Federal Reserve Bank of St. Louis

REVIEW

MARCH/APRIL 2008 VOLUME 90, NUMBER 2

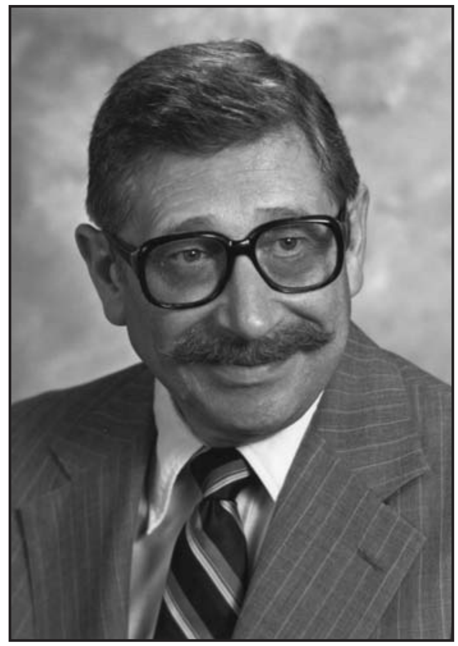
Review is published six times per year by the Research Division of the Federal Reserve Bank of St. Louis and may be accessed through our web site: **research.stlouisfed.org/publications/review**. All nonproprietary and nonconfidential data and programs for the articles written by Federal Reserve Bank of St. Louis staff and published in *Review* also are available to our readers on this web site. These data and programs are also available through Inter-university Consortium for Political and Social Research (ICPSR) via their FTP site: www.icpsr.umich.edu/pra/index.html. Or contact the ICPSR at P.O. Box 1248, Ann Arbor, MI 48106-1248; 734-647-5000; **netmail@icpsr.umich.edu**.

Single-copy subscriptions are available free of charge. Send requests to: Federal Reserve Bank of St. Louis, Public Affairs Department, P.O. Box 442, St. Louis, MO 63166-0442, or call (314) 444-8809.

General data can be obtained through FRED (Federal Reserve Economic Data), a database providing U.S. economic and financial data and regional data for the Eighth Federal Reserve District. You may access FRED through our web site: **research.stlouisfed.org/fred**.

ISSN 0014-9187

In Memoriam: Anatol "Ted" Balbach, 1927-2007



Anatol "Ted" Balbach, who served as director of research at the Federal Reserve Bank of St. Louis between 1975 and 1992, died in St. Louis on December 1, 2007.

Bank president William Poole came to know Ted and his wife, Rae, during the 1970s: "Ted continued and strengthened the St. Louis Fed research tradition developed and nurtured by Homer Jones, and Ted's leadership helped to establish the Homer Jones Memorial Lecture in 1987. Ted and Rae were especially gracious to my wife, Gerie, and me when we arrived in St. Louis in 1998, and, over the years since, the four of us enjoyed many good meals together. Ted's passing is a personal loss to me, as well as a professional one."

Balbach was born in Lithuania on October 31, 1927, and arrived in the United States with his mother in 1948 after World War II. From 1955 to 1957, he served in the U.S. Army and subsequently finished his doctorate in economics at the University of California–Los Angeles. He taught for 15 years at California State University–Northridge.

In 1971, Balbach joined the Bank's Research department as a visiting scholar and became a staff member in 1973. He became director of research in 1975. During his tenure, Balbach fostered contacts with central banks around the world, and numerous economists from Europe, Asia, and South America spent year-long exchanges at the St. Louis Fed.

Current research director Bob Rasche met Balbach when the two were visiting scholars at the Bank in the early 1970s: "Ted was the principal economic advisor to the Bank's president when the Great Inflation took place. At that time, the St. Louis Fed was one of the few, if not the only, Bank on the FOMC arguing for low and stable inflation—and arguing that the Fed must be the agent through which low and stable inflation would be achieved. He provided key support while the Fed brought inflation under control."

Following his retirement, Balbach consulted for Fiduciary Asset Management Co. He is survived by his wife of 49 years, Rachel (Rae), and sons Bruce and Adam.

Federal Reserve Bank of St. Louis *Review*, March/April 2008, *90*(2), p. 63.

Market Bailouts and the "Fed Put"

William Poole

This article was originally presented as a speech at the Cato Institute, Washington, D.C., November 30, 2007.

Federal Reserve Bank of St. Louis *Review*, March/April 2008, *90*(2), pp. 65-73.

Federal Reserve policy actions starting this past August to temper strains in financial markets have generated considerable commentary, some of which reflect concerns that policy action in such circumstances creates moral hazard. The issue is extremely important, and, given that it is so current, this is a good time to reflect in general on the Fed's reactions to financial market developments. The concern over moral hazard is that monetary policy action to alleviate financial distress may complicate policy in the future, by encouraging risky investing in the securities markets. There are so few instances of market turmoil similar to the current situation that I'll broaden the analysis to include significant stock market declines. Doing so gives us a substantial sample to discuss. Thus, my topic is whether Federal Reserve policy responses to financial market developments should be regarded as "bailing out" market participants and creating moral hazard by doing so.

To begin to explore the moral hazard issue, consider an extreme case, which I offer as a provocation to promote careful analysis and not as an example directly relevant to today's circumstances. Fact: The U.S. stock market between its peak in 1929 and its trough in 1932 declined by 85 percent. Question 1: If the Fed had followed a more expansionary policy in 1930-32, sufficient to avoid the Great Depression, would the stock market have declined so much? Question 2: Assuming that a more expansionary monetary policy would have supported the stock market to some degree in 1930-32, would it be accurate to say that the Fed had "bailed out" equity investors and created moral hazard by doing so? I note that a more expansionary monetary policy in 1930-32 would, presumably, have supported not only the stock market but also the bond and mortgage markets and the banking system—by reducing the number of defaults created by business and household bankruptcies in subsequent years.

Now apply these questions to the current situation. Did the Fed "bail out" the markets with its policy adjustments starting in August of this year? Have we observed an example of what some observers have come to call the "Fed put," typically named after the chairman in office, such as the "Greenspan put" or the "Bernanke put"?[1] Why has no one, at least not recently to my knowl-

[1] A put option contract provides that the buyer of the contract can sell an item, such as 100 shares of common stock of a particular company, for a certain price—the strike price—for a certain period. The contract protects the buyer from declines in the stock price beyond the strike price. The "Fed put" terminology implies that Fed policy adjustments, by analogy with a put option, will prevent stock price declines beyond some point.

William Poole is president of the Federal Reserve Bank of St. Louis. The author appreciates comments provided by his colleagues at the Federal Reserve Bank of St. Louis. The views expressed are the author's and do not necessarily reflect official positions of the Federal Reserve System.

edge, argued that a more expansionary Fed policy in 1930-32 would have "bailed out" the stock market at that time and, by implication, have been unwise?

I can state my conclusion compactly: There is a sense in which a Fed put does exist. However, those who believe that the Fed put reflects unwise monetary policy misunderstand the responsibilities of a central bank. The basic argument is very simple: A monetary policy that stabilizes the price level and the real economy cannot create moral hazard because there is no hazard, moral or otherwise. Nor does monetary policy action designed to prevent a financial upset from cascading into financial crisis create moral hazard. Finally, the notion that the Fed responds to stock market declines per se, independent of the relationship of such declines to achievement of the Fed's dual mandate in the Federal Reserve Act, is not supported by evidence from decades of monetary history.

Before proceeding, I want to emphasize that the views I express here are mine and do not necessarily reflect official positions of the Federal Reserve System. I appreciate comments and research assistance provided by my colleagues at the Federal Reserve Bank of St. Louis. However, I retain full responsibility for errors.

My approach will be to start by discussing bailouts and moral hazard in general. I will then examine the record of stock market declines and Fed policy adjustments and analyze how monetary policy changes the nature of risks in financial and goods markets. Finally, I will argue that the ways in which monetary policy alters risks in the markets yields benefits for the economy and does not create moral hazard.

UNDERSTANDING BAILOUTS

A traditional bailout involves governmental assistance to a particular firm, group of firms, or group of individuals. For ease of exposition, I'll concentrate on bailouts of firms but the same issues apply to bailouts of households. There may be occasions when a government infusion of capital to save a firm is justified, such as a bailout of a major defense contractor during wartime. However, most economists believe that bailouts are rarely justified and only in compelling circumstances should the government bail out individuals or firms.[2]

An important reason for opposition to bailouts is that it is essentially impossible for a bailout not to set a precedent for the future. A bailout creates what is known in the economics and insurance literature as (aforementioned) "moral hazard" by creating a presumption that in the future the government may again rescue a failing firm. That presumption encourages a firm and its investors to be less careful than they otherwise would be about taking risks. If a firm expects a bailout, it believes that government help will cover losses while the firm's owners can enjoy the gains, if any, from risky strategies. When the government is expected to absorb losses, bailouts unavoidably increase inappropriate risk taking, which increases the likelihood of losses in the future.

A standard problem in writing and administering insurance contracts is that the buyer of insurance has less incentive, by virtue of being insured, to control risk. Almost everyone has had the experience, far from uplifting, of talking with someone who says, "don't worry—it's insured." The very existence of insurance may change the behavior of the insured person. Insurance companies try to deal with moral hazard in a variety of ways, such as by writing contracts with substantial deductibles or loss sharing. Such contract provisions provide an incentive for the insured to control risk.

Government guarantee programs also generally require some loss sharing, but there are many government programs and practices that do not adequately control moral hazard. Perhaps the most dangerous practice is the ex post bailout, where a firm is rescued outside of any regular or

[2] Of course, the Federal Deposit Insurance Corporation (FDIC) is obligated to protect depositors from loss on covered deposits and it is sometimes true that the cheapest way to handle a failed bank is to merge it with another bank, with the FDIC providing a capital infusion. To the extent that there is a safety net for uninsured depositors, a bank bailout does raise moral hazard issues. I do not mean to imply that "too big to fail" is not an important issue for federal policy.

standing program. Such a bailout can change the rules of the competitive game in unpredictable ways. No one can know whether a bailout will be repeated or not. Those who control risks and actually bear losses will justifiably believe that a taxpayer-funded bailout of another firm is unfair.

The Federal Reserve has no funds and no authority to provide capital or guarantees to firms to provide a bailout in the traditional sense. The Fed cannot even bail out banks. The Fed can make loans to banks, but only loans that are fully secured by good collateral and only to banks that are well capitalized. The Fed can lend to weak banks requiring emergency assistance to prevent immediate collapse, but again only to those with adequate collateral. The Fed works cooperatively with the FDIC and other bank regulators to close a bank in distress or to find a willing buyer.

Creditors sometimes bail out debtors to a degree, by restructuring obligations to extend the repayment period or to reduce the interest rate. Restructuring a mortgage is often in the interest of the borrower, who may be able to avoid foreclosure. Restructuring may make sense for the lender to avoid the costs of bankruptcy and to obtain the maximum possible return from a failing loan. Nevertheless, lenders obviously must be careful not to make terms too easy for a borrower lest other borrowers ask for similar terms or future borrowers fail to service their obligations. A bailout of this sort is fundamentally different from a government bailout because the lender suffers the loss and not the taxpayer. Losses motivate lenders to be more disciplined in their future decisions.

Why do we use the term "moral hazard"? Using the insurance example, the hazard to the insurance company arises from behavior induced by insurance that may be adverse to the interests of the insurer. The "moral" in "moral hazard" refers to behavior the insured knows is adverse to the insurer's interest—behavior the insured would *not* engage in were he to suffer the full consequences of the behavior. Insurance companies try to maintain practices designed to encourage appropriate behavior. If an insurance company provides a premium discount for a driver who submits no insurance claims over a certain number of years and the discount in fact encourages safer driving, then that effect is not "moral hazard." From the perspective of the insurer, the policy changes behavior in a desirable rather than a harmful way. This point is a critical one in the context of monetary policy, to which I now turn.

THE FED PUT

The "Fed put" argument is usually stated in terms of monetary policy reactions to stock market declines. Consider Figure 1, which plots the natural log of the S&P 500 index and identifies all stock market declines of 10 percent or more since 1950.[3] The figure also shows a measure of the Federal Reserve's policy rate.[4] The policy rate in the figure is the discount rate before October 1, 1982, and the federal funds target rate thereafter. Shaded areas show recessions as defined by the National Bureau of Economic Research.

The figure shows 21 stock market declines of 10 percent or more. Within three months of these stock market peaks, the Fed held the policy rate constant, or increased it, on 12 occasions. There was a Fed rate cut within three months on nine occasions,[5] but for five of these nine rate cuts the Fed acted *before* the stock market peak; its policy actions could not have been motivated by stock market declines. Fed rate cuts did follow the stock market peak in late September 1976; the first rate cut came nine weeks later.[6] Another case

---

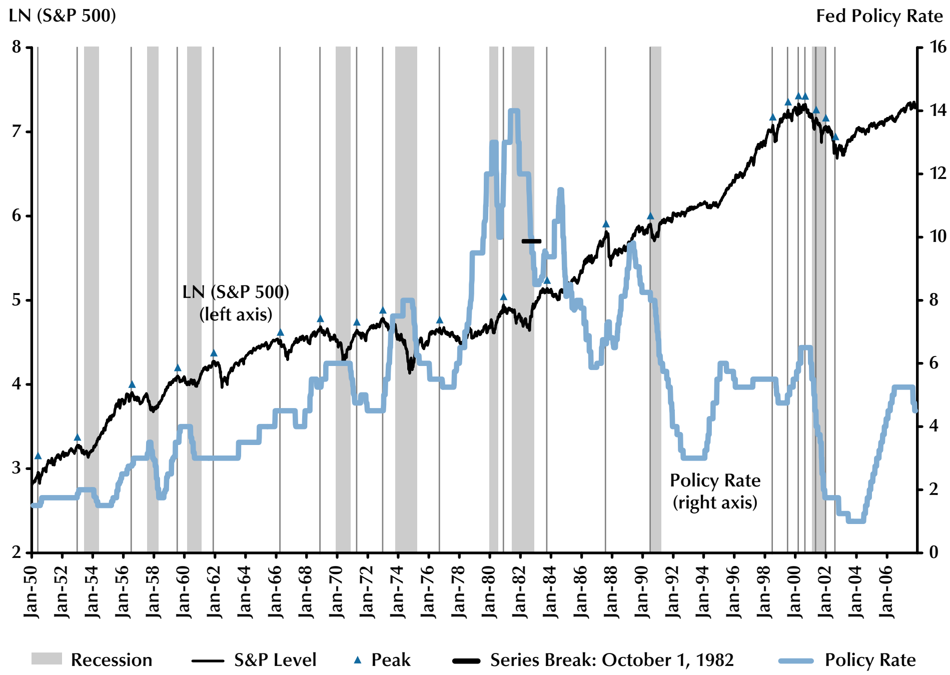
3 The S&P 500 series is the weekly close (Friday close unless Friday is a holiday). Each market peak was defined this way: Under criterion 1, the peak exceeded the previous peak and the market declined by 10 percent or more following the peak. Under criterion 2, the peak, followed by a decline of at least 10 percent, did not exceed the previous peak but a recovery of at least 10 percent had occurred between the two peaks.

4 The policy rate in the figure is the Fed's discount rate before October 1982 and the FOMC's federal funds target rate thereafter. Other measures are available for certain parts of the period before 1982, but using them would create several discontinuities in the policy rate series. See Rudebusch (1995, Table 3a) for a federal funds target rate series for 1974-79.

5 One of the nine was the market peak in November 1968. As the figure makes clear, the rate cut preceding this market peak was small and temporary. Subsequently, the Fed raised rates and the cuts did not begin until the end of the 1969-70 recession, at which point stock prices started to rise.

Figure 1

Declines Greater Than 10 Percent in the S&P 500 and Fed Policy Rate



occurred after the market peak in July 1998; a Fed rate cut in late September was a response to the situation in the money markets following the near collapse of Long-Term Capital Management (LTCM) and not a response to the stock market per se.[7]

The market peak in March 2000 ushered in the great bear market that ended in October 2002. The initial decline was sharp, but the market recovered to reach another peak in early September 2000 that was only slightly lower than the March 2000 peak. During the course of the bear market, there were several peaks, each lower than the one preceding, following significant recoveries. During this period, the Federal Open Market Committee (FOMC) cut the policy rate in 10 steps from 6.5 percent to 1.75 percent in December 2001 and in two more steps to 1 percent in June 2003. The policy rate cuts were not closely related to the stock market declines after the local peaks and declines that continued until the market hit bottom in October 2002.

Because the "put" language became current during the Greenspan era, let's examine stock market declines of 5 percent or more that did not reach the 10 percent threshold. Using the 5 percent criterion, there was a market peak in September 1989, and the Fed did cut its policy rate following that peak. However, the Fed had started to cut rates in June 1989. Another market peak meeting the 5 percent criterion occurred in late January 1994, when the policy rate was 3 percent. There was another such peak in August 1994. The Fed

[6] Rudebusch (1995, Table 3a) identifies two cuts totaling 25 basis points in the FOMC's target federal funds rate in July 1976 and two more totaling another 25 basis points in October. By this measure, therefore, Fed rate cuts began before the September 1976 stock market peak.

[7] The transcripts of FOMC meetings in 1998 provide excellent insight into the Committee's motivation in dealing with the LTCM situation. Of course, motivation is not the end of the matter; well-intentioned actions can have unintended adverse effects. The 1998 and 1999 transcripts show that the Committee was well aware of the potential for inflationary consequences of policy easing in response to the LTCM situation. Transcripts are available at http://www.federalreserve.gov/fomc/transcripts/.

proceeded to raise the policy rate several times in 1994, starting in February, and it reached 6 percent in January 1995.

Another market peak meeting the 5 percent criterion occurred in June 1996. The FOMC had cut the policy rate to 5.25 percent in January 1996, and the policy rate remained there until the FOMC raised it in March 1997. This increase occurred shortly after another stock market peak meeting the 5 percent criterion earlier the same month. Two more peaks meeting the 5 percent criterion—one in August 1997 and one in December 1997—occurred while the FOMC was holding the policy rate constant at 5.5 percent.

This history makes clear that it just is not true that the FOMC has eased policy in systematic fashion at the time of stock market declines, with the exception of the period following the 1987 stock market crash. Even this experience, however, reinforces the argument that the FOMC's primary concern is with its macroeconomic objectives and not with the stock market itself. Policy easing occurs at times of recession, although sometimes is delayed because of concern over inflation. The Fed eased policy ahead of the 1990-91 recession and ahead of the 2001 recession. The Fed has also eased policy in response to turmoil in the credit markets, as in the fall of 1998 and starting in August of 2007. Clearly, though, on numerous occasions the Fed has held its policy rate constant, or raised it, as stock prices declined.

EFFECTS OF FED STABILIZATION POLICY ON FINANCIAL MARKETS

Although there is no evidence that the Fed responds to the stock market per se, there is an element of truth to the argument that Fed policy can limit downside risk in the stock market. The same Fed policy that succeeds in stabilizing the price level and the real economy should tend to stabilize financial markets as well. Thus, the element of truth in the "Fed put" view reflects expected and desirable outcomes from successful monetary policy. General economic stability, by which I mean both stability of the price level and of the real economy, does change the nature of risks in the financial markets and, therefore, changes investor strategies.

Consider the second of Graham and Dodd's "Four Principles for the Selection of Issues of the Fixed-Income Type":

> The rule that a sound investment must be able to withstand adversity seems self-evident enough to be termed a truism. Any bond or preferred stock can do well when conditions are favorable; it is only under the acid test of depression that the advantages of strong over weak issues become manifest and vitally important. For this reason prudent investors have always favored the obligations of old-established enterprises which have demonstrated their ability to come through bad times as well as good." (Graham and Dodd, 1951, p. 289)

With regard to inflation risk, Graham and Dodd say that "[t]hese wide movements of the general price level…seem to carry the lesson that the long-term trend is toward inflation, punctuated by equally troublesome periods of deflation. Investment policy must accommodate itself, as far as it can, to both possibilities" (Graham and Dodd, 1951, p. 8).

How many investors today measure the value of a bond by the likelihood that it will continue to pay interest "under the acid test of depression"? How many investors today maintain portfolios robust against the possibility of inflation of the magnitude experienced in the 1970s or deflation of the magnitude experienced in the early 1930s? The answer, I believe, is "not many."

The fact that few investors worry about extreme economic instability is a benefit of sound monetary policy and not a cost; changes in investor practice are conducive to higher productivity growth. The same is true for changes in household and firm behavior reflecting the greatly reduced risk of economic depression or even severe recession of the magnitude of 1981-82. If we did not believe that economic stability is good for the economy and for society, why would a stable price level and high employment be monetary policy goals? Just as a deductible changes behavior of insurance policyholders, so also does economic stability change investor behavior.

Economists have long argued that price stability improves economic efficiency, in part because businesses and individuals can make decisions under the assumption that they do not need to pursue strategies designed to cope with a changing price level. Inflation and deflation distort relative prices; such distortion leads to misallocations of resources. With greatly reduced risk of price level instability, investors concentrate on risks relating to changes in demands, technology, and relative prices. Better evaluation of these risks promotes more efficient allocation of capital and fosters higher economic growth.

Monetary policy success in stabilizing the general level of prices does not eliminate risks for the economy. The real effects of inflation or deflation, should either occur in the future, will be magnified precisely because the economy today has adjusted relatively completely to an environment of price stability. One of the reasons the Great Inflation was so costly was that economic agents in 1965 did not anticipate the inflation. Decisions and institutions that had been sensible and efficient in an environment of price stability became unprofitable as inflation rose after 1965.

When events threaten to create inflation or deflation, the Fed ought to act to maintain price stability. It is true that Fed actions in such circumstances "bail out" investors who would lose large sums should inflation or deflation take hold. But "bail out" is a completely inappropriate term to use in this context, for it implies costs of the sort discussed earlier when the government provides capital to support firms that would otherwise go bankrupt. The central bank is *supposed* to stabilize the price level; the economy *is* better off when people act on a justified belief that the central bank will be successful.

Exactly the same argument applies to central bank actions in response to events or shocks that might drive the economy into recession, or into an unsustainable boom. Provided that the central bank does not sacrifice long-run price stability, it can and should respond to new information indicating an increased risk of recession. There is no conflict between the goals of price stability and high employment. Price stability and expectations of price stability permit the central bank to respond constructively to shocks that threaten to destabilize the real economy. Those who still believe that there is a trade-off between inflation and unemployment should reflect on the facts that the Great Depression was a consequence of deflation and the recessions of 1969, 1973-75, 1980, and 1981-82 were consequences of the Great Inflation.

With respect to financial instability, the central bank has the responsibility to do what it can to alleviate market turmoil. When there is a widespread increase in risk aversion and a flight to safe assets, the central bank ought to provide extra liquidity to prevent bank runs from bringing down the banking system. Provision of extra central bank liquidity does "bail out" firms that had not maintained sufficient liquidity themselves. Here again, though, the term "bail out," with its pejorative connotations, is completely inappropriate. In a fractional reserve banking system, it is simply impossible for owners of bank liabilities to convert all their liquid claims to cash, but the effort to do so will drive down aggregate demand. The same argument applies to liquid claims issued by non-bank financial firms. Widespread bank failures will destroy the claims of prudent investors, as well as of the imprudent.

For a fractional reserve banking system to work, a central bank must stand ready to be the ultimate source of liquidity for solvent banks, and banks in turn take the credit risk of providing liquidity to solvent non-bank firms. By "solvent," what I mean in this context is that a firm's assets valued at a normal level of economic activity cover the firm's liabilities, leaving a reasonable level of net worth. The firm's capital can absorb losses occasioned by normal business risks. We can argue about what "normal business risks" should be covered; but, in my view, economic depression, hyperinflation, and financial implosion are not included.

The stock market responds to changing expectations concerning corporate profits, which depend in part on the state of the real economy.

Slow economic growth or outright recession tends to reduce profits and the level of stock prices. It is desirable that investors' expectations of profits reflect knowledge that the central bank will respond constructively to new information about the likely course of the real economy. And I use the word "knowledge" deliberately and not just the word "expectation" to emphasize the importance of a high degree of market confidence in the central bank. When there is a high degree of confidence in the central bank, everyone should believe that the central bank will respond to events that might otherwise drive the economy into recession. In this sense, a "Fed put" should exist. A central bank is supposed to do what it can to maintain employment at a high level.

Of course, at the current state of knowledge, the central bank cannot prevent all recessions. A central bank may be unable to prevent some recessions because it has incomplete knowledge of how businesses, households, and markets behave. In other cases, a central bank has no way of forecasting certain events that may drive the economy into recession.

A central bank can do its best to respond appropriately to events like the stock market crash of 1987 and the terrorist attacks of 9/11. When such a shock occurs, market participants may be unsure about the appropriate response, and the central bank may also be unsure. Nevertheless, market participants have good reason to believe that the central bank will respond as the appropriate response becomes clear. Confidence in the central bank in this sense helps to stabilize markets.

I have emphasized the importance of Fed stabilization policy for the financial markets. The same arguments hold with equal force for markets for goods and for productive inputs. Decisions on the allocations of capital and labor are more efficient in an environment of general economic stability. Long-lived capital projects require confidence in monetary stability. Of course, other aspects of government policy are equally important, such as the rule of law and the tax and regulatory environments.

DOES FED POLICY SUCCESS BREED FINANCIAL MARKET INSTABILITY?

Some have argued, Hyman Minsky most prominently,[8] that monetary policy success breeds greater financial instability by encouraging investors to assume more risk, especially through greater leverage. Perhaps this contention is at the heart of the argument that recent Fed policy actions in response to the subprime mortgage mess will only increase financial risks in the future.

It is hard to figure out how to test the Minsky proposition, but my instinct is that it is not correct. As vexing as the current market situation is, it is important to remember that in the early 1980s the unwinding of the Great Inflation led to failure of many industrial firms, farmers, banks, and eventually a large part of the savings and loan industry. The financial turmoil of 1998 seems mild by comparison with the early 1980s; of course, we do not yet know the full extent of the current turmoil in housing and housing finance.

If an empirical test would be inconclusive, which I think it probably would be, our only recourse is to argue from a somewhat abstract perspective. We do have good reason to believe, both from theory and experience, that price level instability increases financial instability. Large changes in the inflation rate, up or down, are always unanticipated. Thus, inflation creates unanticipated changes in the real value of bonds and other contracts stated in nominal terms. The gains and losses tend to be capricious, and losses can be large enough to bankrupt those on the wrong side of the unanticipated change in inflation. The same problem arises when economic activity changes in an unanticipated fashion—bankruptcies rise during recessions.

When the price level is reasonably stable and economic activity is growing reasonably smoothly, macroeconomic risks are reduced.

[8] A convenient bibliography of Minsky's work and of work about his ideas can be found at http://cepa.newschool.edu/het/profiles/minsky.htm.

However, microeconomic risks do not disappear. The hedge fund is a good example of a firm designed to exploit microeconomic risks. The basic idea of the hedge fund is to take positions based on *relative* calculations of various sorts. In a particular industry, a hedge fund might take a long position in what it believes to be stronger firms and a short position in weaker firms. Concentration on microeconomic issues is exactly what is supposed to happen with reduction of macroeconomic risks. High leverage does increase risk, but does so in the context of both macroeconomic and microeconomic uncertainty.

Since the end of the Great Inflation, most bouts of financial instability have been associated with innovation and not with excesses created by economic stability. Innovations of all sorts encourage experimentation; some of the experiments turn out badly until engineering and management practices adapt to the innovations. As the use of steam engines spread in many different applications in the nineteenth century, boiler explosions were common. Railroad bridges fell down. The new technology of the Internet led to the dot-com bubble. We have seen the same process with financial innovation—portfolio insurance failed in the stock market crash of 1987 and highly mathematical trading strategies failed LTCM. Certain underwriting and securitization strategies for subprime mortgages are in the process of failing today, at enormous cost not only to investors but also to homeowners facing foreclosure. I do not believe that the failure of any of these financial innovations was related to the more stable price level and more stable economy of the past quarter century compared with the previous quarter century.

Some financial strategies will go the way of the steam automobile; others will be refined and become as common and routinely successful as the personal computer. Who today does not accept the basic idea of portfolio analysis, in which individual securities are not studied in isolation but in the context of their covariances with other securities?

CONCLUDING COMMENTS

In the Employment Act of 1946, Congress charged the Fed with promoting "maximum employment, production, and purchasing power." Not that long before the Employment Act a different view prevailed. David Cannadine, in his *Mellon: An American Life*, wrote recently about Andrew Mellon's attitudes during the early part of the Great Depression:

> Mellon constantly lectured the president on the importance of letting things be. The secretary belonged (as Hoover would recall) to the "leave it alone, liquidationist school," and his formula was "liquidate labor, liquidate stocks, liquidate the farmers, liquidate real estate." (Cannadine, 2006, p. 445)

That view is long gone. Macroeconomists today do not believe that policies to stabilize the price level and aggregate economic activity create a hazard. Federal Reserve policy that yields greater stability has not and will not protect from loss those who invest in failed strategies, financial or otherwise. Investors and entrepreneurs have as much incentive as they ever had to manage risk appropriately. What they do not have to deal with is macroeconomic risk of the magnitude experienced all too often in the past.

In the present situation, many investors in subprime paper will take heavy losses and there is no monetary policy that could avoid those losses. Clearly, recent Fed policy actions have not protected investors in subprime paper. The policy objective is not to prevent losses but to restore normal market processes. The issue is not whether subprime paper will trade at 70 cents on the dollar, or 30 cents, but that the paper in fact can trade at some market price determined by usual market processes. Since August, such paper has traded hardly at all. An active financial market is central to the process of economic growth, and it is that growth, not prices in financial markets per se, that the Fed cares about.

One of the most reliable and predictable features of the Fed's monetary policy is action to prevent systemic financial collapse. If this regularity of policy is what is meant by the "Fed put," then so be it, but the term seems to me to be extremely misleading. The Fed does not have the desire or tools to prevent widespread losses in a particular sector but should not sit by while a financial upset becomes a financial calamity affecting the entire economy. Whether further cuts in the federal funds rate target will alleviate financial turmoil, or risk adding to it, is always an appropriate topic for the FOMC to discuss. But one thing should be clear: The Fed does not have the power to keep the stock market at the "proper" level, both because what is proper is never clear and because the Fed does not have policy instruments it can adjust to have predictable effects on stock prices.

From time to time, to be sure, Fed action to stabilize the economy—to cushion recession or deal with a systemic financial crisis—will have the effect of pushing up stock prices. That effect is part of the transmission mechanism through which monetary policy affects the economy. However, it is a fundamental misreading of monetary policy to believe that the stock market per se is an objective of policy. It is also a mistake to believe that a policy action that is desirable to help stabilize the economy should *not* be taken because it will also tend to increase stock prices. It makes no sense to let the economy suffer from continuing declines in stock prices for the purpose of "teaching stock market speculators a lesson." "Teaching a lesson" is eerily reminiscent of Mellon's liquidationist view. Nor should the central bank attempt to protect investors from their unwise decisions. Doing so would only divert policy from its central responsibility to maintain price stability and high employment.

The Fed would create moral hazard if it were to attempt to pump up the stock market whenever it fell, regardless of whether or not such policy actions served the fundamental objectives of monetary policy. I have observed no evidence to suggest that the Fed has pursued such a course. That financial markets are more stable because market participants expect the Fed to be successful in achieving its policy objectives is a desirable and expected outcome of good monetary policy. There is no moral hazard when largely predictable policy responses to new information have effects on financial markets.

That the monetary policy principles I have discussed here are unclear to many in the financial markets is unfortunate. Macroeconomic stabilization does not raise moral hazard issues because a stable economy provides no guarantee that individual firms and households will be protected from failure. Improved public understanding of this point will not only help the Fed to do its job more effectively but also will help private sector firms to understand better how to manage risk.

REFERENCES

Cannadine, David. *Mellon: An American Life*. New York: Alfred A. Knopf, 2006.

Graham, Benjamin and Dodd, David L. *Security Analysis*. Third Edition. New York: McGraw-Hill, 1951.

Rudebusch, Glenn D. "Federal Reserve Interest Rate Targeting, Rational Expectations, and the Term Structure." *Journal of Monetary Economics*, April 1995, *35*(2), pp. 245-74.

Thornton, Daniel L. "When Did the FOMC Begin Targeting the Federal Funds Rate? What the Verbatim Transcripts Tell Us." *Journal of Money, Credit, and Banking*, December 2006, *38*(8), pp. 2039-71.

Pandemic Economics: The 1918 Influenza and Its Modern-Day Implications

Thomas A. Garrett

Many predictions of the economic and social costs of a modern-day pandemic are based on the effects of the influenza pandemic of 1918. Despite killing 675,000 people in the United States and 40 million worldwide, the influenza of 1918 has been nearly forgotten. The purpose of this paper is to provide an overview of the influenza pandemic of 1918 in the United States, its economic effects, and its implications for a modern-day pandemic. The paper provides a brief historical background as well as detailed influenza mortality statistics for cities and states, including those in the Eighth Federal Reserve District, that account for differences in race, income, and place of residence. Information is obtained from two sources: (i) newspaper articles published during the pandemic and (ii) a survey of economic research on the subject. (JEL I1, N0, R0)

Federal Reserve Bank of St. Louis *Review*, March/April 2008, *90*(2), pp. 75-93.

The possibility of a worldwide influenza pandemic in the near future is of growing concern for many countries around the globe. The World Bank estimates that a global influenza pandemic would cost the world economy $800 billion and kill tens of millions of people (Brahmbhatt, 2005). Researchers at the U.S. Centers for Disease Control and Prevention (CDC) calculate that deaths in the United States could reach 207,000 and the initial cost to the economy could approach $166 billion, or roughly 1.5 percent of GDP (Meltzer, Cox, and Fukuda, 1999). The U.S. Department of Health and Human Services paints a more dire picture—up to 1.9 million dead in the United States and initial economic costs near $200 billion (U.S. Department of Health and Human Services, 2005). The long-run costs of a modern-day influenza pandemic are expected to be much greater.

Although researchers and public officials can only speculate on the likelihood of a global influenza pandemic, many of the worst-case scenario predictions for a current pandemic are based on the global influenza pandemic of 1918. That pandemic killed 675,000 people in the United States (nearly 0.8 percent of the 1910 population), a greater number than U.S. troop deaths in World War I (116,516) and World War II (405,399) combined.[1] Roughly 40 million people died worldwide from the early spring of 1918 through the late spring of 1919.[2] In all of recorded history, only the Black Death that occurred throughout Europe from 1348 to 1351 is estimated to have killed more people (roughly 60 million) over a similar time period (Bloom and Mahal, 1997).

The years 1918 and 1919 were difficult not only because of the influenza pandemic, but because these years also marked the height of

[1] See Potter (2001) for a discussion of 1918 influenza pandemic mortalities. U.S. troop mortality data can be found at www.fas.org/sgp/crs/natsec/RL32492.pdf.

[2] Although 40 million is the commonly accepted number of worldwide deaths from the pandemic, it is likely an underestimate given the lack of adequate recordkeeping in many parts of the world.

Thomas A. Garrett is an assistant vice president and economist at the Federal Reserve Bank of St. Louis. Lesli S. Ott provided research assistance.

U.S. involvement in World War I. Given the magnitude and the concurrence of both the influenza pandemic and World War I, one would expect volumes of research on the economic effects of each event. Although significant literature on the economic consequences of World War I does exist (Rockoff, 2004), the scope of research on the economic effects of the 1918 influenza pandemic is scant at best. Most research has focused on the health and economic outcomes of descendents of pandemic survivors and the mortality differences across socioeconomic classes. (See, for example, Keyfitz and Flieger, 1968; Noymer and Garenne, 2000; Almond, 2006; and Mamelund, 2006.) Certainly an event that caused 40 million worldwide deaths in a year should be closely examined not only for its historical significance, but also for what we can learn (in the unfortunate chance the world experiences another influenza pandemic).

This paper discusses some of the economic effects of the 1918 influenza pandemic in the United States. The first section discusses demographic differences in pandemic mortalities: Were deaths higher in cities than in rural areas? Did deaths differ by race? Detailed influenza mortality data at various geographic and demographic levels at the time of the pandemic are available. The presentation of mortality data series allows for an almost unlimited number of comparisons and analyses that afford the reader the opportunity to study the available data and generate his own analyses and conclusions in addition to those presented here.

Evidence on the effects of the pandemic on business and industry is obtained from newspaper articles printed during the pandemic, with most of the articles appearing in newspapers from the Eighth Federal Reserve District cities of Little Rock, Arkansas, and Memphis, Tennessee. Newspaper articles from the fall of 1918 were used because of the almost complete absence of economic data from the era, such as data on income, employment, sales, and wages. This absence of data, especially at local levels (e.g., city and county), is a likely reason for the scarcity of economic research on the subject, although several studies that have used available economic data are reviewed here.

Although the influenza pandemic occurred nearly 90 years ago in a world that was much different from today's, the limited economic data and more readily available mortality data from the time of the event can be used to make reasonable inferences about the economic and social consequences of a modern-day pandemic. Despite technological advances in medicine and greater health coverage throughout the twentieth century, deaths from a modern-day pandemic are also likely to be related to race, income, and place of residence. Thus, the geographic and demographic differences in pandemic mortalities from 1918 can shed light on the possible effects of a modern-day pandemic, a point that is taken up in the final section of the paper.

OVERVIEW OF THE 1918 INFLUENZA PANDEMIC

The influenza pandemic in the United States occurred in three waves during 1918 and 1919.[3] The first wave began in March 1918 and lasted throughout the summer of 1918. The more devastating second and third waves (the second being the worst) occurred in the fall of 1918 and the spring of 1919 as the pandemic spread across the country:

> Spanish influenza moved across the United States in the same way as the pioneers had, for it followed their trails which had become railroads…the pandemic started along the axis from Massachusetts to Virginia…leaped the Appalachians…positioned along the inland waterways…it jumped clear across the plains and the Rockies to Los Angeles, San Francisco, and Seattle. Then, with secure bases on both coasts...took its time to seep into every niche and corner of America. (Crosby, 2003, pp. 63-64)

But the pandemic's impact on communities and regions was not uniform across the country. For example, Pennsylvania, Maryland, and

[3] For much more information on the influenza pandemic, including its origins, see Crosby (2003) and Barry (2004).

Colorado had the highest mortality rates, but these states had very little in common. Arguments have been made that mortality rates were lower in later-hit cities because officials in these cities were able to take precautions to minimize the impending influenza, such as closing schools and churches and limiting commerce. The virulence of the influenza, like a typical influenza, weakens over time, so the influenza that struck the East Coast became somewhat weaker by the time it struck the West Coast. But these reasons cannot completely explain why some cities and regions experienced high mortality rates while others were barely hit with the influenza.[4]

The global magnitude and spread of the pandemic was exacerbated by World War I, which itself is estimated to have killed roughly 10 million civilians and 9 million troops. Not only did the mass movement of troops from around the world lead to the spread of the disease, tens of thousands of Allied and Central Power troops died as a result of the influenza pandemic rather than combat (Ayres, 1919). Although combat deaths in World War I did increase the mortality rates for participating countries, civilian mortality rates from the influenza pandemic of 1918 were typically much higher. For the United States, estimates of combat-related troop mortalities are about one-tenth that of civilian mortalities from the 1918 influenza pandemic.

Mortality rates from a typical influenza tend to be the greatest for the very young and the very old. What made the 1918 influenza unique was that mortality rates were the highest for the segment of the population aged 18 to 40, and more so for males than females of this age group. In general, death was not caused by the influenza virus itself, but by the body's immunological reaction to the virus: Individuals with the strongest immune systems were more likely to die than individuals with weaker immune systems.[5] One source reports that, of 272,500 male influenza deaths in 1918, nearly 49 percent were aged 20 to 39, whereas only 18 percent were under age 5 and 13 percent were over age 50.[6] The fact that males aged 18 to 40 were the hardest hit by the influenza had serious economic consequences for the families that had lost their primary breadwinner.

Despite the severity of the pandemic, it is reasonable to say that the influenza of 1918 has almost been forgotten as a tragic event in American history. This is not good, as learning from past pandemics may be the only way to reasonably prepare for any future pandemics. Several factors may explain why the influenza pandemic of 1918 has not received a notable place in U.S. history.[7]

First, the pandemic occurred at the same time as World War I. The influenza struck soldiers especially hard, given their living conditions and close contact with highly mobile units. Much of the news from the day focused on wartime events overseas and the current status of American troops. Thus, the pandemic and World War I were seen almost as one event rather than two separate events. Second, diseases of the day such as polio, smallpox, and syphilis were incurable and a permanent part of society. Influenza, on the other hand, swept into communities, killed members of the population, and was gone. Finally, unlike polio and smallpox, no famous people of the era died from the influenza; thus, there was no public perception that even the politically powerful and rich and famous were susceptible to the virus.

Despite its lack of historical prominence, the influenza pandemic of 1918 created significant economic and social effects, even if these were short-lived. In select areas, increasing body counts overwhelmed city and medical officials. In some cities, such as Philadelphia, bodies lay along the streets and in morgues for days, similar to medieval Europe during the Black Death. In light of the potential economic turmoil and human suffering, an understanding of the state and fed-

[4] Much research has been conducted over the past decades to provide insights into why the pandemic had such different effects on different regions of the country (see, for example, Crosby, 2003, and Barry, 2004). One commonly held reason is the response of local governments to the influenza in their communities, e.g., partial versus full quarantines.

[5] The lungs typically filled with fluid and the victim drowned or died of pneumonia. See Barry (2004).

[6] The 272,500 deaths are from a sample of about 30 states. See Crosby (2003, p. 209).

[7] See Crosby (2003, pp. 319-22).

eral government response to the 1918 pandemic may also shed some light into what government at any level can do, if anything, to prevent or minimize a modern-day pandemic.

PANDEMIC MORTALITIES IN THE UNITED STATES

Data on mortalities from the 1918 influenza pandemic are found in *Mortality Statistics*, an annual publication that is released by the U.S. Census Bureau.[8] Mortalities resulting from hundreds of causes of death are listed (depending on the level of data aggregation) and are also broken down, in some cases, by age, race, and sex. Data are available at the national, state, and municipal levels and may be available by week, month, and year. In terms of coverage, "(a)ll death rates are based on total deaths, including deaths of nonresidents, deaths in hospitals and institutions, and deaths of soldiers, sailors, and marines" (U.S. Department of Commerce, Bureau of the Census, 1922, p. 9).[9] The mortality rates used in this study represent deaths from *both* influenza and pneumonia in a given year because "it is not believed to be best to study separately influenza and the various forms of pneumonia…for doubtless many cases were returned as influenza when the deaths were caused by pneumonia and vice versa" (U.S. Department of Commerce, Bureau of the Census, 1921, p. 28).[10]

Although *Mortality Statistics* provides a remarkable number of statistics, a major disadvantage of the earlier reports is that, in the 1910s, data coverage is for only 75 to 80 percent of the total population. This is because the U.S. Census Bureau acquired the mortality data over time from a registration area that consisted of a growing group of states. So mortality data for certain states are not consistently available over time. For the purposes of this article, influenza mortality data for the 1910s are available for about 30 states and encompass, on average, about 79.5 percent of the U.S. population. A casual look at the states that did and did not report mortality information does not reveal any systematic differences across each group of states with regard to population, income, and race. So the available mortality statistics are unlikely to provide a biased picture of influenza mortalities.

The following sections report select influenza mortality data at various levels of data aggregation (city and state), by race (white and non-white), and by residence (urban versus rural). The abundance of mortality statistics makes it impossible to use all existing data in a single report. However, the statistics used here do reveal some general mortality patterns that provide insights into which groups of people may be most/least affected by a modern-day pandemic, as well as how influenza mortalities differed across cities and states.

State and City Pandemic Mortalities

Pandemic mortality rates (per 100,000) for 27 states are shown in Table 1 for 1918 and 1919. The mortality rate for 1915 is also included and the ratio of 1918 mortalities to 1915 mortalities is calculated to reveal the deaths in 1918 relative to a non-pandemic year.[11] For the states shown in Table 1, Pennsylvania, Maryland, and New Jersey had the highest mortality rates in 1918, whereas Michigan, Minnesota, and Wisconsin had the lowest. The pandemic also lasted throughout the spring of 1919, so the ranking of states in 1918 does not reflect total mortalities in each state for the entire pandemic (although the rankings do remain similar).

The ratio of the 1918 mortality rate to the 1915 mortality rate ranges from a low of 3.2 (Indiana and New York) to a high of 6.5 (Montana). One caveat is that an equal increase in mortalities for a lower-population state and a higher-population state will result in a greater mortality ratio for the

[8] Copies of the historical reports are available at the CDC, National Center for Health Statistics, or at www.cdc.gov/nchs/products/pubs/pubd/vsus/historical/historical.htm. Mortalities are likely to be underestimated, as overburdened health professionals stopped recording deaths during the peak of the pandemic.

[9] Hereafter, this reference will be cited as *Mortality Statistics 1920*.

[10] Hereafter, this reference will be cited as *Mortality Statistics 1919*.

[11] The non-pandemic year is assumed to be a normal influenza year. Later analyses of city influenza mortality rates use actual data on normal and excess mortality rates rather than assuming all years except 1918 and 1919 were normal.

Table 1

Influenza Mortality Rates (per 100,000) for Select States

State	1910 Population	Area (miles²)	Population density	1915 Mortality rate	1918 Mortality rate	1919 Mortality rate	Ratio of 1918 and 1915 rates	1918 Rank
California	2,377,549	155,652	15.27	102.1	537.8	214.7	5.3	15
Colorado	799,024	103,658	7.71	170.5	766.5	253.5	4.5	5
Connecticut	1,114,756	4,820	231.28	169.2	767.7	224.5	4.5	4
Indiana	2,700,876	36,045	74.93	126.1	408.1	213.7	3.2	24
Kansas	1,690,949	81,774	20.68	116.7	474.4	188.1	4.1	22
Kentucky	2,289,905	40,181	56.99	118.0	537.3	284.6	4.6	16
Maine	742,371	29,895	24.83	166.0	589.4	229.2	3.6	14
Maryland	1,295,346	9,941	130.30	171.0	803.6	238.4	4.7	2
Massachusetts	3,366,416	8,039	418.76	170.7	726.7	207.8	4.3	8
Michigan	2,810,173	57,480	48.89	111.9	389.3	192.2	3.5	27
Minnesota	2,075,708	80,858	25.67	100.3	390.5	166.9	3.9	26
Missouri	3,293,335	68,727	47.92	144.2	476.6	206.1	3.3	20
Montana	376,053	146,201	2.57	117.7	762.7	225.4	6.5	6
New Hampshire	430,572	9,031	47.68	153.2	751.6	231.6	4.9	7
New Jersey	2,537,167	7,514	337.66	163.4	769.4	226.5	4.7	3
New York	9,113,614	47,654	191.25	185.2	598.2	233.7	3.2	12
North Carolina	2,206,287	48,740	45.27	148.4	503.1	234.4	3.4	18
Ohio	4,767,121	40,740	117.01	135.2	494.3	222.0	3.7	19
Pennsylvania	7,665,111	44,832	170.97	168.9	883.1	236.5	5.2	1
Rhode Island	542,610	1,067	508.54	185.8	681.2	239.2	3.7	9
South Carolina	1,515,400	30,495	49.69	131.9*	632.6	291.5	4.8*	10
Tennessee	2,184,789	41,687	52.41	135.3*	476.0	234.8	3.5*	21
Utah	373,351	82,184	4.54	119.5	508.8	270.8	4.3	17
Vermont	355,956	9,124	39.01	150.0	597.2	228.9	4.0	13
Virginia	2,061,612	40,262	51.20	131.1	621.1	267.2	4.7	11
Washington	1,141,990	66,836	17.09	78.4	411.5	187.9	5.2	23
Wisconsin	2,333,860	55,256	42.24	119.6	405.6	178.5	3.4	25

NOTE: Mortality rates are from *Mortality Statistics 1920* (U.S. Department of Commerce, Bureau of the Census, 1922) and include mortalities from influenza and pneumonia. *Mortalities for South Carolina and Tennessee in 1915 are 1916 and 1917 figures, respectively. Population density is population per square mile.

Table 2

Correlations of State Characteristics with Influenza Mortalities

	1915 Mortality rate	1918 Mortality rate	Ratio of 1918 and 1915 rates
Density (population/miles^2)	0.632*	0.447*	–0.097
Area (miles^2)	–0.566*	–0.253	0.350
Population	0.250	0.031	–0.236

NOTE: *Denotes statistical significance at 5 percent level or better. Correlations are based on the data in Table 1 (n = 27).

lower-population state because the increase in mortalities is a greater percentage of its population. Nevertheless, a comparison of 1915 mortality rates with those in 1918 and 1919 clearly reveals how much more severe the 1918 influenza was relative to influenza in a non-pandemic year.

Evidence suggests that influenza mortality rates had no relationship with state economic conditions, climate, or geography (see Crosby, 2003, and Brainerd and Siegler, 2003). After providing a survey of anecdotal evidence and conducting statistical analyses, Brainerd and Siegler (2003, p. 7) conclude that "the statistical evidence also supports the notion of influenza mortality as an exogenous shock to the population." However, because influenza is spread by close human contact, influenza infection and mortality rates are commonly greater in more densely populated areas.

It thus serves as an interesting exercise to see whether there is a relationship between pandemic mortalities and state population size and population density. It is also worth exploring whether the relationships are different in a pandemic year compared with a non-pandemic year. Table 2 thus presents pairwise correlations (and their statistical significance) between state population, area, and population density and 1915 mortality rates, 1918 mortality rates, and the ratio of the two mortality rates.

The correlations shown in Table 2 reveal that mortality rates in 1915 were greater in more densely populated states (0.632), but lower in larger states (–0.566). State size had no significant correlation with 1918 mortality rates, but population density was correlated with 1918 mortality rates (0.447). Note, however, that the correlation between mortality rates and density is less for 1918 mortalities than for 1915 mortalities. This finding, in addition to the fewer significant correlations (albeit just one fewer), suggest that state size and population density had less influence on mortality rates in 1918 than in 1915. Thus, as suggested by earlier research, the location of individuals was less of a factor in dying from the 1918 influenza than from a non-pandemic influenza.[12] Furthermore, the ratio of mortality rates had no relationship with state size, population, or population density, as seen in the last column of Table 2.

Mortality statistics for 49 cities are listed in Table 3. As seen in the state-level statistics, influenza mortalities in U.S. cities during the pandemic were three to five times higher, on average, than during a non-pandemic year (1915). There is slightly more variation in 1918 mortality rates across cities ($\sigma = 182$) than across states ($\sigma = 146$). The cities with the highest 1918 mortality rates (Pittsburgh, Scranton, and Philadelphia) are all located in Pennsylvania, and the cities with the lowest rates (Grand Rapids, Minneapolis, and Toledo) are all located in the Midwest.

It is possible to get an idea of the influenza's effect on rural areas versus urban areas by calculating the average 1918 mortality in all cities in a state (for which mortality data were available) and then dividing by the state-level mortality rate.[13]

[12] See Crosby (2003).

[13] Mortality rates for 64 cities (49 of which appear in Table 3) were used in the calculations. The other 15 cities were not included in Table 3 because of missing data. The mortality rates for these 15 cities can be obtained from the author.

Table 3

Influenza Mortality Rates (per 100,000) for Select Cities

City	1910 Population	1915 Mortality rate	1918 Mortality rate	1919 Mortality rate	Ratio of 1918 and 1915 rates	1918 Rank
Albany, New York	100,253	187.1	679.1	244.8	3.6	22
Atlanta, Georgia	154,839	165.7	478.4	291.4	2.9	40
Baltimore, Maryland	558,485	207.1	836.5	230.6	4.0	7
Birmingham, Alabama	132,685	158.1	843.6	319.1	5.3	6
Boston, Massachusetts	670,585	214.6	844.7	256.3	3.9	5
Bridgeport, Connecticut	102,054	206.0	825.4	272.3	4.0	8
Buffalo, New York	423,715	168.7	637.5	206.2	3.8	28
Cambridge, Massachusetts	104,839	157.3	676.5	180.0	4.3	23
Chicago, Illinois	2,185,283	172.7	516.6	191.5	3.0	35
Cincinnati, Ohio	353,591	163.4	605.4	253.2	3.7	29
Cleveland, Ohio	560,663	155.1	590.9	260.5	3.8	30
Columbus, Ohio	181,511	136.5	451.9	213.5	3.3	43
Dayton, Ohio	116,577	142.7	525.2	154.6	3.7	33
Denver, Colorado	213,381	184.8	727.7	228.5	3.9	15
Detroit, Michigan	465,766	148.1	413.4	242.4	2.8	46
Fall River, Massachusetts	119,295	213.5	799.7	216.8	3.7	9
Grand Rapids, Michigan	112,571	100.0	282.7	93.8	2.8	49
Indianapolis, Indiana	233,650	146.7	459.4	240.6	3.1	42
Jersey City, New Jersey	267,779	211.2	756.6	317.0	3.6	13
Kansas City, Missouri	248,381	176.1	718.1	301.1	4.1	17
Los Angeles, California	319,198	87.4	484.5	186.8	5.5	38
Lowell, Massachusetts	106,294	191.3	696.1	198.4	3.6	19
Memphis, Tennessee	131,105	179.3	666.1	340.6	3.7	24
Milwaukee, Wisconsin	373,857	158.9	474.1	187.7	3.0	41
Minneapolis, Minnesota	301,408	121.6	387.7	169.4	3.2	48
Nashville, Tennessee	110,364	179.9	910.2	301.0	5.1	4
New Haven, Connecticut	133,605	207.9	768.0	212.3	3.7	11
New Orleans, Louisiana	339,075	245.8	768.6	333.7	3.1	10
New York, New York	4,766,883	212.1	582.5	265.8	2.7	31
Newark, New Jersey	347,469	146.6	680.4	213.3	4.6	21
Oakland, California	150,174	98.6	496.6	238.2	5.0	36
Omaha, Nebraska	124,096	150.9	660.8	191.8	4.4	26
Paterson, New Jersey	125,600	159.4	683.6	235.7	4.3	20
Philadelphia, Pennsylvania	1,549,008	189.2	932.5	222.9	4.9	3
Pittsburgh, Pennsylvania	533,905	260.1	1,243.6	431.8	4.8	1
Portland, Oregon	207,214	69.6	448.2	246.4	6.4	44
Providence, Rhode Island	224,326	191.4	737.4	253.3	3.9	14
Richmond, Virginia	127,628	209.9	661.0	269.5	3.1	25
Rochester, New York	218,149	121.8	522.7	152.8	4.3	34
San Francisco, California	416,912	130.6	647.7	283.3	5.0	27
Scranton, Pennsylvania	129,867	223.7	985.7	247.5	4.4	2
Seattle, Washington	237,194	74.7	425.5	189.8	5.7	45
Spokane, Washington	104,402	91.9	487.4	210.7	5.3	37
St. Louis, Missouri	687,029	156.7	536.5	202.3	3.4	32
St. Paul, Minnesota	214,744	127.8	480.6	145.9	3.8	39
Syracuse, New York	137,249	120.5	704.6	155.9	5.8	18
Toledo, Ohio	168,497	126.8	401.0	181.9	3.2	47
Washington, D.C.	331,069	189.8	758.8	225.9	4.0	12
Worcester, Massachusetts	145,986	188.9	727.1	248.9	3.8	16

NOTE: Mortality rates are from *Mortality Statistics 1920* and include mortalities from influenza and pneumonia.

Table 4

City Influenza Mortality Rate Relative to State Mortality Rate (1918)

State	Average of cities relative to state
Michigan	0.89
Colorado	0.95
California	1.01
New York	1.02
Maryland	1.04
Massachusetts	1.06
Connecticut	1.07
Washington	1.11
Pennsylvania	1.11
Minnesota	1.11
Indiana	1.13
New Jersey	1.16
Wisconsin	1.17
Virginia	1.17
Ohio	1.19
Missouri	1.32
Kansas	1.58
Tennessee	1.66

These ratios are shown in Table 4. A ratio greater than 1 suggests influenza deaths were, on average, greater in a state's cities than in the rural areas of the state—and vice versa for a ratio less than 1. As seen in Table 4, most of the ratios are greater than 1, with some much greater than 1 (Missouri, Kansas, and Tennessee), thus revealing that cities in their respective state had higher mortality rates than rural areas of that state. This finding supports the positive correlation between population density and influenza mortalities shown in Table 2.

Influenza Mortalities and Race

Influenza mortalities by race are available for some cities in the United States, although the racial breakdown is not as detailed as it is for modern-day mortality statistics. Mortality statistics for 1918 are provided on the basis of white and non-white. Table 5 presents a breakdown of white and non-white mortality rates (per 100,000 for each racial group) for 14 U.S. cities. For each racial group, influenza mortality rates for 1915 are also included so a comparison can be made between a pandemic year and a non-pandemic year. The first six columns of Table 5 clearly show that non-white influenza mortalities are higher than white influenza mortalities in both pandemic and non-pandemic years (except for Kansas City in 1918). Whites experienced relatively higher mortality during the pandemic year 1918 (compared with the non-pandemic year 1915) than did non-whites.

It is likely that racial differences in influenza mortality rates reflect, to some degree, differences in population density (as seen in Table 2) and geography (as seen in Table 4). Data on white and non-white populations as well as rural and urban residences for several decennial Census years are shown in Table 6. In 1910, the great majority of the urban population (having a higher population density than rural areas) in the United States was white (over 90 percent). This offers some explanation as to why whites as a group had a much larger increase in influenza mortalities during the pandemic than did non-whites. But, the decline in the strength of the mortality/density relationship in 1918 compared with that of 1915 (see Table 2) suggests that urban location alone cannot account for the relatively large increase in influenza mortalities among whites.

What does this imply if an influenza pandemic struck today? The last two columns of Table 6 reveal that the non-white population in the United States has become much more urban (27 percent in 1910 and 91 percent in 2000) compared with the white population (49 percent in 1910 and 75 percent in 2000). However, the fact that both racial groups are becoming more urban does not bode well for either group because population density will certainly be a significant determinant of mortality. However, a modern-day pandemic may result in greater non-white mortality rates because a greater percentage of the non-white population in the United States lives in urban areas.

Table 5

Influenza Mortality Rate By Race and City, 1915 and 1918

City	White mortality rate 1918	Non-white mortality rate 1918	White, as percent of non-white 1918	White mortality rate 1915	Non-white mortality rate 1915	White, as percent of non-white 1915	White, 1915, as percent of white 1918	Non-white 1915, as percent of non-white 1918
Birmingham	676.3	1,101.8	61.4	114.7	225.0	51.0	17.0	20.4
Atlanta	362.2	730.3	49.6	99.3	305.5	32.5	27.4	41.8
Indianapolis	440.6	615.2	71.6	132.9	264.5	50.2	30.2	43.0
Kansas City, Missouri	758.5	701.6	108.1	216.9	445.2	48.7	28.6	63.5
Louisville	1,012.3	1,015.5	99.7	111.2	369.6	30.1	11.0	36.4
New Orleans	679.7	1,019.0	66.7	165.1	472.3	35.0	24.3	46.3
Baltimore	787.8	1,086.9	72.5	169.3	406.0	41.7	21.5	37.4
Memphis	608.0	766.0	79.4	111.4	290.7	38.3	18.3	38.0
Nashville	884.0	1,060.4	83.4	130.0	288.7	45.0	14.7	27.2
Dallas	572.8	845.8	67.7	67.9*	149.8*	45.3*	11.9*	17.7*
Houston	485.8	618.5	78.5	98.0*	143.9*	68.1*	20.2*	23.3*
Norfolk	739.8	835.6	88.5	98.8	305.8	32.3	13.4	36.6
Richmond	555.8	883.4	62.9	131.5	367.0	35.8	23.7	41.5
Washington, D.C.	694.3	942.0	73.7	129.9	354.9	36.6	18.7	37.7

NOTE: *Mortality rates for Dallas and Houston for 1915 are 1916 and 1917 figures, respectively.

Table 6

Location and Race, 1890-2000

Year	White as percent of U.S. urban population	Non-white as percent of U.S. urban population	Percent of white population that is urban	Percent of non-white population that is urban
1890	93.35	6.65	35.06	17.54
1910	93.45	6.55	48.73	27.26
1930	92.18	7.82	57.63	43.20
1950	89.93	10.07	64.29	61.64
1970	86.24	13.76	72.45	80.71
1990	76.88	23.12	72.02	88.21
2000	71.45	28.55	75.17	90.59

SOURCE: Population data are from *Historical Statistics of the United States*, U.S. Census.

Table 7

Influenza Mortalities—Cities in Eighth District States

Year	Total influenza deaths per 100,000	Total "excess" influenza deaths	"Normal" influenza deaths	Ratio of total deaths to "normal" deaths
Louisville, Kentucky				
1915	156.5	359	340	1.06
1916	185.2	427	342	1.25
1917	209.5	485	366	1.33
1918	1,012.9	2,357	1,287	1.83
1919	357.8	837	322	2.59
1920	197.2	463	322	1.44
Memphis, Tennessee				
1915	179.3	263	261	1.01
1916	N/A	N/A	N/A	N/A
1917	219.0	335	282	1.19
1918	666.1	1,040	312	3.33
1919	340.6	542	316	1.71
1920	311.4	506	369	1.37
Nashville, Tennessee				
1915	179.9	206	209	0.98
1916	N/A	N/A	N/A	N/A
1917	188.6	219	230	0.95
1918	910.2	1,063	249	4.27
1919	301.0	354	234	1.51
1920	301.9	357	232	1.54
St. Louis, Missouri				
1915	156.7	1,144	1,191	0.96
1916	200.4	1,480	1,212	1.22
1917	227.0	1,696	1,216	1.39
1918	536.5	4,054	1,262	3.21
1919	202.3	1,546	1,207	1.28
1920	262.9	2,032	1,198	1.70

NOTE: Column 1: Total influenza deaths per 100,000 are from *Mortality Statistics 1920*. Column 2: The number of influenza deaths was computed by multiplying the death rates in column 1 by the city population for the respective year. Column 3: This variable uses information on excess influenza deaths. Excess deaths from influenza are reported in U.S. Treasury and Public Health Service (1930, Table A). In the preceding report, excess deaths (on an annual basis) per 100,000 are defined as the excess over the median monthly rate for the period 1910-16 prior to July 1, 1919, and as the excess over the median monthly rate for the period 1921-27 after July 1, 1919. For the purpose here, the rates on an annual basis were converted to a monthly basis, then converted to levels, and then summed for the year to get a measure of the total number of excess deaths for the city for the year. It is this number that is subtracted from total deaths (column 2) to get the number of "normal" deaths shown in column 3. Column 4: Column 2 divided by column 3.

Of course, race and place of residence (and population density) are not the only factors that are likely to influence mortality rates. Access to health care is likely to be critical (assuming health professionals themselves are not decimated by the pandemic). So it stands to reason that mortality rates in urban areas may be somewhat mitigated given the relatively greater access to health care than in rural areas. Ability to pay, which relates to income, may also be important. Urban areas, on average, tend to have greater incomes, but this is an average and ignores those individuals with low incomes in urban areas who cannot afford health care. The ability of free clinics and

Table 7, cont'd

Influenza Mortalities—Cities in Eighth District States

Year	Total influenza deaths per 100,000	Total "excess" influenza deaths	"Normal" influenza deaths	Ratio of total deaths to "normal" deaths
Kansas City, Missouri				
1915	176.1	504	386	1.31
1916	138.7	408	397	1.03
1917	205.0	618	407	1.52
1918	718.1	2,220	479	4.64
1919	301.1	954	429	2.22
1920	353.6	1,147	489	2.35
Chicago, Illinois				
1915	172.7	4,220	4,884	0.86
1916	168.4	4,202	5,000	0.84
1917	201.7	5,137	5,082	1.01
1918	516.6	13,423	5,433	2.47
1919	191.5	5,075	4,388	1.16
1920	223.9	6,049	2,893	2.09
Indianapolis, Indiana				
1915	146.7	420	383	1.10
1916	153.7	452	396	1.14
1917	156.6	472	301	1.57
1918	459.4	1,420	467	3.04
1919	240.6	762	425	1.79
1920	240.9	782	432	1.81

emergency rooms to remain open during a pandemic will be crucial to the treatment of lower-income individuals. The final section of this article will expand on these points.

Pandemic Mortalities in the States of the Eighth Federal Reserve District

Table 7 shows available data on mortalities from 1915 to 1920 for cities located in the states of the Eighth Federal Reserve District. The first column of data contains mortality rates per 100,000 population (from *Mortality Statistics 1920*). The number of deaths (found by multiplying the rate in the first column by city population) is shown in the second column. The third column contains "normal" influenza deaths and was calculated by subtracting the number of excess deaths in each year from the total number of deaths shown in column 2. Normal influenza deaths reflect the number of influenza deaths absent a pandemic and are based on deviations from historical median monthly rates.[14] The ratio of total deaths to normal deaths presented in column 4 provides a measure of the severity of influenza in each year relative to a normal influenza. Clearly, this ratio is much larger for the years 1918 and 1919.

The data in Table 7 allow for several interesting comparisons. First, in all cities, the ratio of total deaths to normal deaths in pandemic years was at least twice the normal rate. The ratio was over four times as high in Nashville and Kansas City, Missouri, in 1918 and at least three times as high in Memphis, St. Louis, and Indianapolis. Chicago and Louisville had the lowest ratios in 1918 (2.47 and 1.83, respectively). So, although larger cities such as Chicago had more influenza

[14] See the note for Table 7 for a description of how normal and excess influenza mortality rates were calculated for the 50 largest cities in the United States.

Table 8

Urban/Rural Influenza Mortalities: Eighth District States and Cities

Year	State mortality rate per 100,000	Rural mortality rate per 100,000	Rural rate as percent of Louisville rate
Kentucky			
1915	118.0	113.9	72.8
1916	152.7	149.3	80.6
1917	144.7	137.8	65.8
1918	537.3	486.8	48.1
1919	284.6	276.7	77.4
1920	197.6	197.6	100.2

Year	State mortality rate per 100,000	Rural mortality rate per 100,000	Rural rate as percent of Chicago rate
Illinois			
1915	N/A	N/A	N/A
1916	N/A	N/A	N/A
1917	N/A	N/A	N/A
1918	498.8	486.2	94.1
1919	187.9	185.4	96.8
1920	213.2	205.9	92.0

Year	State mortality rate per 100,000	Rural mortality rate per 100,000	Rural rate as percent of Indianapolis rate
Indiana			
1915	126.1	123.8	84.4
1916	147.1	146.4	95.2
1917	146.2	145.0	92.6
1918	408.1	401.9	87.5
1919	213.7	210.4	87.5
1920	211.7	208.1	86.4

NOTE: The rural mortality rates are for the state less the city(ies) listed. This statistic was computed by obtaining the number of influenza deaths at the state level (the first column multiplied by population) and then subtracting the number of city dead (shown in Table 7). This value was then normalized by the rural population (the difference between the state population and the city population). The final column was computed by dividing the rural mortality rate by the city mortality rate shown in the first column of Table 7.

SOURCE: The state mortality rates are from *Mortality Statistics 1920*.

deaths in 1918 (and other years as well), the relative mortality of influenza in these larger cities was less than that in smaller cities such as Nashville and Kansas City.

State-level mortality rates and rural mortality rates for states located in the Eighth Federal Reserve District are shown in Table 8. The rural mortality rates are not necessarily reflective of what one thinks a rural area to be: The rural mortality rates in Table 8 are computed by subtracting the number of mortalities in a city (from Table 7) from the number of mortalities at the state level (first column of Table 8).[15] Thus, for example, the rural mortality rate in Kentucky is the mortality rate for all of Kentucky except for Louisville. Certainly, there are other non-rural areas in Kentucky in addition to Louisville, but mortality

[15] See the note for Table 8 for more information on how the rural mortality rate was calculated.

Table 8, cont'd

Urban/Rural Influenza Mortalities: Eighth District States and Cities

Year	State mortality rate per 100,000	Rural mortality rate per 100,000	Rural rate as percent of St. Louis rate	Rural rate as percent of Kansas City rate
Missouri				
1915	144.2	N/A	N/A	N/A
1916	167.9	N/A	N/A	N/A
1917	181.4	164.4	72.4	80.2
1918	476.6	423.5	78.9	59.0
1919	206.1	194.2	96.0	64.5
1920	261.2	247.6	94.2	70.0

Year	State mortality rate per 100,000	Rural mortality rate per 100,000	Rural rate as percent of Memphis rate	Rural rate as percent of Nashville rate
Tennessee				
1915	N/A	N/A	N/A	N/A
1916	N/A	N/A	N/A	N/A
1917	135.3	126.1	57.6	66.9
1918	476.0	436.1	65.5	47.9
1919	234.8	222.7	65.4	74.0
1920	220.0	208.0	66.8	68.9

data on these areas are not available. Nevertheless, because mortality rates are generally available for the largest cities in a state, the rural mortality rates are likely to provide an approximate picture of the influenza's impact on the less-populated areas of a state.

The data in Table 8—rural mortality rate relative to the city mortality rate for each state—are similar to the data presented in Table 4; but, the data in Table 8 allow for multiple-year comparisons and a comparison between "rural" and "city" rather than city and state. As Table 8 shows, the state rural rate is almost always less than the city rate (except for Kentucky in 1920), which also supports the results in Table 2 that reveal a positive correlation between population density and influenza mortalities.

Although the rural mortality rate is less than the city rate in most cases, there are differences in rates across states and over time. For example, the rural-to-city mortality ratio in Illinois averages about 94 percent, whereas the rate averages around 77 percent in Missouri. There does not appear to be, however, a consistent difference in mortality rates between pandemic years and non-pandemic years when comparing across the states, although it appears that the rural-to-city mortality ratio is substantially higher in non-pandemic years in Kansas City, Louisville, and Nashville. What one can conclude from Table 8 is that rural influenza mortality rates were typically less than city influenza rates in both pandemic and non-pandemic years, and only in the case of a few cities is there evidence that the rural-to-city mortality ratio was less in a pandemic year compared with non-pandemic years.

ECONOMIC EFFECTS OF THE 1918 INFLUENZA PANDEMIC

As mentioned earlier, the greatest disadvantage of studying the economic effects of the 1918 influenza is the lack of economic data. There are some academic studies that have looked at the economic effects of the pandemic using available data, and these studies are reviewed below. Given the general lack of economic data, however, a remaining source for information on (some) economic effects of the 1918 pandemic is print media. Newspapers in the Eighth Federal Reserve District cities of Little Rock and Memphis that were printed in the fall of 1918 contained information on the effects of the influenza pandemic in these cities. Piecing together anecdotal information from individual cities provides a relatively good picture of the general effects of the pandemic and the potential economic effects of a modern-day pandemic.

The 1918 Influenza Pandemic in the News

This section presents headlines and summaries from articles appearing in two newspapers in Eighth Federal Reserve District cities: *The Arkansas Gazette* (Little Rock) and *The Commercial Appeal* (Memphis). Articles listing the number of sick or dead from the influenza appeared almost daily in these newspapers and other papers as well (St. Louis and Louisville, for example). Also appearing frequently were articles on church, school, and theater closings, as well as dubious remedies and cures for the influenza.[16] However, articles that described the influenza's effects on the local economy were far less numerous. The several articles that appeared in the fall of 1918 that did discuss the economic impact of the influenza are summarized below.

Little Rock, Arkansas
"How Influenza Affects Business."
***The Arkansas Gazette*, October 19, 1918, p. 4.**

- Merchants in Little Rock said their business had declined 40 percent. Others estimated the decrease at 70 percent.
- The retail grocery business was reduced by one-third.
- A department store that had been doing $15,000 in daily business ($200,265 in 2006 dollars) reported that it was doing no more than half that.
- Bed rest was emphasized in the treatment of influenza. As a result, there was increased demand for beds, mattresses, and springs.
- Little Rock businesses were losing $10,000 per day on average ($133,500 in 2006 dollars). This was from actual loss of inventory, not a decrease in business that may have been covered by an increase in sales when the quarantine order was over. (That is, certain items could not be stored and sold at a later time.)
- The only business in Little Rock that showed an increase in sales was the drug store.

Memphis, Tennessee
"Influenza Crippling Memphis Industries."
***The Commercial Appeal*, October 5, 1918, p. 7.**

- Physicians reported they were kept too busy combating the disease to report the number of their patients and had little time to devote to other matters.
- Industrial plants were running under a great handicap. Many of them were already short of help because of the draft.
- Railway service was curtailed when, out of a total of about 400 men used in the transportation department of the Memphis Street Railway, 124 men were incapacitated.
- The Cumberland Telephone Co. reported more than one hundred operators absent from their posts. The telephone company asked that unnecessary calls be eliminated.

"Tennessee Mines May Shut Down."
***The Commercial Appeal*, October 18, 1918, p. 12.**

- Coal mine operators reported a 50 percent decrease in production.
- Mines throughout east Tennessee and southern Kentucky were on the verge of closing down, owing to the epidemic raging through the mining camps.
- Coalfield, Tennessee, with a population of 500, had "only 2 percent of well people."

16 Copies of all articles are available from the author, including articles from the St. Louis *Post-Dispatch* and the Louisville *Courier-Journal*.

Survey of Academic Research

Garrett (2006) examines the immediate effect of influenza mortalities on manufacturing wages in U.S. cities and states for the period 1914-19. The testable hypothesis of the paper is that influenza mortalities had a direct impact on wage rates in the manufacturing sector in U.S. cities and states during and immediately after the 1918 influenza pandemic. The hypothesis is based on a simple economic model of the labor market: A decrease in the supply of manufacturing workers that resulted from influenza mortalities would have had the initial effect of reducing manufacturing labor supply, increasing the marginal product of labor and capital per worker, and thus increasing real wages. In the short term, labor immobility across cities and states is likely to have prevented wage equalization across the states, and a substitution away from relatively more-expensive labor to capital is unlikely to have occurred.[17] Garrett (2006) finds that states and cities having had greater influenza mortalities experienced greater wage growth from 1914 to 1919—roughly 2 to 3 percentage points for a 10 percent change in per capita mortalities. Approximately 4 percent of total wage growth from 1914 to 1919 is attributed to influenza mortalities.

Brainerd and Siegler (2003) explored the impact of the influenza pandemic on state income growth for the decade after the influenza pandemic. The authors argue that states that experienced larger numbers of influenza deaths per capita would have experienced higher rates of growth in per capita income after the pandemic. States with higher influenza mortality rates would have had a greater increase in capital per worker and thus also output per worker and higher incomes after the pandemic. Using state-level personal income estimates for 1919-21 and 1930, Brainerd and Siegler (2003) do find a positive and statistically significant relationship between statewide influenza mortality rates and subsequent state per capita income growth.

Almond (2006) explored the longer-term effect of the 1918 influenza. The author questions whether in utero exposure to the influenza had negative economic consequences for individuals later in their lives. The author's hypothesis is that an individual's health endowment is positively related to his human capital and productivity and thus also to wages and income (the fetal origins hypothesis). Using 1960-80 decennial census data, Almond (2006) found that cohorts in utero during the 1918 pandemic had reduced educational attainment, higher rates of physical disability, and lower income. Specifically, "[m]en and women show large and discontinuous reductions in educational attainment if they had been in utero during the pandemic. The children of infected mothers were up to 15 percent less likely to graduate from high school. Wages of men were 5-9 percent lower because of infection" (Almond, 2006, p. 673).

Most of the evidence indicates that the economic effects of the 1918 influenza pandemic were short term. Many businesses, especially those in the service and entertainment industries, suffered double-digit losses in revenue. Other businesses that specialized in health care products experienced an increase in revenues. Some academic research suggests that the 1918 influenza pandemic caused a shortage of labor that resulted in higher wages (at least temporarily) for workers, although no reasonable argument can be made that this benefit outweighed the costs from the tremendous loss of life and overall economic activity. Research also suggests that the 1918 influenza caused reductions in human capital for those individuals in utero during the pandemic—therefore having implications for economic activity occurring decades after the pandemic.

[17] The long-run effect of influenza and war mortalities on manufacturing wage growth is less clear. Although the Solow (1956) growth model suggests that capital per worker will eventually fall (due to diminishing returns to capital) and therefore decrease wages, Romer's (1986) growth model predicts capital per worker will continue to rise over time as a result of non-diminishing returns to capital, thereby increasing wages. It is also possible that the war and the pandemic decreased consumer confidence, investment, and savings, and long-term income growth of households due to the death of households' primary breadwinners. These factors would result in lower aggregate output and production, thereby decreasing the demand for labor and placing downward pressure on manufacturing wages. Finally, the higher wages would eventually be bid down as more people would be attracted to areas initially offering higher wages.

IMPLICATIONS FOR A MODERN-DAY PANDEMIC

As mentioned at the beginning of this article, the potential financial costs and death tolls from a modern-day pandemic in the United States suggest an initial cost of several hundred billion dollars and the deaths of hundreds of thousands to several million people. The information presented here and information provided in two prominent publications (see Crosby, 2003, and Barry, 2004) on the 1918 influenza pandemic can be used to formulate a list of the likely economic effects of a modern-day influenza pandemic and possible ways to mitigate its severity:

- Given the positive correlation between population density and influenza mortalities, cities are likely to have greater mortality rates than rural areas. Compared with 1918, however, urban and rural areas are more connected today, which may decrease the difference in mortality rates between cities and rural areas. Similarly, a greater percentage of the U.S. population is now considered urban (about 79 percent) compared with the U.S. population at the time of the pandemic (51 percent in 1920).
- Non-white groups as a whole have a greater chance of death because roughly 90 percent of all non-whites live in urban areas (compared with about 75 percent of whites). This correlates with lower-income individuals being more likely to die—non-white (excluding Asians) households have a lower median income ($30,858 in 2005) compared with white households ($50,784 in 2005) Similarly, only 10 percent of whites were below the poverty level in 2005 compared with over 20 percent for various minority groups (except Asians) (DeNavas-Walt, Proctor, and Lee, 2006, Table 4).
- Urban dwellers are likely to have, on average, better physical access to quality health care; however, nearly 19 percent of the city population in the United States has no health coverage, compared with only 14 percent of the rural population (DeNavas-Walt, Proctor, and Lee, 2006, Table 8). Questions remain regarding the affordability of health care and whether free-service health care providers, clinics, and emergency rooms (the most likely choices for the uninsured) are able to handle victims of the pandemic.
- Health care is irrelevant unless there are systems in place to ensure that an influenza pandemic will not incapacitate health-care provision and prevent the rapid disposal of the dead in the cities (as it did in Philadelphia in 1918, exacerbated by medical leaves during World War I). If medical staff succumbs to the influenza and facilities are overwhelmed, the duration and severity of the pandemic will be increased. In Philadelphia, for example, "the city morgue had as many as ten times as many bodies as coffins" (Crosby, 2003, p. 82).
- A greater percentage of families with life insurance would mitigate the financial effects from the loss of a family's primary breadwinner. However, life insurance is a normal good (positively correlated with income), so low-income families are less likely to be protected with insurance than are higher-income families (Cummins and Mahul, 2004).
- Local quarantines would likely hurt businesses in the short run. Employees would likely be laid off. Families with no contact to the influenza may too experience financial hardships.
- Some businesses could suffer revenue losses in excess of 50 percent. Others, such as those providing health services and products, may experience an increase in business (unless a full quarantine exists). If the pandemic causes a shortage of employees, there could be a temporary increase in wages for remaining employees in some industries. This is less likely than in 1918, however, given the greater mobility of workers that exists today.

- Can we rely on local, state, and federal governments to help in the case of a modern-day pandemic? Government has shown its inability to coordinate some disasters in the past (e.g., Hurricane Katrina). Governmental decisions at the time of the 1918 influenza also had unfortunate consequences. In fact, the decision of local officials in Philadelphia to proceed with a Liberty Bond parade during the pandemic significantly increased mortality rates. Nearly 20,000 people gathered together in downtown Philadelphia for the event. Days later, influenza mortality rates in Philadelphia soared, making Philadelphia one of the hardest hit cities during the pandemic. Officials in St. Louis (a comparable city to Philadelphia at the time), however, responded quickly to the influenza by closing nearly all public places as soon as the influenza had reached the city. As a result, influenza mortality rates were much lower than in Philadelphia.

FINAL THOUGHTS

The influenza of 1918 was the most serious epidemic in the history of the United States. Hundreds of thousands of people died and millions were infected with the highly contagious influenza virus. The possibility of a future influenza pandemic has focused research back to the 1918 pandemic as a foundational model for the likely effects of a modern-day influenza outbreak in the United States. Despite the severity of the 1918 influenza, however, there has been relatively little research done on the economic effects of the pandemic. This article has provided a concise, albeit certainly not complete, discussion and analysis of the economic effects of the 1918 influenza pandemic based on available data and research.

The influenza of 1918 was short-lived and "had a permanent influence not on the collectivities but on the atoms of human society—individuals" (Crosby, 2003, p. 323). Society as a whole recovered from the 1918 influenza quickly, but individuals who were affected by the influenza had their lives changed forever. Given our highly mobile and connected society, any future influenza pandemic is likely to be more severe in its reach, and perhaps in its virulence, than the 1918 influenza despite improvements in health care over the past 90 years. Perhaps lessons learned from the past can help mitigate the severity of any future pandemic.

An important difference between 1918 and now is that we have the CDC and similar organizations in other countries that monitor outbreaks of disease, send teams to identify and isolate diseases, and coordinate responses.[18] We also have national flu vaccination programs and funding. The question remains whether all of this is adequate in the event of a pandemic. A recent report from the Infectious Diseases Society of America (2005) suggests that the United States is not prepared for an influenza pandemic. Although federal, state, and local governments in the United States have started to focus on preparedness in recent years, it is fair to say that progress has been slow, especially at local levels of government.[19] The key to mitigating a pandemic is the successful cooperation and planning of all levels of government, something that has not always occurred in the past. Although we are certainly more prepared for an influenza pandemic now than in 1918, there should still be concern about government's readiness and ability to protect citizens from a pandemic.

REFERENCES

Almond, Douglas. "Is the 1918 Influenza Pandemic Over? Long-Term Effects of In Utero Influenza Exposures in the Post-1940 U.S. Population."

---

[18] The CDC's pandemic influenza plan has two parts: (i) delay or prevent the influenza from reaching the United States and (ii) if the influenza does reach the United States, minimize the rates of infection (and thus mortalities); see www pandemicflu.gov. Local governments' plans focus on minimizing the rates of infection.

[19] See www.pandemicflu.gov, a site managed by the U.S. Department of Health and Human Services. The lack of influenza vaccines, low production capacity, inadequate supply networks, slow government response, and poor public education are cited as problems.

Journal of Political Economy, August 2006, *114*(4), pp. 672-712.

Ayres, Leonard P. *The War With Germany: A Statistical Summary*. Washington, DC: U.S. Government Printing Office, 1919.

Barry, John M. *The Great Influenza: The Epic Story of the Deadliest Plague in History*. New York: Viking, 2004.

Bloom, David E. and Mahal, Ajay S. "AIDS, Flu, and the Black Death: Impacts on Economic Growth and Well-Being," in David Bloom and Peter Godwin, eds., *The Economics of HIV and AIDS: The Case of South and Southeast Asia*. Delhi: Oxford University Press, 1997, pp. 22-52.

Brahmbhatt, Milan. "Avian Influenza: Economic and Social Impacts." Washington, DC: World Bank, September 23, 2005.

Brainerd, Elizabeth and Siegler, Mark. "The Economic Effects of the 1918 Influenza Epidemic." Discussion Paper 3791, Centre for Economic Policy Research, 2003.

Crosby, Alfred W. *America's Forgotten Pandemic: The Influenza of 1918*. Cambridge: Cambridge University Press, 2003.

Cummins, J. David and Mahul, Olivier. "The Demand for Insurance with an Upper Limit on Coverage." *Journal of Risk and Insurance*, June 2004, *71*(2), pp. 253-64.

DeNavas-Walt, Carmen; Proctor, Bernadette and Lee, Cherry Hill. *Income, Poverty, and Health Insurance Coverage in the United States: 2005*. Washington, DC: Census Bureau, 2006; www.census.gov/prod/2006pubs/p60-231.pdf.

Garrett, Thomas A. "War and Pestilence as Labor Market Shocks: U.S. Manufacturing Wage Growth 1914-1919." Working Paper 2006-018C, Federal Reserve Bank of St. Louis, 2006. Forthcoming in *Economic Inquiry*.

Infectious Diseases Society of America. "IDSA's Principles for Actions Needed to Prepare the U.S. to Effectively Respond to Interpandemic/Pandemic Influenza." March 2005; www.idsociety.org.

Keyfitz, Nathan and Flieger, Wilhelm. *World Population: An Analysis of Vital Data*. Chicago: University of Chicago Press, 1968.

Mamelund, Sven-Erik. "A Socially Neutral Disease? Individual Social Class, Household Wealth and Mortality from Spanish Influenza in Two Socially Contrasting Parishes in Kristiania, 1918-19." *Social Science and Medicine*, February 2006, *62*(4), pp. 923-40.

Meltzer, Martin I.; Cox, Nancy J. and Fukuda, Keiji. "The Economic Impact of Pandemic Influenza in the United States: Priorities for Intervention." *Emerging Infectious Diseases*, September/October 1999, *5*(5), pp. 659-71.

Noymer, Andrew and Garenne, Michel. "The 1918 Influenza Epidemic's Effects on Sex Differentials in Mortality in the United States." *Population and Development Review*, September 2000, *26*(3), pp. 565-81.

Potter, C.W. "A History of Influenza." *Journal of Applied Microbiology*, October 2001, *91*(4), p. 572-79.

Rockoff, Hugh. "Until It's Over, Over There: The U.S. Economy in World War I." NBER Working Paper No. 10580, National Bureau of Economic Research, 2004.

Romer, Paul M. "Increasing Returns and Long-Run Growth." *Journal of Political Economy*, October 1986, *94*(5), pp. 1002-37.

Solow, Robert M. "A Contribution to the Theory of Economic Growth." *Quarterly Journal of Economics*, February 1956, *70*(1), p. 65-94.

U.S. Department of Commerce, Bureau of the Census. *Mortality Statistics 1919*. Twentieth Annual Report. Washington, DC: U.S. Government Printing Office, 1921.

U.S. Department of Commerce, Bureau of the Census. *Mortality Statistics 1920*. Twenty-First Annual Report. Washington, DC: U.S. Government Printing Office, 1922.

U.S. Department of Health and Human Services. "HHS Pandemic Influenza Plan." Washington, DC: U.S. Government Printing Office, November 2005.

U.S. Treasury and Public Health Service. *Mortality from Influenza and Pneumonia in 50 Large Cities of the United States, 1910-1929*. Washington, DC: U.S. Government Printing Office, 1930.

Friedman and Taylor on Monetary Policy Rules: A Comparison

Edward Nelson

The names Milton Friedman and John Taylor are associated with different monetary policy rules; but, as shown in this paper, the difference between their perceptions of how the economy works is not great. The monetary policy rules advanced by Taylor and Friedman are compared by linking the rules to the two economists' underlying views about nominal rigidity, the source of trade-offs, the sources of shocks, and model uncertainty. Taylor and Friedman both emphasized Phillips curve specifications that impose temporary nominal price rigidity and the long-run natural-rate restriction; and they basically agreed on the specification of shocks, policymaker objectives, and trade-offs. Where they differed was on the extent to which structural models should enter the monetary policy decisionmaking process. This difference helps account for the differences in their preferred monetary policy rules. (JEL E42, E51, E61)

Federal Reserve Bank of St. Louis *Review*, March/April 2008, *90*(2), pp. 95-116.

Over 25 years ago, John Taylor observed, "Of course, you have to go back and try to interpret what early economists actually said. Because they were never quite as explicit as economists tend to be now, this is not easy."[1] Taylor probably did not have Milton Friedman in mind when he made those remarks. But, in retrospect, they fit Friedman very well, as Friedman's work rarely used models that were very explicit, especially by today's standards. Moreover, Friedman qualifies as a significant "early economist" for the research areas that Taylor has been most associated with: nominal rigidities, the role of expectations in price setting, welfare analysis and trade-offs for monetary policy, and monetary policy rules. In the discussion that follows, I attempt to provide a systematic comparison of Friedman's and Taylor's views on these issues and their implied modeling choices.

OBJECTIVE FUNCTION

How do Friedman's and Taylor's views of policymaker objective functions compare? Taylor was more explicit on this issue, so I consider him first.

Taylor on Policymaker Objectives

As is well known, Taylor (1979) worked with a policymaker objective function that penalized deviations of inflation from a target and output from its natural level. The function consisted of the expected value of the sum across periods of the loss function,

$$(1) \qquad \lambda\left(y_t - y_t^*\right)^2 + (1-\lambda)\left(\pi_t - \pi^*\right)^2, \lambda \in [0,1],$$

[1] November 1982 remarks, quoted in Klamer (1983, p. 173).

Edward Nelson is an assistant vice president and economist at the Federal Reserve Bank of St. Louis. An earlier version of this paper was presented at the session, "Looking Back: Historical Perspective on Taylor Rules," for the Federal Reserve Bank of Dallas conference, "John Taylor's Contributions to Monetary Theory and Policy," October 12-13, 2007. The author thanks Otmar Issing, Benjamin Keen, Kevin Kliesen, Evan Koenig, and John Taylor for helpful comments on an earlier version of this paper. Faith Weller provided research assistance.

where $y_t - y_t^*$ is the output gap (i.e., the difference between the logs of output and the value that output would take if there were no nominal rigidities), π_t is inflation, and π^* is an inflation target. Taylor subsequently argued that this choice of objective function was an implication of rational expectations models that included nominal rigidities:

> [T]he objective of macroeconomic policy is to reduce the size (or the duration) of the fluctuations of output, employment, and inflation from normal or desired levels…[T]he rational expectations approach is fairly specific about what the objectives of policy should be. Changing the natural or normal levels of output and employment is not the direct objective of stabilization policy…As a first approximation, these normal levels are not influenced by macroeconomic policy…[I]t is important to choose a target [inflation] rate that maximizes economic welfare…[and] to minimize fluctuations around the target… (Taylor, 1986a, pp. 159, 160)

On the other hand, Taylor (1986a, p. 153) conceded that rational expectations models with staggered nominal contracts "need some bolstering of their microeconomic foundations"; he also described the aforementioned stabilization goal as the "assumed goal" (1987, p. 351), not necessarily the model-implied goal. In fact, staggered-contracts models with deeper microfoundations and a model-consistent welfare function do largely support the loss function that Taylor used, as shown by Rotemberg and Woodford (1997).[2] There are, however, two major qualifications:

First, the setting of the output target at the natural output level is not automatically implied by these models. One case that delivers a zero-output-gap target is when the natural level of output corresponds to the efficient level of output. This is essentially what occurs in Rotemberg and Woodford (1997): Though their model contains imperfect goods-market competition and so tends to deliver inefficiently low aggregate output, they assume that a fiscal subsidy raises steady-state output to the efficient level.

Alternatively, the natural level of output may be lower than the social optimum, but the monetary authority might explicitly disown attempts to push output above its natural value. Taylor has consistently advocated this stance, most explicitly in Taylor (1987); it is also the position taken by McCallum (1995), King (1997), and Svensson (1997).[3] Specifically, Taylor has argued that monetary policy analysis should not concern itself with whether the natural level of output is efficient and should instead treat the natural level as the value around which output should be stabilized (Taylor, 1987, p. 351; Hall and Taylor, 1997, p. 478).

The zero-output-gap target is natural to Taylor because it captures the message of the natural rate hypothesis. He has always endorsed this hypothesis in his writings, maintaining (i) that models should reflect and that policymakers should take into account the notion that "the economy tends to return to the natural rate of unemployment" irrespective of monetary policy rule and (ii) that, conformably, "no long-term relationship exists between inflation and the deviation of real GDP from potential GDP."[4] With no scope for policymakers to steer output away from the natural level in the long run, a loss function featuring a zero-output-gap objective better reflects the economic structure. Likewise, Taylor has not been in favor of economic analysis that postulates a policymaker desire to target a positive output gap, either in positive economics or normative applications. This was a major reason why Taylor was one of the earliest to speak out against time-consistency explanations for the Great Inflation, which rely on policymakers having an output target in excess of the natural level of output (see, e.g., Taylor, 1992, pp. 14-15).

---

2 The Rotemberg-Woodford objective function that sums the loss function across periods does differ from the one that Taylor proposed, because Taylor argues for no discounting (see Taylor, 1979, p. 1276, and 1986a, p. 159; and Hall and Taylor, 1997, p. 474); whereas, Rotemberg and Woodford recommend that the welfare function feature discounting (using the representative household's rate of discount).

3 As shown in Woodford (2003), a model with inefficient potential output and no subsidy usually does not admit a quadratic approximation for the welfare function. My conjecture is that in this environment the Taylor (1987) procedure amounts to the following: As far as is possible, rewrite the approximation of the welfare function so that terms in output appear as deviations from potential output; any left-over output terms are then ignored when the policymaker carries out optimization.

4 The quotations regarding unemployment and gross domestic product (GDP) are from Taylor (1987, p. 351) and Taylor (1994, p. 38).

The second qualification is that the presence of wage stickiness means that price-inflation variability is generally not the only inflation term in the social welfare function; wage-inflation variability appears too (Erceg, Henderson, and Levin, 2000). I defer discussion of wage stickiness in Taylor's framework until later in the paper.

Friedman on Policymaker Objectives

A close reading of Friedman's work suggests that he favored a policymaker objective function close to that advanced by Taylor—that is, one penalizing inflation deviations from target and output gap deviations from zero, with no other terms in the objective function. Moreover, he believed that by the early 1980s policymakers had moved to a strategy meant to pursue this objective.

To establish this interpretation of Friedman's position, the first thing to note is that his advocacy of monetary targeting (discussed further below) did not amount to a denial of the position that the principal objective of monetary policy should be price stability. Though believing that real money holdings generate utility (see Friedman, 1969), Friedman did not base his advocacy of monetary targeting on this component of utility; he did not list stability in real money balances as an ultimate objective.[5] Rather, the appropriate welfare function for monetary policy puts highest weight on price stability:

> With respect to ultimate objectives, it's easy to cite the holy trinity that has become standard: full employment, economic growth, and stable prices…What is the special role of monetary policy in contributing to these objectives?...[T]here is today a worldwide consensus, not only among most academic economists but also among monetary practitioners, that the long-run objective of monetary policy must be price stability. (Friedman, 1982a, p. 100)

As would be expected from his work on the natural rate hypothesis (Friedman, 1968), Friedman interpreted the full-employment objective as a stabilization objective—that is, minimizing fluctuations in the output gap. Therefore, the goals of policy should be "a reasonably stable economy in the short run and a reasonably stable price level in the long run" (Friedman, 1959, p. 136).

Friedman acknowledged that the stabilization objective could in principle be pursued jointly with the price-stability objective, in which case one would be "pursuing the long-run policy in a manner that contributes to minimizing economic fluctuation" (Friedman, 1982a, p. 100). He also indicated that he did not disagree with the weights in the objective function used in Keynesian work.[6] Acceptance of such an objective function would imply some allowance, in setting policy, for trade-offs between objectives to the extent that such trade-offs existed. Friedman granted this in principle, subscribing to the view that in public policy there should be "a sane balance among competing objectives" (Friedman, 1979a). Indeed, Friedman's belief in the existence of a short-run output gap/inflation trade-off, considered further below, was one reason for his preference, in a situation starting from high inflation, for a progressive step-down in money growth toward a constant money growth rule. He argued that such a program offered a way of managing the short-run trade-off that was superior to what had been pursued in practice during the Volcker disinflation. The Volcker disinflation, he argued, had brought inflation down too quickly and produced a deeper-than-necessary trough in output.[7] Further details of the arguments underlying Friedman's advocacy of constant money growth can be brought out by considering his and Taylor's positions on monetary policy rules.

MONETARY POLICY RULES

It is tempting to think of Friedman and Taylor as being on opposite ends of the spectrum on the

[5] Friedman specifically disavowed the rule he derived in his 1969 paper as one that monetary policymakers should or did use to conduct policy; so, he was not interested in bringing the level of real balances to the value that satiated households.

[6] "I doubt very much that there is any significant difference between [Modigliani] and me, for example, on the value judgments we attach to unemployment and inflation" (Friedman, 1977a, p. 12).

[7] Friedman and Friedman (1984, pp. 91-92).

issue of monetary policy rules. That may seem a natural conclusion given the rules they came to advocate: Friedman, a constant money growth rule; Taylor, an activist interest rate rule. And, yes, Taylor (1982) went on record with the view that Friedman's constant money growth rule was "extremely undesirable." But focusing on this statement by Taylor, or on a contrast between Taylor's (1993a) rule and Friedman's monetary rule, would lead one to overstate the differences between the two on the issue of policy rules. As we will see, Taylor has often emphasized the links between his recommendations and those of Friedman; in particular, the focus on a non-accommodative and rule-based policy. Taylor also downplayed the distinction between money growth and interest rate rules. Where Friedman and Taylor differ most is in their judgments about the extent to which monetary policy should be based on assumptions about the structural behavior of the economy. This starting point leads naturally to different judgments about the appropriate degree of activist stabilization policy and also about the connection of policy decisions to ultimate policy objectives.

Friedman's Framework

Friedman's money growth rule separates the variables that he believed should appear in the policymaker objective function (inflation and the output gap) from the variable that policy should directly target (money or money growth). The focus on an intermediate variable and on a non-activist rule reflected his opposition, discussed below, to deploying optimal control methods; more generally, it reflected his doubts about the practical success of monetary policy rules that responded to ultimate objectives or rested on structural economic models.

Friedman's opposition to responding to ultimate objectives was based on somewhat distinct rationalizations for the two ultimate objectives, inflation and the output gap.

Inflation. Friedman noted that monetary policy affected inflation with a lag; current inflation was therefore unsuitable as a target, and inappropriate as a variable on which to feed back, because that "would produce a monetary policy that was always fighting the last war."[8] Targeting expected future inflation, on the other hand, would require too much reliance by policymakers on their estimates of structural relationships linking monetary policy actions and inflation (i.e., inflation behavior would be sensitive to the specification of the IS, LM, and Phillips curve relationships); and policy actions could then be destabilizing in practice: hence, Friedman's judgment that responding to prices or inflation implied "a bad rule although a good objective" and his conclusion that a "rule in terms of the quantity of money seems...far superior, for both the short and the long run, than a rule in terms of price-level stabilization."[9]

Nevertheless, Friedman did not regard activist rules that responded to inflation, nominal income, or nominal income growth as nonmonetarist. He noted that an implication of his own research was that "monetary policy is an appropriate and proper tool directed at achieving price stability or a desired rate of price change" (Friedman, 1977a, p. 13). Though targeting nominal variables other than the money stock required too much fine-tuning for Friedman's liking, he saw them as monetarist rules because they shared "the quantity theory emphasis on nominal magnitudes" (Friedman, 1987, p. 18). This way of phrasing matters actually does not adequately reflect the relationship between the quantity theory and policy rules. A more precise way of putting things is that these rules reflect the quantity theory's emphasis on nominal magnitudes as *the variables ultimately determined by the monetary authorities*. Many expositions of the quantity theory, including some of Friedman's, do emphasize real variables, but as variables determined in the long run by factors other than monetary policy.

---

[8] Friedman (1982b). This argument foreshadowed Bernanke et al.'s (1999, p. 298) criticism of "policies that react to inflation only after it has become a problem," although their suggested solution, in contrast to Friedman's proposals, was to concentrate on expected future inflation. As it turns out, policy rules that respond to current inflation typically perform well—i.e., are stabilizing—in simulated New Keynesian models, largely because the forward-looking nature of price setting compensates for the delayed character of the policy response.

[9] The quotations are from Friedman (1982b) and Friedman (1967, p. 4; p. 84 of 1969 reprint).

Output Gap. Friedman's most important basis for excluding real variables from the list of targets was the natural rate hypothesis: Real variables reverted to their natural values in the long run, irrespective of what monetary policy did. This position, however, was not a satisfactory basis for denying monetary policy a stabilization role. In principle, as Friedman (1968) acknowledged, the absence of a long-run influence still left real variables as candidate data on which policy might feed back, provided they appeared in gap form. Gaps would likely provide information about inflation; moreover, the stabilization of gaps was itself a desirable goal.

But Friedman came out against policy responses to unemployment or output even when these were expressed as deviations from natural values; instead, he argued, full employment should not be sought "directly" by monetary policy (Friedman, 1982a, p. 100). First, the lack of knowledge required for fine-tuning again produced the danger of policy being destabilizing in practice. Second, targeting a gap variable required estimation of the unobserved natural rates of interest, output, or unemployment: In principle, this was subject to bias because "it is almost impossible to define full employment in a way that is logically precise"; in practice, it had resulted in "unduly ambitious targets of full employment."[10] Stabilization policy intended to promote a zero output gap had thus led to unintended targeting of positive gaps, making inflation worse. Money growth targeting protected monetary policy from problems associated with responding to gaps.

There are clear links between these positions and the work of Orphanides (2003) on the danger of relying on real-time measures of the output gap when formulating policy. Orphanides himself motivates his work with statements from Friedman going back to the 1940s. Orphanides also notes that Friedman's money rule is in terms of growth rates; it is based on data that tend not to have the large serially correlated revisions associated with levels of series. Friedman's money growth rule was also insensitive to data revisions for a more subtle reason: Although Friedman generally advocated an M2-type aggregate as the monetary target, he stressed that an important advantage of the rule used to hit the M2 target is that the implied open market operation could be announced ahead of time (see especially Friedman, 1982a). That is, Friedman's M2 growth rule is less usefully thought of as a targeting rule (as in Svensson, 2005) than as an operational instrument rule (as in McCallum and Nelson, 2005). Accordingly, information obtained in subsequent periods would not lead to a different retrospective prescription from the rule, even if such information would have secured more precision in hitting the M2 target. Data revisions would fall into this category. Strictly speaking, therefore, Friedman's money growth rule prescription is not subject to a real-time/final data distinction.

Friedman (1960, pp. 23, 98) freely acknowledged that a constant money growth rule did not correspond to optimal monetary policy. Rather, he offered it as a way of preventing both the policy mistakes that could result from activist monetary policy in the presence of imperfect knowledge and repetition of the historical policy mistakes that had been associated with large variations in the money stock. The latter consideration comes out in Friedman's statement that "the major argument for the rule has always seemed to me to be far less that it would moderate minor cyclical fluctuations than that it would render impossible the major mistakes in monetary policy that have from time to time had such devastating effects."[11]

Monetary Policy Rules in Taylor's Framework

Taylor saw rational expectations as changing the monetary policy debate from being about "rules versus discretion" to being about the choice of monetary policy rule:

> [M]acroeconomic policy should be stipulated and evaluated as a rule, rather than as one-time changes in the policy instruments…There is a big distinction between "discretionary" and

[10] The quotations are from Friedman (1963; p. 40 of 1968 reprint) and Friedman and Friedman (1980, p. 311).

[11] Friedman (1966a; 1969 reprint, p. 154).

> "activist" policies…Activist and constant-growth-rate policy rules have much more in common with each other than do activist policy rules and discretionary policy. (Taylor, 1986a, pp. 155, 157)

Taylor therefore was not inclined to see the constant money growth rule as being in a different spirit from the feedback rules, nor did he always stress a contrast between interest rate and money growth rules. He looked on arrangements that used money as the instrument as implying a particular form of the interest rate rule, and he wrote favorably of aspects of a constant money growth rule in that light: Fixed money growth implied "an automatic increase in the interest rate" when aggregate demand rises, and this was one of the rule's "stabilizing properties" (Taylor, 1999a, pp. 64-65). Confirming these stabilizing properties of a constant money growth rule, Taylor (1979, p. 1282) found in simulations that the rule produced a lower output gap variance than did the historical postwar U.S. policy rule.

But the fact is that Taylor was never a supporter of a constant money growth rule, coming up with an alternative rule in his published research in 1979 (Taylor, 1979) and strongly rejecting constant money growth as a desirable policy option in a Congressional submission in 1982 (Taylor, 1982). His own proposed activist rules have evolved from optimal-control-based rules in the 1970s, to simple policy rules for money in the early and mid-1980s, to his advocacy of interest rate rules today. The constant theme has been rule-based policymaking with feedbacks but with lack of accommodation of inflation.

Optimal Control

An initial source of disagreement between Friedman and Taylor in the 1970s was the value of optimal control in monetary policy analysis. The disagreement is made clear by simply juxtaposing the title of Taylor's (1979) paper "Estimation and Control of a Macroeconomic Model with Rational Expectations" against Friedman's (1973a, p. 9) statement that "control theory…requires delicate fine-tuning for which the Fed has neither the knowledge nor the demonstrated capacity."

This did not become, however, the area of durable disagreement between Taylor and Friedman on rules. By the early 1980s, Taylor was deemphasizing optimal control in favor of simple policy rules (see, for example, Taylor, 1981a). He stressed that optimal control was complex and model-specific (Taylor, 1986a, p. 162) and at this point what he emphasized instead was "a simpler rule," relying on few arguments, which might be a good approximation of optimal policy in Taylor's model but by implication was less model-sensitive.

Taylor's Move to Simple Rules

These early rules had the money supply as the policy instrument. Taylor (2007, p. 195) has described the money supply rule inspired by his 1979 analysis as "effectively a 'Taylor rule,' though for the money supply." Experiments in Taylor (1981a) intended to determine the best simple-rule approximation to the optimal rule of 1979 actually reached a rule with somewhat different arguments from those in the Taylor rule. Instead of responding to inflation and the output gap, the simple rule for money growth had no inflation term, with responses to the output gap and the change in the output gap. But the absence of inflation from the money growth rule is not a source of material difference from the Taylor rule. A zero response of money growth to inflation implies policymaker non-acquiescence to the existing inflation rate, while anything short of a larger-than-unit response of an interest rate rule to inflation will (other things equal) tend to perpetuate the existing inflation rate, or worse. The simplified 1979 money rule therefore is qualitatively similar to the Taylor rule in that it is nonaccommodative and both rules encapsulate Taylor's (1986a, p. 162) position that "monetary policy has a stabilization role but no accommodation role." The nonzero response to the change in the output gap (a speed-limit response) is a material difference between the simplified 1979 rule and the Taylor rule. But estimates of interest rate rules inspired by the Taylor rule sometimes allow for a speed-limit response by including more than one lag of the output gap (or of detrended output) as regressors (see, for example, Rotemberg and Woodford, 1997).

Taylor was unequivocal on the point that his proposed feedback rules were preferable to Friedman's money growth rule: "[A] specific activist rule would work better than a monetarist rule" (Taylor, 1986a, p. 162). He recognized, as many had not, that Friedman had never claimed that a money growth rule was optimal. The claim that the money growth rule could not be beaten was a product of the flexible-price rational expectations literature, not a contention of Friedman's. Indeed, Taylor offered one of Friedman's most clear-cut statements on the issue: "A believer in monetarist theory still can favor an activist monetary policy as a way to offset other changes in the economy."[12] Taylor understood that Friedman's case for a money growth rule rested instead on a model-uncertainty argument. But Taylor disagreed with Friedman on the quantitative importance of this issue and rejected model uncertainty as the basis for refraining from activist rules. Taylor's definitions of policy rules tended to presume an activist rule, as in his reference to policy rules as "the way the policymakers respond to events" (Taylor, 1986b, p. 2039).

In discussing Taylor's position on activism, I find it useful to separate the discussion into two issues: allowing the money supply to respond to money demand shocks; and then, more generally, systematic monetary policy responses to other economic shocks.

Money Demand Shocks. An area of direct disagreement between Friedman and Taylor was whether the monetary policy rule should attempt to accommodate money demand shocks. Friedman argued that there was too much uncertainty about money demand to make accommodation desirable:

> In principle, if we knew about autonomous changes in the real demand for money, it would be right to adjust the nominal supply to them. However, we don't know about them. (Friedman, 1973b, p. 31)

> [W]hat you really have to demonstrate is that, over time, you will in fact know enough about such changes and will be able to identify them soon enough, so that you can make adjustments which, on the average, will do more good than harm. (Friedman, 1977a, p. 26)

There is considerable substance to this reservation on Friedman's part. How much accommodation is needed to insulate the economy from money demand shocks is not a question that can be put on autopilot. For example, using changes in velocity to gauge the required amount of monetary accommodation is not without problems. Because velocity is defined residually as the ratio of nominal GDP to money, a velocity movement might reflect not a permanent money demand shift, but instead a faster response of money than of nominal income to a shock that will ultimately move income by the same amount as money. Of course, holding the nominal interest rate constant in the face of a money demand shock will mean that the shock is accommodated one-for-one, but it will also mean that other shocks that create pressure on interest rates will be accommodated. Thus Friedman feared that a scheme other than constant money growth would provoke monetary responses to "all sorts of changes that…should not be accommodated" (Friedman, 1977a, p. 18).

This criticism applies more fundamentally to interest rate pegging than to an appropriately formulated, non-accommodative, interest rate rule; it would not usually apply to the Taylor rule, for example. In fact, while Friedman was a notable critic of using the short-term nominal interest rate as a policy instrument, his two main objections were not generic criticisms of interest rate rules, but instead highlighted two particularly weak types of rule: pure rate pegging and rules that did not take into account the nominal rate/real rate distinction. It is true that, in the 1960s and 1970s, examples of successful interest rate rules were hard to find, so that one was more entitled to the presumption that movement to a base money rule was in practice necessary for delivering the requisite anti-inflationary movements of interest rates. When considering the choice between instrument rules that were more competitive with one another—that is, money base instrument rules

[12] Friedman (1984, p. 3), quoted in Taylor (1986a, p. 153).

versus nominal interest rate rules that incorporated vigorous responses to nominal variables—Friedman continued to be in favor of base rules, but admitted that it was a tactical, not a strategic, issue.

Taylor, by contrast, has been consistently more optimistic than Friedman on the scope for monetary policy to offset money demand shocks. Taylor (1982) observed the following:

> In my view, however, it is possible for the monetary authorities to discover shifts in money demand and to react to them with a relatively short lag. Such shifts should be accommodated by changing the supply of money.

Other Shocks. Taylor (1992, p. 29) observed that "good policy is characterized by systematic responses to economic shocks." Identifying economic shocks such as disturbances to the Phillips curve, production function, and preferences and determining the stabilizing policy reaction are surely even more model-dependent exercises than in the case of money demand shocks. Accordingly, Taylor firmly associated himself with using structural models, both in policy analysis and policy formulation. He judged that the appropriate response to the Lucas critique was to use models whose parameters (including parameters governing nominal rigidity) could be legitimately treated as structural and not as functions of the policy regime.[13] Monetary policy rules could then be coherently analyzed with these models. Moreover, he stressed that structural models should be used in policymaking: "[P]olicy actions should be based on structural relationships" and "structural models…might be useful for formulating policy."[14]

The position that policymakers should use structural models is also implied by Taylor's advocacy of monetary policy rules that include a response to the output gap. Taylor (1999a, p. 63) acknowledged "a large degree of uncertainty about measuring potential GDP (and, thus, the output gap)." But he argued that the appropriate policy reaction to this uncertainty was to use a simple policy rule with a reduced, but still positive, response to the output gap (Taylor, 1999a, pp. 63-64). He has encountered this issue both in his policy and research work. While at the Council of Economic Advisers in 1976-77, Taylor was involved in a major downward revision of potential output that was published in the 1977 *Economic Report of the President*. Furthermore, in his early work Taylor used output gap estimates that implied an average postwar gap for the United States of –1.9 percent (Taylor, 1979, p. 1282); afterward he used more economic structure when estimating the gap, by imposing the natural-rate-hypothesis condition that the gap be zero on average in postwar data (see, for example, Taylor, 1986c, p. 641).

Taylor also acknowledged that the Taylor rule requires an estimate (for the intercept term in the rule) of the steady-state equilibrium real interest rate, but he has rejected this problem as a justification for turning to money growth rules. Instead, he has argued that the way to overcome policy errors that might result from a biased equilibrium-rate estimate is to increase the response to inflation in the interest rate rule (Taylor, 1994, p. 26).

Even in 1982 during the new-operating-procedures period, Taylor thought of the Fed as operating on interest rates.[15] So, whereas his early work used money growth rules, Taylor was probably more accustomed than most U.S. monetary economists at the time to viewing monetary policy in terms of an interest rate rule. By 1992 he had concluded that the monetary policy literature would now focus on rules "probably with the interest rate as the instrument" (Taylor, 1992, p. 15). Because even his proposals for money supply rules involved accommodation of money demand shocks (and other sources of permanent velocity movement), Taylor did not see a dramatic normative contrast between money stock rules

[13] See Taylor (1986a, p. 156; 1986b, p. 2038).

[14] The quotations are from Taylor (1981b, p. 81) and Taylor (1993b, p. 5).

[15] "[T]he Fed would have to contract demand by increasing interest rates" (Taylor, 1982). Also, Taylor (1981b, p. 78) thought of stabilization policy in terms of policy influence on real interest rates on securities and noted the zero lower bound on nominal interest rates as an obstacle to achieving the required real interest rate movement.

and interest rate rules and he emphasized the mapping between money growth rules and interest rate rules.[16] But a focus on interest rate rules made it easier to compare proposed rules with reaction functions used in historical and present-day monetary policy.

Friedman's Later Views on Rules

It would be inaccurate to say that Friedman ever stopped favoring fixed money growth as his first preference for a monetary policy rule. But his criticisms of alternative rules did diminish in the 1990s. He acknowledged that understanding of the economy had improved since the 1960s and that he had been surprised at the success with which this knowledge had been translated into successful stabilization policy by policymakers since the mid-1980s. Moreover, financial changes had unambiguously made money harder to define, reflected in the increased tendency for alternative monetary aggregates to give different signals; in that environment, money growth targeting did not imply stepping away from activism, given the increased difficulty of settling on the right concept of money and hitting the target. He still saw value in a money growth rule as a constraint on policymaker discretion. And Japan's experience in the 1990s served as Friedman's trump card in support of his older arguments, suggesting to him that a money growth rule might still be preferable to an interest rate reaction function based on ultimate objectives.

SOURCES OF NOMINAL RIGIDITY

Nominal rigidity plays a central role in both Friedman's and Taylor's views of the transmission mechanism. They each contributed theoretical breakthroughs related to nominal rigidity: the natural rate hypothesis in Friedman's case; staggered contracts in Taylor's. As I will discuss, both of them emphasized simultaneous wage and price stickiness. At the same time, I believe that their views of the transmission mechanism are actually better represented by a model in which there is little wage stickiness and that their views on the social welfare function are to some extent inconsistent with the existence of substantial wage stickiness.

Friedman on Nominal Rigidity

Turning to Friedman first, I have occasionally seen interpretations of his view of the transmission mechanism that characterize him as making an implicit assumption of both price and wage flexibility—so that the effect of monetary policy on output comes only from imperfect information.[17] But in fact such a vision is not implicit in his view of the economy, and the explicit record of Friedman's writings shows repeated stress on the role of nominal rigidity. Taylor (1999c) recognized this by opening his article on nominal rigidities with a capsule Friedman quotation from 1982: "Prices are sticky."[18] Indeed, as early as 1967, Friedman described himself as "in full agreement" with the view that it "is the rigidity of prices that converts fluctuations in aggregate demand into fluctuations in output and employment."[19] He made specific reference to "wage and price contracts" in one of his earliest expositions of the vertical Phillips curve idea (Friedman, 1966b).

Taking for granted therefore that Friedman had in mind a long-run vertical expectational Phillips curve based on nominal rigidity, what is the most appropriate way of formalizing his views further? I find it useful to break the discussion of Friedman's price adjustment ideas into several considerations: whether the expectations term is formed rationally, whether prices are a "jump" or predetermined variable, and the date of the expectation in the Phillips curve (i.e., whether it refers to inflation in period $t + 1$ or t and whether this expectation is based on an information set from period t or period $t-1$). I defer until my

[16] See, for example, Taylor (1998, pp. 5-6; 1999b).

[17] See the Nobel Committee (2006) for a recent discussion in which this position is attributed to Friedman.

[18] Friedman (1982c). In addition to opening Taylor's chapter (1999c), this quotation also appears in Hall and Taylor (1997, p. 235).

[19] Friedman (1967, pp. 2, 6; pp. 82, 86 of 1969 reprint). The rigidity Friedman endorsed as relevant was temporary nominal rigidity, rather than the permanent nominal rigidity which he associated with early Keynesianism.

discussion of Taylor the issue of whether nominal rigidity pertains to wages or prices in Friedman's framework.

Forward-Looking Behavior

Though he is often associated with adaptive expectations and with accelerationist versions of the natural rate hypothesis, Friedman does not appear to have been opposed to rational expectations in principle. He accepted that it was "most unreasonable" to use adaptive expectations when this involves extrapolating from a different regime (Friedman and Schwartz, 1982, p. 569) and suggested that rational expectations models were acceptable, provided they got away from the implication of serially uncorrelated effects of monetary policy on output (Friedman, 1977a, p. 14). He spoke out in favor of rational expectations models with long-term nominal contracts and defended these models against critics of rational expectations (Friedman and Schwartz, 1982, p. 415).

The above elements suggest that a forward-looking Phillips curve represents Friedman's views well. He did suggest (see Friedman, 1974a) that commodity price shocks could stimulate inflationary expectations, a suggestion that might imply the presence of some price indexation and a lagged inflation term in the Phillips curve. But there is strong evidence that he did not believe in full indexation: The aforementioned effect of commodity price shocks on expectations was described as temporary, and Friedman emphasized the need for reforms to make indexation more widespread and so reduce relative price distortions (see Friedman, 1974b).[20]

Prices: Jump or Predetermined

Friedman (1979b) noted the existence of

> contractual arrangements that fix prices and wages in advance for some time. Even when prices and wages are not fixed explicitly, it is often undesirable to change them frequently. As a result, output and employment are generally more flexible over short periods than prices and wages, though less flexible over long periods.

While recognizing here the existence of long-term price contracts, Friedman nevertheless believed that a portion of the aggregate price index is a jump variable. It is clear from his expositions on the vertical Phillips curve (e.g., in Friedman, 1966b, 1968) that he saw some prices as able to increase immediately when nominal aggregate demand rises. Therefore, the price level is a jump variable notwithstanding the presence of a predetermined subset of prices. As Friedman (1974b, p. 30) put it, "Some prices…are fixed a long time in advance; others can be adjusted promptly."

The coexistence of some predetermined prices and some "jump" prices makes Friedman's framework compatible with a Calvo (1983) or Taylor price contract scheme, but not with Rotemberg (1982) price setting.

Reference Date for Expectations

Does the expected-inflation term in Friedman's Phillips curve refer to inflation in period t or period $t + 1$? And when are these expectations formed? Traditionally, the expected-inflation term in Friedman's Phillips curve is interpreted as being lagged expectations of current inflation: that is, $E_{t-1}\pi_t$. Certainly the $t-1$ date on expectations formation is justified: Friedman (1974b, p. 30) said, "It will take still more time before expectations about inflation are revised"; that is, expectations of π are inertial relative to π itself.

In some of Friedman's discussions, it is implied that the inflationary expectations that matter for period-t inflation are forward-looking—that is, they pertain to expectations of policy beyond period t. For example, Friedman (1966b) said that prices are "set in the light of anticipations of inflation." Friedman (1972) argued that business decisions depend on confidence in future monetary policy and that a preannounced policy of steady money growth was more stabilizing than a discretionary policy that ex post delivered the same degree of steadiness in money growth. And Friedman and Friedman (1980, p.

[20] In this respect, Friedman anticipated the cost of inflation that is emphasized in the New Keynesian literature. See Nelson and Schwartz (2008) for further discussion.

326) observed that inflation expectations depend on signals about future policy. So Friedman's framework is compatible with $E_{t-1}\pi_{t+1}$ rather than $E_{t-1}\pi_t$ in the Phillips curve.

Summing up, Friedman's Phillips curve views seem to be in line with the Christiano, Eichenbaum, and Evans (2005) generalization of Calvo contracts, as expressed in output gap space by Giannoni and Woodford (2005):

(2) $$\left(\pi_t - \gamma\pi_{t-1}\right) = \left(E_{t-1}\pi_{t+1} - \gamma\pi_t\right) + \alpha\left(y_t - y_t^*\right) + u_t,\ \alpha > 0.$$

Relative to Giannoni-Woodford, equation (2) has been modified by (i) imposing a vertical-Phillips-curve restriction (i.e., a unit weight on expected inflation), following Roberts (1995); and (ii) allowing some response by a portion of firms to current information by making the output gap appear in realized rather than expected form. In both cases, the modifications are designed to make the specification better reflect Friedman's views. For reasons discussed above, the indexation coefficient γ is likely nonzero, but low, in Friedman's framework. I will discuss the cost-push shock term u_t, familiar from Clarida, Galí, and Gertler (1999) as an addition to the New Keynesian Phillips curve, when I turn to sources of trade-offs.

Taylor on Nominal Rigidity

Taylor argued in 1982 (in Klamer, 1983, p. 174), "I do not think that you can accurately model macroeconomic behavior assuming that prices are perfectly flexible." That view has underpinned Taylor's emphasis on contracting models. It is also implied by Taylor's emphasis, since the 1970s, on the output gap concept and on stabilization of the output gap as a goal to be pursued through monetary policy rules. This approach distinguished him from many earlier users of rational expectations models. In most of these early models, the flexible-wage/flexible-price assumption meant that the gap was identically zero or, at best, a white noise process incapable of being influenced by activist, predictable monetary policy actions.

Taylor has proposed a very specific Phillips curve, based on staggered contracts. Neglecting shock terms for the moment, we see in the two-period-contract case that this is built up from a "basic…contract determination equation" for the log contract,

$$z_t = 0.5\ z_{t-1} + 0.5\ E_t z_{t+1} + \xi\left[\left(y_t - y_t^*\right) + E_t\left(y_{t+1} - y_{t+1}^*\right)\right],\ \xi > 0,$$

and an "aggregate price level" definition describing log prices, $p_t = 0.5z_t + 0.5z_{t-1}$—the latter definition presuming a constant markup (Taylor, 1981b, p. 72). After some further approximations (see Roberts, 1995),[21] the result is a version of the New Keynesian Phillips curve:

(3) $$\pi_t = E_t\pi_{t+1} + \alpha\left(y_t - y_t^*\right) + u_t.$$

Taylor contracts imply a mixed backward-looking/forward-looking price level and a strictly forward-looking inflation rate. The absence of an indexation term from equation (3) reflects Taylor's view that the dynamics of this equation should be relied on to deliver inflation persistence (see Hall and Taylor, 1997, p. 441) and that this is preferable to appealing to intrinsic inflation persistence as in Christiano, Eichenbaum, and Evans (2005) or Fuhrer and Moore (1995). The equation also reflects Taylor's belief, in contrast to Friedman's, that the expected-inflation term in the Phillips curve is formed using period-t information (see Taylor, 1986a, p. 158).

Wage Versus Price Stickiness

Despite the explicitness of Taylor's specification and its nominal-contracts motivation, there is an important ambiguity common to the discussion of nominal rigidity in both Taylor's and Friedman's work. They both tended to refer to both price and wage stickiness and to play down

[21] These approximations involve suppressing some endogenous expectational errors that appear in the linearized Phillips curve. Because these endogenous terms are responsible for some of the effects of monetary policy in Taylor-contracts models, some authors have argued that the approximations are not innocuous—see, e.g., Westaway (1997) and Musy (2006)—and that the New Keynesian Phillips curve should not be used to represent Taylor staggered contracts.

the distinction between the two. Occasionally, they would highlight wages as being subject to contracts to a greater degree than prices (see Friedman, 1966b; Taylor, 1982). But I will argue that the staggered-contracts specification that best describes their views about policy and economic structure refers to the gradual adjustment of prices, not wages.

Taylor (1986a, p. 153) was an economist who early on accepted the label of "New Keynesian"[22]; in the 1980s, New Keynesian economics was sometimes characterized as entailing a shift from sticky-wage models to sticky-price models (see, e.g., Mankiw, 1987). There have been occasions where Taylor has himself given the appearance of moving from a framework based on sticky wages to one based on sticky prices. For example, Taylor (1981b, p. 72) gave a price-contract interpretation of his work, explicitly replacing, in that application, an interpretation of the Phillips curve based on nominal wage contracts. Similarly, Taylor (1992, p. 22) said, "The structural interpretation I have favored involves a macroeconomic model with sticky prices and rational expectations…" More recently, Taylor (2000a, p. 1401) again explicitly reinterpreted his model as "referring directly to prices," taking firms as having staggered price contracts, and abstracting from labor market frictions. And Hall and Taylor (1997, p. 432) cited sticky prices as important, noting that "firms…find it convenient to stay with existing prices."

But evidently these exercises did not signify a fundamental change in Taylor's position, for he has never disowned the importance of wage stickiness. His belief in the importance of wage stickiness resurfaced in his recent remark, "If I had to give a list of criticisms of the recent work, it would start with the frequent abstraction from wage rigidities" (Taylor, 2007, p. 198).

Nevertheless, the move between sticky-wage and sticky-price assumptions in Taylor's work, as well as his remark in Taylor (1981b, p. 72) that his setup was "general enough" to be interpreted as referring to either wages or prices, does suggest something else to me.

What these elements suggest is that Friedman and Taylor believed that wage stickiness was largely manifested in—or was a motivation for—price stickiness. Accordingly, in both Friedman's and Taylor's work, there was a single Phillips curve in which price inflation and the output gap were the only endogenous variables. Taylor (1980, pp. 5-6), for example, moved from wages to prices by way of a constant markup and worked with a price-inflation Phillips curve. As users of dynamic general equilibrium models have shown, this Phillips curve can be rigorously derived from sticky-price models, not sticky-wage/flexible-price models (see, e.g., Chari, Kehoe, and McGrattan, 2000, and Erceg, Henderson, and Levin, 2000).

Another reason why a sticky-price rather than sticky-wage assumption is closer to Taylor's framework is that, from the beginning, Taylor made goods-price inflation the variable that policymakers care about. Both Friedman and Taylor, as we have seen, treated the social welfare function as containing only price inflation variability and output gap variability arguments. But nominal wage inflation variability becomes a third argument of the welfare function if wages are sticky (see Erceg, Henderson, and Levin, 2000).[23] In fact, I do not think that either Friedman or Taylor failed to recognize that wage stickiness in principle made wage stabilization a desirable objective. In discussing the views of Henry Simons, Friedman observed Simons's belief

> that the sticky and inflexible prices were factor prices, especially wages…[Aggregate] stability in these prices…would minimize the necessity for changes in the sticky prices. (Friedman 1967, footnote 11)

This passage is notable for showing Friedman's recognition of the idea that the location of nominal stickiness bears on what is the

[22] Taylor (1981a, p. 146) noted, however, that his modeling choices and his emphasis on rules "a few years ago…[would] seem monetarist from the start," an observation which sheds light on the connections between New Keynesian economics and monetarism.

[23] It may be, as Schmitt-Grohé and Uribe (2006) argue, that rules that respond only to price inflation still perform well when wage inflation variability matters for welfare; but the issue that concerns me here is instead how to rationalize Taylor's exclusion of wage inflation fluctuations from the policymaker objective function.

appropriate price index to target; if wages are sticky, the wage index should be stabilized. The fact that he and Taylor nevertheless focused on price inflation as a final objective could be taken as implying that goods-price stickiness is the economy's main nominal distortion.

It is true that Hall and Taylor (1997, pp. 433-43) stress the empirical relevance of wage staggering. But they also place emphasis on the notion that wages in period *t* are set before the realization of the period-*t* price level. Predetermined wages, and in particular the idea that wage contracts are conditional on lagged expectations of the price level, are also an important element of Friedman's (1968, 1976) analysis. So I would suggest that, although prices are sticky in both Friedman's and Taylor's frameworks, the only essential assumption about the labor market is that wages are predetermined, not that they are staggered. Wage behavior therefore might be adequately represented by one-period Fischer (1977) contracts rather than by a dynamic Phillips curve.

It is true that wage stickiness provides a rationale for a disturbance term such as u_t in equation (2) or (3) (see Erceg, Henderson, and Levin, 2000). It has therefore been argued that wage stickiness delivers a trade-off between inflation variability and output-gap variability that is absent from the sticky-price baseline. But other rationalizations are available for the u_t term that do not rely on wage stickiness. Let us therefore consider the issue of the source of policy trade-offs in Friedman's and Taylor's analyses.

SOURCE OF TRADE-OFFS

Taylor (1986d, p. 673) made this observation:

> [A]s I showed in a 1979 *Econometrica* paper [Taylor, 1979], the shocks to the price adjustment equation are what cause the tradeoff between output and inflation variance: attempts to stabilize inflation sometimes require increased fluctuations in output, a factor…that I think is a major factor in the business cycle.

The Phillips curve or price adjustment equation in Taylor's framework therefore contained a shock term, for which Taylor (1981b, p. 79) offered the terminology "cost-push or supply shocks" or "contract shocks." Of these labels, "supply shocks" is less attractive because it has connotations of shocks to potential output; the shocks in question, however, are not potential GDP shocks but instead shocks to inflation that occur for a given path of the output gap (i.e., given the path of output relative to its flexible-price value).

As Taylor observes in the preceding quotation, the cost-push shock rationalizes an output-gap variance/inflation variance trade-off. It is this trade-off that Taylor has emphasized as the durable trade-off implied by Phillips curves that incorporate the natural rate hypothesis and so imply no long-run output gap/inflation level relationship. The cost-push shock therefore also underpins the "Taylor curve," depicting the menu of output-gap variance/inflation variance combinations arising under optimal monetary policy for various weights in the policymaker objective function (see Taylor, 1979). But, as discussed below, the existence of cost-push shocks is also implicit in *Friedman's* framework, though considerable digging is required to ascertain his views on the issue. Moreover, the treatment of cost-push shocks is symmetric across Taylor and Friedman's writings. In both their frameworks, cost-push shocks are white noise and only monetary accommodation of these shocks can propagate them (as sources of inflation movement) beyond their initial impact effect. It is Taylor's contention that monetary authorities, historically, *have* accommodated these shocks in the course of trading off output-gap and inflation stabilization.

Friedman (1980) acknowledged the existence of cost-push shocks: There is a "basic inflation rate" from which actual inflation can deviate as a result of "transitory shocks." That such shocks included cost-push shocks, and not just transitory shocks to the components of the output gap, is implied by Friedman and Friedman's (1984, p. 84) observation that "a sudden upward jump in the price of a product that is widely used…may temporarily raise the level of inflation."

Cost-push shocks therefore exist in Friedman's framework, but are white noise. The transitory character of the shocks is why he classified them as "sources of temporary blips of inflation" (Friedman, 1977b)—or, equivalently, as sources of once-and-for-all movements in the price level. In an exposition of his monetary explanation for inflation, Friedman noted that "[m]any phenomena can produce temporary fluctuations in the rate of inflation" for given money growth (Friedman, 1987, p. 17), thereby allowing for cost-push shocks; but he emphasized that only monetary accommodation can make them relevant for ongoing inflation. The existence of cost-push shocks is also implied by Friedman's (1987, p. 18) recognition of "often conflicting objectives of policymakers"; an expectational Phillips curve does not in itself usually imply conflicting objectives, but does so in the presence of cost-push shocks. Similarly, Friedman (2006) acknowledged the existence in principle of an inflation variance/output-gap variance trade-off of the type that Taylor uses in his work.

Taylor (1993a, p. 196) himself observed that quarterly inflation movements can reflect "blips in the price level due to factors such as temporary changes in commodity prices."

He had earlier judged these blips as reflecting "changes in relative supplies and demands for commodities [which] can cause a price index to move erratically" (Taylor, 1982). These fluctuations rationalize a cost-push shock because not all the sources of the erratic price movements can be summarized by an index of the output gap; for example, increases in a national sales tax "create a price shock" (Hall and Taylor, 1997, p. 497). The characterization of these shocks as erratic blips reflects Taylor's view of them as volatile but not persistent. Accordingly, Taylor (1981b, p. 79) suggested that the cost-push shocks have an "impulse effect" on inflation but that "monetary policy is crucial for the propagation effect." Taylor is therefore in agreement with Friedman that cost-push shocks are a white noise process with no automatic tendency to produce persistent movements in inflation. In line with this position, Hall and Taylor (1997, pp. 231, 441) use the label "price shocks"—rather than inflation shocks—for cost-push shocks; they emphasize that it is the extent to which monetary policy is predicted to accommodate these shocks that determines whether "inflation may be expected in the future" in the wake of a price shock.[24]

The plausibility of the white-noise characterization of the shock depends, of course, on the shock's precise rationalization. In the preceding discussion, I took the potential output concept underlying the output gap definition as inclusive of inefficient variations in potential GDP, as in Friedman (1968) and Taylor (1987). Leaving them out of the output-gap definition would put them into the Phillips curve disturbance. (See Giannoni and Woodford, 2005.) Also, if the shock is to contracts (as in Taylor, 1981b) rather than to the aggregate price level equation, this tends to imply a moving-average Phillips curve shock due to staggering of contracts. In line with this alternative, the Phillips curve shock is treated as MA(1) in some of Taylor's work. But on the whole there is a strong presumption in Friedman's and Taylor's work that the Phillips curve shock will be close to white noise.

SOURCES OF SHOCKS

Other than the white-noise Phillips curve shock, what other types of shocks did Taylor and Friedman emphasize?

Taylor has relayed a complex but consistent picture of the U.S. business cycle, which can be summarized as follows: (i) Monetary policy shocks—in the sense of exogenous, univariate shocks to the monetary policy rule—have not been an important source of U.S. business cycle fluctuations in the postwar period. (ii) Although real shocks, in addition to the Phillips curve shock discussed above, have been an important contributor to fluctuations, pre-1984 business cycle fluctuations did not reflect variations in potential output in response to real shocks. Instead,

[24] A white-noise interpretation of Phillips curve shocks is also consistent with Bernanke et al.'s (1999, p. 59) observation that "prior price-level rises" do not rule out the possibility that "inflation expectations remain contained." In a New Keynesian Phillips curve environment, the insensitivity of inflation expectations ($E_t\pi_{t+1}$) to price-level shocks that affect π_t is implied by the fact that those shocks are white noise (assuming no accommodation, and therefore unchanged expectations of the output gap).

they reflected inefficient monetary policy interacting with price stickiness. (iii) Potential output does not typically vary much in response to real shocks; so, if prices were flexible and/or monetary policy were efficient, real shocks would not lead to large output fluctuations. (iv) Smooth output in the era of the Great Moderation reflects efficient monetary policy, not a reduction in the variance of real shocks.

A denial of an important role for monetary policy shocks and a stress instead on the systematic component of the monetary policy rule as an important source of fluctuations were laid out by Taylor (1982):

> [I]n the last 15 to 20 years in the United States…instability has originated in supply shocks, such as the OPEC price increases. Monetary policy has influenced how these supply shocks have affected the economy…

He went on to argue that price stickiness magnified output fluctuations in the United States over the period 1952-83 (Taylor, 1986c), implying that output is more variable than potential output. Indeed, Taylor has frequently modeled potential output using a smooth trend, which suggests that he does not believe that real shocks would produce much output variability under price flexibility (see, for example, Taylor, 1986c and 1994).[25] Rather, monetary policy reaction to the shocks in the postwar decades produced cycles in output and opened up the output gap, in turn leading to movements in inflation and to a later policy reaction. Taylor (1987, p. 355) went so far as to say this:

> It is not much of an exaggeration to say that all the significant fluctuations in the macroeconomy during the last thirty years have been due to these relationships between output and inflation.

Although this may seem an extreme statement, it is much the same conclusion as that stated by Bernanke et al. (1999, p. 298). It also underlines the fact that attributing output instability to real shocks, as Taylor does, is not the same thing as endorsing a real business cycle account of cyclical fluctuations; on the contrary, Taylor's is a monetary view of the business cycle based on the scope for monetary policy (interacting with price stickiness) to magnify the effects of real shocks on output.

Monetary Policy Rules and the Great Moderation

Given his belief that nominal rigidities magnified U.S. output fluctuations in 1952-83 and in the existence of a cost-push shock in the Phillips curve, and assuming constant parameters in all relevant structural equations,[26] the source of the Great Moderation after 1983 is limited in Taylor's framework to the following:

- reduction in the variance of monetary policy shocks,
- reduction in the variance of Phillips curve shocks,
- reduction in the variance of preference and production shocks, and
- more efficient monetary policy, reducing the upward effect of nominal rigidity on the variance of output.

The first three candidate explanations above are not ones that Taylor favors. As noted above, Taylor (1982) ruled out monetary policy shocks as important in postwar data up to 1982, so any reduction in their variance cannot be important in explaining post-1982 economic stability. An explanation based on a reduction in Phillips curve shock variance is ruled out by his confidence in a reasonably stable inflation variance/output-gap variance trade-off (see Taylor, 1994, and 1999a,

[25] Likewise, Hall and Taylor (1997, p. 408) *define* "business cycle fluctuations" as "the percentage deviation of real GDP from potential GDP."

[26] Taylor (2005, p. 274) expresses doubt about the importance of structural change for understanding changes in U.S. business cycle behavior. Of course, the natural rate of unemployment has likely fallen in many countries, but this does not necessarily imply a structural change in aggregate output behavior. The relationship between production and employment (i.e., the Okun's law relationship) might change at the same time that the natural unemployment rate changes, in a way that cancels out implications for potential output.

p. 60). Taylor (2000b) casts doubt on the likelihood that the variances of real shocks have subsided, concluding that "on balance it seems hard to make the case that exogenous shocks have gotten smaller, less frequent, or more benign."[27]

Logically, therefore, we come to Taylor's explanation for the Great Moderation: monetary policy. Hall and Taylor (1997, p. 429) referred to "the stability of monetary policy in the United States and other major economies from 1982 to the present." Appealing to such stability, Taylor (1999a, p. 60) argues that changes since 1982 in observed inflation variance/output-gap variance combinations reflect a movement toward the efficient policy frontier. In particular, with stable inflation there are fewer recessions triggered by attempts to rein inflation in, so the "improvement in output stability…is an important consequence of the improvement in price stability." In terms of the Phillips curve equation (3), the variability of the expected-inflation term has been reduced by the change in monetary policy rule; the inflation/output gap cycle that Taylor (1987) argued was responsible for essentially all important GDP variation has been removed.

In Taylor's framework, this change in policy did not constitute a switch from "discretion" to "rules," but instead an improvement in the specification of the U.S. monetary policy rule. Taylor (e.g., 1979, 1999c) found it useful to characterize U.S. monetary policy in the postwar decades[28] as following a "rule," even though that period was frequently associated with poor economic outcomes. By taking the form of a rule (a reaction function) rather than a series of one-time decisions, monetary policy responses in this regime were often quite predictable; nevertheless, this predictability did not contribute to reduced macroeconomic uncertainty. Both Taylor and Friedman shared the belief that the virtue of rules is that they can and should reduce uncertainty. This shared perspective is brought out by considering a statement by Friedman (1983, p. 3)

> [P]olicy implications that monetarists like myself have drawn…is that the primary task of the monetary authorities should be to avoid introducing uncertainty in the economy. (Friedman, 1983, p. 3)

alongside one by Taylor (1993b, p. 6)

> Economic theory shows that things work better if there is more certainty about the conduct of monetary policy. (Taylor, 1993b, p. 6)

But the monetary policy rule in the initial postwar decades did not make "things work better," because it implied responses to the state of the economy that worsened inflation and output fluctuations.

Friedman's View of Fluctuations

Friedman advanced positions that were in essential agreement with Taylor's. Specifically, while real shocks have been a major source of economic fluctuations, this reflected monetary policy reaction to those shocks, whose effect has "merely [been] to make the economy less rather than more stable"[29] (Friedman, 1959, p. 144) and to "produce inappropriate fluctuations in output" (Friedman, 2006). Many real shocks are relevant for potential output but, were it not for monetary policy's role in magnifying their effect on actual output, the shocks would merely constitute "the myriad of factors making for minor fluctuations in economic activity" (Friedman, 1959, p. 144). Accordingly, Friedman regarded potential output as smooth: With the exception of events like the major OPEC actions, "[t]he real factors that determine the potential output of an economy…generally change slowly and gradually" (Friedman and Schwartz, 1982, p. 414). Friedman, like Taylor, accordingly attributed the Great Moderation to a more efficient monetary policy, which eliminated the destabilizing properties that monetary policy

[27] See also Taylor (1998, p. 5). Because, in Taylor's view, potential output varies little, lower real shock variance would not necessarily remove a major source of fundamental output variation. Rather, lower real shock variance would imply a lower variance for the inputs of the monetary policy reaction function and so would reduce the destabilizing effects of an inefficient monetary policy.

[28] Specifically, 1953:Q1–1975:Q4 in Taylor (1979); 1960:Q1–1979:Q4 in Taylor (1999c).

[29] Woodford (1998) similarly interprets Friedman as implying that monetary policy actions in postwar decades destabilized the economy's adaptation to real shocks.

has exhibited historically (see Friedman's observations in Taylor, 2001).

Friedman and Taylor therefore shared similar views on the sources of shocks. In line with the subsequent New Keynesian dynamic general equilibrium literature, they emphasized the importance of systematic monetary policy in determining output behavior. In contrast to most New Keynesian discussions, however, Friedman and Taylor treated potential output as smooth. Real shocks presumably can generate large variations in the natural rate of interest in their frameworks, but, typically, not in potential output.

THE POWER AND DUTY OF MONETARY POLICY

One way of thinking about John Taylor's work on nominal contracts is that it formalized the natural rate hypothesis, and in particular treated expectations formation and adjustment rigorously, while still preserving the emphasis on nominal rigidity (wage or price stickiness) that had been common to both Friedman's and A.W. Phillips's work. Earlier formalizations of the natural rate hypothesis, such as Lucas (1972), had not featured nominal rigidity. Here I discuss another sense in which Taylor followed Friedman's Phillips-curve ideas and in so doing further departed from the original Phillips (1958) analysis.

To incorporate Friedman's Phillips-curve views, one needs three elements: Expectations have to appear in the Phillips curve; their coefficient should be unity; and *they must be endogenous*. If you add expectations as an exogenous forcing process in the Phillips curve, you are introducing a variable that shifts the relation between the output gap and inflation, but you are not capturing the notion that monetary policy ultimately pins down inflation and inflation expectations alike.

And it seems to me that some of Phillips's work on inflation might be vulnerable to this criticism. Certainly Friedman thought so: He suggested (1976, p. 219) that the absence of expectations adjustment from the original Phillips curve analysis followed from the Keynesian tradition that the "price level could be regarded as an institutional datum." The fundamental contribution of Phillips curve analysis relative to pre-Phillips curve Keynesianism was to make inflation an endogenous variable. But this contribution was not integrated completely into Phillips's own analysis, as he was willing to treat a large fraction of inflation variation as exogenous (an institutional datum, in Friedman's terminology). For example, Phillips (1958) related wage inflation to unemployment and made exogenous movements in inflation a curve-shifting variable—so, for example, he attributed deviations from the empirical Phillips curve to import price inflation and invoked this factor as an exogenous source of wage-price spirals.[30] This perspective is clearly different from that in Friedman's writings, where monetary restraint is (by means of control of aggregate demand) a necessary and sufficient condition for inflation control. In Friedman's framework, as discussed above, there *is* an exogenous element to current inflation—the u_t term in equation (2)—but it is a transitory element that hardly matters for expected inflation; in fact, it does not matter for expectations at all if the lagged-coefficient term γ in equation (2) is zero rather than merely low.

The Friedman framework rejects the notion that shocks to specific prices can in themselves be a source of ongoing inflation. If these shocks are associated with a change in the mean of inflation, it is because the monetary authority's reaction to the shock has had the effect of shifting the mean of inflation. This position on the power of monetary policy is also that adhered to by Taylor,[31] as discussed above, and shows up clearly also in policy discussions such as that of Mishkin (2007).

In Friedman's framework, therefore, inflation and expected inflation are endogenous variables ultimately pinned down by monetary policy; and the convergence of inflation and expected

---

[30] See especially Phillips (1958, p. 284).

[31] This is not to deny influences of Phillips's work on Taylor, which are stressed by Asso, Kahn, and Leeson (2007). But I argue that these influences were mainly reflected in Taylor's early interest in optimal control analysis rather than in Taylor's ultimate views on what monetary policy could and should do.

inflation means that the output gap is zero on average irrespective of monetary policy. Phillips, on the other hand, attributed considerable inflation variation to exogenous factors, while also advancing an aggregate supply specification that implied that the output gap was generally nonzero in the long run.

CONCLUSIONS

The preceding discussion has emphasized that, although the names of Taylor and Friedman are associated with different monetary policy rules, the difference between Taylor and Friedman on how the economy works is not great. Taylor and Friedman both emphasized Phillips curve specifications that impose temporary nominal price rigidity and the long-run natural-rate restriction; and there was basic agreement between them on policymaker objectives, the sources of shocks, and policy trade-offs. Where they differed was on the extent to which structural models should enter the monetary policy decisionmaking process. This difference helps account for the differences in their preferred monetary policy rules. Their rules do share an emphasis on nominal variables and reflect the agreement between them that it is both feasible and desirable for monetary policy to preclude deviations in inflation expectations from a constant, low rate. In this respect, Taylor and Friedman both put greater emphasis than Phillips did on the power and duty of monetary policy.

REFERENCES

Asso, Francesco; Kahn, George and Leeson, Robert. "Monetary Policy Rules: From Adam Smith to John Taylor." Presented at Federal Reserve Bank of Dallas conference *John Taylor's Contributions to Monetary Theory and Policy*, October 12-13, 2007.

Bernanke, Ben. S.; Laubach, Thomas; Mishkin, Frederic S. and Posen, Adam S. *Inflation Targeting: Lessons from the International Experience*. Princeton, NJ: Princeton University Press, 1999.

Calvo, Guillermo A. "Staggered Prices in a Utility-Maximizing Framework." *Journal of Monetary Economics*, September 1983, *12*(3), pp. 383-98.

Chari, V.V.; Kehoe, Patrick J. and McGrattan, Ellen R. "Sticky Price Models of the Business Cycle: Can the Contract Multiplier Solve the Persistence Problem?" *Econometrica*, September 200, *68*(5), pp. 1151-79.

Christiano, Lawrence J.; Eichenbaum, Martin and Evans, Charles. "Nominal Rigidities and the Dynamic Effects of a Shock to Monetary Policy." *Journal of Political Economy*, February 2005, *113*(1), pp. 1-45.

Clarida, Richard; Galí, Jordi and Gertler, Mark. "The Science of Monetary Policy: A New Keynesian Perspective." *Journal of Economic Literature*, December 1999, *37*(4), pp. 1661-707.

Erceg, Christopher J.; Henderson, Dale W. and Levin, Andrew T. "Optimal Monetary Policy with Staggered Wage and Price Contracts." *Journal of Monetary Economics*, October 2000, *46*(2), pp. 281-313.

Fischer, Stanley. "Long-Term Contracts, Rational Expectations, and the Optimal Money Supply Rule." *Journal of Political Economy*, February 1977, *85*(1), pp. 191-206.

Friedman, Milton. "Statement on Monetary Theory and Policy," in *Employment, Growth and Price Levels*, hearings before the Joint Economic Committee. Washington, DC: Government Printing Office, 1959, pp. 605-12. Reprinted as "Monetary Theory and Policy," in R.J. Ball and P. Doyle, eds., *Inflation*. London: Penguin, 1969, pp. 136-45.

Friedman, Milton. *A Program for Monetary Stability*. Fordham, NY: Fordham University Press, 1960.

Friedman, Milton. *Inflation: Causes and Consequences*. New York: Asia Publishing House, 1963. Reprinted in Milton Friedman, *Dollars and Deficits: Living with America's Economic Problems*. Eaglewood Cliffs, NJ: Prentice Hall, 1968, pp. 21-71.

Friedman, Milton. "Interest Rates and the Demand for Money." *Journal of Law and Economics*, October 1966a, *9*, pp. 71-85. Reprinted in Milton Friedman, *The Optimum Quantity of Money and Other Essays*. Chicago: Aldine, 1969, pp. 141-56.

Friedman, Milton. "An Inflationary Recession." *Newsweek*, October 17, 1966b, p. 92.

Friedman, Milton. "The Monetary Theory and Policy of Henry Simons." *Journal of Law and Economics*, October 1967, 10, pp. 1-13. Reprinted in Milton Friedman, *The Optimum Quantity of Money and Other Essays*. Chicago: Aldine, 1969, pp. 81-96.

Friedman, Milton. "The Role of Monetary Policy." *American Economic Review*, March 1968, *58*(1), pp. 1-17.

Friedman, Milton. "The Optimum Quantity of Money," in *The Optimum Quantity of Money and Other Essays*. Chicago: Aldine, 1969, pp. 1-50.

Friedman, Milton. "The Case for a Monetary Rule." *Newsweek*, February 7, 1972, p. 67.

Friedman, Milton. "How Much Monetary Growth?" *Morgan Guarantee Survey*, February 1973a, pp. 5-10.

Friedman, Milton. *Money and Economic Development: The Horowitz Lectures of 1972*. New York: Praeger, 1973b.

Friedman, Milton. "Inflation Prospects." *Newsweek*, November 4, 1974a, p. 52.

Friedman, Milton. "Monetary Correction." IEA Occasional Paper No. 41, Institute for Economic Affairs, 1974b. Reprinted in Milton Friedman, ed., *Tax Limitation, Inflation and the Role of Government*. Dallas: Fisher Institute, 1978, pp. 22-51.

Friedman, Milton. *Price Theory*. Second Edition. Chicago: Aldine, 1976.

Friedman, Milton."Discussion of 'The Monetarist Controversy'." Federal Reserve Bank of San Francisco *Economic Review*, Spring 1977a, *3*(1, Suppl.), pp. 12-26.

Friedman, Milton. "Why Inflation Persists." *Newsweek*, October 3, 1977b, p. 84.

Friedman, Milton. "Iran and Energy Policy." *Newsweek*, December 31, 1979a, p. 61.

Friedman, Milton. "Inflation and Jobs." *Newsweek*, November 12, 1979b, p. 97.

Friedman, Milton. "Carter's Anti-Inflation Plan." *Newsweek*, March 24, 1980, p. 40.

Friedman, Milton. "Monetary Policy: Theory and Practice." *Journal of Money, Credit, and Banking*, August 1982a, *14*(3), pp. 98-118.

Friedman, Milton (1982b). "Letter to Senator Roger W. Jepsen, Vice-Chairman, Joint Economic Committee," in *Monetarism and the Federal Reserve's Conduct of Monetary Policy: Compendium of Views Prepared for the Use of the Subcommittee on Monetary and Fiscal Policy*, hearings before the Joint Economic Committee. Washington, DC: Government Printing Office, 1982b, pp. 73-74.

Friedman, Milton. "Defining 'Monetarism'." *Newsweek*, July 12, 1982c, p. 64.

Friedman, Milton. "Monetarism in Rhetoric and in Practice." *Bank of Japan Monetary and Economic Studies*, October 1983, *1*, pp. 1-14.

Friedman, Milton. "Has Monetarism Failed?" *Manhattan Report*, 1984, *4*(3), pp. 3-4.

Friedman, Milton. "Quantity Theory of Money," in J. Eatwell, M. Milgate, and P. Newman, eds., *The New Palgrave: A Dictionary of Economics*. Volume 4, Q to Z. London: Macmillan, 1987, pp. 3-20.

Friedman, Milton. "Tradeoffs in Monetary Policy." Unpublished manuscript, Stanford University, 2006.

Friedman, Milton and Friedman, Rose D. *Free to Choose*. New York: Harcourt Brace Jovanovich, 1980.

Friedman, Milton and Friedman, Rose D. *The Tyranny of the Status Quo*. New York: Harcourt Brace Jovanovich, 1984.

Friedman, Milton, and Schwartz, Anna J. *Monetary Trends in the United States and the United Kingdom: Their Relation to Income, Prices, and Interest Rates,* *1867-1975*. Chicago: University of Chicago Press, 1982.

Fuhrer, Jeffrey C. and Moore, George R. “Inflation Persistence.” *Quarterly Journal of Economics*, February 1995, *110*(1), pp. 127-59.

Giannoni, Marc P. and Woodford, Michael. “Optimal Inflation-Targeting Rules,” in Ben S. Bernanke and Michael Woodford, eds., *The Inflation-Targeting Debate*. Chicago: University of Chicago Press, 2005, pp. 93-162.

Hall, Robert E. and Taylor, John B. *Macroeconomics*. Fifth Edition. New York: W.W. Norton, 1997.

King, Mervyn A. “Changes in U.K. Monetary Policy: Rules and Discretion in Practice.” *Journal of Monetary Economics*, June 1997, *39*(1), pp. 81-97.

Klamer, Arjo (1983). “Interview: John Taylor,” in Arjo Klamer, ed., *Conversations with Economists*. Totowa, NJ: Rowman and Littlefield, 1983, pp. 170-76.

Lucas, Robert E. Jr. “Expectations and the Neutrality of Money.” *Journal of Economic Theory*, April 1972, *4*(2), pp. 103-24.

Mankiw, N. Gregory. “The New Keynesian Microfoundations: Comment.” *NBER Macroeconomics Annual*, 1987, *2*(1), pp. 105-10.

McCallum, Bennett T. “Two Fallacies Concerning Central Bank Independence.” *American Economic Review*, May 1995 (Papers and Proceedings), *85*(2), pp. 207-11.

McCallum, Bennett T. and Nelson, Edward. “Targeting versus Instrument Rules for Monetary Policy: What Is Wrong with McCallum and Nelson?: Comment.” Federal Reserve Bank of St. Louis *Review*, September/October 2005, *87*(5), pp. 627-31.

Mishkin, Frederic. “Globalization, Macroeconomic Performance, and Monetary Policy.” Presented at the Board of Governors of the Federal Reserve System conference *Domestic Prices in an Integrated World Economy*, Washington, DC, September 27, 2007; www.federalreserve.gov/newsevents/speech/mishkin20070927a.htm.

Musy, Olivier. “Inflation Persistence and the Real Costs of Disinflation in Staggered Prices and Partial Adjustment Models.” *Economics Letters*, April 2006, *91*(1), pp. 50-55.

Nelson, Edward and Schwartz, Anna J. “The Impact of Milton Friedman on Modern Monetary Economics: Setting the Record Straight on Paul Krugman’s ‘Who Was Milton Friedman?’” *Journal of Monetary Economics*, 2008 (forthcoming).

Nobel Committee. “Edmund S. Phelps’s Contribution to Macroeconomics.” October 9, 2006.

Orphanides, Athanasios. “The Quest for Prosperity Without Inflation.” *Journal of Monetary Economics*, April 2003, *50*(3), pp. 633-63.

Phillips, A.W. “The Relationship Between Unemployment and the Rate of Change of Money Wage Rates in the United Kingdom, 1861-1957.” *Economica*, November 1958, *25*(100), pp. 283-99.

Roberts, John M. “New Keynesian Economics and the Phillips Curve.” *Journal of Money, Credit, and Banking*, November 1995, *27*(4), pp. 975-84.

Rotemberg, Julio J. “Sticky Prices in the United States.” *Journal of Political Economy*, December 1982, *90*(6), pp. 1187-211.

Rotemberg, Julio J. and Woodford, Michael. “An Optimization-Based Econometric Framework for the Evaluation of Monetary Policy.” *NBER Macroeconomics Annual*, 1997, *12*(1), pp. 297-346.

Schmitt-Grohé, Stephanie and Uribe, Martin. “Comparing Two Variants of Calvo-Type Wage Stickiness.” NBER Working Paper No. 12740, National Bureau of Economic Research, 2006.

Svensson, Lars E.O. “Inflation Forecast Targeting: Implementing and Monitoring Inflation Targets.” *European Economic Review*, October 1997, *41*(6), pp. 1111-46.

Svensson, Lars E.O. “Targeting versus Instrument Rules for Monetary Policy: What Is Wrong with McCallum and Nelson?” Federal Reserve Bank of St. Louis *Review*, September/October 1997, *87*(5), pp. 613-25.

Taylor, John B. "Estimation and Control of a Macroeconomic Model with Rational Expectations." *Econometrica*, September 1979, *47*(5), pp. 1267-86.

Taylor, John B. "Aggregate Dynamics and Staggered Contracts." *Journal of Political Economy*, February 1980, *88*(1), pp. 1-23.

Taylor, John B. "Stabilization, Accommodation, and Monetary Rules." *American Economic Review*, May 1981a (Papers and Proceedings), *71*(2), pp. 145-49.

Taylor, John B. "On the Relation Between the Variability of Inflation and the Average Inflation Rate." *Carnegie-Rochester Conference Series on Public Policy*, Autumn 1981b, *15*, pp. 57-86.

Taylor, John B. "Letter to the Honorable Robert W. Jepsen, August 31, 1982," in *Monetarism and the Federal Reserve's Conduct of Monetary Policy: Compendium of Views Prepared for the Use of the Subcommittee on Monetary and Fiscal Policy*, hearings before the Joint Economic Committee. Washington, DC: Government Printing Office, 1982, pp. 156-59.

Taylor, John B. "An Appeal for Rationality in the Policy Activism Debate," in R.W. Hafer, ed., *The Monetary Versus Fiscal Policy Debate: Lessons from Two Decades*. Littlefield, N.J.: Rowman and Allenheld, 1986a, pp. 151-63.

Taylor, John B. "New Econometric Approaches to Stabilization Policy in Stochastic Models of Macroeconomic Fluctuations," in Z. Griliches and M. Intriligator, eds., *Handbook of Econometrics*. Volume 3. Amsterdam: Elsevier, 1986b, pp. 1998-2055.

Taylor, John B. "Improvements in Macroeconomic Stability: The Role of Wages and Prices," in R.J. Gordon, ed., *The American Business Cycle: Continuity and Change*. Chicago: University of Chicago Press, 1986c, pp. 639-59.

Taylor, John B. "Reply," in R.J. Gordon, ed., *The American Business Cycle: Continuity and Change*. Chicago: University of Chicago Press, 1986d, pp. 672-77.

Taylor, John B. "Externalities Associated with Nominal Price and Wage Rigidities," in W.A. Barnett and K.J. Singleton, eds., *New Approaches to Monetary Economics*. Cambridge, U.K.: Cambridge University Press, 1987, pp. 350-67.

Taylor, John B. "The Great Inflation, the Great Disinflation, and Policies for Future Price Stability," in A. Blundell-Wignall, ed., *Inflation, Disinflation and Monetary Policy*. Sydney: Ambassador Press, 1992, pp. 9-31.

Taylor, John B. "Discretion versus Policy Rules in Practice." *Carnegie-Rochester Series on Public Policy*, 1993a, *39*, pp. 195-214.

Taylor, John B. "Price Stabilization in the 1990s: An Overview," in K. Shigehara, ed., *Price Stabilization in the 1990s: Domestic and International Policy Requirements*. London: Macmillan, 1993b, pp. 1-6.

Taylor, John B. "The Inflation-Output Variability Tradeoff Revisited," in J.C. Fuhrer, ed., *Goals, Guidelines, and Constraints Facing Monetary Policymakers*. Boston: Federal Reserve Bank of Boston, 1994, pp. 23-38.

Taylor, John B. "Monetary Policy and the Long Boom." Federal Reserve Bank of St. Louis *Review*, November/December 1998, *80*(6), pp. 3-11.

Taylor, John B. "Commentary: Challenges for Monetary Policy: New and Old," in *New Challenges for Monetary Policy*. Kansas City, MO: Federal Reserve Bank of Kansas City, 1999a, pp. 59-68.

Taylor, John B. "An Historical Analysis of Monetary Policy Rules," in John B. Taylor, ed., *Monetary Policy Rules*. Chicago: University of Chicago Press, 1999b, pp. 319-41.

Taylor, John B. "Staggered Price and Wage Setting in Macroeconomics," in John B. Taylor and Michael Woodford, eds., *Handbook of Macroeconomics*. Volume 1B. Amsterdam: Elsevier, 1999c, pp. 1009-50.

Taylor, John B. "Low Inflation, Pass-Through, and the Pricing Power of Firms." *European Economic Review*, June 2000a, *44*(7), pp. 1389-408.

Taylor, John B. "Remarks for the Panel Discussion 'Recent Changes in Trend and Cycle'." Presented at the Federal Reserve Bank of San Francisco/SIEPR conference *Structural Change and Monetary Policy*, March 3-4, 2000b.

Taylor, John B. "An Interview with Milton Friedman." *Macroeconomic Dynamics*, February 2001, *5*(1), pp. 101-31.

Taylor, John B. "The International Implications of October 1979: Toward a Long Boom on a Global Scale." Federal Reserve Bank of St. Louis *Review*, March/April 2005, *87*(2), pp. 269-76.

Taylor, John B. "Thirty-Five Years of Model Building for Monetary Policy Evaluation: Breakthroughs, Dark Ages, and a Renaissance." *Journal of Money, Credit, and Banking*, February 2007, *39*(1, Suppl.), pp. 193-201.

Westaway, Peter. "How Costly Is Inflation Reduction? An Investigation of Simple Interest-Rate Rules Under Different Models of Nominal Inertia." Presented at a Reserve Bank of New Zealand monetary policy workshop, October 20, 1997.

Woodford, Michael. "A Frictionless View of U.S. Inflation: Comment." *NBER Macroeconomics Annual*, 1998, *13*(1), pp. 390-418.

Woodford, Michael. *Interest and Prices: Foundations of a Theory of Monetary Policy*. Princeton, NJ: Princeton University Press, 2003.

A Primer on the Empirical Identification of Government Spending Shocks

Kristie M. Engemann, Michael T. Owyang, and Sarah Zubairy

The empirical literature on the effects of government spending shocks lacks unanimity about the responses of consumption and wages. Proponents of shocks identified by structural vector autoregressions (VARs) find results consistent with New Keynesian models: consumption and wages increase. On the other hand, proponents of the narrative approach find results consistent with neoclassical models: consumption and wages decrease. This paper reviews these two identifications and confirms their differences by using standard economic series. It also uses alternative measures of government spending, output, and the labor market and shows that, although there are minor fluctuations within each identification, the disparate results between the two are robust to the alternative measures. However, under the structural VAR approach, the authors find some differences between the responses to federal and state/local government spending. (JEL C32, E62)

Federal Reserve Bank of St. Louis *Review*, March/April 2008, *90*(2), pp. 117-132.

Many textbook macroeconomic models contain predictions about the effects of fiscal policy. Unfortunately, these models lack unanimity about the response of some variables to surprise increases in government spending. For example, neoclassical models and New Keynesian models have opposing predictions regarding the direction of the effect of government spending shocks on consumption and real wages. Neoclassical models predict that, when a government spending shock hits the economy, households, facing the prospect of higher taxes, experience a negative wealth effect. Households respond by lowering their consumption and leisure. The increased labor supply from households also leads to a fall in real wages for any given labor demand. New Keynesian models instead predict that consumption and real wages rise in response to a positive government spending shock. These models often contain features that generate countercyclical markups (e.g., nominal price rigidities or deep habits [see Ravn, Schmitt-Grohé, and Uribe (2006)]), which in turn cause labor demand to shift up in response to a government spending shock. This results in rising wages and higher consumption for the households due to substitution effects or the presence of credit constraints.

The empirical literature has been unable to resolve this controversy. Depending on the nature of the identifying assumptions, the empirical literature finds disparate stylized facts regarding the responses of some variables to government spending shocks. The responses of consumption and wages, in particular, can take on different signs depending on the assumptions used to identify fiscal policy shocks. The structural vector autoregression (VAR) approach that Blanchard and Perotti (2002) and Fatás and Mihov (2001) use to identify government spending shocks yields a positive response for output, consump-

Kristie M. Engemann is a senior research associate and Michael T. Owyang is a research officer at the Federal Reserve Bank of St. Louis. Sarah Zubairy is a PhD candidate in economics at Duke University. The authors thank Riccardo DiCecio, Valerie Ramey, Bob Rasche, and Howard Wall for suggestions and useful conversations. Heidi Beyer-Powe provided research assistance.

tion, and real wages. On the other hand, the narrative approach introduced by Ramey and Shapiro (1998) uses military spending events as a proxy for exogenous shocks to government spending. This approach typically finds that, in response to these large military buildups, output rises but consumption and real wages fall.[1]

A few recent papers (e.g., Ramey, 2006, and Perotti, 2007) have reenergized this debate regarding the responses of economic variables under different identification schemes. One concern is that the structural VAR approach may not be identifying exogenous innovations to fiscal policy. That is, the timing restrictions used in structural VARs may identify shocks that are anticipated by economic agents. This would confound the econometrician's ability to disentangle the effects of fiscal policy. This results in responses that are biased by some omitted predictors.[2] Criticism can also be levied on the narrative approach. This methodology treats all of the large fiscal episodes equally rather than allowing for some variation in the size and shape of the response.

In this paper, we review some of the findings in this empirical literature on government spending. In addition, we are interested in distinguishing between shocks to total government spending and disaggregated measures such as federal government spending and state and local government expenditures. A third issue we address is whether the responses of macro variables are robust to alternative measures of data: for example, using real personal income (PI) as a measure of real economic activity instead of real gross domestic product (GDP) or using employment instead of hours worked. Our findings suggest that the choice of macro variables in the VAR is important. For instance, we find that employment is generally more responsive than hours and that personal income is less responsive than GDP to a total government spending shock.

The remainder of the paper is organized as follows. The next section reviews two of the identification procedures common to the literature on fiscal policy. It first outlines the identification of the structural VAR based on timing restrictions and then considers the dummy-variable identification using the military spending dummies defined by Ramey and Shapiro (1998). The third section reviews the data used in the estimation. In particular, it discusses differences across the government spending series and suggests alternative measures of output and the labor market. The fourth section presents the results from the estimation using various identifications and specifications.

MODEL AND IDENTIFICATION

Since Sims (1980), the VAR has become a staple in the empirical literature on monetary, technology, and fiscal policy shocks. In each case, for VARs to be useful for policy analysis, they require some restrictions to transform the reduced-form residuals into structural innovations. With these structural innovations in hand, one can then determine the responses of non-policy variables (e.g., output, prices) to the shock in question. In this section, we consider two common identifying assumptions used in the empirical literature on fiscal policy: (i) a structural VAR approach that uses timing restrictions (Blanchard and Perotti, 2002), which assumes that innovations to government spending occur prior to the determination of other variables, and (ii) the narrative approach that uses military spending dummy variables (Ramey and Shapiro, 1998) as a proxy for exogenous shocks to government spending.[3]

Timing Restrictions

Consider the following reduced-form *p*-order VAR

[1] In this growing literature, other researchers have considered the effect of government spending on different economic variables. For example, Tavares and Valkanov (2003) examine the effect of fiscal policy on asset prices and Favero and Giavazzi (2007) examine the effect on interest rates.

[2] We may view these biases as similar to the manner in which the price response to monetary shocks exhibits a price puzzle when commodity prices are omitted (Sims, 1992). Agents may have information about the economy that would be outside the VAR. Thus, the identified monetary shocks might be mixing exogenous shocks with an endogenous response to omitted variables.

[3] Mountford and Uhlig (2005) and Pappa (2005) posit an alternative identifying assumption using sign restrictions. The findings for these identifications are similar to those of the structural VAR literature and are not considered here.

(1) $$y_t = B(L) y_{t-1} + \varepsilon_t,$$

where y_t is the $n \times 1$ vector of economic variables including government spending, $B(L)$ is a polynomial of lag operators, and $\varepsilon_t \sim N(0,\Sigma)$ are reduced-form innovations. The structural representation of the VAR can be written as

$$A_0 y_t = A(L) y_{t-1} + v_t.$$

The objective, then, is to uncover the structural innovations, v_t, defined by an orthonormal rotation of the reduced-form residuals:

(2) $$v_t = A_0 \varepsilon_t,$$

where $A_0^{-1}\Omega A_0^{-1\prime} = \Sigma$, $v_t \sim N(0,\Omega)$, and the covariance matrix Ω of the structural innovations is diagonal. The well-known problem in the literature on structural VARs is that $A_0^{-1}\Omega A_0^{-1\prime} = \Sigma$ does not define a unique rotation. The matrix A_0 contains n^2 coefficients, which need to be determined to identify a unique rotation. However, the system $A_0^{-1}\Omega A_0^{-1\prime} = \Sigma$ provides only $[n(n+1)]/2$ parameters to tie down elements of A_0. To identify the true structural innovations, one must place some restrictions on the system. If restrictions can be placed on A_0 itself, one would need at least $[n(n-1)]/2$ binding restrictions for identification.[4] Often, theory does not provide enough assumptions to identify the full complement of parameters in A_0. In such cases, one might choose to place fewer restrictions on the system and identify only a particular shock (e.g., a single row of A_0).

These restrictions can be of several forms. Exclusion restrictions assume that some variables do not respond contemporaneously to the shock. These restrictions are implemented by setting elements of A_0 to zero and generally imply a causal ordering across the variables (e.g., the federal funds rate responds to innovations in output but not vice versa). Sign restrictions identify the shock by imposing the direction of the impulse responses of certain variables at predefined horizons. For example, we can identify contractionary monetary shocks by restricting the resulting impulse responses of the federal funds rate (positive) and inflation rate (negative) at short horizons.[5] Forecast-error-variance restrictions identify the shocks through their relative power in explaining fluctuations in certain economic variables. These types of restrictions are particularly useful if theory indicates that the structural innovation should be neutral or dominant at long horizons.[6]

The timing identification involves the assumption that government spending is determined before the realizations of output and any other economic variables in quarterly data. Essentially, this assumption presumes that all other variables have no contemporaneous impact on government spending. This is accomplished by ordering government spending first in the VAR and identifying the matrix A_0^{-1} as the Cholesky decomposition of Σ. The fiscal shock, then, is represented by the first row of the rotation matrix A_0.

Defense Spending Dummies

Identification of fiscal policy by means of the narrative approach is conducted in a similar framework as the timing restrictions outlined above.[7] In this case, however, the VAR in equation (1) is augmented by a series of dummy variables representing the timing of large military buildups:

$$y_t = B(L) y_{t-1} + C(L) d_t + \varepsilon_t,$$

where d_t is the period-t realization of the military spending dummy having an effect on the variables of interest at, say, q lags and both $B(L)$ and $C(L)$ are lag polynomials of possibly different orders.[8] The effect of fiscal policy is then computed as the impulse response to the military spending dum-

4 Weaker identifying assumptions may require more than $[n(n-1)]/2$ restrictions (see Paustian, 2007).

5 See Uhlig (2005) for implementation.

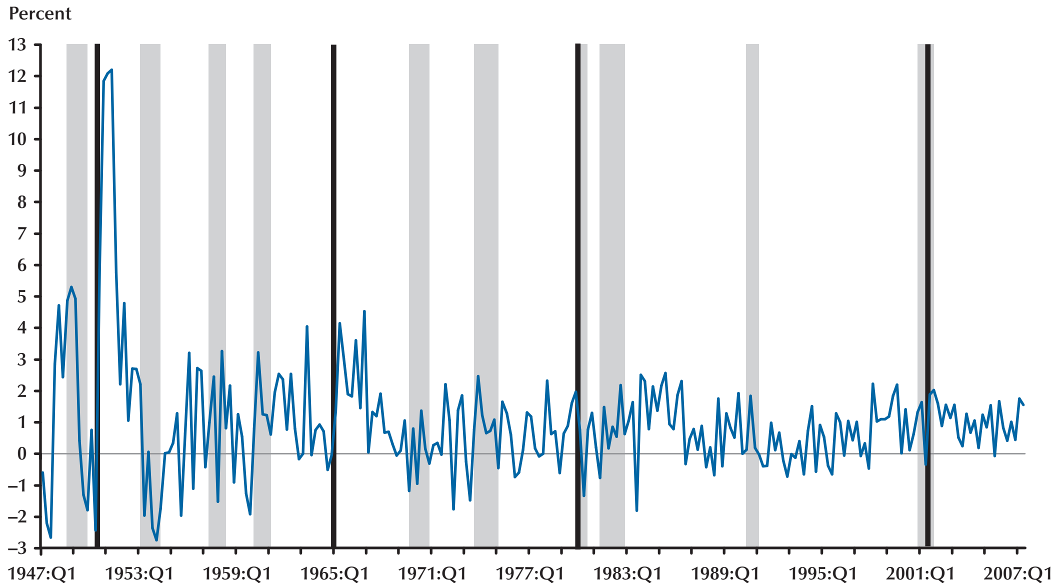
6 For example, Faust (1998) identifies monetary shocks by assuming they have no long-run effect on output. Similarly, Francis, Owyang, and Roush (2007) identify technology shocks by assuming they are the largest contributor of labor productivity volatility.

7 Ramey and Shapiro (1998) estimate a two-variable version of the fiscal policy dummy. Edelberg, Eichenbaum, and Fisher (1999) extend the analysis to a VAR framework.

8 We use military dummies and military spending dummies interchangeably.

Figure 1

U.S. Government Spending Growth Rate (quarter/quarter percent change)



NOTE: Black bars indicate Ramey and Shapiro military buildup dates; gray shaded areas indicate NBER recession dates. The government spending series used is real government consumption expenditures and gross investment (seasonally adjusted, annual rate [SAAR]).

mies. The k-step-ahead response to the military shock is defined by the coefficient of L^k in the expansion of $(I - B(L)L)^{-1}C(L)$.

Ramey and Shapiro's (1998) series of military spending dummies is constructed similarly to Romer and Romer's (1994) series of monetary policy innovations. While Romer and Romer consult the transcripts from FOMC meetings, Ramey and Shapiro use historical accounts and *Business Week* to identify periods in which the private sector revised upward their forecasts of future government spending.[9] Given their definition of fiscal spending shocks, Ramey and Shapiro (1998) identify four episodes in the post-World War II period that qualify as exogenous shocks to government spending.[10] The black vertical lines in Figure 1 depict these dates, along with the growth rate in government spending and the recessions (as defined by the National Bureau of Economic Research; NBER) for the period 1947:Q1 to 2007:Q3.[11]

DATA

In addition to identification assumptions, the choice of variables included in the regression can affect the results. In particular, the choice of the government spending variable can significantly alter the conclusions drawn. Here, we discuss alternative government spending series. In

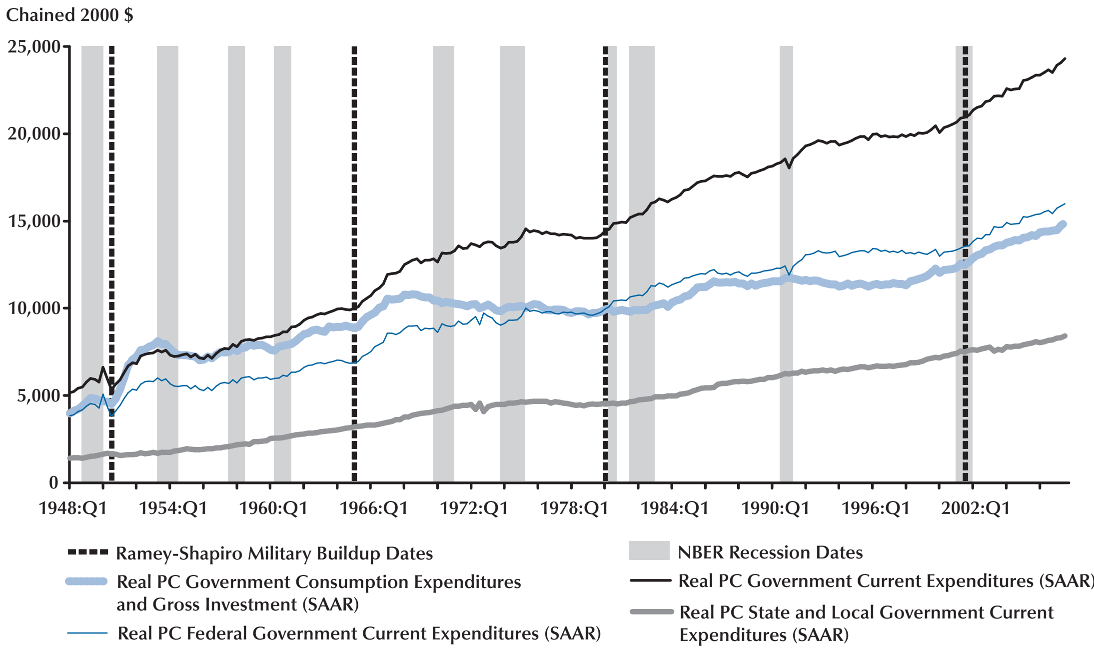
[9] Romer and Romer (2007) employ a similar narrative method to identify fiscal shocks, namely, tax shocks. They examine news, speeches of government officials, and other government documents.

[10] Ramey and Shapiro (1998) originally identified three episodes: the Korean War, the Vietnam War, and the Carter-Reagan buildup. Ramey (2006) and Eichenbaum and Fisher (2005) extend the sample by adding the post-9/11 buildup.

[11] Other papers have used slightly different versions of the military dummies. For example, Burnside, Eichenbaum, and Fisher (2004) relax the restriction of responses to the military dummies in all episodes to have the same size and allow the fiscal episodes to have different intensities.

Figure 2

U.S. Government Spending



NOTE: PC is per capita. To obtain per capita series, the original series were divided by the civilian labor force.

addition, we outline some of the possible economic data that can be used to measure the effect of fiscal policy.

Government Spending

Not surprisingly, the effect of an innovation to government spending depends on one's definition of government spending. The government spending series frequently used in the literature with timing restrictions is real per capita government consumption expenditures and gross investment. This series includes federal, state, and local expenditures. Figure 2 shows how the latter two components have increased in recent years, both in per capita levels and as a fraction of total government spending. In addition, the figure depicts the differences in tax and transfer payment policies over time.

Although this might suggest a rise in the importance of state and local spending, a few qualifications must be noted. First, many states and municipalities have balanced budget requirements. The degree to which these requirements are enforced varies across states (Wagner and Sobel, 2006), which may suggest a different level of fiscal flexibility for state and local spending versus federal spending. Second, innovations to state and local government spending may be more likely to be anticipated. This is one of the primary motivations for Ramey and Shapiro's use of military buildups as a proxy for unanticipated fiscal shocks.

In light of these issues, we consider a few alternative government spending series.[12] The first is real per capita government consumption expenditures and gross investment (G1), as the

[12] Other papers have also considered the effects of different types of fiscal policy. For example, Fatás and Mihov (2001) consider deficits. Rotemberg and Woodford (1992) identify fiscal policy using military purchases as the government spending variable. Perotti (2004) considers the effects of government consumption versus government investment. Perotti (2007) differentiates between expenditures on government goods and outlay toward government employees.

literature suggests.[13] We also consider G1 net of gross investment (per capita real government current expenditures, G2) and G2 net of state and local spending (per capita federal government real current expenditures, G3).[14]

Economic Variables

We include a number of variables that typically appear in both empirical and theoretical models. These include output, consumption, investment, hours, and the real wage.[15,16] The latter four variables reflect private sector contributions; government contributions to these variables are either modeled explicitly or are embedded in government spending. Real GDP, real nondurables and services consumption, and real investment and durables consumption are expressed in per capita terms.[17] We then perform some additional experiments, for example, replacing hours with employment.[18]

Much of the disparity between the two identification schemes can be highlighted by a comparison of the responses of consumption and the real wage to various identified spending shocks. In addition to these two responses, we measure the effect of government spending shocks on various components of output. Although most other papers focus on breaking down output into consumption and investment, we also consider an alternative decomposition including PI, corporate taxes, and government transfers. Simple GDP accounting yields the following:

$$\text{GDP} \simeq \text{PI} - \text{transfers} + \begin{bmatrix} \text{corporate taxes} \\ \text{indirect business tax} \\ \text{retained earnings} \\ \text{Social Security} \\ \text{depreciation} \end{bmatrix} - \begin{bmatrix} \text{net income from abroad} \\ \text{net interest} \end{bmatrix}.$$

This decomposition may allow us to determine how differences in the composition of the government spending shock affect components of the output portfolio.

EMPIRICAL RESULTS

In this section, we report the resulting impulse responses to fiscal innovations identified from the different methods described above. Each empirical model with timing restrictions is a VAR(4) with a constant and linear time trend estimated at a quarterly frequency with the logs of the variables listed. In the dummy-variable approach, each equation includes four lags of the endogenous variables and the Ramey and Shapiro dummy variables are entered with lags 0 to 6. The data range from 1948:Q1 to 2007:Q2. Each case varies slightly as we alter the decomposition of output. The point values of the impulse responses are accompanied by their corresponding 95 percent confidence intervals.[19]

Baseline Results

Figure 3 depicts the baseline case estimated with GDP, consumption, hours, investment, and real wages. The first three columns of responses correspond to different identifications for fiscal spending using the timing restrictions. The first column shows the responses of these variables to a one-standard-deviation increase to G1. The second column presents the impulse responses to a similar shock to G2. The third column presents the responses to G3 in the baseline model. In

[13] Government consumption expenditures and gross investment is defined by the Bureau of Economic Analysis (BEA) as the value of services produced by government, measured as the purchases made by government on inputs of labor, intermediate goods and services, and investment expenditures. Government consumption expenditures includes compensation of government employees, consumption of fixed capital, intermediate purchases of goods and services less sales to other sectors, and own-account production of structures and software: www.bea.gov/glossary/glossary.cfm?letter=G.

[14] Current expenditures are consumption expenditures plus current transfer payments, interest payments, and subsidies.

[15] For details on the data series we used, see the data appendix.

[16] The real wage variable used here is the real product wage. The consumption wage, which may be substituted in other studies, is the nominal wage deflated by the GDP deflator.

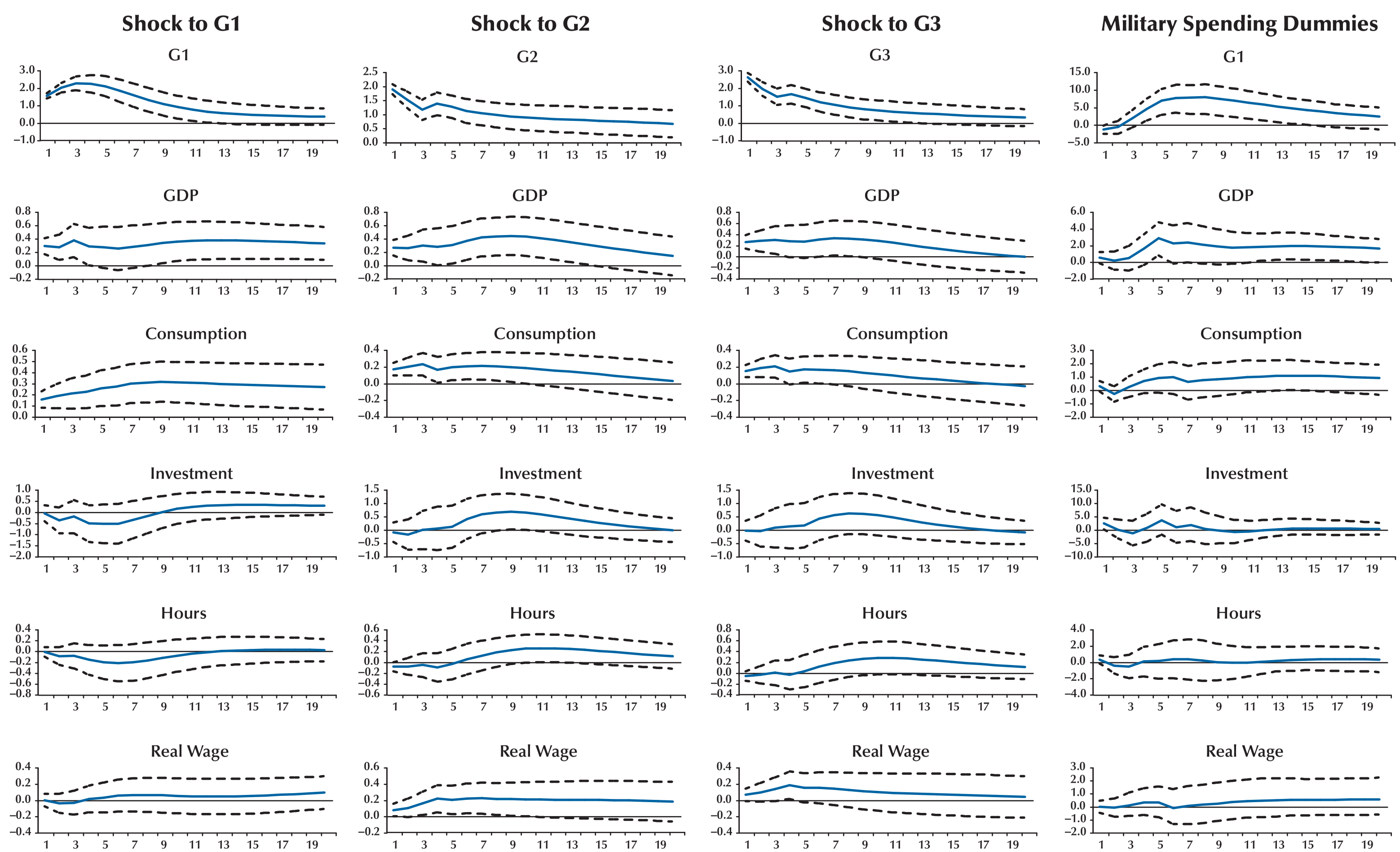
[17] To obtain per capita series, we divided by the total civilian labor force. For every series in real terms (except real wage), we deflated the nominal series by the GDP implicit price deflator.

[18] Hours and employment have some obvious differences in the nature of their business cycle fluctuations. However, the hours series may be unavailable for some applications (e.g., transition country analyses).

[19] Confidence intervals for each structural VAR are computed analytically. For the VARs with the military dummies, we use bootstrapped confidence intervals.

Figure 3

Impulse Responses from Baseline Model



NOTE: The dotted lines indicate 95 percent confidence intervals. All variables are in real terms except for hours.

this case, we isolate the impact of federal versus state and local spending. The last column depicts the impulse responses to the military dummies using G1 as the measure of government spending.

For each structural VAR, the fiscal policy shock raises GDP and government spending on impact. The hours and investment responses are weakly negative on impact; however, these effects are statistically insignificant at all horizons.[20] We find, as does the recent literature, that there are differences between the responses of consumption and real wage across identifications. A shock to any of the three spending variables produces a positive response in consumption at all horizons except for the last four periods of the G3 identification. The response in the real wage is positive for the G2 and G3 identifications but initially negative for the G1 identification.[21] On the other hand, the responses of consumption and the real wage to the military dummies are statistically insignificant; but the point estimate is slightly negative on impact for the real wage (for the first two periods), positive for consumption for the first period, but negative for consumption for the second.

Hours versus Employment

The canonical macroeconomic models typically contain predictions about hours. However, in some cases of interest to econometricians, hours data may be unavailable. Employment data can then be used as an alternative to hours data. To demonstrate the similarities and differences between the empirical responses of these two series, we reestimate the VARs and replace hours with employment in the baseline specification.[22] We use the same four identifications of fiscal policy and the same set of economic variables described in the previous specification. The responses of these other variables, as shown in Figure 4, remain qualitatively similar to those from the baseline specification with hours. The response of employment, however, differs slightly from that of hours. Based on the point responses, employment does seem more responsive to both government spending (except G1) and the military dummies.[23] Also, both employment and hours seem relatively more responsive to G3 than consumption and the real wage but not GDP or investment.

We are interested in determining whether the sensitivity of employment to government spending is attributable to differences in government versus private hires. We can accomplish this by replacing employment with private employment. These results are shown in Figure 5. Here, the responses of private employment more closely match the responses displayed in Figure 4. It seems that the major component of the weak increase in employment seen in Figure 4 is the rise in private not government hires.

Personal Income versus GDP

The output measure used in most fiscal policy regressions is GDP. However, in some cases, an equivalent measure of output may not be available at a high frequency (e.g., for U.S. states). One alternative to GDP is PI. Figure 6 shows the responses of the baseline model in which GDP has been replaced by real per capita PI. Of note are the responses to shocks to G1 and G3. In the first case depicted in the first column of Figure 6, the response of PI is qualitatively similar to GDP but shifted downward. That is, the impact response of PI to a shock to G1 is smaller than that for GDP. However, when a shock to G3 is isolated, the impact responses of GDP and PI have similar magnitudes. The responses of PI and GDP to an innovation in the military spending dummy are

[20] These results may differ slightly across the literature depending on which investment series is used. Ramey (2006), for example, uses gross domestic investment and finds a stronger response to government spending. Fatás and Mihov (2001) estimate disaggregated investment responses. They find residential investment rises and nonresidential investment falls in response to a government spending shock in a structural VAR.

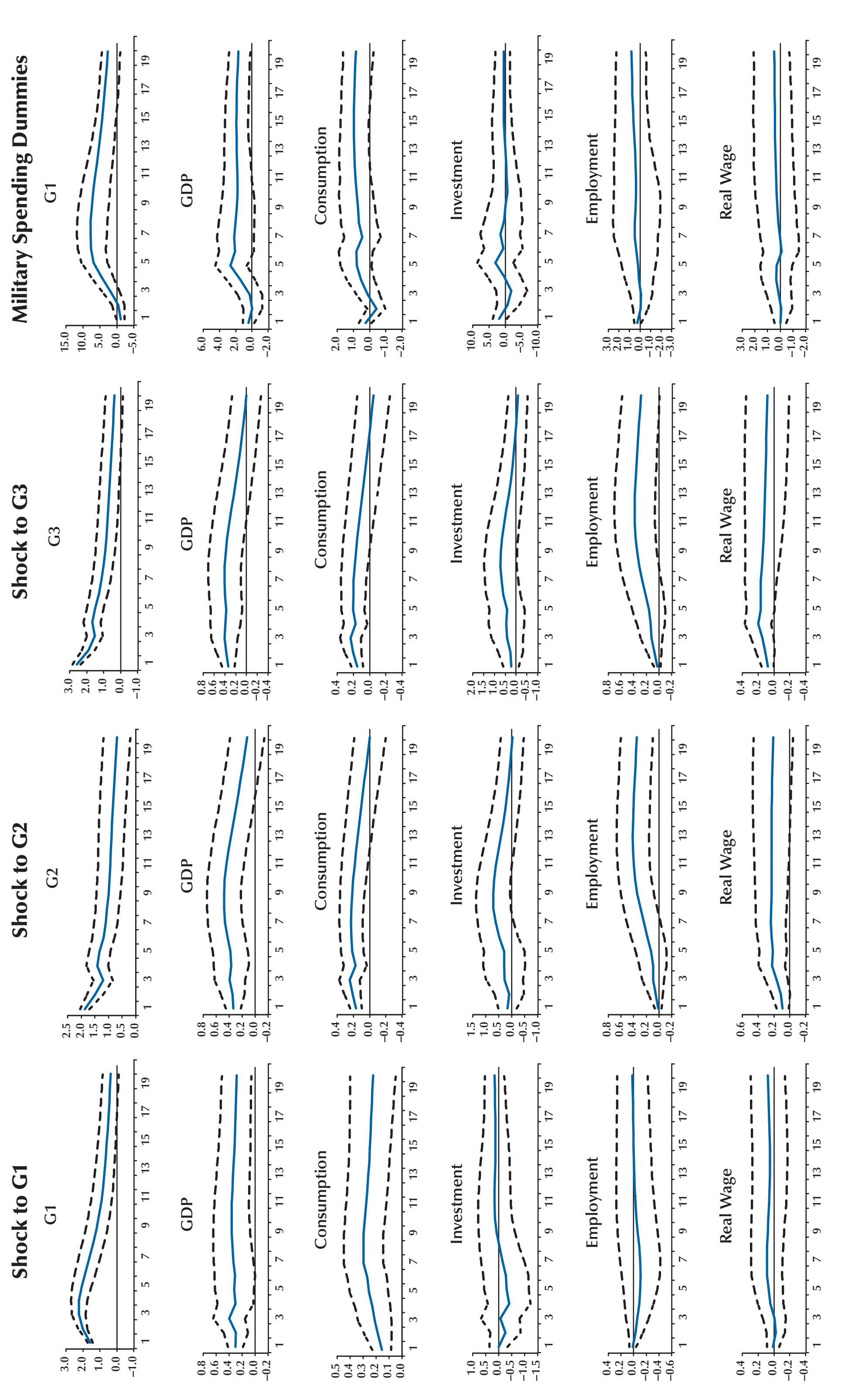
[21] The point estimate of the real wage response is positive but statistically insignificant for the government spending identifications. This result is consistent with other studies.

[22] Using employment instead of hours may change the theoretical conclusions of some macro models. Differences in the predictions might be attributed to differences between labor usage at the extensive versus the intensive margins.

[23] One difference between the two series is that hours measures action at the intensive margin while employment measures the extensive margin (see Fang and Rogerson, 2007).

Figure 4

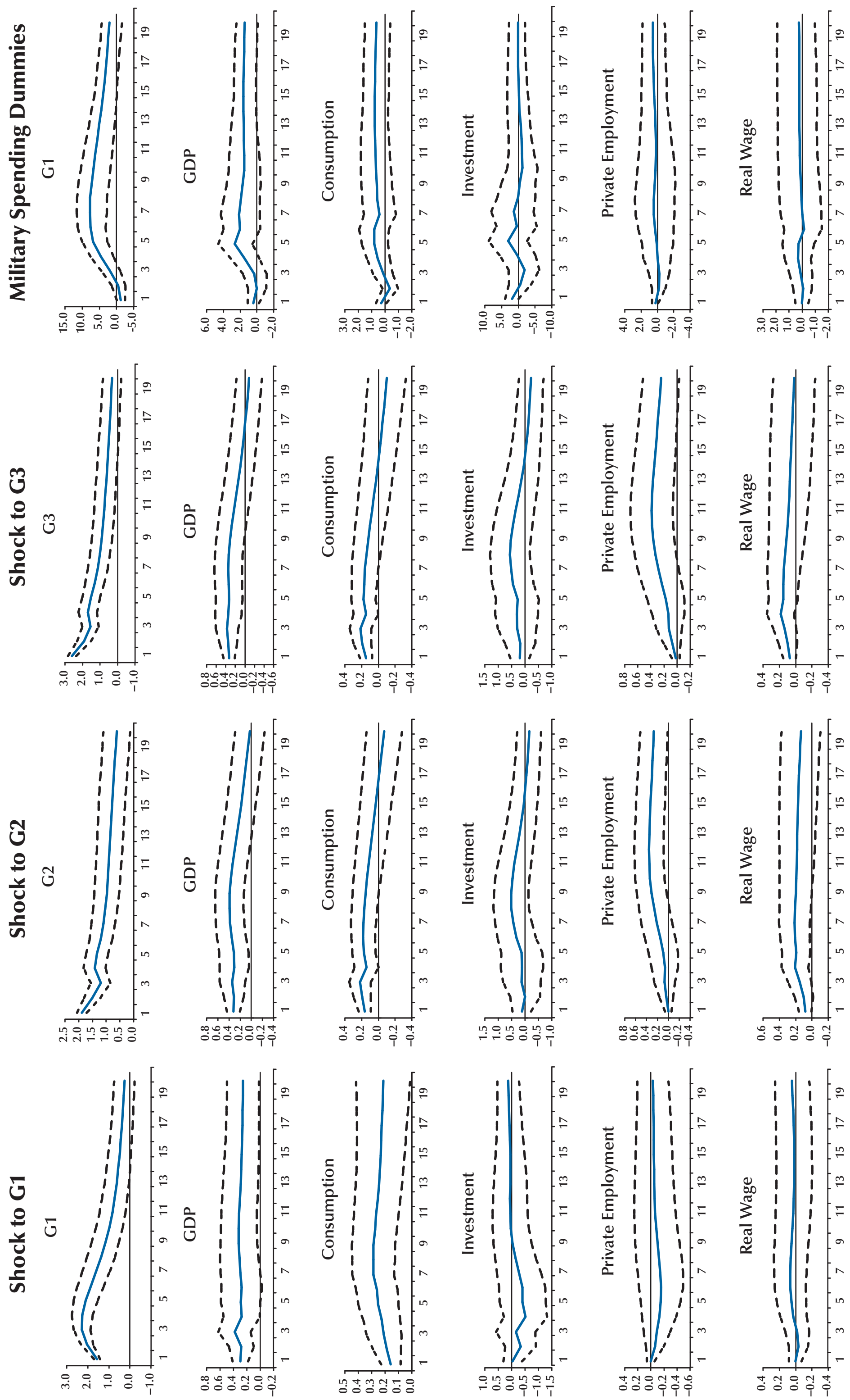
Impulse Responses Using Employment Instead of Hours



NOTE: The dotted lines indicate 95 percent confidence intervals. All variables are in real terms except for employment.

Figure 5

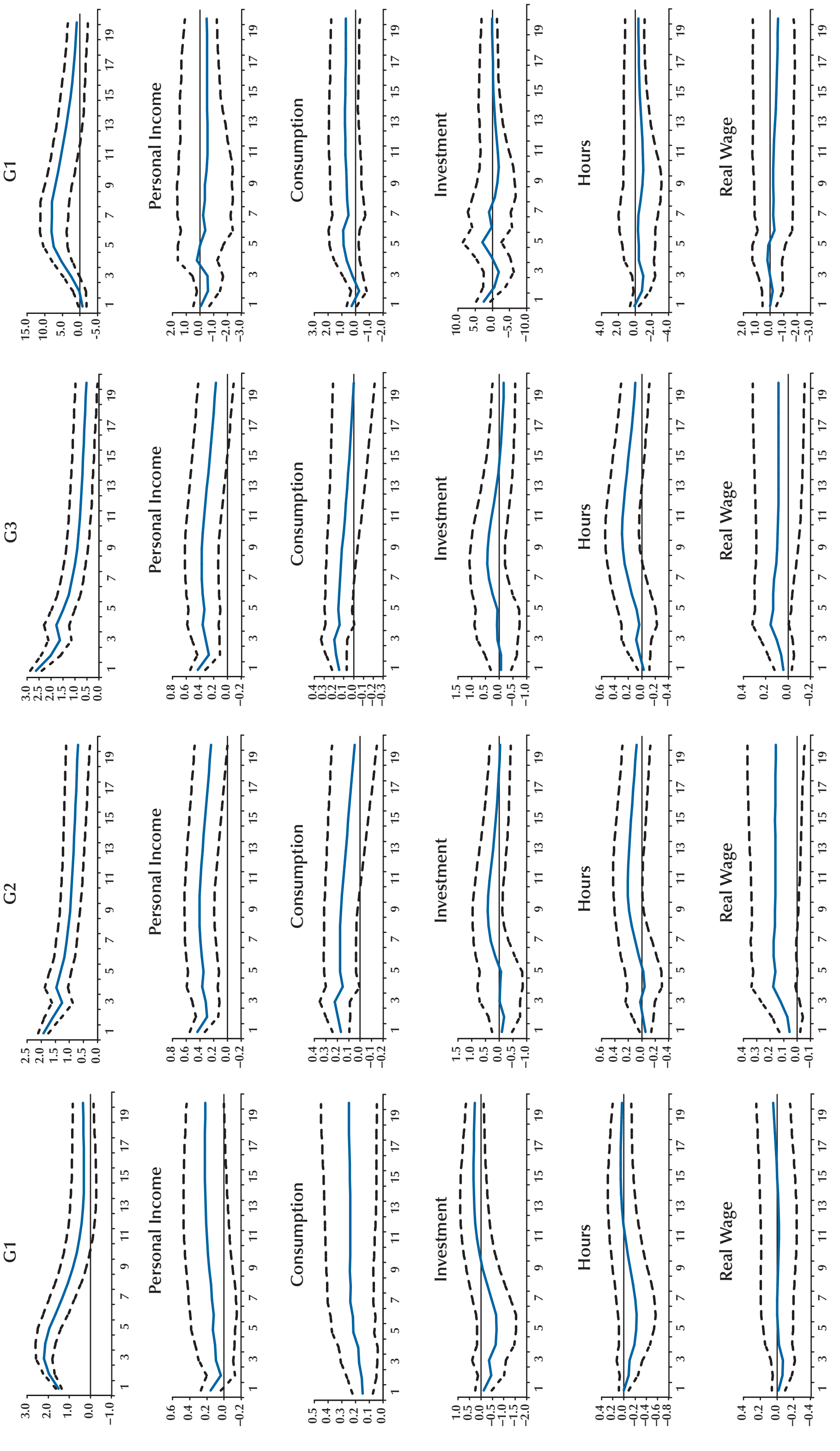
Impulse Responses Using Private Employment Instead of Hours



NOTE: The dotted lines indicate 95 percent confidence intervals. All variables are in real terms except for private employment.

Figure 6

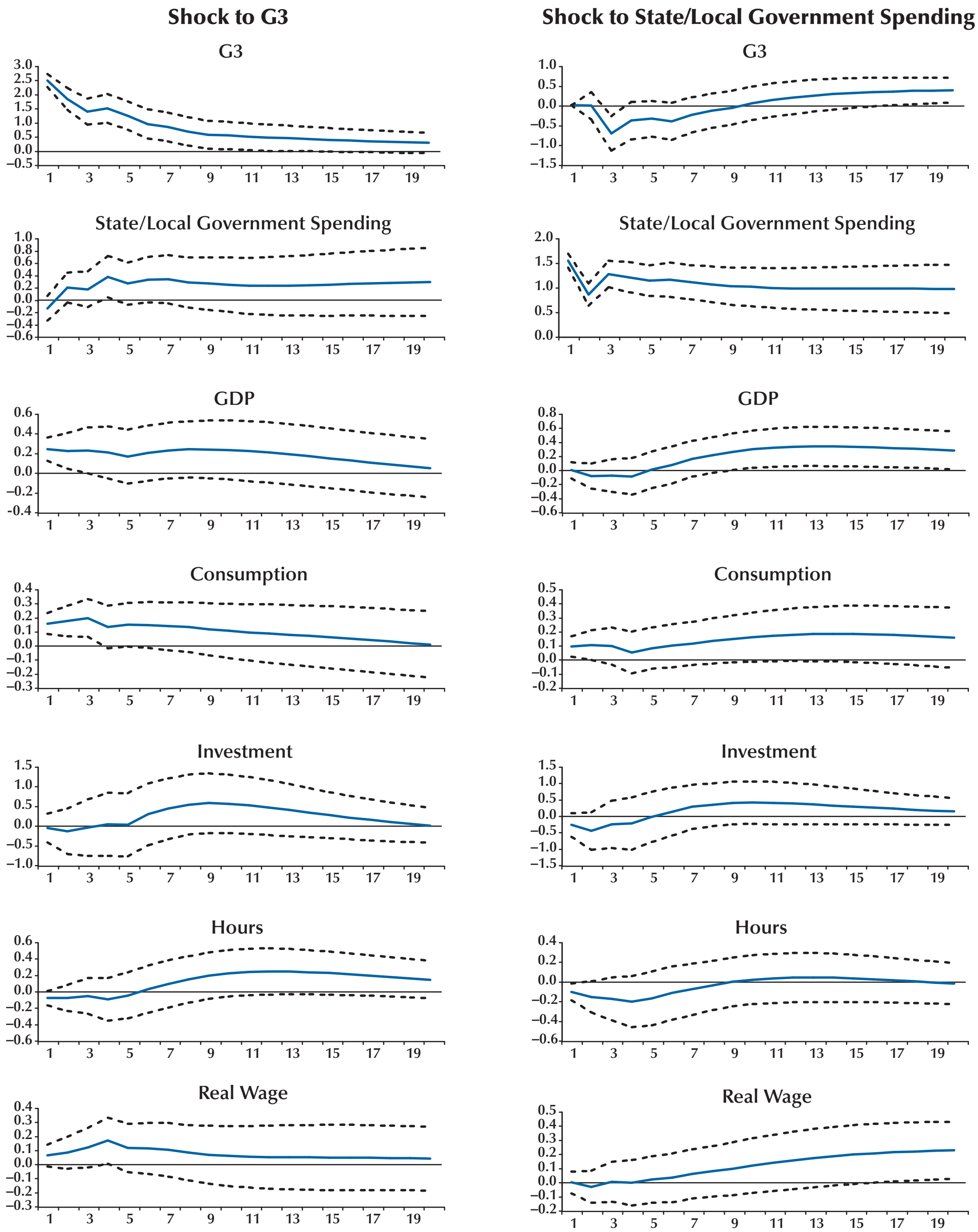
Impulse Responses Using Personal Income Instead of GDP



NOTE: The dotted lines indicate 95 percent confidence intervals. All variables are in real terms except for hours.

Figure 7

Impulse Responses Using Federal and State/Local Government Spending



NOTE: The dotted lines indicate 95 percent confidence intervals. All variables are in real terms except for hours.

similarly shaped; however, the magnitude of the PI response is muted.

The Effect of Local Spending

In some of the regressions above, we analyzed the effect of an increase in total government spending and the effect of an increase in federal government spending. The next logical step is to determine the effect of an increase in state and local spending. To accomplish this, we reestimate the structural VAR with both federal and state/local spending, along with the previously mentioned economic variables.[24] The series for state/local spending is the sum of all quarterly state and local spending.[25] The structural VAR identification orders federal spending first and state spending second.[26] Figure 7 reports the responses of the variables to shocks to both federal and state/local spending.

The impulse responses of the system of variables remain qualitatively similar to those for the baseline model when federal and local shocks are explicitly modeled. The relevant comparison is the first column of Figure 7 to the third column of Figure 3. We include the response of local spending, which begins weakly negative but rises over time.

The second column reports the response of the system to a shock to the sum of state and local spending. Federal spending remains unaffected for the first two periods after a state-and-local shock, but then it falls, whereas GDP, investment, and hours all fall upon impact. Consumption is the only measure that rises after a shock to state and local government spending.

While looking at the effects of an increase in federal government spending versus the effects of an increase in state and local spending, it is important to consider the composition of each. The major component of federal spending is defense spending, whereas state and local expenditures are primarily toward education and various public services. Therefore, federal spending shocks might be considered more exogenous and not as prone to being anticipated by the economy. With state and local spending, however, the shocks might be anticipated a quarter or two before spending actually goes up.

The two different spending aggregates might also be capturing different effects on the economy. Because state and local spending goes mostly toward public services, a positive local spending shock may not have as large a negative wealth effect. Thus, people may not feel as compelled to increase their labor supply, reducing the magnitudes of the responses of hours and output in Figure 7.

CONCLUSIONS

This paper reestablishes the conflicting predictions of identifying fiscal shocks using structural VARs versus the narrative approach using military spending dummy variables from Ramey and Shapiro (1998). Both identification schemes have strengths and weaknesses. The Ramey and Shapiro (1998) approach identifies dates of military buildup and does not require any additional identifying restrictions. However, this approach relies heavily on only four episodes in the post-World War II era. The structural VAR approach raises questions as well. Specifically, the changes it identifies in government spending might be anticipated a few quarters before they actually occur.

We find the conflicting results of the two identifications to be generally robust to the use of alternative data series. For both identifications, using employment rather than hours produces a stronger labor market response. Using PI rather than GDP produces a weaker output response. This weaker response is mitigated if the government spending shock focuses on federal expenditures.

Finally, using the structural VAR identification only, we find that disaggregating the government spending shock may be important. Although

---

24 Obviously, the military dummy identification cannot be reproduced for state and local government spending.

25 In a separate paper, Owyang and Zubairy (2007) consider the effect of state and local spending shocks on their respective regions.

26 This implies that state and local spending may respond to federal spending contemporaneously but not vice versa. We could further assume that state and local spending and federal spending are determined simultaneously.

the responses to federal spending shocks are, for the most part, preserved, shocks to state and local spending produce very different responses. In fact, the response of output to a state/local spending shock is negative. These results suggest that the state and local shocks may have important compositional and locational differences from the federal shocks.

REFERENCES

Blanchard, Olivier and Perotti, Roberto. "An Empirical Characterization of the Dynamic Effects of Changes in Government Spending and Taxes on Output." *Quarterly Journal of Economics*, November 2002, *117*(4), pp. 1329-68.

Burnside, Craig; Eichenbaum, Martin; and Fisher, Jonas D.M. "Fiscal Shocks and Their Consequences." *Journal of Economic Theory*, March 2004, *115*(1), pp. 89-117.

Edelberg, Wendy; Eichenbaum, Martin; and Fisher, Jonas D.M. "Understanding the Effects of a Shock to Government Purchases." *Review of Economic Dynamics*, January 1999, *2*(1), pp. 166-206.

Eichenbaum, Martin and Fisher, Jonas D.M. "Fiscal Policy in the Aftermath of 9/11." *Journal of Money, Credit, and Banking*, February 2005, *37*(1), pp. 1-22.

Fang, Lei and Rogerson, Richard. "Policy Analysis in a Matching Model with Intensive and Extensive Margins." NBER Working Paper No. 13007, National Bureau of Economic Research, April 2007.

Fatás, Antonio and Mihov, Ilian. "The Effects of Fiscal Policy on Consumption and Employment: Theory and Evidence." Unpublished manuscript, INSEAD, August 2001.

Faust, Jon. "The Robustness of Identified VAR Conclusions about Money." *Carnegie-Rochester Conference Series on Public Policy*, December 1998, *49*, pp. 207-44.

Favero, Carlo and Giavazzi, Francesco. "Debt and the Effects of Fiscal Policy." NBER Working Paper No. 12822, National Bureau of Economic Research, January 2007.

Francis, Neville; Owyang, Michael T.; and Roush, Jennifer E. "A Flexible Finite-Horizon Identification of Technology Shocks." Working Paper No. 2005-024D, Federal Reserve Bank of St. Louis, April 2007.

Mountford, Andrew and Uhlig, Harald. "What Are the Effects of Fiscal Policy Shocks?" SFB 649 Discussion Paper 2005-039, Humboldt University, Collaborative Research Center 649, July 2005.

Owyang, Michael T. and Zubairy, Sarah. "The Differential Effects of State and Federal Fiscal Policy." Unpublished manuscript, 2007.

Pappa, Evi. "New-Keynesian or RBC Transmission? The Effects of Fiscal Shocks in Labour Markets." CEPR Discussion Paper No. 5313, Centre for Economic and Policy Research, October 2005.

Paustian, Matthias. "Assessing Sign Restrictions." *B.E. Journal of Macroeconomics*, January 2007, *7*(1), pp. 1-33.

Perotti, Roberto. "Estimating the Effects of Fiscal Policy in OECD Countries." Working paper, Innocenzo Gasparini Institute for Economic Research, October 2004.

Perotti, Roberto. "In Search of the Transmission Mechanism of Fiscal Policy." NBER Working Paper No. 13143, National Bureau of Economic Research, June 2007.

Ramey, Valerie A. "Identifying Government Spending Shocks: It's All in the Timing." Manuscript, University of California, San Diego, July 2006.

Ramey, Valerie A. and Shapiro, Matthew D. "Costly Capital Reallocation and the Effects of Government Spending." *Carnegie-Rochester Conference Series on Public Policy*, June 1998, *48*, pp. 145-94.

Ravn, Morten; Schmitt-Grohé, Stephanie, and Uribe, Martín. "Deep Habits." *Review of Economic Studies*, January 2006, *73*(1), pp. 195-218.

Romer, Christina D. and Romer, David H. "Monetary Policy Matters." *Journal of Monetary Economics*, August 1994, *34*(1), pp. 75-88.

Romer, Christina D. and Romer, David H. "The Macroeconomic Effects of Tax Changes: Estimates Based on a New Measure of Fiscal Shocks." NBER Working Paper No. 13264, National Bureau of Economic Research, July 2007.

Rotemberg, Julio J. and Woodford, Michael. "Oligopolistic Pricing and the Effects of Aggregate Demand on Economic Activity." *Journal of Political Economy*, December 1992, *100*(6), pp. 1153-207.

Sims, Christopher. "Macroeconomics and Reality." *Econometrica*, January 1980, *48*(1), pp. 1-48.

Sims, Christopher. "Interpreting the Macroeconomic Time Series Facts: The Effects of Monetary Policy." *European Economic Review*, June 1992, *36*(5), pp. 975-1011.

Tavares, José and Valkanov, Rossen. "Fiscal Policy and Asset Returns." Working paper, Rady School, August 2003.

Uhlig, Harald. "What Are the Effects of Monetary Policy on Output? Results from an Agnostic Identification Procedure." *Journal of Monetary Economics*, March 2005, *52*(2), pp. 381-419.

Wagner, Gary A. and Sobel, Russell S. "State Budget Stabilization Fund Adoption: Preparing for the Next Recession or Circumventing Fiscal Constraints?" *Public Choice*, January 2006, *126*(1-2), pp. 177-99.

DATA APPENDIX

Variable	Original series name	Source	Additional comments
G1*,†	Government consumption expenditures and gross investment (SAAR, $ billions)	BEA	Nominal series deflated by GDP deflator
G2*,†	Government current expenditures (SAAR, $ billions)	BEA	Nominal series deflated by GDP deflator
G3*,†	Federal government current expenditures (SAAR, $ billions)	BEA	Nominal series deflated by GDP deflator
State and local government expenditures*,†	State and local government current expenditures (SAAR, $ billions)	BEA	Total government current expenditures less federal government current expenditures
GDP*,†	Gross domestic product (SAAR, $ billions)	BEA	Nominal series deflated by GDP deflator
Personal income*,†	Total personal income, total U.S. (SAAR, $ millions)	BEA	Nominal series deflated by GDP deflator
Consumption*,†	Personal consumption expenditures: nondurable goods (SAAR, $ billions); personal consumption expenditures: services (SAAR, $ billions)	BEA	Sum of nominal series deflated by GDP deflator
Investment*,†	Gross domestic investment (SAAR, $ billions); personal consumption expenditures: durable goods (SAAR, $ billions)	BEA	Sum of nominal series deflated by GDP deflator
Employment†	All employees: total nonfarm payrolls (SA, thousands)	BLS	
Private employment†	All employees: total private nonfarm payrolls (SA, thousands)	BLS	
Hours†	Nonfarm business sector: hours of all persons (SA, 1992=100)	BLS	
Product wage†	Nonfarm business sector: compensation per hour (SA, 1992=100); nonfarm business sector: implicit price deflator (SA, 1992=100)	BLS	Deflated compensation by price deflator
	GDP: implicit price deflator (SA, 2000=100)	BEA	
	Civilian labor force (SA, thousands)	BLS	Used to make series per capita

NOTE: BEA is Bureau of Economic Analysis, BLS is Bureau of Labor Statistics, SAAR is seasonally adjusted annual rate, and SA is seasonally adjusted. *Variables are in per capita terms. †We took the natural log of the series.

SOURCE: All data were obtained from Haver Analytics.

Federal Reserve Bank of St. Louis
P.O. Box 442
St. Louis, MO 63166-0442