Federal Reserve Bank of St. Louis

REVIEW

SEPTEMBER/OCTOBER 2009 VOLUME 91, NUMBER 5, PART 2

II

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ISSN 0014-9187

II

Milton Friedman and U.K. Economic Policy: 1938-1979

Edward Nelson

Milton Friedman's publications and commentaries became the subject of enormous publicity and scrutiny in the United Kingdom. This paper analyzes the interaction of Milton Friedman and U.K. economic policy from 1938 to 1979. The period under study is separated into four subperiods: 1938-46, 1946-59, 1959-70, and 1970-79. For each of these subperiods, the author considers Friedman's observations on, and role in, key developments in U.K. monetary policy and in general U.K. economic policy. (JEL E31, E32, E51, E52)

Federal Reserve Bank of St. Louis *Review*, September/October 2009, *91*(5, Part 2), pp. 465-506.

INTRODUCTION

When invited to comment on economic developments in the United Kingdom, Milton Friedman frequently prefaced his remarks with a caveat. Thus in 1964 he testified, "I have not followed in detail the current circumstances of the British economy."[1] And much later in 2005, Friedman likewise stated, "I have no expertise on recent British experience." But it was rare for him to confine his remarks to this caveat. U.K. economic conditions were an unrelenting source of interest to Friedman, a self-described "life-long student of the monetary and economic experience" of the United Kingdom,[2] who, as we will see, was as early as 1943 citing speeches by contemporary U.K. policymakers and drawing on U.K. economic data.

In time, Friedman's influence on U.K. economic discussion would become so pervasive that he was part of the U.K. economic policy debate whether he liked it or not. The fact is that Friedman's celebrity was proportionately much greater in the United Kingdom than in the United States. The January 1977 issue of the U.K. business magazine *Management Today* referred to the "present controversy, more acute in Britain than anywhere else, over the teachings of Professor Milton Friedman"; and even in 2001, long after the peak of his fame, the London *Independent* newspaper described Friedman as "one of the few economists to have become a household name" (*Independent*, August 28, 2001).[3]

Friedman's emphasis on the effects of monetary policy, and his opposition to state intervention in the economy, guaranteed that he would be classified as a marginal figure—if not ignored outright—by U.K. academic and policy circles in the early postwar period. Friedman discovered this for himself during spells in the United

[1] From the question-and-answer portion of Friedman's March 3, 1964, testimony, in Committee on Banking and Currency (1964, p. 1144).

[2] Friedman (1980a, p. 55).

[3] A bibliographical appendix gives details for newspaper and periodical articles cited in this paper. Sources in the appendix are arranged chronologically.

The author is indebted to Charles Goodhart, David Laidler, Alvin Marty, Anna Schwartz, and Gloria Valentine for answers to many inquiries on the subject matter of this paper. For help on specific issues, he thanks Terry Arthur, Anna Burke, Elizabeth Ennion, Robert Leeson, Mervyn Lewis, and Louise North. The author also made considerable use of the services of the Federal Reserve Bank of St. Louis Research Library in the course of obtaining material for this paper; the library staff who have helped include Adrienne Brennecke, Kathy Cosgrove, Barbara Dean, Julia Seiter, Katrina Stierholz, and Anna Xiao. Luke Shimek and Faith Weller provided research assistance.

Kingdom in the early 1950s. The marginal status of Friedman and his positions persisted, with short-lived exceptional periods, well into the 1960s. But from the late 1960s and afterward, Friedman's positions, while still encountering resistance at the policymaking level, became the subject of enormous publicity and scrutiny in the United Kingdom. The control of inflation was at the center of U.K. political debate from 1968 to 1979, dominating other policy issues during that period in a way that it did not in the United States, where Vietnam, Watergate, and superpower relations competed with, and frequently superseded, inflation in prominence.

Particularly over this most intense period, Friedman made interventions himself on the U.K. scene. He provided commentary on British policy developments during U.K. visits as well as by long distance from the United States. Friedman's U.K. contributions also included some fundamental statements of his positions—most notably his lecture, "The Counter-Revolution in Monetary Theory" (Friedman, 1970a). This lecture, delivered at the University of London in September 1970, was treated by Bernanke (2004) as the most representative statement of Friedman's views on monetary matters and was what Friedman cited as the place for a list of "some fundamental propositions of monetarism."[4]

Friedman's contributions to the U.K. scene included several rebuttals to criticisms of his research findings on monetary relations. In 1970 he had stated, "I am so happily blessed with critics that I have been forced to adopt the general rule of not replying to them."[5] In light of this policy, the extent to which critics based in the United Kingdom were able to smoke him out, and provoke a direct, published rejoinder from Friedman, is impressive: Nicholas Kaldor in the 1970s, Frank Hahn and Robert Neild in the 1980s, David Hendry and Neil Ericsson in the 1990s.

The emergence of the United Kingdom as a major battleground for the debate on Friedman's views, and particularly on Friedman's version of monetarism, was amplified by the positions of the leading Keynesians in the United Kingdom. As Cobham (1984, p. 160) observes, "British Keynesianism has traditionally been more 'extreme,' more 'hardline,' than that prevalent for example in North America." In particular, in the first several postwar decades, U.K. Keynesians were more inclined than their U.S. counterparts to dismiss altogether the importance of monetary policy. The United Kingdom featured a greater and much longer-lasting "nonmonetary," or "money does not matter," brand of Keynesianism. That this viewpoint was the establishment position in U.K. economics until the 1980s is reflected in the names of those U.K. economists leading the opposition to Friedman and monetarism. Among them were an array of knights and barons: Sir Roy Harrod, Sir John Hicks, Sir Alec Cairncross, Lord Kahn, Lord Kaldor, and Lord Balogh.[6]

Because Keynesianism took a more militant form in the United Kingdom than in the United States, the U.K. debates on monetary policy were more fundamental, and their outcome produced a greater break in the direction of U.K. policymaking.

This brings me to the subject matter of this paper, which is the interaction of Friedman and U.K. economic policy over the period from 1938 to 1979. An obstacle to carrying out a study of this kind is that Friedman never published a single, definitive account encapsulating his views on U.K. developments. True, Friedman and Anna Schwartz wrote a detailed study of U.K. monetary relations, their *Monetary Trends* (Friedman and Schwartz, 1982). But while Friedman once made a shorthand reference to this book as a study of "U.K. monetary history" (*Wall Street Journal*, February 12, 1987), the volume was not, in fact, a U.K. counterpart to Friedman and Schwartz's (1963) *A Monetary History of the United States*. Rather, its focus was on the quantitative analysis of longer-term economic relations, with Friedman and Schwartz (1982, p. 605) acknowledging, "We have not made a similarly exhaustive study of United Kingdom monetary history." *Monetary Trends* does contain along the way many observations on U.K. develop-

[4] Friedman, quoted in Snowdon, Vane, and Wynarczyk (1994, p. 174).

[5] Friedman (1970b, p. 326).

[6] After 1979, as governments more sympathetic to monetarism took charge of the honors system, the tables were turned, and some of the U.K. monetarist writers received titles: Sir Alan Walters, Sir Samuel Brittan, Sir James Ball, Lord Griffiths, and so on.

ments that are relevant to the present paper and that are incorporated into my discussion. But the book is not a sufficient statistic when it comes to studying Friedman's views on U.K. economic developments; it does not contain most of Friedman's observations on the year-to-year course of U.K. economic policy. For that, one must turn to other places.

And, for a comprehensive account, this means looking in a lot of places. Friedman's remarks are widely dispersed across time and location. Not only his writings but also many interviews are relevant, as they frequently contain, in the words of Friedman and Schwartz (1982, p. 623), "illuminating side comments" on U.K. economic matters. And of those interviews Friedman gave in which the United Kingdom was the major topic, many were in U.K. newspapers that have been neither indexed nor electronically archived.

At first sight, the multiplicity of sources might not seem too troublesome: Perhaps, it could be argued, there are only a few basic Friedman references, the remainder being repetition and propagation of his key work. It is true that in the course of countless lectures, writings, and interviews, Friedman repeated himself on every dimension: on the points he made, the historical examples he cited, the analogies he drew, the anecdotes he related. Putting aside actual reprints, the repetition is most evident in the considerable number of his writings that include extended quotations from previous works. Even the largely new Friedman-Schwartz *Monetary Trends* opened its concluding chapter with a lengthy excerpt from a 1972 Friedman paper. And in his 1992 book, *Money Mischief*, Friedman only makes it through six lines of text before deploying a quotation from an earlier book of his. On one occasion, Friedman, using a stop-me-before-I-kill form of words, acknowledged this practice: "I'm sorry to quote myself all the time, but I can't help it" (*Fortune*, March 19, 1984).

Notwithstanding the heavy repetition, there is usually some marginal contribution—perhaps an added observation or an update or qualification to previous analysis—even in those works of Friedman that drew most heavily on his previous writings. In other words, while Friedman repeated himself often, he rarely repeated himself *completely*. It would, furthermore, be misguided to think that Friedman's most relevant observations on a particular subject appeared in his most prominent journal publications or in his most widely cited articles. If anything, the opposite is the case. This reflects the pattern summarized by Johnson (1974, p. 346) as Friedman's "life-long habit of scattering his new empirical results and ideas in unlikely places." Friedman's tendency to "fractionate" his written output by spreading it across an enormous variety of outlets means that, to obtain the full picture, one has to reconstitute the record from this very wide base.

I have carried out such a reconstitution for this study. The deployment of extensive source material is a principal contribution of this paper. The research here is based on an analysis of Friedman's publications, including many articles neither appearing in his book collections nor available electronically; his op-ed contributions; his published interviews in newspapers, magazines, and journals, as well as my own meetings with him; and much unpublished material. I have built a database of Friedman's public statements, based on my extensive microfilm searches, on physical inspection of hard copies of newspapers, on information from search services offered by companies and by newspapers, and on searches of newspaper and other databases that are publicly electronically archived. My search through Friedman correspondence included examination of samples from the Hoover Institution Archives' catalogued correspondence and of correspondence yet to be catalogued by the Archives,[7] and my own correspondence with Friedman from 1991 to 2006.

Also, crucially, I draw extensively on material (both correspondence and memoranda) provided to me by Anna Schwartz from her own files.[8] As

[7] I am indebted to Friedman's secretary, Gloria Valentine, for fulfilling my requests for information about as-yet-uncatalogued Friedman files before her retirement in 2007, as well as answering many inquiries from me on the subject matter of this paper.

[8] As well as for the generous access she granted me to this material, I am indebted to Anna Schwartz for answering numerous inquiries on the subject matter of this paper. This includes responses to specific inquiries I made during the course of writing this paper and, more generally, information conveyed in assorted correspondence, conversations, and meetings with me from 1991 onward.

well as (obviously) covering much of her work with Friedman on monetary policy generally, these files cover such U.K.-relevant material as Friedman's lecture to the London School of Economics in May 1952.[9] I use Congressional testimony and submissions by Friedman, including several items not included in his comprehensive published bibliographies. I also draw on transcripts of television interviews Friedman gave in the United States and the United Kingdom in the 1960s and 1970s that have been infrequently, if ever previously, cited.

The remaining discussion in this paper is divided into chronological segments. For each segment, I consider the main U.K. economic events and Friedman's interaction with them and then, particular issues in each period. Brief concluding remarks and a bibliographical appendix complete the paper.

1938-1946

Events

In 1938, Milton Friedman, then age 25, was based in New York City at the National Bureau of Economic Research (NBER), where he was working primarily on completion of his dissertation. His dissertation work came under the umbrella of what would subsequently be called "microeconomics," but Friedman also kept up with the literature on monetary policy and business cycles. It was in this connection that, as he told Brian Snowdon and Howard Vane, "I bought [*The General Theory of Employment, Interest and Money* (Keynes, 1936)] in 1938 and paid a dollar and eighty cents for it."[10]

Friedman's recollection was that he was "if anything[,] somewhat hostile" to the *General Theory* (Friedman 1972a, p. 936), and that he was influenced by the fact that among older economists there had been "a good deal of skepticism and dissatisfaction" in response to the book (Friedman, 1982a, p. 9). Moreover, of the younger economists closest to Friedman, Arthur Burns expressed reservations about the novelty of the *General Theory*, later contending that he had favored expansionary measures "as early as 1930, before Keynes' theories were known."[11]

Unlike the initial skeptics, Friedman did not deny the novelty of Keynes's theoretical contribution. The *General Theory*'s explanation for the Depression and its rationalization for fiscal expansion, Friedman would conclude, were *not* merely restatements of preexisting ideas; he would credit Keynes with a "rigorous and sophisticated analysis" (Friedman, 1968b, p. 1) that provided "a new, bold, and imaginative hypothesis" (Friedman 1972a, p. 908).

Friedman did share the concern that Keynes's book would be seen as giving the green light for a permanent increase in the size of government. To the critics, Keynes was providing a respectable theoretical rationalization for extensive government intervention, through his depiction of the income-expanding effects of government purchases and his characterization of private investment demand as destabilizing. In addition, Friedman later argued that the underemployment-equilibrium argument in the *General Theory* was "highly congenial to the opponents of the market system" (Friedman and Schwartz, 1982, p. 43). Friedman's verdict at the end of the 1970s was that the idea that "deficits…were a way of expanding the economy" led to a "tremendous growth in government spending" (May 17, 1979, testimony, in Committee on the Judiciary, 1980, p. 149).

These misgivings about the perceived policy implications of the *General Theory* reflected Friedman's free-market, small-government attitudes, already entrenched by 1938. Friedman assessed in retrospect that "I was mildly socialistic" before graduate study (*Newsweek*, June 15, 1998). But he had been converted to free-market attitudes during the portion of his graduate studies that he took at the University of Chicago[12]—

[9] Where known, I also identify, for the Friedman-Schwartz correspondence, the location of the corresponding copy of the material in the Friedman papers at the Hoover Archives.

[10] In Snowdon and Vane (1997, p. 195).

[11] Burns, October 2, 1975, testimony in Committee on the Budget (1975, p. 170). Friedman described Burns as "really my mentor" during Friedman's early career (C-SPAN, November 20, 1994).

[12] For example, Friedman (1976a, p. xxi) acknowledged, "I was influenced in this direction by my teachers at the University of Chicago."

"an excellent Department of Economics, I think the greatest in the country, even before I was there."[13]

What Friedman in 1938 called "this damn European situation"[14] led to the United Kingdom going to war in September 1939. Following the United States' entry into World War II in 1941, Friedman joined the U.S. Treasury. He later said that his Treasury colleagues and superiors saw him as a "starry-eyed theorist."[15] This being the case, it was as a Keynesian theorist, for Friedman had largely accepted the theoretical contribution of the *General Theory*. In particular, he embraced its skeptical perspective on monetary policy. Friedman acknowledged in a television interview in 1994 that "when I was at the Treasury, I was essentially a Keynesian, as I believed that the way to control inflation was by controlling government spending. I paid very little attention to money" (C-SPAN, November 20, 1994).

The Keynesian perspective is so clear in Friedman's early 1940s writings that monetarists such as Laidler (2003) have marveled at the contrast with Friedman's later work. Friedman expressed a similar sense of surprise when looking at the 1940s work from the vantage point of three decades later. "In a note on the inflationary gap that I published in 1942," Friedman said in a November 1971 talk (Friedman, 1972b, p. 183), "I never mentioned the quantity of money or monetary factors at all!"

The Keynesian position that there was a region where money and income had a very loose relationship with one another was, to Friedman, seemingly confirmed by his look at data. His (1943) paper on inflation, written while at the Treasury, plotted growth rates of nominal money and nominal income for the United States for 1899-1929; the plot led to Friedman's judgment that the relationship was "extremely unstable."[16] This judgment seems untenable. Simple inspection of the scatterplot in Friedman's paper (Friedman, 1943, p. 121) indicates that the money growth–income growth relationship is clearly positive and reasonably tight by the standards of rate-of-change data.

Friedman also embraced some of Keynes's post–*General Theory* ideas, notably those in Keynes's *How to Pay for the War* (1940). Friedman's contribution to "inflationary gap" analysis was in this tradition. This work (Friedman, 1942, 1943) revealed a close following of U.K. developments. Specifically, Friedman (1943) discussed "recent English discussion of fiscal policy [that] has centered on the so-called 'inflationary gap,'" discussed U.K. gap estimates made by British economist Frank Paish, and cited a 1943 House of Commons speech by the Chancellor of the Exchequer, Kingsley Wood, a speech known to have been drafted by Keynes (see Samuelson, 1946).

"Inflationary gap" analysis had in common with Friedman's later work the portrayal of inflation as *demand* inflation. The details of how inflation emerged, however, were different in his 1940s' analysis. Inflation in this analysis was seen as serving to equalize the nominal value of potential output and the nominal volume of aggregate spending. Potential output was assumed to have a physical ceiling, so that price change took up all the excess spending above this maximum. There was, in contrast to later Keynesian and monetarist work, no allowance for "overfull employment." Reflecting his later use of the overfull employment concept, Friedman would say in 1972, "I think people are wrong in supposing that there is a rigid ceiling on output such that further increases in real output are impossible…It is possible to have overemployment as well as underemployment."[17]

[13] *Milton Friedman Speaks*, Episode 8, p. 30 of transcript. *Milton Friedman Speaks* was the name given to a series of Friedman talks in the United States videotaped over 1977-78 and used to promote interest in a projected television series for Friedman; see Friedman and Friedman (1998, pp. 477-78, 604). The series was released on a limited basis on videotape, accompanied by official transcripts, in 1980, and was more recently repackaged as a commercially available DVD set. References made in this paper are generally to the transcripts, but all the quotations from the transcripts also appear on the DVD releases.

[14] March 17, 1938, letter from Milton Friedman to Rose Friedman, quoted in Friedman and Friedman (1998, p. 77).

[15] For example, in *Newsweek*, July 24, 1978.

[16] Friedman (1943, p. 119). Friedman contrasted this with what he called the "considerably more regular" empirical relation between consumption and income, which supported Keynesian theory (1943, p. 120).

[17] Friedman (1973a, p. 35).

In postwar work, Friedman and others would accordingly distinguish carefully between potential and maximum output.[18] In particular, the notion that output could *temporarily* exceed potential, and unemployment fall below its natural rate, was a contribution of Friedman's natural rate hypothesis (e.g., 1968b, 1977a). Nevertheless, the concepts of positive output gaps, and associated overfull employment, were innovations neither of Friedman nor of the Phillips curve literature; they were in place earlier than the 1950s and 1960s. The possibility that overfull employment could occur was specifically embodied in the U.K. policymaking framework by the late 1940s.

"During World War II," Friedman later recalled, "governments everywhere had largely assumed control of the economy. And it was simply almost taken for granted that they would have to continue to do so in the postwar period."[19] The Attlee government was elected in the U.K. general election of July 1945. Friedman noted, "In Britain, the Labour Party's postwar victory over Winston Churchill spelled a commitment to central planning" (*Newsweek*, July 14, 1975).

Issues

Nationalization and Central Planning. Friedman observed that the postwar shift to greater government economic control had been justified on efficiency grounds: It was believed "that centralized and comprehensive economic planning and control by government is an essential requisite for economic development" (Friedman, 1958a, p. 505). He noted that, in particular, nationalization of industries was motivated by this consideration (*The Listener*, April 27, 1978). The Attlee government used the efficiency argument when implementing a broad nationalization program after it came to power.

This nationalization program was believed to be appealing to U.K. electors, to judge by the notice of their plans that leading Labour politicians gave in the months approaching the election. Stafford Cripps, later Chancellor of the Exchequer, said, "We must replace the libertinism of private enterprise by a planned system of economy which calls for a considerable measure of state control and ownership." As did many in the West, Cripps cited the Soviet Union as a successful economic model: "In Russia you have a State-planned and controlled industry, and I cite this as an example to show that some form of centralized planning and control helps and does not retard efficient production and full employment" (*News-Chronicle*, December 18, 1944).

The nationalizations undertaken by the Attlee government (1945-51) encompassed mining, communications, the railway system, and steel.[20] Friedman had anticipated that a still more comprehensive nationalization scheme would be carried out. He observed in 1972 of the late 1940s, "If you had asked us then about the health of capitalism and free enterprise 25 years later, I think we would have said it would be closer to its deathbed than it actually is now."[21]

In fact, Friedman's *Capitalism and Freedom* (1962a) contains remarks to the effect that he thought that in the United Kingdom the move toward greater government intervention had peaked even before the Attlee government left office. In particular, Friedman took comfort from the fact that central planning, as opposed to nationalization, had not endured in Western economies beyond the 1940s. The detailed direction of resources, public and private, had been foreshadowed by the Attlee government; as Friedman noted, "immediately after World War II, it was thought that the government was going to get involved, especially in Britain…in central economic planning on a large scale."[22] Efforts to replace the price system with government direction of allocation decisions had, he argued, faltered and led to socialism peaking in the United Kingdom in 1948 (*Vision*, April 1972). Friedman

[18] Friedman would still believe that there was a physical ceiling on output (see the expositions of his plucking model in Friedman 1964b, 1993), but he no longer treated this ceiling as synonymous with the natural level of output.

[19] Quoted in Levy (1992).

[20] See Childs (2006, p. 14) for a tabulation of the Attlee government's nationalizations.

[21] Friedman speaking in *Business and Society Review*, Spring 1972; reprinted in Friedman (1975a, p. 253).

[22] *Free to Choose*, PBS debate 1980, Episode 3, p. 9 of transcript.

(1962a, p. 11) singled out the fate of the Attlee government's Control of Engagements Order, which, he said, would have meant a directed-labor economy if it had been enforced. The Order was not, in fact, enforced heavily and was then repealed, an event Friedman identified as a "turning point" (Friedman, 1962a, p. 11) when "central planning came to a screeching halt" in the United Kingdom.[23]

But Friedman further revised his opinion in the 1970s: He observed in 1972, "I was much more optimistic in 1962 than was justified by what happened later."[24] Friedman continued to acknowledge that the momentum for planning and nationalization had stalled, noting that "[t]here is less central planning in Britain now than in 1946,"[25] but he now judged that this had "diverted…growth [in government] to a different channel" (*New York Times*, August 13, 1994). Greater government influence on resource allocation, he argued, had instead been achieved via expansion of government spending (including transfer programs) and of regulation (Friedman, 1976c; Friedman and Friedman, 1998, p. 582). This changed perception was reflected in Friedman's descriptions of the U.K. system: Whereas he characterized what was launched in the United Kingdom in the 1940s as "a policy of welfare statism and central planning" (*Saturday Evening Post*, May/June 1977), Friedman argued that the system evolved into "a socialist and welfare state" (*National Review*, December 31, 1997).

Cheap Money. Many countries followed "cheap money" policies in World War II and its aftermath; the U.S. case is the subject of Friedman and Schwartz (1963, Chaps. 10 and 11). In the United Kingdom, the postwar "cheap money" policy is associated with the attempt by Chancellor of the Exchequer Hugh Dalton to break with the practice his adviser, John Maynard Keynes, had described as "the unwillingness of most monetary authorities to deal boldly in debts of long term" (Keynes, 1936, p. 207). Although announced by the Attlee government upon its election, the long-term bond program began in earnest in October 1946, several months after Keynes's death. Among the new government bonds created in 1946 was a series of 2.5 percent "irredeemable" securities, that is, securities that might be held indefinitely as a source of interest income; these new long-term securities were unofficially known as "Daltons" or "Dalton consols." By issuing very-low-yield medium- and long-term securities, Dalton attempted to extend the government's existing low interest rate policy to the entire term structure. "We have been gradually conditioning the capital market to a long-term rate of 2½%," Dalton observed. "...I am sure that our cheap money policy should continue to be resolutely pressed home" (*Financial Times*, October 17, 1946).

The U.K. and U.S. authorities' interest in influencing long-term rates rested heavily on the Keynesian position that long-term rates mattered for aggregate demand much more than short rates; this interest was qualified by the consideration that, as Friedman and Schwartz (1963, p. 700) observe, even the sensitivity of demand to long-term rates was not thought to be substantial. Low long-term interest rates were also perceived as contributing to the flexibility of fiscal policy by easing the financing of the national debt. So the extension of the cheap money policy to the long-term market had both Keynesian and debt-management motivations.

The Dalton program of October 1946, while involving the creation of new debt instruments, was intended to drive existing longer-term securities' rates down to 2.5 percent too (see, e.g., Hallowell, 1950, p. 41); this contrasted with the rates between 3 percent and 3.5percent prevailing for most of the period since 1932 (Hallowell, 1950, p. 23; Robertson, 1949, p. 22).[26] Since Bank Rate was left unchanged, the experiment was not making use of the expectations theory of the term structure. On the contrary, the expectations theory

[23] In *Business and Society Review*, Spring 1972; reprinted in Friedman (1975a, p. 254).

[24] Friedman, October 20, 1972, remarks, in Selden (1975, p. 51).

[25] From *Business and Society Review*, Spring 1972, reprinted in Friedman (1975a, p. 254).

[26] When serving as a discussant of Friedman's in 1970, Sir Roy Harrod paraphrased Dalton's rationale for the reduction in the long-term rate as that if "we could run a great war at 3%, we ought to be able to run the peace at 2.5%" (Harrod, 1971, p. 59).

would suggest that keeping the short rate unchanged tended to work against the success of a policy to reduce long-term rates. From the perspective of the *General Theory*, however, the approach made some sense: The *General Theory* saw securities as becoming equivalents of money before their yield became zero; insofar as short-term interest rates were perceived as already having hit their floor, but long-term rates had not reached their floor, the monetary authorities could carry out operations directly in long-term securities markets to encourage reductions in longer-term rates.

Wilson (1984, p. 76) observes that there were "few British economists in the 1950s and 1960s who advocated control of the money supply—Robertson, Robbins, Paish, Dacey, myself [Thomas Wilson], and one or two others." Among those on this list who were active in the 1940s, Robertson was perhaps the leader and is acknowledged in Friedman and Friedman (1998, p. 247) as an early distinguished skeptic regarding Keynesian economics. That skepticism is evident in Robertson's discussions of the Dalton monetary policy, as Robertson (1949, p. 22) counts himself among those "who dared to question the wisdom of this [1946] further turn of the [cheap money] screw." A fellow critic, Dacey (1947, p. 59) wrote that "it is surprising that Mr. Dalton should have thought it good statesmanship to press rates down further at a time when inflationary forces are only kept in check with the assistance of a formidable administrative apparatus [i.e., price controls]."

Friedman's own later work contained critical observations on the U.K.'s monetary framework during the 1940s. There are many further criticisms implicit in Friedman's descriptions of what monetary policy can and should do. Friedman's framework centered, first and foremost, on the point that "monetary policy is not about interest rates; monetary policy is about the rate of growth of the quantity of money."[27] The Fisher relation provided the only enduring channel by which the central bank could affect interest rates, be they short- or long-term. Monetary policy could exert other, more transitory influences on interest rates, but Friedman was skeptical that these influences justified central banks' claims that they could control long-term interest rates (see, e.g., Friedman and Schwartz, 1963, p. 514). Certainly, he believed that a base money injection could produce some temporary downward pressure on long-term rates, both via the standard expectations channel (i.e., via the liquidity effect on current and expected future short rates) and via a portfolio effect on the long-short spread or term premium (see, e.g., his June 1966 memorandum to the Board of Governors, published in Friedman, 1968a, p. 156). But for the central bank to exploit these effects in a way that made the long-term interest rate a policy instrument would require being able to overwhelm the "nonmonetary forces affecting interest rates"[28] as well as the Fisher effect, which showed up in "long-term interest rates much sooner"[29] than in short-term rates and worked in the opposite direction of the liquidity and portfolio effects of the money injection. Moreover, Friedman noted, the sustainability of this policy was doubtful, since for a central bank to "peg a particular interest rate…it must accept whatever happens to other magnitudes affected by the [monetary] base, including the level of inflation" (*Wall Street Journal*, April 5, 1990).

The Dalton attempt to bring down long-term rates had the temporary appearance of success. Rates on existing long-term securities fell toward the new 2.5 percent baseline. For example, Friedman and Schwartz's (1982, Table 4.9, p. 133) data on "old" consols (i.e., the perpetual-horizon security already being traded before the release of the new, "Dalton" consols) show an average rate of 2.92 percent in 1945 and 2.6 percent in 1946. But the effort to hold down long-term interest rates did prove unsustainable, and in the course of 1947 long-term rates rebounded; Friedman and Schwartz's series shows an average for 1947 of 2.76 percent, rising to 3.21 percent in 1948. Friedman (1970a, p. 8) observed, "Chancellor Dalton tried to follow the Keynesian policy of keeping interest rates very low. As you all know,

[27] From Friedman's appearance on *Meet the Press*, October 24, 1976.

[28] From Friedman's October 1965 memorandum to the Board of Governors, published in Friedman (1968a, p. 136).

[29] Friedman (1983, p. 11).

he was unable to do so and had to give up." A few months after Dalton left office, the *Financial Times* reported, "The attempt to hold the rate of interest on government long-term borrowing at 2½% has now been officially abandoned" (*Financial Times*, January 3, 1948).

The pressure on the cheap money policy in the United Kingdom was more acute than in the United States because the associated pressure on aggregate demand was one-sided. An interest rate peg can in principle be contractionary in effect, as when the central bank has to withdraw base money to enforce the peg. Friedman and Schwartz (1963, p. 596) find that the U.S. cheap money policy indeed produced deflationary pressure over 1948, and Friedman and Schwartz (1982, p. 76) classify 1948-49 as a business contraction in the United States. No corresponding contractionary episode is evident during the U.K. postwar cheap money period, and Friedman and Schwartz (1982, p. 76) classify 1946-59 as a continuous expansion in the United Kingdom. The U.K. experience consequently corresponded more literally than did the U.S. case to Friedman's summary statement that "the stock of money rose as a result of the cheap-money policies and so did prices, either openly or in whatever disguise."[30]

The Dalton policy on long-term interest rates was the first element of the U.K. monetary framework to break under this one-sided pressure; the exchange rate policy was next, with sterling devaluation taking place in 1949. The outbreak of the Korean War in 1950 magnified the pressure on the remaining component of the cheap money policy, namely, the holding of Bank Rate at 2 percent. The *Financial Times* noted (February 10, 1951): "Both the British and American governments seem determined not to use higher interest rates to combat inflation." This determination contrasted with Friedman's position, which he articulated in 1952 as follows: "The purpose of monetary policy is to maintain price stability, and on some occasions this will call for actions that tend to raise interest rates."[31] Bank Rate was finally raised by the newly elected Churchill government in November 1951. "No country succeeded in stemming inflation without adopting measures that made it possible to restrain the growth in the stock of money," Friedman observed at the end of the 1950s. "And every country that did hold down the growth in the stock of money succeeded in checking the price rise."[32]

1946-1959

Events

Friedman's first visit to the United Kingdom in 1948, consisting of "two or three days in England, in London,"[33] left him convinced that it was being "economically strangled by the law obedience of her citizens" (Friedman, 1962b). Friedman was persuaded by the argument made by George Stigler, with whom he made the trip, that price controls were distorting the United Kingdom to an extent that they were not in France, because of the more-extensive French underground economy.[34] Price controls had been introduced by the United Kingdom in wartime, Friedman later observed, in an "attempt to suppress the inflation arising from wartime spending, financed largely by increasing the money supply" (*Newsweek*, November 27, 1972). The Attlee government continued the controls into peacetime. Friedman opposed price controls both as a wartime and a peacetime measure (see his October 6, 1969, testimony in Joint Economic Committee, 1970, pp. 815-16) The peacetime controls in the United Kingdom and on the Continent were, he argued in an early intervention, impeding Europe's economic recovery (*New York Times*, January 11, 1948).

Other damaging restrictions, Friedman contended, came in the "foreign exchange controls that strangled Western Europe after the war."[35]

[30] May 25, 1959, testimony, in Joint Economic Committee (1959a, p. 607; p. 138 of 1964 reprint).

[31] March 25, 1952, testimony, in Joint Committee on the Economic Report (1952, p. 736).

[32] May 25, 1959, testimony, in Joint Economic Committee (1959a, p. 607; p. 138 of 1964 reprint).

[33] *Milton Friedman Speaks*, Episode 1, p. 9 of transcript.

[34] Friedman (1962b); and *Milton Friedman Speaks*, Episode 1, p. 9 of transcript.

[35] Friedman (1964a); p. 78 of 1969 reprint.

Foreign exchange controls were not initially emphasized by Friedman in the 1940s, when his focus was on price control. "I wrote my first article [on exchange controls] in 1950," Friedman later recalled, "when I was in France in connection with the Common Market arrangements" (*Jerusalem Post*, November 6, 1987). For the United Kingdom, Friedman's position, maintained from the early 1950s, was that exchange controls had such a depressing effect on the level of the pound sterling that if the U.K. authorities floated the pound and maintained exchange controls, the pound would tend to depreciate, whereas if they floated simultaneously with the removal of exchange controls, the pound would appreciate (Friedman, 1953b; Friedman and Schwartz, 1982a, pp. 290-94). This conjecture was borne out when the pound appreciated after the abolition of exchange controls in 1979. Friedman criticized the exchange controls on economic grounds, but "entirely aside" from their economic aspects, he opposed them on grounds of "human freedom."[36] It violated the "free market in ideas," he said in 1977, "if a country, as Great Britain did immediately after the war, has exchange controls under which no citizen of Britain may buy a foreign book unless he got authorization from the Bank of England."[37]

Even by the early 1950s, the United Kingdom had acquired a basket-case image in the United States for its postwar performance, with extensive economic aid by the United States to the United Kingdom highlighting the problem. "When people say, 'Well, American aid bailed Germany out,' I add that American aid also bailed Britain out," Friedman later observed. "The amount we gave to Britain in the British-American loan was far greater than anything Germany got" (*Saturday Evening Post*, May/June 1977).

The United Kingdom was also a recipient of aid from the United States via the Marshall Plan. Friedman contended that "Europe would have recovered with or without the Marshall Plan," and opposed the Plan at the time and in retrospect (Friedman, 1982a, pp. 32-33). He argued that the "Marshall Plan and similar programs" of the U.S. government had "been harmful to the rest of the world"[38] because government-to-government economic aid strengthened the government sector at the expense of the private sector.[39] It was, nevertheless, as an adviser to the Marshall Plan that Friedman made a second visit to Europe, in late 1950, basing himself in Paris (Friedman, 1992, p. 248; Friedman and Friedman, 1998, Chap. 12).

"Plans to spend a quarter at the London School [of Economics] in the spring of 1952 fell through," Friedman recalled in 1994, "but did lead to my making a three-week trip to Britain and France, giving two lectures at the London School of Economics."[40] Friedman's talk, "Classical Counter-Revolution and Monetary Theory and Policy," at the London School of Economics in May 1952, opened with a major gaffe—or a clanger, to use the U.K. nomenclature. His speaking notes state, "With some hesitancy the American speaks on this topic to an English audience. Basic contributions all English. Classical—Hume, Ricardo, Thornton, Marshall…"[41] The problem, of course, was that David Hume was Scottish, not English. Indeed, the two British economists with whom Friedman identified most and whom he would most often quote in his writings, Hume and Adam Smith, were Scottish. To judge by Friedman's later statements, he learned his lesson and became more careful about distinguishing the Scottish from the English. In arguing against the Bretton Woods system during Congressional testimony in 1963, Friedman noted that what mattered to the U.K. consumer was a good's U.K. price, not the same price expressed in U.S. dollars; different dollar values of the price were "all the same to an Englishman—or even a Scotsman."[42]

Around the time of Friedman's 1952 visit, the U.K. unemployment rate averaged 2.1 percent;

[36] *Free to Choose* BBC2 debate, March 22, 1980, p. 21 of transcript.

[37] *Milton Friedman Speaks*, Episode 3, p. 16 of transcript.

[38] September 23, 1971, testimony, in Joint Economic Committee (1971, p. 722).

[39] For Friedman's elaborations of this argument, see Friedman (1958a) and *Newsweek*, December 21, 1970.

[40] From Friedman's notes "1952 MF trip," in a 1994 letter to Anna Schwartz; courtesy Anna Schwartz.

[41] May 1952 lecture notes by Friedman; courtesy Anna Schwartz.

[42] November 14, 1963, testimony, in Joint Economic Committee (1963, p. 454).

in 1951 it was only 1.2 percent, and in 1953 the average was 1.8 percent.[43] These were lower rates than prevailing in the United States, but Friedman later cautioned against interpreting such low rates as a badge of honor. In his Nobel lecture, Friedman cited the United Kingdom in the 1950s as an example of a country with an inefficiently low unemployment rate, reflecting the fact that a "highly static rigid economy may have a fixed place for everyone" (Friedman, 1977a, p. 459). Friedman elaborated in 2004: "Progress depends—it sounds funny, but it's true—on unemployment… Because that's the only way you can provide the necessary labor force for the new development, the new industries that are coming along" (*Investor's Business Daily*, April 15, 2004).

The U.K. economy in the 1950s not only featured an inefficiently low unemployment rate, but also a tendency for aggregate demand to be expanded too rapidly, forcing unemployment temporarily below its low natural value and creating inflationary pressure. Since U.K. policymakers were aware by the late 1940s of the distinction between full and overfull employment, the question arises why they kept overdoing expansion. Some of the overheating in the late 1940s and the 1950s might be attributed to preemptive stimulus in anticipation of a collapse in private demand. Friedman (1973a, p. 5) noted that while "a great post-war depression…was widely predicted," it "kept being expected but it never occurred." This observation was true of the United Kingdom, with Chancellor Dalton stating in 1945 that the "government must arm itself with anti-slump powers, so that never again, as in past years, shall prices, productivity, and employment all fall away through the failure of private enterprise" (*Financial Times*, November 23, 1945). In particular, "secular stagnation," due to drying up of private investment opportunities and to excessive consumer saving, was feared in the 1940s and cited as a reason for the government stepping in with its own demand for output. Friedman was an early critic of the secular stagnation thesis (Friedman, 1948, p. 262), and the criticism had become widespread by the 1950s as the prospect of a consumption collapse dwindled. In fact, the secular stagnation theory was one aspect of Keynes's thinking that was widely rejected in the United Kingdom even in the Keynesian 1950s; the *Financial Times*, for example, referred to "Keynes' one-sided fear of over-saving" (May 23, 1955) and to "Keynes' incredibly shortsighted forecast of declining investment opportunities" (October 15, 1956). But precisely because the relevance of the secular stagnation hypothesis was in so much doubt by the 1950s, it is hard to cite belief in it as the reason for continued U.K. overexpansion in that decade.

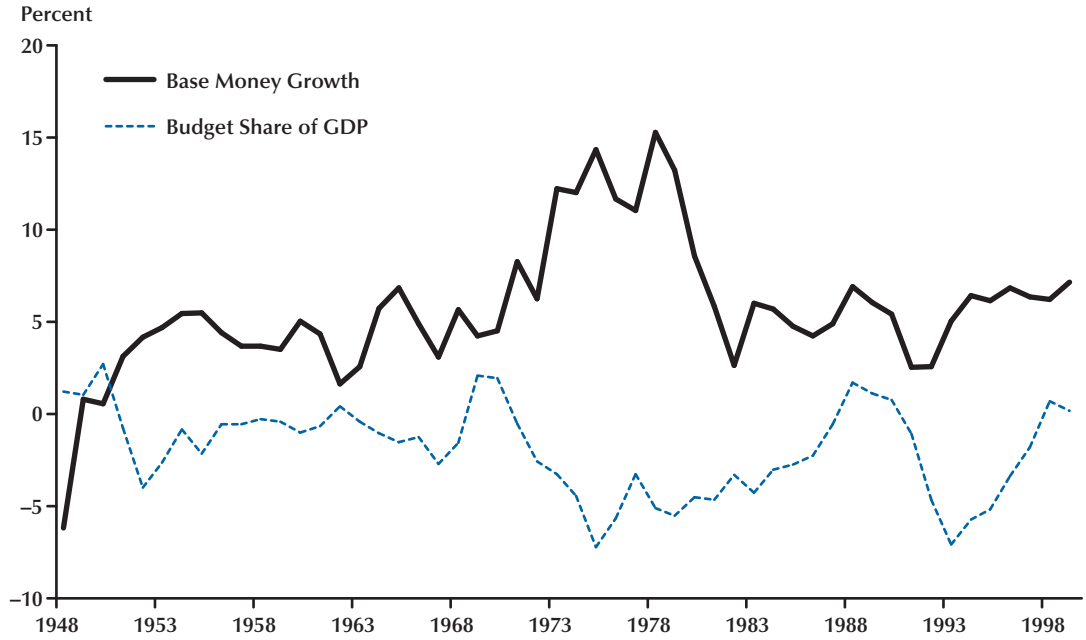
The repeated failures over the 1950s to deliver the proper dosage of demand seem most attributable to the U.K. government's misguided view of how to affect demand. Here, fiscal policy received pride of place, reflected in Chancellor of the Exchequer Peter Thorneycroft's observation in 1957, "The big instrument of government policy in all these matters is the budget" (*Daily Express*, July 13, 1957). This contrasts with Friedman's position on fiscal policy which, of course, was this: "In my opinion, a budget deficit is 'expansionary' only if it is financed by printing money" (*Newsweek*, February 15, 1971). According to this interpretation, any apparent connection between fiscal actions and aggregate demand was not an indication of the working of the Keynesian multiplier process, but was instead a by-product (a "disguised reflection" in the terminology of Friedman and Meiselman, 1963) of the fact that deficits in practice were monetized.

Figure 1 plots the ratio of the U.K. budget balance to nominal gross domestic product (GDP), and the growth rate of the U.K. monetary base. The budgetary series is one available for 1948-99, now discontinued but formerly reported as line 80 in hard copies of the International Monetary Fund (IMF)'s *International Financial Statistics* (IFS). Nominal GDP is the annual average series for the United Kingdom from Haver-IFS (downloaded March 2009). The monetary base series is the annual average of a series obtained by splicing annual averages of the Capie-Webber series (1985) into the Bank of England break-adjusted base money series. The plotted growth rate of the base closely resembles that depicted in Benati (2005, Chart 1), which was based on similar

[43] London and Cambridge Economic Service (1963, Table F, p. 11).

Figure 1

U.K. Budget-Balance Share of GDP versus Monetary Base Growth (1948-99, annual data)



sources; in addition, for 1948-71, the growth rates resemble those of the high-powered money series in Friedman and Schwartz (1982, pp. 125-26). Base growth is highly correlated with the budget-balance share for 1948-79 (correlation = –0.80), while the correlation for 1980-99 is negligible (correlation = 0.08). The association between base growth and deficits supports Friedman's contention that in the early postwar decades U.K. government deficits were monetized,[44] so that the period simply does not provide clean evidence of the effects of "pure" fiscal policy. Attempts to quantify a multiplier impact of deficits and surpluses without attempting to hold constant the reaction of the monetary authorities merely beg the question.

It may seem perplexing that my discussion of demand policies in the United Kingdom in the 1950s has been able to proceed so far without a discussion of the fact that the pound sterling was on a fixed exchange rate. I have occasionally seen it argued that the U.K. authorities did not actually pursue Keynesian policies before the 1970s because they were constrained by their Bretton Woods obligations.[45] This argument overlooks the extent to which foreign exchange controls reduced the impact of the exchange rate constraint on the formation of demand management policy. As Friedman and Schwartz (1963, p. 105) observe in discussing the postwar United Kingdom, "the development of direct exchange and trade controls gave it means of affecting its balance of payments other than through movements in prices and incomes"; relatedly, exchange controls gave the U.K. authorities some room to separate fixing the exchange rate from setting interest rates. There was no occasion in the 1950s when there was a Bank Rate increase that could not be justified by domestic conditions; reflecting this, the Bank of

[44] Friedman (1975c, pp. 72-73) argues that fiscal deficits were stimulative in Western countries in the postwar period (up to that point) because they were, in practice, monetized.

[45] This argument was used, for example, by R.J. Ball in the *Financial Times*, February 4, 1981.

England governor testified in 1958 that foreign exchange market considerations had determined the timing (in terms of the specific week) of Bank Rate moves, but not the actual moves themselves, which were invariably also shaped by internal considerations.[46] Bretton Woods did not override what the *Financial Times* (December 21, 1953) called the "modern principle of shaping policy by reference to domestic monetary needs." The coexistence of substantial monetary policy independence and a fixed exchange rate explains why, during the 1950s and 1960s, Lionel Robbins simultaneously criticized the idea of floating rates and advocated that the U.K. monetary authorities manipulate the monetary base to achieve price stability. As Friedman (1978) later pointed out, a bona fide conflict between the exchange rate and domestic considerations in the Bretton Woods system led typically to the exchange rate giving way, as it did in the United Kingdom in 1949, 1967, and 1972.

By the time of Friedman's 1953-54 spell in the United Kingdom, the development of Friedman's monetarism was well advanced, to the point where, in reprinting his (1942) essay on inflation, he added material on money, attributing its previous absence to the "prevailing Keynesian temper" of the 1940s (Friedman, 1953a, p. 253). Friedman also had had a letter published in *The Economist* (January 3, 1953) advocating that sterling be floated.

A review of Friedman's *Essays in Positive Economics* (1953a) appeared in the *Financial Times* of February 8, 1954, apparently the first-ever mention of Friedman in that newspaper. The review was devoted mostly to Friedman's argument for floating exchange rates (Friedman, 1953b). The review said that Friedman "grossly overstates his case…when claiming that flexible exchanges would have obviated the sterling crises of 1947, 1949, and 1951." The review apparently regarded Friedman as neglecting the possibility that devaluation could worsen the current account balance measured in pounds. This was one of several critiques of Friedman's argument for floating exchange rates that took Friedman as implying that a float removes current account deficits in the balance of payments. It is true that Friedman generally regarded depreciations as good for the sterling trade balance and that a low-enough sterling exchange rate would remove the trade deficit; he said so in his 1953 letter to *The Economist*, for example. But, to my knowledge, Friedman did not claim that a floating exchange rate would converge to the value consistent with a zero trade or current account balance; his claim for floats was the correct and general one that they eliminate the possibility of balance of payments deficits or surpluses, so that "[b]alance of payments problems in the technical sense are a reflection of price fixing."[47]

Issues

The Early Monetarist. Two beliefs are widespread about Friedman's origins as a monetarist. The first belief is that his earliest monetarist work appeared in 1956.[48] The second is that, in the 1956 paper and elsewhere, Friedman merely dotted the *i*'s and crossed the *t*'s of existing work by Keynes and of pre-1956 Keynesian work, so that the theoretical innovations of monetarism were negligible (at least if contributions regarding the expectational Phillips curve are put to one side). Both beliefs are misconceptions. They are naturally handled together since the non-Keynesian aspects of Friedman's framework are not all present in his 1956 paper, but are evident if the totality of his work over 1948-58 is considered. The discussion below shows that the literature's characterization of the 1956 paper as the launching pad for Friedman's monetarism has obscured some of the major theoretical differences with Keynesianism that were already visible in other work by Friedman in the 1950s. It complements the cataloguing by Friedman (1972a) and Meltzer (1977) of distinctions between Friedman's framework and Keynesianism but includes items not in their lists. It also serves to

[46] See the answers by Bank of England Governor Cobbold, in Radcliffe Committee (1960, pp. 137, 155).

[47] September 23, 1971, testimony, in Joint Economic Committee (1971, p. 701).

[48] See, for example, Eshag (1983).

confirm Friedman's observation (in his *Reason* interview of June 1995) that the key arguments he made in his 1960s' and 1970s' publications were already present in his 1950s' work.

First, Friedman argued as early as 1948 for a focus on monetary policy for the control of inflation. A letter he and other Chicago faculty members wrote to the *New York Times* in early 1948 was entitled "Control of Prices: Regulation of Money Supply to Halt Inflation Advocated" (January 11, 1948). In claiming that "a marked increase of the general level of prices unaccompanied by a marked increase in the supply of money is a rare if not nonexistent phenomenon," this letter reflected early dissent by Friedman from Keynesianism[49] and was followed by Friedman's (1950, p. 474) sympathetic remarks about the quantity theory. In 1952, Friedman was firmly associated with the quantity theory position, and Friedman published his finding that income and price changes in U.S. wartime episodes were "more readily explicable by the quantity theory than by the income-expenditure theory" (Friedman, 1952, p. 721).[50]

Second, Friedman (1951) argued for treating prices and wages as endogenous variables at all levels of employment, in contrast to the Keynesian treatment (and Friedman's in 1942-43) of prices as insensitive to aggregate demand until full employment was reached.

Third, Friedman advocated floating exchange rates from 1950, when his (1953b) essay on the subject was drafted, using arguments that rested on the ability of monetary policy to deliver price stability.

Fourth, in the 1950s Friedman rejected cost-push factors as a source of sustained inflationary pressure. While Friedman (1948) had given credence to cost-push factors as one factor driving up wages, in 1951 he said, "My views about this have changed considerably in the last few years" (Friedman, 1951, p. 228). In 1952 Friedman testified, "I think the so-called wage-price spiral has been enormously exaggerated, that what we have had has been inflationary pressure pulling both wages and prices up."[51] His rejection of cost-push is clear also in his repudiation in Friedman (1953b) of the idea that exchange rate depreciation could trigger a self-sustaining wage/price spiral. Friedman's position from the early 1950s was that cost-push factors had a zero mean in themselves; upward pressure on wages or prices in one sector would be "balanced by declines elsewhere in other prices and costs."[52] Any tendency for inflation to exhibit a sustained rise in the face of a positive wage or price shock reflected monetary accommodation, so cost-push factors could not alter inflationary expectations in the absence of a monetary expansion. This rejection of cost-push distinguished Friedman not only from Keynesians (among whom the popularity of cost-push explanations *increased* over the 1950s, in both the United States and the United Kingdom), but also from some advocates of monetary control such as Robbins. In contrast to Friedman, Robbins believed that wage-push factors put a positive bias into U.K. wage inflation, in the face of which monetary policy needed to be contractionary (rather than simply nonaccommodating) to deliver price stability.[53]

Fifth, Friedman rejected the notion of a long-run trade-off between unemployment and inflation. In a 1950 symposium (Wright, 1951, p. 243), Friedman said "I don't know what you mean by saying unemployment will police inflation." In 1952 Congressional testimony he said, "Rather

[49] Brunner and Meltzer (1993) date Friedman's earliest dissent to Friedman (1944).

[50] Friedman had also confronted Roy Harrod with his views on velocity in a meeting in Chicago in early 1951 (Harrod, 1971, p. 58).

[51] March 25, 1952, testimony, in Joint Committee on the Economic Report (1952, p. 727); see also Friedman (1952, fn. 7).

[52] March 25, 1952, testimony, in Joint Committee on the Economic Report (1952, p. 736).

[53] Note that the view that unions can be an autonomous source of wage-push is distinct from the view that unions can raise the natural rate of unemployment, since wage- or cost-push refers to inflationary pressure created for a given difference between unemployment and the natural rate. As for whether union pressure could affect the natural rate, Friedman regarded the conditions for this to occur as restrictive—he argued that unions could raise unemployment in certain sectors, but not necessarily in the aggregate (Friedman, 1951; *The Times*, August 29, 1973; *The Economist*, September 28, 1974)—but he sometimes implied that the conditions for an effect on aggregate unemployment might have been satisfied in the postwar United Kingdom (e.g., Friedman 1963a; Friedman and Friedman, 1980).

than regarding the objectives of high employment and of price stability as inconsistent, I think that fundamentally price stability will promote a high level of output by avoiding a good many of the interruptions to output that we have had in the past, by giving people stable expectations, and so on."[54]

Sixth, the preceding two points combined with his doubts about fiscal policy meant that Friedman believed that monetary policy was *sufficient* to control inflation. The "sufficient" language was used in Friedman (1958b), and contrasts Friedman's position directly with Keynes's view that monetary policy actions could not be sufficient for delivering price stability (see Nelson and Schwartz, 2008).

Turning now to contributions present in Friedman (1956), a seventh 1950s' contribution by Friedman was to specify money demand as dependent on a vector of interest rates. This means that the monetary policy transmission mechanism cannot be summarized by a single interest rate. Patinkin (1969) claims that Friedman's specifying money demand as dependent on interest rates makes his specification Keynesian. This overlooks the fact that Friedman does not condense the nonmoney assets into a single asset, as Keynesian analysis typically did. Moreover, pre-Keynes writers had made money demand interest elastic, and the specifics of Friedman's money demand approach differ from those of Keynes. Keynes had broken money demand into transactions and speculative components, with only the second component interest-elastic and otherwise "idle." Friedman (1956) rejects the concept of idle money and instead models every unit of money as interest-elastic (possibly relative to own-rates), and held for all motives at the same time (an aspect of Friedman's analysis acknowledged by Patinkin, 1965, p. 75).

Eighth, Friedman (1956) indicates that his conception of money demand rules out the liquidity trap, which he argued in Friedman (1972a) was central to the *General Theory*. In light of the discussions of the liquidity trap in recent years by Paul Krugman and Lars Svensson,[55] and their attribution of liquidity-trap views to Keynes, it may come as a surprise that Patinkin (1972a) and many other Keynesians objected to Friedman's association of the *General Theory* with the liquidity trap. But it was hardly an off-the-wall interpretation on Friedman's part. James Schlesinger, by no means a close ally of Friedman, argued strongly that the liquidity-trap thesis was central to the *General Theory* in his 20-year retrospective on the book. Schlesinger (1956), Friedman (1972a), and Beenstock (1980) all provide their own, apparently independently constructed, lists of quotations from Keynes (1936) supporting this interpretation, and even Patinkin (1976a) acknowledged that passages of the *General Theory* treat the liquidity trap as empirically relevant. As Friedman (1972a, p. 942) put it, again and again, Keynes's "final line of defense is absolute liquidity preference."

Over 1948-58, all the elements of Friedman's monetarism fell into place and are recognizable as the positions he took in what he termed the "dispute in the 1950s or early 1960s" in the United States[56] and in the subsequent debate around the world from the late 1960s. This crystallization of Friedman's framework was occurring when the dominant thinking on monetary policy in the United Kingdom was converging toward an almost completely different framework.

THE ROAD TO RADCLIFFE

Friedman (1968c, p. 439) noted, "Experience with monetary policy after World War II very quickly produced a renewed interest in money and a renewed belief that money matters." But later, viewing the 1950s and 1960s as a whole, Friedman (1987, p. 13) concluded that the experience of the period "strongly reinforced" the Keynesian critique of monetary policy, and Friedman and Schwartz (1982, p. 17) argue that

[54] March 25, 1952, testimony, in Joint Committee on the Economic Report (1952, p. 727). Formulations such as this were precursors to the descriptions of the inflation problem given by many who worked in policymaking from the late 1970s onward, both in the United Kingdom and the United States (regarding the latter, see the statements by Paul Volcker and Alan Greenspan quoted in Lindsey, Orphanides, and Rasche, 2005).

[55] See, e.g., Krugman (1998) and Svensson (2003).

[56] Friedman (1977b, p. 12).

the revival of the quantity theory of money did not really take off until the 1960s. Evidently, the 1951 abandonment of cheap money policies was not quite as great a breakthrough as Friedman and other advocates of monetary policy had imagined. What went wrong?

Friedman was, on the whole, pleased with the course that monetary policy followed in the United States during the 1950s. But even in the U.S. case he was uneasy about the continuing emphasis on fiscal and other nonmonetary influences when it came to accounting for economic fluctuations; thus Friedman (1955, p. 32) referred to "the intellectual climate of today and the recent past, with its derogation of the significance of monetary factors." Furthermore, diagnoses of inflation were becoming less orthodox with the growing appeal from the mid-1950s of explanations that downplayed demand factors and instead stressed "cost-push."

In the United Kingdom, the trend of opinion against monetary policy was even stronger. The ending of cheap money was more hesitant; certainly interest rates were raised in 1951, but they were cut in 1954 while the economy was gathering steam. The really concerted tightening of monetary policy in the United Kingdom in the 1950s was concentrated in the years 1955-58, which may be why Friedman (1963b, p. 7) once said that the U.K. cheap money period ended "a few years" after 1951. The tightening began with increases in Bank Rate in January-February 1955 under Chancellor of the Exchequer R.A. Butler, and was followed by further increases under his successors in 1956 and 1957. The 1955-58 subperiod distinguishes itself from the preceding and subsequent epochs by the extent to which the authorities were attributed interest in control of the stock of money. For example, *Financial Times* columnist Harold Wincott contemplated what would happen "if Mr. Butler continues with his policy of contracting the supply of money and credit" (*Financial Times*, October 4, 1955), while the *Financial Times*'s "Lombard" commentator said that the "ultimate aim of the Government's credit restriction drive is, *of course*, to exert a downward pressure on the supply of money strong enough to keep spending power within the limits of the country's available resources" (November 30, 1955; emphasis added). In the September 1957 round of tightening, policymakers themselves became very explicit about their intention to restrict the money supply, with speeches to that effect by the prime minister (Harold Macmillan) and Treasury ministers.

The emphasis on monetary aggregates at this early stage may seem anomalous, as the official money series (M0, M1, M3, and so on) that would become familiar in later years were not available. Many have noted that U.K. money supply data were not available to Keynes when he wrote on monetary affairs (see, e.g., Patinkin, 1976b), and Walters (1970) conjectures that a historical series for U.K. money was not put together until the early 1960s. One should not exaggerate the absence of monetary data, however; the weekly release of the Bank of England's balance sheet gave most of the information needed to construct currency and monetary base series; and the various releases of the clearing banks and other institutions provided information on deposits. These releases were the subject of regular attention in the financial press.[57] The main problem for potential investigators of monetary relations was constructing long series free of breaks and double-counting. It was also well known that the basic data for constructing a long historical money series were available far back for the United Kingdom; Friedman (1961b, p. 270) referred to the availability of U.K. deposit data back to the 1870s.

The would-be revival of monetary policy in the United Kingdom suffered severe criticism once the 1955 interest rate increases failed to deliver the desired results during the year. The *Guardian*'s financial editor had already claimed, "It is now generally agreed that the experiment of checking inflation by monetary policy alone has not been a success" (*Manchester Guardian*, December 12, 1955).

To many critics, the apparent failure of monetary policy to deliver low inflation vindicated

[57] For example, for much of the 1940s and 1950s there was regular space devoted in the *Financial Times* to the Bank of England's balance sheet release. Deposit data were discussed regularly too; for example, in the *Financial Times* of November 30, 1955.

the notion that monetary policy was ineffective because aggregate demand was interest inelastic. This notion, already embodied in the *General Theory* to some extent,[58] had been reinforced in the United Kingdom in the prewar and early postwar period by surveys of firms carried out by Oxford University. The survey results seemed, as discussed, for example, by Schlesinger (1956, p. 603), to vindicate the view that firms' investment decisions were interest inelastic (and with them the whole of aggregate demand, as most Keynesian work had already narrowed the interest rate channel to investment).

Friedman was scathing about the value of questionnaires of businessmen. "That is not evidence…I do not care what they have said," he said on a panel in 1950.[59] In 1979, Friedman added, "Economics is a serious subject, and one of the things we've learned in that subject is that if you want to know how people behave, you don't ask them. You look."[60] The joint behavior of real returns and the stock of productive capital led Friedman to believe that investment was instead "highly elastic" with respect to real interest rates (Friedman and Schwartz, 1982, p. 494). Friedman's reaction to the survey results paralleled that of his hero Dennis Robertson, who had said in 1949 that he had "a hunch that the reaction among the neo-Keynesians against the importance of the causal influence of the rate of interest on capital outlay has been carried too far…Does anyone here, I wonder, share my doubts—my very respectful doubts—about the significance of those replies to questionnaires?" (Robertson, 1949, p. 20).

Keynesians not only doubted the effectiveness of monetary policy as a demand-control measure; they argued that efforts to control inflation via demand measures might in any case be misguided. The *Financial Times* editorialized during the initial tightenings that there was "still something of a mystery about the origins of the inflationary forces threatening the British economy" (February 15, 1955). Despite their use of monetary policy tightening, policymakers shared the view that much inflation was cost-push in character, a view evident in their repeated attempts to secure an agreement with the private sector on wage growth limits.

The skeptical sentiments regarding monetary policy made by Keynesians since the 1930s were synthesized and consolidated in the report on monetary policy delivered by the Radcliffe Committee to the U.K. government in August 1959. The Radcliffe Report argued that monetary policy was ineffective. It did not rely on the liquidity trap argument but used lines of reasoning that delivered as complete an ineffectiveness result as that associated with the liquidity trap. Whereas Keynes's liquidity trap argument said that money and government securities could become equivalent at a low interest rate, the Radcliffe Committee argued that important asset prices were unaffected by open market operations that switched money for short-term securities, even when these operations changed the short-term interest rate. Thus while open market operations could alter policy rates, they affected only the composition, not the aggregate, of "liquidity," which was the financial quantity that really mattered; financial innovation, it was argued, had eliminated much of the difference between money and Treasury bills. The interest rates that policy could affect, the Committee argued, mattered negligibly for aggregate demand, while the asset prices that did matter for aggregate demand depended on the "liquidity" total, which was generally not susceptible to central bank manipulation. The Committee also endorsed cost-push views of inflation.[61]

Friedman and Schwartz (1982, p. 52) observe that the Radcliffe Committee was "faithful to Keynes" in emphasizing the ineffectiveness of monetary policy arising from the alleged equivalence of money and securities. But the Committee, by claiming that monetary policy was ineffective generally, not just in Depression conditions, was taking a harder-line position than Keynes usually did. The Committee's basis for this conclusion

[58] See, for example, the passage cited in Patinkin (1976a, p. 103).

[59] Friedman, speaking in Wright (1951, p. 251).

[60] In Anderson (1982, pp. 201-02).

[61] See Radcliffe Committee (1959); and for references to the relevant sections, see, e.g., Laidler (1989) and Nelson (2009).

was that financial innovation—for example, the growth of nonbank intermediaries—put the determination of important asset prices outside the reach of monetary policy. While the emphasis on financial innovation did not have a counterpart in Keynes's *General Theory*, it paralleled the approach that Gurley and Shaw (1960) were taking in analyzing U.S. financial behavior. Noting the connection, Friedman and Schwartz (1970) treated Gurley-Shaw and Radcliffe as advocates of the same "liquidity" argument, while Friedman and Schwartz (1982, p. 209) referenced Radcliffe and Gurley-Shaw together when citing studies that minimized the significance of money and monetary policy actions. Monetarists were not impressed by the Radcliffe/Gurley-Shaw arguments from the beginning, and the monetarist side of the argument was what—eventually—won the day in the economics profession. Brunner (1985, p. 22) observed witheringly that there really was "no logical link between negative conclusions bearing on monetary policy, and the discussion of financial innovations…Gurley and Shaw argued more than 20 years ago that the explosive growth of savings and loans associations erodes the potency of monetary policy. The subsequent evolution discredited such fears or hopes."

Friedman's initial public response to the Radcliffe Report was muted. Alvin Marty thanked Friedman for "exceedingly helpful substantive comments" on a paper published in early 1961 in which Marty said the "Radcliffe Report is a striking example of failure to offer a shred of evidence."[62] In a book review published at the end of 1961, Friedman (1961a, pp. 1052-53) noted the problems in defining "liquidity"; in 1964, he said that the Radcliffe Committee's liquidity concept was "an undefined term which covers the universe,"[63] while Friedman and Schwartz (1970, p. 130) added that the Radcliffe Committee itself could not settle on a firm liquidity definition.[64] Friedman denounced the theories offered by the Radcliffe Committee as "a false trail that will not in fact be productive" (Friedman, 1963b, p. 9) and went on to applaud the negative reception the Radcliffe Report had received among "academic economists and others."[65] Friedman's appraisal that the Report had been received negatively rested largely on the U.S. reaction; in the United Kingdom, many policy and academic figures greeted the Report favorably.

In U.K. policymaking, confidence in monetary policy restriction as the cure for inflation reached its peak in 1957 and was followed by a period of substantial monetary policy easing. Friedman (1980b) argued that the shift to expansionary policies (both monetary and fiscal) in the United Kingdom from the later 1950s onward was a vindication of his (1954) prediction. He had predicted that overreaction to actual or prospective minor recessions would produce a tendency for the authorities to overexpand. Complementing this explanation is the fact that the U.K. authorities after 1957 were much more inclined to view incomes policies as the appropriate means of fighting inflation. Even when inflation fell in the late 1950s, to the point of delivering price stability in 1960, the success was attributed to favorable cost-push shocks rather than to the 1955-58 restrictive monetary policy. For example, *The Economist* (August 29, 1970) attributed the fall in inflation from 1958 to 1960 to less-militant union behavior after the defeat of the 1958 London bus strike.

1959-1970

Events

In a new round of monetary policy tightening in July 1961, the Macmillan government raised Bank Rate to 7 percent. This was not an unambiguous affirmation of the role on monetary policy, because it was muddied by a simultaneous attempt by the government at a wage freeze (a "pay pause"). But after a year of tight monetary policy, the Chancellor of the Exchequer, Selwyn Lloyd, showed signs of determination to maintain

[62] Marty (1961, pp. 56, 59).

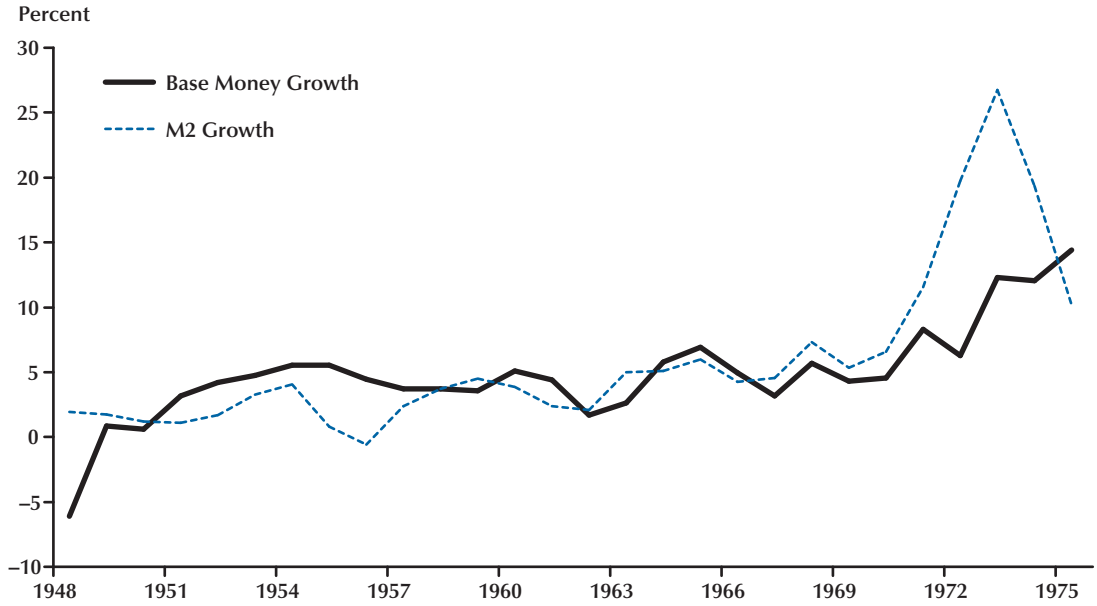
[63] Friedman (1964a; p. 73 of 1969 reprint).

[64] The critical discussion of the Radcliffe Committee in Friedman and Schwartz (1970) was originally part of their unpublished first draft of their *Trends* study (Friedman and Schwartz, 1966).

[65] Friedman (1964a; p. 73 of 1969 reprint).

Figure 2

U.K. M2 and Monetary Base Growth (1948-75, annual data)



a restrictive stance: "18 months ago there was excess demand…Now, I think there is a measure of disinflation…I think the economy is in better shape, but you can't have disinflationary measures without there being, in result, a measure of disinflation…That means that some order books will be shorter" (*Yorkshire Post*, July 11, 1962). Shortly afterward, Lloyd was fired. The restrictive monetary policy episode turned out to be only an interruption in the de-emphasis on monetary policy signaled by the Radcliffe Report. The expansionary policies prevailing before 1961 were revived in a more-intense form. The more-expansionary policy was associated with a shift up in money growth, whether measured by the monetary base or by Friedman and Schwartz's (1982) M2 measure, reversing the moderation in growth observed during the 1961 squeeze (Figure 2). Consequently, while the pickup in U.S. money growth in the 1960s initially exceeded that in the United Kingdom, Friedman and Schwartz (1982, p. 157) note that, from the mid-1960s, "the United Kingdom took the lead—if that is the right word."

Friedman viewed the U.K. monetary expansion of the 1960s as contributing to the mood of the country. "[T]he fact is that most people enjoy the early stages of the inflationary process. Take, for example, Britain in the Swinging Sixties" (*The Listener*, April 24, 1980). One aspect of the sustained expansionary policies of the 1960s that did cause concern to the U.K. authorities was one that Friedman would prefer they had been sanguine about: the threat to the exchange rate. The sterling/dollar rate was becoming more difficult to maintain, even with the exchange control apparatus, and speculation against it increased after the Labour Party under Harold Wilson was elected to office in 1964. Friedman (1965, p. 179) said that while he happened to disagree with the policies Wilson had promised to carry out, he found it objectionable that foreign central banks and other holders of sterling were perceived as having a "veto power" over their implementation. He elaborated in September 1965 that this meant "that British internal policy was shaped by officials who were not responsible to the British

electorate."[66] Friedman concluded that Wilson, instead of negotiating a sterling rescue, should have floated the pound on coming to office, blaming the predecessor government for the likely depreciation.[67]

The Wilson government finally did devalue the pound sterling in November 1967, with the 14 percent adjustment providing another example of what Friedman (1969, p. 20) called "this awful business of holding and holding and holding to the last gasp and then having to make a big change." Friedman had anticipated the devaluation and had wanted to speculate $30,000 against the pound, only to find that his Chicago banks did not have the wherewithal to carry out the foreign exchange transaction (*Sydney Morning Herald*, October 9, 1986). After the devaluation, the Wilson government started expressing policy commitments to the IMF in terms of quantitative financial targets, and in 1969 it announced a target for domestic credit expansion (DCE). Although interpreted as a concession to monetarists, the DCE targets had the decidedly un-monetarist implication of encouraging the authorities to regard money base growth that came from balance of payments surpluses as "good." The policy framework of a fixed exchange rate, attention to DCE at the expense of the aggregate monetary base, and incomes policies, contrasted with Friedman's recommendation of a sterling float, no incomes policy, and direct control of the aggregate base or aggregate bank reserves.

Issues

Monetarism and the Quantity Theory. It is difficult to convey the dramatic shift in the amount of coverage given to monetary policy in the U.K. financial press and political debate in the years 1968 to 1970 compared with the preceding three years 1965 to 1967. The increased degree of coverage turned out to be permanent. Some flavor is captured by the observation of the magazine *Management Today* (August 1976): "A decade ago economists, pundits and politicians alike would have been amazed to learn that in the mid-1970s debates over monetary policy would have come to dominate the national and international economic scene."

The upsurge in the coverage of monetary policy was accompanied by greatly increased discussion of Friedman. While "Chicago School" was probably the most widely used term to describe the school of thought emphasizing money (e.g., *The Sun*, October 7, 1968; *The Observer*, April 20, 1969), and "Friedmanite" was used from an early stage (e.g., *Sunday Times*, November 10, 1968), the U.K. debate also rapidly proliferated a term that *The Economist* had used as early as 1963, but which was starting to become prevalent in the United States: "monetarist." Robert Solow used "monetarism" repeatedly in an article he wrote for *The Times* (December 23, 1968), and Paul Samuelson criticized "crude monetarism" in a contribution to the *Sunday Telegraph* (December 15, 1968).

Friedman often publicly criticized the terms "monetarist" or "monetarism." In an interview with *The Times* in 1976, Friedman said, "It is not a new position, and that is one of the reasons I don't like the word monetarism" (*The Times*, September 13, 1976).[68] As Friedman saw it, he was not launching a new theory but bringing quantity theorists' work "down to date,"[69] so that it could be applied to the problems of the "bad old present."[70] But Friedman (1978) confessed that there was utility to the term monetarism because there were some elements of older quantity theory work that he and other monetarists had discarded. In particular, an aspect of earlier quantity theory analysis that Friedman explicitly rejected was regarding velocity behavior as the outcome of an institutionally determined payments process,

[66] From Friedman's 1965 Mont Pelerin Society meeting remarks, published in Friedman (1968a, p. 274).

[67] See, e.g., his remarks in Friedman and Roosa (1967, pp. 114-15) and his 1968 memorandum in Friedman (1988).

[68] Anna Schwartz has suggested that another reason for Friedman's reservations about the word "monetarism" is that "I think he attributed it to Karl Brunner, who was not really a master of English." (Conversation with author, trip to Vermont Great Inflation conference, September 25, 2008.) Brunner is typically credited with the term monetarist or monetarism, but both terms predate Brunner's usage of them (see Laidler, 2001; and the *Oxford English Dictionary*, 1976 and online editions).

[69] Friedman (1972d, p. 12).

[70] Friedman speaking in *The Guardian*, September 16, 1974.

instead of as the result of decision problems by households (e.g., Friedman, 1956, point 11; Friedman, 1963b, p. 10; Friedman and Schwartz, 1982, pp. 38, 40, 62). Friedman himself used "monetarism" and "monetarist" in his address at the University of London (1970a). He became accustomed to using these terms readily and without prompting, including in correspondence and conversation.

On some occasions Friedman also associated the older quantity theory with a further retrograde aspect, namely the assumption of price flexibility, or, more fairly, of not having a firm description of the adjustment process of prices to money (e.g., Friedman and Schwartz, 1982, p. 44). But he generally qualified this case by describing it as the "simple quantity theory" (e.g., Friedman and Schwartz, 1982, pp. 59, 398). Further consideration by Friedman during the 1970s of the quantity-theory literature had the effect of leading him, if anything, to attribute more to the older writers. In particular, Friedman was struck by how explicit David Hume's writings had been on the role of expectations: "David Hume has a statement somewhere about the fact that an increase in the quantity of money stimulates economic activity only so long as it keeps on increasing and people don't expect it" (*The Times*, September 13, 1976). Friedman decided that while he, and Lucas and Sargent after him, had expressed the point more formally, the expectational Phillips curve idea was due to Hume (Friedman, 1975b). While Patinkin (1972b) claimed that Hume did not believe in a long-run vertical Phillips curve, the explicit quotations given in Friedman (1975b) support the crediting of this idea to Hume, and Friedman's interpretation was more recently reaffirmed by Mankiw (2001) in his study of the same Hume passages.

Friedman further credited Hume with the demand-for-money perspective on the quantity theory that Friedman had used (Friedman and Schwartz, 1982, p. 621). So whereas Friedman (1968c, p. 433) attributed to Hume the "broad outlines of the quantity theory," by the early 1980s he was crediting Hume with both the aggregate demand and aggregate supply aspects of his own framework and so, he said, Hume was the true originator of monetarism. Appropriately enough, it was on British television that Friedman said: "I really would like to make clear that the doctrines I proclaim are not original with me by any means, in fact if I have to find a source for them they are [from] David Hume."[71]

In their interventions in the U.K. debate, Samuelson and Solow argued that monetarism was not making valid points about monetary relations that U.S. Keynesianism had not long since incorporated. Samuelson, for example, said: "Money was, so to speak, 'rediscovered' in my country around 1950…When Professor Friedman formulates his system in generality…it coincides with the post-Keynesianism of the Tobin-Modigliani type." But the record does not support the denial of Friedman's contributions, nor Samuelson and Solow's confidence that U.S. Keynesianism circa 1968 had incorporated the role for monetary policy adequately. As we have seen, Friedman's emphasis on money predated 1950, and his elaboration of it incorporated a general transmission mechanism not covered in Keynesian work; in particular, more than one interest rate, and (in contrast to positions taken by Samuelson and Modigliani) a sensitivity of consumption (not merely investment) to interest rates.[72] Furthermore, Solow and Samuelson in both the 1960s and 1970s disputed Friedman's expectational Phillips curve analysis, contesting both its long-run vertical property and its exclusion of a systematic role for cost-push factors. In well-known lectures given in Manchester, for example, Solow (1969) argued that cost-push factors mattered greatly in both the United Kingdom and the United States, that demand factors barely mattered at all for U.K. inflation, and that the U.S. Phillips curve was permanently nonvertical.

Some influence of the money supply debate was felt in what policymakers said: Chancellor

[71] *Free to Choose*, BBC2 debate, March 22, 1980, p. 15 of transcript. Likewise, at a press conference in Wellington, New Zealand, in 1981, Friedman said, "What is called monetarism, the quantity theory of money, was developed by David Hume in the eighteenth century. It is not 'my' theory—I have no patent on it" (*Evening Post*, April 27, 1981).

[72] See Blinder (1986) and Modigliani (1986) for characterizations of the pre-monetarist Keynesian view as one that denied the interest sensitivity of consumption.

of the Exchequer Roy Jenkins stated, "I attach the greatest importance to monetary policy" (*House of Commons Debates*, April 15, 1969, p. 1007); and *The Economist* (April 18, 1970) referred to the "new importance attached to monetary policy in Britain." As noted above, however, the changes actually made in the macroeconomic policy framework were not truly in the direction Friedman wanted. Indeed, at the U.K. Treasury, skepticism prevailed from top to bottom about the attention being given to Friedman. At a junior level, Treasury economist Stephanie Edge spoke in favor of the power of fiscal policy and criticized Friedman's findings to the contrary: "The idea that simple one-equation comparisons can reveal anything is one that should be vigorously attacked" (Edge, 1967, p. 205). At a senior level, Treasury adviser Alec Cairncross wrote in his diary of October 6, 1968, that "the English press (and especially *The Times*) was making such a fool of itself over Milton Friedman" (Cairncross, 1997, p. 327).

Cairncross's reference to *The Times* concerned the articles being written by its economics editor, Peter Jay. Jay was initially regarded with suspicion on each side of the money supply debate as a sympathizer with the other side. But further articles by Jay brought him out as a supporter of the monetarist arguments, and Jay later identified himself among "those…who began to advocate proper control of the money supply from the late 1960s" (*Independent*, September 23, 1991). Friedman and Jay became good friends, appearing together on several episodes of both the U.S. and U.K. versions of the *Free to Choose* television program in 1980. In one of these programs, Friedman said that he and Jay "are in almost complete agreement on the desirable monetary policy."[73]

Another journalist, Samuel Brittan, serves as an illustration of Friedman's observation that "accidents play an enormous role in mankind's experience."[74] Friedman happened to be visiting Cambridge University while Brittan was an undergraduate student there, and Brittan happened to have Friedman assigned to him as his tutor. Brittan joined the *Financial Times* after graduation. Brittan considered the arrival of monetarism the "most interesting event for a very long time in the realm of economic ideas" (*Financial Times*, January 8, 1970). A long article by Brittan, headed "MONEY SUPPLY: The Great Debate" in an enormous font, appeared in the October 25, 1968, edition of the *Financial Times*, discussing Friedman's views and covering his American Economic Association presidential address (Friedman, 1968b). Brittan and David Laidler, who had been a Ph.D. student of Friedman's at Chicago and from the late 1960s was at Manchester University, became two leading voices of monetarism in the United Kingdom during the 1970s. Cobham (1984) notes that although Laidler departed for Canada in 1975, he remained prominent in the U.K. debate during the second half of the 1970s. Neither Brittan nor Laidler was an echo chamber for Friedman, and both disagreed with him in print, but they both had firsthand knowledge of his positions. As Friedman observed, "You have the interesting phenomenon that whereas David Laidler came to Chicago, Chicago came to Sam Brittan."[75]

Alan Walters, an academic and financial consultant who had been undertaking U.K. analogues of some of Friedman's empirical work since the early 1960s, wrote to Friedman at the end of the decade to let him know that he was close to being a household name in the United Kingdom.[76]

The Beginning of *Monetary Trends*. In November 1966, Friedman and Anna Schwartz completed a draft of their manuscript on monetary trends, concerned solely with U.S. data, and submitted it to the NBER review process. Friedman (1955, p. 30) had written about the desirability of studying U.K. monetary data, and, somewhere along the line, Friedman and Schwartz elected to cover U.K. data in the revised version of their *Trends* manuscript, although it was not a change specifically requested by the NBER. A major obstacle, the construction of historical data, was

[73] *Free to Choose*, PBS debate, Episode 3, pp. 16-17 of PDF transcript.

[74] April 16, 1996, talk by Friedman at Claremont College (broadcast C-SPAN, December 26, 1996).

[75] Friedman, speaking in September 1974, in Institute of Economic Affairs (1974, p. 102).

[76] Letter from Alan Walters to Milton Friedman, December 4, 1969, Friedman office correspondence (uncatalogued as of end-2007).

partly overcome when David Sheppard, a Harvard Ph.D. graduate shortly to return to the United Kingdom, contacted Friedman to let him know of his work in the area. In his reply to Sheppard, Friedman indicated that U.K. data were being incorporated into his new volume with Schwartz.[77] He and Schwartz thereafter used the Sheppard data on money. Anna Schwartz used the data in a 1969 paper for U.K. audiences (Schwartz, 1969), and Friedman referred fleetingly to the *Trends* project during Congressional testimony in October 1969, where he said he had been "working on some British data which go back a century. They show the same relation [as in the United States]."[78] Considering this energetic start in the late 1960s, Friedman and Schwartz surely could not have imagined that *Monetary Trends* would not be finished until the early 1980s.

1970-1979

Events

The Conservative Party, led by Edward Heath, won the U.K. general election of June 1970. Friedman had not met Heath (*The Listener*, February 11, 1971). There is nevertheless evidence of an influence of Friedman's writings on Heath's statements. Heath's introduction to his 1970 party platform said, "[O]nce a policy is established, the Prime Minister and his colleagues should have the courage to stick to it. Nothing has done Britain more harm in the world than the endless backing and filling which we have seen in recent years" (*The Guardian*, May 27, 1970). It is possible that the drafting of this passage was influenced by Friedman and Schwartz's (1963, p. 289) characterization of the Federal Reserve's history as one of "so much backing and filling, so much confusion about purpose…"

There was less indication that the new government would be influenced by the Friedman-Schwartz work when it came to monetary policy formulation, as Anna Schwartz discovered on a visit to the United Kingdom very soon after the election result. Writing to Friedman about her meetings with U.K. academic economists and Bank of England officials, Schwartz reported: "Much talk generally of what difference the Conservative Government would make for monetary policy…[The Bank officials] didn't see that it would make any difference. Apparently, Bank policy is perfect."[79]

On the matter of the government's role in the market, the prospect seemed more favorable that U.K. economic policy would go in a direction favored by Friedman. Friedman said in 1971, "My own personal view is that…the most effective road to development is through free enterprise and private investment, and that the government can serve best by limiting itself to essential government functions."[80] In the same year, the Heath government objectives were laid out by the Chancellor of the Exchequer, Tony Barber, in a form that seemed in keeping with Friedman's views: "Our object is to lessen government interference and reduce government subsidies; to extend the opportunities for profitable enterprise; to widen the area within which industry rather than government will make decisions" (*Dallas Morning News*, February 9, 1971). Friedman expressed cautious approval, observing that the United Kingdom had "potential, but only if you could by some miracle get rid of the enormous mass of controls, interventions, welfare-state measures and so on…Heath has been moving somewhat in that direction" (*Vision*, April 1972).

The Heath government moved away from free-market policies during 1971 and made the break more explicit with the passage of the Industry Act 1972. The act introduced extensive subsidies to private investment, contrary to Friedman's dictum, "Capital investment that has to be subsidized is not worth having" (*Wall Street Journal*, February 12, 1997).

[77] Friedman, letter to David K. Sheppard, July 10, 1967, Friedman office files (uncatalogued as of end-2007).

[78] October 6, 1969, testimony, in Joint Economic Committee (1970, p. 826).

[79] July 2, 1970, letter from Anna Schwartz to Friedman. The Hoover Archives' copy of this letter is in Box 91, folder 7, of the Friedman papers.

[80] Friedman (1971a, p. 847).

The change in direction was felt also in the Heath government's policies against inflation. The government started cutting Bank Rate in April 1971. It had consistently seen inflation as a nonmonetary problem and in November 1972 imposed wage-price controls.

Also in 1970, Friedman had his first published exchange with a U.K. critic of his work. The Radcliffe Report had given Friedman a heads-up about the skepticism regarding monetary policy prevailing in the United Kingdom. Along with Richard Kahn, Nicholas Kaldor was regarded as a major academic influence on the Report. Shortly before Friedman's visit, Kaldor (1970) restated the U.K. anti-monetary policy position and used a reverse causation argument to dispute Friedman's findings. If, Kaldor argued, the authorities actually undertook measures that delivered them control of the money stock, their actions on money would face permanent, completely offsetting movements in velocity. Historical relations between money and other variables, according to this argument, simply reflected reverse causation—the passive creation of money in response to price and output movements. Price and output behavior would, it was argued, have been no different if the monetary authorities had somehow prevented this money creation from taking place.

The way that Friedman (1970c) answered Kaldor was by appealing to the fact that money had been connected to income and inflation under many different monetary arrangements, undermining an explanation of the correlation like Kaldor's that relied on the existence of a particular policy regime or on the institutional conditions prevailing in the United Kingdom.

The Bretton Woods system collapsed during the early 1970s, despite what Friedman said was officials' belief that "they can put Humpty Dumpty together again."[81] The London *Evening Standard*'s financial columnist blamed the foreign exchange market turmoil on the "incredible influence of economist Milton Friedman," charging that Friedman's theories had discouraged international coordination of policies (May 5, 1971). Though he would surely have liked to accept the credit for the advent of floating rates, Friedman concluded that his advocacy of flexible rates had had "absolutely no effect," and that it was instead the "brute force of events" that had forced governments to realize that fixed exchange rates were untenable (*The Listener*, April 27, 1978). The pound sterling began floating in June 1972.

In early December 1972, Friedman learned that a debate had been taking place in the London *Times* about his 1967 American Economic Association presidential address. His statement there (Friedman, 1968b, p. 11), that "full adjustment" to a shift in the inflation rate takes "a couple of decades," had been interpreted by one critic as implying that removing inflation would take 20 years of above-normal unemployment. Friedman wrote a letter to *The Times* (dated December 6 and published December 12) to clarify that by "full adjustment" he meant resettling at the steady state. "The important point is that while 'full' adjustment may well last several decades, the period of unusually high unemployment is far shorter, more like two to five years." Around the same time, in a submission to the U.S. Congress Joint Economic Committee, Friedman had occasion to convey his opinion of the U.K./France Concorde project. That project had reached fruition partly from the injection of funds from the U.K. government. The result was an air service that took hours off of intercontinental travel, but only for the elite class of customers who could afford the ultrahigh ticket prices. Friedman (1958a) had criticized the involvement of the government sector in the creation of "monuments" that did not raise ordinary living standards, and his Congressional submission (dated December 11, 1972) urged that the United States government not follow the Concorde precedent by subsidizing a U.S. supersonic transport (SST). "A governmental decision to produce an SST largely at its own expense is a step toward socialism and away from free enterprise."[82]

Though the preceding two items do not appear in his published bibliographies, they could easily have become the last things Friedman ever wrote

[81] September 23, 1971, testimony, in Joint Economic Committee (1971, p. 699).

[82] In Joint Economic Committee (1973a, p. 81).

for publication. On December 15, Friedman had open-heart surgery. The surgery was successful and Friedman was discharged on December 26 (*Kansas City Times*, December 27, 1972), but Friedman lost considerable weight in the wake of the surgery, and his family medical history was inauspicious. As Anna Schwartz has observed, "Who would have thought at that time that Friedman would live on to ninety-four?"[83]

From the United States, Friedman criticized U.K. monetary policy developments during 1973, observing in Congressional testimony that "defective as our policy has been, it has been less erratic, more moderate than the policy of most other leading countries."[84] He observed later in the year that Heath had gone from what Friedman perhaps too generously called a "tight money policy" to "a policy of stimulating…Now you have prices rising in Britain at a rate of something over 10% a year" (Friedman, 1973b, p. 33). The British magazine *Management Today* pondered Heath's and Friedman's contrasting diagnoses of the inflation problem in its August 1973 issue: "But is the entire phenomenon of unusually rapid and apparently ineradicable inflation new in itself?," its editorial asked. "Is it a different variety, considerably more virulent, of the disease to which Western society has been susceptible for many decades past? The temptation, of course, is to say that it is: to blame union militancy…But economic historians half a century hence may well not be impressed by this argument. To them, the inflation will probably seem a classic case of monetary inflation, engendered by the usual process of overproduction of liquid currencies."

The editorial just quoted was something of an outlier in the general U.K. discussion in 1973. The tendency to consider inflation to be nonmonetary in character intensified in the wake of the oil and other commodity price shocks of that year. Wherever he went during the 1970s, Friedman found himself having to explain the fallacy inherent in special-factor explanations for inflation. "Arithmetic is one thing and economics is a very different thing," was how Friedman put it in one appearance (*Meet the Press*, November 12, 1978). "The great confusion in this area is to confuse particular prices with prices in general." In 1979 Friedman, perhaps near his wit's end, reiterated: "OPEC does not cause inflation; no, sir."[85]

What Friedman (1975a, p. 137) called the U.K.'s "major economic crisis in early 1974," with U.K. inflation passing 15 percent, culminated in an election that returned Harold Wilson to power. Friedman noted that the recent U.K. elections had helped refute the claim that governments do not lose elections because of high inflation. "Inflation surely helped to make Mr. Edward Heath Prime Minister in 1970," Friedman (1974, p. 44) observed, "and, even more surely, ex-Prime Minister in 1974."

The centerpiece of the Wilson government's incomes policies measures was intended to be its "Social Contract" agreement with the unions. Friedman said that "the so-called Social Contract… gives people a false impression of both the causes of inflation and the way to cure it" (BBC2, November 9, 1976). The Social Contract would do "no good whatsoever as long as they continue to run the printing press" (Friedman, 1975d, p. 20); and, if money growth was slowed, the Social Contract would be seen as having been successful, even though the reduction in inflation would be the same without the Contract (*Newsweek*, September 20, 1976).

Friedman paid a one-week visit to the United Kingdom in September 1974. Reflecting on his visit a few months later, Friedman was particularly struck by the continuing popularity of the wage-push explanation. "In Britain, the explanation that everybody gives for inflation is that inflation is caused by trade unions, the greedy grasping laborers who force up the wages that cause inflation" (Friedman, 1975d, pp. 5, 7). During his visit Friedman had written to *The Economist* saying he had "been dismayed, even in my few days in London, at the widespread

[83] Remarks of Anna Schwartz to author, New York office of NBER, May 27, 2008.

[84] Friedman, June 21, 1973, testimony, in Joint Economic Committee (1973b, p. 120).

[85] May 17, 1979, testimony, in Committee on the Judiciary (1980, p. 154.)

support of 'union bashing' as a way to attack inflation" (*The Economist*, September 28, 1974).

While Friedman argued in 1978 that there was "almost no one who any longer has a good word to say about nationalization," he had to admit that in the United Kingdom the trend had been to extension of nationalization (*The Listener*, April 27, 1978). Indeed, Harold Wilson's account of his 1974-76 term in office (Wilson, 1979, p. 35) includes a six-line list of the nationalization plans his government outlined in 1974, and the Labour government did proceed to extend nationalization to the shipbuilding industry. With Thomas Balogh, one of Friedman's Keynesian opponents, as one of the ministers responsible, the Department of Energy also increased the government's stake in the oil industry—leading to Friedman's observation, "You have been nationalizing North Sea oil" (*The Listener*, April 27, 1978). Some of the oil-industry nationalization was wound back in the austerity measures of late 1976, when the Callaghan government announced the sale of part of its interest in British Petroleum.

The U.K. private corporate sector over this period was suffering a pronounced squeeze. "Great Britain had a much more severe financial crisis than we did," Friedman observed.[86] The stock market experienced a major collapse, its index standing in 1974 at its late 1950s' value (Bordo and Wheelock, 2004), and the U.K. long-term corporate bond market virtually disappeared in the second half of the 1970s. Friedman noted that a "proper climate" for growth required "investment, enterprise, the ability to borrow capital" (*Dallas Morning News*, October 17, 1975), but in the U.K. case he observed that "the domestic capital markets are so disorganized by erratic inflation, excessive taxation, and government intervention" (*Newsweek*, December 27, 1976).

It was, however, foreign exchange market turmoil that led to the United Kingdom negotiating a loan from the IMF in late 1976. The Callaghan government, including Chancellor of the Exchequer Healey, appealed to the stringent terms of the loan as the reason it had to undertake cuts in government expenditure. Friedman maintained that the government's recourse to the IMF was just a charade: "Your government has gone to the IMF so that they can lay down rules for the management of your economy…It's like the way big corporations use management consultants. The corporations know perfectly well what must be done, but they want to blame the unpleasant remedies on someone else" (*Daily Mail*, September 30, 1976). No doubt there was a considerable element of validity to this conjecture, as senior members of the Callaghan government had indeed accepted the need to shift the division of resources between the public and private sectors. But Friedman went too far with his further claim, "The British government knows that the only way to stop inflation is for government to spend less and to create less money" (*Newsweek*, October 11, 1976). This claim attributed, yet again, acceptance of a monetary view of inflation to the government. Such an acceptance is not supported by the record of U.K. policymakers' views or behavior; on the contrary, the government continued to point to the Social Contract as a central part of its fight against inflation and to claim that monetary policy alone could not defeat inflation. A wage-push view of U.K. inflation continued to dominate, and the government saw monetary targets—the first publicly announced target was for the financial year starting in April 1976—as a means of helping to avoid adding demand-pull to the wage-push pressures. Friedman subsequently pulled back from his late 1976 claim that the authorities knew inflation was a monetary phenomenon. In a November 7, 1977, talk, Friedman said, "If you listen to anybody telling you about Great Britain's plight, they will tell you that the real problem in Great Britain is that you have such strong trade unions, that they push up wages and that causes inflation."[87]

When the government announced its budgetary program in the wake of the IMF loan negotiation, Friedman pronounced himself unimpressed, pointing to the modest nature of the public expenditure cuts, the use of devices such as asset sales, and the failure to cut tax rates (*Daily Mail*, December 17, 1976). But the U.K. government's expenditure did fall substantially after 1976 as a

[86] *Milton Friedman Speaks*, Episode 5, p. 23 of transcript.

[87] *Milton Friedman Speaks*, Episode 6, p. 6 of transcript.

share of output. Acknowledging this, Friedman said in February 1978 that a "rather curious reason for hope" was the fact that a "Labour Government for two years in a row has been led by political pressures to reduce government spending as a fraction of income."[88]

Another source of hope for Friedman was a speech that Prime Minister Callaghan made on September 28, 1976. The speech, written with the assistance of Peter Jay (who, as well as being an advocate of money supply control, was Callaghan's son-in-law), was widely interpreted as signaling a repudiation of fine-tuning and Keynesian demand management, and invoked elements of the natural rate hypothesis. Friedman quoted the speech in his *Newsweek* column (December 6, 1976) and in his Nobel lecture given in December 1976 (Friedman, 1977a). Truth to tell, Friedman cited the speech excessively[89] and exaggerated its significance. The speech was not so dramatic a break with the past. The fact is that there were many occasions since the 1950s when prime ministers had talked about inflation moving up together with unemployment and on the danger of overstimulating the economy. Callaghan's speech sidestepped the greater problem with U.K. policymakers' outlook on inflation, namely their appeal to nonmonetary explanations.

Friedman appeared on BBC television in late 1976, in a studio debate taped in Chicago with former Wilson government adviser and minister Thomas Balogh. In the debate, Balogh said, "I think that the Professor really is terribly naïve." Friedman responded, "Well, I may be naïve but let me point out first that Mr. Balogh is simply defending his own record. Britain is in the position that it is because it listened to his advice and the advice of people who believe the way he does." Friedman emphasized that he was not referring only to the Labour governments with which Balogh had been affiliated, but to postwar governments of both parties, which he said had "generally followed very much the same policies...I am trying to argue against the general drift that has affected both parties" (BBC1, December 6, 1976).

"That great prophet of monetarism, Milt [sic] Friedman, is coming to Strathclyde University [in Glasgow, Scotland] in April to lecture on inflation," observed *The Scotsman*'s business columnist at the start of 1978 (January 25, 1978). In April 1978, Friedman, now 65 and sometimes describing himself as retired, duly appeared in the home city of Adam Smith to give a lecture and press conference. At his Glasgow appearances, Friedman qualified his praise for the Callaghan government's reduction in the ratio of government spending to GDP with criticism of its extensions of government intervention in the marketplace. He also pronounced himself unimpressed by the practical changes made in U.K. monetary policy. "In Britain, monetary targets have been adopted but have not been kept. Mr. Callaghan has said there will be no fine-tuning, but Mr. Healey has been fine-tuning" (*The Scotsman*, April 22, 1978).

Another aspect of the U.K. policy framework that was anathema to Friedman was the continued proffering of incomes policies—or in Friedman's (1976b, p. 233) blunt characterization, "general price or wage controls, euphemistically referred to as 'incomes policies'"—as a part of the government's anti-inflation strategy. Friedman had observed early in the U.S. wage/price control experiment (*Newsweek*, January 31, 1972): "Experience in other countries [beside the United States] suggests that for about a year such controls generally look good; after about two years, they collapse." The incomes policies put in place in the United Kingdom from 1972 to 1979 fell roughly into the pattern Friedman described. Heath's wage-price controls imposed in 1972 suppressed inflationary pressure for about a year before a breakout at the end of 1973 and in early 1974. The Social Contract of the Wilson government was largely violated until a more legally binding version was introduced in July 1975. U.K. inflation then generally declined for three years (1975-78), not two. But monetary policy had been tightened in late 1975 and over 1976; it was only from early 1977 that the government's incomes policies were attempting to push inflation away from the direction implied by monetary policy. The substantial

[88] *Milton Friedman Speaks*, Episode 7, pp. 20-21 of transcript.

[89] For example, in *Free to Choose* (both the television and book versions), in *Milton Friedman Speaks*, in Friedman (1992, 1997), etc.

monetary stimulus created in 1977 was followed by a collapse of the Social Contract at the end of 1978 and at the beginning of 1979. Shortly afterward, the Callaghan government lost a confidence vote in Parliament and had to hold a general election.

Friedman's travels, interviews, and commentary on current events meant that the Friedman-Schwartz *Monetary Trends* study of the United States and the United Kingdom was being slowed down. Friedman told an audience in Sheffield, England, in September 1970, "Mrs. Anna Schwartz and I are currently engaged on a comparison of U.S. and U.K. monetary trends…I had initially hoped to present a paper on this work at this seminar, but unfortunately the research did not go rapidly enough" (Friedman, 1971b, p. 151). In both that presentation and in Friedman lectures in the following years (Friedman, 1972c, 1973a), the U.K. coverage was limited mainly to discussions of data plots. Friedman and Schwartz (1972, p. 32) admitted, "Our estimate of the time it would take us to complete the manuscript on monetary trends has been unduly optimistic in the past… [W]e shall refrain from projecting a date for completion." The publication of Schwartz (1975) indicated that progress was being made, and by 1979 the project was edging to the finishing line, with publication projected for sometime in the early 1980s.

Issues

Common Market Entry. Though he sometimes referred to the "European countries and Britain" (e.g., in Pringle, 2002, p. 22), Friedman usually counted the United Kingdom as part of Europe. In 1948 Friedman referred to "Europe, including England" (*New York Times*, January 11, 1948) and classed the United Kingdom within Europe or Western Europe on later occasions too, including in Friedman (1958a, p. 510) and Friedman and Schwartz (1982, p. 309). A major issue for the United Kingdom in the 1960s and continuing into the 1970s was whether it should join other major European countries in the European Economic Community (EEC) or Common Market. Friedman reminded people that he played a part in the preparations for "the so-called Coal and Steel Community—a precursor to the Common Market" when he served as an adviser to the Marshall Plan in 1950 (*Newsweek*, May 24, 1971). In 1967, Friedman warned London newspaper readers not to expect too much of EEC membership. "Membership of the Common Market may or may not be good for Britain, but it is not a necessary part of the solution to Britain's difficulties. Germany in 1948 achieved an economic-miracle policy by decontrol without any Common Market" (*Sunday Telegraph*, June 25, 1967).

After unsuccessful attempts to negotiate entry in the 1960s, the United Kingdom joined the EEC at the start of 1973. By then, Friedman had expressed concern about the direction of the EEC, in particular the danger that it was "dominated by the notion that it's to serve as the super-central planning body" for member countries (*Vision*, April 1972). One of the planning measures undertaken by the EEC also went against Friedman's belief in free trade. "So far as the Common Market is concerned, they have engaged in agricultural protectionism on a large scale, as you say," Friedman observed in April 1978.[90] "They are making a mistake in doing that." In late 1978, the EEC started to move against something Friedman regarded as one of the few favorable economic policy developments in the 1970s: floating exchange rates. The European Monetary System (EMS) was set up at the end of 1978, to commence in 1979. But, for now, the United Kingdom would not be participating. The Callaghan government had decided to stick to a floating pound.

Democracy. Like Keynes before him, Friedman in his work talked about the damaging effects that inflation could have on the stability of a democracy. For example, Friedman opened his testimony to Congress in May 1959 with the following: "Unless we can achieve both a reasonably stable economy in the short run and a reasonably stable price level in the long run, our free enterprise economy is unlikely to be permitted to survive."[91] In further testimony later that year, Friedman said, "Wars aside, the chief economic

[90] *Milton Friedman Speaks*, Episode 8, p. 26 of transcript.

threats to the preservation of a free society have come from the sharp fluctuations…in economic activity and in prices…that have threatened to tear the social fabric asunder."[92] Another theme in his work, central to Friedman (1962a), was the presence of a sizable private sector as a necessary condition for political freedom.

Friedman produced a storm when, in 1976, he made use of these two themes to discuss the state of the United Kingdom. On *Meet the Press* (October 24, 1976), Friedman said, "Great Britain is another horrible example…Britain is on the verge of collapse." Around the same time, *Encounter* magazine published an article by Friedman, arguing that the public sector had become so large a fraction of the U.K. economy that democracy was threatened: "I fear very much that the odds are at least 50-50 that within the next five years British freedom and democracy, as we have seen it, will be destroyed."[93] The controversy intensified when Friedman made similar remarks in a *60 Minutes* special on the U.K. economy broadcast in the United States on November 28.

Friedman's observations produced a backlash in the U.K. press. The *Daily Mirror* called Friedman the "smiling man of woe," and an editorial criticized his "biased view" and "doomsday solutions" (November 30, 1976a and 1976b). The *Daily Express* (November 30, 1976) said that Friedman's "sensible followers in this country—particularly Mrs. Thatcher and Sir Keith Joseph—must be in near despair" about his "absurdities." John Kenneth Galbraith joined in the criticism, observing, "If the economists were right every time they predicted a country was going down the drain, there would be nowhere left" (*Daily Mirror*, January 10, 1977).

The criticism that prompted Friedman to react came from Samuel Brittan. Brittan published an open letter to Friedman in his *Financial Times* column, arguing that his "recent warnings about the United Kingdom…represent personal hunches, individual value judgments or exaggerations" that could detract from Friedman's insights on monetary matters (*Financial Times*, December 2, 1976). Friedman in turn had an "Open Reply" to Brittan defending his statements (*Financial Times*, January 6, 1977).

This backlash against Friedman's warnings reflected a certain inconsistency on the part of U.K. commentators. A substantial number of U.K. leaders and U.K. economists had made comments similar to Friedman's about the threat to democracy coming from economic instability. For example, in a September 1976 television interview, Prime Minister Callaghan had said, "If we were to fail, I don't think another government could succeed. I do not think that would mean a National [coalition] government. I fear it would lead to a totalitarian government of the Left or the Right" (*The Sun*, October 1, 1976). An economist at the Bank of England, Charles Goodhart, had warned (1975, p. 221) that continued stagflation of the sort the Western economies had faced in the 1970s "may well serve to destroy the atomistic, democratic, capitalist structure of their existing system."

One element that contributed to the controversy was Friedman's emphasis on the threat to democracy from a large government sector rather than from inflation alone. Friedman was not, however, conflating the issues; he explicitly maintained that excessive growth of government did social harm even if it were not accompanied by inflation. "Ending inflation, in my opinion," Friedman said in 1981, "is a very desirable thing to do. In my opinion, it is likely to be a necessary precondition for resolving the other problems that countries have, but it is not a be-all and end-all of economic policy" (*Evening Post*, April 27, 1981). In particular, Friedman contended that an inexorable rise in the government spending share of GDP, "even if were accomplished without any inflation whatever…would ultimately destroy our freedom and society" (*Evening Capital*, November 18, 1978).[94]

It was Friedman's discussion of the U.K. government-to-GDP share that became the matter for which a number of commentators took him to task.

[91] Friedman, May 25, 1959, testimony in Joint Economic Committee (1959a, p. 605; p. 136 of reprint).

[92] October 30, 1959, testimony, in Joint Economic Committee (1959b, p. 3020).

[93] Friedman (1976c, p. 9); also published in *Sunday Telegraph*, October 31, 1976.

[94] A prior occasion on which Friedman separated the issues of inflation and the "threat to the maintenance of a free society" from a large public sector was in a letter to *The Economist* (September 28, 1974).

The figure he used in Friedman (1976c) and elsewhere of 60 percent was indeed the figure reported by U.K. government publications as of early 1976. But revisions during the year exposed double-counting, and the official estimate was revised to about 45 percent to 47 percent. Chancellor Healey said that the corrected number refuted "the picture of a profligate public sector as ignorantly presented by Professor Milton Friedman" (*House of Commons Debates*, November 30, 1976, p. 715). Some discussions, such as Begg (1987) and Tomlinson (1990), mention Friedman's use of the 60 percent figure and create the impression that, had the corrected number been known from the start, there would have been no basis for Friedman's warnings about public expenditure in the United Kingdom. This is questionable; for one thing, Friedman stressed that his argument did not rest on the present number being as high as 60 percent (BBC1, December 6, 1976). For another, Friedman would likely not have agreed with all the statistical decisions used to reach the 45 percent to 47 percent share.

Friedman likely would have insisted that subsidies to firms and transfer payments to individuals be counted in the government spending aggregate, and not (as is often the practice) as "negative taxes." My suspicion is that an estimate that classed these items as spending—and which was sure to include all government enterprises in the government-spending estimate—would show the share peaking above 50 percent during the mid-1970s.

The Thatcher Government. "It's not my job to persuade people about things," Friedman argued (*Omaha World-Herald*, October 20, 1976; "I just develop ideas and leave them around for people to pick up." Among those seen as picking up Friedman's ideas in the late 1970s was Margaret Thatcher, who had replaced Edward Heath as Conservative Party leader in February 1975. Some have claimed that Thatcher had monetarist ideas even in the late 1960s (Wapshott and Brock, 1983, pp. 88, 187). But the Thatcher statements offered as evidence on this point are similar to those common among politicians at the time—that is, she urged giving monetary policy greater weight among the tools used for demand management. The monetarist view of inflation was not present in Thatcher's 1960s' statements. But there is no doubt that the position on inflation taken by Thatcher and other senior Opposition personnel converged in the late 1970s toward the familiar monetarist one, and that the policy framework of the Conservative Party on returning to office in 1979 was shaped by the monetarist position on inflation. Friedman himself is said to have first had detailed conversations with Thatcher in 1978 (Campbell, 2000, p. 372). These probably took place during his April 1978 visit to the United Kingdom.

Friedman, as discussed previously, was critical of the U.K. Conservative Party's historical record on economic policymaking. The impact of his ideas on Conservative Party policy formation after 1975 did not come in for universal welcome on the part of conservatives in the United Kingdom. The most well-known critics were Edward Heath and other Conservative Party advocates of incomes policy to fight inflation.[95]

But the economic substance of Friedman's arguments was nonpartisan. As early as 1968, Robert Solow noted, "the association of monetarism with right-wing politics is not at all necessary" (*The Times*, December 23, 1968). Friedman's own observations were in emphatic agreement with Solow's assessment. For example, Friedman (1978) argued: "No doubt there are strong ideological elements in the susceptibility of individuals, including politicians and their advisors, to persuasion by either the monetarist or Keynesian views. Yet the basic issue is scientific, not ideological…Whatever a man's objectives, whatever his ideology, he can pursue them more intelligently the better he understands how the world works."

A specific scientific question underlay much of the U.K. political debate from 1974 onward. The issue was whether incomes policy was a valid weapon against inflation or whether instead only monetary policy could accomplish disinflation. The outcome of this debate rested on the scientific question of whether inflation

[95] The projected companion paper covering 1979-2006 will deal in detail with Heath's disagreements with Friedman, including those covered in their radio debate in 1980.

was a monetary phenomenon. One Labour-supporting writer recognized the scientific aspect to the debate in 1974, observing: "There is a danger that socialists will dismiss the monetarists' argument simply because that school of thought has hitherto been associated with right-wing conservatism. This is because the leading monetarist, Professor Milton Friedman, has some very eccentric right-wing views. But, in fact, the analysis of the rate of inflation is in no way related to ideological conservatism…Socialists will have to come to terms with this school of thought if we are to effectively fight inflation" (*New Statesman*, October 25, 1974).

Governments of both political parties in the United Kingdom eventually assigned inflation control to monetary policy. This reflected not the triumph of ideology, but the fact that policymakers wanted inflation down, and had accepted that, as a technical matter, the only way this could be accomplished was through monetary policy. As Friedman put it, "It's not what I advocate that matters; there is only one way to do it" (*St. Louis Globe-Democrat*, December 16, 1977).

That convergence of the political parties' positions had not yet occurred when the 1979 U.K. general election was held. The Callaghan government went to the election with incomes policy prominent in its platform, including a new union/government agreement on wages to replace the Social Contract, and plans to extend price control, while the Conservative Party under Thatcher rejected incomes policy in favor of a focus on monetary control. Interviewed by BBC television several months after Thatcher's election victory, Friedman underlined the change in direction, both with respect to monetary policy and to the role of government, implied by Thatcher's coming to power. "If the Thatcher government succeeds," Friedman said, "it will be an example that will not be lost on the United States or the rest of the world."[96]

CONCLUSION

The U.K. banking periodical *Midland Bank Review* commented in its Summer 1979 issue on the implications of the change of government for the direction of economic policy. "The mantle of Keynes, and particularly the embroideries of his followers, appears to have worn thin; and the mode has shifted towards the sterner lines of thought popularized by Milton Friedman. The new Government comes to office in a climate of opinion very different from that which influenced its Conservative predecessor." Nevertheless, Nicholas Kaldor was able to boast accurately that Friedman had "made comparatively few converts among academic economists" in the United Kingdom.[97] The support for Friedman's ideas was also thin among members of the new government, once one looked below the most senior levels. In these circumstances, and despite his drawing of distinctions between his own positions and those of the Thatcher government, Friedman would find himself a central figure in the debate over the new economic policies. In 1979 he was about to shift to an even-higher profile in the United Kingdom and, in defending his positions on monetary policy and on the role of government, would encounter in debate some of the most formidable figures in U.K. economics and some of the biggest names in both major political parties.

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[96] October 11, 1979, broadcast of interview with Friedman on BBC television program *Newsweek*.

[97] Kaldor (1982, p. xxii).

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II

Do Macroeconomic Announcements Move Inflation Forecasts?

Marlene Amstad and Andreas M. Fischer

This paper presents an empirical strategy that bridges the gap between event studies and macroeconomic forecasts based on common-factor models. Event studies examine the response of financial variables to a market-sensitive "surprise" component using a narrow event window. The authors argue that these features—narrow event window and surprise component—can be easily embedded in common-factor models that study the real-time impact of macroeconomic announcements on key policy variables such as inflation or gross domestic product growth. Demonstrative applications are provided for Swiss inflation that show that (i) the communication of monetary policy announcements generates an asymmetric response for inflation forecasts, (ii) the pass-through effect of import price releases on inflation forecasts is weak, and (iii) macroeconomic releases of real and nominal variables generate nonsynchronized effects for inflation forecasts. (JEL E37, E52, E58)

Federal Reserve Bank of St. Louis *Review*, September/October 2009, *91*(5, Part 2), pp. 507-18.

An attractive feature of diffusion indices is their ability to embed timely information from macroeconomic releases. Studies using common-factor procedures by Forni et al. (2000) and Stock and Watson (2002) show that updated forecasts have lower forecast errors because additional observations from macroeconomic releases are included in a growing panel. Evans (2005) and Giannone, Reichlin, and Small (2008) develop a procedure that updates quarterly U.S. gross domestic product (GDP) nowcasts (i.e., forecasts for the current quarter) as information from staggered macroeconomic releases arrives. Similarly, Altissimo et al. (2007) argue that integration of early information at a monthly frequency improves quarterly GDP nowcasts for the euro area. At a higher frequency, Amstad and Fischer (2009a) show that weekly updates enhance the forecast accuracy for monthly Swiss inflation. These studies argue that sequentially updating the forecast on incoming macroeconomic information is informative for analysts monitoring nominal and real activity.

A drawback of diffusion indices is that they are statistical models without economic structure. A naive method of uncovering the driving forces behind forecasts from common-factor models compares the forecasting performance between included and excluded variable blocks in the panel. Forni et al. (2001) use this method to show that financial variables are important for inflation forecasts. Analogous to the naive method, the impact of macroeconomic announcements on indices can be interpreted using an event study framework. The "impact effect" is defined as the difference between the forecast conditional on

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information *after* the macroeconomic release and the forecast conditional on information *before* the macroeconomic release. Event studies, which measure the impact of an economic event on a variable of interest, have a rich tradition in macroeconomics and finance.[1] These studies often work with a narrow event window to capture the financial market response to an announcement surprise component. In a similar manner, forecast innovations from a common-factor model centered on a macroeconomic release with a narrow and fixed event window lend themselves readily to an event study interpretation.

Our objective here is to bridge the gap between event studies examining the impact of macro announcements for financial variables and conventional macro models embodying a broad range of macroeconomic information. More specifically, we want to know whether macroeconomic announcement effects for a narrow event window have a strong impact on the inflation forecast. It is no surprise that wide event windows—say, more than one month—generate large forecast revisions, but it is unclear whether the same is true for narrow event windows of one day. The proposed identification procedure relies on generating forecast innovations for the macroeconomic series based on panels updated on a daily basis using the dynamic common-factor procedure developed by Forni et al. (2000). The one-day event window defined by the postrelease and prerelease dates of macroeconomic releases allows interpretation of the announcement's impact on inflation forecasts.

The advantages of our procedure over previous event studies that analyze announcement effects are twofold. The first concerns the information breadth captured in the anticipated component of the event. The pre-event forecast from the common-factor model is projected on a data-rich environment, whereas previous event studies rely on information from simple ordinary least squares regressions and survey data or have no prior information. The second advantage is that announcement effects can be analyzed for a wide range of variables. They include all the variables in the panel. Previous event studies focused exclusively on financial variables to capture the announcement effect.

The empirical analysis considers three applications of the event study procedure to Swiss inflation. The case studies are demonstrative, reflecting the view that the proposed framework has broad applications. The first exercise examines the size of forecast innovations on days when the Swiss National Bank (SNB) announces its target range for its policy interest rate. Numerous studies surveyed by Blinder et al. (2008) have examined the response of financial markets to central bank communications but not whether central bank communications can have an impact on the inflation forecast through the market's response and subsequent effect on financial variables. We want to know whether the financial variables in the panel respond to SNB announcements and, in turn, influence the inflation forecast. The second exercise investigates whether forecast errors generated by the release of real and nominal macroeconomic variables influence inflation forecasts in a synchronous manner. With this information we want to understand how forecasts behave over the cycle. The third exercise analyzes whether inflation forecasts respond to import price releases. We argue that the forecast innovation centered on import price releases can be interpreted as an alternative measure of the pass through from import prices to consumer prices.

The paper is organized as follows. The next section outlines the event study procedure for common-factor models used for real-time forecasting. Then we discuss the structure of the panels and the forecasting windows and the event-study applications of common-factor models to Swiss inflation.

THE IDENTIFICATION SCHEME

The identification scheme to analyze announcement effects in macroeconomic models with data-rich environments involves the following steps. The first step generates the projection

[1] See MacKinlay (1997) for a survey of the literature and empirical tests.

for the variable of interest one day before the release of macroeconomic information. The projections are based on panels that encompass real-time information from financial variables and data releases that are updated on a daily basis. Estimation follows the dynamic common-factor procedure by Forni et al. (2000). The second step reestimates the projections for the variable of interest one day later that include cross-sectional information from the macroeconomic release. The third step constructs the forecast innovation linked to the announcement surprise—that is, the one-day difference in the two projections. The main steps of the estimation procedure are defined using the terminology of MacKinlay (1997).

Defining the Event

The monthly release of macroeconomic variables is defined as the event with the kth event date $\tau_k = (j,t)$ corresponding to day j and month t in calendar time and $k = \{1,\ldots, K\}$. We assume that new information attributed to the event stems from the monthly macroeconomic release. This assumption means that updated panels at the time of the event are not subject to data revisions on days when macroeconomic information is released. Ideally, we focus on macroeconomic releases that are large in the cross section (i.e., the consumer price index [CPI] and its subcomponents) to reduce the influence of measurement error in estimation.

Estimation

The empirical model relies on data-reduction techniques that can handle real-time panels that are updated daily. We follow the estimation procedures of Forni et al. (2000), Cristadoro et al. (2005), and Altissimo et al. (2001). We provide an informal outline of the estimation procedure, but readers may refer to the individual papers for specific details.

As in Forni et al. (2000) and following their notation, we assume that the factor structure has N variables in the generic panel, $\mathbf{x}_t = (x_{1,t}, x_{2,t},\ldots, x_{N,t})'$, where $x_{1,t}$ is the variable of interest. In most cases, $x_{1,t}$ is either inflation or output. The variables in the panel are first differenced when necessary for stationarity purposes. Next, $x_{1,t}$ is assumed to be the sum of two unobservable components: a signal, $x^*_{1,t}$, and a component capturing short-run dynamics, seasonality, measurement error, and idiosyncratic shocks, $e_{i,t}$:

$$x_{1,t} = x^*_{1,t} + e_{1,t}. \tag{1}$$

The objective of the generalized dynamic factor model of Forni et al. (2000) is to estimate the signal, $x^*_{1,t}$, in equation (1) using information from the present and past of the x's (i.e., a contemporaneous linear combination of the x's).

More formally, it is assumed that the variables in equation (1), $x_{1,t}$, can be represented as the sum of two stationary, mutually orthogonal, unobserved components. The first component is the common component, $\chi_{i,t}$, which is assumed to capture a high degree of comovement between the variables in the panel, $\mathbf{x}_t$. The second component is the idiosyncratic component, $\xi_{i,t}$. The common component is defined by q common factors, $u_{h,t}$, that are possibly loaded with different coefficients and (finite) lag structures, say, of order s. Formally, Forni et al. (2000) specify $x_{1,t}$ as a generalized dynamic factor model:

$$x_{i,t} = \chi_{i,t} + \xi_{i,t} = \sum_{h=1}^{q}\sum_{k=0}^{s} b_{i,h,k} u_{h,t-k} + \xi_{i,t}, \tag{2}$$

where $\xi_{i,t}$ is the idiosyncratic component and $\chi_{i,t} = x_{i,t} - \xi_{i,t}$ is the common component.

The estimation procedure as in Cristadoro et al. (2005) involves three steps. The first step estimates the common factors. In particular, the cross spectra for the common component of $\chi_{1,t}$ are estimated following Forni et al. (2000). The second step computes the implied covariance of $\chi_{1,t}$ and the factors by integrating the cross spectra over a specified frequency band. The last step involves performing a linear projection of the common component on the present and the lags of the common factors:

$$\hat{\chi}_{1,t} = Proj\left[\chi_{1,t} \middle| u_{h,t-k}, h = \{1,\cdots,q; k = 0,\cdots,s\}\right]. \tag{3}$$

To generate the projections at time t, we apply the shifting procedure for the covariance matrix by Altissimo et al. (2001; see their Appendix B.4 on filling in incomplete observations for unbal-

anced panels).[2] Altissimo et al. (2001) compute values of $\hat{\chi}_{i,t}$ g months ahead by individually shifting each series in $x_{i,t}$ so that the most recent observation aligns g months ahead to form a balanced panel. Afterward the generalized principal component is evaluated for the realigned $x_{i,t}$.

Announcement Surprise Component

The event study literature frequently uses the term "abnormal returns" for the response of financial variables to an examined event. This is defined as the actual ex post return of the (financial) variable over the event window minus the normal return—the return that would be expected if the event did not take place. Instead of returns, we work with innovations of the projections. Thus, to identify the influence of new information from monthly releases in import prices, a measure of innovations for event date $\tau_k = (j,t)$ is needed. This is defined as the one-day difference in the projections of $\hat{\chi}_{1,t}$ around the event (i.e., the release dates). More specifically, $\varepsilon_{1,t}$ is the innovation from the projections for $\hat{\chi}_{1,t \mid P_{j,t}}$ conditional on the daily panel, $P_{j,t}$, before and after the release of the macroeconomic variable on day j in month t:

$$\hat{\varepsilon}_{1,t} = \hat{\chi}_{1,t|P_{j,t}} - \hat{\chi}_{1,t|P_{j-1,t}}. \tag{4}$$

Similarly, the h-ahead forecast innovation for $h > 0$ is $\hat{\varepsilon}_{1,t+h} = \hat{\chi}_{1,t+h \mid P_{j,t}} - \hat{\chi}_{1,t+h \mid P_{j-1,t}}$. Equation (4) represents the full-day impact from the macroeconomic announcement.[3]

The anticipated component for inflation in equation (4), $\hat{\chi}_{1,t \mid P_{j-1,t}}$, is conditional on a broad range of information. In a similar manner, the anticipated component can be derived for any variable in the panel, $P_{j-1,t}$. This represents an improvement over earlier studies reviewed in MacKinlay (1997) that used survey data or simple regression techniques projected on a handful of variables to generate the anticipated component. As discussed in Rigobon and Sack (2008), anticipated components using survey data are problematic because of irregular timing and a limited number of surveyed panelists. Rigobon and Sack argue that these problems contaminate the surprise terms and generate a biased impact effect in event studies. They propose an error-in-variables procedure to overcome these problems stemming from survey data. Equation 4 has no such problems.

THE (DAILY) PANELS AND DATA RELEASES

All economic series used to construct the data panels are from the SNB's data bank. The dataset's construction is intended to replicate the contours of a data-rich environment in which the SNB operates.

The Panels

Because we are concerned with the problem of how to weigh the most recent information against what we already know at daily intervals, we are interested in economic data that are frequently released, which means working with data with a daily or monthly frequency. Table 1 shows the breakdown of the 434 series into nominal and real variables and their frequency. There are 27 financial variables at the daily frequency and 407 nominal and real variables at the monthly frequency. Quarterly variables such as industrial production or GDP were intentionally excluded to avoid contaminating our estimates with revision errors.[4]

Two types of panels are constructed. The first uses end-of-month data from 1993:05 to 2003:11; we generate our initial forecasts with this panel. After 2003:11:01, we update the panels daily. The starting date 1993:05 is chosen because a large number of series do not go farther back than 1990 and 1993:05 coincides with a major revision in the CPI.

An explicit intention in constructing the dataset was to transform the series as little as possible.

[2] Giannone, Reichlin, and Small (2008) offer an alternative procedure for forecasts of the common component based on the Kalman filter, which are qualitatively the same.

[3] Event studies frequently analyze the immediate impact, which is generally defined as the market response 30 minutes before and 30 minutes after the macroeconomic announcement, rather than the full-day impact.

[4] See Amstad and Fischer (2009a) for a discussion of data revision at the monthly frequency. Also, preliminary estimates revealed that the introduction of the quarterly information from GDP or industrial production did not alter our estimates.

Table 1

Data and Release Frequencies

Data category	Release frequency		
	Monthly	Daily	Total
Nominal			254
Prices (CPI total, subcomponents, cores)	178		
Money	9		
Financial	6	9	
Interest rates	12	11	
Exchange rates	4	3	
Foreign prices	10		
Foreign interest rates	8	4	
Real			180
Survey	40		
External trade	83		
Labor	14		
Demand	16		
Foreign industrial production	8		
Foreign labor market	19		
Total	407	27	434

First, we undertake no seasonal filtering because of its reliance on future information. Amstad and Fischer (2009a) demonstrate that seasonal adjustment can be treated through band-pass filtering. The absence of seasonal revisions allows better interpretation of the forecast innovations.

Several data transformations, however, were necessary at the initial stages of estimation. The series were filtered in the following manner. First, logarithms were taken for nonnegative series that were not in rates or in percentage units to account for possible heteroskedasticity. Second, the series were first-differenced, if necessary, to account for stochastic trends. Third, the series were taken in deviation from the mean and divided by their standard deviation to remove scalar effects.

Clustered Data Releases

Figure 1 provides an example of the clustering of macroeconomic releases for December 2003. The number of data releases for a particular day is listed on the vertical axis with the calendar dates denoted on the horizontal axis. The releases are divided into nominal (shaded bars) and real variables (open bars). Of interest are the clusterings on December 2 and 19. The first spike stems from CPI releases and their subcomponents, whereas the second is the result of the release of trade volumes across sectors.

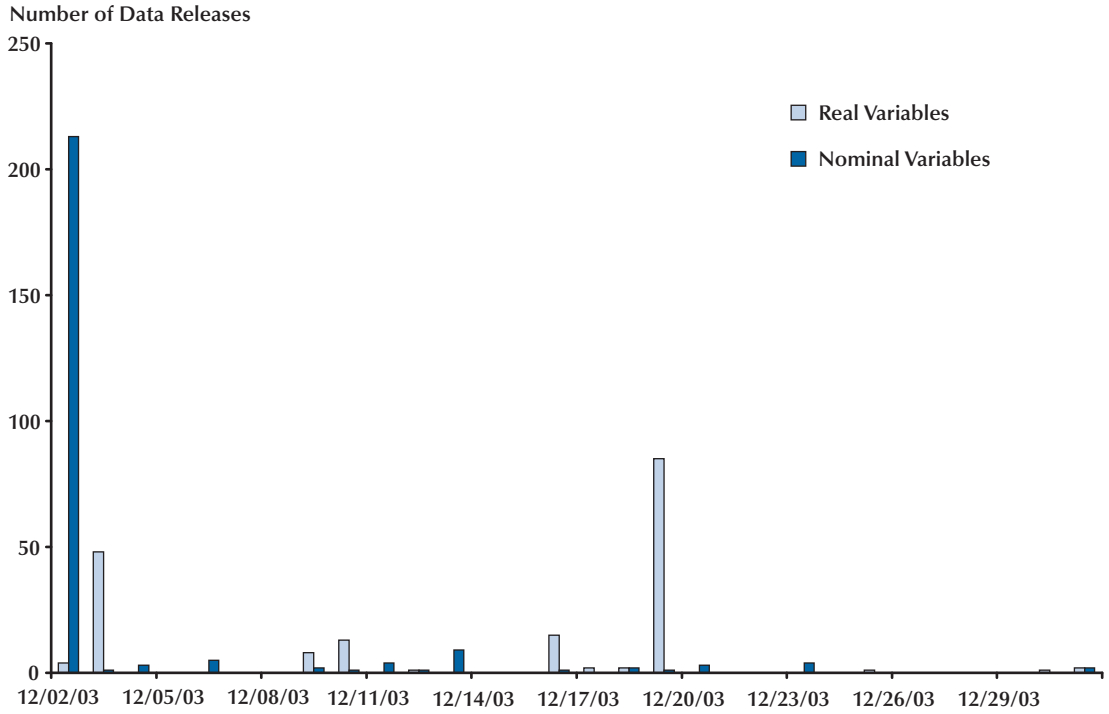
APPLICATIONS TO SWISS INFLATION FORECASTS

This section presents three empirical applications of analyzing the impact of macroeconomic announcement effects on Swiss CPI inflation.[5] The case studies were chosen to reflect the view that the event study framework for common-factor

[5] The empirical model is defined in Amstad and Fischer (2009a). The same paper provides forecasting properties for a model with 12 static factors and 2 dynamic factors. Inflation is annualized and uses a band-pass filter at $2\pi/12$ to remove seasonality. This is also the same dataset and estimation procedure used to estimate the SNB's monthly measure of core inflation, called dynamic factor inflation. See page O15 of the SNB's *Monthly Statistical Bulletin*.

Figure 1

Nominal and Real Data Releases for December 2003



models has broad applications. The first exercise considers forecast innovations generated on days when the SNB announced its target range for the 3-month London Interbank Offering Rate (LIBOR) in 2004. In particular, we are interested in how financial variables respond to SNB communication and its impact on the inflation forecast. The second application asks whether forecast innovations generated by data releases of real and nominal variables to CPI inflation are synchronized. In other words, do the data releases from real and nominal variables influence the inflation forecast in a similar manner? The last application examines whether forecast innovations generated by import price releases influence CPI inflation. In particular, we want to know whether the impact is similar in magnitude to pass-through ratios estimated in other studies that use traditional time-series methods.

SNB Announcement Surprises in 2004

The SNB defines a target range of 100 basis points for the 3-month LIBOR as its operating target. To steer the LIBOR within the target range, the SNB sets the 1-week repurchase (repo) rate. Four times per year on scheduled dates, the SNB releases a policy statement in which it announces a change or no change in the target range.[6] In 2004, the announcement dates were March 18, June 17, September 16, and December 16. We use these four policy dates to generate the SNB announcement surprises. The SNB "announcement surprise" is defined as the one-day difference in the inflation forecast conditional on postrelease information minus the inflation forecast conditional on prerelease information. The difference in this information set captures only information from

[6] Outside these prearranged dates, the SNB reserves the right to change the target range.

(daily) financial variables and their reaction to the policy statement (i.e., no releases of macroeconomic data were made public on the four SNB policy dates). These differences in the panels pertain to daily updates in the 27 financial variables in our panel.

Figure 2 plots innovations of 24-month-ahead inflation forecasts at the time of the four SNB announcement dates. In June and September the SNB's board of directors raised the target range by 25 basis points, whereas in March and in December the target range was left unchanged. The responses to the SNB announcement surprises differ considerably. For the March release, there is no change in the forecast. However, for the dates when the SNB raised its target range, we observe a strong response in the inflation forecast but in opposite directions. Contractionary behavior is observed for the June rate hike and expansionary behavior for the September rate hike. For the last announcement surprise in December, we observe a weak but expansionary response to the "no-change" decision. Although the forecast innovations on days when changes to the target range are larger than on days with no changes to the target range, we do not find them to be statistically significant compared with forecast innovations on SNB days in the years between 2000 and 2003. Next, we focus on the direction of the forecast innovation.

How do we explain the differing reactions to the change and no-change decisions in the target range? The release dates that signal a change in the target range account for larger reactions in the inflation forecast. The stronger forecast response on SNB days with a change in the target range rests on the fact that many financial contracts in Switzerland (i.e., automobile leases, home and commercial property loans) are tied to the 3-month LIBOR. To determine the innovation's direction, it is necessary to control for what the markets had anticipated. As in Hamilton and Jorda (2002), one possible method (aside from the projection one day before the SNB announcement day) is to use a spread of the SNB's policy rates: the 3-month LIBOR rate minus the repo rate. This interest rate spread is plotted in Figure 3 along with the midpoint in the SNB's target range for the 3-month LIBOR.[7] The interest rate spread shows that the market anticipated the rate hikes in June and September; the spreads widen. For the no-change decisions, the spreads do not change in March and widen slightly before the December policy release.

To understand the postrelease estimate, we need to examine what happens to the spread the day after the SNB policy statements are released. For the March release, the spread does not change between the preforecast and postforecast. This is consistent with the March response of no reaction to the SNB announcement surprises. For the June release, the change in the spread is 0.01, whereas for the September release it is –0.14. In the latter case, the SNB did not raise the repo rates high enough to move the 3-month LIBOR to the midpoint of the target range. In other words, the short end of the yield curve was steeper than was anticipated by the market. This led to a rise in the postrelease estimate of inflation. The response to the December release of no change in the target range is similar to the response for the September release. Although the reaction for September is small, the change in the spread for the postrelease and prerelease dates of –0.04 is consistent with the innovation's direction.

Are Real and Nominal Forecast Innovations Synchronized?

Next, we test whether forecast innovations from data releases of real and nominal variables are synchronized. We generate the forecast innovations from the monthly trade releases (i.e., "real innovations") and the forecast innovation from the monthly CPI releases (i.e., "nominal innovations"). A priori, we do not expect the two types of forecast innovations to be similar. First, the size and dynamics of the individual forecast innovations can differ from month to month. Second, the comovement of real and nominal innovations should not be restricted to be the same for each month. In related empirical studies on the procyclicality of prices in the long run, Backus and Kehoe (1992), Ravn and Sola (1995), and Smith (1992) find that the cyclical properties of prices and output are not stable.

[7] The repo rate is either the 1-week or the 2-week repo rate; in most cases, it is the former. See Dueker and Fischer (2005).

Figure 2

Forecast Innovations of SNB Announcements to the Target Range

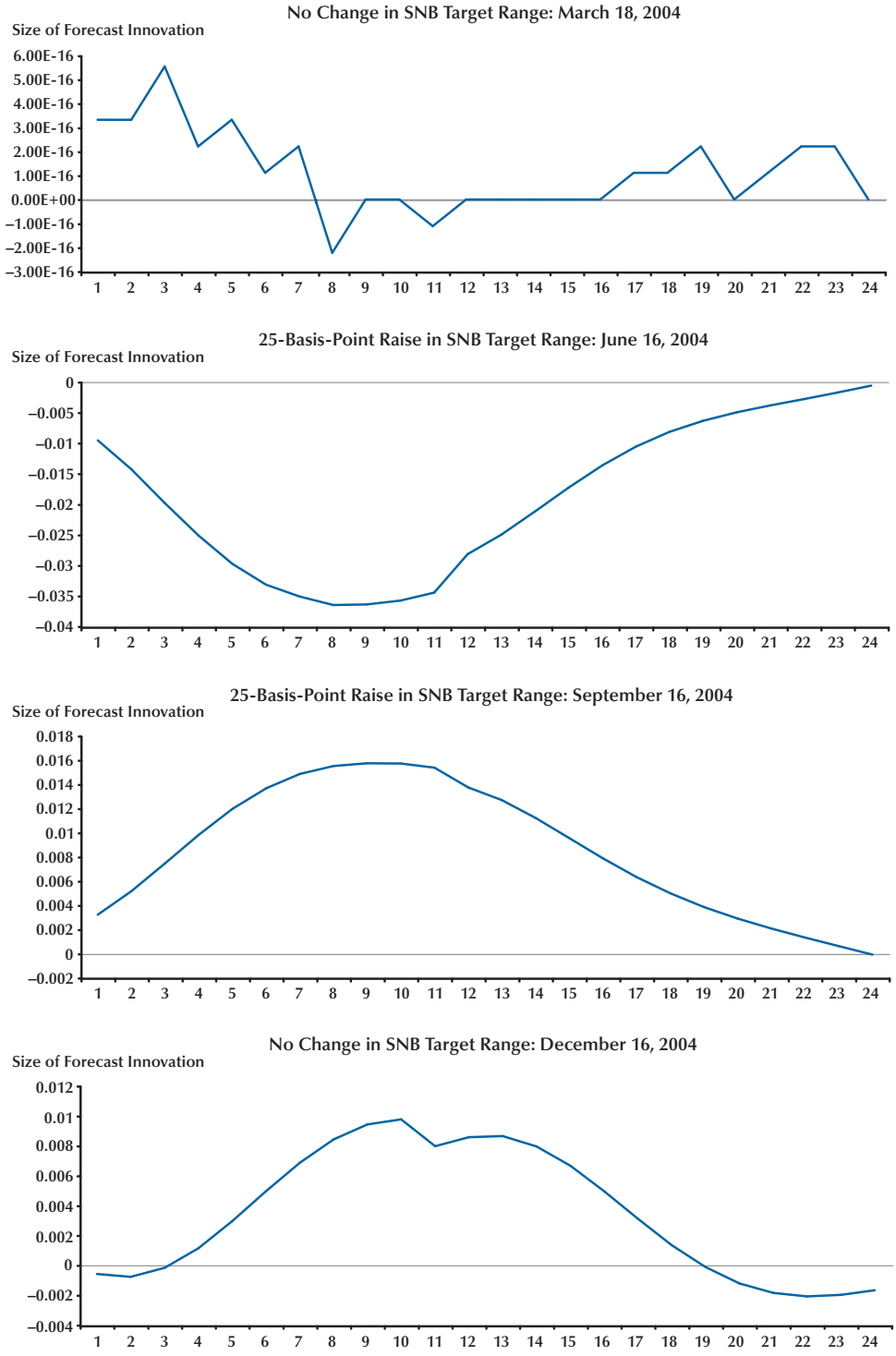
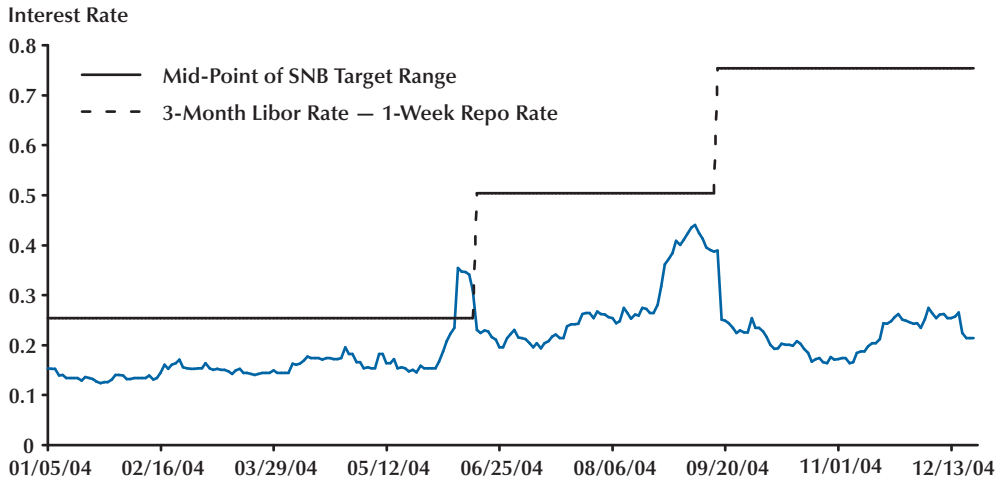


Figure 3

SNB Policy Rates in 2004



SOURCE: Dueker and Fischer (2005).

Table 2

Synchronization of Forecast Innovations from Nominal and Real Variable Releases

Forecast innovations	November 2004	October 2004	September 2004	August 2004	July 2004	June 2004
$\varepsilon^{n}_{\pi,t+h\|j,t}$, $\varepsilon^{r}_{\pi,t+h\|k,t}$	0.174	0.348	0.130	0.348	0.826	0.565
$\varepsilon^{n}_{\pi,t+h\|j,t}$, $\varepsilon^{n}_{\pi,t+h\|j-1,t}$	0.522	0.522	0.870	0.822	0.391	0.261
$\varepsilon^{r}_{\pi,t+h\|k,t}$, $\varepsilon^{r}_{\pi,t+h\|k-1,t}$	0.610	0.740	0.565	0.478	0.652	0.434

NOTE: The forecast innovations generated by real and nominal variable releases are denoted by $\varepsilon^{r}_{\pi,t+h|k,t}$ and $\varepsilon^{n}_{\pi,t+h|j,t}$. The index for concordance by Harding and Pagan (2002) lies between 0 (countercyclical) and 1 (procyclical). The index is calculated for the months June through November 2004.

To test whether the two types of forecast innovations are synchronous, we calculate the concordance index of Harding and Pagan (2002). The application of the index examines whether the comovement of real and nominal innovations can be quantified by the fraction that both series are simultaneously in the same state of expansion ($S_t = 1$) or contraction ($S_t = 0$) with the index,

$$I_{1,2} = \frac{\sum_{t=1}^{24} S_{1,t}S_{2,t} + \left(1-S_{1,t}\right)\left(1-S_{1,t}\right)}{24},$$

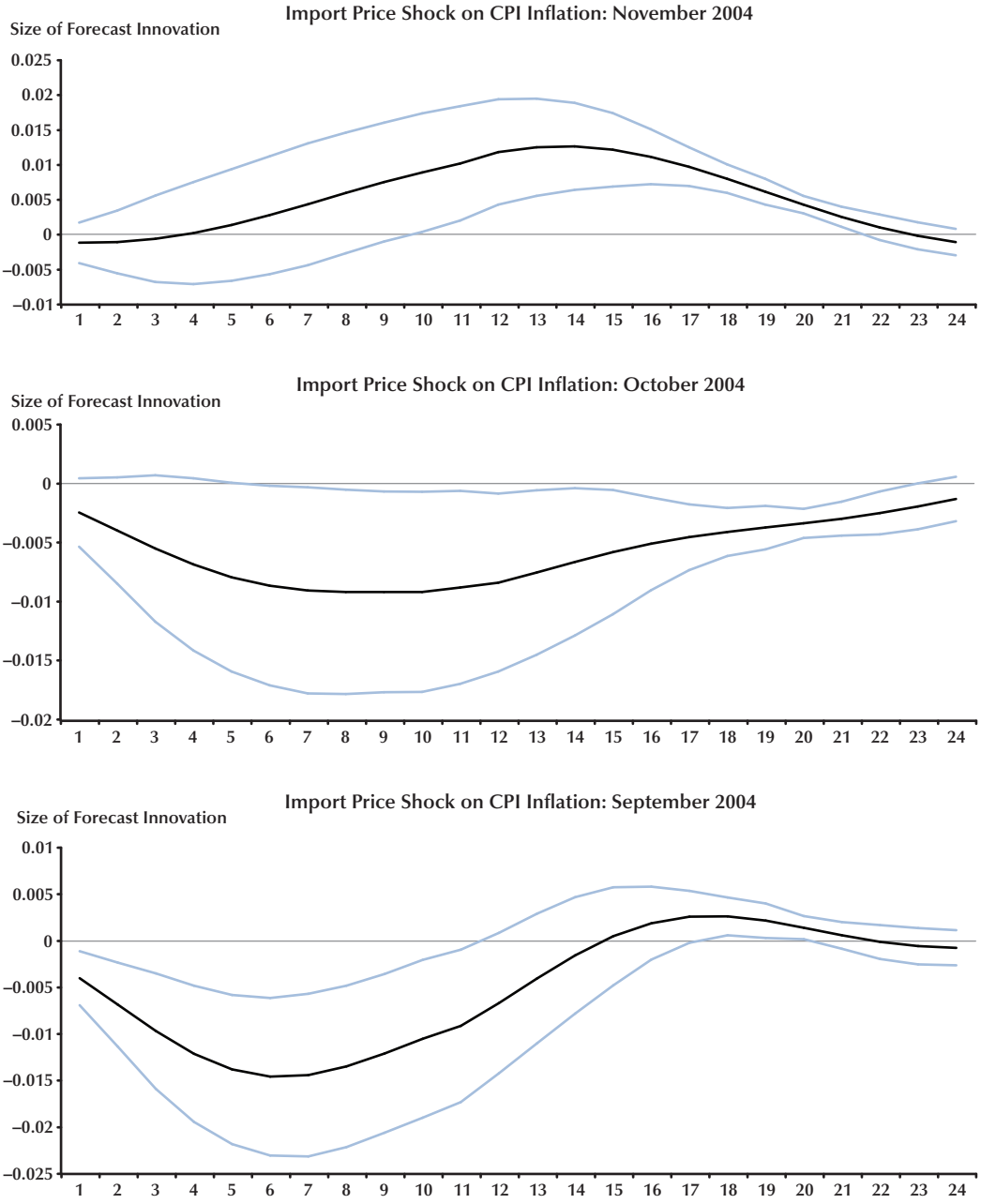
measuring the degree of concordance between series 1 and 2, which are $\varepsilon^{r}_{\pi,t+h|k,t}$ and $\varepsilon^{n}_{\pi,t+h|j,t}$ in our case.[8]

The concordance index can be used to determine whether nominal and real innovations to inflation are procyclical or countercyclical. If they are exactly procyclical, then the index is unity, whereas a zero value denotes evidence of countercyclical behavior. Table 2 presents the

[8] The concordance index has similar properties as the Cowles-Jones test used for testing an i.i.d. random walk process.

Figure 4

Exchange Rate Pass Through



degree of concordance between $\varepsilon^{r}_{\pi,t+h|k,t}$ and $\varepsilon^{n}_{\pi,t+h|j,t}$ for June to November 2004. In the first row of the table, the index values for $\varepsilon^{r}_{\pi,t+h|k,t}$ and $\varepsilon^{n}_{\pi,t+h|j,t}$ show that the innovations behaved in a procyclical manner in June and July, but the real and nominal innovations to inflation behaved in a countercyclical manner from August through November. In the second and the third rows of the table, information on the persistence of the innovations is given by constructing the index for $\varepsilon^{n}_{\pi,t+h|j,t}$ and $\varepsilon^{n}_{\pi,t+h|j-1,t}$ and $\varepsilon^{r}_{\pi,t+h|k,t}$ and $\varepsilon^{r}_{\pi,t+h|k-1,t}$. Here, the evidence shows that the likelihood of the two types of forecast innovations behaving in the same manner (as in the previous month) is stronger for real innovations than for nominal innovations to inflation. In other words, the forecast innovations from *real* data releases demonstrate a higher level of persistence than do forecast innovations from *nominal* data releases.

Do Inflation Forecasts Respond to Releases in Import Prices?

The response of CPI inflation forecasts to import price releases should be informative about the pass through from import prices to consumer prices.[9] In our setup, the forecast innovation around the import price release is defined as the difference in the 24-month-ahead forecasts in CPI inflation based on the daily panel that includes the postrelease information from import prices and the previous day's panel that entails information from the prerelease.

Figure 4 shows the response of CPI inflation to new information from import price releases for November, October, and September 2004. A one-standard-deviation band, based on past innovations since December 2003, is depicted around the forecast's response. The evidence indicates that the pass through under this measure is small. In November and October, the innovations of the import prices were slightly negative for the first 15 months and zero thereafter. The response for September was stronger; again the effect of import prices is absorbed within 18 months.

The finding that the Swiss pass through is weak in 2004:Q4 does not contradict the cross-country evidence by Campa and Goldberg (2005), Gagnon and Ihrig (2004), and McCarthy (2000). These studies find that the pass through for Swiss prices is surprisingly small compared with the empirical evidence for other small open economies.

[9] This section relies heavily on Amstad and Fischer (2009b).

CONCLUSION

Understanding the influence of real-time information on inflation forecasts is vital for policymakers. The proposed forecasting framework based on the common-factor procedure with daily updated panels is a step in this direction. As in event studies that focus on the response of high-frequency financial data to new information around a narrow event window, the identification scheme herein relies on the recognition that macroeconomic announcement effects can also be interpreted as a forecast innovation with a one-day event window. The case studies for Swiss inflation demonstrate that the event study framework for common-factor models is flexible to handle numerous applications in real time.

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II

Challenges in Macro-Finance Modeling

Don H. Kim

This article discusses various challenges in the specification and implementation of "macro-finance" models in which macroeconomic variables and term structure variables are modeled together in a no-arbitrage framework. The author classifies macro-finance models into pure latent-factor models ("internal basis models") and models that have observed macroeconomic variables as state variables ("external basis models") and examines the underlying assumptions behind these models. Particular attention is paid to the issue of unspanned short-run fluctuations in macroeconomic variables and their potentially adverse effect on the specification of external basis models. The author also discusses the challenge of addressing features such as structural breaks and time-varying inflation uncertainty. Empirical difficulties in the estimation and evaluation of macro-finance models are also discussed in detail. (JEL E43, E44, G12)

Federal Reserve Bank of St. Louis *Review*, September/October 2009, *91*(5, Part 2), pp. 519-44.

In recent years there has been much interest in developing "macro-finance models," in which yields on nominal bonds are jointly modeled with one or more macroeconomic variables within a no-arbitrage framework. Academic researchers and policymakers alike have long recognized the need to go beyond "nominal yields only" no-arbitrage models (i.e., to include a description of the macroeconomy or other asset prices). Campbell, Lo, and MacKinlay (1996), for example, have emphasized that "as the term structure literature moves forward, it will be important to integrate it with the rest of the asset pricing literature." Policymakers have often used traditional theories such as the expectations hypothesis and the Fisher hypothesis to extract an approximate measure of market expectations of interest rates and macroeconomic variables such as inflation, but they are also aware that risk premia and other factors might complicate the interpretation of the information in the yield curve; policymakers therefore would welcome any progress in term structure modeling that would facilitate greater understanding of the messages in the yield curve.[1]

Despite much exciting work in macro-finance modeling of late,[2] as a central bank economist who monitors markets regularly, I have found it difficult to bring the current generation of models to bear on the practical analysis of bond market developments or to implement the models in real time to obtain a reliable measure of the market's expectation of key variables such as inflation.[3]

[1] See, for example, Bernanke (2004a).

[2] Examples include Ang and Piazzesi (2003), Hördahl, Tristani, and Vestin (2006), Rudebusch and Wu (2003), and Ang, Bekaert, and Wei (2007 and 2008).

[3] I emphasize that I speak as one of many central bank economists and that my views as stated in this paper do not necessarily represent the general consensus among economists at central banks.

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The academic literature provides little evidence in this regard (either for or against macro-finance models). One exception is the recent paper of Ang, Bekaert, and Wei (2007, ABW), who performed an extensive investigation of the out-of-sample inflation forecasting performance of various models and survey forecasts. The authors found that the no-arbitrage models they used perform *worse* than not only survey forecasts but also other types of models.[4]

It thus seems useful to review and discuss various challenges in the specification and implementation of macro-finance models that might help shed light on the lack of documented practicality of macro-finance models in general and on the findings of ABW in particular. To this end, I take a closer look at the role of the no-arbitrage principle in macro-finance models and reconsider the assumptions often made in this literature. The no-arbitrage principle itself is clearly a reasonable assumption, but the models also make *additional* assumptions whose validity may not have been discussed thoroughly in the existing literature. I also discuss "more advanced" issues (such as structural breaks and time-varying volatility) that require going beyond the standard affine-Gaussian framework of most macro-finance models and the challenges encountered in this regard. Much of the challenge in macro-finance modeling is *empirical*; hence, I also discuss at length the difficulties in the implementation stage (estimation and evaluation of models). Although the main focus of this article is the extraction of information from the yield curve (particularly inflation expectations), much of the discussion may be relevant for macro-finance models developed to address other issues, as they share some of the key assumptions discussed in this article.

The state variables in the reduced-form no-arbitrage model framework (on which most macro-finance models are based) can be heuristically viewed as forming a *basis* onto which to project information in yields and other data. Herein I make a distinction between models that use (what I shall call) an "internal basis" versus those that use an "external basis." By an internal basis, I refer to a basis that is determined inside the estimation; hence, it is unknown before the estimation. Latent-factor models that describe inflation expectations and term structure jointly (e.g., Sangvinatsos and Wachter, 2005, and D'Amico, Kim, and Wei, 2008) are examples of internal basis models. By an external basis, I mean a basis that is a priori fixed completely or partially, as when a specific macroeconomic variable (such as inflation) is taken as a state variable. Note that no-arbitrage guarantees the existence of *some* pricing kernel, but it does not mean that the pricing kernel can be represented well by a priori selected variables. I shall argue that external basis models involve strong assumptions, and I discuss potential problems that may occur with their use. All is not well with internal basis models either: The weaker assumptions of these models may come at the cost of the ability to give specific, intuitive interpretation of the yield curve movements. Most important, internal basis models face many empirical difficulties similar to those in the estimation of external basis models, in particular, overfitting and small-sample problems.

The remainder of this article is organized as follows. The next section reviews the standard affine-Gaussian setup of macro-finance models, derives the affine bond pricing formula in a way that emphasizes the replicating portfolio intuition, and introduces a distinction between internal basis models and external basis models. A critical examination of the assumptions in both "low-dimensional" and "high-dimensional" external basis models follows this review. Next, I then discuss the challenge of accommodating nonlinear/non-Gaussian effects, such as structural breaks and time-varying uncertainties, and potential problems with empirical techniques commonly used in the estimation and evaluation of macro-finance models. I then return to *why* surveys perform better than models in inflation forecasting (as documented by ABW).

[4] For example, the root mean square errors (RMSEs) for 1-year consumer price index (CPI) inflation forecasts based on the two no-arbitrage models in ABW (what they refer to as MDL1 and MDL2) are larger than those of autoregressive (AR)(1) and autoregressive moving average (ARMA)(1,1) models by more than 30 percent for the post-1995 window. Furthermore, all but 1 of ABW's 11 regression models that involve term structure variables (what they refer to as TS1-TS11) produce smaller RMSEs in forecasting 1-year CPI (PUNEW) inflation than the no-arbitrage models in the post-1995 sample.

THE BASIC MODEL

Affine-Gaussian Framework

Most macro-finance models in the literature are based on the "affine-Gaussian" model, denoted as

(1)

$$
\begin{aligned}
&m_{t+1} \equiv \log M_t = -r(x_t) - \lambda(x_t)' \varepsilon_{t+1} - \frac{1}{2}\lambda(x_t)' \lambda(x_t) \\
&x_{t+1} = \Phi x_t + (I - \Phi)\mu + \Sigma \varepsilon_{t+1}, \\
&r(x_t) = \rho_o + \rho' x_t \\
&\lambda(x_t) = \lambda_a + \Lambda_b x_t,
\end{aligned}
$$

where M_t is the pricing kernel, x_t is an n-dimensional vector of state variables, r_t is the nominal short rate (i.e., one-period yield), and λ_t is the market price of risk of the n-dimensional shocks ε_{t+1} (Φ, Σ, and Λ_b are $n \times n$ constant matrices, ρ and λ_a are constant n-dimensional vectors, and ρ_o is a constant). A well-known result in finance theory states that no-arbitrage implies the existence of a pricing kernel (stochastic discount factor) of the form (1).[5]

There is freedom in choosing the specific functional form of r_t and λ_t and the dynamics of x_t. Use of the affine forms for r_t and λ_t and the Gaussian specification (VAR(1) specification) of x_t constitutes the affine-Gaussian model. This form has certain limitations (discussed later), but it is still quite general and capable of encompassing many of the known models in finance and macroeconomics.

Using the recursion relation for the price of a τ-period zero-coupon bond at time t,

$$
P_{\tau,t} = E_t\left(P_{\tau-1,t+1} M_{t+1}\right), \tag{2}
$$

it is straightforward to show that bond prices in this model are given by

$$
P_{\tau,t} = \exp\left(A_\tau + B_\tau' x_t\right), \tag{3}
$$

where A_τ and B_τ are the solution of the difference equations,

$$
\begin{aligned}
&0 = \rho_o + A_\tau - A_{\tau-1} - \frac{1}{2} B_{\tau-1}' \Sigma\Sigma' B_{\tau-1} \\
&-B_{\tau-1}'\left((I - \Phi)\mu - \Sigma\lambda_a\right) \text{ and} \\
&0_{n\times 1} = \rho + B_\tau - \left(\Phi - \Sigma\Lambda_b\right)' B_{\tau-1},
\end{aligned} \tag{4}
$$

with boundary condition $A_{\tau=0} = 0$, $B_{\tau=0} = 0_{n\times 1}$ (see, for example, Ang and Piazzesi, 2003 [AP]). The bond yield $y_{\tau,t}(=-\log(P_{\tau,t})/\tau)$ is given by

$$
y_{\tau,t} = -\frac{1}{\tau} A_\tau - \frac{1}{\tau} B_\tau' x_t, \tag{5}
$$

that is, it takes an affine form.

The original "finance term structure models" such as those by Dai and Singleton (2000) and Duffee (2002) were written for nominal bond yields only. For example, the model defined by equation (1) could be estimated with just nominal yields data, with suitable (normalization) restrictions on the parameters Φ, μ, ρ,... to ensure that the model be econometrically identified. The state variables in this case are "latent factors" without an explicit economic meaning.

In a seminal paper, AP proposed combining this setup with a description of the macroeconomy. Their basic insight is that the well-known Taylor-rule specification of the short rate also has an affine form:

$$
r_t = \rho_\pi \pi_t^Y + \rho_g \, gap_t + const, \tag{6}
$$

where π_t^Y is the annual inflation and gap_t is the gross domestic product (GDP) gap (log GDP minus log potential GDP).[6] Therefore, using variables such as inflation and the GDP gap as part of the state vector in equation (1), that is,

$$
x_t = \left[\pi_t^Y, gap_t, \ldots\right]', \tag{7}
$$

provides a system in which bond yields are linked to key macroeconomic variables. Some macroeconomic variables might not be well described by simple VAR(1) dynamics, but this is, in principle, not a problem, as a higher-order VAR process (VAR(q) model) can be written as a VAR(1) process

[5] Duffie (2001) discusses this in the continuous-time formalism; see also Cochrane (2001).

[6] To be precise, AP use GDP growth, instead of the GDP gap, in their formulation.

with an expanded state vector that includes lags of these variables (e.g., $[\pi_t^Y, \pi_{t-1}^Y, \ldots, gap_t, gap_{t-1}, \ldots]'$).

Various macro-finance models differ by the choice of the restrictions imposed on the matrices (like Φ, ρ,..., etc.). For example, AP adopt an atheoretical (statistical) approach, reminiscent of Sims's (1980) original VAR proposal; Hördahl, Tristani, and Vestin (2006; HTV) impose more structure, based on a New Keynesian model as in Clarida, Galí, and Gertler (2000), though still remain in the reduced-form framework.

These models are an innovation from the earlier approach of handling long-term bond yields in macroeconomic models. In fact, most macroeconomic models have not dealt with long-term bond yields at all, despite their importance for savings and investment decisions in the economy. Precursors to the macro-finance models, such as the Federal Reserve's FRB/US model, do contain the 5-year and 10-year nominal yields, which are specified as the expectations hypothesis–implied yield plus a term premium (the 5-year term premium and the 10-year term premium are modeled separately),[7] but the framework (equation (1)) allows not just a few selected long-term yields but information from the *entire* yield curve to be integrated with a description of the macroeconomy.

No-Arbitrage and Replicating Portfolios

Although the derivation of the affine bond pricing equation (3) using the recursion relation involving the pricing kernel is simple and elegant, it is useful to re-derive it using the hedging (spanning) argument[8] to get a better sense of the role that the no-arbitrage principle plays in macro-finance models. Suppose there are n-dimensional shocks underlying the term structure movements, denoted by a standard normal random vector ε_t. The change in the value of a bond with maturity τ can be expressed generally as

$$\frac{\delta P_{\tau,t+1}}{P_{\tau,t}} = \mu_{\tau,t} + \gamma'_{\tau,t}\varepsilon_{t+1}, \tag{8}$$

where I have used the notation $\delta P_{\tau,t+1}$ for $P_{\tau-1,t+1} - P_{\tau,t}$ (the change in the value of a bond which was of time-to-maturity τ at time t) to avoid confusion with simple time-differencing; $\Delta P_{\tau,t+1} = P_{\tau,t+1} - P_{\tau,t}$; $\mu_{\tau,t}$ is the one-period expected return on a bond that has time-to-maturity τ at time t (i.e., $\mu_{\tau,t} = E_t(\delta P_{\tau,t+1}/P_{\tau,t})$); and the n-dimensional vector $\gamma_{\tau,t}$ is the loading on the shocks that determine the unexpected return.

Consider a portfolio formed by taking positions in $n+1$ bonds with maturities $\tau_1, \tau_2, \ldots, \tau_{n+1}$, with portfolio weights $w_{1t}, \ldots, w_{n+1,t}$. Denoting the value of this portfolio, the return on the portfolio V is given by

$$\begin{aligned}\frac{\delta V}{V} &= w_1 \frac{\delta P_{\tau_1}}{P_{\tau_1}} + \ldots + w_{n+1}\frac{\delta P_{\tau_{n+1}}}{P_{\tau_{n+1}}} \\ &= \sum_{i=1}^{n+1} w_i \mu_{\tau_i} + \left(\sum_{i=1}^{n+1} w_i \gamma_{\tau_i}\right)' \varepsilon,\end{aligned} \tag{9}$$

where the time index t has been suppressed for notational simplicity. If the portfolio is locally risk-free

$$\left(\sum_{i=1}^{n+1} w_i \gamma_{\tau_i} = 0\right),$$

then by no-arbitrage it should yield a risk-free rate (one-period yield); that is,

$$\sum_{i=1}^{n+1} w_{it}\mu_{\tau_i,t} = r_t,$$

which is equivalent to

$$\sum_{i=1}^{n+1} w_{it}\left(\mu_{\tau_i,t} - r_t\right) = 0$$

since

$$\left(\sum_{i=1}^{n+1} w_{it} = 1\right).$$

Summarizing, we have

$$\sum_{i=1}^{n+1} w_{it}\left(\mu_{\tau_i,t} - r_t\right) = 0, \ \sum_{i=1}^{n+1} w_{it}\gamma_{\tau_i,t} = 0_{n\times 1}. \tag{10}$$

[7] See, for example, Brayton et al. (1997).

[8] The derivation here can be viewed as a discrete-time analogue of Cox, Ingersoll, and Ross's (1981) continuous-time derivation.

Recall that the latter of these equations is n-dimensional, since $\gamma_{\tau_i,t}$'s are n-dimensional. Equation (10) can be put in the matrix form,

$$(11)\quad \begin{bmatrix} \mu_{\tau_1,t} - r_t & \cdots & \mu_{\tau_{n+1},t} - r_t \\ \gamma_{\tau_1,t} & \cdots & \gamma_{\tau_{n+1},t} \end{bmatrix} w_t = 0_{(n+1)\times 1},$$

where $w_t = [w_{1t},\ldots,w_{n+1,t}]'$. In order for this matrix equation to have a nontrivial (i.e., nonzero) solution w_t for an arbitrary choice of τ_i's, the expected excess return $\mu_{\tau,t} - r_t$ has to be a linear combination of $\gamma_{\tau,t}$; that is,

$$(12)\quad \mu_{\tau,t} - r_t = \gamma'_{\tau,t}\lambda_t,$$

where the n-dimensional vector λ_t ("market price of risk") expresses the linear dependence between $\mu_{\tau,t} - r_t$ and $\gamma_{\tau,t}$.

It is often more convenient to deal with log prices and log returns on bonds, $\delta \log P_{\tau,t+1}$ $(=\log P_{\tau-1,t+1} - \log P_{\tau,t})$. From the discrete-time version of Ito's lemma,[9] one has

$$(13)\quad \delta \log P_{\tau,t+1} = \tilde{\mu}_{\tau,t} + \gamma'_{\tau,t}\varepsilon_{t+1},$$

where

$$(14)\quad \tilde{\mu}_{\tau,t} = E_t\left(\frac{\delta P}{P}\right) - \frac{1}{2}var_t\left(\frac{\delta P}{P}\right) = \mu_{\tau,t} - \frac{1}{2}\gamma'_{\tau,t}\gamma_{\tau,t}.$$

Thus, equation (12) can be also written

$$(15)\quad \tilde{\mu}_{\tau,t} - r_t + \frac{1}{2}\gamma'_{\tau,t}\gamma_{\tau,t} = \gamma'_{\tau,t}\lambda_t.$$

Note that the derivation thus far has been quite general. If the short rate and market price of risk are affine in the state variables and if the state variables follow a VAR(1) process (i.e., equation (1)), one obtains a particularly simple result. Positing that the bond prices take the form $\log P_{\tau,t} = A_\tau + B'_\tau x_t$, one has (from equation (13))

$$(16)\quad \tilde{\mu}_{\tau,t} = A_{\tau-1} - A_\tau + B'_{\tau-1}(I - \Phi)\mu + \left(B'_{\tau-1}\Phi - B'_\tau\right)x_t$$

$$(17)\quad \gamma'_{\tau,t} = B'_{\tau-1}\Sigma.$$

Substituting these (and the expressions for r_t and λ_t) into equation (15) gives the same difference equation for bond prices as in equation (4)—hence, the same bond prices, as promised earlier.

Internal Basis Models versus External Basis Models

The key formula in the above derivation of the bond pricing equation is equation (12), or equivalently, equation (15). It states that the expected return on a bond of arbitrary maturity in excess of the short rate depends on the product of the bond-independent market price of risk, λ_t, and the bond's sensitivity to risk, $\gamma_{\tau,t}$. The basic intuition underlying equation (12) is that *the yield curve is "smooth," so the risks to a bond can be hedged well by a portfolio of (a relatively small number of) other bonds*. This is well known from the factor analysis of Litterman and Scheinkman (1991) and other studies. One can also see this from the regression of the quarterly change in the 5-year yield on the changes in 6-month, 2-year, and 10-year yields, which gives very high R-squareds (e.g., 99 percent).

Note that equation (12) itself is silent about the structure of the λ_t vector, except for the condition that it does not depend on bond-specific information (like maturity). In fact, the early generation of affine-Gaussian models assumed a constant market price of risk vector λ, which in effect implied a version of the expectations hypothesis. Later studies recognized that λ_t can depend on the state of economy; thus, a variable influencing the market price of risk would also influence bond prices.[10] However, this creates, in a sense, too large a set of possibilities: Any variable could, in principle, enter the expression for the market price of risk and, in turn, the expression for bond yields.

Latent-factor models of the term structure, such as the affine-Gaussian model of Duffee (2002) (the $EA_0(n)$ model in Duffee's terminology), partly get around this problem by implicitly defining the model in statistical terms. A "maximally flexible" n-dimensional affine-Gaussian model (1) can be viewed as an answer to the question, "what is the most general n-dimensional representation of the yield dynamics in which yields

[9] See, for example, Campbell, Chan, and Viceira (2003).

[10] Thus, a shock that changes λ_t, say ξ_t, should also be included in the vector of shocks ε_t that moves bond prices.

are Gaussian, linear in some basis, and consistent with no arbitrage?" As the yield curve seems to be well described by a small number of risk sources, it stands to reason that there exists a suitable representation for a relatively small n. Thus, the no-arbitrage principle in this setting can help describe the rich variation of the yield curve in a tractable and relatively parsimonious way, while allowing for a general pricing of risk (as opposed to the expectations hypothesis).

Duffee's (2002) affine-Gaussian model describes only the nominal yield curve, but it is straightforward to write down a "joint model" of nominal yields and inflation in the same spirit by combining equation (1) with the following specification of the inflation process:

$$\begin{aligned} \pi_{t+1} &= \chi\left(x_t\right) + \tilde{\sigma}'\tilde{\varepsilon}_{t+1} \\ \chi\left(x_t\right) &= \psi_0 + \psi' x_t, \end{aligned} \tag{18}$$

where the one-period inflation π_{t+1}(=log(Q_{t+1}/Q_t), Q_t being the price level) consists of the one-period *expected* inflation $\chi(x_t)$ and unforecastable inflation $\tilde{\sigma}'\tilde{\varepsilon}_{t+1}$. As in the case of the nominal short rate r_t, the one-period inflation expectation is specified as an affine function of the state vector x_t. The disturbance vector $\tilde{\varepsilon}_t$ includes the vector of shocks that move interest rates (ε_t in equation (1)) and a shock (say ε_t^+) that is orthogonal to the interest rate shocks.[11] As in the nominal-yields-only model, the state vector x_t is a vector of statistical variables (latent variables), which is determined only up to normalization restrictions (on parameter matrices Φ, ρ_o, ρ, ψ_o, ψ,...) that ensure the (maximal) identification of the model. I shall refer to such a model as an "internal basis model," as the state vector is unknown before the estimation and is determined *inside* the estimation with yields, inflation, and possibly other data.[12]

Such a joint model makes only fairly weak assumptions: Writing the one-period inflation as the sum of expected inflation and unexpected inflation in equation (18) is quite general, and it makes intuitive sense to have the state vector x_t describe inflation expectations and bond yields together, as a variable that moves inflation expectation would also be expected to move nominal interest rates. At the same time, this formulation relaxes the assumptions implicit in the two traditional theories of nominal yields: It goes beyond the expectations hypothesis—as it now allows for time-varying term premia—and the Fisher hypothesis—as it now implicitly allows for a general correlation between real rates and inflation. Note that the state vector x_t in the joint model has more economic meaning than the nominal-yields-only model in the sense that it is now (implicitly) related to objects such as inflation expectations and inflation risk premia. However, the fact that the x_{it}'s are still latent factors is potentially an unattractive feature and makes it difficult to discuss bond market developments in a simple manner.

Thus, many papers in the macro-finance literature take all or part of the state vector to be specific macroeconomic variables (or variables with clear macroeconomic interpretation) so as to make the connection between the yield curve and macroeconomy more explicit. These variables form an external basis, in the sense that they are a priori fixed, partially ("mixed" models) or completely (observables-only models). Simply speaking, internal basis models try to project information in yields $y_{\tau,t}$ and "observable" macroeconomic variables f_{it}^o onto the state vector x_t consisting of unobservable variables f_{it}^u, while external basis models try to project information in yields onto "observable" macroeconomic variables f_{it}^o and latent variables (if there are any). Schematically,

$$\textit{internal basis}: \left\{y_{\tau,t}\right\}, \left\{f_{it}^o\right\} \Rightarrow x_t = \left[f_{1t}^u, f_{2t}^u, \ldots\right]'$$

$$\textit{external basis}: \left\{y_{\tau,t}\right\} \Rightarrow x_t = \left[f_{1t}^o, f_{2t}^o, \ldots, f_{1t}^u, f_{2t}^u, \ldots\right]'.$$

As one moves on to external basis models, one might be also moving away from the relative comfort of the original intuition behind no-arbitrage (the smoothness of the yield curve); hence, close scrutiny of the additional assumptions they involve is warranted.

[11] This shock (ε_t^+) is introduced to allow for shocks to inflation that are not spanned by interest rate shocks. (One can also define the ε_t vector in equation (1) to include this shock.)

[12] Perhaps the best-known example of internal basis models is factor analysis (e.g., Litterman and Scheinkman, 1991). As in the no-arbitrage internal basis models, the factors in factor analysis are determined only up to an invariant transformation; thus, normalization restrictions are needed to define them.

EXAMINING THE ASSUMPTIONS IN EXTERNAL BASIS MODELS

Unspanned Short-Run Inflation

One implication of having a macroeconomic variable like inflation as a state variable in the set-up of equation (1) is that short-run inflation risk can be hedged by taking positions in nominal bonds.[13] Many practitioners, however, would be skeptical about this claim. Policymakers are well aware of large short-run variations in price indices such as the producer price index (PPI) and consumer price index (CPI) that do not require a policy response, and they are careful to "smooth through the noise" in interpreting data on inflation. Blinder (1997) puts this clearly and strongly: "[The noise issue] was my principal concern as Vice-Chairman of the Federal Reserve. I think it is a principal concern of central bankers everywhere."

Market participants are also (implicitly) cognizant of these issues. One evidence is the bond market's reaction to the announcement of total CPI (also called "headline CPI" or simply "CPI") and core CPI (which is an inflation measure obtained by stripping out the volatile food and energy prices from total CPI): Bond yields are known to react mainly to the surprise component of core CPI, not total CPI.[14] This raises the question whether it is reasonable to treat the fluctuation in *total* CPI as risks that are spanned by the yield curve factors (an implicit assumption in most external basis macro-finance models).

One can also consider the regression of the change in quarterly inflation onto the changes in 6-month, 2-year, and 10-year yields,[15] which gives an R^2 of at most 10 percent in the 1965-2006 period, in stark contrast to the aforementioned regression of the change in the 5-year yield (R^2 of 99 percent). Even when the lagged inflation terms are included, as in

$$\Delta\pi_t = a + b_1\Delta\pi_{t-1} + b_2\Delta\pi_{t-2} + b_3\Delta\pi_{t-3} + b_4\Delta y_{6M,t} + b_5\Delta y_{2Y,t} + b_6\Delta y_{10Y,t} + e_t, \quad (19)$$

the R^2's do not exceed 40 percent[16]; the use of more than three yields does not make much difference. This exercise is similar in spirit to Collin-Dufresne and Goldstein (2002), who argue that the relatively low R^2's in the regressions of the changes in interest rate derivative prices on the changes in interest rates indicate the presence of "unspanned stochastic volatility" in interest rates.

Note lastly that, although we have focused on inflation (CPI) here, the concern about unspanned shocks in macroeconomic variables is more general; for example, variables such as quarterly GDP growth face similar problems.

Do Macro Variables Form a Suitable Basis for Representing Expectations?

Let us now address a related question: whether external basis models can properly describe inflation expectations, which, according to the Fisher hypothesis intuition, is an important determinant of the nominal term structure.

To those who engage in inflation forecasting extensively, the poor inflation forecast performance of macro-finance models like those of ABW might not be a surprise: A long line of research has explored the inflation forecasting performance of the yield curve information and generally obtained disappointing results. Stock and Watson (2003) summarize the situation thus: "With some notable exceptions, the papers in this literature generally find that there is little or no marginal information content in the nominal interest rate term structure for future inflation."

Most of the regression-based inflation forecasting models in the literature include current and

[13] Let the first element of the state vector x_t in equation (1) be inflation. The formalism (1) then implies that one can in general form a portfolio of bonds that replicates the inflation shock ε_{1t}.

[14] One can regress the change in bond yield (around the announcement) on the surprise components in total CPI and core CPI (computed as the announced number minus the Bloomberg consensus prediction). The coefficient on the total CPI surprise is found to be insignificant.

[15] Let y_t denote a vector of n yields. In the affine model, one has $y_t = a + Bx_t$, where x_t is the n-dimensional vector of state variables, a is an n-dimensional constant vector, and B is an $n \times n$ constant matrix. Inverting it gives $x_t = B^{-1}a + B^{-1}y_t$. Thus, if π_t is an element of x_t, this implies $\Delta\pi_t = c'\Delta y_t$, where c is a constant vector. This is in the linear regression form *without the residual error term* (and the intercept term).

[16] In the regression (19), I have tried quarterly inflation based on both the quarter-averaged CPI and the end-of-quarter (last month of the quarter) CPI. In these cases, the quarter-averaged yields and end-of-quarter yields were used, respectively.

lagged inflation as regressors to take into account the persistence of inflation. The expected inflation over the next year in these models takes the form

$$(20) \quad E_t\left(\pi_{t+1Y,t}\right) = a + b_0\pi_t^* + b_1\pi_{t-1}^* + \ldots + c'z_t,$$

where π_t^* is either the one-period inflation or annual inflation and the vector z_t denotes other regressors, which could include term structure variables.

Consider a macro-finance model (1) that has quarterly (one-period) inflation π_t as a state variable. In other words, $x_t = [\pi_t, \tilde{z}_t]'$, where $\tilde{z}_t(=[\tilde{z}_{1t}, \tilde{z}_{2t},\ldots]')$ denotes other state variables. The expected inflation over the next year is

$$\begin{aligned} & E_t\left(\pi_{t+1Y,t}\right) \\ (21) \quad & = [1,0,\ldots,0]\left(\left(\Phi + \Phi^2 + \Phi^3 + \Phi^4\right)\left(x_t - \mu\right) + \mu\right) \\ & = \tilde{a} + \tilde{b}_0\pi_t + \tilde{b}_1\tilde{z}_{1t} + \tilde{b}_2\tilde{z}_{2t} + \ldots, \end{aligned}$$

which is in the same form as equation (20).[17] (The case is similar with models that use annual inflation as a state variable.) As such, the difference between the macro-finance models formulated this way and the regression models is simply in the *coefficients*, not in the *basis*. There is a possibility of an "efficiency gain" with no-arbitrage models (through the imposition of useful constraints on the coefficients), but even this is not ensured if the results in ABW are any indication. More fundamentally, though, the frequently poor inflation forecast performance of regression models and macro-finance models like ABW raises questions about the efficacy of the basis itself.

Lessons from Simple Models

Some of the key conceptual issues in the representation of the yield curve and inflation expectations may be explained through a comparison of two simple models of inflation—namely, AR(1) and ARMA(1,1) models:

$$(22) \quad \pi_t = (1-\phi)\mu + \phi\pi_{t-1} + \varepsilon_t \,(\text{AR}),$$

$$(23) \quad \pi_t = (1-\phi)\mu + \phi\pi_{t-1} + \varepsilon_t - \alpha\varepsilon_{t-1} (\text{ARMA}).$$

The τ-period-ahead inflation expectations in both models take the form

$$(24) \quad E_t\left(\pi_{t+\tau}\right) = \phi^{\tau-1}\left(\chi_t - \mu\right) + \mu,$$

where the expected one-period inflation $\chi_t \equiv E_t(\pi_{t+1})$ for the AR(1) model is given by

$$(25) \quad \chi_t = \phi\pi_t + (1-\phi)\mu$$

and χ_t for the ARMA(1,1) model is given by

$$(26) \quad \chi_t = \phi\pi_t - \alpha\varepsilon_t + (1-\phi)\mu.$$

The estimate of ϕ in the AR(1) model, based on U.S. quarterly CPI inflation data from 1960:Q1 to 2005:Q4, is $0.785_{(0.045)}$, while the estimates of ϕ and α in the ARMA(1,1) model are $0.935_{(0.031)}$ and $0.341_{(0.081)}$, respectively, with standard errors listed in parentheses. These numbers imply fairly similar 1-quarter-ahead inflation expectations, as can be seen in Figure 1A. (There is somewhat more jaggedness in the AR(1) forecast.) The same parameter estimates, however, imply very different longer-horizon inflation expectations (Figure 1B): The 5-year-ahead (20-quarter-ahead) inflation expectation from the AR(1) model is almost constant, while the 5-year-ahead inflation expectation from the ARMA(1,1) model is more variable. (This reflects the difference between $0.785^{20-1} = 0.01$ versus $0.935^{20-1} = 0.28$ in equation (24).)

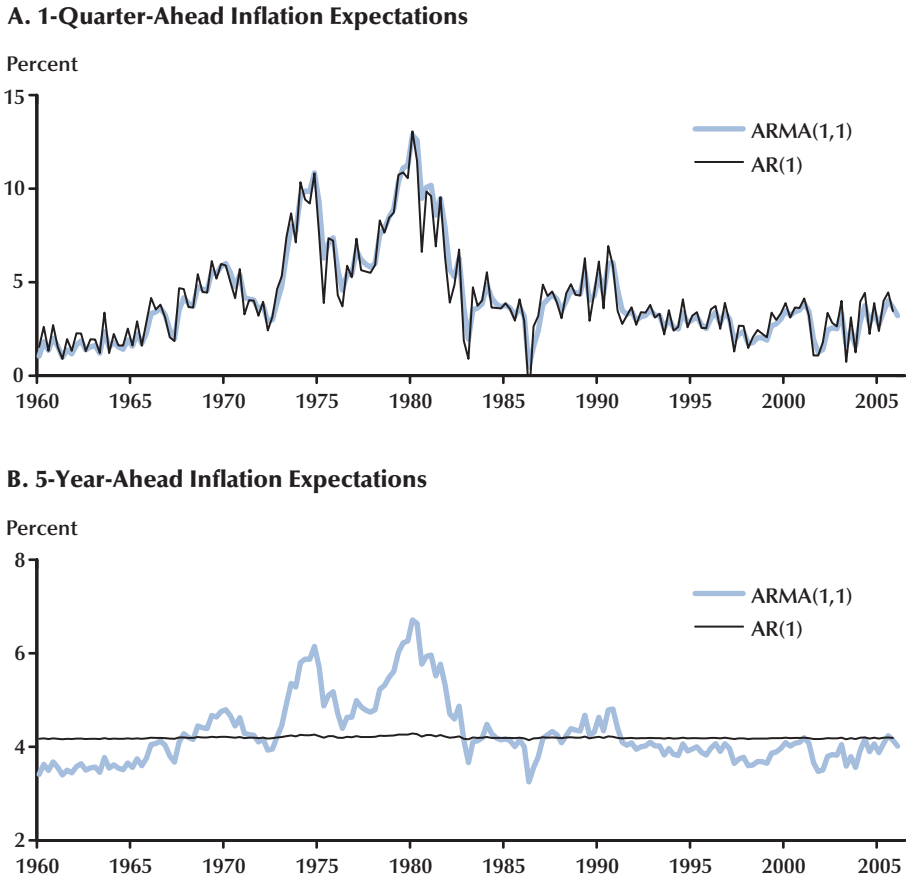
An almost constant 5-year-ahead inflation expectation from the AR(1) model in the past 40 years is implausible. The main reason for the qualitative difference between the AR and ARMA models is that the ARMA(1,1) model tries to separate the "unforecastable inflation" from the expected inflation, while the AR(1) model does not. This can be seen from the fact that the ARMA(1,1) model is a univariate representation of the following "two-component model":

$$\begin{aligned} & \pi_t = \chi_{t-1} + \eta_t \\ (27) \quad & \chi_t = (1-\phi)\mu + \phi\chi_{t-1} + e_t, \\ & \eta_t \sim N\left(0,\sigma_\pi^2\right),\ e_t \sim N\left(0,\sigma_x^2\right),\ corr\left(\eta_t, e_t\right) = \varrho, \end{aligned}$$

[17] If the $\tilde{z}_t$ vector includes latent factors, they can be "inverted" and expressed in terms of yields (because of the linear relationship between the yield and the factors), which again leads to the form of equation (20). However, latent factors in these "mixed" models are partly defined by their relation to the macro factors, and this may entail complications, as discussed later in the "Low-Dimensional External Basis Models" subsection.

Figure 1

U.S. Inflation Expectations Based on AR(1) and ARMA(1,1) Models



in which χ_t is an expected inflation process and η_t is an unforecastable inflation.[18] Though simple, this two-component model (of which the internal basis model [equation (18)], previously discussed in the "Internal Versus External Basis Models" subsection, can be viewed as an extension) is quite useful for illustrating some of the key points in this paper.[19]

The unforecastable inflation component η_t in equation (27) can help explain several puzzling empirical results in the literature. Among them is the negative one-lag autocorrelation of the changes in quarterly inflation $\Delta\pi_t (= \pi_t - \pi_{t-1})$, which, according to Rudd and Whelan (2006, Sec III.C), is evidence against the New Keynesian Phillips curve models (which generate positive one-lag autocorrelation). In the case of the two-component model (27), one has

[18] The MA(1) coefficient in the ARMA(1,1) model is related to the two-component model parameters as

$$\alpha = c - \sqrt{c^2 - 1}, \text{ where}$$
$$c \equiv \left[\left(1+\phi^2\right)\sigma_\pi^2 + \sigma_x^2 - 2\varrho\phi\sigma_\pi\sigma_x\right] \Big/ \left[2\left(\phi\sigma_\pi^2 - \varrho\sigma_\pi\sigma_x\right)\right].$$

See, for a derivation, Cochrane (2001, pp. 418-20).

[19] This model of inflation has an interesting parallel with the consumption-based asset pricing model of Bansal and Yaron (2004), who argue that writing the consumption growth Δc_t as the sum of expected component and unexpected component ($\Delta c_t = \chi_t + \eta_{t+1}$) can help resolve the equity premium puzzle.

$$
\begin{aligned}
& cov\left(\Delta\pi_t, \Delta\pi_{t-1}\right) \\
(28) \quad & = cov\left(\Delta\eta_t, \Delta\eta_{t-1}\right) + cov\left(\Delta\chi_{t-1}, \Delta\chi_{t-2}\right) \\
& + cov\left(\Delta\chi_{t-1}, \Delta\eta_{t-1}\right).
\end{aligned}
$$

The obviously negative first term dominates the second and third terms at appropriate parameter values, resulting in a negative $cov(\Delta\pi_t, \Delta\pi_{t-1})$. The unforecastable component η_t also plays the role of putting an upper bound on the predictability of inflation.

Economically, the η_t term represents very-short-run effects in total CPI inflation, including part of the food and energy prices that create the wedge between total CPI and core CPI, as well as the unforecastable components of the core CPI inflation and potential errors in the measurement of CPI.

The importance of the η_t term in the two-component model (27) has a parallel implication for no-arbitrage macro-finance models: The failure to separate out the "unspanned macro shocks" in macro-finance models may produce problems that mirror those of the AR(1) inflation model. It is worth mentioning here that Stock and Watson (2007) have also recently emphasized that separating inflation into a trend component and a serially uncorrelated shock (like η_t in equation (27)) is useful for explaining key features of U.S. inflation dynamics,[20] though they do not discuss the ramifications for macro-finance (no-arbitrage) models.

It is instructive to ask about the basic variable underlying the term structure of inflation expectations in the ARMA(1,1) model. As is clear from equation (24), the basic variable is χ_t, not realized inflation, π_t. Note that in the case of the AR(1) model, χ_t *is* π_t (up to a prefactor and an intercept), as can be seen from equation (25). This is not the case for the ARMA(1,1) model: It is straightforward to show (by solving for ε_t in equation (23) and recursively substituting into equation (26)) that χ_t in the ARMA(1,1) model depends on an infinite number of lags of π_t:

$$
(29) \qquad \chi_t = (\phi - \alpha)\sum_{j=0}^{\infty}\alpha^j\left(\pi_{t-j} - \mu\right) + \mu.
$$

This is in the exponential smoothing form, which has been familiar at least since the work of Muth (1960).

The expression (29) suggests that (i) the connection between realized macroeconomic variables and state variables in no-arbitrage term structure models could be complicated and (ii) the poor inflation forecasting performance of regression models and no-arbitrage models with macroeconomic variables may be a more complex issue than just a matter of having "efficient" coefficients (with conventional basis). To be sure, the state variables in nominal term structure models are not simply those that underlie the variation of inflation expectations. Factors that affect the real term structure and inflation risk premia should also be included in the nominal term structure model. However, it is not clear that these additional aspects would be any better described by macroeconomic variables.

Low-Dimensional External Basis Models

Let us now consider some specific issues that arise in external basis models with a "low-dimensional" state vector. Suppose that one has a three-factor macro-finance model in the setup of equation (1), with the state vector x_t consisting of all "observable" macroeconomic variables, say, quarterly inflation, π_t, quarterly GDP growth, g_t, and the effective federal funds rate, ff_t. The inflation expectations in this model are then linear functions of contemporaneous variables π_t, g_t, and ff_t. (To see this, simply substitute $\tilde{z}_{1t} = g_t$, $\tilde{z}_{2t} = ff_t$ in equation (21).) This type of forecast (VAR(1)) has more qualitative similarity to the AR(1) model than the ARMA(1,1) model; in particular, despite its multifactor nature, it still mixes "signal" with "noise" and can therefore be expected to inherit many of the problems with the AR(1) model.

Some of the macro-finance models in the literature, including Ang, Dong, and Piazzesi (2005; ADP) and ABW, remain in a relatively low-

[20] Stock and Watson (2007) write the U.S. inflation process for the past half century as $\pi_t = \tau_t + \eta_t$, where η_t is a serially uncorrelated disturbance term. The τ_t term (what they refer to as the trend component) can be identified as χ_{t-1} in equation (27). Stock and Watson's (2007) τ_t and η_t have time-varying volatilities, a feature which they argue is important in the inflation persistence debate.

dimensional framework but use a mix of latent factors and macroeconomic variables, but these "mixed models" may still have difficulties. Consider, for example, the ABW affine model (their MDL1 model) with quarterly inflation and two latent factors, that is, $x_t = [\pi_t, f_{1t}, f_{2t}]'$. If the latent factors f_{1t}, f_{2t} are interpreted as π_{t-1}, π_{t-2}, equation (21) takes a form similar to the smoothing form (29). However, besides the issue that two lags might not be enough, one may not have the freedom to interpret f_t's this way, as that would deprive the ability to describe other aspects of the nominal term structure (e.g., real interest rates, the time-varying risk premium, or time-varying perceived inflation target).

In the mixed models, having a macroeconomic variable like π_t as a part of the state vector may cause a distortion in the inference, as the latent factors can end up absorbing the "unspanned" variation in π_t. To illustrate this schematically, suppose that the true model of the short rate is

$$r_t = \rho\tilde{\pi}_t + f_t, \tag{30}$$

where $\tilde{\pi}_t$ is the "spanned" part of the one-period inflation π_t; that is,

$$\pi_t = \tilde{\pi}_t + e_t, \tag{31}$$

with e_t denoting the unspanned component. If one uses realized inflation π_t in place of $\tilde{\pi}_t$, then

$$r_t = \rho(\pi_t - e_t) + f_t = \rho\pi_t + (f_t - \rho e_t). \tag{32}$$

Thus, the latent factor f_t would be distorted by an amount ρe_t. Though one might be tempted to regard this simply as a redefinition of the latent factor, it would imply practical differences, such as the reduced persistence of the factor dynamics.

High-Dimensional External Basis Models

Some of the external basis macro-finance models in the literature use a fairly large number of state variables that include lagged macroeconomic variables. Many such models (including those of AP and HTV) use annual inflation π_t^Y $(= \pi_{t,t-1Y})$ as a state variable instead of one-period inflation. This may help alleviate concerns about the problem with the use of one-period inflation, since the year-on-year inflation partly "smooths out" the noise in quarterly inflation: π_t^Y can be written

$$\pi_t^Y = \sum_i w_i \pi_{t-i}, \tag{33}$$

where the weights w_i are ¼ for $i = 0,1,2,3$, and 0 for $i > 3$.

Note, however, that the construction (33) automatically implies a moving average structure in π_t^Y, which suggests that the simple VAR(1) description would not be a good description of its dynamics. Thus, macro-finance models that use annual inflation as a state variable typically include additional lags, for example, AP use 12 monthly lags, in effect having a VAR(12) model. Bond yields in this case depend on a "large" set of state variables that include lagged macroeconomic variables.[21]

A problem with this type of "high-dimensional" specification is that it inherits the well-known problems of the unrestricted VAR models. In fact, AP's inflation dynamics *is* a conventional VAR. They separate the vector of relevant variables into an "observable" macro vector f_t^o and an unobservable (latent) vector f_t^u, that is, $\tilde{x}_t = [f_t^{o'}, f_t^{u'}]'$,[22] and impose the restriction that the latent factors do not affect the expectation of macroeconomic variables. Their macro vector dynamics are given by the VAR(q):

$$f_t^o = \Phi_1^o f_{t-1}^o + \Phi_2^o f_{t-2}^o + \cdots + \Phi_q^o f_{t-q}^o + c^o + \Sigma^o \varepsilon_t^o, \tag{34}$$

where $q = 12$. Although the parameters in the matrices $\Phi_1^o, \ldots, \Phi_q^o$ are, in principle, identified and can be estimated by ordinary least squares (OLS), this kind of unrestricted VAR is well known to suffer from overparameterization problems (which

[21] Since an invertible ARMA(1,1) model can be written as an AR model with infinite lags, the use of lagged macroeconomic variables in an external basis model may partly address the deficiency of the AR(1) model (relative to the ARMA(1,1) model) discussed in the "Low-Dimensional External Basis Models" subsection. However, identifying inflation as a state variable may still be problematic conceptually (especially in the case of one-period inflation, π_t), in view of our earlier discussion regarding the difference between χ_t and π_t.

[22] Here I have attached a tilde to x_t to clarify that this is not the full state vector. The full state vector (on which bond yields depend) in AP is larger: $x_t = [f_t^{o'}, f_{t-1}^{o'}, \ldots, f_t^{u'}]'$.

will be discussed further in the "Empirical Issues" subsection).[23]

By having only the macroeconomic variables describe inflation dynamics, AP suppressed the possibility of the yield curve saying something about future inflation. Unfortunately, it is difficult to lift that restriction. The overparameterization problem would worsen, as the full (maximally identified) model would have an even larger number of parameters: In the specification of the state vector dynamics

$$\begin{bmatrix} f_t^o \\ f_t^u \end{bmatrix} = \begin{bmatrix} \Phi_1^o & \Phi_1^{ou} \\ \Phi_1^{uo} & \Phi_1^u \end{bmatrix} \begin{bmatrix} f_{t-1}^o \\ f_{t-1}^u \end{bmatrix} + \begin{bmatrix} \Phi_2^o & \Phi_2^{ou} \\ \Phi_2^{uo} & \Phi_2^u \end{bmatrix} \begin{bmatrix} f_{t-2}^o \\ f_{t-2}^u \end{bmatrix} + \cdots + c + \Sigma\varepsilon_t, \tag{35}$$

the matrices $\Phi_1^{ou}, \Phi_2^{ou}, \ldots$ are now nonzero and have to be estimated. Furthermore, the two-step estimation procedure that AP used is no longer applicable; hence, the estimation now involves a "one-step" optimization of a very-high-dimensional likelihood function.

For specifying external basis models that contain lags of macroeconomic variables in the state vector, it is common practice to set the coefficients of the market price of risk (Λ_b matrix in equation (1)) that load on lagged macroeconomic variables to zero (e.g., AP and HTV). Even with this restriction, the number of remaining market price risk parameters is large, and modelers often make additional ad hoc restrictions on the Λ_b matrices to reduce the number of parameters further.[24] Unfortunately, setting the Λ_b coefficients on lagged macroeconomic variables to zero may be a problematic practice. It implies that the expected excess return on a bond, $\mu_{\tau,t} - r_t$, is completely spanned by *contemporaneous* macroeconomic variables (and latent factors, if there are any). Recall, from equations (12) and (17), that

$$\mu_{\tau,t} - r_t = B_\tau' \Sigma \lambda_t. \tag{36}$$

Therefore, if λ_t does not depend on lagged macroeconomic variables, neither does the bond return premium. This means that while one has

$$y_{\tau,t} = a_\tau + b_{\tau,1}\pi_t + b_{\tau,2}\pi_{t-1} + b_{\tau,3}\pi_{t-2} \cdots, \tag{37}$$

one cannot have

$$\mu_{\tau,t} - r_t = \alpha_\tau + \beta_{\tau,1}\pi_t + \beta_{\tau,2}\pi_{t-1} + \beta_{\tau,3}\pi_{t-2} + \cdots, \tag{38}$$

that is, there is an asymmetry (in the way yields and bond risk premia depend on lagged macroeconomic variables) that was not motivated by theory. Thus, in order to cast the model in a "no-arbitrage" framework, many external basis macro-finance models may be introducing arbitrary and nontrivial assumptions about the market price of risk.[25]

AFFINE-GAUSSIAN MODELS VERSUS NON-AFFINE/ NON-GAUSSIAN MODELS

Structural Stability

One potential limitation of the general framework (1) is structural stability. To be sure, the debate about the structural stability of macroeconomic relationships is not new (see, e.g., Rudebusch, 1998, and Sims, 1998). However, it may have different ramifications for internal basis models and external basis models, and hence merits a discussion here.

Note that external basis macro-finance models have often used a framework based on the Taylor rule and VARs, but many have raised questions about the instability of these specifications.[26] One may hope that concerns about structural

[23] Models like HTV have more structure (in the form of the New Keynesian Phillips curve and IS equations), which may help alleviate overparameterization concerns, but at a possibly greater misspecification risk.

[24] For example, AP and HTV assume that Λ_b is a block-diagonal matrix (a block matrix for macro factors and a block matrix for latent factors).

[25] Duffee (2006) has also recently questioned the modeling of the term premium in the macro-finance literature, more specifically, the finding in some macro-finance papers of a strong relationship between the term premium and the macroeconomy. His point is that these studies often do not provide alternatives other than the "expectations hypothesis" (zero or constant return premium) and a term premium that depends on macro variables, leading to an exaggerated role of macro variables in term premium variation.

[26] See, for example, Clarida, Galí, and Gertler (2000) about the instability of Taylor-rule coefficients and Stock and Watson (1996) about the instability of VAR coefficients.

instability would be alleviated if latent factors are also included in external basis models. For example, a macro-finance model with a Taylor-rule–like mixed specification of the short rate (similar to ADP, 2005)

$$r_t = const + \rho_\pi \pi_t^Y + \rho_g \, gap_t + f_t, \tag{39}$$

where f_t is a latent factor, can be written as

$$r_t = const + \pi_t^Y + (1-\rho_\pi)\left(\pi_t^Y - \pi_t^*\right) + \rho_g \, gap_t, \tag{40}$$

where π_t^* $(=-f_t/(1-\rho_\pi))$ is the time-varying inflation target. However, the factor f_t may have to play a number of other roles in the model, for instance, the interest rate smoothing term, time-varying risk premium, and so on (analogously to an earlier discussion in the "Low-Dimensional External Basis Models" subsection regarding ABW's affine model). Thus, a model written with f_t as a time-varying inflation target in mind might have some difficulty capturing the intended effect.

Furthermore, there may be instabilities other than the time-varying intercept: for instance, changes in the conditional correlation of various macroeconomic variables, changes in the persistence of the macroeconomic variables, and so on. Imagine, heuristically, a situation in which the "true" model is

$$r_t = c + \rho_{\pi,t} \, \pi_t^Y + \rho_{g,t} \, gap_t, \tag{41}$$

that is, a Taylor-rule–like short rate with time-varying loadings on the macroeconomic variables. In this case, the two-factor affine model in which the state variables are $[\pi_t^Y, \, gap_t]'$ is obviously misspecified. For another example, consider a "time-varying inflation-persistence model":

$$\pi_t^Y = \phi_{t-1} \pi_{t-1}^Y + c + \varepsilon_t. \tag{42}$$

Again, identifying π_t^Y as a state variable in an affine setting would be a misspecification.

One way to address this problem is to model these effects explicitly in non-affine/non-Gaussian models.[27] However, these models, being richer than affine-Gaussian models, may be even more susceptible to overfitting concerns and may incur a greater risk of misspecification. Alternatively, the use of an internal basis (while still remaining in the affine-Gaussian setup) may allay structural instability concerns to some extent: Internal basis models are agnostic as regards the definition of the factors; thus, a model that is obviously unstable from the point of view of an external basis may not necessarily be so from the point of view of an internal basis. For example, going back to equation (41), choosing the state variable as $x_t = [\rho_{\pi,t} \pi_t^Y, \rho_{g,t} gap_t]'$ may be more effective than having $x_t = [\pi_t^Y, gap_t]'$, although there may be an even better internal basis for the problem (depending on how the rest of the model is defined).[28]

Of course, no-arbitrage models with an internal basis should not be expected to answer all structural stability concerns. A strong structural instability may be difficult to capture even with an internal basis model, in which case it might be better to use a shorter, structurally more homogeneous sample.

Time-Varying Uncertainty

Another limitation of the affine-Gaussian models (both internal and external basis models) is that they imply homoskedastic yields, while there is copious evidence for time-varying volatility of yields.

Theoretically and intuitively, one *should* expect a relation between term structure variables and time-varying uncertainty about interest rates: To the extent that bond market term premia arise from risk, the changing amount of interest rate risk should translate to a changing term premium. It also stands to reason that at least a part of the variation in interest rate volatility is linked to the variation in the uncertainties about key macroeconomic variables. Various studies have noted that macroeconomic uncertainties (inflation, GDP, monetary policy) have declined since the Volcker disinflation, a phenomenon often dubbed the "Great Moderation."[29] One can expect this effect

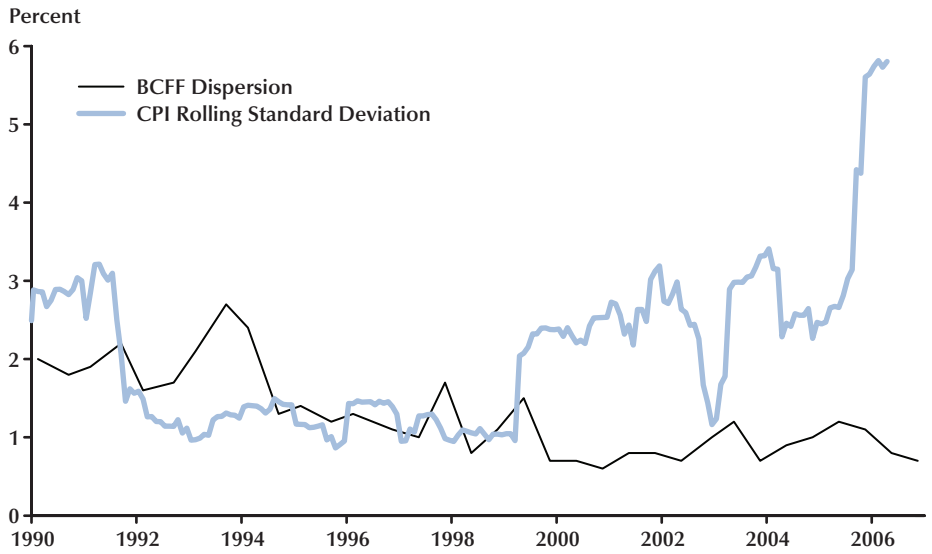
[27] For a work in this direction, see Ang, Boivin, and Dong (2007).

[28] If π_t^Y and $\rho_{\pi,t}$ are Gaussian processes, the process $\rho_{\pi,t}\pi_t^Y$ would be non-Gaussian (with time-varying volatility). However, one can still think of the affine version as an approximation of the non-Gaussian process.

[29] Bernanke (2004b) discusses this phenomenon from a policymaker's perspective.

Figure 2

The Dispersion of Long-Horizon U.S. Inflation Forecasts in the BCFF and the 1-Year Rolling Standard Deviation of Monthly CPI Inflation



to be accompanied by a corresponding reduction in term premia in the bond market. Kim and Orphanides (2007) indeed report positive relationships between the term premium in the 10-year-forward rate and proxies for uncertainties about monetary policy and inflation based on the dispersion of long-horizon survey forecasts.[30]

However, much work remains to be done to properly address the relationship between term premia and macroeconomic uncertainties—in particular, inflation uncertainty. The key difficulty is measuring the relevant inflation uncertainty. For instance, one can debate whether the survey dispersion measure used in Kim and Orphanides (2007) is a reliable proxy for uncertainty. Inflation uncertainty measures based on a GARCH-type model also would be problematic, as they posit too tight a relationship between long-term and near-term uncertainty.[31] As can be seen in Figure 2, 1-year rolling standard deviation of monthly (total) CPI inflation (a proxy for near-term inflation uncertainty) has been elevated from around 1999 on, but this does not seem to have translated to an increase in the perception of longer-term uncertainty, proxied by the dispersion of surveyed forecasts of long-horizon inflation. Even granting the imperfection of the long-horizon inflation uncertainty measure, this contrast is noteworthy.[32]

The complexity of inflation dynamics can thus create considerable challenge for attempts to go beyond homoskedastic models: It may be that a nonlinear model with time-varying inflation uncertainty can lead to *poorer* results if the model's inflation uncertainty is misspecified, as when a model that does not make a qualitative distinction between short- and long-run inflation uncertainties tries to link the rise in the volatility of short-run inflation of the recent several years

[30] See also Backus and Wright (2007).

[31] Consider a GARCH specification of one-period inflation, $\pi_{t+1} = f(\pi_t, \pi_{t-1}, \ldots) + \varepsilon_{t+1}$, $\varepsilon_{t+1} \sim N(0, \sigma_t^2)$, $\sigma_t^2 = \alpha + \beta\sigma_{t-1}^2 + \gamma\varepsilon_t^2$. It is straightforward to show that the uncertainty about multiperiod inflation $\pi_{t+\tau,t} = (\pi_{t+1} + \pi_{t+2} + \cdots \pi_{t+\tau})/\tau$, has similar qualitative time variation as σ_t (short-run inflation uncertainty).

[32] Interestingly, earlier literature including Ball and Cecchetti (1990) and Evans (1991) has also emphasized in another context the need to distinguish between the short-run and long-run inflation uncertainties.

(as seen in Figure 2) to bond market term premia.[33]

EMPIRICAL ISSUES

Overfitting Problems

Flexibly specified no-arbitrage models tend to entail much estimation difficulty. Part of the problem is one that is familiar from unrestricted VAR models explored in the 1980s macroeconomics literature. It is well known that unrestricted VARs often lead to poor results,[34] as these models get easily overparameterized, and as the "atheoretical" (statistical) nature of these models means that there is little structure in the model to prevent estimations from generating unreasonable outcomes.

The no-arbitrage macro-finance models have two features that exacerbate the difficulty. First, unlike the unrestricted VARs, the estimation of macro-finance models typically requires nonlinear optimization (instead of OLS), because of the nonlinear relationship between bond yields and parameters such as the market price of risk and because of the latent factors (recall the discussion regarding equation (35)).

Second, the overparameterization problem can be exacerbated by a large number of additional parameters unique to macro-finance models, including those describing the dynamics of latent factors and their interaction with observable factors (e.g., Φ_j^{ou}, Φ_j^{u}, Φ_j^{uo} in equation (35)) and those describing the market price of risk (λ_a and Λ_b in equation (1)). Note that the no-arbitrage principle guarantees the existence of a pricing kernel, such as equation (1), but the principle by itself does not constrain the parameters of the market price of risk (Λ_b matrix). Suppose, as in AP, that one has in the state vector p observable variables, its $q-1$ lags, and m unobservable (latent) variables, in other words,

$$x_t = \left[f_{1t}^o,..,f_{pt}^o,f_{1t}^u,..,f_{mt}^u,f_{1,t-1}^o,.., f_{p,t-1}^o,\cdots,f_{1,t-q+1}^o,..,f_{p,t-q+1}^o \right]' . \quad (43)$$

In that case, the Λ_b matrix in equation (1) can have as many as $(p+m)\cdot(p\cdot q+m)$ parameters.[35] For $p=2$, $q=12$, $m=3$ (as in AP), there are 135 parameters for Λ_b to be determined; even if one chooses a smaller q, the number of parameters is still quite large.

Recall that the key innovation of the macro-finance models like AP, as compared to the traditional macro models, is that they link not only the short rate $r_t(=y_{1,t})$ but also the rest of the term structure ($\{y_{\tau,t}\}_{\tau>1}$) to the macroeconomy by casting the problem in the no-arbitrage framework (1). However, the fact that yields of various maturities tend to be highly correlated (giving rise to the finding in factor analysis and principal components analysis that there is a single dominant factor) means that the pure additional information in longer-term yields (beyond what is in the short rate) may be modest in amount and perhaps too delicate to capture with a specification of the market price of risk that is liable to be overfitted; the relation that one might see between yields and macroeconomic variables in macro-finance models may be more of a statement of the Taylor rule (macro description of the short rate) than no-arbitrage models.

The overparameterization problem may be particularly severe with external basis models that contain lags of macroeconomic variables. However, internal basis models (which tend to be implemented with a comparatively smaller number of factors, e.g., three factors) may also face serious overfitting concerns, because of the especially flexible nature (the definitional freedom) of the latent-factor models. In particular, latent factor models can do a good job of fitting the *data that they are asked to fit*, even if the model or the data are poor. For example, because yield-fitting errors are minimized as a part of the

[33] There has been much discussion about the low level of term premia in recent years (see, e.g., Backus and Wright, 2007). Trying to explain the *low* term premium and *high* uncertainty would be a daunting prospect.

[34] See, e.g., Todd (1984) and Stock and Watson (2001).

[35] The lagged macro variables do not have market price of risk, but the market price of risk of contemporaneous variables and latent variables can still depend on them, as discussed in the "High-Dimensional External Basis Models" subsection.

estimation process, internal basis models with three or four factors can fit the cross section of the yield curve quite well (with much smaller fitting errors than external basis models), but that by itself might not be a sufficient reason to recommend internal basis models.

Small-Sample Problems

The implementation of macro-finance models is also complicated by small-sample problems that arise from the highly persistent nature of the data. Both interest rates and inflation are known to be persistent; unit root tests often fail to reject a nonstationarity (unit root) null for them.

In light of this, many practitioners often use nonstationary models to forecast inflation. For example, many of the inflation forecasting models used by the Federal Reserve staff impose the unit root condition.[36] By the Fisher-hypothesis intuition, unit root inflation dynamics implies unit root interest rate dynamics.

By contrast, most of the estimated macro-finance models (or nominal term structure models) in the literature assume stationarity. Stationarity has an intuitive appeal: We do not expect interest rates and inflation to have infinite unconditional moments. Thus, we may posit that the "true model" of yields is a stationary one, perhaps with many factors to describe the complex dynamics of yields and expectations; schematically,

$$y_{\tau,t} = f_{\tau}\left(x_{1t}, x_{2t}, x_{3t}, ..., x_{Nt}\right). \tag{44}$$

In practice, however, one is forced to deal with relatively low-dimensional models, because either the limited amount of data makes it impossible to pin down the parameters of such a model or one does not have enough knowledge to construct a very detailed model. In this case, it is not clear whether the "best" *low-dimensional approximation*

$$y_{\tau,t} \approx \tilde{f}_{\tau}\left(\tilde{x}_{1t}, ..., \tilde{x}_{nt}\right), \left(n << N\right) \tag{45}$$

of the model (44) should be a stationary or nonstationary (unit root) model.

The distinction between stationary and nonstationary models could be semantic in the sense that a stationary model that is close to the unit root boundary is almost indistinguishable from unit root models, but whether to assume stationarity or not can make a big difference *operationally*, as conventional estimations have the tendency to *bias down* the persistence of stationary time series, the bias becoming stronger as the sample gets smaller. This makes the expectations appear to converge to a long-run level faster than they actually do; thus, longer-horizon expectations of inflation and interest rates in (estimated) stationary models are often artificially stable, varying little from the sample mean of these variables.

Another manifestation of the small-sample problem (besides bias) is *imprecision*: Highly persistent interest rates effectively make the size of the sample "small"; no matter how frequently the data are sampled, some of the key aspects of the term structure model (those pertaining to expectations in the physical measure, as opposed to the risk-neutral measure) are difficult to estimate.[37]

Problems with the Classical Approach

Most implementations of macro-finance models have relied on classical methods such as the maximum likelihood estimation (MLE) and generalized method of moments (GMM), but these methods may be less effective in this context than is often presumed. At the heart of the matter is the fact that reduced-form macro-finance models are obviously an *approximate representation* of data, and hence not very compatible with the classical premise of having the "true model." Though it goes without saying that all models in finance are approximate, this point is particularly relevant here in view of the atheoretical (statistical) nature of the model and the large number of parameters; the MLE or GMM criterion function of these models might thus contain multiple maxima, which capture different aspects of data with differing degree of emphasis. The small-sample problems discussed above add to the difficulty, as they make asymptotic statistics a poor guide to finite sample properties.

[36] Federal Reserve staff make inflation forecast judgmentally, but they do look at a variety of models to inform their judgments. The staff's forecasting procedure is discussed, for example, in Kohn (2005).

[37] The bias and imprecision problems in term structure model estimations are discussed in detail in Kim and Orphanides (2005).

Note also that many of the classical estimation approaches implicitly minimize fitting errors for the one-period-ahead conditional moments. For example, the MLE estimation can be viewed as minimizing the one-period prediction errors or the errors in the fit of the "likelihood score moments,"

$$\left(\frac{\partial}{\partial\theta}\log f\left(y_t \mid y_{t-1},\theta\right)\right),$$

in a GMM framework. While in theory this could yield an asymptotically correct estimate of the true model (if the true model exists), the inherently approximate nature of the model means that fitting the one-period moments as closely as possible might come at the expense of other aspects of the model. Cochrane and Piazzesi (2006) in effect make this point when they note that conventionally estimated affine models may have difficulty producing the kind of term premia that they find based on regressing multiperiod (1-year) excess returns on a set of forward rates. Note also that a GMM approach that matches unconditional moments, such as

$$E\left(y_{\tau,t}\right)=\frac{1}{T}\sum_{t=1}^{T}y_{\tau,t},$$
$$E\left(y_{\tau,t}y_{\tau',t-j}\right)=\frac{1}{T}\sum_{t=1}^{T}y_{\tau,t}y_{\tau',t-j},$$

might not be effective, due to the closeness of the interest rate process (and inflation process) to the unit root behavior.

How Can We Evaluate Models?

The above discussion suggests that looking at the fit of the moments that are often used in the classical estimation might not necessarily be a good criterion for model evaluation. Some papers *do* look directly at practical implications of the model, such as the multiperiod forecasts of inflation and interest rates. Indeed, in view of the fact that the second-moment aspects of affine-Gaussian models are trivial, much attention has focused on these conditional first moments (the forecasting performance) as a part of diagnostic criteria, as in Ang and Piazzesi (2003), HTV, and Moench (2008).

However, it is unclear to what extent summary measures of forecasting performance examined in these papers can help with model evaluation/selection. To be sure, looking at the forecasting performance can be useful for detecting problematic models. In Duffee (2002), for example, interest rate forecast RMSEs that are substantially larger than the random-walk benchmark were used to highlight problems with certain stochastic-volatility no-arbitrage models (e.g., the $EA_2(3)$ specification). Similarly, the inflation forecast RMSEs based on ABW's no-arbitrage models that are substantially larger than the univariate inflation model benchmark may signal problems with the no-arbitrage models that they have used.

Nonetheless, the RMSE measures for in-sample or out-of-sample forecasts are often ineffective in discriminating between models. For instance, ABW obtain very similar RMSEs for the 1-year out-of-sample inflation forecasts from the AR(1) and the ARMA(1,1) models, although the AR(1) model implies qualitatively quite different inflation expectations than the ARMA(1,1) model, as discussed in the "Lessons from Simple Models" subsection.

Furthermore, because a large part of the inflation and interest rate variations are unforecastable, the RMSEs themselves may have substantial uncertainty (sampling variability).[38] Thus, it may happen that the "true model" generates an RMSE that is no smaller than some other models. In this sense, it may be actually misleading to focus on the RMSE as a criterion for selecting the model that best describes reality. With in-sample forecasts, this problem is exacerbated by the possibility that RMSEs are artificially pushed down because of the use of "future information" in generating the forecast, thus making interest rates and inflation look more forecastable than they actually are.

Often there are cases in which classical criteria cannot easily tell if a model's output is unreasonable, while practitioners can do so using "judgmental information." For instance, many macro-finance models estimated with data going back to 1970s generate current (circa 2006) long-horizon inflation expectations that exceed 4 percent.

[38] Clark and McCracken (2006) emphasize that out-of-sample inflation forecast RMSEs may have weak power.

(Recall also the AR and ARMA model outputs in Figure 1B.) Though long-horizon expectations are difficult to evaluate on purely econometric grounds, as there are not many nonoverlapping observations, most policymakers and market participants would immediately say that a 4 percent long-horizon CPI inflation expectation is too high; hence, models with such an output may fail the test of relevance before any statistical tests. Note also that even if two models generated similar forecast RMSEs, practitioners could have a very different assessment of them, depending on the details of the forecast errors from the models (such as the direction of the errors).[39]

These discussions highlight the role of the larger information set of practitioners (as compared with academic researchers). Unfortunately, much of this extra information is difficult to cast in the formal language of statistical tests, and the proper evaluation of models remains a challenge for macro-finance modeling.

Would a Bayesian Approach Help?

The use of Bayesian techniques to address problems with conventional (classical) estimation has a long history, but a particularly relevant early example is the Bayesian approach to VAR forecasting. As discussed in the "High-Dimensional External Basis Models" and "Overfitting Problems" subsections, unrestricted VARs share some of the key problems encountered in flexibly specified macro-finance models, in particular, the statistical (atheoretical) nature of the specification and the tendency for overparameterization. Litterman (1986) and others have documented that a Bayesian implementation with an informative prior ("random-walk prior") can generate better results than the classical implementation. This encourages us to take up a Bayesian strategy to address the empirical difficulties with macro-finance models.

In the macro-finance context, ADP have in fact already proposed a Bayesian approach, but it is not clear that the particular priors that they have used would help overcome the problems with classical estimation discussed above. ADP state that, except for the condition that the model be stationary, their priors are *uninformative*. However, to the extent that the main problem with the classical estimation of macro-finance models is that the data by themselves are not fully informative about the model (especially as regards the overfitting and small-sample problems), it is difficult to see how uninformative priors would solve the problem. Recall that the superior performance of Bayesian VARs (over conventionally estimated unrestricted VARs) came from having an *informative* prior.

When ADP (2005) tried to estimate their model using a classical method (maximum likelihood estimation), they found that the estimated model explained most of the term structure movements in terms of the latent factor, and left little role for macroeconomic variables to explain yield curve movements,[40] an outcome that is unappealing from the viewpoint of making a connection between the macroeconomy and the yield curve. However, even granting the problems with classical methods, there may be a reason for this—namely, that the estimation marginalizes the macroeconomic variables to avoid the counterfactual implication that shocks to inflation (and other macroeconomic variables) have a tight relation to yield curve movements. This is a specification issue (i.e., one has to deal with "unspanned" variation in macroeconomic variables in the model.) Addressing the problem purely as an estimation issue may lead to problems elsewhere in the model.

In my view, the main challenge for a Bayesian implementation is in coming up with suitable

[39] For example, in the 1990s, inflation data often came in on the "low" side, and it is widely believed that not all of this had been predicted by market participants, that is, the "true" market forecast of inflation in this period likely contained a mild *upward bias*. (See Kohn, 1999, and Croushore, 1990, for Fed staff and private sector forecasts in the 1990s, respectively.) Though an "unbiased" multiperiod forecast is often viewed as a consequence of rational expectations, to obtain it one needs tight assumptions that are difficult to justify in reality—in particular, the assumptions that there is a relatively simple, structurally stable model of the economy and that the agents fully know this structure. More realistic rational expectations hypotheses that relax these restrictions, for example, models that allow for learning and time-varying structure, are consistent with biased expectations in "small" samples.

[40] Private conversation with Andrew Ang. ADP's paper (2005) itself does not describe the specifics of the outcome from the classical estimation of their model.

informative priors. This is particularly the case when there are latent factors in the model (external basis models with latent factors or internal basis models): Because the economic meaning of many of the individual parameters related to the latent factors is unclear, it is difficult to provide sensible priors for them. Recall that a flexibly specified latent factor model can be normalized in many different (but equivalent) ways. It would be problematic if a Bayesian prior that was stated for one normalization of the model did not hold in another normalization of the model.[41]

By stating priors about the variables that have direct economic meaning, such as inflation expectations, interest rate expectations, and expected bond returns, one can get around the problem of normalization dependence: Surely these variables must be normalization independent. Recall also that the source of the small-sample problem is the difficulty of estimating the parameters related to expectations (in the physical measure); thus, imposing priors on these variables would help alleviate the problem. A prior about the 10-year inflation expectation, for example, can be expressed as

$$(46)\quad i^{e}_{10Y,t} = a_{10Y}(\theta) + b(\theta)'_{10Y}\, x_t(\theta) \sim N\left(\mu_t, \sigma_t^2\right),$$

with θ denoting the model parameters collectively. For μ_t, one can use a survey median forecast. Setting $\sigma_t = \infty$ corresponds to having no priors on i^{e}_{10Y}. Setting σ_t at a large value, but not large enough to be irrelevant, can be viewed as a quasi-informative prior. Other Bayesian priors that are based on economic concepts and mechanisms may be also useable.[42]

A statement like equation (46) can be conveniently incorporated within a Kalman-filter setting. Running a Kalman-filter–based MLE with a survey median (or mean) forecast (of interest rates and/or inflation) as a noisy proxy, as in D'Amico et al. (2008), can be viewed as a "poor man's Bayesian" implementation, with the point estimate serving as the mode of the Bayesian posterior.

UNDERSTANDING THE SUPERIOR PERFORMANCE OF SURVEY FORECASTS

The specification and implementation problems discussed so far may help explain why macro-finance models, which use *more* information than past inflation data, could generate *poorer* results than simple univariate inflation models. But is the yield curve information useful at all for inflation forecasting? Why do survey forecasts perform better than univariate models (and other models)?

One reason ABW offer for the superior performance of survey forecasts is that survey participants have more information about the economy than econometricians. This is in line with the point made in the "How Can We Evaluate Models?" subsection that informational differences may create a wedge between a practitioner's and an academic researcher's evaluation of a model. But it is worth exploring this issue further.

One could plausibly expect that survey forecasts may have advantages at least at short horizons, in that a potentially vast amount of information that is relevant for forecasting near-term inflation may not be easily summarized into a small number of variables. Thus, it may be instructive to examine the near-term expectations in surveys and how they are linked to longer-term expectations (i.e., the *term structure* of survey inflation forecasts).

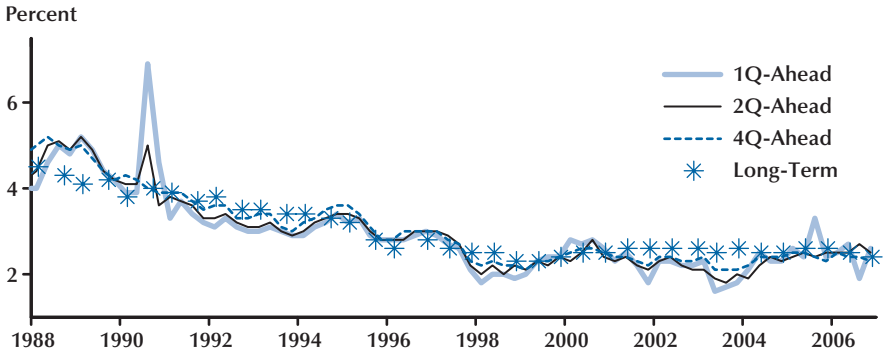
Fortunately, fairly detailed information about the near-term term structure of survey inflation expectations can be obtained, as survey forecasts such as the Survey of Professional Forecasters (SPF) and the Blue Chip Financial Forecasts (BCFF) provide CPI inflation forecasts up to the next four or more quarters. Figure 3 shows the 1-, 2-, and 4-quarter-ahead CPI inflation forecasts from the BCFF survey, based on the surveys published in January, April, July, and October (taken at the end of December, March, June, and September), from 1988 to 2006. The figure also shows the BCFF long-horizon forecast (inflation

[41] See the working paper versions of this paper (Kim, 2007) for elaboration.

[42] "Structural" priors can be also imposed in a Bayesian setting, as in the dynamic stochastic general equilibrium (DSGE) modeling literature.

Figure 3

Forecasts of U.S. Quarterly CPI Inflation from the BCFF



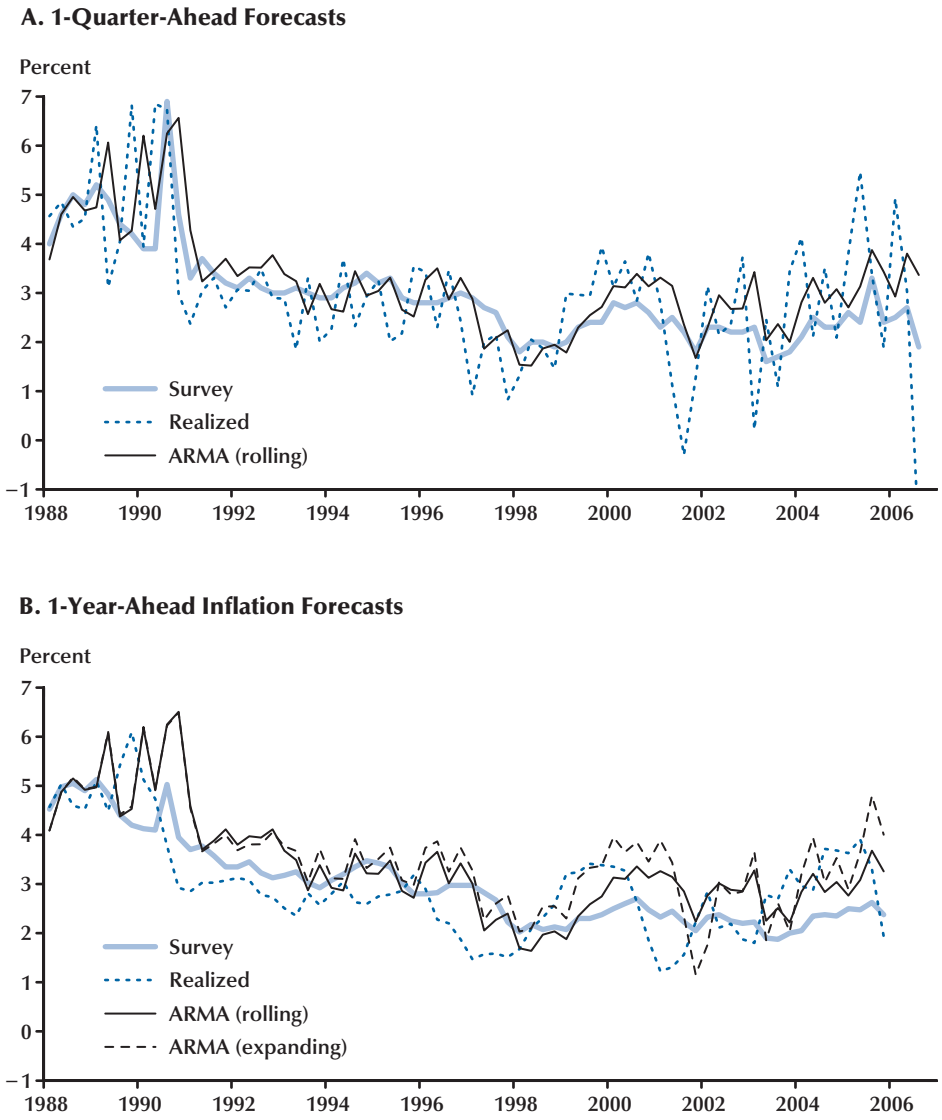
expected between the next 5 and 10 years), which is available twice per year, is also shown. It is notable that this long-horizon forecast, which can be viewed as a "quasi–long-run" mean of inflation, has moved about (shifted down) significantly. It is also notable how quickly the multiperiod forecasts approach the quasi–long-run value. The 4-quarter-ahead forecast and the 2-quarter-ahead forecast are already quite similar to the long-horizon forecast. Note that even in 1990:Q3, when the 1-quarter-ahead inflation expectation peaked, the expectations for longer horizons show that the survey participants expected inflation to come down quickly to the quasi–long-run level. Thus, one comes to a conclusion that "the long term is quite near."

To get further insights into the survey forecasts, it is useful to compare them with ex post realized inflation and the real-time forecasts from the ARMA(1,1) model. Figure 4A shows the 1-quarter-ahead inflation forecasts based on the BCFF survey and the ARMA(1,1) model (20-year rolling sample forecast), as well as the realized quarterly inflation (π_t plotted at $t-1$). The vertical difference between realized inflation and the survey forecast or the ARMA forecast is the forecast error. This error is indeed smaller for the survey forecast. (The RMSEs of the 1-quarter-ahead forecast are 1.19 percent and 1.40 percent in annual percentage units for the survey forecast and the ARMA(1,1) model, respectively.) Note that the 1-quarter-ahead survey forecast is much less jagged than realized inflation or the ARMA(1,1) forecast. Granting the caveat that surveys might not necessarily be the best possible means of forecasting, this suggests that a substantial part of short-run inflation is unforecastable ex ante, lending support to a formulation like the two-component model in equation (27) in which the inflation process is separated into a trend inflation component and an unforecastable component.

Let us now examine the 1-year inflation forecast, shown in Figure 4B. The ARMA forecasts (both the rolling and the expanding samples) performed worse than the survey forecast with RMSEs of 1.04 percent for the 20-year rolling sample ARMA, 1.15 percent for the expanding sample ARMA, and 0.76 percent for the survey. The basic reason for the superior forecast of the survey is that the ARMA model–based forecasts substantially overpredicted inflation in the 1990s. It can be seen that the ARMA forecasts lie notably above the realized inflation (and survey forecast). This overprediction is due in large measure to the fact that the ARMA model in real time tended to generate "too high" values of the long-run mean level (μ in equation (23)) to which the forecasts are converging. This is illustrated in Figure 5, where the long-run mean parameter μ from the expanding sample estimation lies significantly above the long-horizon survey forecast. Because the expand-

Figure 4

Comparison of Realized CPI Inflation, the BCFF Survey Forecasts, and the ARMA(1,1) Model Forecasts

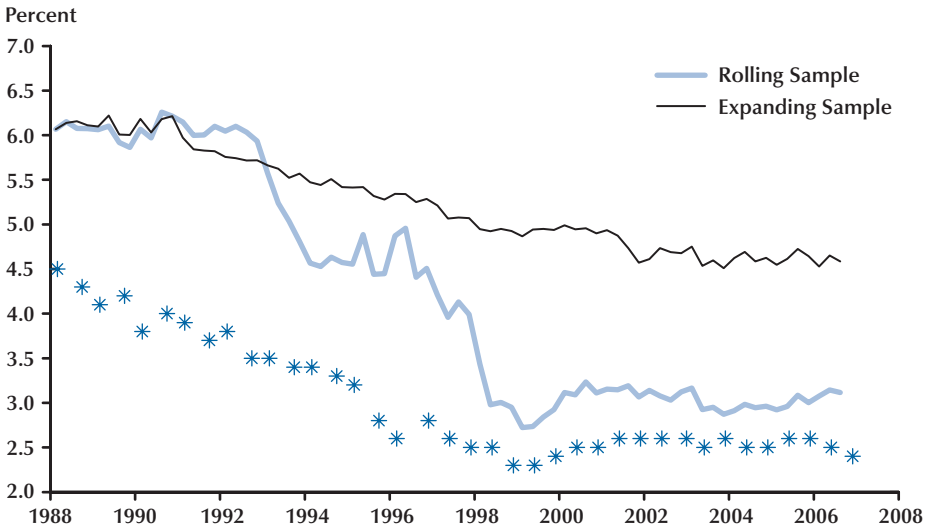


ing sample includes periods of high inflation (1970s and early 1980s), the estimated mean does not fall quickly with declining inflation in the '80s and '90s. The use of the 20-year rolling sample produces lower μ (than the expanding sample) as the estimation sample moves away from those periods, but still the adjustment in the long-run mean is not as fast as in the survey forecast.[43]

[43] ABW also note that the ability of survey forecasts to quickly adapt to major changes in the economic environment contributes to the superior performance of the surveys. While the majority of ABW's estimations were done with expanding samples, they also examine the forecast RMSEs based on rolling-sample estimation for a subset of their models. Because their rolling sample (10 years) is shorter than the 20-year rolling sample used here, ABW's rolling-sample results are even closer to those of the surveys. For example, the ratio of the AR model RMSE and the survey RMSE in the post-1995 window is 0.879/0.861, very close to 1.

Figure 5

Long-Run Means from the ARMA(1,1) Model Estimations of U.S. Quarterly Inflation Data



NOTE: The long-horizon BCFF survey inflation forecast is also shown (*).

The key point that emerges from this discussion is that surveys produce a more successful forecast of inflation in large part because they capture the *trend* component of inflation better than time-series models such as the ARMA(1,1) model. In stationary time-series models (for example, the models in Figure 4), forecasts tend to converge to a value close to the sample mean, while nonstationary models put too much weight on the recent past; thus, there is scope for judgmental information to play a role, especially if trend inflation varies significantly over time. These considerations shed light on the attention that policymakers pay to long-term inflation expectations (a better indicator of the trend inflation than realized inflation) and on the use of judgmental forecasts at central banks such as the Federal Reserve.

The importance of modeling the variation of long-term expectations deepens the challenge for macro-finance models: Besides the specification challenge, the nearly nonstationary nature of the inflation process indicated by the substantial variability of long-term survey forecasts poses considerable empirical difficulties (discussed in the previous section). These challenges notwithstanding, the discussions in this paper can be viewed as encouraging for attempts to use term structure models to extract inflation expectations: It makes intuitive sense that the yield curve contains, at least, information about trend inflation, and the indication that the near-term informational advantage of surveys seems to wear out quickly (beyond a few quarters) gives some hope that models could capture much of the variation in inflation expectations and compete with surveys.[44]

[44] Although ABW find that survey forecasts cannot be improved by combination with models that they consider, few policymakers would regard survey forecasts as the ultimate measure of inflation expectations. Consider, for example, the fact that between 1999 and 2006 the 10-year CPI inflation expectation from the SPF survey has been almost stuck at 2.5 percent. While there is a broad consensus that long-term inflation expectations were "better anchored" in the 2000s than in the earlier decades, it may be a stretch to regard that long-term inflation expectation has become so well anchored as to be practically immovable. This may be one example in which the yield curve contains useful information that is unavailable in the SPF survey.

CONCLUSION

These are some of my key points made in this paper: (i) Not all of the variation in key macroeconomic variables is related to yield curve movements. (ii) The yield curve contains useful information about the *trend* component of inflation. (iii) The no-arbitrage principle might not be sufficient to guarantee sensible outputs from macro-finance models in practice.

As I have stressed in the second section of the paper ("The Basic Model"), the spanning argument is the basis of the no-arbitrage framework; hence the presence of a short-run inflation component that is not related to yield curve movements may undermine the validity of the models that use inflation as a state variable. Such a component may also cause special difficulties when one tries to go beyond the affine-Gaussian setup to model time-varying uncertainties about macroeconomic variables explicitly. For example, as discussed in the "Time-Varying Uncertainty" subsection, monthly CPI inflation in recent years has been more volatile than in the 1990s, but there is no strong evidence that this is reflected in the yield curve (e.g., as an increased term premium); an attempt to link them may thus lead to more serious specification errors.

I have also argued in this paper that much of the "spanned" component of inflation (the part of inflation that *is* related to the yield curve) is about the *trend* component (whose importance was stressed in the discussion in the previous section of why surveys perform better). This can help resolve the puzzle that the "conventional wisdom" that the change in nominal yields often reflects changes in inflation expectations dies hard, despite the poor performance of inflation forecasting models involving term structure variables. In some sense, the latent factor models can be viewed as a way to represent markets' implicit processing (filtering) of information.

No-arbitrage models of the term structure have been viewed as a promising way to go beyond the restrictive assumptions implicit in the expectations hypothesis (about how risk is incorporated in the yield curve). However, reduced-form affine-Gaussian no-arbitrage models with flexible specification of the market price of risk can quickly become "too unrestrictive," with a profusion in the number of parameters. In other words, the no-arbitrage principle by itself may be too weak to provide enough discipline in the model. Note also that the two technical problems with estimation discussed in the "Empirical Issues" subsection (overfitting and small-sample problems) can be viewed as an extension of the specification discussion, as the main source of the problems can be viewed as insufficient information in the data or an incomplete structure in the model. For further progress, it would be desirable to come up with an effective and non-ad hoc structure on the market price of risk and other parameters of macro-finance models—or to come up with a new, intuitively appealing way to represent the term structure.

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II

Investment Analysts' Forecasts of Earnings

Rocco Ciciretti, Gerald P. Dwyer, and Iftekhar Hasan

The literature on investment analysts' forecasts of firms' earnings and their forecast errors is enormous. This paper summarizes the evidence on the distribution of analysts' forecasts and forecast errors using data for all U.S. firms from 1990 to 2004. The evidence indicates substantial asymmetry of earnings, earning forecasts, and forecast errors. There is strong support for average and median earning forecasts being higher than actual earnings a year before the earnings announcement. Such differences between earnings and forecasts also exist across time periods and industries. A month before the earnings announcement, the mean and median differences are small. (JEL G17, C53)

Federal Reserve Bank of St. Louis *Review*, September/October 2009, *91*(5, Part 2), pp. 545-67.

Do stock analysts provide information on stocks, or are they merely salespeople issuing one-sided information about stocks? In addition to forecasting earnings that are used by some investors when they buy various firms' stocks, analysts at investment banks often have participated in other activities such as convincing the same firms to hire the investment bank to issue stock. These activities were the basis of suits by the New York attorney general against major investment banks. Rather than proceeding to trial, the charges were settled in April 2003. In the settlement, investment banks agreed to substantial changes in their business practices designed to provide less incentive for analysts to be influenced by the investment banks' other activities. The investment banks also agreed to make payments totaling $1.4 billion, which covered fines, payments to investors, funding of investor education, and funding of research by independent analysts. This settlement brings into question the informativeness of analysts' projections of earnings, suggesting that analysts' projections of earnings largely or substantially reflect analysts' interests rather than an assessment of a firm's prospects.

On the other hand, charges of an insider-trading scheme in 2007 suggest that analysts' forecasts do contain information and affect prices. This scheme involved an accomplice receiving advance information about analysts' forecasts and taking positions before the announcements (Smith, Scannell, and Davies, 2007). This scheme makes no sense if analysts' forecasts are uninformative and ignored. While indicating that at least some analysts' forecasts may be informative, such activities do not imply that forecasts cannot be improved. It is possible to take imperfect information and filter out predictable misinformation.

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Are there predictable differences between analysts' earnings forecasts and actual earnings? Many papers show that the analysts' forecast errors are predictably different from actual earnings.[1] The evidence indicates that analysts' forecasts of earnings well before the announcement are higher on average than actual earnings. Whatever earnings an analyst forecasts for a firm, a better prediction is a somewhat lower level of earnings. This predictable difference is called a "bias" in the forecasts.[2] Some papers also suggest that analysts' forecasts close to the earnings announcement decline to less than the actual earnings. The rationale for this reverse bias is a suggestion that earnings greater than recent forecasts are interpreted as a positive earnings surprise and the firm's stock price increases.

This paper provides an overview of analysts' forecasts and the forecasts' relationship to actual earnings. Our data are for U.S. analysts' forecasts of U.S. firms' earnings from 1990 through 2004. These data show the usual result that analysts' forecasts are greater than earnings on average. We look at the distribution in more detail and find that the distribution of earnings is asymmetric. The distribution of earnings forecasts also is asymmetric but not sufficiently asymmetric that forecast errors are symmetric; earnings forecast errors also are asymmetric. We also find that median forecasts are closer to actual forecasts than are mean forecasts. We examine differences between actual earnings and earnings forecasts over time and by industry. We find substantial differences in forecast accuracy across industries and larger forecast errors during recessions. Forecast errors at the 1-month horizon are small in magnitude.

ERRORS IN FORECASTING EARNINGS PER SHARE

Data

Analysts forecast companies' earnings per share, and the forecast error is the difference between actual earnings and these forecasts of earnings. There is a scale problem with using the level of forecasts across firms and over time. A firm with the same total earnings as another but half as many shares outstanding will have earnings per share that are twice as large. One way to adjust for differences in the magnitude of earnings per share and forecast errors across firms is to divide the forecast error by the stock price. Dividing by the stock price assumes that errors in forecasting earnings per share relative to the stock price are relatively homogeneous across firms. Earnings per share relative to the stock price is the inverse of the price-to-earnings ratio, often used as part of the information used to evaluate companies.[3]

The forecast error relative to the stock price is indicated as follows:

$$e^{i,j}_{T,t} = \frac{a^i_T - f^{i,j}_{T,t}}{p^i_{T-1}}, \tag{1}$$

where $e^{i,j}_{T,t}$ is the computed relative forecast error for company i by analyst j for year T made t months before the release date, a^i_T is actual earnings per share of company i in year T, $f^{i,j}_{T,t}$ is the forecasted earnings per share for company i by analyst j made for year T with the forecast being made t months before the release date, and p^i_{T-1} is the stock price for company i at the end of the previous year, T–1.

The forecast horizon, t, is calculated as the difference in months between the estimation date (I/B/E/S [Institutional Brokers' Estimate System] variable ESTDATX) and the report date (I/B/E/S variable REPDATX). We use the report date instead of forecast period end date (FPEDATX) because analysts can make forecasts between the fiscal year's end and the date earnings are reported.

The data on forecasts of earnings per share and actual earnings per share are from the I/B/E/S

[1] Sirri (2004) summarizes a few of these papers and provides references.

[2] Not all research agrees that analysts' forecasts are biased—for example, Keane and Runkle (1990, 1998).

[3] Another way to scale earnings per share is to divide by the level of earnings to get the proportional error in forecasting earnings. Earnings close to zero and negative earnings create serious problems for this normalization. Dividing by earnings can generate a very large relative forecast error as earnings go to zero; dividing by negative earnings would change the sign of the forecast error. Stock prices cannot be negative and are strictly positive in our data. Although prices can approach zero, earnings generally approach zero at a related rate, which is another way of saying that earnings per share relative to the stock price are relatively homogeneous across firms.

Detail History (with Actuals) database for 1990 through 2004. Any company with at least one forecast between 1990 and 2004 is included in the initial database.

The stock prices are from the Center for Research in Security Prices (CRSP) database from 1989 to 2003. The earnings in any year are divided by the stock price at the end of trading in the prior year. With this choice of stock price, the stock price does not reflect the changes in forecasts or the ensuing forecast errors made during the year.

The initial number of observations on forecasts is 1,835,642. To avoid nonsynchronized timing of forecasts by year, we restrict the analysis to companies with fiscal years ending in December.[4] This reduces the number of observations to 1,207,445. We further restrict our analysis to forecasts by analysts located within the United States, which reduces the number of observations to 678,427 forecasts for 6,731 companies. In this paper, a company's stock is defined by the six-digit Committee on Uniform Securities Identification Procedures (CUSIP) number followed by an "01"; this indicates a common stock. We match U.S. companies from I/B/E/S and CRSP databases by CUSIP. We also associate an industry code according to the Global Industries Classification Standard from Standard & Poor's.

Finally, to eliminate possible transcription errors, we cut off the distributions of both actual and forecasted earnings per share relative to the stock price at the 1st and 99th percentiles for each year and forecast horizon.[5] This results in a dataset with 662,016 observations for 6,574 companies. The number of firms included in the analysis increases over time. The number of U.S. companies with a fiscal year ending in December and an earnings' forecast by at least one U.S. analyst increased from 1,446 in 1990 to 2,569 in 2004. The analyses by industry use the industry classification, which is not available for 104,840 observations. As a result, the analyses by industry use 557,176 observations instead of the whole sample of 662,016 observations.

Distribution of Forecast Errors

Figure 1 shows the distributions of earnings and forecasted earnings. The graphs show the distribution of actual earnings and the distribution of forecasts by analysts made 1 month, 6 months, and 12 months before the earnings announcement. For example, the first graph (Figure 1A) shows actual earnings per share relative to the stock price and forecasts made 1 month before the announcement of earnings. The second graph (Figure 1B) shows the distribution of earnings and the distribution of the forecasts made 6 months before the earnings announcement, and the third graph (Figure 1C) shows the distribution of earnings and the distribution of the forecasts made 12 months before the earnings announcement.[6] Deleting the top and bottom 1 percent of the distribution still leaves quite long tails to the distribution of earnings and, to a lesser but still easily discernible extent, the forecasts. To avoid obscuring detail, we also truncate these figures at –$0.50 and +$0.50 per dollar of share price. Table 1 shows the distribution of earnings, forecasts, and the forecast errors without the truncation. Relative to the total number of observations, the truncation excludes a small number of observations, mostly in the negative tail of the distributions.

The forecasts and actual earnings are strikingly similar, which is consistent with the forecasts being quite informative about actual earnings. The histograms for forecasts and actual earnings are distinguishable, but the overlap far outweighs the differences. The dashed vertical lines are drawn at the mean of actual earnings. The most common—modal—values of forecasted and actual earnings are similar. The solid curves in the figure represent normal distributions with the same

[4] When looking at data by year, having the same end date means that the same events are occurring at the same horizon for all firms. Firms with fiscal years ending in December represent about 74 percent of all firms in the I/B/E/S database.

[5] The results in Tables 2 through 4 were computed with the tails of the distribution of the data included. The results are broadly similar.

[6] The distribution of earnings is not the same at each of the horizons. The figure shows the distribution of all forecasts and the distribution of the actual earnings that were predicted. Every firm with a forecast appears in the figure; every firm with no forecast does not appear in the figure. In addition, every firm with more than one forecast appears in the figure the same number of times as the number of its forecasts.

Figure 1

Actual Earnings and Earnings Forecast

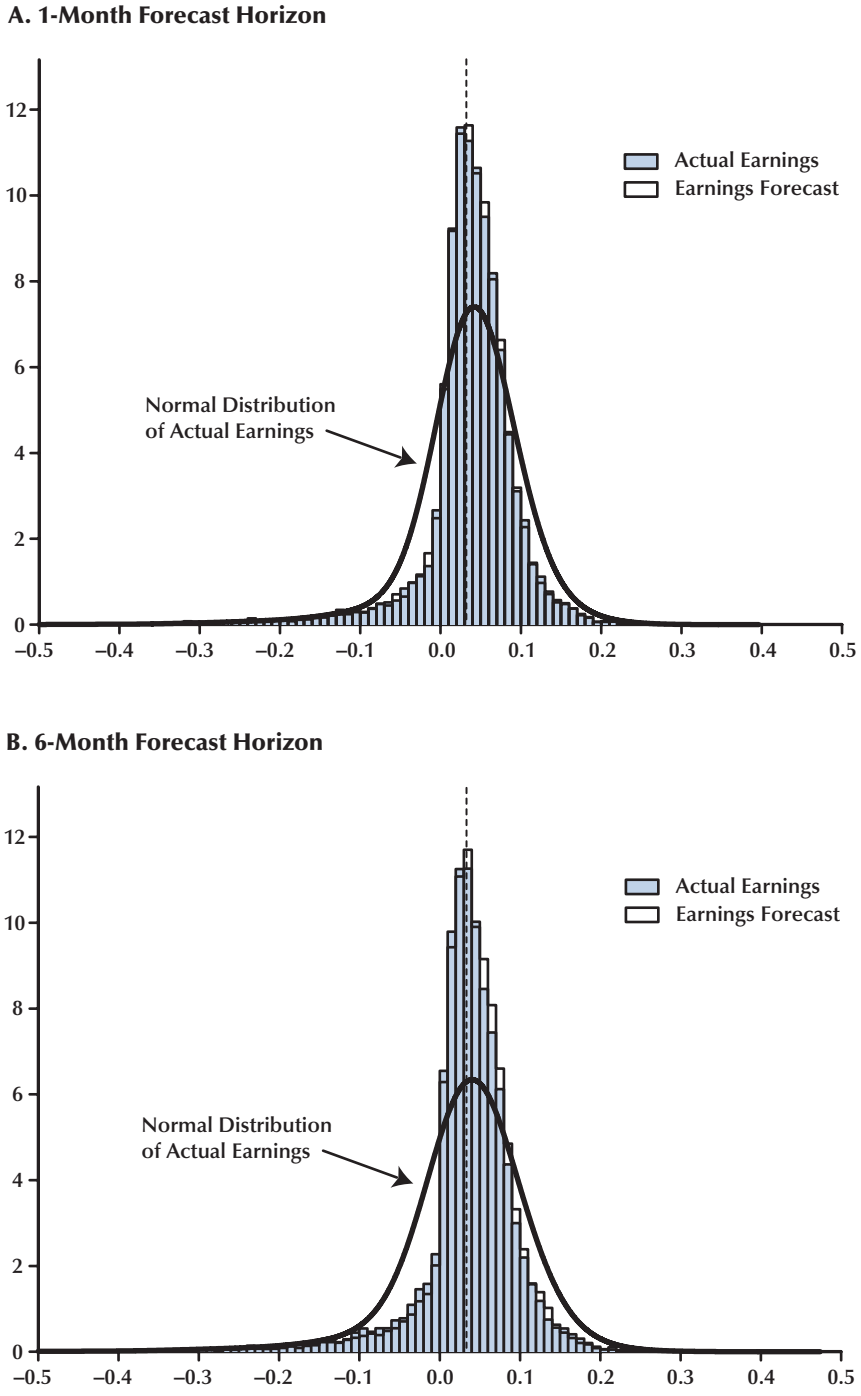


Figure 1, cont'd

Actual Earnings and Earnings Forecast

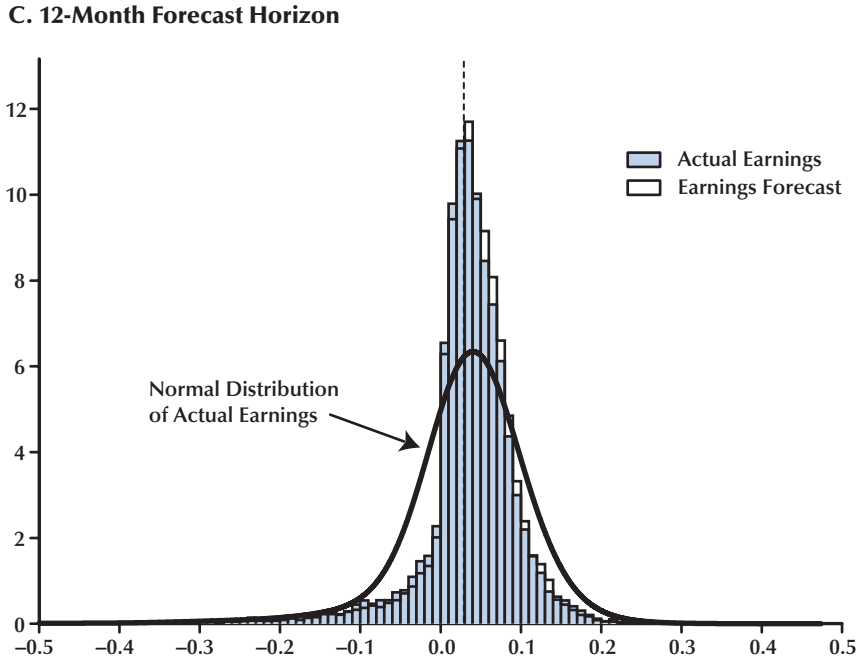


Table 1

Summary of Minimum and Maximum Values and Observations Suppressed in Figures 1 and 2

	12-Month horizon			6-Month horizon			1-Month horizon		
Variable	Minimum	Maximum	Number of suppressed observations	Minimum	Maximum	Number of suppressed observations	Minimum	Maximum	Number of suppressed observations
Actual earnings	–1.6137	0.2844	150	–1.1820	0.3350	58	–0.9026	0.2844	11
Earnings forecasts	–1.1532	0.2933	76	–0.7732	0.3267	21	–0.6487	0.2778	10
Forecast errors	–1.2442	0.7614	89	–1.1561	0.5533	15	–0.6085	0.3531	2

NOTE: For actual earnings and earnings forecasts there are no positive observations outside the –0.5 to +0.5 range. For forecast errors, there are 6, 2, and 0 excluded positive observations at the 12-, 6-, and 1-month forecast horizons, respectively; the remainder are negative observations.

Figure 2

Forecast Errors

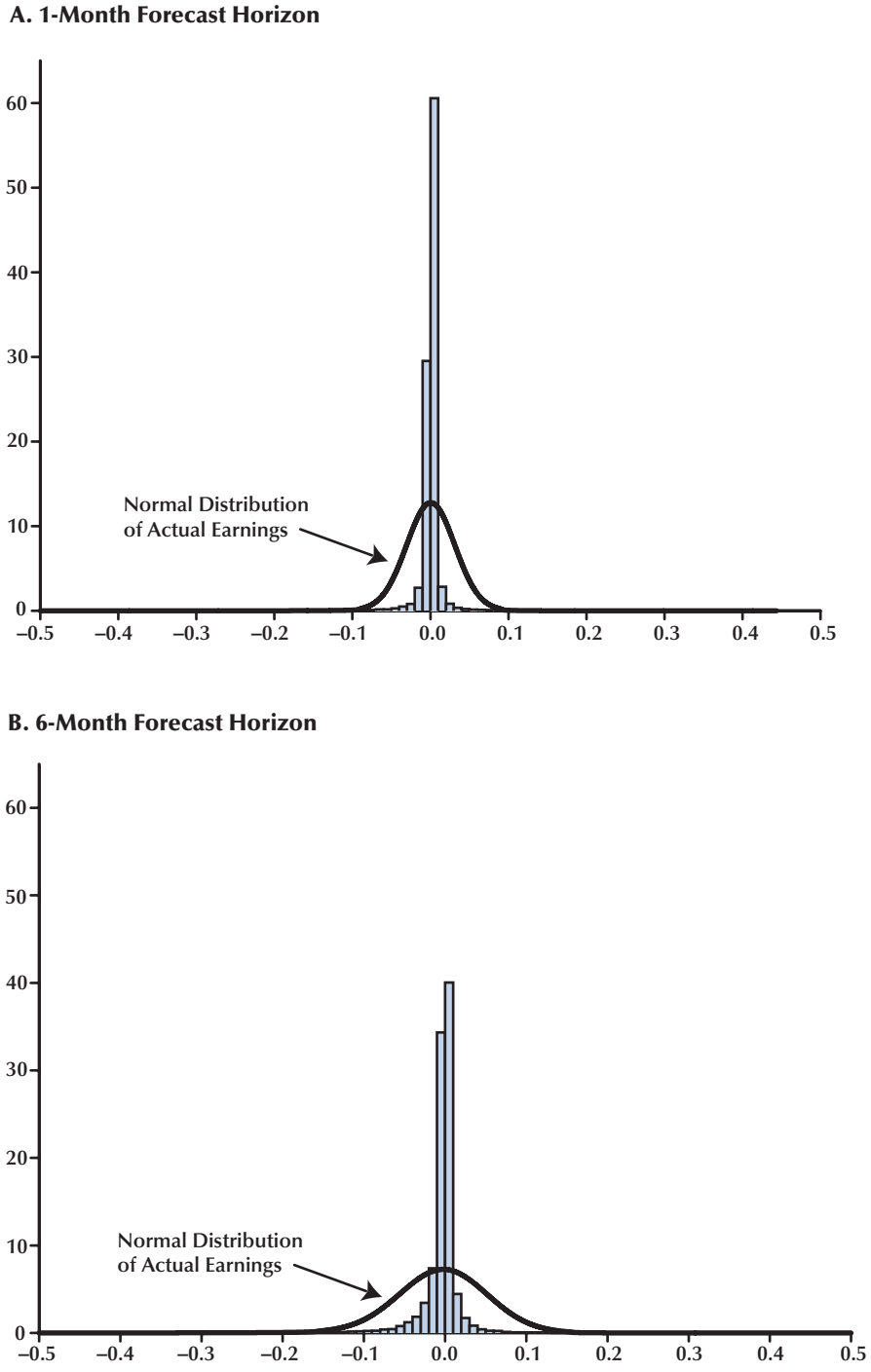
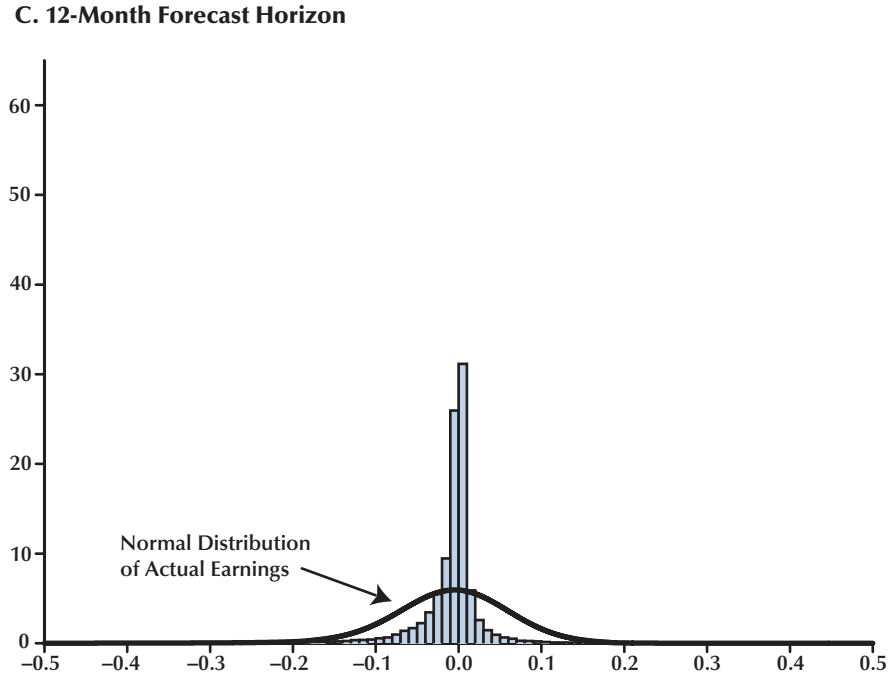


Figure 2, cont'd

Forecast Errors



means and standard deviations as actual earnings. Actual and forecasted earnings have higher peaks at the mean value than the normal distribution and also have fatter tails. Because the total area must add up to 100 percent, this implies that the distributions of actual and forecasted earnings have fewer observations between the tails and the center of the distribution.

The graph of the 12-month-ahead forecasts shows the bias in longer-term forecasts. Although the distributions of actual and predicted earnings are quite similar, the histogram shows the tendency of more forecasts of above-average earnings and fewer forecasts of below-average earnings than actual earnings. The distribution of the 6-month-ahead forecasts shows less bias. The distribution of the 1-month-ahead forecasts is more similar to the actual earnings.

The literature focuses on the deviations between the earnings and the forecasts, which makes it easy to lose sight of how informative the forecasts are about *actual* earnings. Analysts' earnings forecasts are quite informative about actual earnings.

Figure 2 shows the distributions of the forecast errors. A "positive forecast error" means that actual earnings exceed the forecasted earnings. A "negative forecast error" means that actual earnings fall short of the forecasted earnings. If all analysts forecasted earnings within a penny of earnings per dollar of share price, all the forecast errors would be within the two bars surrounding zero. Recall that the share price is the price before the start of the fiscal year, so this indicates that the analysts are coming very close to forecasting actual earnings. In fact, the forecast errors are quite peaked near zero, whether 12 months, 6 months, or 1 month before the announcement of actual earnings.

The earnings forecasts are closer to actual earnings 1 month before the earnings announcement than 12 months before the earnings announcement.

Table 2

Distribution of Forecast Errors by Year and Horizon

	Minimum	1%	5%	10%	25%	Median	75%	90%	95%	99%	Maximum	Mean	Standard deviation	Skewness coefficient	Kurtosis
12-Month horizon															
1990	–0.81	–0.4278	–0.1265	–0.0721	–0.0249	–0.0040	0.0003	0.0059	0.0121	0.0456	0.09	–0.0270	0.0754	–4.98	31.33
1991	–0.88	–0.3711	–0.1320	–0.0770	–0.0245	–0.0048	0.0002	0.0068	0.0177	0.0667	0.30	–0.0249	0.0711	–4.95	37.73
1992	–0.40	–0.2019	–0.0922	–0.0509	–0.0158	–0.0023	0.0012	0.0098	0.0193	0.0557	0.12	–0.0141	0.0418	–3.53	18.96
1993	–0.38	–0.1789	–0.0649	–0.0367	–0.0110	–0.0011	0.0022	0.0088	0.0185	0.0636	0.11	–0.0095	0.0368	–3.69	22.69
1994	–0.47	–0.1807	–0.0629	–0.0334	–0.0091	–0.0003	0.0024	0.0100	0.0194	0.0554	0.17	–0.0096	0.0431	–6.08	52.96
1995	–0.27	–0.1297	–0.0618	–0.0367	–0.0099	0.0000	0.0039	0.0118	0.0201	0.0633	0.18	–0.0071	0.0309	–2.50	16.08
1996	–0.29	–0.1455	–0.0697	–0.0379	–0.0100	–0.0001	0.0032	0.0134	0.0256	0.0593	0.20	–0.0078	0.0337	–2.20	13.34
1997	–0.45	–0.1566	–0.0608	–0.0329	–0.0093	–0.0008	0.0023	0.0085	0.0143	0.0400	0.11	–0.0094	0.0362	–5.56	49.00
1998	–0.49	–0.2378	–0.0704	–0.0495	–0.0198	–0.0035	0.0010	0.0060	0.0131	0.0419	0.27	–0.0154	0.0422	–4.19	29.79
1999	–0.76	–0.2484	–0.0743	–0.0391	–0.0119	0.0000	0.0050	0.0224	0.0430	0.1306	0.39	–0.0079	0.0576	–3.74	39.19
2000	–0.51	–0.2230	–0.0752	–0.0395	–0.0120	0.0003	0.0055	0.0276	0.0634	0.1277	0.31	–0.0054	0.0508	–2.41	17.01
2001	–1.24	–0.3840	–0.1364	–0.0785	–0.0335	–0.0086	0.0007	0.0091	0.0208	0.1803	0.76	–0.0265	0.0895	–4.00	50.19
2002	–0.74	–0.2228	–0.0656	–0.0370	–0.0114	–0.0002	0.0064	0.0234	0.0426	0.0976	0.32	–0.0067	0.0522	–5.09	53.33
2003	–0.71	–0.1839	–0.0617	–0.0339	–0.0104	0.0003	0.0092	0.0266	0.0443	0.0949	0.28	–0.0045	0.0464	–3.98	38.24
2004	–0.33	–0.1148	–0.0438	–0.0212	–0.0068	0.0010	0.0088	0.0264	0.0394	0.0812	0.14	–0.0003	0.0317	–3.10	26.77
6-Month horizon															
1990	–1.16	–0.2730	–0.0955	–0.0427	–0.0122	–0.0016	0.0008	0.0060	0.0142	0.0575	0.20	–0.0162	0.0669	–7.95	92.95
1991	–0.54	–0.2171	–0.0642	–0.0353	–0.0097	–0.0015	0.0009	0.0074	0.0176	0.0600	0.18	–0.0108	0.0441	–5.33	44.17
1992	–0.32	–0.1301	–0.0444	–0.0219	–0.0071	–0.0006	0.0013	0.0062	0.0122	0.0357	0.11	–0.0066	0.0276	–5.01	39.50
1993	–0.16	–0.0814	–0.0247	–0.0137	–0.0037	–0.0001	0.0018	0.0066	0.0142	0.0409	0.18	–0.0024	0.0181	–2.34	24.80
1994	–0.17	–0.0705	–0.0284	–0.0159	–0.0041	0.0000	0.0024	0.0076	0.0129	0.0400	0.16	–0.0025	0.0170	–1.96	20.70
1995	–0.30	–0.0828	–0.0330	–0.0169	–0.0044	0.0000	0.022	0.065	0.111	0.2930	0.10	–0.0038	0.0198	–5.37	52.00
1996	–0.32	–0.0969	–0.0287	–0.0152	–0.0038	0.0001	0.0024	0.0090	0.0151	0.0389	0.19	–0.0029	0.0227	–4.78	54.34
1997	–0.27	–0.0907	–0.0275	–0.0132	–0.0030	0.0001	0.0023	0.0079	0.0146	0.0422	0.17	–0.0021	0.0206	–2.77	38.07

NOTE: *This test statistic has a chi-square distribution with two degrees of freedom under the null hypothesis. The value of this chi-square at the 0.001 level of significance is 13.8. All of the values in the table have p-values less than 10^{-8}.

Table 2, cont'd

Distribution of Forecast Errors by Year and Horizon

	Minimum	1%	5%	10%	25%	Median	75%	90%	95%	99%	Maximum	Mean	Standard deviation	Skewness coefficient	Kurtosis
6-Month horizon, cont'd															
1998	–0.33	–0.0992	–0.0359	–0.0219	–0.0081	–0.0016	0.0008	0.0043	0.0094	0.0290	0.29	–0.0063	0.0226	–3.18	49.61
1999	–0.56	–0.1600	–0.0446	–0.0202	–0.0048	0.0001	0.0031	0.0109	0.0193	0.0533	0.55	–0.0052	0.0383	–3.74	78.39
2000	–0.36	–0.1101	–0.0447	–0.0221	–0.0059	0.0000	0.0022	0.0136	0.0261	0.0668	0.17	–0.0037	0.0273	–2.68	26.48
2001	–0.64	–0.1714	–0.0494	–0.0274	–0.0092	–0.0015	0.0012	0.0074	0.0141	0.0581	0.20	–0.0085	0.0391	–5.95	66.46
2002	–0.38	–0.0997	–0.0325	–0.0158	–0.0054	–0.0003	0.0027	0.0088	0.0159	0.0402	0.21	–0.0038	0.0269	–6.09	76.24
2003	–0.49	–0.0994	–0.0295	–0.0140	–0.0036	0.0004	0.0045	0.0125	0.0213	0.0667	0.38	–0.0011	0.0310	–2.52	68.31
2004	–0.29	–0.0617	–0.0284	–0.0184	–0.0045	0.0000	0.0032	0.0092	0.0164	0.0389	0.09	–0.0025	0.0195	–5.05	57.05
1-Month horizon															
1990	–0.61	–0.0970	–0.0286	–0.0146	–0.0031	–0.0001	0.0014	0.0054	0.0131	0.0526	0.22	–0.0035	0.0342	–11.48	204.59
1991	–0.24	–0.0659	–0.0231	–0.0111	–0.0024	0.0000	0.0020	0.0074	0.0141	0.0395	0.13	–0.0015	0.0188	–2.99	48.29
1992	–0.14	–0.0698	–0.0118	–0.0053	–0.0010	0.0002	0.0025	0.0073	0.0144	0.0402	0.24	0.0006	0.0220	4.09	61.43
1993	–0.26	–0.0659	–0.0127	–0.0064	–0.0012	0.0001	0.0020	0.0062	0.0112	0.0400	0.10	–0.0005	0.0154	–4.97	71.88
1994	–0.11	–0.0274	–0.0079	–0.0039	–0.0007	0.0002	0.0020	0.0057	0.0104	0.0289	0.09	0.0006	0.0102	–1.20	41.64
1995	–0.22	–0.0455	–0.0093	–0.0048	–0.0009	0.0002	0.0019	0.0057	0.0114	0.0390	0.31	0.0004	0.0188	1.28	104.42
1996	–0.20	–0.0277	–0.0078	–0.0036	–0.0005	0.0003	0.0017	0.0054	0.0097	0.0482	0.17	0.0008	0.0137	–0.90	89.84
1997	–0.36	–0.0375	–0.0114	–0.0047	–0.0006	0.0003	0.0019	0.0054	0.0096	0.0325	0.19	0.0002	0.0145	–6.48	217.27
1998	–0.16	–0.0256	–0.0089	–0.0044	–0.0006	0.0003	0.0017	0.0050	0.0089	0.0285	0.20	0.0004	0.0102	1.12	110.97
1999	–0.23	–0.0410	–0.0069	–0.0031	–0.0004	0.0004	0.0023	0.0062	0.0116	0.0457	0.28	0.0011	0.0158	1.31	118.62
2000	–0.24	–0.0673	–0.0141	–0.0057	–0.0007	0.0002	0.0013	0.0044	0.0088	0.0291	0.11	–0.0011	0.0147	–6.61	83.84
2001	–0.18	–0.0371	–0.0101	–0.0038	–0.0005	0.0002	0.0014	0.0038	0.0066	0.0211	0.08	–0.0004	0.0104	–6.24	94.52
2002	–0.26	–0.0340	–0.0079	–0.0036	–0.0005	0.0003	0.0013	0.0038	0.0067	0.0211	0.35	–0.0002	0.0135	–0.63	268.15
2003	–0.36	–0.0645	–0.0100	–0.0047	–0.0007	0.0003	0.0018	0.0054	0.0097	0.0373	0.15	–0.0003	0.0157	–7.81	145.27
2004	–0.15	–0.0333	–0.0078	–0.0037	–0.0007	0.0004	0.0022	0.0052	0.0087	0.0255	0.15	0.0006	0.0092	–0.89	77.55

NOTE: *This test statistic has a chi-square distribution with two degrees of freedom under the null hypothesis. The value of this chi-square at the 0.001 level of significance is 13.8. All of the values in the table have p-values less than 10^{-8}.

This convergence is expected if the forecasts are informed predictions. More information becomes available as time goes on, and this information is substantial: Eleven-twelfths of the year is past when the 1-month-ahead forecast is made. Firms announce earnings quarterly; when the 1-month-ahead forecast is made, earnings for the first three quarters of the year have been announced and are known. Besides this relatively mechanical effect as time passes, other information becomes known about earnings as time passes and the magnitudes of forecast errors can be expected to decrease.

Over 90 percent of the forecasts made 1 month before the earnings announcement are within one penny of earnings per dollar of share price. There is a clear asymmetry in the distribution of these close forecast errors: 60 percent of the earnings are more than the forecasts and within a penny; 30 percent of the earnings are less than the forecasts and within a penny. The larger number of positive forecast errors can reflect analysts' forecasts that the analyst knows are too low; it also can occur for other reasons. For example, firms with actual earnings less than forecasted earnings may provide analysts with information before the announcement and forecasts are revised accordingly. The forecast errors 12 months ahead and 6 months ahead also show asymmetry, with many forecasts within a penny of actual earnings but more above zero than below.

Table 2 shows detailed information about the distributions of forecast errors by year at 12-month, 6-month, and 1-month horizons. The table shows the maximum and minimum values, the mean, standard deviation, measures of the skewness, and kurtosis of the distribution of forecast errors and selected percentiles of the distributions.

As Figure 2 suggests, the forecasts a month before the earnings announcement are much closer to actual earnings than are forecasts a year in advance. The standard deviation of forecast errors is a measure of the size of analysts' errors, independent of whether the forecast is above or below actual earnings. The standard deviation is substantially larger 12 months before earnings are announced than 1 month before the earnings announcement. For example, in 1990, the standard deviation is 0.0754 at a horizon of 12 months, 0.0669 at a horizon of 6 months, and 0.0342 at a horizon of 1 month. In 2004, the standard deviation is 0.0317 at a horizon of 12 months, 0.0195 at a horizon of 6 months, and 0.0092 at a horizon of 1 month.

The mean forecast errors in the table also decline as the announcement of earnings for the year approaches. The largest magnitudes of mean forecast errors in the table are for the 12-month horizon, –2.7 cents per dollar of share price in 1990 and 2001 and –2.5 cents per dollar of share price in 1991. The smallest magnitudes of mean forecast errors are for the 1-month horizon; the mean forecast error farthest from zero is –0.35 cents per dollar of share price in 1990. The mean forecast error has been hundredths of a penny per dollar of share price in most of the years since.

A large segment of the literature examines these mean forecast errors. The negative mean forecast errors are statistically significant and not trivial in magnitude at the 12-month horizon. Twelve months before earnings are announced, analysts' forecasts on average are overestimates of actual earnings. This overestimation is predictable, in an interesting and specific sense. If only the earnings forecasts are known a year in advance, it is predictable that actual earnings will be less on average. The difference is not large, but it is not zero and it is predictable. If analysts are attempting to forecast earnings well on average, their performance is not as good as it could be. In standard parlance, the forecasts are biased: The average forecast error is *not* zero.

Besides the arithmetic average, the median is another measure of the typical forecast. The median is the middle forecast, the forecast that divides the forecasts into two parts, with half the observations above the median and half below the median. The median forecast error is noticeably closer to zero than the average forecast error. This indicates that the typical negative forecast error is larger in magnitude than the typical positive forecast error. In other words, as Figure 2 shows, the distribution of forecast errors is not symmetric. The percentiles of the distribution clearly show this asymmetry of forecast errors. The consistently negative values of skewness in Table 2 also indicate what Figure 2 shows: Nega-

tive forecast errors are larger in magnitude than the positive errors.[7] Consistent with the figures, the measure of skewness indicates that forecast errors are skewed toward negative values.

Kurtosis measures how concentrated a distribution is around the mean compared with the number of observations in the tails of the distribution.[8] The positive values for kurtosis indicate that the tails of the distribution have more observations than would be suggested by a normal distribution. Tests for normality of the distribution of forecast errors uniformly are inconsistent with a normal distribution.[9]

Figures 3 and 4 show aspects of the distributions of forecast errors for all horizons from 1990 to 2004. Figure 3 shows the mean and median forecast errors as the horizon—the length of time before the earnings announcement—approaches zero. It also shows the median in combination with the 25th and 75th percentiles of the distribution of forecast errors. The mean forecast errors are more strongly negative than the medians at long horizons and consequently show more convergence to zero. The median forecast errors are negative, with the largest magnitudes in 1990, 1991, 1998, and 2001 (see Figure 4). With the exception of 1998, these larger-magnitude median forecast errors are associated with recessions.[10] The mean forecast errors are more strongly negative than the median forecast errors but decrease to quite close to zero by 1 month before the earnings announcement.

Figure 4 shows the distribution of forecast errors by year by graphing the median forecast error and the 25th and 75th percentiles of the distribution for each horizon for each year from 1990 to 2004. The asymmetry of the distributions is quite apparent. It also is clear that actual earnings fall short of the longer horizon forecasts during recessions; this is indicated by the much more negative forecast errors during the recession years 1990, 1991, and 2001. Given the unpredictability of recessions, this is not especially surprising. The figure suggests that the distribution has become more symmetric over time, although the occurrence of recessions clearly is associated with greater asymmetry.

Table 3 presents the results of tests to determine whether the apparent skewness in the figures is statistically significant and consistent across horizons and years.[11] The results of two tests are presented. The first is the sign test, which determines whether the median equals the mean. If a series' median exceeds its mean, the value of the statistic is positive. The *p*-value indicates the probability of that difference or a larger one if there really were no difference between the median and the mean. The second test determines whether the skewness coefficient is zero. If the skewness coefficient is zero and moments of the distribution up to the sixth are finite, then the skewness coefficient has an asymptotic normal distribution that can be used to construct a test.[12]

The sign tests indicate an asymmetry in forecast errors that persists from 1990 through 2004. Tests for the equality of the median and mean at all horizons are quite inconsistent with the equality of the two statistics. At the 12-month horizon, the median forecast error is closer to zero than the mean for all years from 1990 through 2004; all of the differences are statistically significant at any usual significance level. There is some suggestion that the difference between the mean and the median has been declining over time. The difference is far smaller in 2004 than in earlier years but the difference still is statistically

[7] The measure of skewness is the third moment about the mean divided by the standard deviation cubed.

[8] The measure of kurtosis is the fourth moment about the mean minus 3, all relative to the fourth power of the standard deviation.

[9] The test for normality is the Bera-Jarque test (1980). The inconsistency with a normal distribution matches up with the figures and tables; a normal distribution is symmetric and does not have the relatively fat tails indicated by the kurtosis statistics. The Bera-Jarque test statistics are not included in the table because the *p*-values uniformly are inconsistent with a normal distribution with *p*-values of 10^{-8} or below.

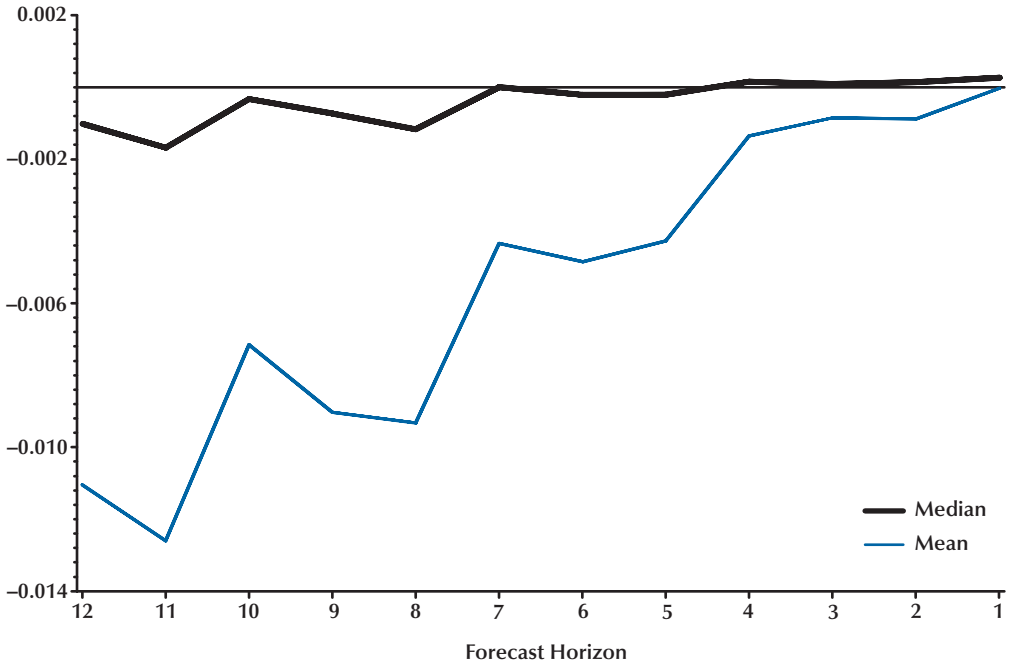
[10] The National Bureau of Economic Research dates the recession in 1990 and 1991 from July 1990 to March 1991 and the recession in 2001 from March 2001 to November 2001.

[11] The observations are repeated measures of forecasts by the same analysts for the same industries. As Keane and Runkle (1998) argue, this can introduce dependence in the data, which results in overstating the statistical significance of test statistics.

[12] The mean of the asymptotic distribution of the skewness coefficient is zero under the null hypothesis and the variance is from Gupta (1967, pp. 850-51.)

Figure 3

Forecast Errors by Horizon



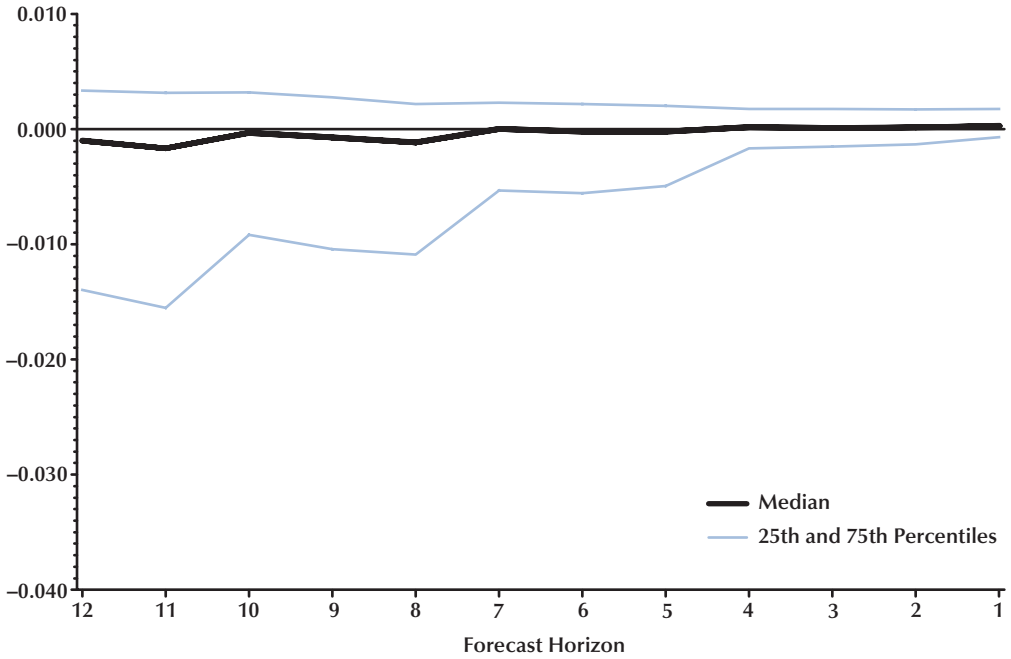
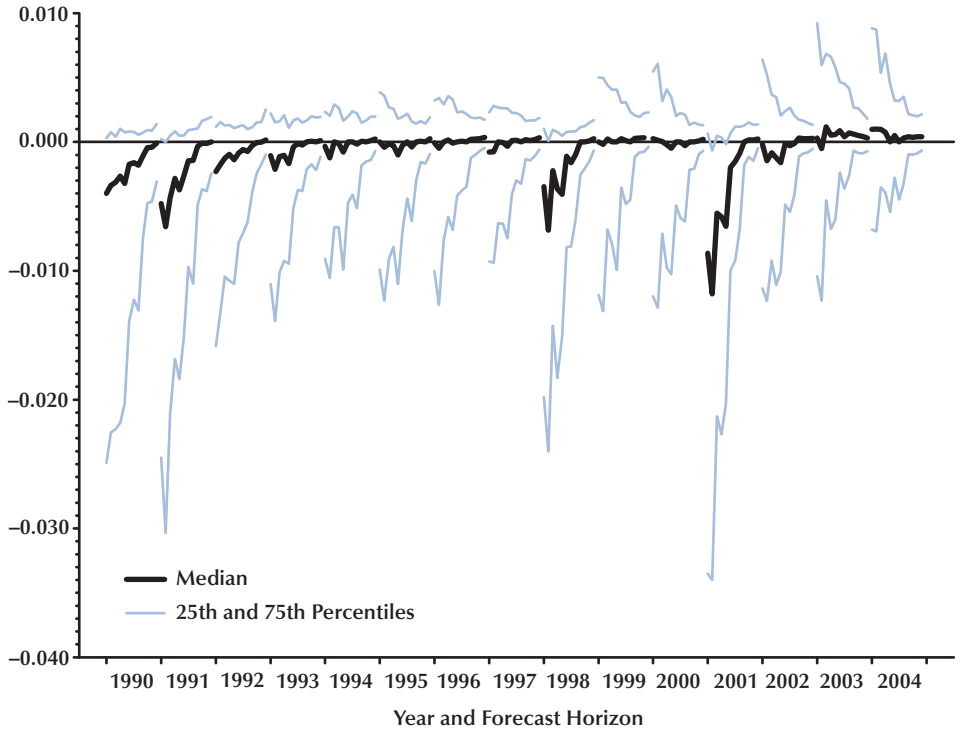


Figure 4

Distribution of Forecast Errors by Year and Horizon



significant. The difference is one-tenth of a penny per dollar share price in 2004. Given a typical price-to-earnings ratio of 15 or 20, this implies a forecast error in earnings on the order of 2 cents per share per dollar of earnings 12 months ahead.

The tests using the skewness coefficient indicate that deviations from symmetry are persistent from 1990 through 2004 only at the 12-month horizon. The null hypothesis of symmetry for the 12-month horizon cannot be rejected in 2002 at the 5 percent significance level, a result most simply interpreted as due to chance rather than anything special about 2002. There is less evidence of overall skewness in any year at the 6-month horizon and scant evidence of asymmetry at the 1-month horizon. This is an interesting contrast to the results using the median and mean. While there are statistically significant differences between the mean and median, the overall skewness of the distribution is less pronounced based on the third moment, which summarizes the asymmetry of the distribution.[13]

Forecasts Errors Across Industries

Forecast errors across firms and analysts are likely to differ for a variety of reasons, one being the likelihood that earnings are more predictable for some industries than others.

Figure 5 shows forecast errors by two-digit Global Industry Classification System categories. Forecast errors vary substantially by industry. All figures have the same scale to facilitate comparison of forecast errors across industries. Earnings in health care are predicted with relatively

[13] Too many rejections of the null hypothesis are possible if data have high kurtosis (Premaratne and Bera, 2005), as ours do. This is an issue only at the 12-month horizon because only that horizon shows rejections. Given the results for the median and mean and the levels of significance, we are inclined to take the rejections as being real rather than an artifact of kurtosis.

Table 3

Sign Test Statistics and Skewness Coefficients by Year and Horizon

	Sign test						Skewness coefficient					
	12-month horizon		6-month horizon		1-month horizon		12-month horizon		6-month horizon		1-month horizon	
Year	Mean minus median	*p*-Value	Mean minus median	*p*-Value	Mean minus median	*p*-Value	Coefficient	*p*-Value	Coefficient	*p*-Value	Coefficient	*p*-Value
1990	–0.0230	0.0000	–0.0146	0.0000	–0.0034	0.0000	–5.806	0.0000	–0.370	0.7116	–0.024	0.9807
1991	–0.0201	0.0000	–0.0094	0.0000	–0.0015	0.0000	–3.825	0.0001	–2.375	0.0176	–0.014	0.9885
1992	–0.0118	0.0000	–0.0060	0.0000	0.0004	0.0000	–16.085	0.0000	–2.999	0.0027	0.337	0.7360
1993	–0.0085	0.0000	–0.0023	0.0000	–0.0006	0.0000	–9.796	0.0000	–5.912	0.0000	–0.183	0.8546
1994	–0.0092	0.0000	–0.0025	0.0000	0.0004	0.0000	–2.374	0.0176	–1.901	0.0574	–0.009	0.9925
1995	–0.0071	0.0000	–0.0038	0.0000	0.0001	0.0007	–20.569	0.0000	–1.532	0.1256	0.046	0.9634
1996	–0.0077	0.0000	–0.0030	0.0000	0.0005	0.0000	–37.030	0.0000	–1.409	0.1588	–0.049	0.9611
1997	–0.0086	0.0000	–0.0022	0.0000	–0.0002	0.0000	–2.637	0.0084	–2.637	0.0084	–0.017	0.9867
1998	–0.0120	0.0000	–0.0047	0.0000	0.0002	0.0000	–14.384	0.0000	–2.011	0.0444	0.008	0.9933
1999	–0.0079	0.0000	–0.0053	0.0000	0.0007	0.0000	–3.588	0.0003	–0.552	0.5812	0.019	0.9849
2000	–0.0057	0.0000	–0.0037	0.0000	–0.0013	0.0000	–24.850	0.0000	–4.124	0.0000	–0.188	0.8507
2001	–0.0179	0.0000	–0.0070	0.0000	–0.0007	0.0000	–2.469	0.0136	–0.864	0.3877	0.000	0.9999
2002	–0.0065	0.0000	–0.0035	0.0000	–0.0004	0.0000	–1.841	0.0657	–0.721	0.4708	0.002	0.9987
2003	–0.0048	0.0000	–0.0015	0.0000	–0.0006	0.0000	–2.926	0.0034	–0.415	0.6780	–0.081	0.9358
2004	–0.0013	0.0000	–0.0025	0.0000	0.0002	0.0026	–7.336	0.0000	–1.362	0.1731	0.033	0.9739

Figure 5A

Distribution of Forecast Errors by Year, Horizon, and Industry

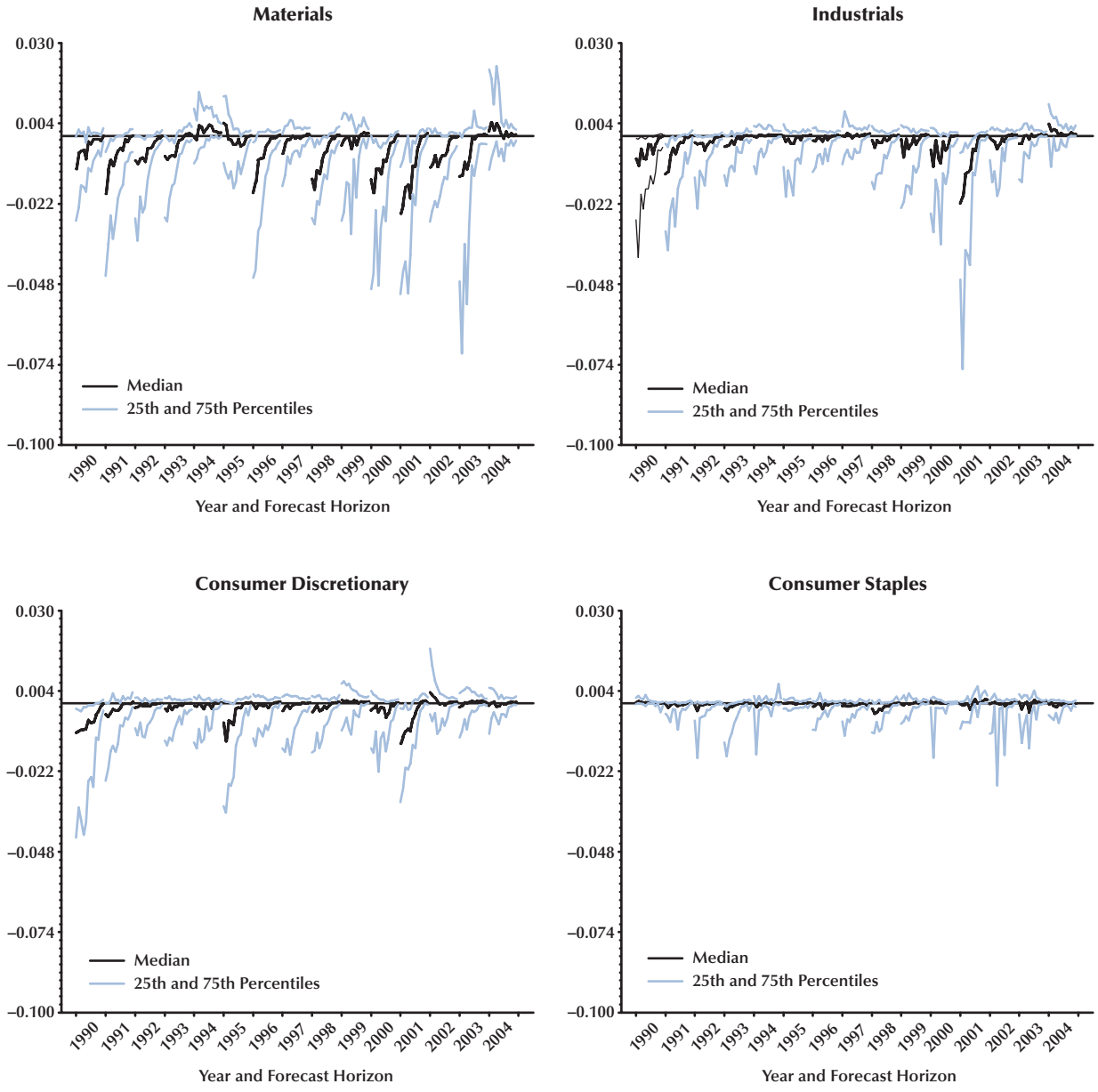


Figure 5B

Distribution of Forecast Errors by Year, Horizon, and Industry

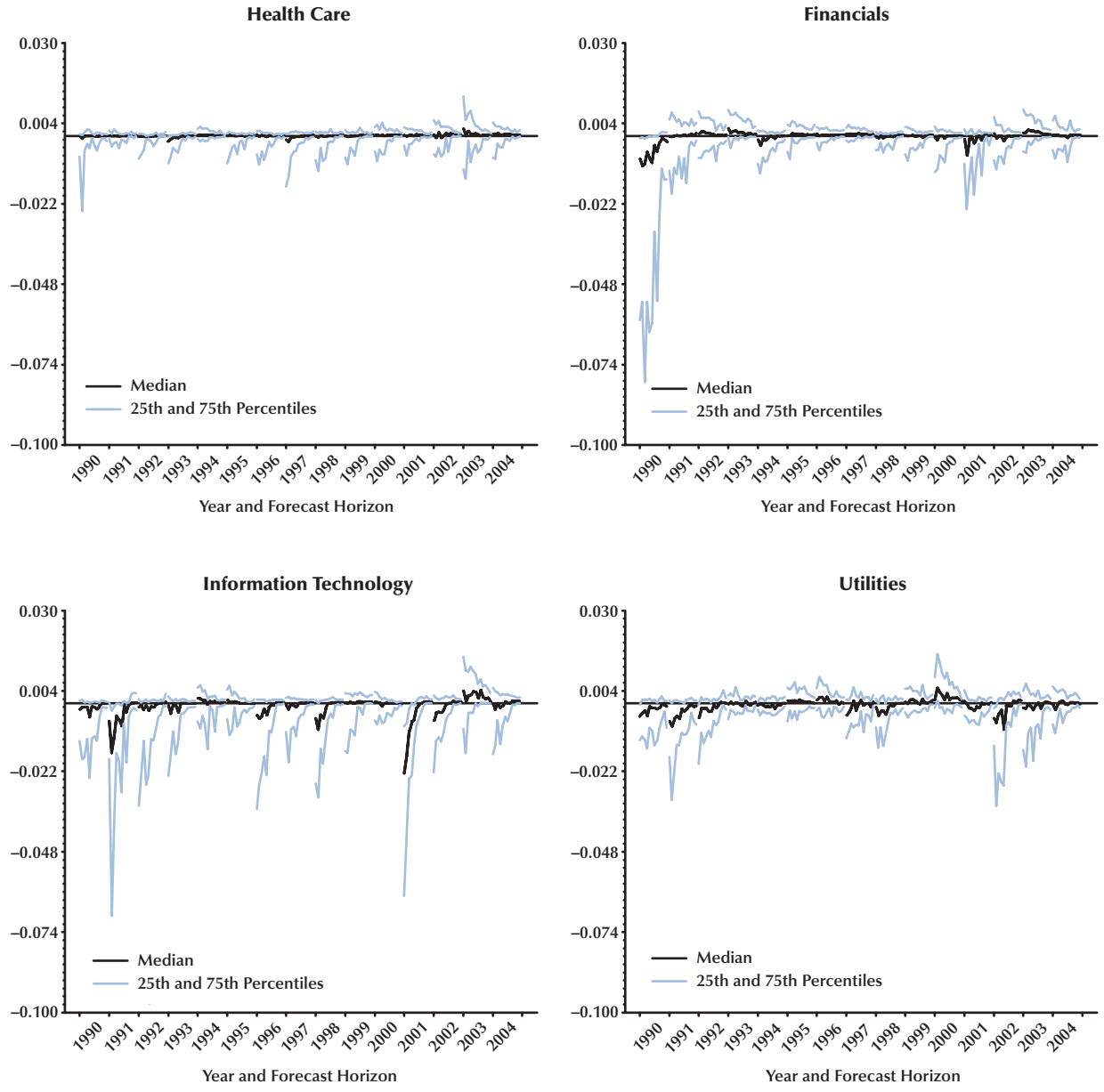
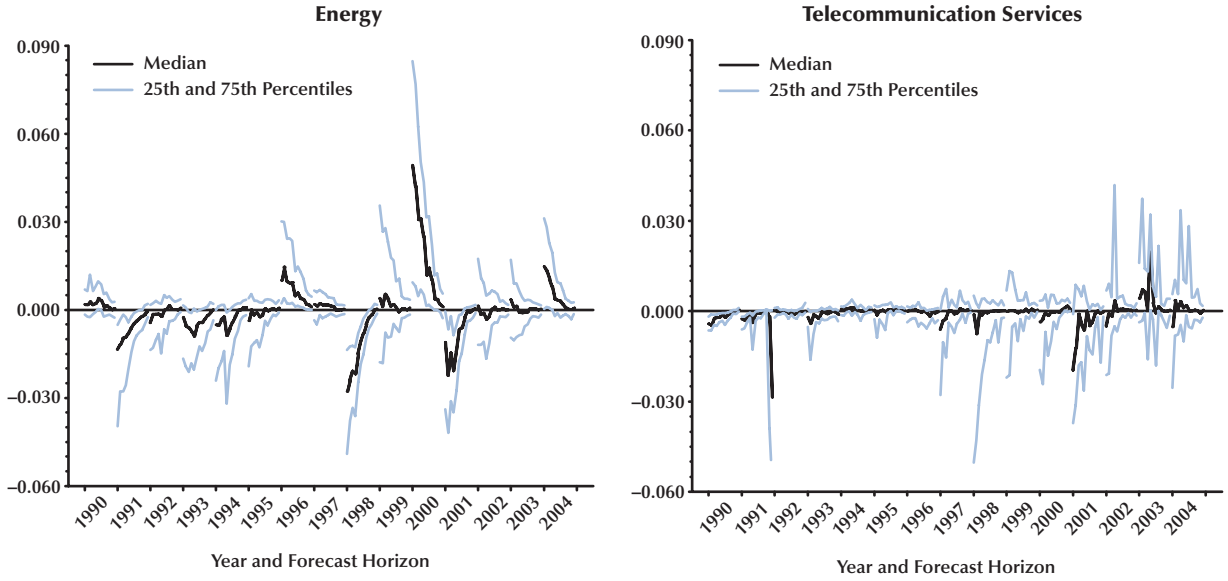


Figure 5C

Distribution of Forecast Errors by Year, Horizon, and Industry



small forecast errors, and earnings in energy firms are predicted particularly poorly. It is plausible that earnings forecasts in less-volatile industries are smaller. Energy prices are subject to large unpredictable price swings, which obviously affect earnings. Although health care prices have risen substantially in recent years, the increases have been relatively persistent and therefore predictable. Health care is virtually unaffected by recessions, while the demand for energy falls in recessions. Some other industries show low earnings around recessions as well, such as materials and consumer discretionary goods. If recessions are not predicted, there is little reason to think that these earnings decreases are predictable either.

Sign tests not reported in the text are consistent with persistent differences between the median and means of the forecast errors across industries but suggest variation in the asymmetry by industry. The evidence is noticeably weaker for telecommunications and utilities.

UNBIASEDNESS OF EARNINGS FORECASTS

Almost all of the existing literature on analysts' forecasts examines whether their forecasts are biased and, generally speaking, finds that analysts overestimate earnings. This overestimation falls as the earnings announcement approaches, as indicated in Table 2, but future earnings typically are noticeably less than the average forecast. Some evidence and analysis suggests that analysts' forecasts change from overestimates to underestimates just before the earnings announcement. Such near-term forecasts are intended to be helpful to a firm's management because the announcement of higher-than-forecasted earnings generates favorable publicity and a higher stock price after the announcement.[14]

Asking for forecasts that are neither too high nor low on average seems like a relatively simple

[14] This is at least one reason to be dubious about this explanation if the near-term underestimation of earnings is persistent and predictable. Investors are likely to notice and discount the overestimation of earnings.

request, especially compared with asking that forecasts be accurate. Even so, it is possible that analysts process the information available to them as best as possible, but some or all analysts do not have an incentive to produce forecasts that are correct on average.

Analysts' Incentives and Forecasts

At first glance, it seems obvious that unbiased forecasts are the best forecasts. A biased forecast is high or low on average. Such a bias suggests that the forecast can be improved by adjusting the forecast by the bias. There are many conditions in which an unbiased forecast is the best one. A common criterion for forecast errors is mean squared error. If a forecaster wants to minimize the expected mean squared error of a forecast, then an unbiased forecast is the best one.[15] The expected squared forecast error applies an increasing penalty to forecasts farther from the average—a forecast twice as far from zero is four times as bad.

The unbiased forecast—the mean—is not necessarily the best forecast in all circumstances. Suppose that someone is trying to forecast the value shown when a fair die is thrown. The mean forecast is the average of 1, 2, 3, 4, 5, and 6, which is 3.5. If the forecaster's earnings depend on how close the forecast is to the actual value, the best forecast in fact is 3.5. On the other hand, if the forecaster gets paid only when the value shown is the same as the value forecasted, this unbiased forecast guarantees that the forecaster always loses. The die will never have the value 3.5. If the forecaster is paid when the forecast is the same as the value thrown and values from 1 to 6 are equally likely, any integer forecast from 1 to 6 is equally good and 3.5 never is predicted. While this is a simple example, the point is more general. The value forecasted depends on the forecaster's incentives and the distribution of the data. An unbiased forecast may not be the "best" forecast.

There also are objectives similar to minimizing the expected squared error that lead to forecasts being "biased." If a forecaster wants to minimize the expected absolute deviation of the forecast error, then the median is the best forecast.[16] The absolute forecast error applies an increasing penalty to forecast errors farther from zero—a forecast error twice as far from zero is twice as bad. The cost of forecast errors increases linearly with the size of the error. The forecast that minimizes the expected absolute forecast error is the median, not the mean (or more precisely, the arithmetic average). If the mean and the median are the same, this is a distinction that does not matter. On the other hand, if the distribution is not symmetric, as the earnings distribution is not, the median is a better forecast than the mean if a forecast error's cost increases linearly with the forecast error.[17]

Analysts do not make forecasts in isolation. Other analysts are making forecasts as well, and the existence of other forecasts can affect an analyst's forecasts in many ways. A simple, common forecasting game illustrates that an unbiased forecast may not be an analyst's best forecast. Consider a forecasting game in which the smallest forecast error wins and receives a prize; everyone else receives nothing. Analysts' situations may be closer to this game than to isolated forecasts. In this game, the incentive is to be the closest. If you are not the closest, then it matters not at all whether your forecast error is almost as good as the best or is far away. More generally, any analyst's forecast will depend on what he or she thinks other people will forecast or what others have already forecasted. A simple example is one in which two people guess someone else's pick of a number between 0 and 10. The unbiased forecast is 5. Suppose that the first person picks 5. If the second person picks 5, then he or she cannot win, only tie. A pick of either 4 or 6 can increase the expected winnings of the second person if there is no payoff from tying. Neither 4 nor 6 is unbiased, but that doesn't matter. Either number maximizes expected winnings, and it is winnings that matter. This suggests that, even if analysts' forecasts are biased, it is important to consider analysts' incentives before denouncing them as "irrational" or "ignoring information readily available to them."

[15] A minimum expected squared error forecast minimizes the expected value of the squared forecast errors.

[16] A minimum expected absolute error forecast minimizes the expected absolute value of the forecast errors.

[17] Gu and Wu (2003) discuss this in more detail.

Among others, Hong and Kubik (2003), Clarke and Subramanian (2006), Ottaviani and Sørensen (2006), and Ljungqvist et al. (2007) highlight factors that can explain a nonzero predictable forecast error. For example, Clarke and Subramanian (2006) suggest that an analyst who performs poorly and is at risk of being fired is more likely to make a "bold" forecast that is unlikely to be correct but will save the analyst's job if it is correct.

Tests for Unbiasedness

The proposition that analysts' forecasts are biased is simple to determine with a test of whether the average difference between actual earnings and forecasted earnings is zero.[18] Given the evidence above that forecast errors are not symmetric, it is worthwhile to test whether the median forecast error is zero, in addition to testing whether the mean forecast error is zero. A simple *t*-test is used for the latter purpose. The test that analysts' median forecast errors are zero is the sign test for deviations from zero.

Table 4 presents the mean and median forecast errors by industry at the various horizons and *p*-values for tests of whether the mean and median forecast errors are zero. The mean forecast errors are far smaller at the 1-month horizon than at longer horizons. At the 12-month horizon, the mean forecast error indicates that forecasted earnings are greater than actual earnings by about 1 cent per dollar of share price. At the 1-month horizon, the mean forecast errors indicate that forecasted earnings are greater than actual earnings by about one-hundredth of a cent per dollar of stock price.

How big are these forecast errors? Mean earnings for all firms in our data are 2 cents per dollar of share price; median earnings are 3.9 cents per dollar of share price. A forecast error of 1 cent per dollar of share price at the 12-month horizon is large relative to average earnings of 2 cents. A forecast error of one-hundredth of a cent at the 1-month horizon is relatively small and not obviously economically insignificant.

The median forecast error for all industries is minus nine-hundredths of a cent per dollar of share price at the 12-month horizon. At the 6-month and 1-month horizons, the median forecast errors are minus two-hundredths of a cent per dollar of share price and three-hundredths of a dollar per dollar of share price. All these magnitudes based on the median are statistically significantly different from zero. Median forecast errors of hundredths of a cent per dollar of share price are not particularly large relative to median earnings of about 4 cents per dollar of share price.

The means and medians vary substantially by industry. The mean forecast errors by industry mirror the overall mean forecast errors, declining in magnitude as the horizon shortens. The median forecast errors show substantial variability across industries in terms of magnitude. At the 1-month horizon, all of the magnitudes are of the same small order as the overall median, with the largest being five-hundredths of a cent per dollar of share price.

Table 5 shows the results of tests to determine whether the average and median forecast errors are zero by year. With the exception of the last year in the table, 2004, all *p*-values for testing whether mean forecast errors are zero at the 12-month horizon are less than 10^{-4}. All mean forecast errors are negative, indicating that forecasts on average are greater than actual earnings. Mean forecasts 6 months ahead look much like the forecasts at the 12-month horizon. The forecasts at the 1-month horizon look quite a bit different. At the 1-month horizon, there is little evidence in our data of bias in the mean forecast: 8 of the 15 forecasts are positive and 7 are negative. Nine of the forecasts are statistically significant at the 5 percent level, but they are not uniformly positive or negative. There is little evidence to support a conclusion that mean forecasts at the 1-month horizon are uniformly above or below zero.

The median forecasts in Table 5 are closer to zero than the mean forecasts. The results of the statistical tests that the median forecasts equal zero indicate that they are not zero, but the magnitudes generally are hundredths of a cent per dollar of share price.

At the 12-month horizon, the overall median forecast error is negative, but this masks interesting variation by year. In five years—1995, 1999,

[18] The test is a standard *t*-test of whether the mean forecast error equals zero using the asymptotic normal distribution.

Table 4

Forecast Errors by Industry and Horizon

	12-month horizon				6-month horizon				1-month horizon			
Industry	Mean	*p*-Value mean equals zero	Median	*p*-Value median equals zero	Mean	*p*-Value mean equals zero	Median	*p*-Value median equals zero	Mean	*p*-Value mean equals zero	Median	*p*-Value median equals zero
All industries	–0.0106	0.0000	–0.0009	0.0000	–0.0048	0.0000	–0.0002	0.0000	–0.0001	0.3456	0.0003	0.0000
Consumer discretionary	–0.0124	0.0000	–0.0017	0.0000	–0.0070	0.0000	–0.0009	0.0000	–0.0002	0.3400	0.0003	0.0000
Consumer staples	–0.0067	0.0000	–0.0003	0.0000	–0.0039	0.0000	–0.0001	0.0078	–0.0002	0.4837	0.0002	0.0000
Energy	–0.0002	0.8012	0.0002	0.1833	–0.0015	0.0001	–0.0003	0.0172	0.0003	0.4056	0.0005	0.0000
Financials	–0.0101	0.0000	0.0000	0.2480	–0.0050	0.0000	0.0001	0.0000	–0.0005	0.0410	0.0002	0.0000
Health care	–0.0043	0.0000	0.0000	0.6499	–0.0017	0.0001	0.0001	0.0000	0.0002	0.5447	0.0002	0.0000
Industrials	–0.0163	0.0000	–0.0025	0.0000	–0.0092	0.0000	–0.0012	0.0000	0.0007	0.0372	0.0003	0.0000
Information technology	–0.0159	0.0000	–0.0016	0.0000	–0.0043	0.0000	0.0000	0.5310	–0.0004	0.0890	0.0003	0.0000
Materials	–0.0208	0.0000	–0.0084	0.0000	–0.0078	0.0000	–0.0027	0.0000	0.0003	0.2840	0.0004	0.0000
Telecommunication services	–0.0099	0.0000	–0.0018	0.0000	–0.0043	0.0001	–0.0002	0.0131	–0.0009	0.2061	0.0002	0.0001
Utilities	–0.0050	0.0000	–0.0009	0.0000	–0.0021	0.0062	–0.0003	0.0220	–0.0006	0.0732	0.0001	0.0004

Table 5

Forecast Errors by Year and Horizon

	12-month horizon				6-month horizon				1-month horizon			
Year	Mean	*p*-Value mean equals zero	Median	*p*-Value median equals zero	Mean	*p*-Value mean equals zero	Median	*p*-Value median equals zero	Mean	*p*-Value mean equals zero	Median	*p*-Value median equals zero
1990-2004	–0.0111	0.0000	–0.0010	0.0000	–0.0048	0.0000	–0.0002	0.0000	–0.0000	0.7701	0.0003	0.0000
1990	–0.0270	0.0000	–0.0040	0.0000	–0.0162	0.0000	–0.0016	0.0000	–0.0035	0.0016	–0.0001	0.0253
1991	–0.0249	0.0000	–0.0048	0.0000	–0.0108	0.0000	–0.0015	0.0000	–0.0015	0.0286	0.0000	0.5331
1992	–0.0141	0.0000	–0.0023	0.0000	–0.0066	0.0000	–0.0006	0.0000	0.0006	0.4243	0.0002	0.0001
1993	–0.0095	0.0000	–0.0011	0.0000	–0.0024	0.0000	–0.0001	0.0012	–0.0005	0.1985	0.0001	0.0000
1994	–0.0096	0.0000	–0.0004	0.0000	–0.0025	0.0000	0.0000	0.6343	0.0006	0.0062	0.0002	0.0000
1995	–0.0071	0.0000	0.0000	0.6729	–0.0038	0.0000	0.0000	0.1360	0.0004	0.3581	0.0003	0.0000
1996	–0.0078	0.0000	–0.0001	0.2249	–0.0029	0.0000	0.0001	0.0117	0.0008	0.0069	0.0004	0.0000
1997	–0.0094	0.0000	–0.0008	0.0000	–0.0021	0.0000	0.0001	0.0019	0.0002	0.6129	0.0004	0.0000
1998	–0.0155	0.0000	–0.0035	0.0000	–0.0063	0.0000	–0.0016	0.0000	0.0004	0.0338	0.0003	0.0000
1999	–0.0079	0.0000	0.0000	0.8265	–0.0052	0.0000	0.0001	0.0015	0.0011	0.0008	0.0004	0.0000
2000	–0.0054	0.0000	0.0003	0.0024	–0.0037	0.0000	0.0000	0.0106	–0.0011	0.0002	0.0002	0.0000
2001	–0.0265	0.0000	–0.0086	0.0000	–0.0085	0.0000	–0.0015	0.0000	–0.0004	0.0297	0.0002	0.0000
2002	–0.0067	0.0000	–0.0002	0.1688	–0.0038	0.0000	–0.0003	0.0001	–0.0002	0.5212	0.0003	0.0000
2003	–0.0045	0.0000	0.0003	0.0289	–0.0011	0.0669	0.0004	0.0000	–0.0003	0.3086	0.0003	0.0000
2004	–0.0003	0.5223	0.0010	0.0000	–0.0025	0.0000	0.0000	0.2696	0.0006	0.0012	0.0004	0.0000

2000, 2003, and 2004—the median forecast error at the 12-month horizon is positive, indicating that the median forecast is an underestimate of earnings. This is the opposite of the bias in the mean forecast. It is interesting that these years are toward the end of the period. For four years—1995, 1996, 1999, and 2002—the median forecast error is not statistically significantly different from zero at the 5 percent significance level. Two of these years have positive median forecast errors and two have negative ones. At this 12-month horizon, only 8 of the 15 years have median forecast errors that are negative and statistically significant. Moreover, of the medians at this 12-month horizon from 1999 to 2004, only the recession year 2001 has a negative median forecast error that is statistically significantly different than zero; 3 of the 5 years have positive median forecast errors that are statistically significant. These results are consistent with the median forecast errors not always being zero, but there is little support for the median forecasts uniformly being too high or too low.

At the 6-month horizon, median forecast errors also provide little support for typical overestimation of earnings throughout the period. The median forecast errors are negative in 8 of the 15 years, barely more than half the 15 years. The median forecast errors are positive and statistically significant at the 5 percent significance level in years 1996, 1997, 1999, 2000, and 2003.

At the 1-month horizon, the median forecast errors are positive in all years but 1990, a result consistent with the stylized view in the literature that forecast errors are underestimates close to the announcement. It is interesting that our data support such an inference using medians but provide much less support with means. All median forecast errors at the 1-month horizon are quite small, never larger in magnitude than four-hundredths of a cent per dollar of share price. Economically, this is not that far from zero.

CONCLUSION

Our data for U.S. analysts' forecasts of U.S. firms' earnings from 1990 through 2004 show typical results: Analysts' forecasts are greater than earnings on average a year before earnings are announced. Six months before the earnings announcements, mean earnings forecasts also are greater than actual earnings. On the other hand, median earnings forecasts are about as likely to be above actual earnings as below them at both the 12-month and 6-month horizons. A month before the announcement, mean forecast errors provide little support for predictable differences between average earnings and forecasts. Median forecast errors at the 1-month horizon, though, generally are positive and statistically significant, indicating that the analysts' median forecast is less than earnings on average. These median forecast errors are relatively small in magnitude, though—on the order of hundredths of pennies of earnings relative to the share price—when average and median earnings are about 2 and 4 cents, respectively, relative to the share price.

Mean forecast errors and median forecast errors differ substantially. The distribution of forecast errors is asymmetric, with mean forecast errors substantially larger in magnitude than median forecast errors at the 6-month and 12-month horizons. The distribution of earnings is asymmetric. The distribution of earnings forecasts also is asymmetric but not sufficiently asymmetric that forecast errors are symmetric. There are substantial differences in mean and median forecast errors across industries. We also find substantial differences in mean and median forecast errors by year, with the largest forecast errors in recession years.

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