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THE MACROECONOMIC EFFECTS OF TAX CHANGES:
ESTIMATES BASED ON A NEW MEASURE OF FISCAL SHOCKS

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ABSTRACT

This paper investigates the impact of changes in the level of taxation on economic activity. We use
the narrative record -- presidential speeches, executive-branch documents, and Congressional reports
-- to identify the size, timing, and principal motivation for all major postwar tax policy actions. This
narrative analysis allows us to separate revenue changes resulting from legislation from changes occurring
for other reasons. It also allows us to further separate legislated changes into those taken for reasons
related to prospective economic conditions, such as countercyclical actions and tax changes tied to
changes in government spending, and those taken for more exogenous reasons, such as to reduce an
inherited budget deficit or to promote long-run growth. We then examine the behavior of output following
these more exogenous legislated changes. The resulting estimates indicate that tax increases are highly
contractionary. The effects are strongly significant, highly robust, and much larger than those obtained
using broader measures of tax changes. The large effect stems in considerable part from a powerful
negative effect of tax increases on investment. We also find that legislated tax increases designed
to reduce a persistent budget deficit appear to have much smaller output costs than other tax increases.

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I. __INTRODUCTION__

Tax changes have been a major public policy issue in recent years. The tax cuts of 2001 and 2003 were passed amid firestorms of debate about their likely effects. Some policymakers claimed that the cuts would both stimulate the economy in the short run and increase normal output in the long run. Others argued that they would raise interest rates and lower confidence, and thereby reduce output in both the short run and the long run.

That views of the effects of tax changes vary so radically largely reflects the fact that measuring these effects is very difficult. Changes in taxes occur for many reasons. Some legislated tax changes are passed for philosophical reasons or to reduce the budget deficit. Others are passed because a war is in progress or because the economy is weak and predicted to fall further. And many major changes in revenues are not legislated at all, but occur automatically because the tax base varies with the overall level of income, or result from changes in stock prices, inflation, and other non-policy forces. Because the factors that give rise to tax changes are often correlated with other developments in the economy, disentangling the effects of the tax changes from the effects of these underlying factors is inherently difficult.

A concrete example of this difficulty concerns the impact of legislated countercyclical tax changes. Suppose policymakers sometimes deliberately change taxes to prevent recessions or booms. If the policies are well timed and have the desired effects, output will grow normally following these changes. And if such changes make up an important part of overall tax changes, a statistician confronted with data on tax changes and the behavior of output will tend to underestimate the effects of tax changes on output.

More generally, the central reason that estimating the effects of tax changes is so difficult is that

there are pervasive possibilities for omitted variable bias. In the case of legislated countercyclical tax changes, some factor is both affecting the future path of output and causing policymakers to change taxes. In the case of the automatic response of taxes to output, any omitted factor that affects output also affects taxes. And many non-policy forces other than output that affect revenues, such as movements in stock prices, may have direct effects on output. Failure to take the omitted variables into account will result in inaccurate estimates of the macroeconomic effects of tax changes.

Unfortunately, controlling for the omitted variables is exceedingly difficult. The automatic response of taxes to output can be addressed by cyclical adjustment of revenues, although one has to be concerned about the accuracy of the adjustment. But controlling for all other non-policy factors that affect revenues and that could directly affect output is essentially impossible, and there are estimates of the contribution of these factors to changes in revenues only for the recent past. And for legislated countercyclical changes, the problems are even more severe. Here the omitted variable is information available to policymakers about how output would evolve in the absence of tax changes. However, useful numerical forecasts of any sort are largely non-existent before the mid-1960s, and forecasts based on the crucial assumption of no policy change are rarely available even for recent tax changes.

We therefore pursue an alternative approach. There exists a vast narrative record describing the history and motivation of tax policy changes. We use the narrative history to isolate legislated tax changes from changes in revenues related to economic conditions and other non-policy influences. We use the information on motivation to separate legislated tax changes into those that can legitimately be used to measure the macroeconomic effects of tax changes and those that are likely to be contaminated by other developments. We then use the legitimate observations to derive estimates of the effects of tax changes on output that are likely to be less biased than previous estimates.

The narrative analysis, which is described in Section II of the paper, is clearly the foundation of the study. We use sources such as presidential speeches, the Economic Report of the President, and the reports of Congressional committees to identify the motivation for each legislated tax change. Most fundamentally, we separate legislated tax changes into those taken to return output growth to normal (or

to maintain normal growth in the anticipation of other changes), and those taken for other reasons. The changes taken to achieve or maintain normal growth, which for want of a better term we call endogenous, are likely to lead to biased estimates of the effects of fiscal policy. The changes not motivated by current or projected economic conditions, which we call exogenous, are appropriate for measuring the effects of tax changes on output. These exogenous changes constitute our new measure of fiscal shocks. We also look at finer breakdowns of motivation. The endogenous legislated tax changes can be divided into truly countercyclical changes and changes undertaken because government spending was changing. The exogenous legislated tax changes can be divided into changes taken to reduce an inherited budget deficit and those taken to spur long-run growth. In addition to identifying the motivation for each tax change, we also use the narrative record to identify the timing and magnitude of the revenue effects.

This cataloging and classifying of all legislated U.S. tax changes since World War II reveals some interesting patterns and trends, which we discuss in Section III. The most striking finding is that endogenous tax changes virtually disappeared after 1980: both countercyclical and spending-driven tax changes were common in the 1960s and 1970s, but have been very rare in the last 25 years. Tax changes for long-run growth (broadly defined to include such philosophical motivations as shrinking the size of government and promoting entrepreneurship) are relatively common throughout the postwar era. A comparison of our new measure of exogenous tax changes with the behavior of cyclically adjusted revenues, a more common measure of tax changes, shows that there are crucial differences between the two series.

Armed with our series of exogenous tax changes, we examine the effects of taxes on real output. In Section IV we present baseline estimates of these effects using our new measure, and investigate whether the results differ from those derived using broader measures of tax changes. We find that exogenous tax increases have a large, rapid, and highly statistically significant negative effect on output. The estimated impact is larger and more significant than when broader measures, such as the change in cyclically adjusted revenues or all legislated tax changes, are used. This suggests that the bias economic reasoning predicts could arise from using the broader measures is substantial. Looking at the behavior of

output when we disaggregate the motivations for tax changes more finely provides additional insight into the baseline estimates. For example, among endogenous legislated tax changes, the behavior of output following countercyclical tax changes is only moderately different from its behavior following exogenous changes, while its behavior following spending-driven tax changes is radically different. Among exogenous tax changes, we find that tax increases motivated by a desire to reduce an inherited deficit appear to have much smaller effects on output than tax changes taken for long-run reasons.

In Section V, we test the robustness of our estimates along a number of dimensions. Most importantly, we include a number of control variables in our baseline regression. Though our measure of exogenous tax changes should not be systematically correlated with current or near-term projected developments, such correlation is possible in small samples. For this reason, we reestimate our regression controlling for the state of the economy, monetary policy shocks, and the behavior of government spending. We find that the estimated effect of exogenous tax changes on output is extremely robust.

In Section VI, we extend our findings in four ways. First, we ask whether tax changes have important effects through expectations. Second, we test whether the effects of tax actions have changed over time. Third, we look at the effects of our new measure of fiscal shocks on inflation and unemployment, as a way of gauging whether demand-side or supply-side effects appear to dominate. Finally, we examine how exogenous tax changes affect the components of GDP, such as consumption, investment, and imports. The most striking finding of this exercise is that tax increases have a large negative effect on investment.

The literature examining the effects of changes in the level of taxes on output is relatively small. Some early studies, such as Andersen and Jordan (1968), simply regress output growth on measures of high-employment spending and receipts. Two more sophisticated recent studies are Blanchard and Perotti (2002) and Perotti (1999). Like the earlier studies, these studies assume that once one corrects for the impact of economic activity on revenues and controls for the behavior of government spending, changes in revenues are uncorrelated with other determinants of output growth. Thus, they do not address the possibilities of forward-looking policy or of correlations between non-cyclical, non-policy influences on

revenues and other determinants of output growth.

A related literature looks at the possibility of "expansionary fiscal contractions" (for example, Giavazzi and Pagano, 1990, and Alesina and Perotti, 1997). However, these papers also measure tax changes using cyclically adjusted or actual revenues. We extend work on this possibility not only by employing a sounder measure of fiscal shocks, but also by looking at actions designed to lower budget deficits in less extreme circumstances than the ones considered in previous studies.

In its analysis of the components of GDP and the transmission mechanism, our study ties in with a larger literature. Studies by Kormendi (1983), Cardia (1997), and a host of others surveyed in Gale and Orszag (2004) analyze the impact of tax changes on consumption. The estimated impact of tax increases on consumption in these studies ranges from roughly no effect to a substantial negative effect. The results using our new measure of fiscal shocks support the view that the effects are large and negative.

Methodologically, the approach we use is related to our earlier work on monetary policy (Romer and Romer, 1989, 2004). As with fiscal policy, estimating the effects of monetary policy is difficult both because measures of policy can respond automatically to economic developments, and because policymakers often adjust policy on the basis of information about prospective economic developments. Our work addresses these difficulties by bringing in information from the narrative record about the motives for policy changes. The research on fiscal policy that is most similar in methodology is the work of Ramey and Shapiro (1998) examining the effect of changes in government spending. Using news reports in Business Week and other historical accounts, Ramey and Shapiro identify major military build-ups that occurred for reasons unrelated to the state of the economy or prospective macroeconomic developments. Because the build-ups were the result of outside forces, they can be used to estimate the impact of government purchases on the economy. Ramey and Shapiro find that this approach leads to a view of the impact of government purchases that differs considerably from the conventional wisdom.

II. **DERIVING THE NEW MEASURE OF FISCAL SHOCKS**

The first, and by far the most time-consuming, part of our study is the narrative analysis of tax changes. The goal of this analysis is not simply to construct a narrative history of postwar tax changes. Rather, the analysis is designed to provide an input into the empirical estimation. We use the historical record to identify the motivation, revenue effects, and other characteristics of legislated postwar tax changes. This analysis allows us to separate tax changes into those that can successfully be used to estimate the macroeconomic effects of tax changes, and those that cannot.

A. **Sources**

The sources for the narrative analysis are primary documents produced by policymakers at the time. For the executive branch, one key source is the Economic Report of the President (abbreviated in what follows as Economic Report). This document is released each January and typically provides a detailed discussion of the motivation, revenue effects, and nature of tax changes in the previous calendar year. A second key source is presidential speeches.[1] Tax actions are often discussed in the State of the Union Address, the Annual Budget Message, and addresses proposing or upon signing the legislation. Because tax actions are often first proposed during presidential campaigns, we also examine the acceptance speeches at the nominating conventions. The other two executive branch documents that we consult systematically are the Annual Report of the Secretary of the Treasury on the State of the Finances (abbreviated as Treasury Annual Report; the Treasury stopped publishing detailed annual reports in 1981) and the Budget of the United States Government (abbreviated as Budget).

For the legislative branch, our main sources are the reports prepared on each tax bill by the House Ways and Means Committee and the Senate Finance Committee. When the bill appears to have changed substantially after the reports, we examine the floor debate in the Congressional Record. The Conference

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[1] Presidential speeches and other presidential papers are available online from John Woolley and Gerhard Peters, The American Presidency Project (www.presidency.ucsb.edu).

report prepared on each bill is sometimes a useful source of revenue estimates. Likewise, summaries prepared by the Joint Committee on Internal Revenue Taxation (after 1975, the Joint Committee on Taxation) often provide detailed information about the timing and revenue effects. The reports of the Congressional Budget Office, which was created in 1974, are also a useful source of revenue estimates.

For Social Security tax changes, we consult two additional sources. The Social Security Bulletin typically contains one or two articles on the motivation and revenue effects of Social Security tax actions. Similar material is sometimes also contained in the Annual Report of the Board of Trustees of the Federal Old Age and Survivors Insurance Trust Fund.

B. Identifying Legislated Tax Changes

The first step in the analysis is to identify all significant legislated tax changes in the period 1947 to 2006. To do this we simply look for tax changes that receive more than incidental mention in our sources. Any measure, including executive actions, that receives serious discussion is included in our enumeration. Measures that are referred to only in passing or are discussed only in lists of all measures that affected revenues over some period are excluded. Since this approach leads us to include even tax changes with very small revenue effects, we feel this is a fundamentally sensible listing of the important tax policy changes over the postwar era.

In considering tax changes, we limit ourselves to actions that actually change tax liabilities from one quarter to the next. Because our interest is in the effects of changes in the actual level of taxation, a new law that merely extends an existing tax does not count as a change for our purposes.

Identifying legislated tax changes is a useful exercise in its own right. Focusing on legislated changes is clearly one way to address the fact that tax revenues are affected by the state of the economy. It also eliminates the effect of other non-policy factors, such as inflation and the distribution of income, which may affect tax revenues.

C. **Classifying Motivation**

A second component of the narrative analysis is the identification of the primary motivation for each legislated tax change. The classification that we use reflects the effects we are trying to measure. We are interested in the effects of tax changes on real economic performance. Therefore, we want to separate legislated tax changes into those that are motivated by the state of the economy and those that are not. More specifically, we want to test whether tax changes cause output growth to differ from normal. Therefore, in our classification scheme what we want to separate are changes aimed at keeping growth at normal and changes that are either aimed at moving growth away from normal, or taken without regard to their effects on growth. We will refer to tax changes aimed at keeping growth at normal as "endogenous" and tax changes taken for other reasons as "exogenous."

Endogenous Tax Changes. The most obvious kind of endogenous tax changes are countercyclical actions. If the economy is predicted to decline and Congress passes a tax cut to mitigate the downturn, this is clearly an action motivated by a desire to return growth to normal. Likewise, if the economy is predicted to grow faster than normal and taxes are raised to slow growth, this is an endogenous, countercyclical action. Such countercyclical actions are precisely the kinds of observations whose inclusion could bias estimates of the effects of tax actions on the real economy.

In identifying a countercyclical motivation, we take policymakers' statements at face value. However, it is obviously possible that policymakers invoke countercyclical arguments when other motivations are in fact key, or that their perceptions of normal growth are overly optimistic. Both of these possibilities may cause us to overclassify actions as countercyclical, and hence endogenous. Therefore, taking policymakers at their word causes us to err on the side of excluding legitimate observations, rather than including illegitimate ones. This strategy may make our estimates of the effects of tax changes less precise, but ensures that the bias in the point estimates is as small as possible.

There are at least two ways to deduce from the narrative record whether actions were intended to return growth to normal. Often, it is simply discussed directly. Additionally, if output is growing normally, the unemployment rate typically will not rise or fall. Therefore, policymakers' predictions of

what would happen to unemployment provide a way of judging if a tax change is intended to return growth to normal or to generate abnormal growth.

An even more important subcategory of endogenous tax actions are tax changes motivated by spending changes. Suppose that policymakers increase spending, perhaps because the country is involved in a war or because a new social insurance program is added, and they decide to raise taxes to pay for it. We feel that it is reasonable to assume that policymakers understand that the spending change will tend to push growth above normal if they do not make the counteracting tax change. Therefore, this is, in effect, a tax change aimed at keeping growth normal. Classifying such a spending-driven tax change as endogenous is crucial because the tax change is clearly correlated with another development, a spending change, that is likely to affect output.[2]

Exogenous Tax Changes. We classify as exogenous any tax change not motivated by a desire to return output growth to normal. This category therefore encompasses a wide range of motivations. The unifying principle is that these are either changes designed to move growth away from normal or changes taken irrespective of their effects on growth. In essence, they are tax changes not taken in response to information about future economic developments. As a result, they are legitimate observations to use to estimate the macroeconomic effects of tax policy.

One common type of action in this category are tax increases to deal with an inherited budget deficit. An inherited deficit reflects economic conditions and budgetary decisions in the past, not current economic conditions or contemporaneous spending changes. If policymakers decide to raise taxes to reduce such a deficit, this is not a change motivated by a desire to return growth to normal or to prevent abnormal growth. So it would be exogenous.[3]

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[2] Also included as endogenous tax changes are procyclical actions. On one occasion (the Tax Reduction and Simplification Act of 1977), policymakers acknowledged that the economy was growing faster than normal, but they nevertheless cut taxes because they wanted even faster growth. Because we would classify such a change as endogenous if growth were below normal, treating it as exogenous when growth begins above normal would tend to bias the results toward finding a large effect of tax changes on output. For this reason, we classify procyclical actions as endogenous. Because there is only one such action, we do not emphasize this third subcategory of endogenous actions.

[3] One difficult case that occurs periodically in the 1980s and 1990s is a deficit reduction package that includes both a tax increase and a spending decrease. Since such packages are not motivated by the desire to return growth to

All other tax changes in this category can be thought of as being, at some level, motivated by a desire to raise long-run growth. A very common tax cut is one in which policymakers say that the economy is doing fine (output is growing normally), but they want output to grow faster than normal. Occasionally the motivation is expressed as a desire for a temporary boom, but more often it is expressed as a belief that the tax reduction will raise the growth rate of potential output. Tax cuts for philosophical reasons, such as to shrink the size of government or for fairness, also typically have at their core a belief that they will raise long-run growth. Because it is often hard to separate these various motivations, we combine them under the broad rubric of long-run growth. But, this is admittedly a catchall category of exogenous tax changes not motivated by a desire to deal with the budget deficit.

Applying the Criteria. Armed with this classification scheme, it is usually straightforward to categorize the motive for each action. Typically, there is a single motive emphasized in a source, and there is substantial agreement across sources. When sources disagree, we attempt to ascertain what the bulk of the evidence suggests was the motive. Likewise, when multiple motives are mentioned, we attempt to see if one is clearly emphasized over the others.

Occasionally, there appear to genuinely be multiple motivations for a single bill. This is the case, for example, with the Economic Recovery and Tax Relief Reconciliation Act of 2001. A large tax cut was originally proposed during the 2000 presidential campaign, when the economy was growing normally. The key motivations appear to have been a belief in limited government and a desire to stimulate long-run growth. Thus, at a fundamental level, it was not a tax cut aimed at restoring growth to normal, and so would be classified as exogenous. However, by the time the cut was passed in June 2001, concerns about a developing recession were frequently mentioned. The plan was also changed to include an immediate rebate to jumpstart the economy, rather than being phased in beginning in 2002 as called for in the original proposal. In this and in the few other cases like it, we find that it is possible to apportion

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normal, they are exogenous in our classification scheme. However, these cases have the unfortunate characteristic that the spending change and the tax change are clearly correlated, and are likely to affect output in the same direction. As a result, the inclusion of these episodes in the empirical analysis may lead to an overestimate of the effects of tax changes. Fortunately, the most problematic of these cases for our study, times when the tax increase is small and the spending decrease is large, are rare. Nevertheless, in the empirical work we test whether deficit-driven tax changes have different effects from other exogenous tax changes.

motivation quite well. The changes to taxes in 2001 that were added to the bill because of concern about the recession are endogenous (for countercyclical purposes). The reductions in taxes in 2002 and later years are exogenous (for long-run growth). Fortunately, we find that cases such as this one, where the stated motives change substantially over time or suggest a troubling mix of endogenous and exogenous considerations, are uncommon.

D. <u>Timing and Revenue Estimates</u>

In addition to identifying the motivation for each tax change, we identify other characteristics from the narrative record. One obvious one is the quarter that the tax change took effect. The narrative record is virtually always explicit about whether the action took place immediately upon signing, at a certain date in the future, or was phased in over several dates. To convert these specific dates to a quarterly value we follow a simple rule: if the effective date is on or before the midpoint of the quarter, it is assigned to that quarter; if it is after the midpoint of the quarter, it is assigned to the subsequent quarter.

In our baseline analysis, we date the changes according to when liabilities changed, not according to when households and firms learned that tax liabilities would be different. For example, we treat the tax increases resulting from the Social Security Amendments of 1983 not as a single large tax increase in 1983Q2, when the bill was signed, but as five separate tax increases over the period 1984 to 1990, when taxes actually changed. This approach is consistent with an extensive literature, much of it based on natural tax experiments, that finds consumers respond strongly to current disposable income.[4] In Section VI, however, we investigate the impact of assigning the revenue effects to the dates that bills were passed rather than the dates the changes took effect.

Deducing the revenue effects of the tax changes is often challenging. Conceptually what we want is the effect of a tax change on liabilities at the prevailing level of GDP. Since retrospective figures are rarely available and contemporaneous documents generally provide enough information for us to

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[4] See, for example, Shapiro and Slemrod (1995), Parker (1999), Souleles (1999), and Johnson, Parker, and Souleles (2006).

construct such estimates, we rely on contemporaneous sources. Whenever possible, we derive a consensus estimate from multiple sources. We place particular emphasis on the estimates in the Economic Reports because they are the most straightforward and appear to be consistent over time. We also find that the Conference report on a bill often provides detailed revenue information for the final form of the legislation.

We express all revenue effects at an annual rate. Often, the revenue effects are simply stated this way in our sources. Sometimes, the sources give the expected effects of the change on the path of revenues. In these cases, we use the estimated impact of the action in its first full calendar year (which, for the many tax actions that take effect on January 1, is equivalent to the effects in the first year). Occasionally, if only fiscal year data are available, we use the estimated impact in the first full fiscal year after the change.

Finally, tax changes often have retroactive components. We treat these components as one-time levies or rebates in the quarters the bills that included them were passed. For example, suppose a tax increase is retroactive for one quarter and this extra quarter will bring in $1 billion in revenues. In the quarter the tax increase is passed, there is an extra increase of $4 billion (because of conversion to annual rates). Then, in the next quarter, there is a tax cut of $4 billion because the retroactive feature disappears. As this example suggests, accounting for the retroactive features of tax changes often makes our estimates quite volatile on a quarter-to-quarter basis. But these short-run ups and downs should have little impact on the broad features of our estimates of the effects of tax changes. Nonetheless, as a robustness check, we examine the effects of excluding the retroactive features from our series for tax changes.

E. Application of the Narrative Methodology

A companion background paper (Romer and Romer, 2007) provides more information about our analysis of the narrative record and discusses our application of the methodology for each major legislated tax change since 1947. In every case we give our overall assessment of what the record reveals about the motivation and the revenue effects of the change. More importantly, we attempt to give enough

quotations and citations that other researchers can see why we classify tax changes as we do and can check our analysis.

To give a sense of how we apply our procedures, Exhibits 1 through 4 reproduce four of our narrative summaries. We choose the examples to illustrate the four different types of motivation: a countercyclical change, a tax change tied to a spending change, a change for deficit reduction, and a change to encourage long-run growth. We also try to show changes from different eras.

III. <u>POSTWAR LEGISLATED TAX CHANGES</u>

Our narrative sources identify 49 tax laws (and administrative actions) between 1947 and 2006 that significantly changed tax liabilities. These laws changed taxes in 82 quarters. A number of these quarters had tax changes of multiple types. In all, we identify 104 separate quarterly tax changes. Of these, 65 were exogenous and 39 were endogenous.

As described above, the exogenous tax changes should be valid observations for investigating the macroeconomic effects of tax changes. The first step in using this new series of fiscal shocks is to discuss some of its properties. Have there been changes over time in the number, size, and specific motivation of exogenous tax changes? It is also possible to examine trends in the endogenous changes. Finally, it is useful to examine how our new series on exogenous tax changes compares with the change in cyclically adjusted revenues, the most commonly used alternative indicator of tax changes.

Our estimates of the revenue effects of each tax change are in nominal terms. Before one can sensibly discuss trends over time or include the series in an empirical framework, we need to put the values on a consistent basis. To do this, we express each revenue effect as a percent of nominal GDP in the quarter the change occurred.[5]

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[5] The data on nominal GDP are from the National Income and Product Accounts, Table 1.1.5 (www.bea.gov, downloaded 9/16/06).

A. Exogenous Tax Changes

Panel (a) of Figure 1 shows our series of exogenous tax changes since 1947.[6] The graph shows that there have been both positive and negative exogenous tax changes throughout the postwar era. Certain eras, however, have seen particularly frequent changes. The early 1960s, the late 1970s, and the 1980s were periods of rapid and extreme exogenous tax actions. While some of these changes were fairly small, quarterly changes of ½ to 1 percent of GDP have been fairly common. The largest quarterly exogenous tax action was a cut in taxes of nearly 2 percent of GDP in 1948Q2.

Panel (b) shows the two types of exogenous tax changes, those for deficit reduction and those for long-run growth, separately. Not surprisingly, the vast majority of tax actions in the catchall category of long-run growth are tax cuts. However, because this group includes tax reforms for efficiency and fairness, it contains some tax increases. For example, the Tax Reform Act of 1976 closed tax loopholes that were thought to be encouraging efforts at tax evasion. The most significant tax cuts to stimulate long-run growth are well known: the 1948 tax cut passed over Truman's veto; the 1964 Kennedy-Johnson tax cut; the 1981 Reagan tax cut; and the 2001 and 2003 Bush tax cuts.

All of the deficit-driven tax changes were tax increases. The figure makes clear that while deficit-driven tax increases have occurred throughout the postwar era, they were most prevalent in the early and mid-1980s. Many of these deficit-driven tax actions were related to Social Security: of the 26 actions in the category, 15 were designed to deal with the long-run solvency of the Social Security system. The Social Security Amendments of 1977 and 1983, in particular, were major tax actions that raised taxes in a number of steps and did not simultaneously increase benefits. The largest deficit-driven tax increases not related to Social Security were those contained in the Tax Equity and Fiscal Responsibility Tax Act of 1982, and the Omnibus Budget Reconciliation Acts of 1987, 1990, and 1993. The first two of these were Reagan-era measures; the third was the Bush tax increase that defied his famous "Read my lips: no new taxes" campaign speech; and the fourth was the Rubinomics-defining tax

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[6] As described above, many of the tax laws contain retroactive provisions, which tend to make the series somewhat volatile. To facilitate the identification of trends over time, the figures show the series excluding retroactive provisions.

increase of the early Clinton administration.

B. Endogenous Tax Changes

Panel (a) of Figure 2 shows our series on endogenous tax changes. The graph shows that there were many endogenous actions before 1980 and almost none since then. Panel (b) shows the two subcategories of endogenous actions, countercyclical and spending-driven, separately.

The heyday for countercyclical tax changes was the ten years from 1965 to 1975. We find no actions in the 1950s for which the primary motivation was a desire to counteract current or prospective economic conditions. The two largest countercyclical tax changes were the 1968 tax surcharge and the 1975 tax cut. Countercyclical actions were nonexistent in the 1980s and 1990s. We find, however, that countercyclical motives were present for part of the 2001 Bush tax cut and all of the post-September-11th cuts contained in the Job Creation and Worker Assistance Act of 2002.

Spending-driven tax actions were virtually always tax increases. The two exceptions occurred in 1954 and 1998: Eisenhower and Clinton emphasized that taxes were being reduced in these years because spending was being cut. A large fraction of the spending-driven tax increases were related to Social Security: 12 of the 22 changes in this category were tax increases specifically tied to contemporaneous increases in Social Security spending. The most extreme example was the substantial payroll tax increase to pay for the introduction of Medicare in 1965. However, there were numerous tax increases to finance expansions of coverage and increases in benefits in the 1950s, 1960s, and early 1970s. The largest spending-driven tax increases occurred during the Korean War.

C. Comparison with Cyclically Adjusted Revenues

The change in cyclically adjusted revenues is the standard macroeconomic measure of tax changes. For that reason, it is useful to see how our measure of exogenous tax changes compares with this series.

Cyclical adjustment is designed to deal with the fact that tax revenues rise and fall with GDP

automatically because many taxes are a function of income or expenditure. Cyclically adjusted revenues are calculated as what revenues would be if GDP were at its normal trend level. Estimates of cyclically adjusted revenues are constructed by the Congressional Budget Office.[7] To make the comparison to our series as direct as possible, we divide cyclically adjusted revenues by the chain-type price index for GDP to convert it to a real series, and then compute the change in real cyclically adjusted revenues. This change is then normalized by dividing by real GDP. Thus, both this measure of cyclically adjusted revenues and our series of exogenous tax changes show the change in revenues as a percent of GDP.[8]

One complication is that the quarterly data on cyclically adjusted revenues are not available before 1960. However, over the period where the data are available, the change in the gap between actual and cyclically adjusted real revenues can be predicted almost perfectly using real output growth. As a result, it is straightforward to project the change in real cyclically adjusted revenues backward. To do this, we first estimate the relationship between the change in the gap between actual and cyclically adjusted revenues and output growth over an adjacent period for which we have data on cyclically adjusted revenues. We then apply the estimated relationship to the earlier period and combine the fitted values with data on the change in actual revenues to obtain estimates of the change in cyclically adjusted revenues.[9]

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[7] We use the unpublished quarterly values, which are generated in a manner consistent with CBO's annual figures.

[8] The data on the price index for GDP are from the National Income and Product Accounts, Table 1.1.4 (downloaded 9/15/06). We calculate real GDP by dividing nominal GDP by the price index for GDP. The obvious difference in normalization is that for our series we divide the nominal revenue effects by nominal GDP and for cyclically adjusted revenues we divide the change in real revenues by real GDP. This difference is necessary because much of the change in nominal cyclically adjusted revenues from one quarter to the next is due to inflation, while the revenue effects of particular laws are relatively unaffected by inflation.

[9] Specifically, we regress the change in the difference between the logarithms of actual and cyclically adjusted real revenues on a constant and the growth rate of real GDP. The sample period is the first decade for which we have quarterly data on cyclically adjusted revenues (1960Q2–1970Q1). This yields:

$$\Delta\,[\ln R_t - \ln C_t] \;=\; -0.019 \;+\; 1.88\,\Delta\ln Y_t,$$
$$(0.001)\quad(0.05)$$
$$R^2 = 0.97,\ \text{s.e.e.} = 0.0029,\ \text{D.W.} = 1.46.$$

R is actual real revenues, measured as the ratio of federal current receipts from the National Income and Product Accounts, Table 3.2 (downloaded 9/17/06) to the chain-type price index for GDP. C is cyclically adjusted real revenues. Y is the chain-type quantity index for GDP from the National Income and Product Accounts, Table 1.1.3 (downloaded 9/15/06). The numbers in parentheses are standard errors. The variable we wish to construct, the change in real cyclically adjusted revenues divided by real GDP, equals $[\Delta R_t - \Delta(R_t - C_t)]/\text{Real GDP}_t$. This is approximately equal to $\{\Delta R_t - [\Delta(\ln R_t - \ln C_t)]R_t\}/\text{Real GDP}_t$. Our constructed estimate for the period 1947Q2 to 1960Q1 is therefore $(\Delta R_t - Z_t R_t)/\text{Real GDP}_t$, where Z_t is the fitted value from the regression.

Panel (a) of Figure 3 compares cyclically adjusted revenues with our measure of exogenous tax changes. This graph shows that most of the movements in our series of exogenous tax changes are reflected in movements in cyclically adjusted revenues. There are sometimes important differences in timing and noticeable differences in magnitude, but overall there is a substantial relationship, especially in the large exogenous tax changes.

At the same time, there are crucial differences between the two series. Many movements in cyclically adjusted revenues have no counterpart in our series. Some of the largest of these correspond to endogenous legislated tax changes that we deliberately exclude. This is true, for example, of the Korean War tax increases in the early 1950s and the 1975 tax cut. These substantial differences suggest that eliminating endogenous actions makes our new series of exogenous tax changes very different from the standard indicator of tax policy.

Another key source of differences between the two series is our focus on legislated changes. The easiest way to see this is to compare the change in cyclically adjusted revenues with our series of all legislated tax changes (both exogenous and endogenous). This comparison is shown in panel (b) of Figure 3. Many of the largest differences in the previous comparison disappear when endogenous changes are included in our measure. But, some large differences still remain. Notice, for example, the sustained increases in cyclically adjusted revenues in the mid- and late 1990s, a time when there was only one minor legislated tax change. This rise was due in considerable part to the booming stock market (Congressional Budget Office, 2002, pp. 50-52). The early and late 1970s are another period when non-policy factors were important. Rapid inflation, fueled in part by oil price increases, resulted in substantial bracket creep, and hence unlegislated, non-cyclical increases in revenue. These differences show that non-legislated factors are an important source of movements in cyclically adjusted revenues.[10]

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[10] Auerbach (2000) stresses that many non-policy factors other than changes in overall economic activity affect revenues, and thus that the change in cyclically adjusted revenues is a highly imperfect measure of policy-induced tax changes.

IV. THE BEHAVIOR OF OUTPUT FOLLOWING TAX CHANGES OF DIFFERENT TYPES

The next step is to use our measures of tax changes to examine the relationship between taxes and economic activity. In this section, we examine the behavior of output following exogenous tax changes, and compare this behavior with that following tax changes of other types. Examining how output behaves after the exogenous changes should provide the most reliable estimates of the macroeconomic effects of tax changes. Investigating whether the other measures yield similar estimates can show whether those measures are significantly correlated with other influences on output, and thus whether it is important to focus on the exogenous changes.

A. Specification

We focus on the reduced-form relationship between the various measures of tax changes and the growth of real output. Since the exogenous changes reflect policies adopted for reasons essentially unrelated to other factors likely to influence growth, a regression of output growth on these changes without any control variables should, in principle, yield unbiased estimates of the impact of changes in the level of taxes on output.

Our starting specification is therefore very simple: we regress output growth on a constant and the contemporaneous value and twelve lags of our measure of exogenous tax changes. That is, we estimate:

(1)
$$\Delta \ln Y_t = a + \sum_{i=0}^{12} b_i \Delta T_{t-i} + e_t,$$

where $\Delta \ln Y$ is the growth rate of the chain-type quantity index for GDP and ΔT is our measure of exogenous tax changes as a percent of nominal GDP.[11] The data are quarterly. Since our tax measure is available beginning in 1947Q1 and the regression includes twelve lags, the earliest possible starting date is 1950Q1. The final observation is 2006Q2. In the next section, we consider numerous variations on

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[11] The data on the quantity index for GDP are from the National Income and Product Accounts, Table 1.1.3 (downloaded 9/15/06).

this specification, including alternative choices of sample periods and specifications that add control variables.

To see whether the behavior of output following tax changes occurring for other reasons is similar to its behavior following exogenous changes, we estimate equation (1) using other tax measures as well. Our broadest alternative measure is the change in real cyclically adjusted revenues, divided by real GDP.[12] Our measure of exogenous tax changes includes only a subset of legislated changes, and the measure based on cyclically adjusted revenues includes more than just policy-induced changes. An obvious intermediate measure is all legislated changes. Specifically, we consider the sum of our series for exogenous and endogenous tax changes, again expressed as a percent of nominal GDP. To show more clearly whether the economy behaves differently following endogenous changes than following exogenous ones, we also estimate equation (1) using only the endogenous changes.

B. Results

Table 1 presents the estimates of equation (1) using our series of exogenous tax changes. The estimated coefficients on the contemporaneous value and the first ten lags of the tax measure are all negative. The first few are small and far from significant. The coefficients on lags three through nine, however, are all substantial, and several are individually significant. The coefficient on lag ten is smaller, and the final two coefficients are substantively and statistically indistinguishable from zero.[13]

Figure 4 summarizes the estimates by showing the implied effect of a tax increase of one percent of GDP on the path of real GDP (in logarithms), together with the one-standard-error bands. The effect is steadily down, first slowly and then more rapidly, finally leveling off after ten quarters. The estimated maximum impact is a fall in output of 3.0 percent. This estimate is overwhelmingly significant (t = −3.5).

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[12] The broadest possible measure would be based on total revenues, not cyclically adjusted revenues. Since revenues rise when output rises, using this measure would introduce an obvious bias toward finding that increases in taxes raise output. And indeed, when we estimate equation (1) using the change in real revenues divided by real GDP, the coefficient on the contemporaneous value of ΔT is positive, large, and overwhelmingly significant.

[13] The Durbin-Watson statistic for the regression indicates that the residuals are moderately serially correlated. Reestimating the regression correcting for first-order serial correlation (using maximum likelihood) has little impact on the point estimates and increases the standard errors only moderately.

The two-standard-error confidence interval is (–4.7%,–1.3%). In short, tax increases appear to have a very large, sustained, and highly significant negative impact on output. Since most of our exogenous tax changes are in fact reductions, the more intuitive way to express this result is that tax cuts have very large and persistent positive output effects.[14]

The results using the change in cyclically adjusted revenues as the tax variable are quite different. Table 1 also shows the estimates of equation (1) using this broader measure.[15] As before, the coefficients on the tax measure are generally negative. But the pattern is not as consistent, and the magnitudes are considerably smaller. Panel (a) of Figure 5 shows the implied impact of a tax increase of one percent of GDP using this measure. To facilitate comparison, it also repeats the results using our measure of exogenous tax changes. The estimated maximum effect using the change in cyclically adjusted revenues is an output decline of just 1.1 percent (t = –2.2), roughly a third as large as that using exogenous tax changes. In addition, the impact is slower. The point estimates imply that output first rises slightly, peaking at 0.3 percent after two quarters, and does not turn negative until four quarters after the tax change. Thus it appears that the inclusion of endogenous policy-induced changes and non-policy influences in traditional measures of tax changes leads to considerable underestimates of the effects of tax changes on output.

To investigate this finding further, in panel (b) of Figure 5 we show the results when we use our two alternative measures of tax changes caused by changes in policy. First, consider the effects when we use all legislated tax changes. As when we use cyclically adjusted revenues, the behavior of output following a generic policy-induced tax change is slower and considerably smaller than following the exogenous changes. The estimated maximum effect of a tax increase of one percent of GDP is –1.3 percent (t = –2.4). Thus for this sample and specification, the bias caused by using cyclically adjusted

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[14] In the baseline regression, we use the variant of our measure of exogenous tax changes that includes the revenue effects of any retroactive features of the bills. When one uses the version that ignores the retroactive features, the results are virtually identical. The estimated maximum impact of a tax increase of one percent of GDP on GDP is –3.1 percent (t = –3.5). Because the retroactive provisions are a genuine feature of these bills, all the regressions that follow use the variant of the series that includes these provisions.

[15] Because the measure based on cyclically adjusted revenues is computed in changes, it is first available in 1947Q2. We therefore lose the first observation from our sample when we use this measure. Dropping this observation has little impact on the results using our measure of exogenous tax changes.

revenues appears to stem mainly from the inclusion of endogenous policy changes, not from the inclusion of outside influences on revenues.

Next, consider the behavior of output following an endogenous tax change. Consistent with the previous results, the estimated impact is small and slow. Output first rises slightly, reaching a maximum of 0.4 percent after two quarters, and then falls gradually to –0.7 percent after twelve quarters. None of the effects are significant; for example, the t-statistic for the maximum impact is –0.9.

This first look at the data suggests two conclusions. First, changes in the level of taxes have large effects on economic activity: following tax changes undertaken for reasons largely unrelated to other influences on output, there are large and significant output movements in the opposite direction. Second, how one measures tax changes matters: using broader measures substantially obscures the impact of tax changes on the economy.

C. A Finer Breakdown of Tax Changes

Our endogenous and exogenous categories each consist of more than one type of tax change. It is therefore natural to ask if our findings for the broader categories hold for each type of change within them. For example, perhaps only certain types of endogenous changes yield biased estimates.

Panel (a) of Figure 6 displays the results for the two types of endogenous changes.[16] Following a spending-driven tax increase, GDP on average rises moderately, reaching a maximum of 0.7 percent after two quarters (t = 1.5). Thereafter the effect fluctuates irregularly around zero and is always far from significant. Thus looking at how the economy behaves after tax changes driven by spending changes yields estimates of the effects of tax changes that are starkly different from those based on exogenous tax changes.

The behavior of GDP following countercyclical tax changes, in contrast, is more similar to its

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[16] Recall that one endogenous change was intended to magnify other influences on the economy, and so does not fit into either subcategory. Since this procyclical change was a small, one-time event, looking at how the economy behaved in its wake is not very informative. We therefore include this change in the endogenous category, but do not attempt to examine its effects separately.

behavior following exogenous changes. After hovering around zero for a few quarters, GDP falls steadily, reaching a low of –2.1 percent after nine quarters. For comparison, recall that the maximum impact following an exogenous tax change is –3.0 percent after ten quarters. Because countercyclical tax changes are uncommon, the behavior of GDP in their wake is not estimated very precisely. The t-statistic for the estimated effect after nine quarters, for example, is –1.4.

The fact that the point estimates differ only moderately from those for exogenous changes suggests that countercyclical fiscal policy is not achieving its intended purpose. If policymakers were successfully adjusting taxes in response to information about other forces affecting the economy, countercyclical tax changes would be substantially positively correlated with other influences on output growth, and so a regression of growth on the countercyclical changes would yield estimates with a large positive bias. We do not appear to observe this. This finding is consistent with the often-noted fact that it is difficult for fiscal policy to respond quickly to economic developments. Consider, for example, the two largest countercyclical changes in our sample. The 1968 surcharge was first proposed in January 1967, but was not passed until June 1968. And although less than three months elapsed between the proposal and passage of the 1975 tax rebate, the rebate was not proposed until the economy had been in a recession for fourteen months.

Panel (b) of Figure 6 shows the results for the two types of exogenous changes. For long-run changes, which make up most of this category, the results are quite similar to those for all exogenous changes. For tax increases to deal with an inherited budget deficit, the results are more interesting. The point estimates imply that output does not fall at all following deficit-driven tax increases. The estimated effect peaks at 1.4 percent after two quarters, and then fluctuates around 1 percent. However, there are too few tax changes of this type for the effects to be estimated very precisely. The maximum t-statistic, for example, is just 1.2. Nonetheless, the estimates are suggestive that tax increases to reduce an inherited deficit may be less costly than other tax increases, and they provide no evidence that they have substantial

output costs.[17]

The finding that tax increases to reduce the deficit appear to have little negative impact on output is particularly surprising given a possible bias in this regression. Deficit-reduction packages, especially later in the sample, often include at least small cuts in spending. Thus, deficit-driven tax increases are potentially correlated with another force likely to depress output. One might therefore expect the negative effects of a tax increase in this regression to be overstated. That the results nevertheless show a smaller output effect could imply that this bias is minimal. Alternatively, it could suggest that the beneficial impact of a deficit-reduction package on expectations or long-term interest rates is substantial.

V. **ROBUSTNESS**

The evidence presented thus far suggests that changes in taxes have large effects on output. In this section, we test the robustness of this finding. We investigate the sensitivity of the results to outliers and the sample period. More importantly, we examine the effects of including numerous control variables. As described above, because our new series on exogenous tax changes was derived to exclude changes motivated by the current or projected state of the economy, it is in principle unnecessary to include control variables to estimate the effects of tax changes. But, because our identification of motivation could be imperfect, and because the exogenous tax changes could be correlated with other influences just by chance, it is useful to see if including other variables affects the results.

For the most part, this section focuses on our measure of exogenous tax changes. In cases where the results for the other measures change importantly, or where there is an obvious reason to be concerned with those measures, we discuss the results for them as well.

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[17] The deficit-driven tax change most associated with the notion that such changes might have few adverse consequences is the 1993 Clinton tax increase. However, this observation is not driving the estimates. When it is excluded, the maximum effect is 1.7 percent (t = 0.8).

A. **Outliers and Sample Period**

The most obvious robustness check involves extreme observations. While large exogenous tax changes are surely reasonable observations to consider, it is useful to check whether they are driving the results. To do this, we simply drop one at a time the four largest exogenous tax actions: those included in the Revenue Act of 1948, the Revenue Act of 1964, the Economic Recovery Tax Act of 1981, and the Economic Growth and Tax Relief Reconciliation Act of 2001 together with the Jobs and Growth Tax Relief Reconciliation Act of 2003.[18]

The results of this exercise suggest that the estimates are quite durable. For two of the acts, excluding the extreme observations has virtually no impact on the estimated cumulative effect of a tax change on output. The maximum effect of a tax increase of one percent of GDP excluding the 1964 Kennedy-Johnson tax cut is –2.8 percent (t = –2.8); the maximum effect excluding the 1981 Reagan tax cut is –2.9 percent (t = –3.0). Recall that for the full sample, the maximum effect is –3.0 percent (t = –3.5). Excluding the 2001 and 2003 Bush tax cuts (jointly) substantially increases the negative impact of a tax change. The maximum effect is now –3.6 percent (t = –3.8). Only in the case of the 1948 tax cut does excluding the extreme observation weaken the estimated effects noticeably. But even then, they remain large and highly significant. When the 1948 action is excluded, the implied maximum impact of a tax increase of one percent of GDP is –2.2 percent (t = –2.7). That leaving out this one action matters somewhat it not surprising: it is the largest exogenous tax change in the postwar era and was followed nine quarters later by the start of the Korean War.

Another way to address the possible importance of the extreme observations in the early postwar era is to start the sample period in 1955Q1. This shorter sample period leaves out the large 1948 tax cut and is safely after the wartime boom in GDP. Figure 7 shows the cumulative impact of an exogenous tax increase when the sample period is 1955Q1 to 2006Q2. The estimated maximum output effect of a tax increase of one percent of GDP is now –2.5 percent (t = –3.4).

Shortening the sample period has substantially more effect on the estimates based on broader

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[18] Since the acts each included multiple changes, excluding a single act excludes changes in a number of quarters.

measures of tax changes. Figure 7 also shows the estimated cumulative impact of a tax increase of one percent of GDP when the change in real cyclically adjusted revenues is used as the tax variable. The change in the sample period causes the estimates using this measure to move noticeably closer to those for the exogenous changes, but the gap remains substantial. The estimated maximum effect is now a change in GDP of –1.8 percent, as opposed to –2.5 percent for the exogenous changes. In addition, the effects using the change in cyclically adjusted revenues remain noticeably slower than those using exogenous changes. When our measure of all legislated tax changes is used as the tax variable, the results for the shorter sample are very similar to those using only the exogenous changes. Thus for this sample, including policy-induced changes motivated by countercyclical considerations or spending changes does not appear to cause important bias. As the results using cyclically adjusted revenues show, including tax changes driven by non-policy, non-cyclical factors does, however.

B. Controlling for the State of the Economy

The simplest, and arguably most important, control variable to consider is lagged GDP growth. Including several lags of GDP growth should obviously help control for the normal dynamics of output. Because many factors affecting output are likely to be serially correlated, including lagged GDP growth is also an easy way to control for a multitude of other influences. Finally, accounting for the state of the economy by including lags of GDP growth provides a crucial test for hidden motivation. One worry is that even though policymakers say they are changing taxes for reasons unrelated to the state of the economy, perhaps the democratic process makes such changes correlated with economic performance. For example, candidates advocating tax cuts may be more likely to win when the economy is weak. Thus, perhaps seemingly exogenous tax cuts are more common when output is below normal, and so the apparent stimulatory effects of tax cuts are in part simply the usual reversion of output to normal. Controlling for numerous lags of GDP growth addresses this possibility.

Including lagged GDP growth in our regression is straightforward. Because the quarterly GDP data start in 1947Q1, we can include eleven lags of GDP growth and retain our baseline sample of

1950Q1 to 2006Q2. We therefore reestimate equation (1) using twelve lags of our measure of exogenous tax changes and eleven lags of GDP growth. To summarize the results, one now needs to include not only the direct effect of the tax changes on output, but also the effects working through lagged output.[19]

Figure 8 shows the estimated impact on GDP of an exogenous tax increase of one percent of GDP, with and without controlling for lagged growth. The results are very similar. Controlling for lagged GDP growth reduces the maximum effect only trivially, from –3.02 percent (t = –3.5) to –2.95 percent (t = –2.8).

The specification including lagged growth allows for the possibility of further effects of tax changes on output beyond twelve quarters. The estimates suggest, however, that these effects are minor. When we carry the simulation out to 24 quarters, the estimated effect diminishes from –2.95 percent in quarter 10 to –2.6 percent in quarter 15, and then remains between –2.6 and –2.7 percent.[20]

Another way to address the possibility that the exogenous tax changes we identify may be influenced by the state of the economy is to consider a simple vector autoregression (VAR). A VAR allows the tax changes to respond to output, and it accounts for the normal dynamics of output and the tax changes. We therefore estimate a VAR with two variables, our series of exogenous tax changes and the logarithm of GDP. As in the simple regression, we include twelve lags. In keeping with our regression specification, which allows tax changes to affect output within the quarter, we place the tax series first and GDP second.

The results provide no evidence that our exogenous tax changes respond to economic conditions. The impulse response function of the tax changes to an orthogonalized innovation to GDP is small and

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[19] Specifically, the estimated impact of the tax change is now the dynamic multiplier accounting for the implied changes in the path of lagged GDP growth. The standard errors are computed by taking 10,000 draws of the coefficient vector from a multivariate normal distribution with mean and variance-covariance matrix equal to the point estimates and variance-covariance matrix of the regression coefficients.

[20] It is useful to note that controlling for lagged GDP growth is not a solution to the possible bias from using broader measures of tax changes. Many endogenous tax changes, particularly those motivated by spending changes, are not responses to the current state of the economy. As a result, if one regresses GDP growth on twelve lags of the change in real cyclically adjusted revenues (as a percent of real GDP) and eleven lags of GDP growth, the results (including the effects through lagged output) are very similar to those from the regression not controlling for lagged GDP growth, though the standard errors increase sufficiently that none of the effects are significant. This suggests that the estimated impact of tax changes using broader measures is still very biased even when lagged GDP growth is included.

insignificant at all horizons. The p-value for the test of the null hypothesis that output does not Granger cause the tax changes is 0.49.

In addition, the implications of the VAR for the output effects of tax changes are very similar to those of the simple regression. As described above, the fact that tax bills often include retroactive components causes our series of exogenous tax changes to be negatively serially correlated. As a result, a one percentage point innovation in our tax series in the VAR is a somewhat different experiment than the permanent one percentage point change in taxes we consider in presenting the regression results. In particular, following a one-percentage-point innovation in our series, just over 40 percent of the change is reversed the following quarter, with only small and irregular changes after that.

Given this difference in the experiments, the output response in the VAR is exactly what one would expect. Following a one-percentage-point innovation in our series, there is a rapid, large, and highly significant fall in output, with a time pattern very similar to that implied by the simple regression. The t-statistic for the maximum effect is –2.8. The only notable difference is that the effects are roughly 40 percent smaller than those implied by the regression. For example, the maximum effect is now –1.8 percent rather than –3.0 percent. This is what one would expect in light of the difference in the experiments.

C. Controlling for Monetary Policy

A second type of control variable is an indicator of monetary policy. Monetary policy shocks are an important determinant of short-run fluctuations. It is therefore important to check that our new measure of fiscal shocks does not happen to be correlated with monetary disturbances. In controlling for monetary policy, however, it is important not to control for changes that are responses to the tax changes. For example, suppose the response of monetary policy typically mitigates the expansionary effects of tax cuts. In this case, asking how output behaves in the wake of tax cuts holding monetary policy fixed would lead to overestimates of the actual effects of the cuts. Thus what we want to control for are not all changes in monetary policy, but changes that are not just the usual response of monetary policy to

economic developments.

We consider two measures of such independent changes in monetary policy. The first is a dummy variable for shifts to anti-inflationary monetary policy that we identified in earlier work (Romer and Romer, 1989, 1994). This series has the advantage of covering the full sample, but the disadvantage of being fairly crude. Our second series is a continuous indicator of monetary shocks derived as the residuals of a regression of the change in the federal funds rate target on the Federal Reserve's internal forecasts of inflation and real growth (Romer and Romer, 2004). This series is available for the period 1969Q1–1996Q4. We control for monetary policy by adding twelve lags of the appropriate measure to the regression.[21]

Figure 9 shows the estimated impact of an exogenous tax increase of one percent of GDP on GDP, both with and without controlling for monetary policy. Panel (a) displays the results when the dummy variable is used as the monetary policy indicator. Adding this control reduces the estimated impact of tax changes by about twenty percent, but the effect remains very large and highly significant.[22]

Panel (b) considers the continuous measure of monetary policy shocks. Because of the limited availability of this series and the inclusion of lags, the sample period is now 1972Q1–1996Q4. For this sample, the estimated impact of a tax increase when we do not control for monetary policy is very similar to the estimates for the full sample. The estimated maximum effect is an output decline of 3.1 percent after nine quarters rather than 3.0 percent after ten. Controlling for monetary policy again reduces the estimated effect by about twenty percent. The effect remains statistically significant, but not overwhelmingly so; the largest t-statistic is now –2.4. Given that we have discarded over half our sample, this is not surprising.

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[21] We convert each series from monthly to quarterly in the obvious way: the quarterly dummy is set to one in any quarter that contains a month when there was a shift to anti-inflationary policy, and the quarterly observations for the shock series are the averages of the corresponding monthly observations.

[22] Excluding the Korean War has slightly more impact on this experiment than on the others we consider. The estimated maximum impact of an exogenous tax increase of one percent of GDP on GDP in the regression including the dummy variable estimated over the period 1955Q1–2006Q2 is –1.8 percent (t = –2.5). Without the control, the maximum impact is –2.5 percent (t = –3.4).

D. **Controlling for Government Spending**

Government spending, especially large military actions, can have a powerful impact on output. Thus in examining the effects of tax changes, another candidate control variable is changes in government spending. As before, if we have constructed our measure of exogenous tax changes correctly, controlling for spending changes is not necessary.[23] But, chance correlation is always a possibility. And, to the degree that deficit-driven tax increases are parts of budget packages that include spending cuts, there is reason to fear there is some systematic correlation as well.

We use as our measure of spending changes the change in real federal total gross government expenditures less interest payments, divided by real GDP.[24] The expenditure data start in 1947Q1. Therefore, as with lagged GDP, we can include eleven lags of the change in spending and retain the baseline sample period. We also include the contemporaneous value.

Panel (a) of Figure 10 shows that controlling for changes in government spending has little impact on the estimated effects of exogenous tax changes. The maximum effect of a tax increase of one percent of GDP is now an output decline of 2.9 percent, which is only slightly smaller than the estimate from the simple regression. The effect remains highly statistically significant, with a maximum t-statistic of -3.4.

Panel (b) shows that controlling for changes in spending has a moderate impact on the estimated behavior of output following tax increases for deficit reduction. The maximum impact of a deficit-driven tax increase of one percent of GDP on GDP rises from 1.1 percent (t = 0.5) to 1.6 percent (t = 0.8). While the difference is far from significant, it is in the direction one would expect if there were occasionally spending cuts at the same time as deficit-driven tax increases. Controlling for the depressing effects of the spending cuts suggests that the deficit-driven tax increases have a larger positive impact on output.

One reason that broader measures of tax changes yield biased estimates of the effects of tax

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[23] Further, as with monetary policy, if exogenous tax changes affect output in part by changing the path of spending, controlling for spending changes will lead to misestimates of the overall output effects of tax changes.

[24] The government expenditure data that we use are from the National Income and Product Accounts, Table 3.2 (downloaded 9/17/06). We calculate federal total gross expenditures less interest payments as current expenditures + gross government investment + capital transfer payments + net purchases of nonproduced assets − interest payments. We divide this by the price index for GDP to convert it to real values.

changes on output is that they include tax changes motivated by spending changes. A natural question is whether including a measure of spending changes in the regressions using these broader measures reduces this bias. To test for this possibility, we reestimate equation (1) using both the change in cyclically adjusted revenues and all legislated tax changes, including the contemporaneous value and eleven lags of the spending variable. We find that controlling for spending changes is at best only a partial fix. For the change in cyclically adjusted revenues, including spending makes the estimated effect of a tax increase on GDP more negative, as one would expect if the omission of spending changes biased the estimates toward zero. The change is very small, however, and the estimates remain far smaller than those estimated using the exogenous tax changes. For all legislated tax changes, controlling for spending has a larger effect. The inclusion of government spending increases the estimated impact of tax changes by about fifty percent. The estimates remain substantially smaller than those using only exogenous changes, but about half the gap is eliminated.

The bottom line of the robustness analysis is that our baseline results are very durable.[25] None of the robustness checks greatly lessens the estimated negative impact of an exogenous tax increase on output. At the same time, shortening the sample period and including sensible control variables typically reduces the estimated cumulative impact slightly. Whereas in the baseline regression the maximum effect of a tax increase of one percent of GDP is a fall in GDP of 3.0 percent, in many of the robustness exercises the fall is closer to 2.5 or 2.75 percent. In all of the exercises, the effect is highly statistically significant.

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[25] We have also investigated the effects of controlling for the contemporaneous value and eleven lags of the percentage change in the relative price of crude oil; for the contemporaneous value and twelve lags of a dummy variable for whether the president is a Democrat; and for the contemporaneous value and twelve lags of dummies for the beginnings of Democratic and Republican administrations. In all of these cases, the results change only trivially.

VI. EXTENSIONS

In this section, we extend our results in four directions. First, we examine whether the output effects of a legislated tax change are more closely related to the enactment of the legislation or to the change actually taking effect. Second, we ask whether the effects of tax changes have changed over time. Third, we present some evidence concerning the issue of whether the relatively long-lived output effects that we find reflect persistent departures of output from its normal level or changes in the normal level of output. Finally, we examine how tax changes affect the components of output, and in doing so provide some evidence about why the output effects of tax changes are so large.

A. Role of Expectations

In deriving our measure of fiscal shocks, we date tax changes when they actually took effect. A tax law that changed liabilities in a series of steps is therefore recorded as a sequence of fiscal shocks. Our empirical analysis then looks at the output effects of these actual changes in taxes. This approach is almost surely appropriate. As discussed in Section II, there is considerable evidence that consumers do not smooth consumption in response to moderate anticipated variations in taxes. And, any supply-side effects of tax changes are likely to occur when marginal tax rates actually change.

However, expectations could also matter. Under the permanent income hypothesis, consumers should react to news about a sequence of future tax increases by immediately reducing consumption. They should then not respond further when tax liabilities actually change. Other expectations effects are also possible. News of a tax increase may improve people's expectations about the government's future fiscal health. This could have a positive impact on confidence, and hence on spending and output. News of an increase in marginal tax rates could cause intertemporal substitution toward income generation before the tax increase. Like the confidence effect, this effect goes in the opposite direction from the permanent income effect.

To investigate the effects of news about future taxes, one would ideally want continuous data on

the perceived probability of a tax change and the present value of the possible action. As a step in that direction, we calculate the present value of the legislated tax changes included in a given bill at the time of passage. This calculation adjusts the timing of the revenue effects of an action to be much closer to the time the news of the action became available. We can then investigate the response of output to this alternative measure of fiscal shocks.

The calculation of the present values is straightforward. We date an action in whatever quarter the bill was signed. We discount the future changes using the three-year Treasury bill rate.[26] When the individual actions for a given act have multiple motivations, we calculate a separate present value for each type of motivation.[27]

We then augment our regression to include not only the contemporaneous value and twelve lags of our baseline measure of tax changes, which dates changes in the quarter they actually took effect, but also the contemporaneous value and twelve lags of the alternative present-value measure, which dates tax changes in the quarter of passage. This specification allows us to test whether the data prefer one timing assumption to the other, and whether there is a separate expectations effect. We estimate the regression over the full postwar sample using both exogenous tax actions and the subset of exogenous actions motivated by a desire to deal with an inherited budget deficit.

The results using exogenous tax changes are shown in panel (a) of Figure 11. The solid line shows the cumulative impact on GDP of a tax increase of one percent of GDP dated in the quarter the change took effect, holding constant the behavior of the present-value series; the dashed line shows the

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[26] The data are from the Board of Governors of the Federal Reserve System, www.bog.gov, series H15/H15/RIFLGFCY03_N.M (downloaded 5/18/07). The data do not begin until 1953Q4. We extend the series back to 1947Q1 using the 3-month Treasury bill rate (series H15/H15/RIFSGFSM03_N.M). The two interest rates differ by only 0.3 percentage points in 1953Q4.

[27] One complication that arises in calculating present values involves some of the tax changes classified as deficit-driven. In our baseline series, a tax increase that is legislated in a bill increasing spending, but that occurs more than a year after the spending increase that was its ultimate motivation, is classified as deficit-driven. This makes sense in the framework where output reacts to the actual change in taxes because the tax change is substantially after the spending change. But, in a framework emphasizing news, the future tax change should be treated as spending-driven. For this reason, we reclassify six deficit-driven tax changes as spending-driven. These observations are the 1954Q1 increase from the Social Security Amendments of 1950; the 1954Q1 decrease from PL125; the 1960Q1 increase from the Social Security Amendments of 1958; the 1963Q1 increase from the Social Security Amendments of 1961; the 1971Q1 increase from the Social Security Amendments of 1967; and the 1978Q1 increase from the 1972 changes to Social Security.

impact of a present-value tax increase of one percent of GDP dated in the quarter of passage, holding constant the behavior of actual tax changes.

The estimated impact of a tax change dated in the quarter it actually went into effect in this expanded specification is very similar to that for our baseline specification (given in Figure 4). The maximum impact is –3.0 percent (t = –2.4). That the results do not change noticeably when the measure of news is added suggests that output responds to tax changes when they take effect.

The impact of a present-value tax increase dated in the quarter of passage is initially small and then consistently positive. The maximum impact is 1.1 percent (t = 1.7). That the impact is not significant at conventional levels suggests that the news effects of tax changes may be of secondary importance. That the point estimates are positive is perhaps more surprising. This is certainly not the sign predicted by the permanent income hypothesis. It is consistent with news of a tax increase having a positive effect on consumer confidence or an intertemporal substitution effect on labor supply.

Panel (b) shows the estimated cumulative impacts in the expanded specification using only deficit-driven tax actions. For this subset of exogenous tax changes there is somewhat more evidence of a meaningful news effect. First, the impact of a tax increase dated in the quarter it took effect changes substantially between the baseline and the expanded specification. For the baseline specification the impact is consistently positive, though imprecisely estimated. The impact in the expanded specification is largely negative, though still imprecisely estimated: the maximum impact is –1.5 percent (t = –0.8). This change could indicate that the present value dated at the time of passage is an important explanatory variable. When it is included, tax changes dated at the time they took effect have the more conventional negative effect.

The estimated impact of an increase in present value dated at the time of passage is again positive. However, it is larger for deficit-driven tax changes than for exogenous changes more generally. The maximum impact is 2.2 percent (t = 1.6). This finding may suggest that tax increases explicitly for deficit reduction have a particularly large positive impact on confidence. However, the effect is not significant at conventional levels.

Overall, these results suggest that our baseline method of dating tax changes in the quarter in which liabilities actually changed is largely appropriate. There is a significant negative relationship between exogenous tax increases dated this way and output that is robust to the inclusion of a proxy for fiscal news. On the other hand, there is some evidence that expectations effects could also exist, especially for the subset of exogenous tax changes motivated by concern for an inherited budget deficit.

B. Changes Over Time

There are several reasons that the aggregate effects of tax changes might have become smaller over time. First, monetary policy has come to play an increasingly central role in stabilization policy. Thus the Federal Reserve may have done more to counteract the effects of tax changes in the later part of the sample. Second, there has been more concern about the United States's long-run fiscal health in recent decades. As a result, the direct contractionary effects of tax increases may have been offset to a greater extent by effects operating via confidence. And third, the increasing depth and scope of financial markets could have made the dependence of consumer spending on disposable income smaller later in the sample. On the first two of these dimensions, there were significant changes around 1980, which is also roughly the midpoint of our sample. We therefore investigate the effects of splitting the sample at the end of 1980.

Figure 12 shows that the effects of tax changes appear to have become considerably smaller over time. For the early period, the estimated maximum effect of an exogenous tax increase of one percent of GDP is a fall in output of 3.9 percent. For the later period, it is a fall of 2.3 percent. Starting the early sample in 1955Q1 yields a decline similar to that obtained using 1950Q1 to 1980Q4.

Despite the large difference in the point estimates, the results do not provide overwhelming evidence that the effects of fiscal policy have changed over time. For the full sample, the largest t-statistic for the difference in the effects is 1.3. Excluding the Korean War period, however, increases it to 2.2. Thus the data suggest that there has been a large fall in the effects of tax changes, but they are not decisive.

The other important feature of these results is that the impact of tax changes remains large and highly significant for the later period. For the later sample, the estimated maximum effect of a tax increase of one percent of GDP on output is –2.3 percent, with a t-statistic of –3.3.

C. __Why Are the Effects So Persistent?__

One striking feature of our results is that the estimated effect of tax changes on output shows little tendency to return to zero at longer horizons. In both the baseline specification and most of the robustness tests, the effect does not reach its maximum until more than two years after the tax change, and it weakens only trivially over the next few quarters.

At a general level, there are two possible explanations for this pattern. Tax changes could have large supply-side effects; that is, they could have large effects on the economy's normal, flexible-price level of output. In this view, the important effects of tax changes, at least at moderate horizons, are on incentives and productivity rather than on disposable income. Alternatively, tax changes could have highly persistent demand-side effects; that is, their impact on the gap between actual and normal output could be very long-lasting. In this view, adjustment to the economy's flexible-price equilibrium is slow. Although this idea is not appealing theoretically, there is considerable support for the hypothesis that macroeconomic adjustment is sluggish. There is strong evidence that inflation is much more persistent than baseline models predict (for example, Fuhrer, 1997, and Rudd and Whelan, 2006). And estimates of the real effects of monetary policy, which on theoretical grounds should be neutral in the long run, often suggest considerable persistence (for example, Bernanke and Mihov, 1998, and Romer and Romer, 2004).

One way to try to distinguish between these two views is to directly estimate the output effects at longer horizons. If the persistent output effects reflect changes in normal output, there is little reason to expect them to dissipate. If they reflect long-lasting departures of output from normal, on the other hand, one would expect them to fade. Unfortunately, this approach yields little information. We have experimented with including up to twenty lags of our measure of exogenous tax changes in the baseline regression. The estimated output effects weaken slightly between 10 and 14 quarters after the tax change,

but then strengthen again in quarters 15 to 20. Because the standard errors rise as the horizon increases, neither the hypothesis of no reversion to trend nor the hypothesis of substantial trend reversion can be rejected.

Because this direct test is inconclusive, we examine the behavior of two other variables. The first is inflation. If the fall in output in response to a tax increase reflects a fall in normal output, there is no particular reason for inflation to change following a tax increase. But if it reflects a fall in output relative to its flexible-price level, one would expect inflation to fall.

The second variable is the unemployment rate. If a tax increase reduces output by lowering normal output, it is unlikely that there would be a substantial increase in unemployment. Changes in productivity, labor force participation, and hours of work all appear more likely channels through which a tax change could affect normal output than large, rapid changes in the natural rate of unemployment. Since unemployment is highly cyclical, however, the view that the output decline reflects a temporary departure of output from normal predicts a substantial increase in the unemployment rate.

We therefore reestimate equation (1) using both the change in the inflation rate and the change in the unemployment rate as the dependent variable, and using our measure of exogenous tax changes as the independent variable.[28] Because the dependent variables are expressed as differences, the cumulative effects show the impacts of a tax increase of one percent of GDP on the levels of the inflation rate and the unemployment rate. These responses are shown in Figure 13.

Panel (a) shows that inflation appears to fall substantially. The point estimates show the impact reaching –2.2 percentage points after ten quarters, then spiking down to –3.1 percentage points in quarter 11 before returning to –2.2 percentage points in quarter 12.

One way to put these numbers in context is to consider a traditional Phillips curve that relates the change in inflation to the output gap. The estimated cumulative impact shown in Figure 4 suggests that in response to a tax increase of one percent of GDP, over the next three years output is on average 1.8

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[28] The inflation rate is measured as the change in the logarithm of the chain-type price index for GDP. The unemployment data are from the Bureau of Labor Statistics, www.bls.gov, series LNS14000000 (downloaded 10/21/06).

percent lower than it otherwise would have been. Standard estimates of the Phillips curve suggest that if output is one percent below normal for a year, inflation falls by 0.25 to 0.50 percentage points (Fuhrer, 1997; Gordon, 1997). Thus, if the output effects of tax changes were entirely departures of output from normal, one would expect inflation to fall by 1.35 to 2.7 percentage points. The estimates suggest that inflation falls by at least that much. The one important caveat to this discussion is that the response of inflation to the tax cut is not precisely estimated. For example, there is only one quarter for which the null hypothesis of no effect can be rejected.

Panel (b) shows that a tax increase is followed by a large and highly significant rise in the unemployment rate. The peak effect is an increase of 1.1 percentage points after twelve quarters, with a t-statistic of 3.1. The size of the response is again consistent with what one would expect if the output declines reflected only departures of output from its flexible-price level. The estimated output effect after twelve quarters is a decrease of 2.9 percent, suggesting an Okun's law coefficient of 2.6. This is consistent with conventional estimates (for example, Attfield and Silverstone, 1996).

Thus, the behavior of inflation and unemployment are exactly what one would expect if the estimated long-lasting output effects of tax changes represented persistent departures of output from its flexible-price level. The evidence is not definitive, however: the inflation response is not precisely estimated, and it is conceivable that tax changes have large effects on the natural rate of unemployment.

D. **The Components of Output and the Transmission Mechanism**

Our results suggest that exogenous tax increases have a strong negative effect on output. An obvious question is whether we can go beyond the reduced-form results and shed light on how or why fiscal changes have such pronounced effects. To that end, we examine the response of the various components of GDP, such as consumption and investment, to our new measure of exogenous tax changes.[29] We also look at the response of the components of GDP to the subcategory of our exogenous actions aimed at reducing an inherited budget deficit. These actions are the ones that might most

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[29] This focus on the behavior of the components is similar to the approach in Blanchard and Perotti (2002).

plausibly have special effects through their impact on expectations and interest rates. We examine whether certain components respond unusually to this kind of fiscal shock.

Our specification mirrors that in Section IV. We regress the growth rate of a component of GDP on a constant and the contemporaneous value and twelve lags of our measure of exogenous tax changes (or the subset of these changes aimed at deficit reduction).[30] The regressions are run over the full postwar sample (1950Q1–2006Q2).

Responses to Exogenous Tax Changes. Panel (a) of Figure 14 shows the estimated responses of consumption and investment to an exogenous tax increase. For comparison, it also repeats the results for GDP. The key results are that both components fall, and that the fall in investment is much larger than that in consumption. In response to a tax increase of one percent of GDP, the maximum fall in personal consumption expenditures is 2.6 percent ($t = -3.8$), just slightly less than the maximum fall in GDP. The maximum fall in gross private domestic investment is 12.6 percent ($t = -2.9$).[31]

Conventional models predict that a tax increase lowers interest rates. Thus, the fact that investment falls so strongly in response to a tax increase suggests that conventional interest rate effects are not key. One possibility is that interest rates do not respond much, or respond perversely, to tax changes.[32] The strong response of investment to tax changes is consistent with research showing that investment depends strongly on cash flow and overall economic conditions.[33]

The strong negative relationship between tax changes and investment also helps to explain the size of our estimated overall effect on output. Recall that we find that a tax increase of one percent of GDP lowers GDP by about 3 percent. An important part of that effect appears to be due to the procyclical

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[30] The particular output series that we use are the chain-type quantity indexes from the National Income and Product Accounts, Table 1.1.3 (downloaded 9/15/06).

[31] Blanchard and Perotti also find that investment falls in response to their measure of a positive tax shock, and that the percentage fall in investment is substantially larger than the percentage fall in consumption.

[32] Our series on exogenous tax changes is not well suited to measuring the impact of tax changes on interest rates. Our series identifies the times when tax changes actually affected liabilities (or, in the variant we consider in Part A of this section, when legislation changing taxes was enacted), but it does not indicate when there was news that a tax change would occur. Since interest rates presumably respond to news, they are likely to change before liabilities actually change (and before the legislation changing taxes is actually enacted).

[33] See, for example, Fazzari, Hubbard, and Petersen (1988) and Oliner, Rudebusch, and Sichel (1995).

behavior of investment.[34]

Panel (b) of Figure 14 shows the estimated response of the three categories of consumption. Personal consumption expenditures on both nondurables and services fall relatively little: the maximum impact is –2.0 percent (t = –3.3) for nondurables and –1.5 percent (t = –3.3) for services. Expenditures on durables fall substantially more: the maximum impact is –8.6 percent (t = –2.6). As with investment, the fact that durables purchases respond so negatively to a tax increase suggests that the general cyclical volatility of durables swamps any countervailing interest rate effect.

Panel (c) shows the estimated responses of nonresidential and residential fixed investment. It also repeats the effect for gross private domestic investment, so that one can implicitly see the response of inventory investment.[35] Fixed investment of both types falls less than total investment, suggesting that inventory investment falls particularly strongly. But, fixed investment responds sharply as well. The maximum effect is –6.2 percent (t = –2.7) for nonresidential fixed investment and –8.2 percent (t = –2.1) for residential fixed investment.

The responses of imports and exports are shown in panel (d). These responses have the signs predicted by textbook models. Exports appear to rise substantially in response to a tax increase, but the effect is not statistically different from zero. The maximum impact is 2.6 percent (t = 1.2). Imports fall significantly. The maximum impact is –10.1 percent (t = –2.8). The implied rise in net exports is consistent with the tax increase lowering interest rates and hence capital inflows. But the fact that the effect is so much stronger for imports suggests that the fall in income may be more important than the

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[34] Exogenous tax changes sometimes include changes in the investment tax credit or in the tax treatment of depreciation that directly affect incentives for investment. Thus another possibility is that the sharp estimated response of investment reflects the impact of these incentives. However, cases of substantial exogenous tax changes accompanied by important changes in tax incentives for investment are uncommon. By far the largest case occurred with the Reagan tax cuts in the early 1980s. The second largest case was the change in depreciation guidelines and adoption of an investment tax credit in 1962. To address the possibility that such incentives are a crucial source of the investment response, we reestimate the effect of exogenous tax changes on investment with the series for exogenous tax changes set to zero over the periods 1962Q3–1963Q1 and 1981Q3–1984Q1. This change lowers the estimated investment response only slightly.

[35] The BEA does not provide a quantity index for inventory investment before 1990.

interest rate/exchange rate linkage.[36]

Responses to Deficit-Driven Tax Changes. In Section IV, we found that the response of GDP to a deficit-driven tax increase is positive, though not significant. Since the literature has suggested that deficit-reducing fiscal reforms may have such positive effects, we look at the response of the components of GDP to deficit-driven tax changes.

The results are shown in Figure 15. Panel (a) shows the results for GDP and its two main components, consumption and investment. For consumption, the effect of a deficit-driven tax increase is mildly positive for the first two quarters and then mildly negative. None of the estimated effects have a t-statistic greater than 0.7 in absolute value. For investment, the results are strikingly different from those for consumption, and from those for investment using all exogenous tax changes. Deficit-driven tax increases appear to raise gross private domestic investment substantially over the first three quarters. The effect then gradually declines over the next seven quarters. While none of the estimated effects are different from zero at conventional significance levels, the t-statistics on the initial positive impacts are all greater than 1.5.[37]

Panel (b) shows the estimated responses of the three types of consumption. The effects are very imprecisely estimated, so none of the effects are statistically significant. The point estimates, however, differ substantially. For both nondurables and services, the estimated effects are fairly consistently negative, though smaller than when all exogenous tax changes are used as the independent variable. For durables, the estimated response is typically positive and of moderate size, though it fluctuates somewhat.

Panel (c) shows the estimated responses of total investment, residential fixed investment, and nonresidential fixed investment. Residential fixed investment rises substantially in the first two quarters

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[36] We also estimate the responses of exports and imports splitting the sample after the end of fixed exchange rates in 1973Q1. Consistent with standard models, the estimated impact of a tax increase on exports in the fixed exchange rate period is small, irregularly signed, and highly insignificant. The estimated decline in imports is smaller under flexible exchange rates than under fixed exchange rates. Although the response of exchange rates should magnify the decline in the later period, the fact that the output response is considerably smaller in that period works in the opposite direction. Thus the smaller decline in the second part of the sample is not surprising.

[37] When the sample is restricted to the post-Korea period (1955Q1–2006Q2), the results are qualitatively similar, but less dramatic. The maximum impact of a deficit-driven tax increase of one percent of GDP on consumption is –0.8 percent (t = –0.7) and on investment is 5.8 percent (t = 0.9).

after a deficit-driven tax increase, and the effect is marginally significant. The estimated maximum effect is 10.4 percent (t = 1.9). Nonresidential fixed investment rises as well, but much less sharply, and the effects are imprecisely estimated. The behavior of total investment is similar to the behavior of residential fixed investment.

The pattern of responses to exogenous tax changes as a whole seemed to indicate that the behavior of expenditures on durables and investment was just a more extreme version of the other types of consumption. For deficit-driven tax changes, in contrast, investment and consumer spending on durables move in the opposite direction from consumer spending on nondurables and services. This could suggest that interest rates move particularly strongly in response to deficit-driven tax changes. It could also suggest an important role for confidence. Perhaps deficit-driven tax changes make consumers more optimistic and thus cause them to buy more durables and to spur residential investment.[38]

Neither the interest-rate story nor the confidence story is without weaknesses. If interest rates move particularly much in response to deficit-driven tax changes, one might expect nonresidential fixed investment to respond more forcefully. If confidence is the crucial link, why does consumer spending on nondurables and services still fall?

The estimated responses of imports and exports to a deficit-driven tax change may be most consistent with a combination of the two views. Panel (d) shows that both exports and imports rise, though again the responses are not significantly different from zero. If all that were happening were a large fall in interest rates in response to the tax increase, one would expect depreciation of the dollar to raise exports and lower imports. That imports rise in response to a deficit-driven tax increase (but fall substantially in response to a generic exogenous tax increase) suggests a positive confidence effect. This could be consistent with the results for the components of consumption, because a large fraction of imports are consumer durables.

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[38] Giavazzi and Pagano (1990) argue that the confidence effect explains why dramatic fiscal reforms have been expansionary in some European countries.

VII. <u>CONCLUSIONS</u>

This paper investigates the causes and consequences of changes in the level of taxation in the postwar United States. In terms of causes, we find that despite the complexity of the legislative process, most significant tax changes have a dominant motivation that fits fairly clearly into one of four categories: counteracting other influences on the economy, paying for increases in government spending (or lowering taxes in conjunction with reductions in spending), addressing an inherited budget deficit, and promoting long-run growth. The last two motivations are essentially unrelated to other factors influencing output, and so policy actions taken because of them can be used to estimate the effects of tax changes on output.

We also find that the motivations for tax changes have changed substantially over time. Countercyclical changes were frequent from the mid-1960s to the mid-1970s, but were unheard of before that time and from the mid-1970s until 2001. Tax changes motivated by spending changes were commonplace in the 1950s, 1960s, and 1970s, but have virtually disappeared since then. Tax increases to address inherited deficits were common from the late 1970s to the early 1990s, but rare before and after this period. Only tax changes motivated by long-run considerations have been a constant feature of the fiscal landscape since World War II.

In terms of consequences, our results indicate that tax changes have very large effects on output. Our baseline specification suggests that an exogenous tax increase of one percent of GDP lowers real GDP by roughly three percent. Our many robustness checks for the most part point to a slightly smaller decline, but one that is still well over two percent. The output effects are highly persistent. The behavior of inflation and unemployment suggests that this persistence reflects long-lasting departures of output from its flexible-price level, not large effects of tax changes on the flexible-price level of output. We also find that the output effects of tax changes are much more closely tied to the actual changes in taxes than to news about future changes, and that investment falls sharply in response to exogenous tax increases. Indeed, the strong response of investment helps to explain why the output consequences of tax changes are so large.

We also examine the behavior of output following changes in other measures of taxes. The estimated output effects obtained using broader measures of tax changes, such as the change in cyclically adjusted revenues or all legislated tax changes, are substantially smaller than those obtained using our measure of exogenous tax changes. Thus, failing to account for the reasons for tax changes can lead to substantially biased estimates of the macroeconomic effects of fiscal actions. We also find that the behavior of output following countercyclical tax changes is only moderately different from that following exogenous actions. This suggests that policymakers' efforts to adjust taxes to offset anticipated changes in private economic activity have been largely unsuccessful. Finally, we find suggestive evidence that tax increases to reduce an inherited budget deficit do not have the large output costs associated with other exogenous tax increases. This is consistent with the idea that deficit-driven tax increases may have important expansionary effects through expectations and long-term interest rates, or through confidence.

In considering the implications of these findings, it is important to note that our estimates are not highly precise. The overall estimates of the effects on output are overwhelmingly significant, but the confidence interval is substantial. And when we ask narrower questions – such as how a volatile component of output, such as investment, responds to tax changes, or how output behaves following a narrowly defined type of tax change, such as a deficit-driven change – the confidence interval is generally quite wide.

Although we place great emphasis on identifying tax changes that occur for reasons largely unrelated to other influences on output, there is an important sense in which this study is not in the "natural experiment" tradition. Rather than considering only a small fraction of tax changes, we examine all major postwar tax changes that resulted from policy actions, and we conclude that a substantial fraction of them can be used to estimate the effects of tax changes on output. Thus, the scope for increasing the precision of the estimates of the short-run and medium-run macroeconomic effects of tax changes appears limited. One could imagine improving the revenue estimates or finding better control variables, but the potential improvement through these channels is likely to be small.

A more promising route for extending the analysis is to use our new series to investigate the

importance of the characteristics of tax changes for their macroeconomic effects. There are strong reasons to expect the effects of a tax change on output to depend on such features of the change as its perceived permanence, its impact on marginal tax rates, and how it affects the tax treatment of investment. Thus it is natural to ask whether the output consequences of tax changes depend not only on their size, but on their other characteristics as well.

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Exhibit 1
Narrative Analysis of a Countercyclical Tax Change

Tax Reduction Act of 1975

      Signed   3/29/75
      1975Q2   –$58.1 billion   (Endogenous; Countercyclical)
      1975Q3   +45.3 billion   (Endogenous; Countercyclical)

The Tax Reduction Act of 1975 was a change in taxes that was made to try to return economic growth to normal. In early 1975, growth was weak and expected to remain weak in the absence of changes in policy. The 1975 Economic Report stated, "As 1975 begins, … production and employment are declining sharply. … It is quite likely … that the contraction of business activity and rising unemployment will continue for several more months" (p. 19). Likewise, the President's Annual Budget Message to the Congress, Fiscal Year 1976 declared: "It must be clearly understood that these problems are serious and that strong remedies are fully justified. The economy is now in a recession" (2/3/75, p. 2).

The administration therefore proposed a major tax cut "[t]o provide support for the economy" (1975 Economic Report, p. 20). However, the Economic Report was explicit that the tax cut would, at best, merely mitigate the expected decline: "The tax cut will not prevent a decline in real output from 1974 to 1975 but it will reduce the extent of the year-over-year decline" (p. 20). Presidential speeches confirm the view that the tax cut was designed to return growth to normal. In his Address Before a Joint Session of the Congress Reporting on the State of the Union, President Ford stated: "Cutting taxes now is essential if we are to turn the economy around. A tax cut offers the best hope of creating more jobs" (1/15/75, p. 2). Likewise, in the Annual Budget Message, he said: "These policies call for decisive action to restore economic growth" and "include a one-time $16 billion tax cut … to stimulate economic recovery" (2/3/75, p. 2). In his Address to the Nation Upon Signing the Tax Reduction Act of 1975, Ford again said: "Our country needs the stimulus and the support of a tax cut and needs it now" (3/29/75, p. 2). He said that though the tax cut was somewhat larger than he originally proposed, "the $23 billion tax reduction is within reason" (p. 1). He mainly lamented that the bill included "a lot of extraneous changes in our tax laws" and said, "This is no way to legislate fundamental tax reforms" (p. 1).

Congressional documents also suggest that the act was motivated by a desire to return growth to normal. The House report gave as the prime motivation for the bill the need "to check the drastic downward slide in our economy and to restore economic growth" (94th Congress, 1st Session, House of Representatives Report No. 94-19, 2/25/75, p. 5). It also stated: "The overall tax cut provided by your committee's bill is larger than the $16 billion tax cut recommended by the administration. However, your committee believes that the larger tax cut is more appropriate in the present situation, because the economic situation has deteriorated and forecasts of future economic activity in absence of remedial action are more pessimistic than at the time the administration presented its recommendations" (p. 8). This suggests that even the cuts beyond what the president proposed were aimed at securing normal, not supranormal growth.

Because the act was designed to stop the decline and return growth to normal, we classify it as an endogenous, countercyclical fiscal action.

Our sources give several figures for the size of the tax cut, all of them quite similar (Economic Report, 1976, pp. 48, 50-51; 1977, p. 75; Address to the Nation Upon Signing the Tax Reduction Act of 1975, 3/29/75, p. 1; 1977 Budget, p. 44). One very clear statement of the size and timing comes from the 1976 Economic Report. It stated: "In all, the Tax Reduction Act of 1975 lowered receipts by around $42 billion at an annual rate in the second quarter of 1975, but most of this drop was temporary. The tax cuts that remained in effect during the last half of 1975 amounted to around $15 billion (annual rate)" (p. 51). Translated into changes at an annual rate, these figures imply a tax cut of $42 billion in 1975Q2 and an increase of $27 billion in 1975Q3. These numbers, however, are for receipts, not liabilities, and do not appear to take into account the fact that the act not only included the rebate of 1974 taxes, but also a retroactive cut to January 1975.

The House report and the Conference report on the bill gave detailed revenue estimates. The final bill included a rebate of $8.125 billion of 1974 taxes (House Report No. 94-19, Table 1, p. 17; Congressional Record, 94th Congress, 1st Session, Vol. 121—Part 7, 3/26/75, p. 8880). Since the act was signed at the end of March, we date this as occurring in 1975Q2. At an annual rate, this was an endogenous tax cut of $32.5 billion. The

Conference report showed additional net tax cuts in 1975Q2 of $12.8 billion at an annual rate.[a] Because these cuts were retroactive to January 1, 1975, this implies an additional tax cut in 1975Q2 of $12.8 billion. Therefore, there was a total endogenous tax change in 1975Q2 of –$32.5 billion minus $12.8 billion minus $12.8 billion, or –$58.1 billion. Then, in 1975Q3 when the rebate and the retroactive tax cut disappeared, there was an endogenous tax increase of $32.5 billion plus $12.8 billion, or $45.3 billion. These numbers, while somewhat larger than those in the Economic Report, are broadly consistent. If the retroactive feature (but not the rebate) is ignored, the tax change would be –$45.3 billion in 1975Q2 and +$32.5 billion in 1975Q3, which is even closer to the Economic Report numbers.

The House report showed the revenue effects in 1976 as –$1.5 billion, implying a substantial tax increase in 1976Q1 (House Report No. 94-19, Table 1, p. 17). This is consistent with the tax cut being explicitly temporary. However, its provisions were extended and enlarged by the Revenue Adjustment Act of 1975, so the legislated tax increase did not take place.

Almost all the major provisions of the act were scheduled to be temporary. The large majority of the tax reductions took the form of rebates, tax credits, and increases in the standard deduction (1976 Economic Report, pp. 50-51). Thus, the changes lowered taxes for most taxpayers by similar amounts, with little impact on marginal tax rates. The act also included a temporary increase in the investment tax credit.

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[a] The table shows total net revenue effects from tax changes of –$20.9 billion. Subtracting off the –$8.1 billion due to the rebate yields –$12.8 billion.

Exhibit 2
Narrative Analysis of a Spending-Driven Tax Change

Revenue Act of 1951

      Signed   10/20/51
      1951Q4   +$10.0 billion   (Endogenous; Spending)
      1952Q1   −$4.6 billion   (Endogenous; Spending)

The motivation for the Revenue Act of 1951 was again the increase in spending related to the Korean War. The 1951 Economic Report stated: "These new taxes are required to finance the defense effort; and to help keep total spending within the capacity of current production, so that inflation does not reduce the purchasing power" (p. 17). This same sentiment was echoed in a number of presidential speeches. On February 2, 1951, Truman said, "If we do not tax ourselves enough to pay for defense expenditures, the Government will … add to total purchasing power and inflationary pressures" (Special Message to the Congress Recommending a "Pay as We Go" Tax Program, p. 2). These quotations make it clear that the tax increase was designed to keep growth normal.

The House report on the bill emphasized that the tax increase was explicitly to pay for the Korean War. It stated: "The military action in Korea, coupled with the general threat to world peace, has made it necessary to provide extraordinary increases in revenues to meet essential national defense expenditures" (82^d Congress, 1st Session, House of Representatives Report No. 586, 6/18/51, p. 1). It also stressed the temporary nature of the tax, saying: "The 12½-percent flat across-the-board increase [in individual income tax rates] was selected as the form of increase, since it is not intended that this increase will be permanent, and therefore, it was desired to provide an increase which would not become an integral part of the rate structure" (p. 8).

Because the tax increase was designed to counteract the effects of higher military spending and thereby keep growth normal, we classify it as an endogenous, spending-driven action.

Truman's speech upon signing the bill stated that it would raise about $5.5 billion of additional revenues at an annual rate (Statement by the President Upon Signing the Revenue Act of 1951, 10/20/51, p. 1). Both the 1952 Economic Report (p. 135) and the 1951 Treasury Annual Report (p. 44) gave the figure as $5.4 billion. We follow our usual practice and use the number from the Economic Report.

The increase in corporate taxes was retroactive to April 1, 1951, while the remainder took effect on November 1, 1951 (1951 Treasury Annual Report, p. 51). Because the action took place before the middle of the quarter, we date the tax increase in 1951Q4. Approximately $2.3 billion of the tax increase was from corporate taxes (p. 501). Therefore, the retroactive portion of the bill increased taxes at an annual rate of $2.3 billion · 2 extra quarters, or $4.6 billion. Adding this to the steady-state effect of $5.4 billion yields a tax increase of $10 billion in 1951Q4. The return to the steady-state level of tax increase implies a tax cut of $4.6 billion in 1952Q1. If one ignored the retroactive part of the legislation, there would be just a tax increase of $5.4 billion in 1951Q4.

The tax increase largely took the form of an increase in marginal rates. The act also raised the capital gains tax, the tax on corporate profits, and some excise taxes (1951 Treasury Annual Report, pp. 50-52). Most of the changes were explicitly temporary. The individual income tax increases were legislated to expire on January 1, 1954; the corporate and excise tax increases were to continue until March 31, 1954 (1951 Treasury Annual Report, pp. 51-52).

Exhibit 3

Narrative Analysis of a Deficit-Driven Tax Change

Omnibus Budget Reconciliation Act of 1993

Signed   8/10/93
1993Q3   +$68.4 billion   (Exogenous; Deficit)
1993Q4   –$40.3 billion   (Exogenous; Deficit)
1994Q1   +$13.4 billion   (Exogenous; Deficit)

The motivation for this tax change was deficit reduction. In a speech to Congress describing his economic proposals, President Clinton called for "a deficit reduction program that will increase the savings available for the private sector to invest, will lower interest rates, will decrease the percentage of the Federal budget claimed by interest payments, and decrease the risk of financial market disruptions that could adversely affect our economy" (Address Before a Joint Session of Congress on Administration Goals, 2/17/93, p. 2). He went on to say, "Over the long run, all this will bring us a higher rate of economic growth, improved productivity, more high-quality jobs, and an improved economic competitive position in the world" (p. 2). In a radio address, he said his plan called for "a little more in deficit reduction today, so that we can all enjoy better jobs and higher incomes tomorrow" (The President's Radio Address, 5/15/93, p. 1). Similarly, the 1994 Economic Report stated, "Reducing the budget deficit was a necessary part of clearing away the financial underbrush … so that economic growth could be put on a sounder and more sustained footing" (p. 31).

Congress also viewed the central purpose of the tax change as deficit reduction. The House report on the bill stated, "This bill embodies all the basic elements of President Clinton's program to turn our country away from being an excess-consumption economy and toward investment in the future productivity of our people" (103rd Congress, 1st session, House of Representatives Report No. 103-111, 5/25/93, pp. 1-2). The report went on to describe the bill as "the necessary first step in long-run deficit control" (p. 2), and to say that "[i]t reduces the huge drain on the nation's savings pool that the deficit represents" (p. 3). A secondary motive in Congress was increased progressivity. For example, the House report stated, "The tax package restores tax code progressivity lost in recent years" (p. 4). The justifications for the major specific revenue-increasing features of the package focused almost entirely on deficit reduction and progressivity (see, for example, pp. 635, 643, and 655). A desire to offset short-term cyclical factors was never mentioned as a reason for the changes. Thus, this tax change is clearly an exogenous, deficit-driven action.

The timing of the tax changes called for in the legislation was somewhat complicated. Large parts of the changes were retroactive to January 1, 1993, and some smaller changes were retroactive as well. Other major parts went into effect on January 1, 1994. Finally, some features went into effect between these two dates, notably an increase in the gasoline tax on October 1, 1993 (1995 Budget, Analytical Perspectives, pp. 36-39).

The effects of the bill on fiscal 1994 revenues were complicated by the retroactive features and by the fact that not all of the changes were in effect for the full fiscal year. We therefore use the estimated revenue effects for fiscal 1995 (the first full fiscal year the changes were in effect) as the starting point for our estimates. CBO reported that the bill was expected to increase revenues in fiscal 1995 by $41.5 billion (An Economic Analysis of the Revenue Provisions of OBRA-93, January 1994, pp. 2-3). A Joint Committee on Taxation document (Estimated Budget Effects of the Revenue Provisions of H.R. 2264 (the Omnibus Budget Reconciliation Act of 1993) as Agreed to by the Conferees, JCX-11-93, 8/4/93) provided a detailed breakdown of this figure by provision, together with their effective dates. $22.8 billion was retroactive, almost entirely to the beginning of 1993; $5.3 billion went into effect during 1993, almost entirely on October 1; and $13.4 billion went into effect at the beginning of 1994.

Combining these estimates and using our usual procedure for dealing with retroactive changes yields a tax increase of $68.4 billion in 1993Q3, a cut of $40.3 billion in 1993Q4, and an increase of $13.4 billion in 1994Q1. If one did not account for the retroactive features, the estimates would be an increase of $22.8 billion in 1993Q3, an increase of $5.3 billion in 1993Q4, and an increase of $13.4 billion in 1994Q1.

The bill also included provisions calling for substantial spending cuts. The administration estimated the reductions, including lower interest payments because of lower deficits, at $255 billion over five years (Remarks on Signing the Omnibus Budget Reconciliation Act of 1993, 8/10/93, p. 1). CBO estimated the reductions, excluding reduced interest payments, as $146 billion over the same period (An Economic Analysis of the Revenue Provisions of OBRA-93, January 1994, p. 1).

Roughly two-thirds of the additional revenues came from higher marginal rates on high-income individuals (from both the regular income tax and the repeal of the cap on income subject to the Medicare tax). The remaining third came from a wide array of sources. The changes were almost all intended to be permanent.

Exhibit 4

Narrative Analysis of a Long-Run Tax Change

Revenue Act of 1964

        Signed   2/26/64
        1964Q2   –$16.8 billion   (Exogenous; Long-run)
        1964Q3   +$8.4 billion   (Exogenous; Long-run)
        1965Q1   –$4.5 billion   (Exogenous; Long-run)

        The motivation for the 1964 tax cut was the same as for the 1962 investment tax credit: faster long-run growth. Once again, there was no fear of a recession at the time the act was proposed or passed. The Revenue Act of 1964 was first proposed in the summer of 1962. President Kennedy, in his Radio and Television Report to the American People on the State of the National Economy, stated explicitly that the tax cut was not for countercyclical reasons: "Let me emphasize, however, that I have not been talking about a different kind of tax cut, a quick, temporary tax cut, to prevent a new recession" (8/13/62, p. 5). This view was repeated in two speeches in January 1963 (Annual Message to the Congress on the State of the Union, 1/14/63, pp. 1-2; Special Message to the Congress on Tax Reduction and Reform, 1/24/63, p. 1). Likewise, the 1963 Economic Report stated: "We approach the issue of tax revision, not in an atmosphere of haste and panic brought on by recession or depression, but in a period of comparative calm" (p. xiii). The Economic Report mentioned the possible countercyclical benefits of the tax cut, but made it clear that they were a sidelight. It stated: "While the basic purpose of my tax program is to meet our longer run economic challenges, we should not forget its role in strengthening our defenses against recession" (p. xxi). A similar statement was made in the 1965 Economic Report (p. 8). If anything, the economy was even stronger by the time the act was passed. President Johnson, in his Annual Budget Message to the Congress, Fiscal Year 1965, cited statistics showing solid economic growth and emphasized: "This is a record of strong expansion" (1/21/64, p. 3).

        Kennedy and Johnson both gave as the rationale for the tax cut the need to eliminate fiscal drag so the economy could grow faster. In his August 1962 address, President Kennedy said: "our present tax system is a drag on economic recovery and economic growth," and "this administration intends to cut taxes in order to build the fundamental strength of our economy, to remove a serious barrier to long-term growth" (Radio and Television Report to the American People on the State of the National Economy, 8/13/62, p. 4). In his Special Message to the Congress on Tax Reduction and Reform, Kennedy stated: "the largest single barrier to full employment of our manpower and resources and to a higher rate of economic growth is the unrealistically heavy drag of Federal income taxes on private purchasing power, initiative, and incentive" (1/24/63, p. 1). Johnson reiterated this view (Annual Budget Message to the Congress, Fiscal Year 1965, 1/21/64, p. 1). Both administrations argued that the tax cut would stimulate economic growth. For example, the 1964 Economic Report stated: "The tax cut will give a sustained lift, year-in and year-out, to the American economy" (p. 8).

        As with the 1962 tax cut, there was much discussion of an output gap and less-than-full employment. But, it is clear that performance was not perceived as low relative to normal, only low relative to ideal. For example, Kennedy stated in his Annual Message to the Congress on the State of the Union: "America has enjoyed 22 months of uninterrupted economic recovery. But recovery is not enough. If we are to prevail in the long run, we must expand the long-run strength of our economy. We must move along the path to a higher rate of growth and full employment" (1/14/63, pp. 1-2). Johnson sounded a similar theme in January 1964. He stated: "despite the creation of 2 1/2 million new jobs in our economy, the unemployment rate now stands at 5 1/2 %. Our factories continue to produce below their optimum rate. As a nation we are producing at a rate at least $30 billion below our comfortable capacity" (Annual Budget Message to the Congress, Fiscal Year 1965, 1/21/64, p. 3).

        The discussion of the reason for the tax cut given in Congressional documents parallels those in administration sources. The House report on the 1963 version of the bill stated: "The principal purpose of the revenue bill of 1963 is to remove from the private sector of the American economy its present high-tax straitjacket; that is, to lessen restraints which prevent the American free-enterprise system from itself generating necessary growth. A purpose of this bill also is to improve the equity of the tax laws" (88th Congress, 1st Session, House of Representatives Report No. 749, 9/13/63, p. 6). The Senate report also stressed the motivation of improving incentives and equity: "The bill will cut back on excessive tax rates which unnecessarily restrain individual and business incentives, it will provide the increased consumer and business purchasing power to assure continued expansion, and it will improve the equity of the tax system" (88th Congress, 2[d] Session, Senate Report No. 830,

1/28/64, p. 1).

Like the administration sources, both Congressional reports mention the need to reduce unemployment as an important motivation. However, it is clear that the desire was to reduce unemployment below its historical norm. The Senate report stated: "Despite the fact that business conditions have been improving over the past 33 months, unemployment still is at the high rate of 5.5 percent" (Senate Report No. 830, p. 6). It also noted: "we have experienced a succession of disappointing recoveries in which the unemployment rate has remained disturbingly high; this rate, in fact, has not been below 5 percent since 1957" (p. 6). It concluded that "the growth rate of our economy must be increased if the requisite jobs are to be found for this expanding labor force" (p. 6). The House report was even more explicit that the motivation of the bill was to provide supranormal growth. It stated: "Maintaining the 3-percent rate of growth as the United States has done since 1956, not only will fail to eliminate the present excessive unemployment, but unemployment will continue to rise as the increasing numbers of children born during the war and early postwar years reach employment age" (House Report No. 749, p. 10).

Because the Revenue Act of 1964 was motivated by a desire for faster-than-normal growth, and not by concern about current cyclical conditions, we classify it as an exogenous, long-run change.

The legislation cut taxes in two stages. The cut in 1964, which was passed in late February, was made retroactive to January 1, 1964. There was an additional cut in January 1965. Around the time the bill was passed, the revenue effects were generally reported as a decline of $7.7 billion in 1964 and $11.5 billion in 1965 (for example, 1963 Treasury Annual Report, pp. XVII, XXIII; Radio and Television Remarks Upon Signing the Tax Bill, 2/26/64, p. 1; and 1964 Economic Report, p. 8). However, these calculations were performed at 1963 income levels (1963 Treasury Annual Report, p. XXIII). The 1965 Economic Report reported the effects in 1964 at expected 1964 income levels as $8.4 billion (p. 65). We use this figure as our estimate of the tax cut in 1964.

Both the 1965 Economic Report (p. 65) and the President's 1966 Budget Message (Annual Budget Message to the Congress, Fiscal Year 1966, 1/25/65, p. 4) reported that the full cut would reduce revenues in 1965 by $14 billion. The 1965 Treasury Annual Report gave the figure of $13.7 billion, and made it clear that this was at expected 1965 income levels (pp. 275, 294). Some of the additional revenue loss in 1965 compared with 1964, however, reflected not the additional cuts in 1965, but a greater effect of the initial cuts because of rising incomes. It appears that the effect of a given set of tax cuts was expected to increase by about 9 percent per year. For example, the estimated effect of the 1964 cuts was 8.7 percent greater at 1964 incomes than at 1963 incomes, and the estimated effect of the overall cut was 17.5 percent higher at 1965 incomes than at 1963 incomes (percentage changes are computed as changes in logs, and $13.7 billion is used for the effect of the overall cut at 1965 incomes). This is consistent with 6 percent annual nominal GNP growth (1966 Budget, p. 50) and an elasticity of the revenue loss with respect to nominal GNP of about 1.5. We therefore estimate that in the absence of the second round of tax cuts, the 1964 reductions would have lowered revenue in 1965 by 9 percent more than $8.4 billion, or $9.2 billion. Thus our estimate of the effect of the additional cuts at the beginning of 1965 is a revenue reduction of $13.7 billion minus $9.2 billion, or $4.5 billion.

This estimate is broadly consistent with the statement in the 1965 Economic Report that the 1965 cuts would lower individual income taxes by $3 billion and corporate income taxes by $1 billion (p. 10). It is also consistent with the fact that two-thirds of the reduction in individual income tax rates – which were by far the largest part of the tax cut – occurred in 1964 and one-third in 1965 (1964 Treasury Annual Report, p. 243).

The tax cut was signed more than halfway through the first quarter of 1964. Therefore, following our usual procedure, we assign the first stage of the cut to 1964Q2. Because the tax cut was retroactive to January 1, 1964, our usual procedures identify a tax cut (at an annual rate) of $8.4 billion plus ¼ ($8.4 billion) · 4, or $16.8 billion, in 1964Q2. The retroactive part then disappeared in 1964Q3. Thus, there was an exogenous tax increase of $8.4 billion in that quarter. We then identify a second exogenous tax cut of $4.4 billion in 1965Q1. Note, if one chose to ignore the retroactive nature of the tax cut, the revenue estimates would be: –$8.4 billion in 1964Q2 and –$4.5 billion in 1965Q1.

The Revenue Act of 1964 lowered marginal tax rates from the previous range of 20-91% to 14-70%. It also lowered corporate tax rates, with the largest reduction being for small businesses (Annual Budget Message to the Congress, Fiscal Year 1965, 1/21/64, pp. 3-4). The tax decrease was permanent.

Table 1
Estimated Impact of Tax Changes on GDP Growth

Variable	Using New Measure of Exogenous Tax Changes	Using Change in Cyclically Adjusted Revenues
Constant	0.79	1.03
	(0.07)	(0.12)
Tax Change:		
Lag 0	–0.16	0.03
	(0.18)	(0.14)
Lag 1	–0.16	0.19
	(0.20)	(0.14)
Lag 2	–0.04	0.12
	(0.20)	(0.14)
Lag 3	–0.33	–0.23
	(0.20)	(0.14)
Lag 4	–0.33	–0.14
	(0.20)	(0.14)
Lag 5	–0.27	–0.20
	(0.20)	(0.14)
Lag 6	–0.26	–0.33
	(0.20)	(0.14)
Lag 7	–0.57	–0.16
	(0.17)	(0.14)
Lag 8	–0.36	–0.21
	(0.17)	(0.14)
Lag 9	–0.35	–0.17
	(0.17)	(0.14)
Lag 10	–0.20	0.01
	(0.17)	(0.14)
Lag 11	0.05	0.11
	(0.17)	(0.14)
Lag 12	0.05	–0.15
	(0.16)	(0.14)
R^2	0.09	0.09
D.W.	1.29	1.33
s.e.e.	0.96	0.94

Note: Estimates of equation (1) in the text. The sample period is 1950Q1–2006Q2 (1950Q2–2006Q2 for cyclically adjusted revenues). The numbers in parentheses are standard errors.

Figure 1
New Measure of Exogenous Tax Changes

a. All Exogenous Tax Changes
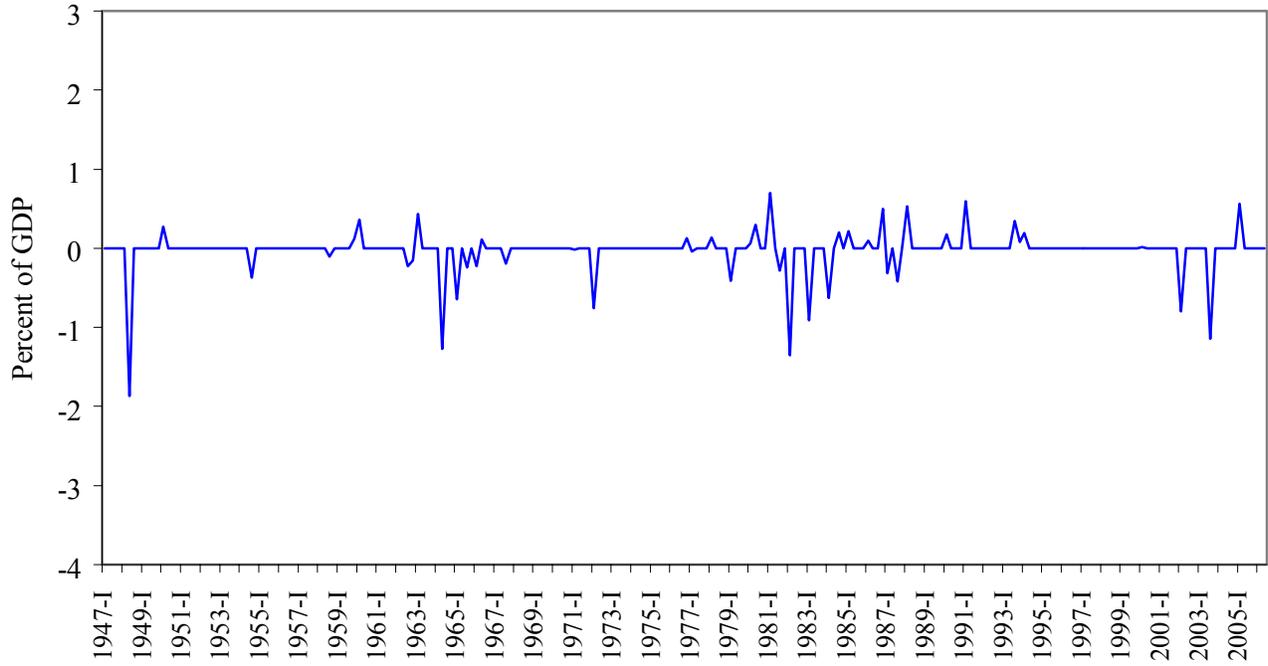
b. Long-Run and Deficit-Driven Tax Changes
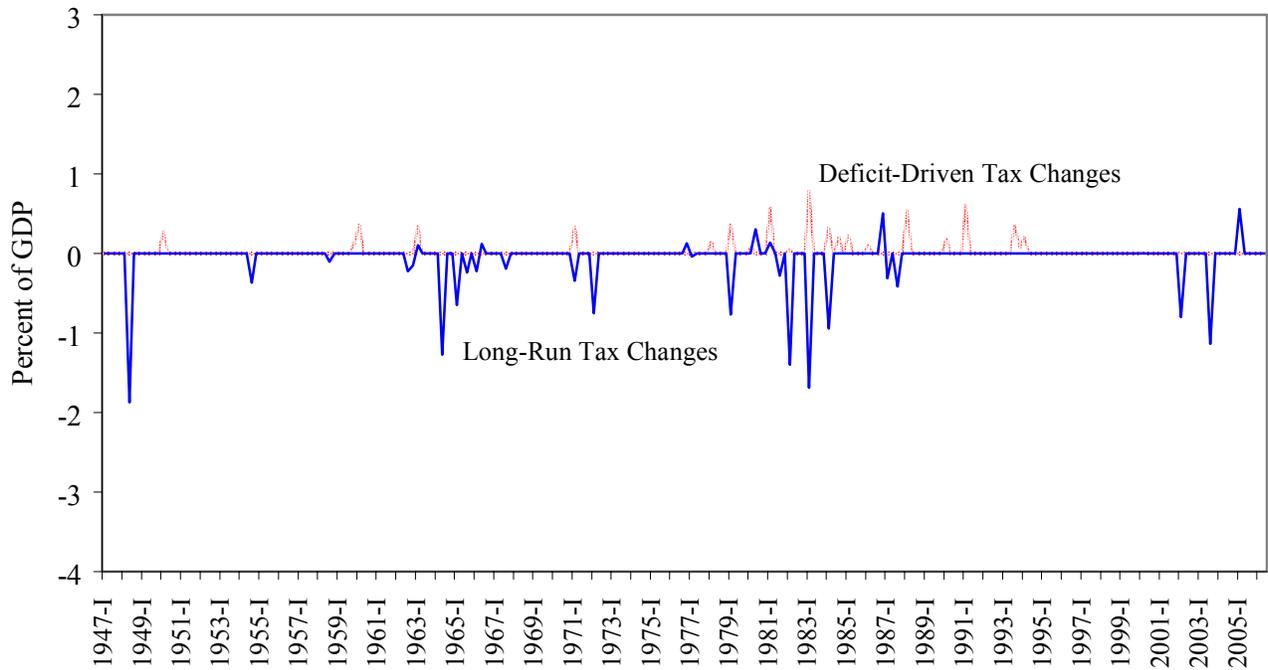
Figure 2
New Measure of Endogenous Tax Changes

a. All Endogenous Tax Changes
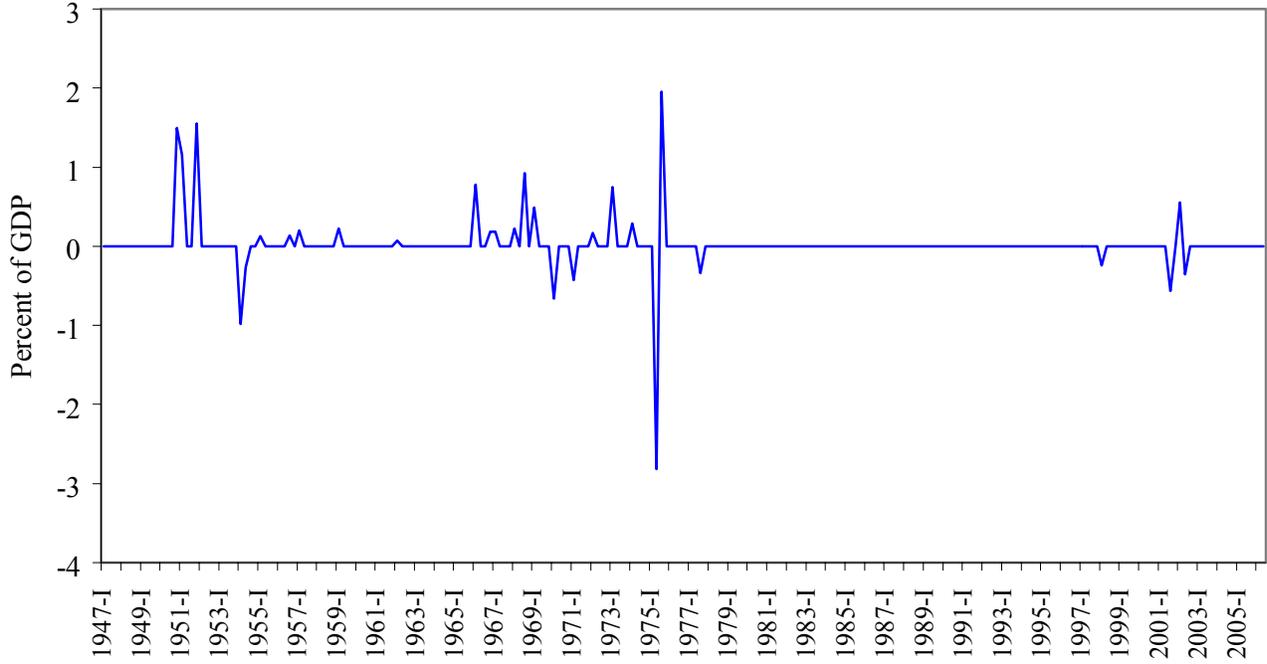
b. Countercyclical and Spending-Driven Tax Changes
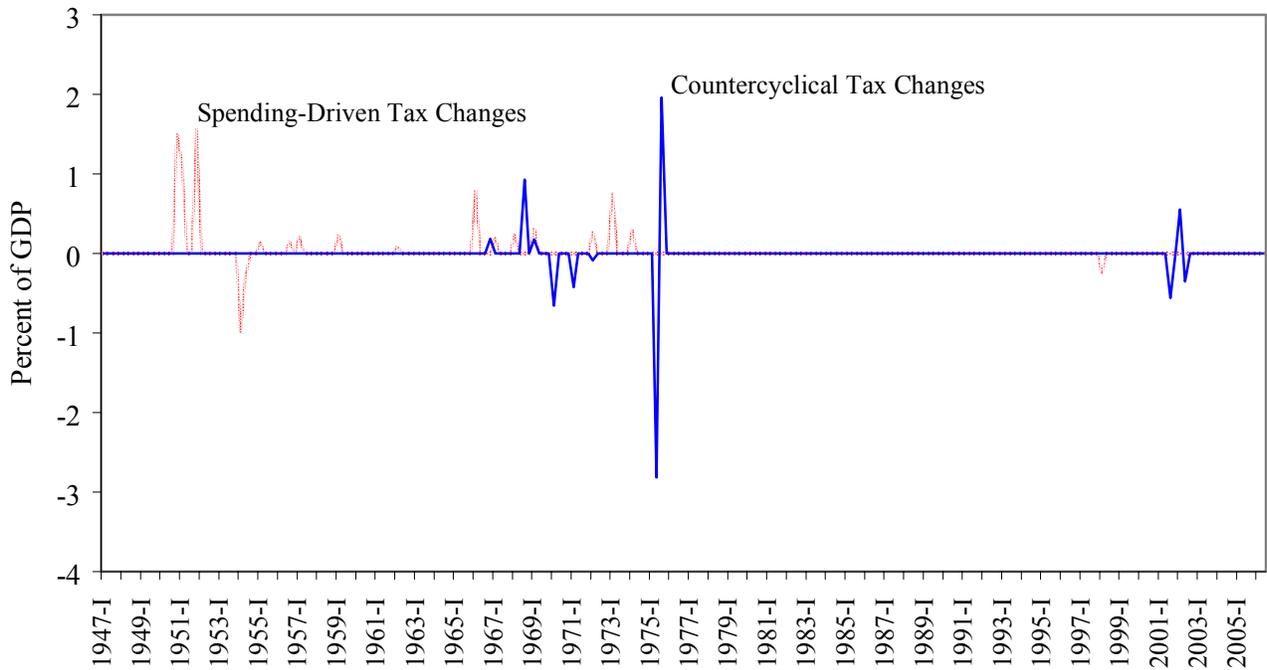
Figure 3

Comparing New Measures of Tax Changes and Cyclically Adjusted Revenues

a. Exogenous Tax Changes and the Change in Cyclically Adjusted Revenues
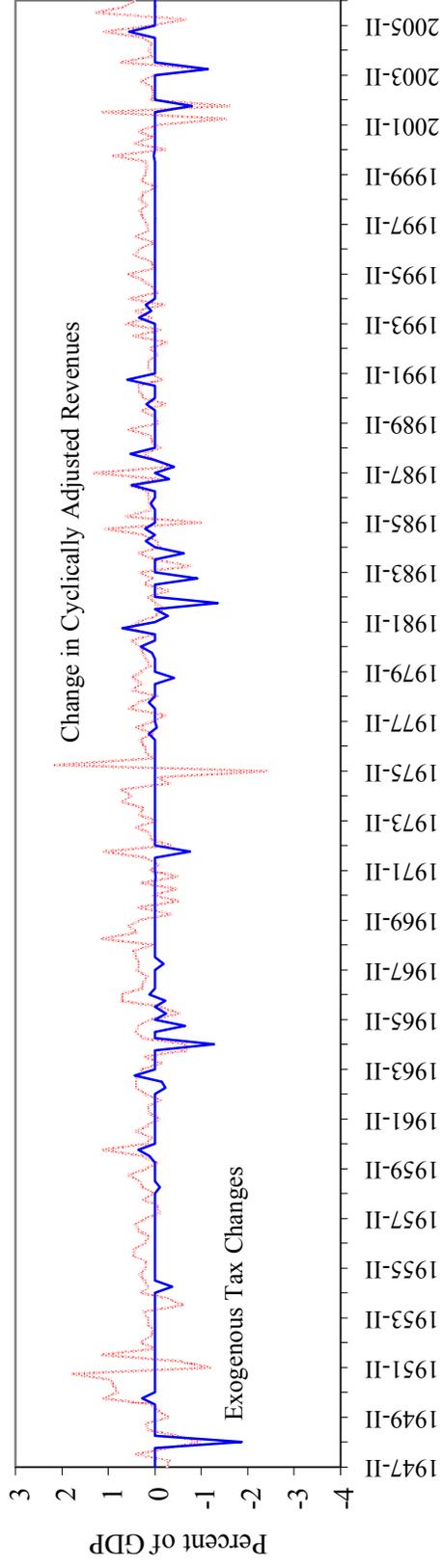
b. All Legislated Tax Changes and the Change in Cyclically Adjusted Revenues
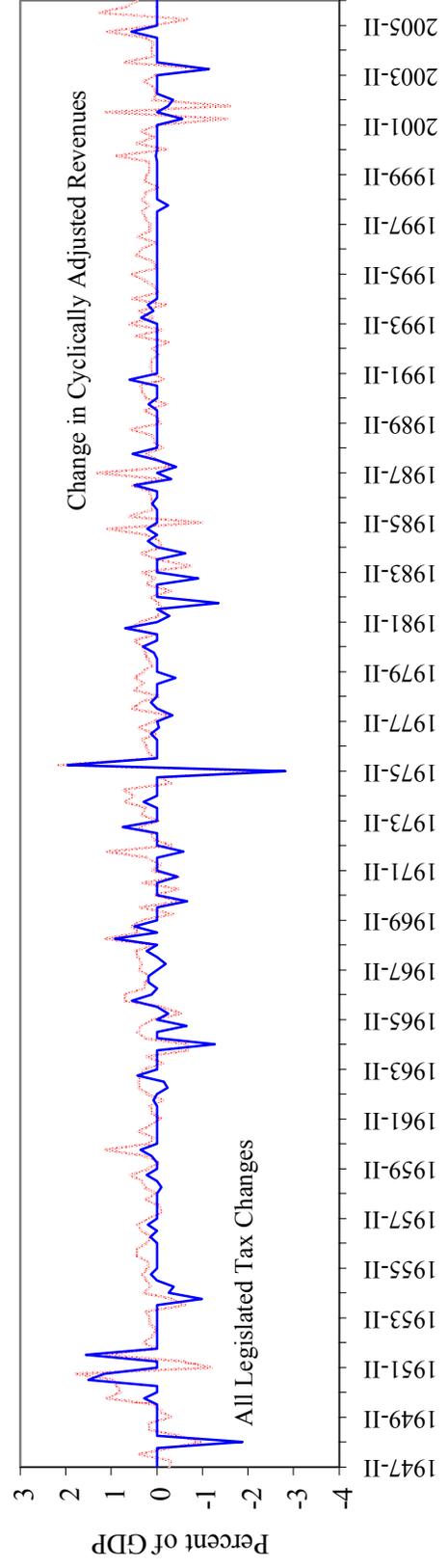
Figure 4

Estimated Impact of an Exogenous Tax Increase of 1% of GDP on GDP
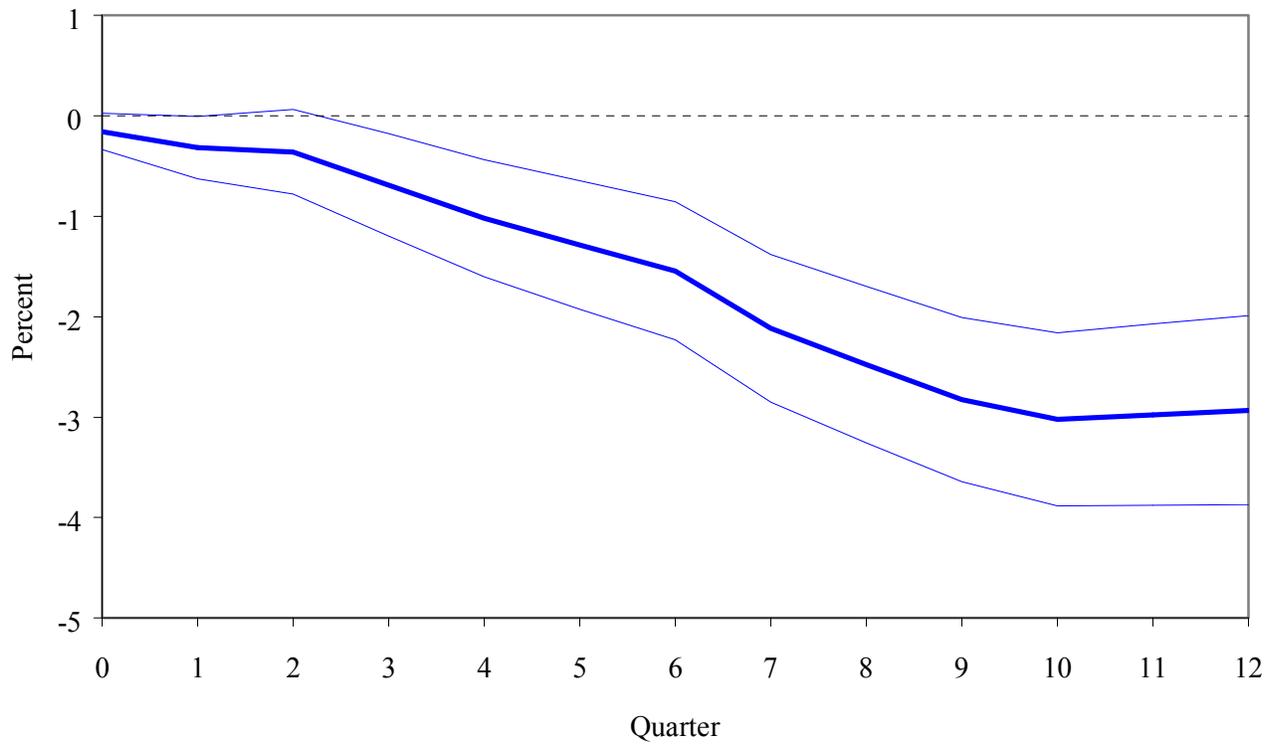
Figure 5
Estimated Impact of a Tax Increase of 1% of GDP on GDP

a. Using Exogenous Tax Changes and the Change in Cyclically Adjusted Revenues
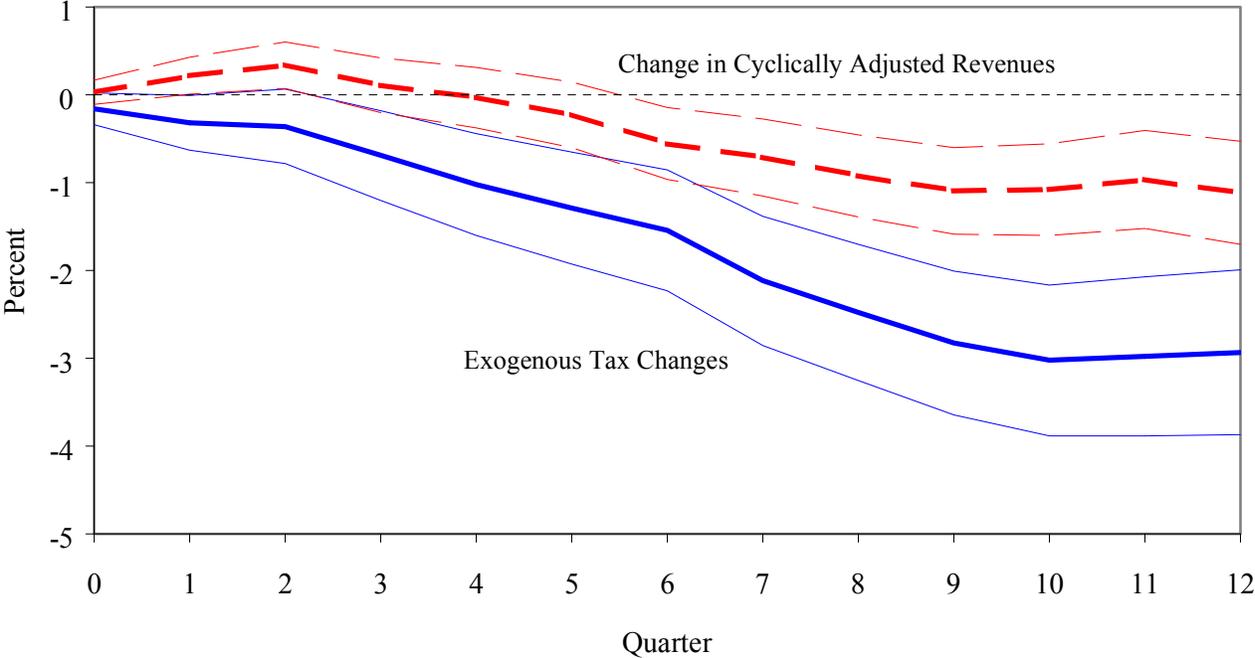
b. Using All Legislated Tax Changes and Endogenous Tax Changes
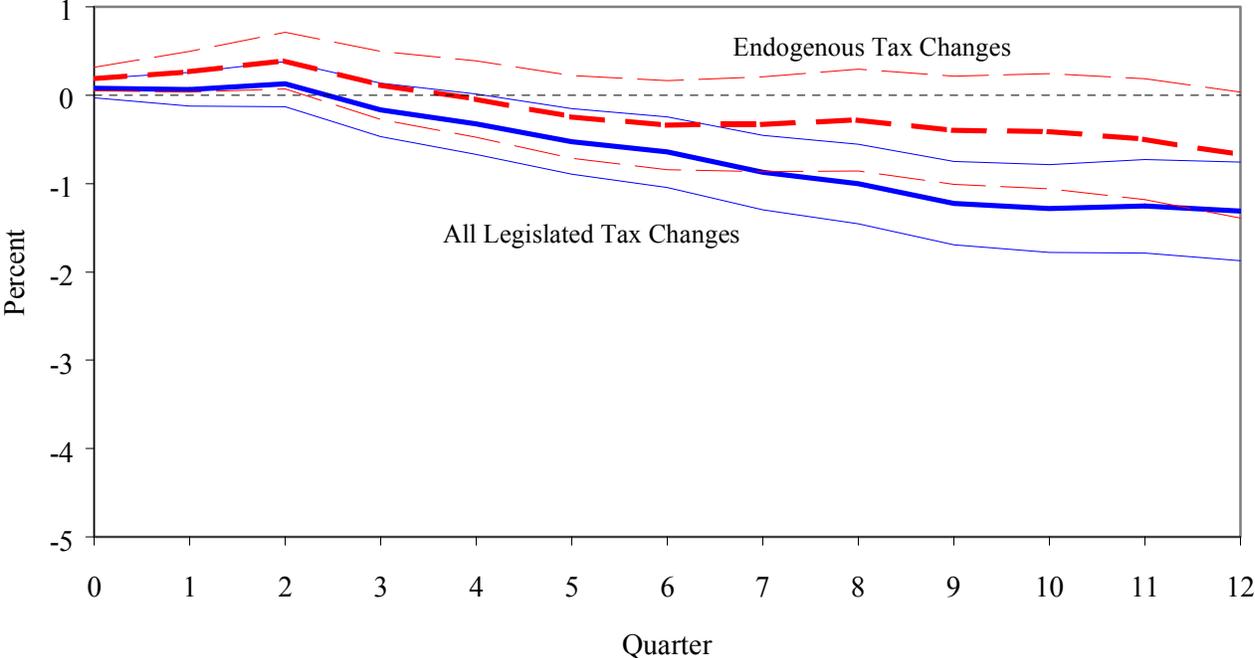
Figure 6
Estimated Impact of a Tax Increase of 1% of GDP on GDP

a. Using Countercyclical and Spending-Driven Tax Changes

Spending-Driven Tax Changes

Countercyclical Tax Changes
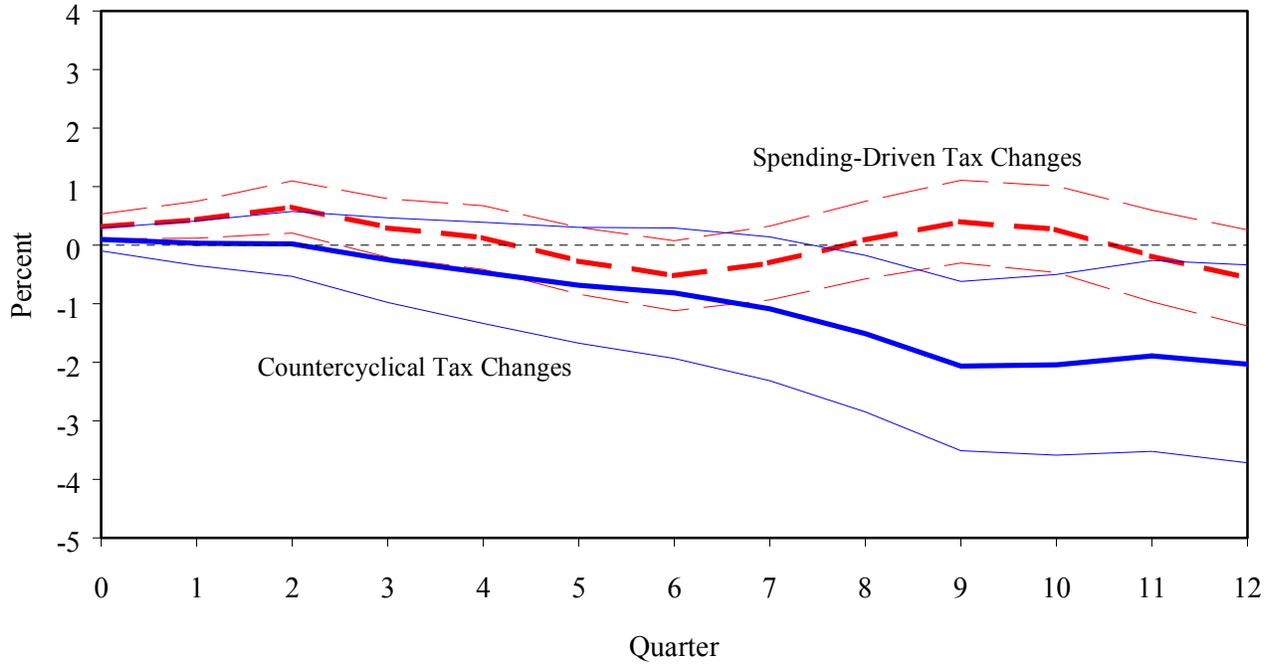
b. Using Long-Run and Deficit-Driven Tax Changes

Deficit-Driven Tax Changes

Long-Run Tax Changes
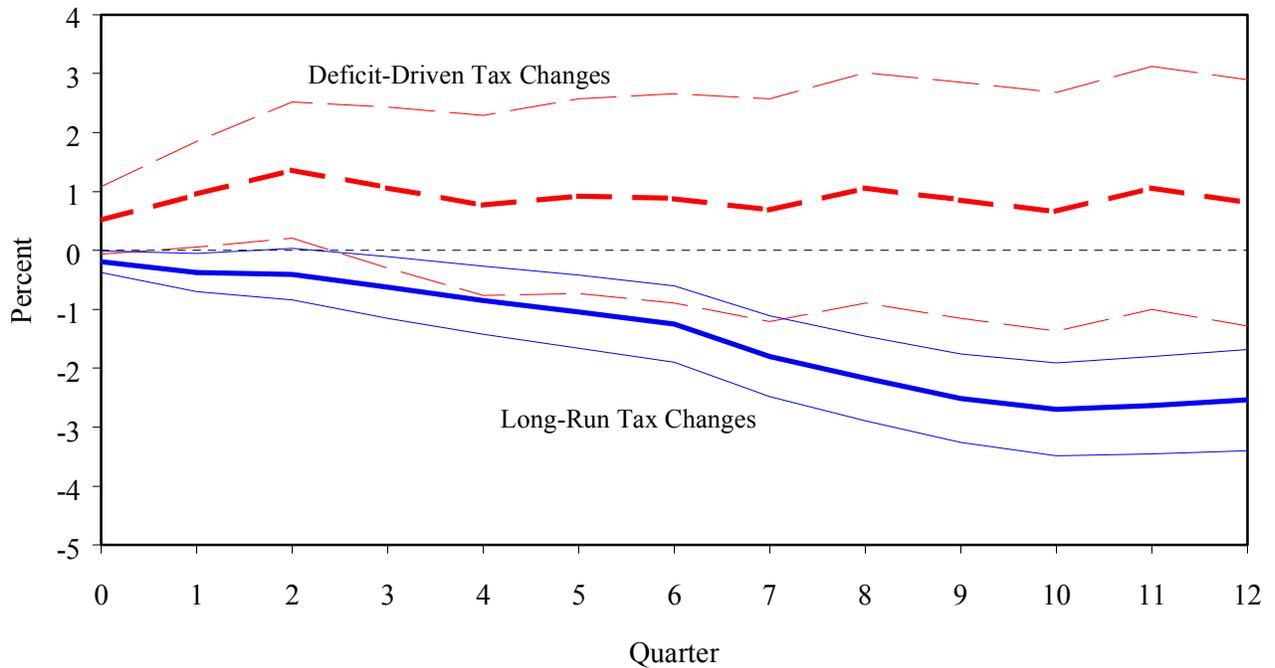
Figure 7
Estimated Impact of a Tax Increase of 1% of GDP on GDP Excluding Korea
Using Exogenous Tax Changes and the Change in Cyclically Adjusted Revenues
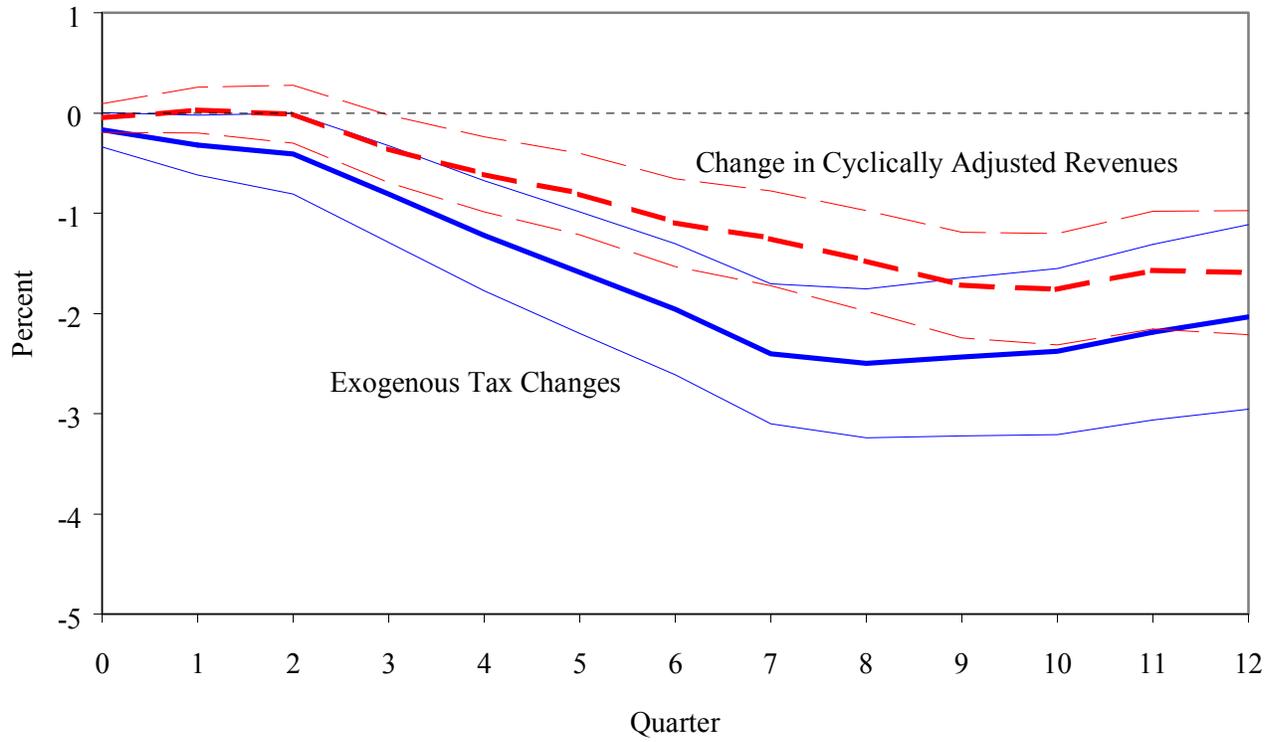
Figure 8
Estimated Impact of an Exogenous Tax Increase of 1% of GDP on GDP
Controlling for Lagged GDP Growth
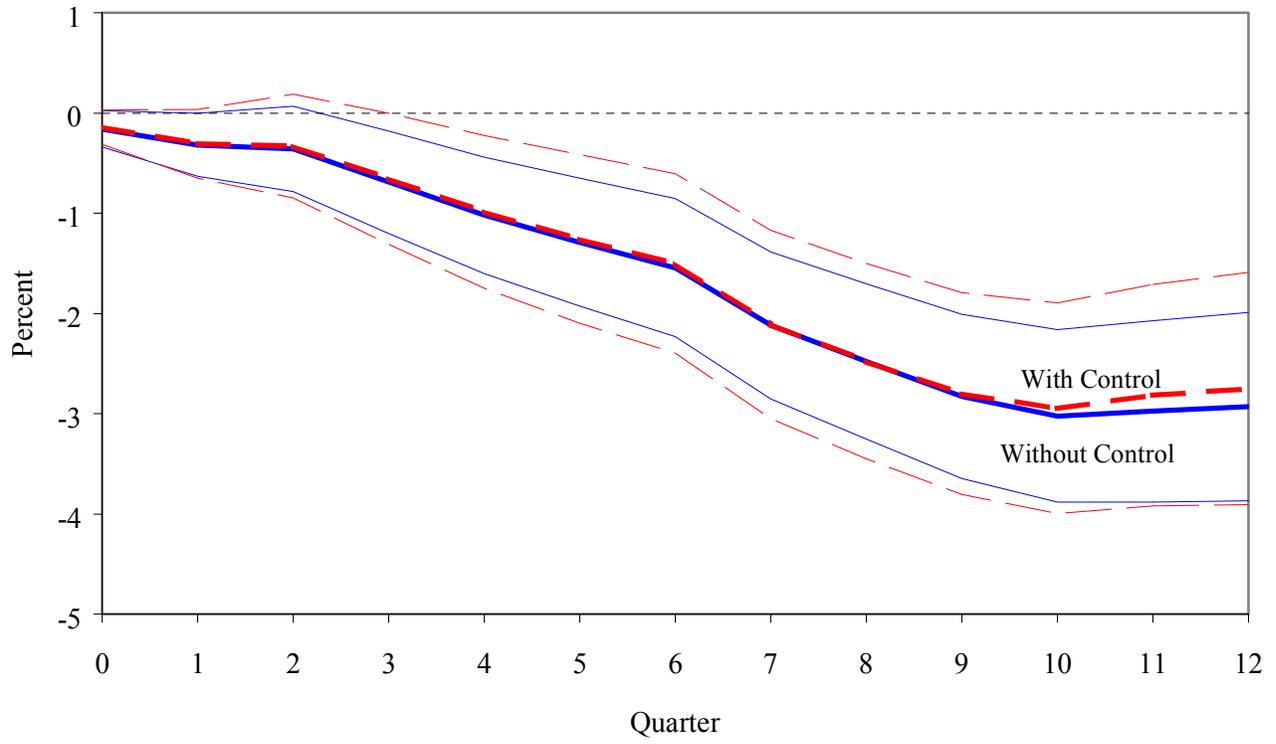
Figure 9
Estimated Impact of an Exogenous Tax Increase of 1% of GDP on GDP
Controlling for Monetary Policy

a. Using Dummy Variable for Shifts to Anti-Inflationary Policy
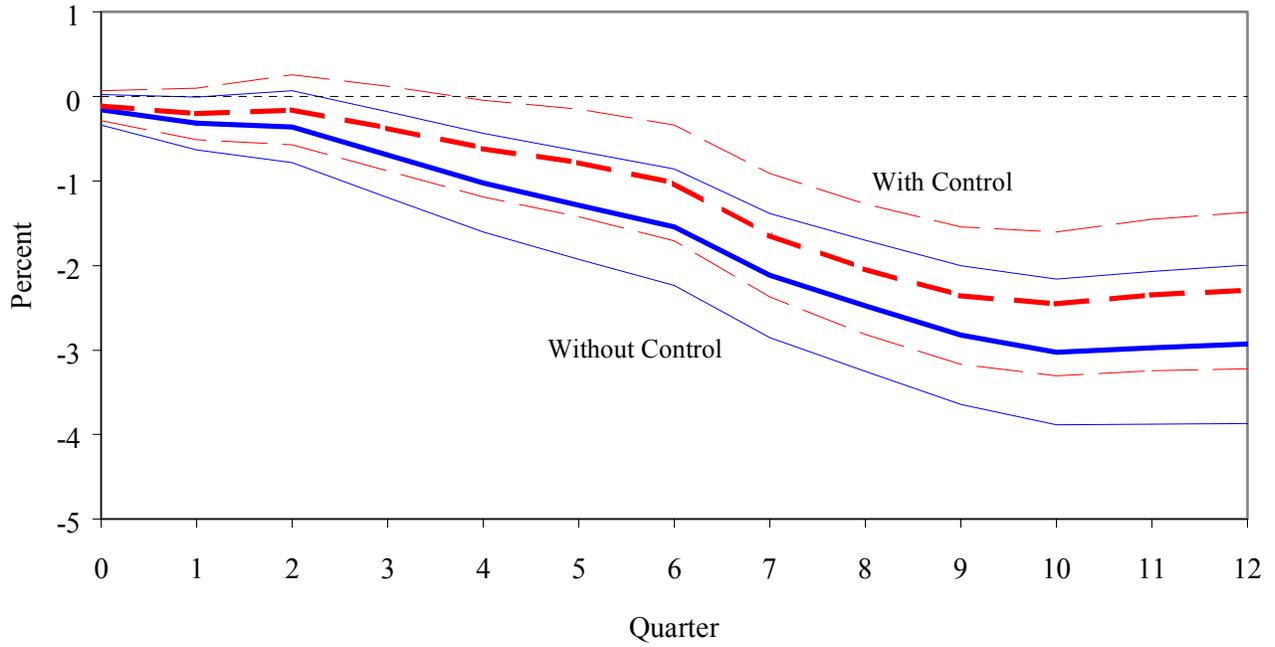
b. Using Measure of Monetary Shocks Derived from FOMC Forecasts
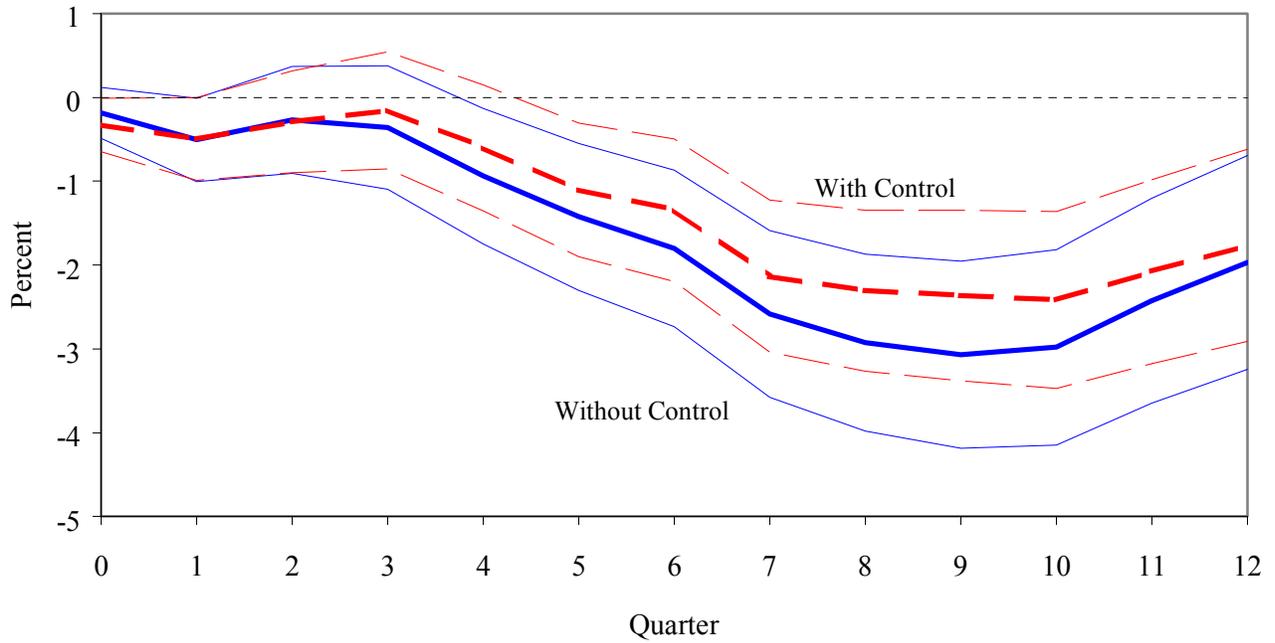
Figure 10
Estimated Impact of a Tax Increase of 1% of GDP on GDP
Controlling for Government Spending

a. Using Exogenous Tax Changes
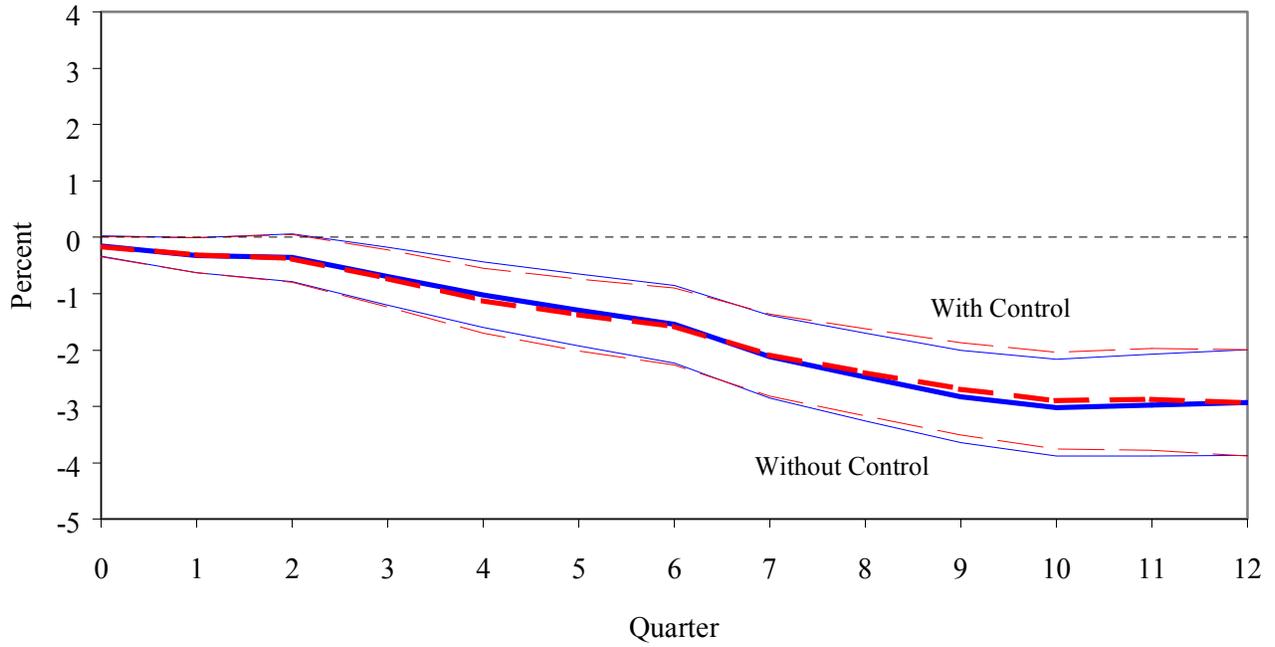
b. Using Deficit-Driven Tax Changes
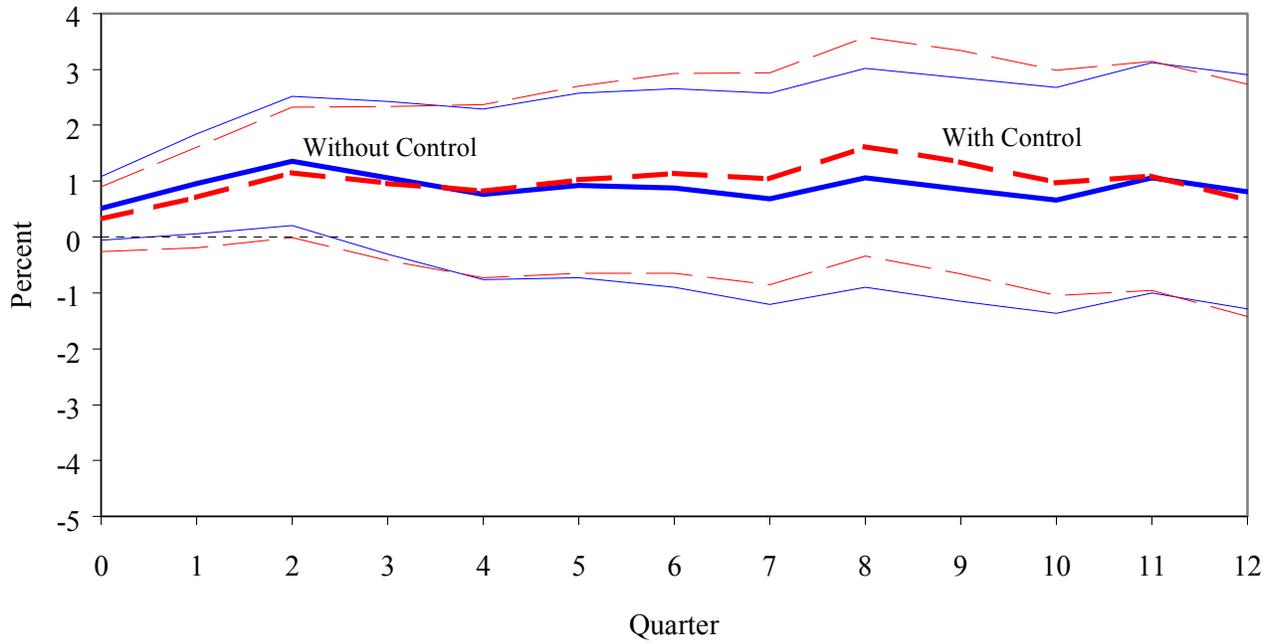
Figure 11
Estimated Impact of a Tax Increase of 1% of GDP on GDP
Including Tax Changes Dated Both at Time of Change and at Time of Passage

a. Using Exogenous Tax Changes

Present Value of Tax Changes Dated in Quarter of Passage

Tax Changes Dated in Quarter of Change

Quarter

b. Using Deficit-Driven Tax Changes

Present Value of Tax Changes Dated in Quarter of Passage

Tax Changes Dated in Quarter of Change

Quarter
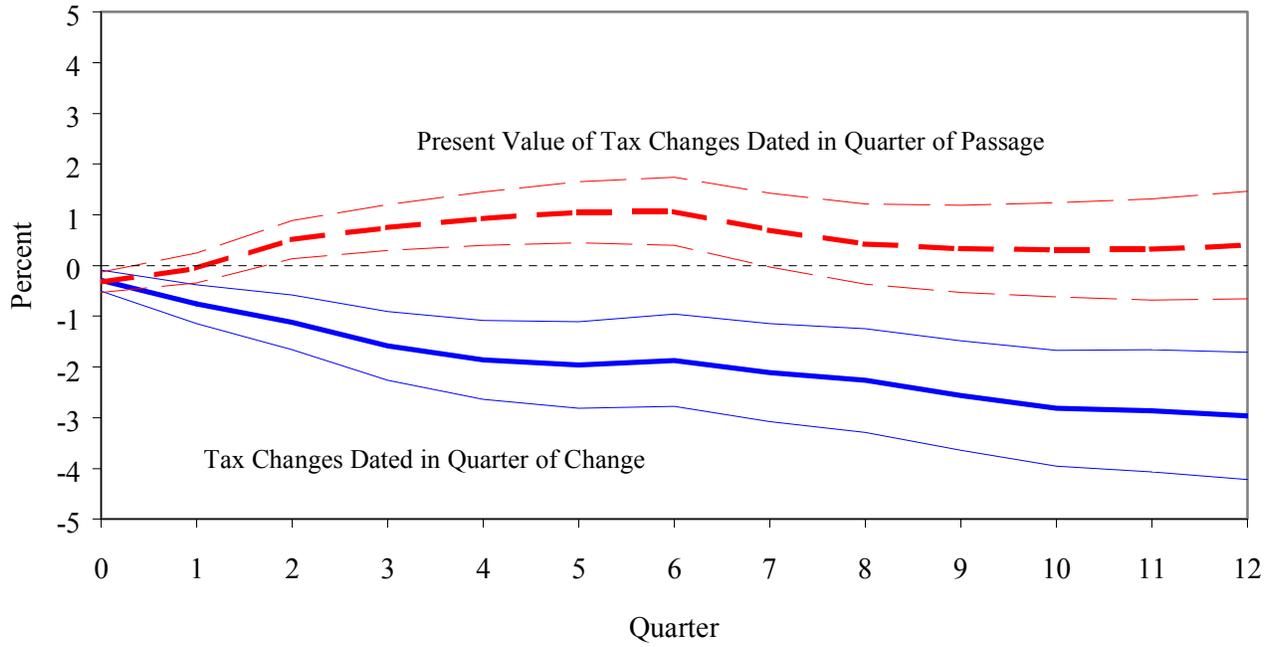
Figure 12
Estimated Impact of an Exogenous Tax Increase of 1% of GDP on GDP
Splitting the Sample in 1980Q4
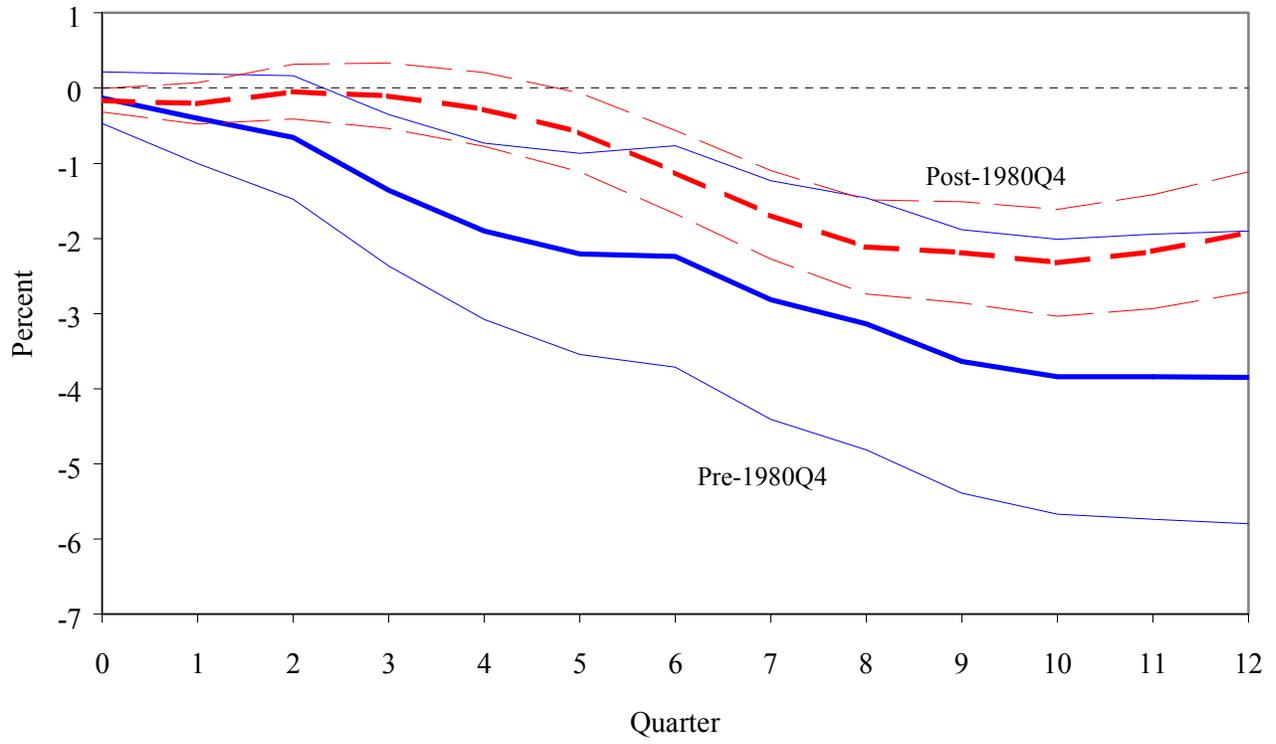
Figure 13
Estimated Impact of an Exogenous Tax Increase of 1% of GDP
on Inflation and Unemployment

a. On the Inflation Rate
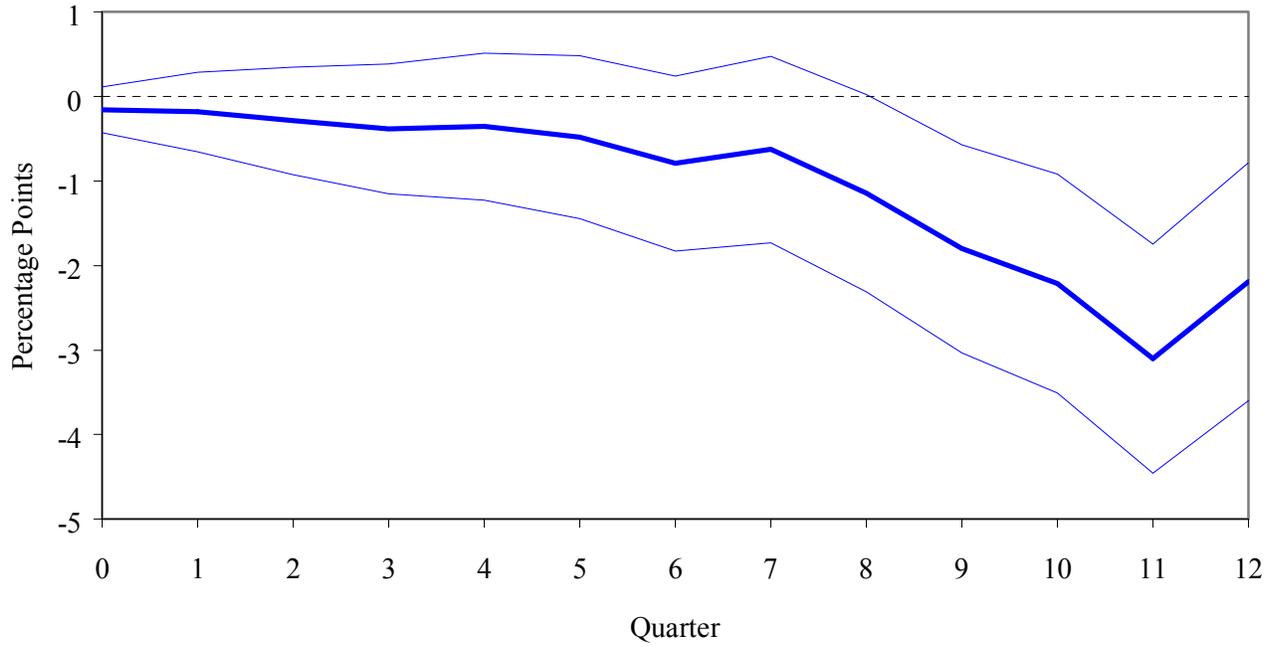
b. On the Unemployment Rate
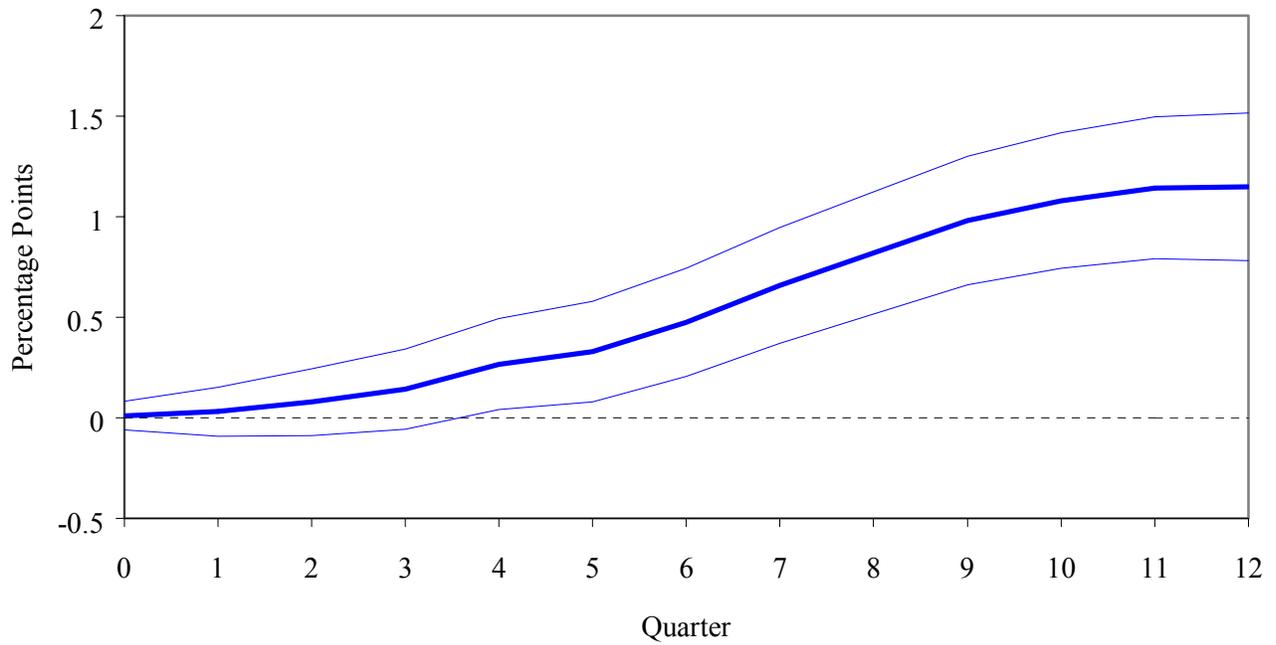
Figure 14

Estimated Impact of an Exogenous Tax Increase of 1% of GDP on the Components of GDP

a. GDP, Consumption, and Investment
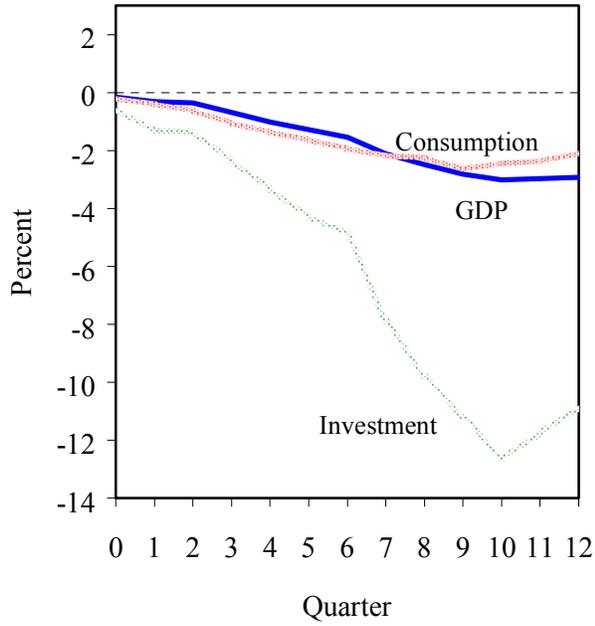
b. Consumption Expenditures on
    Durables, Nondurables, and Services
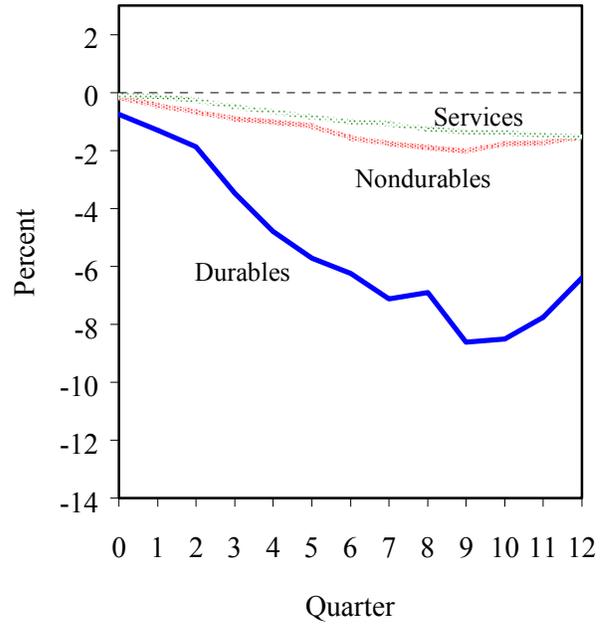
c. Investment, Nonresidential, and
    Residential Fixed Investment
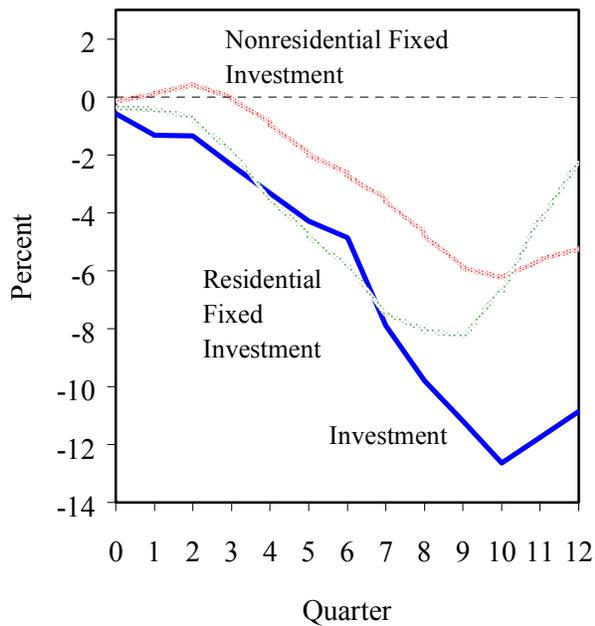
d. Exports and Imports
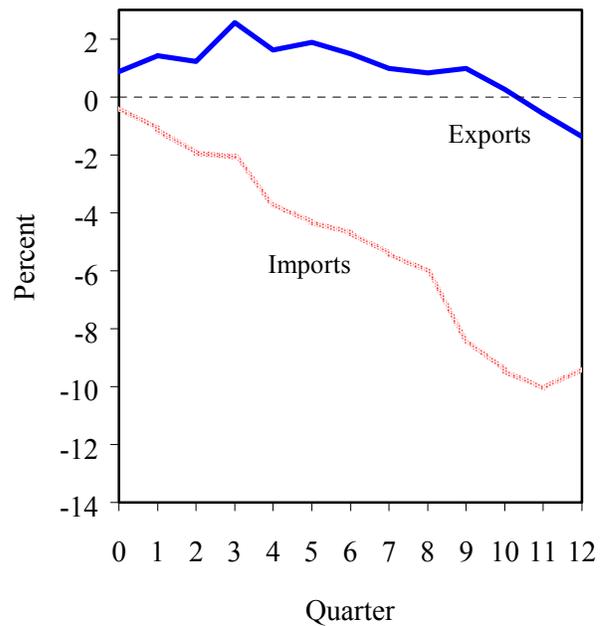
Figure 15
## Estimated Impact of a Deficit-Driven Tax Increase of 1% of GDP on the Components of GDP

a. GDP, Consumption, and Investment

b. Consumption Expenditures on Durables, Nondurables, and Services

c. Investment, Nonresidential, and Residential Fixed Investment

d. Exports and Imports