need is a couple of years of 6 to 7 per cent real growth followed in the final years of the 1970s by a growth rate of about 5%.'<sup>25</sup> In

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<sup>24</sup> My figure for Samuelson's estimate is based on assuming that he set potential growth to 4% a year, and believed that 5.5% average growth was needed in the six quarters from 1971 Q3 to 1972 Q4 inclusive to deliver a zero output gap in 1972 Q4. Other interpretations of Samuelson's statement give a larger estimate of the output gap in 1971 Q2. For example, if I take Samuelson's statement that 'more than 4 per cent' potential GDP growth rate is to mean 4.25%, and his estimate of '5 to 6 per cent' required growth to mean 6%, then the implied estimate of the output gap is about  $-2.6\%$ .

<sup>25</sup> Similarly, in his May 6, 1975 column, Samuelson wrote that '[a] prudent target for annual real GNP growth... would be at least 6 per cent for some time'.

addition, in his 1975 and 1976 columns Samuelson was still giving the US potential output growth rate as 4 per cent, failing to acknowledge a post-1973 slowdown (May 6, 1975; July 28, 1975; October 18, 1976).<sup>26</sup> Together, these statements suggest Samuelson's 1976 estimate of the output gap in the US as of late 1975 was -7%, not as pessimistic as the official output gap estimate at the time,<sup>27</sup> but much more so than the current CBO estimate of a 1975 Q4 output gap of -3.6%. Moreover, as is clear from the quotations, Samuelson advocated targets for real GDP expansion based on his estimate of the gap.

By contrast, output gap measurement issues and the productivity slowdown had little effect on the analysis in Friedman's columns. This was not because of superior judgement on his part regarding the behaviour of productive potential; Friedman's columns provide no evidence of greater insight than other observers about the extent and timing of the 1973 productivity slowdown. But both Friedman's inflation forecasts and his policy recommendations were largely insulated from output gap measurement error. Because Friedman eschewed recommendations of countercyclical monetary policy, he did not advance, as Samuelson did, target paths for real GDP growth based on the estimated distance of the economy from full employment. And his inflation forecasts were informed mainly by the behaviour of prior monetary growth. This approach was vulnerable to lasting changes in velocity growth—for example, the break in the trend of M1 velocity in the early 1980s. But one advantage of Friedman's inflation projections was that they were relatively insensitive to errors in measuring the output gap. A slowdown in potential GDP growth does raise the inflation rate associated with a maintained money growth rate, and so will induce a bias in inflation forecasts based on money growth. But this error does not grow over time, whereas forecasts of inflation using the output gap have cumulating errors when a productivity slowdown is not recognised.

Friedman's and Samuelson's different approaches were reflected in the January 10, 1977 edition of *Newsweek*, a rare occasion where both economists contributed

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<sup>26</sup> Samuelson's columns of August 19, 1974 and January 1, 1979 instead presented a range for potential GDP growth of '3 to 4 per cent', which still seems high by post-1973 standards. Earlier, in a May 21, 1973 column, Samuelson gave a range of '4 to 5 per cent' for annual growth in potential.

<sup>27</sup> Estimates of the output gap in real time had the series at around -12% at the end of 1975 (Orphanides, 2000a, Figure 11). Prominent economists other than Samuelson also overestimated the output gap in the 1970s. For example, Tobin (1975) proposed a programme of 10% GDP growth in 1976 and 7% in 1977, contended that this programme was consistent with falling inflation, and stated that the growth of potential was (still) 4% per year. This implies an output gap in late 1975 of -9%, which, like Samuelson's and the real-time official series, suggested considerably more slack than today's estimates of the output gap in 1975. Tobin's denial of a change in the behaviour of potential output in the 1970s was noted by Brunner (1983, p. 50).

columns to the same issue. Each column provided recommendations for economic policy to the new administration. Samuelson endorsed ‘the 6 per cent real rate of growth [for 1977] agreed upon a reasonable target by President-elect Carter and Fed chairman Arthur Burns’, and recommended an ongoing programme of monetary expansion to bring the unemployment rate ‘to below 6 per cent’ by 1979.<sup>28</sup> Friedman argued for ‘a gradual reduction in the rate of monetary growth to a level consistent with zero inflation... That is the policy I favoured a year ago, six months ago, and shall favour six months from now.’

If the evidence from Samuelson’s columns is any indication, two main points emerge regarding outside observers’ estimates of the output gap during the 1970s. First, Taylor (2000) appears correct that some key commentators did not believe that the output gap was as negative in the mid-1970s as the official statistics suggested. Secondly, there is nevertheless support for Orphanides’ contention that outside observers in the 1970s did have estimates of the output gap based on potential GDP growth assumptions that were no longer valid, and that ‘none of these estimates was anywhere as pessimistic as the present perspective would suggest would have been appropriate’ (2000a, p. 24). Samuelson’s columns also support Orphanides’ claim that economists made policy recommendations based on these severely exaggerated estimates of the output gap. Friedman’s *Newsweek* discussions, like his other work, are notable for not making these kinds of policy prescriptions.

## 6. Conclusions

In this paper, I looked at Milton Friedman’s *Newsweek* columns on monetary policy. This examination provided support for Walters’s (1987) position that the columns were consistent with Friedman’s academic writings. I established that the columns did not claim that the money supply was the only variable relevant for nominal income fluctuations; the columns instead took an eclectic view on the issue consistent with the modern quantity theory familiar from Friedman’s scientific work. The columns also shed light on Friedman’s position that post-war monetary policy (up to the early 1980s) primarily contributed to, rather than dampened, variations in physical output. Some have interpreted this position as implying that the Fed added to overall cyclical variability by contributing exogenous policy shocks; others have interpreted it as implying that the Fed magnified the effects of non-policy shocks through an

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<sup>28</sup> Unemployment did fall to consistently below 6 per cent in the first half of 1979, by which time monetary policy had shifted to tightening in response to the behaviour of inflation, which, by the CPI annual inflation measure, had risen from around 5 per cent in late 1976 to over 11 per cent by mid-1979. See Orphanides (2000a, 2000b) for discussion of monetary policy developments in the 1970s.

inappropriate monetary policy reaction function. The *Newsweek* columns, like many of Friedman's scientific writings, are consistent with the second view, and so indicate that recent VAR evidence on the relative unimportance of monetary policy shocks does not undercut Friedman's position. And, like Friedman's other work, the columns were sceptical about the trade-offs and growth opportunities faced by the US economy—a scepticism not shared by many of Friedman's contemporaries, but now part of consensus macroeconomic opinion.

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