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

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April 2008

We are grateful to Susanto Basu, Olivier Blanchard, Raj Chetty, John Cochrane, Francesco Giavazzi, and Fabio Schiantarelli for helpful comments and suggestions, to Priyanka Rajagopalan for research assistance, and to the National Science Foundation for financial support.
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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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. Tax changes occur for many reasons. Some legislated tax changes are passed for philosophical reasons or to reduce an inherited budget deficit. Others are passed because the economy is weak and predicted to fall further, or because a war is in progress and government spending is rising. And, many tax changes are not legislated at all, but occur automatically because the tax base varies with the overall level of income, or because of 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. There is pervasive omitted variable bias in any regression of output on an aggregate measure of tax changes.

This paper suggests one way of dealing with this omitted variable bias. There exists a vast narrative record describing the history and motivation of tax policy changes. We first use this narrative history to separate legislated tax changes from those arising from non-policy developments. We then use the information on motivation to separate the 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. Finally, we 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.

Section I of the paper elaborates on the conceptual framework for this study. It emphasizes that what we seek to identify from the narrative record are tax changes that are not systemically correlated
with other developments affecting output. For want of a better term, we call these tax changes “exogenous.” It demonstrates why broader measures of tax changes are likely to lead to biased estimates of the effects of tax policy, and shows that simply including available control variables is very unlikely to eliminate the bias.

Section II discusses the narrative analysis that forms the foundation of this study. We use sources such as presidential speeches, the *Economic Reports of the President*, and reports of Congressional committees to identify the key characteristics of postwar legislated tax changes. Most fundamentally, we classify the motivation for each tax change. We find that most tax changes have a single, clearly identifiable motivation that falls into one of four broad categories: offsetting a change in government spending; offsetting some factor other than spending likely to affect output in the near future; dealing with an inherited budget deficit; and achieving some long-run goal, such as higher normal growth, increased fairness, or a smaller role for government. We also measure the revenue effects of the tax changes, and identify the nature of the changes.

Tax changes taken because spending was changing or to offset another factor likely to affect output are clearly actions that are correlated with other developments affecting output. As such, they are not legitimate observations to use to estimate the output effects of tax changes. Tax changes taken to deal with an inherited budget deficit or to achieve a long-run goal, in contrast, are changes motivated by past decisions, philosophy, and beliefs about fairness. As a result, they are unlikely to be systematically correlated with other factors affecting output in the short or medium run, and so are legitimate observations to use. These tax changes motivated by factors unrelated to the current or prospective state of the economy form our new series of fiscal shocks. An analysis of the new series, contained in Section III, shows that these exogenous tax actions are fairly evenly distributed across the postwar era. Tax actions motivated by factors likely to affect output, in contrast, were common in early postwar era, but virtually disappeared after 1980. 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 new measure of fiscal shocks, we examine the effects of taxes on real output. In Section IV we present baseline estimates of these effects derived from three progressively more complicated specifications. 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. An examination of the two types of exogenous tax changes separately shows 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 specification. 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 system controlling for monetary policy shocks, the behavior of government spending, and other variables. We find that the estimated effect of exogenous tax changes on output is extremely robust.

In Section VI, we extend our findings in three 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. And third, 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 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. For example, 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.
I. FRAMEWORK

This section outlines the conceptual framework that motivates our analysis. We discuss both the likely problems with existing methods and the logic of our approach.

A. Set-Up

Begin by considering the following minimalist specification of how tax changes affect real output growth:

\[ \Delta Y_t = \alpha + \beta \Delta T_t + \varepsilon_t, \]

where \( Y_t \) is the logarithm of real GDP and \( \Delta T \) is a measure of legislated tax changes. Presumably tax changes do not affect output only in the current quarter. However, for simplicity, we ignore these dynamics for now.

Obviously, many developments besides legislated tax changes affect real growth. Monetary policy shocks, government spending, natural disasters, and expectations about a wide range of future developments are all likely components of \( \varepsilon_t \). Likewise, lagged output growth has been shown to be an important predictor of current growth. Thus, we can think of \( \varepsilon_t \) as being composed of a large number of disparate factors:

\[ \varepsilon_t = \sum_{i=1}^{M} \varepsilon_{i,t}. \]

There is no reason to think that the \( \varepsilon_{i,t} \)'s are necessarily uncorrelated with each other.

Now consider a specification for the determinants of legislated tax changes:

\[ \Delta T_t = \sum_{i=1}^{M} b_i \varepsilon_{i,t} + \sum_{j=1}^{N} \omega_j, \]

where the \( \varepsilon_{i,t} \)'s are the same as before, and the \( \omega_j \)'s are additional influences on tax policy. Equation (3)
captures the crucial fact that some tax changes are taken in response to factors likely to cause output growth to be different from normal (the $\epsilon_t^i$'s). Policymakers may see a recession coming and cut taxes to offset it. Or, they may increase spending to fight a war and increase taxes to pay for it. Equation (3) also captures the notion that some tax changes are taken for reasons unrelated to developments likely to affect output. For example, policymakers may cut taxes because they believe lower marginal rates are good for long-run growth. This idea that some tax changes are exogenous with respect to the other factors affecting output is captured by the assumption that each $\omega_t^j$ is uncorrelated with the $\epsilon_t^i$'s and the $b_t^i$'s.

The specification in equation (3) makes the response of taxes to $\epsilon_t^i$ (the b’s) specific to each episode (hence, the t subscript). This reflects the fact that legislated tax changes are inherently discrete events. In many episodes, policymakers do not respond to the various shocks to output at all, while in others they respond to varying degrees. Furthermore, how much policymakers respond to a given $\epsilon_t^i$ may depend on the other $\epsilon_t^i$'s; for example, policymakers may respond more to an increase in government spending if other factors are also tending to increase output.

B. Implications

Combining the equations for output and taxes yields:

\begin{equation}
\Delta Y_t = \alpha + \beta \left[ \sum_{i=1}^M b_t^i \epsilon_t^i + \sum_{j=1}^N \omega_t^j \right] + \epsilon_t.
\end{equation}

Writing the process for output this way makes it clear why just regressing output growth on all legislated tax changes is likely to lead to a biased estimate of the effect of tax changes: some tax changes are

\footnotesize
\begin{itemize}
  \item In a situation like that, a natural alternative would be to say that the tax increase consisted of two components, a response to the spending increase and a response to the other developments. With such an approach, one could assume that each $b_t^i$ was uncorrelated with the other $\epsilon_t^i$'s. Unfortunately, this approach is not feasible in practice. For example, policymakers sometimes indicate that their views about other factors affecting the economy are influencing their views about the size of the tax change that is appropriate in light of a given change in spending, but they do not provide information that would allow us to determine the size of the effect.
\end{itemize}
correlated with the error term in this regression.

Equation (4) also implies that the bias is likely to be even worse if one uses measures of tax changes that are broader than just legislated changes. A conventional measure of tax changes is the change in cyclically adjusted revenues. But, cyclically adjusted revenues include a substantial number of non-policy movements that may be correlated with other developments affecting output. For example, a boom in the stock market both raises cyclically adjusted tax revenues by increasing capital gains realizations and is likely to reflect other developments that will cause output to rise in the future. As a result, the correlation between this measure of tax changes and the error term in the regression may be even more severe.

This specification also suggests why just regressing output growth on all legislated tax changes including some known shocks to output is unlikely to solve the problem. First, it is impossible to proxy for all the information about future output movements that policymakers may have had. The kind of numerical forecasts of what policymakers thought would happen to output in the absence of tax changes that would be ideal for this exercise are not available even for most recent tax changes. More fundamentally, the fact that the $b_i$'s vary from episode to episode and may be correlated with other $\varepsilon_i$'s means that putting in the obvious known shocks is unlikely to remove the correlation between tax changes and the error term.

These problems with conventional approaches are what lead us to pursue an alternative. The narrative record shows that in the postwar United States, legislated tax changes have been discrete events. Thus, we can use the historical record to identify all legislated tax changes. More importantly, the extensive discussion in the narrative record of why each action was taken reveals that most actions had a single predominant motivation, and that some of those motivations are essentially unrelated to other factors likely to have important effects on output growth and to any other tax responses policymakers may have been making to those factors at around the same time (Romer and Romer, 2008). Thus, we can use the narrative record to in effect directly identify the $\sum \omega_i$ and $\sum b_i \varepsilon_i$ in each quarter of the postwar era.
We can rewrite equation (4) folding the effects of tax changes motivated by other shocks to output into the error term:

\[ \Delta Y_t = \alpha + \beta \sum_{j=1}^{N} \omega^j_t + \nu_t, \]

where \( \nu_t \) is a function of the \( \epsilon^i_t \)s. Provided that we have identified the \( \omega^j_t \)s accurately from the narrative record, this measure of tax changes should be uncorrelated with the error term. Thus, a regression of output growth on \( \sum \omega^j_t \) should yield an unbiased estimate of the impact of a change in tax policy on output. The \( \sum \omega^j_t \) in each quarter is our new measure of fiscal shocks.

Equation (5) not only illustrates the essence of our approach, but also suggests some possible tests of the validity of our new measure of fiscal shocks. We can observe some of the shocks to output directly, or at least have reasonable proxies. For example, we have measures of monetary policy shocks, the change in government spending, and the change in oil prices. We also have the lagged changes in real output, which may be a good proxy for other shocks to output that are serially correlated. If the tax changes we identify as being motivated by factors unlikely to affect output growth (the \( \omega^j_t \)s) were in fact responses to other influences on output growth, it is likely that the \( \omega^j_t \)s would be predictable using the proxies for those influences. Likewise, moving from specifications that do not control for those measures to ones that do would have an important impact on our estimates of the output effects of tax changes. As we describe below, neither of these possibilities occur.

II. NARRATIVE ANALYSIS

The conceptual framework makes clear what we seek to determine from the narrative analysis. We obviously need to first identify legislated tax changes. More fundamentally, we need to identify the motivation for each change. Finally, we need to identify the sizes of the tax changes.
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. 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*) 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 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*.

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2 Presidential speeches and other presidential papers are available online from John Woolley and Gerhard Peters, *The American Presidency Project* (www.presidency.ucsb.edu).
B. Identifying Legislated Tax Changes

The first step in the analysis is to identify all significant legislated tax changes in the period 1945 to 2007. 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. 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. A law that merely extends an existing tax does not count as a change for our purposes. This rule is both necessary for feasibility and sensible. There are many taxes, typically excise taxes, that are renewed every year. These renewals are virtually automatic, so even their news value is minimal.

Identifying legislated tax changes directly is a useful exercise in its own right. As described above, conventional proxies for legislated changes, such as the change in cyclically adjusted revenues, include the effects of many non-policy factors. Our direct identification of legislated tax changes gives a more accurate representation of actual policy actions.

C. Identifying Motivation

The framework described above suggests that we need to use the narrative sources to separate legislated tax changes into two broad categories: those taken in response to other factors likely to affect output growth in the future, which we will call endogenous, and those taken for any other reason, which we will call exogenous. The obvious question is what we look for in the narrative sources to make this classification.

Endogenous Tax Changes. Since exogenous tax changes are, at a fundamental level, identified as the residual, the crucial step is identifying tax changes taken in response to other factors likely to affect output. More precisely, since output is typically growing over time, we need to identify tax actions taken to offset developments that would cause output growth to differ from normal. The obvious kind of tax
change that we are attempting to identify is a tax cut taken because policymakers are forecasting a recession. In this case, some other factor is thought to be reducing output growth, and policymakers are changing taxes to try to return growth to normal. Such a tax change is clearly one of our $b_i^i t^i s$.

A particular type of shock that is likely to affect output growth that policymakers often respond to is a change in government spending. Especially in the 1950s and 1960s, policymakers frequently said they were raising taxes because they were increasing spending. Two classic examples are the large tax increase during the Korean War contained in the Revenue Act of 1951, and the sharp increase in payroll taxes that accompanied the introduction of the Medicare program in 1965. Often, policymakers were quite explicit that the tax increases were intended to offset the expansionary effects of government spending. Even when that link was not made explicitly, it is appropriate to classify these spending-driving tax changes as endogenous. They are always tax actions taken to offset another factor that would tend to move output growth away from normal.

Other than spending changes, policymakers rarely mention particular shocks they are trying to counteract. Rather, they tend to say they are responding to current or projected economic conditions. For this reason, we label the endogenous tax changes that are not related to spending changes as countercyclical actions. A classic example of such a countercyclical tax change is the Tax Reduction Act of 1975. In this case, policymakers were explicit they were cutting taxes because the economy was predicted to fall further and they were attempting to mitigate the decline.

Because policymakers mention a desire to stimulate growth in a wide range of situations, the key to identifying countercyclical tax actions is to discern whether the goal is to merely return growth to normal or to raise growth above its historical norm. Actions taken to return growth to normal are inherently designed to offset other factors affecting output. 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.
In identifying a countercyclical motivation, we take policymakers’ statements at face value. However, it is obviously possible that policymakers say they are seeking to return growth to normal 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.

**Exogenous Tax Changes.** Exogenous tax changes are those not taken to offset other factors pushing output growth away from normal. The quintessential exogenous tax change might be a tax cut taken for philosophical reasons, say because of a belief that lower marginal tax rates will raise output in the long run. Such an action is fundamentally different from the countercyclical actions discussed above because it is taken with the goal of raising normal output, rather than counteracting other shocks acting to reduce growth relative to normal.

We identify exogenous tax changes from the narrative record in two ways. The first, and most straightforward, is by the absence of any discussion of counteracting shocks or of a motivation to return output growth to normal. Second, we look at the actual motivation given and verify that it does not appear to be related to other factors affecting output.

For a tax action to be classified as exogenous, it is not crucial that the economy be growing normally. As long as policymakers are not motivated by the state of the economy, the resulting tax actions should not be systematically correlated with prospective economic conditions. As a result, they are legitimate actions to use to estimate the output effects of tax changes. However, because accidental correlation is always a possibility in small samples, our statistical analysis includes a number of checks. For example, we show that our exogenous tax changes are not Granger caused by past output growth.

One particular motivation that is mentioned frequently and falls into the exogenous 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. An example of such a deficit-driven tax change is the Clinton tax increase contained in the Omnibus Budget Reconciliation Act of 1993. Policymakers were not raising taxes because they felt the economy was overheated and needed to be returned to normal growth. Rather, they felt it was simply prudent fiscal policy and might raise long-run growth.

All exogenous tax changes other than the deficit-driven ones 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. A classic example of such an exogenous tax cut to stimulate long-run growth is the Kennedy-Johnson tax cut contained in the Revenue Act of 1964. 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 somewhat catchall category.

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,

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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 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, in every case the spending decline is small relative to the tax increase. Nevertheless, in the empirical work we test whether deficit-driven tax changes have different effects from other exogenous tax changes.
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 motivation quite well. We classify the changes to taxes in 2001 that were added to the bill because of concern about the recession as endogenous (for countercyclical purposes). The reductions in taxes in 2002 and later years are classified as 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. Measuring the Size and Timing of Tax Changes

Our main measure of the size of tax changes is their impact at the time they were implemented on current tax liabilities at the prevailing level of GDP. Measuring the magnitude of changes in taxes in this way is consistent with a large body of evidence, much of it based on natural tax experiments, that finds consumers respond to current disposable income.\footnote{See, for example, Shapiro and Slemrod (1995), Parker (1999), Souleles (1999), and Johnson, Parker, and Souleles (2006).} In Section V, however, we investigate the effect of measuring tax changes in a way that more closely reflects the news about future taxes contained in passage of a bill.

Policymakers are almost always concerned with the expected effects of tax actions on revenues at a given level of income. In addition, retrospective figures are rarely available. Thus, we again use our narrative sources to derive estimates of the expected revenue effects. 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 on the path of revenues. In these cases, we use the estimated impact of the tax action in its first full calendar year (which, for the many 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.

We assign revenue effects roughly to the quarter when tax liabilities actually changed. Thus, if a tax law changes taxes in steps, we identify a sequence of revenue effects. We follow the convention that if the effective date of an action is before the midpoint of the quarter, we assign it to that quarter. If it is after the midpoint, we assign it to the next quarter.

One issue that arises with the revenue effects is that tax changes often have retroactive components. A tax bill passed in July of some year, for example, may be made retroactive to the previous January. In the baseline version of our revenue estimates, we simply ignore such retroactive features. In an alternative version, we estimate the revenue effects of these provisions. To derive these estimates, we treat any retroactive component as a one-time levy or rebate in the quarter to which we assign the bill.

E. Results of the Narrative Analysis

A companion background paper (Romer and Romer, 2008) provides more information about our analysis of the narrative record and discusses our application of the methodology for each major legislated tax change since 1945. 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 tax change tied to a spending change, a countercyclical change, a change for deficit reduction, and a change to encourage long-run growth.

III. POSTWAR LEGISLATED TAX CHANGES

Our narrative sources identify 50 tax laws (and administrative actions) between 1945 and 2007 that significantly changed tax liabilities. Ignoring retroactive changes, these laws changed taxes in 69 quarters. A number of these quarters had tax changes of multiple types. In all, we identify 84 separate quarterly tax changes. Of these, 54 were exogenous and 30 were endogenous.5

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.6

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5 If one includes the retroactive changes, the laws changed taxes in 83 quarters and there were 98 separate quarterly tax changes. Of these, 62 were exogenous and 36 were endogenous.
6 The data on nominal GDP are from the National Income and Product Accounts, Table 1.1.5 (www.bea.gov, downloaded 2/17/08). Quarterly nominal GDP data are only available after 1947. We therefore normalize the one tax change in 1946 using the annual nominal GDP figure.
A. **Exogenous Tax Changes**

Panel (a) of Figure 1 shows our series of exogenous tax changes since 1945. 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 long-run growth category 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 23 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.

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 19 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.\footnote{We use the unpublished quarterly values, which are generated in a manner consistent with CBO’s annual figures.} 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.

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.

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.

8 The data on the price index for GDP are from the National Income and Product Accounts, Table 1.1.4 (downloaded 2/22/08). 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 logs 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:

$$\Delta (\ln R_t - \ln C_t) = -0.019 + 1.89 \Delta Y_t,$$

\[ R^2 = 0.97, \text{s.e.e.} = 0.0029, \text{D.W.} = 1.52. \]

R is actual real revenues, measured as the ratio of federal current receipts from the National Income and Product Accounts, Table 3.2 (downloaded 2/17/08) to the chain-type price index for GDP. C is cyclically adjusted real revenues. Y is the log of the chain-type quantity index for GDP from the National Income and Product Accounts, Table 1.1.3 (downloaded 2/17/08). 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 $(\Delta R_t - Z_t R_t)/\text{Real GDP}_t$, where $Z_t$ is the fitted value from the regression.
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

IV. THE EFFECT OF TAX CHANGES ON OUTPUT

The next step is to use our new measure of fiscal shocks to examine the relationship between tax changes and economic activity. In this section, we estimate the relationship between exogenous tax changes and real GDP in three progressively more complicated ways. We also compare the results using

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.
our new measure with those using broader measures of tax changes to see if the potential omitted variable bias from using broader measures is indeed present. Finally, we examine the effect of the two categories of exogenous tax changes, those to deal with an inherited budget deficit and those for long-run growth, separately.

A. Specifications

Our series on exogenous tax changes reflects policies adopted for reasons essentially unrelated to other factors likely to influence the behavior of real output. Thus, there is no reason to expect systematic correlation between these tax changes and other determinants of output growth. Our most basic specification is therefore extremely simple: we regress real output growth on a constant and the contemporaneous value and numerous lags of our measure of exogenous tax changes. That is, we estimate:

\[ \Delta Y_t = a + \sum_{i=0}^{M} b_i \Delta T_{t-i} + \epsilon_t, \]

where \( Y \) is the logarithm of real output and \( \Delta T \) is our measure of exogenous tax changes. The analysis in Section I implies that OLS estimation of (6) should, in principle, yield unbiased estimates of the reduced-form impact of changes in the level of taxes on output.

In Section V, we examine the effects of adding various control variables to (6). One control variable, however, is sufficiently important that we consider it from the outset: lagged output growth itself. That is, in addition to (6), we estimate:

\[ \Delta Y_t = a + \sum_{i=0}^{M} b_i \Delta T_{t-i} + \sum_{j=1}^{N} c_j \Delta Y_{t-j} + \epsilon_t. \]

Including lagged output growth obviously helps to control for the normal dynamics of output. Further, because many factors affecting output growth are likely to be serially correlated, including lagged growth is an easy way to control for a multitude of other influences. Finally, and most importantly, controlling
for past growth provides a crucial test of hidden motivation. One worry is that even though policymakers may say they are changing taxes for reasons unrelated to current macroeconomic conditions, perhaps the democratic process makes such changes correlated with economic performance. For example, candidates advocating tax cuts might be more likely to win when the economy is weak. Thus, perhaps seemingly exogenous tax cuts are more common when output is below normal, so what appear as stimulatory effects of tax cuts are in part simply the usual reversion of output to normal. Controlling for the state of the economy by including lags of output growth addresses this possibility.

Our third specification is a natural variation on equation (7): we run a two-variable vector autoregression (VAR) with log output and the exogenous tax changes. This specification allows for effects of both lagged output and past exogenous tax changes on the tax series. In keeping with the regression specifications, which allow tax changes to affect output contemporaneously, we place the tax series first and output second in the VAR.

We measure Y using the chain-type quantity index for GDP. \[\Delta T\] is our measure of exogenous tax changes as a percent of GDP. In our basic specifications, we use the version of the series that does not account for retroactive features of tax actions.

The data are quarterly. Our tax measure is available beginning in 1945Q1 and real GDP is available beginning in 1947Q1. To allow for a substantial number of lags, we begin our estimation in 1950Q1. The final observation is 2007Q4. In estimating equation (6), we set the number of lags of the tax variable equal to twelve. In estimating equation (7), we again include twelve lags of the tax variable, but add only eleven lags of GDP growth, which allows us to keep the same sample period. In the VAR, where the output variable is the level of log GDP, we are able to use twelve lags and keep the baseline sample period.

\[\text{11} \text{ The quantity data are from the National Income and Product Accounts, Table 1.1.3 (downloaded 2/17/08).}\]

\[\text{12} \text{ We do this to make it easier to compare the regression and VAR results. By their nature, retroactive tax changes involve a one-time tax change that then disappears the next quarter. As a result, the series that accounts for retroactive changes exhibits substantial negative serial correlation. Thus, an innovation to the series (which is the obvious experiment to consider in a VAR framework) is quite different from a one-time change in taxes (which is the obvious experiment to consider in a regression framework). This difference aside, the results using the retroactive and nonretroactive versions of the series are extremely similar.}\]
B. Results

Figure 4 summarizes the results of estimating equation (6) by showing the implied effect of a tax increase of one percent of GDP on the path of real GDP relative to normal (in logs), together with the one-standard-error bands. Because of the simple structure of the regression, the implied effect after m quarters is just the sum of the coefficients on the contemporaneous value and the first m lags of the tax variable. The figure shows that the effect is consistently negative. In the quarter of the tax change and the next two quarters, the effect is small and not significant. It is then steadily and rapidly down for the next two years before rebounding slightly in the final two quarters. The maximum effect is a fall in output of 3.08 percent after ten quarters (t = –3.53). 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.

Figure 5 shows the results of the estimation controlling for lagged GDP growth (equation 7). We again show the implied effects of a tax increase of one percent of GDP on the path of log real output. These effects now include not only the direct impact of the tax changes on output, but also the effects working through lagged output. For comparison, the figure also repeats the results from the specification without lagged output.

The figure shows that controlling for lagged output growth has almost no effect on the results. The two sets of estimates track one another very closely at all horizons. The estimated maximum effect falls trivially from –3.08 percent (t = –3.53) to –3.02 percent (t = –2.90). Thus, the most basic test for the possibility that what appear as effects of tax changes actually reflect the normal dynamics of the economy provides no support for this view.

The specification including lagged growth allows for the possibility of further effects of tax

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13 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.
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 –3.02 percent in quarter 10 to –2.41 percent in quarter 12, and then fluctuates between –2.3 and –2.5 percent.

Figure 6 presents the results from the VAR. We show the impulse response functions of the tax series and log GDP to innovations of one percent of GDP to the tax series and of one percent to real GDP, again with the one-standard-error bands. Panel (a) shows that the tax series exhibits few dynamics in response to a tax shock: after an innovation to our series of exogenous tax changes, the subsequent movements in the series are small and irregular. Panel (d) shows that the behavior of output to an output innovation is exactly what one would expect: real GDP is highly serially correlated.

Panel (b) shows that the tax series moves little following movements in output. After a one percent innovation to real GDP, movements in the tax variable fluctuate between –0.02 and 0.02 percentage points and are not consistently of either sign. The effect is not only small, but also highly insignificant. The p-value for the test of the null hypothesis that real GDP does not Granger cause our tax series is 0.78. This is reassuring confirmation that the tax shocks identified from narrative sources are indeed unrelated to output movements.

The key result of the VAR is in panel (c), which shows the behavior of real GDP following an innovation of one percentage point to our series of exogenous tax changes. The results are very similar to those from the simple regressions. The estimated maximum effect is a decline of 2.93 percent after ten quarters (t = –2.80), almost the same as the other estimates. The only notable difference is that the VAR suggests a somewhat stronger tendency of output to return to normal: the effect falls to –1.84 percent after fifteen quarters, and then remains at roughly that level.

Thus, a first look at the data suggests that 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.

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14 The standard errors are computed in the same way as those for the regression that includes lagged GDP growth.
C. Comparison to Broader Measures

The motivation for our paper is the fear that conventional measures of tax changes contain many observations that are correlated with other factors affecting output. As a result, using conventional indicators may yield biased estimates of the effect of tax changes on output. It is therefore useful to compare the results using the new series with those using conventional broader measures to see if the potential bias is indeed present.

We consider two broader measures. The obvious one is the change in real cyclically adjusted tax revenues as a percent of real GDP. As described in Section III, this commonly used measure reflects both all legislated tax changes and tax changes caused by all non-policy factors other than output growth. The second alternative is our new measure of all legislated tax changes (as a percent of nominal GDP). This series includes both our exogenous tax changes and the tax changes we identify as being correlated with other factors affecting output. But, unlike cyclically adjusted revenues, this series does not include any non-policy changes.

Panel (a) of Figure 7 shows the implied impact of a tax change on real GDP from the simple one-equation specification excluding any control variables (equation 6) using the change in cyclically adjusted revenues as the tax variable. Panel (b) shows the implied impact using all legislated tax changes. For comparison, both panels repeat the results using our new measure of exogenous tax changes. The results show that the estimates based on the broader measure are indeed biased toward zero. The implied impact of a change in real cyclically adjusted revenues of one percent of GDP on GDP is initially positive and then falls slowly, reaching a maximum effect of −1.11 percent (t = −2.21) after nine quarters. This estimated maximum effect is less than half as large as that found using our new measure. The implied impact of a generic legislated tax change of one percent of GDP is again initially positive. The maximum effect is a change in GDP of −1.43 percent (t = −2.79). That the effect is somewhat larger than that for the change in cyclically adjusted revenues is consistent with this series excluding the non-policy, non-cyclical factors present in cyclically adjusted revenues.

The results from the simple one-equation specification suggest that conventional measures of tax
changes do contain changes correlated with other factors affecting output. However, there remains the question of whether the resulting possible bias can be easily dealt with by including the most straightforward control—lagged real GDP. Figure 8 addresses this question by estimating the two-variable VAR with the tax change and real GDP described above using each of the two broader tax measures. Panels (a) and (c) show the results using the change in cyclically adjusted revenues; panels (b) and (d) show the results using all legislated tax changes. Both figures also repeat the VAR results for our new measure of exogenous tax changes.

The results in panels (a) and (b) show that the two broader measures of tax changes respond positively and significantly to output changes. This is consistent with there being endogenous changes in both series. Recall that our new series does not show a large or significant response to output. Similarly, while GDP does not Granger cause our new measure of exogenous tax changes, it does Granger cause both broader measures. The p-value for the test that all the lagged GDP coefficients in the tax change regression are zero is 0.02 for the change in cyclically adjusted revenues, and 0.002 for our measure of all legislated tax changes.

The results in panels (c) and (d) show that controlling for lagged GDP by estimating the effect of tax changes in a VAR does not eliminate the omitted variable bias. The negative output effect of a tax increase of one percent of GDP is noticeably slower and smaller using the two broader measures than using the new measure. The maximum impact is a decline of −1.26 percent (t = −2.14) using the change in cyclically adjusted revenues; −1.86 percent (t = −2.74) using all legislated tax changes; and −2.93 percent (t = −2.80) using our new measure. These findings suggest that about two-thirds of the bias that results from using the conventional measure (the change in cyclically adjusted revenues) is due to the fact that some legislated tax changes are correlated with other factors affecting output, and about one-third is due to the fact that this measure includes non-policy, non-cyclical factors that are nevertheless correlated with developments affecting output in the future.
D. The Effects of the Two Types of Exogenous Tax Changes

As described in Section II, our new measure of exogenous tax changes is comprised of tax changes with two categories of motivations: those taken to deal with an inherited budget deficit and those taken to achieve some long-run goal such as higher normal growth or increased fairness. It is natural to consider the impact of each of these types of exogenous tax changes on output separately. Figure 9 shows results from the two-variable VAR specification for each of the two subcategories.

Consider first the results for tax changes taken to achieve long-run goals. As described in Section III, these changes account for the vast majority of our exogenous tax changes. The results in panel (a) show that there is a small positive effect of lagged output on long-run tax changes. This suggests that long-run tax increases are slightly more common following periods of high growth and long-run tax cuts slightly more common following periods of negative growth. However, output movements do not Granger cause long-run tax changes. The p-value for the test that all of the GDP coefficients in the tax regression are zero is 0.45.

The results in panel (c) show that the output effect of a long-run tax increase of one percent of GDP in the two-variable VAR is virtually identical to that for a generic exogenous tax increase of the same size. The maximum cumulative effect is a decline in GDP of 2.99 percent ($t = -2.92$) after ten quarters. As with all exogenous tax changes, the output declines occur rapidly and are only moderately undone by five years after the change.

The results for deficit-driven tax changes are quite different from those for long-run changes. Panel (b) shows that deficit-driven tax changes are slightly negatively related to lagged GDP changes. The p-value for the test that all of the GDP coefficients are zero in the tax regression is 0.05. This suggests that while the narrative sources show no evidence that deficit-driven tax increases occur in response to anticipated output changes, such increases are more common following periods of low growth. This would be consistent with the obvious fact that periods of low growth tend to give rise to persistent budget deficits that are occasionally dealt with through deficit-driven tax increases. However, the fact that the coefficients vary between positive and negative, and are largest at quite long lags,
suggests that the tax changes are unlikely to be highly correlated with other developments affecting output in the future.

The results in panel (d) show that the point estimates for the effect of a deficit-driven tax increase of one percent of GDP on GDP are consistently positive. However, there are too few tax changes of this type for the effects to be estimated precisely. The maximum impact is a rise in GDP of 2.48 percent ($t = 1.03$). While one should be very cautious in reading anything into such imprecise estimates, the results 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.\footnote{15}{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 2.18 percent ($t = 0.85$).}

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

In this section, we test the robustness of the finding that changes in tax have large effects on output. 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.

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 reestimate the VAR with our measure of exogenous tax changes and log GDP,
dropping one at a time the four largest exogenous tax actions: those in the Revenue Act of 1948, the
Relief Reconciliation Act of 2001 together with the Jobs and Growth Tax Relief Reconciliation Act of
2003.16

The results of this exercise suggest that the estimates are quite durable. For two of the actions,
excluding the extreme observations has very little impact on the estimated 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 –2.64 percent (t = –2.32); the maximum effect excluding the 1981 Reagan tax cut is
–3.04 percent (t = –2.48). Recall that for the full sample, the maximum effect is –2.93 percent (t = –2.80).
Excluding the 2001 and 2003 Bush tax cuts (jointly) substantially increases the negative impact of a tax
change. The maximum effect is now –3.39 percent (t = –2.84). 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 significant. When the 1948 action is excluded, the implied maximum impact of a tax increase
of one percent of GDP is –2.20 percent (t = –2.23).

The sensitivity of the results to excluding the 1948 tax cut stems in large part from the fact that
this tax change was followed by the Korean War and the associated wartime boom. Another way to

16 Specifically, we set our series on exogenous tax changes to zero in all quarters from the first to the last quarter
affected by the particular act being excluded. This procedure has the effect of also setting to zero tax changes not
legislated in the excluded act, but occurring in the same time period.
address the possible importance of the extreme observations in the early postwar era is to start the sample period used to estimate the VAR in 1955Q1. Figure 10 shows the cumulative impact of an exogenous tax increase on GDP when the sample period is 1955Q1 to 2007Q4. The estimated maximum output effect of a tax increase of one percent of GDP is now \(-2.63\) percent \((t = -2.68)\).

Shortening the sample period has substantially more effect on the estimates based on broader measures of tax changes. Figure 10 also shows the estimated 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 \(-1.63\) percent \((t = -2.94)\). In addition, the estimated 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: the maximum effect of a tax increase of one percent of GDP on GDP is estimated to be \(-2.36\) percent \((t = -3.55)\), only slightly smaller than the estimate of \(-2.63\) percent using our measure of 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 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. We consider three measures of monetary policy. The first is simply the federal funds rate. This series has the advantages of being a standard indicator of monetary policy and of being available for
almost our entire sample period. As is well known, however, innovations to this series reflect much more than shocks to monetary policy. Our second series is therefore a dummy variable for shifts to anti-inflationary monetary policy that we identified in earlier work (Romer and Romer, 1989, 1994). This series reflects independent shifts in monetary policy and covers the full sample, but is fairly crude. Our third 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 a better calibrated measure of independent shifts in monetary policy, but is available only for the period 1969Q1–1996Q4.

We control for monetary policy by estimating VARs with three variables: our measure of fiscal shocks, log GDP, and one of the monetary policy measures. As before, we include twelve lags. Figure 11 shows, for each of the three measures of monetary policy, the estimated impact of an innovation to our series of exogenous tax changes of one percent of GDP on GDP both with and without controlling for monetary policy. In each case, the results without controlling for monetary policy are estimated over the same period as those with the control.

Panel (a) displays the results when the funds rate is used as the monetary policy indicator. Because the funds rate data do not start until 1950, the sample period for the VAR is 1953Q1 to 2007Q4. Adding the funds rate to the system reduces the estimated impact of tax changes by about twenty percent, but the effect remains large and significant. The maximum impact of an exogenous tax increase of one percent of GDP is a change in GDP of –2.18 percent (t = –2.73) with the monetary control, compared with a change of –2.76 percent (t = –1.82) without the control for the same sample period. The estimates also suggest that tax increases may lead to modest reductions in interest rates. Following an innovation to our tax series of one percent of GDP, the funds rate falls gradually over the next two and a half years, with a maximum effect of –1.31 percentage points, and then returns to its pre-shock level. None of the

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17 The data for the funds rate from 1954Q3 to 2007Q4 are from the Board of Governors of the Federal Reserve System, series H15/H15/RIFSPFF_N.M (www.bog.gov, data for 2/22/08). We extend the series back to the beginning of 1950 using the data in Martens (1958).

18 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 funds rate and the shock series are the averages of the corresponding monthly observations.
estimated effects are statistically significant, however; the t-statistic for the peak effect is –1.40.

Panel (b) considers the dummy variable for anti-inflationary shifts in monetary policy. The effects of including the dummy are very similar to those of adding the funds rate: the estimated impact of tax changes on output declines by about twenty percent, but remains large and significant. The maximum impact of an exogenous tax increase of one percent of GDP is a change in GDP of –2.32 percent (t = –2.41) with the control, compared with a change of –2.93 percent (t = –2.80) without the control.

Panel (c) 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. In this case, controlling for changes in monetary policy raises the estimated output effect of tax changes by almost a third and makes it even more significant. The maximum impact of an exogenous tax increase of one percent of GDP is a change in GDP of –3.61 percent (t = –3.99) with the control, compared with a change of –2.72 percent (t = –1.91) without the control for the same sample period.

Finally, there is somewhat mixed evidence about whether our fiscal shocks respond to monetary policy. For the dummy variable measure, the null hypothesis that monetary policy does not enter the equation for our shock series cannot be rejected: the p-value is 0.79. For both the funds rate and the continuous measure of monetary shocks, the null hypothesis that these two measures do not enter the equation for our measure of exogenous tax changes is rejected: the p-value is 0.02 for the funds rate and 0.003 for the monetary shock series. This finding, however, is due almost entirely to just a few observations around 1981. The Reagan tax cuts, which stand out as some of the largest in the sample periods covered by these two monetary series, happened to come soon after the dramatic shift to tight policy under Paul Volcker, which is when these fund-rate-based monetary series take on their most extreme values. However, the narrative record is very clear that the tax cuts were in no way motivated by the monetary developments. The tax cuts were proposed long before the most dramatic surges in interest rates, and were made without concern about current macroeconomic conditions. Therefore, it is likely that this apparent correlation is spurious. If we drop the Reagan tax cuts from our measure of fiscal shocks, the p-value for the hypothesis that monetary shocks do not affect our series rises to 0.46 for the
funds rate and 0.33 for our continuous measure of monetary shocks.

C. 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 government spending. As before, if we have constructed our measure of exogenous tax changes correctly, controlling for spending changes is not necessary. 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 measure government spending as real federal total gross government expenditures less interest payments. The expenditure data start in 1947Q1. Paralleling our treatment of monetary policy, we add the log of government spending to the VAR with our tax series and log GDP.

Panel (a) of Figure 12 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 2.75 percent, which is trivially smaller than the estimate from our baseline VAR. The effect remains highly statistically significant, with a maximum t-statistic of –2.53. And again, there is no evidence that the tax changes we have identified as exogenous are in fact responses to government spending: the p-value for the test of the null hypothesis that spending does not enter the equation for our tax series is virtually one.

Panel (b) shows that controlling for changes in spending has a moderate impact on the estimated behavior of output following the subset of exogenous tax changes that are for deficit reduction. The maximum impact of a deficit-driven tax increase of one percent of GDP on GDP rises from 2.48 percent (t = 1.03) to 2.99 percent (t = 1.13). While the estimates are very imprecise, the difference is in the direction one would expect if there were occasionally spending cuts at the same time as deficit-driven tax

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19 The government expenditure data are from the National Income and Product Accounts, Table 3.2 (downloaded 2/17/08). We calculate federal total gross expenditures less interest as total expenditures before the subtraction of depreciation, minus interest payments. We divide this by the price index for GDP to convert it to real values.
increases. Controlling for the depressing effects of the spending cuts suggests that the deficit-driven tax increases have an even larger positive impact on output. The changes in the estimates are quite small, however.

One reason that broader measures of tax changes yield biased estimates of the effects of tax changes on output is that they include tax changes motivated by spending changes. The analysis in Section I implies that simply including spending as a control variable in specifications using the broader measures is unlikely to eliminate this bias. Nonetheless, it is natural to ask whether it reduces the bias. To test for this possibility, we reestimate the VAR using both the change in cyclically adjusted revenues and all legislated tax changes in place of our tax series. We find that controlling for spending changes is a largely inconsequential fix. For both broader measures of tax changes, 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 changes are quite small, however, and the estimates remain far smaller than those estimated using the exogenous tax changes.\footnote{Including government expenditures in the VAR for the change in cyclically adjusted revenues raises the maximum impact of a tax cut of one percent of GDP on GDP from \(-1.26\) (\(t = –2.14\)) to \(-1.27\) (\(t = –2.73\)); including expenditures in the VAR for all legislated tax changes raises the maximum impact from \(-1.86\) (\(t = –2.74\)) to \(-2.01\) (\(t = –2.73\)).}

D. \textbf{Controlling for Other Variables}

We have also investigated the effect of adding the relative price of crude oil to the VAR.\footnote{The crude oil price data are from the Bureau of Labor Statistics, series WPU0561 (www.bls.gov, downloaded 3/7/08). We divide this series by the price index for GDP.} This change also has little impact on the results: the estimated maximum effect of a tax increase of one percent of GDP on GDP is now \(-2.54\) percent (\(t = –2.37\)) after ten quarters.

Finally, one possible concern is that political variables affect tax policy and also independently affect macroeconomic developments. For example, perhaps Republican administrations consistently cut taxes for exogenous reasons and also lessen regulation. We therefore consider two sets of political variables. The first is simply a dummy variable for whether the president is a Democrat. The second is the contemporaneous value and twelve lags of dummy variables for the beginnings of Democratic and
Republican administrations. We find that in both cases, controlling for the political variables has very little impact on the estimated effects of tax changes.

The bottom line of the robustness analysis is that our baseline results are very durable. 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 VAR the maximum effect of a tax increase of one percent of GDP is a fall in GDP of 2.93 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 statistically significant.

VI. EXTENSIONS

We extend our results in three 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 actions have changed over time. Third, 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 reduce consumption in response to moderate anticipated increases in taxes. And, any adverse supply-side effects of tax increases 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 expectational 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. When the individual actions for a given act have multiple motivations, we calculate a separate present value for each motivation.

We then estimate an expanded version of equation (7), the single-equation specification of the relationship between tax changes and output that includes lagged real GDP growth. In particular, we

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22 The data are from the Board of Governors of the Federal Reserve System, series H15/H15/RIFLGFCY03_N.M (data for 2/15/08). 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.

23 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 Expiration of Excess Profits Tax and of Temporary Income Tax Increases); 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.
regress real GDP growth on eleven own lags, the contemporaneous value and twelve lags of our standard liabilities-based measure of exogenous tax changes (which dates tax changes in the quarter they actually took effect), and the contemporaneous value and twelve lags of the alternative present-value measure of exogenous tax changes (which dates tax changes in the quarter of passage). We then consider two experiments: the effect of a change in the liabilities series of one percent of GDP, holding fixed the present-value series, and a change in the present-value series of one percent of GDP, holding fixed actual liabilities. 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.24

The results are shown in Figure 13. 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; the dashed line shows the impact of a present-value tax increase of one percent of GDP dated in the quarter of passage.

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 the same specification not including the present-value measure (which is given in Figure 5). The maximum impact is −3.50 percent (t = −2.22). 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, negative, and marginally statistically significant. This could suggest that there is some immediate impact of tax news on GDP. Two quarters after the change, the effects turn consistently positive. They are not significantly different from zero, however. The maximum impact is 1.10 percent (t = 1.17). That the

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24 We do not use a VAR specification in this case because the resulting experiments are much more difficult to interpret. In a three-variable VAR including the two measures of tax changes and real GDP, the impulse response function of output to an innovation to the present-value tax series includes both the effects through news and the effects through the usual behavior of actual liabilities following a movement in the present-value series. As a result, it does not provide information on the separate effects of the two series. We do, however, run a two-variable VAR using the present-value tax measure and real GDP to see how the baseline VAR results change. The effect of an exogenous tax increase of one percent of GDP on GDP using the present-value measure is noticeably smaller than our baseline results, but still negative and marginally significant at some horizons. The maximum impact is a change in GDP of −1.26 percent (t = −1.63).
impact after the initial quarter is not significant at conventional levels suggests that the news effects of tax changes may be of secondary importance. That the point estimates are predominantly 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.

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. In contrast, there is only slight evidence of expectational effects.

**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 14 shows the estimated output effects of a shock of one percent of GDP to our measure of exogenous tax changes in our baseline VAR estimated over the periods 1950Q1-1980Q4 and 1981Q1-2007Q4. The results provide modest evidence that the effects of tax changes have become weaker. For the early period, the maximum estimated effect of the tax increase is an output decline of 4.29 percent after seven quarters; for the later period, it is a fall of 3.08 percent after eight quarters. The estimated
effects are considerably more rapid for the earlier period. For example, the effect after a year is –3.69 percent for the early sample and –0.81 percent for the later sample. The estimates are not highly precise, however, particularly in the early period. The t-statistic for the estimated maximum effect in the early sample is –2.19; the two-standard-error confidence interval easily includes the point estimates for the later period.\[\text{ }\]

The other important feature of these results is that the impact of tax changes remains large and significant for the later period. For that sample, the estimated output effect exceeds –2 percent for quarters 6 through 17 after the tax change, and the t-statistic for the maximum effect is –3.61.\[\text{ }\]

C. 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 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.\[\text{ }\]

The specification we use mirrors the earlier ones. We estimate three-variable VARs with our measure of exogenous tax changes, log real GDP, and the log of each major component of real GDP.\[\text{ }\]

As before, we include twelve lags and focus on the full postwar sample (1950Q1–2007Q4). Other specifications, such as excluding the years of the Korean War or running only two-variable VARs, yield similar results.

The results are presented in Figure 15. Panel (a) shows the estimated responses of consumption and investment to an exogenous tax increase. For comparison, it also repeats the estimated response of

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25 The fact that the estimates are faster and larger for the earlier period does not depend on the inclusion of the Korean War period in that sample. When we begin the early sample in 1955Q1, the estimated effect after a year increases to –4.09 percent, and the maximum impact is –4.73 percent after seven quarters.

26 The result that the estimated maximum effect is actually slightly larger in both subsamples than in the full sample does not carry over to simpler specifications, such as the OLS specification in equation (6). Thus, it presumably reflects changes in the own-dynamics of output between the two periods.

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

28 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 2/17/08).
GDP. The key results are that both components decline, and that the fall in investment is much larger than the fall in consumption. In response to a tax increase of one percent of GDP, the maximum fall in personal consumption expenditures is 2.55 percent \( (t = -3.06) \), just slightly less than the maximum fall in GDP. The maximum fall in gross private domestic investment is 11.19 percent \( (t = -3.35) \).

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. And indeed, recall that we find in Section V.B that the federal funds rate declines only moderately following tax increases. The strong response of investment to tax changes is consistent with research showing that investment depends strongly on cash flow and overall economic conditions (for example, Abel and Blanchard, 1986; Fazzari, Hubbard, and Petersen, 1988; and Oliner, Rudebusch, and Sichel, 1995).

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 behavior of investment.

Panel (b) of Figure 15 shows the estimated responses of the three categories of consumption. Personal consumption expenditures on both nondurables and services fall relatively little. For nondurables, the maximum impact is –2.27 percent \( (t = -3.12) \); for services, the effect peaks first at –1.30

29 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.

30 Our series on exogenous tax changes is not well suited to measuring the impact of tax changes on long-term 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 long-term interest rates presumably respond to news, they are likely to change before liabilities actually change (and before the legislation changing taxes is actually enacted).

31 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 in fact strengthens the estimated investment response slightly.
percent \((t = -2.87)\) after eight quarters and then at \(-1.42\) percent \((t = -2.48)\) after twelve. Expenditures on durables fall substantially more: the maximum impact is \(-8.27\) percent \((t = -2.86)\). 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.\(^3\) Fixed investment of both types falls less than total investment, suggesting that inventory investment falls particularly strongly. But, fixed investment appears to respond sharply as well. The maximum effect is \(-8.50\) percent \((t = -2.82)\) for nonresidential fixed investment and \(-5.84\) percent \((t = -1.47)\) for residential fixed investment.

Finally, 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, although the effect is not precisely estimated. The maximum impact is 4.98 percent \((t = 2.08)\). Imports fall significantly. The maximum impact is \(-10.07\) percent \((t = -3.62)\). The implied rise in net exports is consistent with the tax increase lowering interest rates and hence reducing capital inflows. But the fact that the effect is much stronger for imports suggests that the fall in income may be more important than the interest rate/exchange rate linkage.

\(^3\) The BEA does not provide a quantity index for inventory investment before 1990.

VII. CONCLUSIONS

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 response to 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 almost three percent. Our many robustness checks for the most part point to a slightly smaller decline, but one that is still typically over 2.5 percent. 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 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 responds to tax changes, or how output behaves following 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. By using information about these features of our exogenous tax changes, one could investigate 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 Spending-Driven Tax Change

Revenue Act of 1951

Signed: 10/20/51
Change in Liabilities (excluding retroactive changes):
1951Q4  +$5.4 billion (Endogenous; Spending-driven)
Change in Liabilities (including retroactive changes):
1951Q4  +$10.0 billion (Endogenous; Spending-driven)
1952Q1  −$4.6 billion (Endogenous; Spending-driven)
Present Value:
1951Q4  +$5.42 billion (Endogenous; Spending-driven)

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” (82d 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 2
Narrative Analysis of a Countercyclical Tax Change

**Tax Reduction Act of 1975**

Signed: 3/29/75
Change in Liabilities (excluding retroactive changes):
- 1975Q2: $-45.3 billion (Endogenous; Countercyclical)
- 1975Q3: $+32.5 billion (Endogenous; Countercyclical)
Change in Liabilities (including retroactive changes):
- 1975Q2: $-58.1 billion (Endogenous; Countercyclical)
- 1975Q3: $+45.3 billion (Endogenous; Countercyclical)
Present Value:
- 1975Q3: $-13.32 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. 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.

\[ \text{The table shows total net revenue effects from tax changes of } \pm 20.9 \text{ billion. Subtracting off the } \pm 8.1 \text{ billion due to the rebate yields } \pm 12.8 \text{ billion.} \]
Omnibus Budget Reconciliation Act of 1993

Signed: 8/10/93

<table>
<thead>
<tr>
<th>Quarter</th>
<th>Change in Liabilities (excluding retroactive changes):</th>
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<tbody>
<tr>
<td>1993Q3</td>
<td>+$22.8 billion (Exogenous; Deficit-driven)</td>
</tr>
<tr>
<td>1993Q4</td>
<td>+$5.3 billion (Exogenous; Deficit-driven)</td>
</tr>
<tr>
<td>1994Q1</td>
<td>+$13.4 billion (Exogenous; Deficit-driven)</td>
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<th>Quarter</th>
<th>Change in Liabilities (including retroactive changes):</th>
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<tbody>
<tr>
<td>1993Q3</td>
<td>+$68.4 billion (Exogenous; Deficit-driven)</td>
</tr>
<tr>
<td>1993Q4</td>
<td>–$40.3 billion (Exogenous; Deficit-driven)</td>
</tr>
<tr>
<td>1994Q1</td>
<td>+$13.4 billion (Exogenous; Deficit-driven)</td>
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Present Value:
<table>
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<th>Quarter</th>
<th>Present Value:</th>
</tr>
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<tbody>
<tr>
<td>1993Q3</td>
<td>+$41.64 billion (Exogenous; Deficit-driven)</td>
</tr>
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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 Conferencees, 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.
Revenue Act of 1964

Signed: 2/26/64
Change in Liabilities (excluding retroactive changes):

- 1964Q2: $-8.4 billion (Exogenous; Long-run)
- 1965Q1: $-4.5 billion (Exogenous; Long-run)

Change in Liabilities (including retroactive changes):

- 1964Q2: $-16.8 billion (Exogenous; Long-run)
- 1964Q3: $+8.4 billion (Exogenous; Long-run)
- 1965Q1: $-4.5 billion (Exogenous; Long-run)

Present Value:

- 1964Q1: $-12.72 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, 2nd 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.
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
(Single Equation, No Controls)
Figure 5
Estimated Impact of an Exogenous Tax Increase of 1% of GDP on GDP
(Single Equation, Controlling for Lagged GDP Growth)
Figure 6
Results of a Two-Variable VAR for Exogenous Tax Changes and Real GDP

a. Response of Tax to Tax

b. Response of Tax to GDP

c. Response of GDP to Tax
d. Response of GDP to GDP
Figure 7
Estimated Impact of a Tax Increase of 1% of GDP on GDP Using Broader Measures
(Single Equation, No Controls)

a. Using the Change in Cyclically Adjusted Revenues

Using the Change in Cyclically Adjusted Revenues

Using Exogenous Tax Changes

b. Using All Legislated Tax Changes

Using All Legislated Tax Changes

Using Exogenous Tax Changes
Figure 8
Results of a Two-Variable VAR for Broader Measures of Tax Changes and Real GDP

a. Response of Tax to GDP

b. Response of Tax to GDP

c. Response of GDP to Tax

d. Response of GDP to Tax
Figure 9
Results of a Two-Variable VAR for the Two Types of Exogenous Tax Changes and Real GDP

a. Response of Tax to GDP

b. Response of Tax to GDP

c. Response of GDP to Tax

d. Response of GDP to Tax
Figure 10
Estimated Impact of a Tax Increase of 1% of GDP on GDP, Excluding Korea
(Two-Variable VAR)

Using the Change in Cyclically Adjusted Revenues

Using Exogenous Tax Changes
Figure 11
Estimated Impact of a Tax Increase of 1% of GDP on GDP, Controlling for Monetary Policy Variables
(Three-Variable VAR)

a. Using the Federal Funds Rate

b. Using Dummy Variable for Anti-Inflationary Policy

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Estimated Impact of a Tax Increase of 1% of GDP on GDP, Controlling for Government Expenditures (Three-Variable VAR)

a. Using Exogenous Tax Changes

b. Using Deficit-Driven Tax Changes
Figure 13
Estimated Impact of a Tax Increase of 1% of GDP on GDP,
Including Tax Changes Dated Both at Time of Change and Time of Passage
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Estimated Impact of an Exogenous Tax Increase of 1% of GDP on GDP, Splitting the Sample in 1980Q4 (Two-Variable VAR)
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Estimated Impact of an Exogenous Tax Increase of 1% of GDP on the Components of GDP
(Three-Variable VAR)

a. GDP, Consumption, and Investment

b. Consumption Expenditures on Durables, Nondurables, and Services

c. Investment, Nonresidential Fixed Investment, and Residential Fixed Investment

d. Exports and Imports