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The Macroeconomic Effects of Tax Changes: Estimates Based on a New Measure of Fiscal Shocks

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This paper investigates the impact of tax changes on economic activity. We use the narrative record, such as presidential speeches and Congressional reports, to identify the size, timing, and principal motivation for all major postwar tax policy actions. This analysis allows us to separate legislated changes into those taken for reasons related to prospective economic conditions and those taken for more exogenous reasons. The behavior of output following these more exogenous changes indicates 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. (*JEL* E32, E62, H20, N12)

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 nonpolicy 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 nonpolicy developments. We then use the information on motivation to separate the legislated tax changes into those that are likely to be contaminated by other developments affecting output, and those that can legitimately be used to measure the macroeconomic effects of tax changes. 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 systematically

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correlated with other developments affecting output. For want of a better term, we call these tax changes "exogenous." The framework 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 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 the early postwar era but virtually disappeared after 1975. 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 tax changes on real output. In Section IV we present baseline estimates of these effects derived from three progressively more complicated specifications. Our estimates suggest that a tax increase of 1 percent of GDP reduces output over the next three years by nearly three percent. The effect is highly statistically significant. 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. We find that the results are robust to the exclusion of extreme observations and to the inclusion of a wide variety of control variables.

In Section VI, we extend our findings in three ways. First, we ask whether tax changes have important effects through expectations. We find that output responds more closely to the implementation of a tax change than to the news of the change. Second, we test whether the effects of tax actions have changed over time. Our estimates suggest that the response of output is substantially smaller after 1980 than before. 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 Leonall C. Andersen and Jerry L. Jordan (1968), simply regress output growth on measures of high-employment spending and receipts. Two more sophisticated recent studies are Olivier Blanchard and Roberto 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 noncyclical, nonpolicy influences on revenues and other determinants of output growth.

A related literature looks at the possibility of "expansionary fiscal contractions" (for example, Francesco Giavazzi and Marco Pagano 1990; Alberto 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 Roger C. Kormendi (1983), Emanuela Cardia (1997), and a host of others surveyed in William G. Gale and Peter R. 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 approach is the work of Valerie A. Ramey and Matthew D. Shapiro (1998) and Ramey (2008) examining the effect of changes in government spending. Using news reports in *Business Week* and other historical accounts, these authors identify military buildups and other changes in government purchases that occurred for reasons unrelated to the state of the economy or prospective macroeconomic developments. Because the changes were the result of outside forces, they can be used to estimate the impact of government purchases on the economy. These studies 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 of estimating the macroeconomic effects of tax changes, and the logic of our approach.

A. *Set-Up*

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

$$\Delta Y_t = \alpha + \beta \Delta T_t + \varepsilon_t, \tag{1}$$

where Y_t is the logarithm of real GDP and ΔT 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. Government spending, monetary policy shocks, natural disasters, and expectations about a wide range of future developments are all likely components of ε_t. Thus, we can think of ε_t as being composed of a large number of disparate factors:

$$\varepsilon_t = \sum_{i=1}^{K} \varepsilon_t^i. \tag{2}$$

There is no reason to think that the various ε_t^i's are uncorrelated with each other.

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

$$\Delta T_t = \sum_{i=1}^{K} b_t^i \varepsilon_t^i + \sum_{j=1}^{L} \omega_t^j, \tag{3}$$

where the ε_t^i's are the same as before, and the ω_t^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 ε_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 in the near term. For example, policymakers may cut taxes because they believe lower marginal rates are good for long-run growth or because they hope lower revenues will eventually shrink the size of government. This idea that some tax changes are exogenous with respect to the other factors affecting output is captured by the assumption that each ω_t^j is uncorrelated with the ε_t^i's and the b_t^i's .

The specification in equation (3) makes the response of taxes to ε_t^i (the b^i'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 ε_t^i may depend on the other ε_t^i's ; for example, policymakers may respond more to an increase in government spending if other factors are also tending to increase output.[1]

B. *Implications*

Combining the equations for output and taxes yields:

$$\Delta Y_t = \alpha + \beta\left[\sum_{i=1}^{K} b_t^i \varepsilon_t^i + \sum_{j=1}^{L} \omega_t^j\right] + \varepsilon_t. \tag{4}$$

[1] 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^i was uncorrelated with the other ε^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.

Writing the process for output this way shows 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 correlated with the error term in this regression.

Equation (4) also implies that the bias is likely to be even larger 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 many 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 raise output in the future. As a result, the correlation between this measure of tax changes and the error term in the regression may be even stronger.

This specification also suggests why just regressing output growth on all legislated tax changes and 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 generally not available even for recent tax changes. More fundamentally, the fact that the b^i's vary from episode to episode and may be correlated with other ε_t^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 significant 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). Thus, we can use the narrative record to, in effect, directly identify the $\sum \omega_t^j$ and $\sum b_t^i \varepsilon_t^i$ 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:

$$\Delta Y_t = \alpha + \beta \sum_{j=1}^{L} \omega_t^j + \upsilon_t, \tag{5}$$

where $\upsilon_t = \sum_{i=1}^{K} (1 + \beta b_t^i) \varepsilon_t^i$. Provided that we have identified the ω_t^j'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 $\sum \omega_t^j$ should yield an unbiased estimate of the impact of a change in tax policy on output. The $\sum \omega_t^j$ 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 measure of fiscal shocks. We can observe some of the shocks to output directly, or at least have reasonable proxies. For example, we have the change in government spending, measures of monetary policy shocks, 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 ω_t^j's) were in fact responses to other influences on output growth, it is likely that the ω_t^j'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 identify legislated tax changes. More fundamentally, we need to identify the motivation for each change. Finally, we need to determine the size and timing of the changes.

A. *Sources*

The sources for the narrative analysis are primary documents produced by policymakers at the time. For the executive branch, our key sources are the *Economic Report of the President* and presidential speeches and statements.[2] The *Economic Report* is released each January and typically discusses the motivation, revenue effects, and nature of tax changes in the previous calendar year. The president often discusses tax actions in the State of the Union Address, the Annual Budget Message, and addresses proposing or upon signing 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, and that often provide information about timing and revenue effects, are the *Annual Report of the Secretary of the Treasury on the State of the Finances* and the *Budget of the United States Government*.

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 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 information about timing and revenue effects. The reports of the Congressional Budget Office, which was created in 1974, also often give revenue estimates.

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

B. *Identifying Legislated Tax Changes*

The first step in the analysis is to identify all significant legislated tax changes in the period 1945–2007. To do this, we simply look for tax changes that receive more than incidental mention in our sources. We include any measure, including executive actions, that receives serious discussion. Since this approach leads us to include even changes with very small revenue effects, we feel this is a fundamentally sensible listing of the important tax policy changes over the postwar era.

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 and sensible. There are many taxes, typically excise taxes, that are renewed virtually every year. These renewals are almost automatic, so even their news value is minimal.

Identifying legislated tax changes is a useful exercise in its own right. As described above, conventional proxies for legislated changes, such as the change in cyclically adjusted revenues,

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

include the effects of many nonpolicy factors. Our identification of legislated tax changes gives a more accurate representation of actual policy actions.

C. *Identifying Motivation*

Our framework implies that we need to separate legislated tax changes into two broad categories: those taken in response to other factors likely to affect output growth in the near future, which we will call endogenous, and those taken for any other reason, which we will call exogenous.[3]

Endogenous Tax Changes.—Since output is typically growing over time, endogenous tax actions are ones taken to offset developments that would cause output growth to differ from normal. The quintessential endogenous action is a tax cut made 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 $b_t^i \varepsilon_t^i$'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. A clear example is the sharp increase in payroll taxes that accompanied the introduction of the Medicare program in 1965. Often, policymakers were 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. A classic example of such a change is the Tax Reduction Act of 1975: policymakers were explicit that they were cutting taxes because the economy was predicted to fall further, and they were attempting to mitigate the decline.

Because policymakers often mention a desire to stimulate growth, the key to identifying countercyclical tax actions is to discern whether the goal is merely to return growth to normal or to raise it 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 greatly. Therefore, policymakers' predictions of what would happen to unemployment provide a way of judging the intent of a tax change.

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. This strategy may make our estimates of the effects of tax changes less precise, but ensures that the bias in the estimates is as small as possible.

[3] Obviously, we do not use the term "exogenous" either in the strict econometric sense or to mean that the changes have no economic causes. An equally appropriate terminology would be "valid" and "invalid," rather than "exogenous" and "endogenous."

Exogenous Tax Changes.—Exogenous tax changes are those not taken to offset factors pushing growth away from normal. The quintessential exogenous change might be a tax cut motivated by 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 the goal is to raise normal growth, not to offset 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 desire to return growth to normal. Second, we look at the actual reasons given for the action and verify that they do not appear related to other factors affecting output in the near future.

For a tax action to be exogenous, it is not crucial that the economy be growing normally. If policymakers are not motivated by the state of the economy, the resulting 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 output growth.

One particular motivation that is common and that falls into the exogenous category are tax increases to deal with an inherited budget deficit. An inherited deficit reflects past economic conditions and budgetary decisions, not current conditions or spending changes. If policymakers 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 is exogenous. An example of such a deficit-driven tax change is the Clinton tax increase in the Omnibus Budget Reconciliation Act of 1993. Policymakers raised taxes not because they felt the economy was overheated and needed to be restrained, but because they felt it was prudent fiscal policy and might increase long-run growth.[4]

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 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 tax changes for long-run growth.

Applying the Criteria.—Armed with this classification scheme, it is usually straightforward to identify 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 an action. This is the case, for example, with the Economic Growth and Tax Relief Reconciliation Act of 2001. A large tax cut

[4] One difficult case that occurs periodically in the 1980s and 1990s is a deficit reduction package that includes a tax increase and a spending decrease. Since such packages are not motivated by a desire to return growth to normal, they are exogenous in our classification scheme. However, they 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, their inclusion in the empirical analysis may lead to an overestimate of the effects of tax changes. Fortunately, the spending declines are generally small relative to the tax increases. Nevertheless, in the empirical work we test whether deficit-driven tax changes have different effects from other exogenous tax changes.

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, it 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 changed to include an immediate rebate to jumpstart the economy, rather than its being phased in beginning in 2002 as called for in the original proposal. In this and the few other cases like it, we find that we can apportion motivation quite well. We classify the reductions in taxes in 2001 that were added to the bill because of concern about the recession as endogenous (for countercyclical purposes). The changes in 2002 and later years are classified as exogenous (for long-run growth). Fortunately, cases such as this one, where the stated motives change substantially 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. Dating the changes at the times when liabilities actually changed is consistent with a large body of evidence, much of it based on natural tax experiments, that finds that consumers respond to current disposable income.[5] In Section VI, however, we investigate the effect of measuring tax changes in a way that more closely reflects the news about future taxes when the bills were passed.

Policymakers are almost always concerned with the likely 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 expected revenue effects. Whenever possible, we derive a consensus estimate from multiple sources. We express all revenue effects at an annual rate. If a law changes tax liabilities 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.[6]

E. *Results of the Narrative Analysis*

A companion background paper (Romer and Romer 2009) provides more information about our analysis of the narrative record. This paper includes a detailed summary of our findings about the motivation, revenue effects, and other characteristics of each legislated tax change over the period 1945–2007. In every case, 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. The background paper is included with the supplemental materials to this paper at http://www.aeaweb.org//articles.php?doi=10.1257/aer.100.3.763.

To give a sense of how we apply our procedures, Exhibits 1 and 2 reproduce two of our narrative summaries. Exhibit 1 illustrates an endogenous, countercyclical action, and Exhibit 2 illustrates an exogenous change to encourage long-run growth.

[5] See, for example, Shapiro and Joel Slemrod (1995), Jonathan A. Parker (1999), Nicholas S. Souleles (1999), and David Johnson, Parker, and Souleles (2006).

[6] 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.

Exhibit 1—Narrative Analysis of a Countercyclical Tax Change

Tax Reduction Act of 1975

Signed:	3/29/75	
Change in Liabilities (excluding retroactive changes):		
1975:II	−$45.3 billion	(Endogenous; Countercyclical)
1975:III	+$32.5 billion	(Endogenous; Countercyclical)
Change in Liabilities (including retroactive changes):		
1975:II	−$58.1 billion	(Endogenous; Countercyclical)
1975:III	+$45.3 billion	(Endogenous; Countercyclical)
Present Value:		
1975:III	−$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 1975:II and an increase of $27 billion in 1975:III. 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 1975:II. At an annual rate, this was an endogenous tax cut of $32.5 billion. The Conference report showed additional net tax cuts in 1975:II of $12.8 billion at an annual rate. (The table shows total net revenue effects from tax changes of −$20.9 billion. Subtracting off the −$8.1 billion due to the rebate yields −$12.8 billion.) Because these cuts were retroactive to January 1, 1975, this implies an additional tax cut in 1975:II of $12.8 billion. Therefore, there was a total endogenous tax change in 1975:II of −$32.5 billion minus $12.8 billion minus $12.8 billion, or −$58.1 billion. Then, in 1975:III 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 1975:II and + $32.5 billion in 1975:III, 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 1976:I (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.

Exhibit 2—Narrative Analysis of a Long-Run Tax Change

Revenue Act of 1964

Signed:	2/26/64	
Change in Liabilities (excluding retroactive changes):		
1964:II	−$8.4 billion	(Exogenous; Long-run)
1965:I	−$4.5 billion	(Exogenous; Long-run)
Change in Liabilities (including retroactive changes):		
1964:II	−$16.8 billion	(Exogenous; Long-run)
1964:III	+$8.4 billion	(Exogenous; Long-run)
1965:I	−$4.5 billion	(Exogenous; Long-run)
Present Value:		
1964:I	−$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 1964 *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. 3). 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 ½ million new jobs in our economy, the unemployment rate now stands at 5 ½ percent. 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 nine 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 six 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 nine 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 1964:II. 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 1964:II. The retroactive part then disappeared in 1964:III. 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 1965:I. Note, if one chose to ignore the retroactive nature of the tax cut, the revenue estimates would be: −$8.4 billion in 1964:II and −$4.5 billion in 1965:I.

The Revenue Act of 1964 lowered marginal tax rates from the previous range of 20–91 percent to 14-70 percent. 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.

III. New Measure of Fiscal Shocks

Our narrative sources identify 54 quarterly tax changes that appear to be relatively exogenous with respect to output. These exogenous tax changes are our new measure of fiscal shocks. As described above, these 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. It is also important to compare the new series with broader measures of tax changes.

A. *Properties of Exogenous Tax Changes*

Our estimates of the revenue effects of tax changes are in nominal terms. Before one can sensibly discuss trends over time or include the series in an empirical framework, the nominal changes need to be put on a consistent basis. To do this, we express each revenue effect as a percent of nominal GDP in the quarter the change occurred.[7] Panel A of Figure 1 shows the resulting values of our measure of exogenous tax changes.

The figure shows one of the crucial features of our new series: many of the observations are zero. This reflects the fact that our series includes only legislated tax changes, and such legislated changes occur at discrete times. The figure also shows that exogenous tax changes are distributed throughout the postwar era. They were, however, particularly common in the 1960s and the 1980s.

Among the exogenous tax changes there are slightly more tax cuts than tax increases. The mean of the new series is –0.03 percent of GDP. While most of these changes were small, a substantial number of quarterly changes have been ½ percent of GDP or more. The largest quarterly exogenous tax action was a cut in taxes of nearly 2 percent of GDP in 1948:II. The standard deviation of the new series is 0.24 percentage points. The new series shows essentially no serial correlation; the p-value for the Q-statistic that all of the autocorrelations are zero is 0.996.[8]

Panel B of Figure 1 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 for long-run growth are tax cuts. However, because this group includes tax reforms for efficiency and fairness, it contains some tax increases. For example, the Tax Reform Act of 1976 closed tax loopholes that were thought to be encouraging efforts at tax evasion. The most significant tax cuts to stimulate long-run growth are well known: the 1948 tax cut passed over Truman's veto; the 1964 Kennedy-Johnson tax cut; the 1981 Reagan tax cut; and large parts of the 2001 and 2003 Bush tax cuts.

All of the deficit-driven changes were tax increases. The figure shows that while deficit-driven tax increases have occurred throughout the postwar era, they were most prevalent in the 1980s. The majority of the deficit-driven actions 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 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 1990 and 1993.

B. *Comparison with All Legislated Tax Increases*

Panel A of Figure 2 shows our new series of exogenous tax changes together with our estimates of all legislated tax changes (that is, the sum of our series on exogenous and endogenous tax changes). The most striking feature of this comparison is that the two series are nearly identical after 1975: there were only a handful of endogenous tax changes over the period 1976–2007. In the first three decades of the postwar period, in contrast, the two series are wildly different. This finding suggests that the importance of controlling for the motivation for legislated tax changes depends strongly on the sample period one is interested in.

[7] 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 available only after 1947. We therefore normalize the one tax change in 1946 using the annual nominal GDP figure.

[8] Summary statistics refer to the period 1947–2007, since this is the period for which we use the series in our empirical analysis and for which the change in cyclically adjusted revenues is available. The statistics for the full period 1945–2007 are virtually identical.

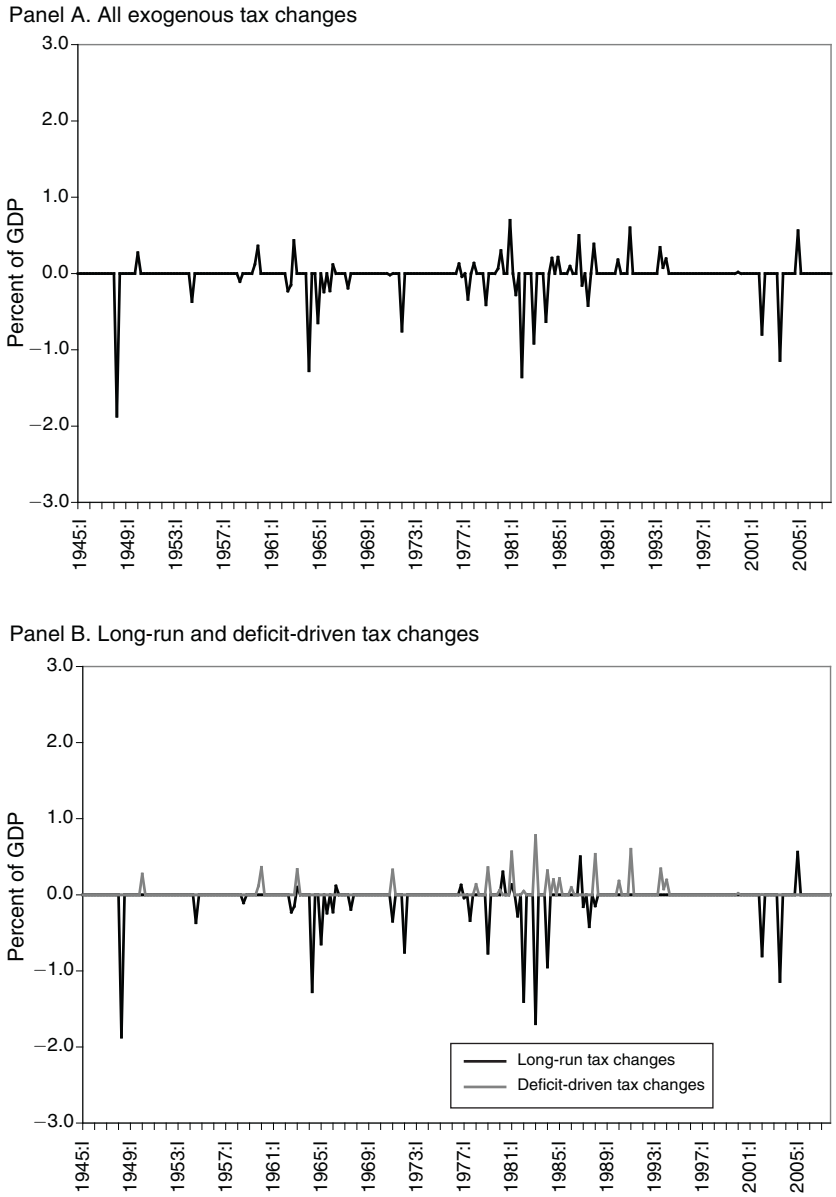

FIGURE 1. NEW MEASURE OF FISCAL SHOCKS

The mean of all legislated tax changes is essentially zero (–0.006 percent of GDP). The standard deviation is 0.38 percentage points, or roughly 50 percent larger than for our series of exogenous tax changes.

The legislated tax changes shown in panel A of the figure that do not correspond to the exogenous changes are the ones we classify as endogenous. To give a better sense of those actions, panel B of the figure shows the two subcategories of endogenous actions, countercyclical and spending driven, separately.

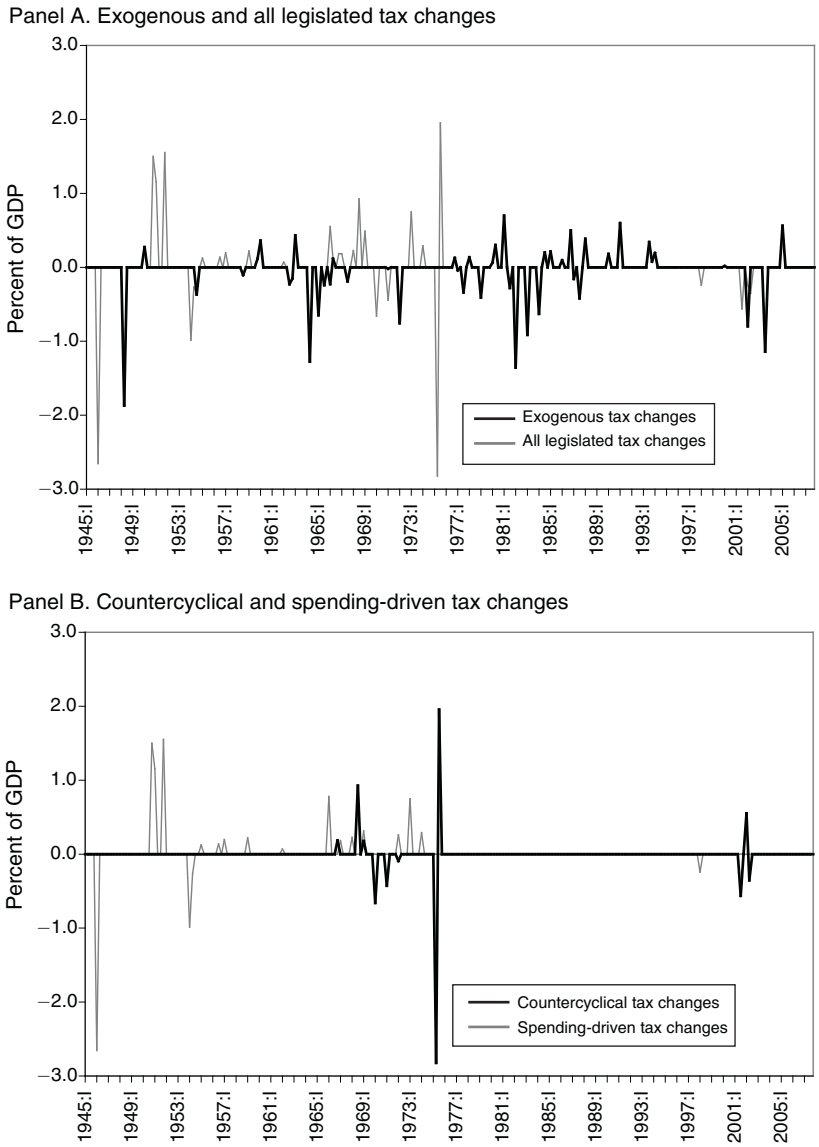

FIGURE 2. COMPARING NEW MEASURE OF FISCAL SHOCKS AND ALL LEGISLATED TAX CHANGES

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 almost always tax increases. The most obvious exception was the huge reduction in taxes in 1946 motivated by the decline in spending related to the end of World War II. A majority of the spending-driven tax increases were specifically tied

to contemporaneous increases in Social Security spending. 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. 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.[9] To make the comparison with 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.[10]

One complication is that the quarterly data on cyclically adjusted revenues are not available before 1960. However, over the period when the data are available, the change in the gap between actual and cyclically adjusted real revenues can be predicted extremely well using real output growth. As a result, it is straightforward to project the change in real cyclically adjusted revenues backward. To do this, we 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 use the estimated relationship to obtain estimates of the change in cyclically adjusted revenues before 1960.[11]

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 are reflected in movements in cyclically adjusted revenues. One way to quantify this relationship is to regress the change in cyclically adjusted revenues on a constant, the contemporaneous value, two leads, and two lags of our measure of exogenous tax changes. The coefficient on the contemporaneous value is 0.51 ($t = 4.18$), and the coefficients on each of the leads and lags is about 0.1 (but not significant). The sum of the coefficients on the five permutations of the new tax variable is 0.94 ($t = 3.52$). This regression confirms that movements in the new series show up in the change in cyclically adjusted revenues, but with some variation in the exact timing.

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

[10] 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.

[11] 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 (1960:II–1970:I). This yields:

$$\Delta[\ln R_t - \ln C_t] = \underset{(0.001)}{-0.019} + \underset{(0.06)}{1.86}\,\Delta Y_t,$$

$$R^2 = 0.97, \text{ s.e.e.} = 0.0030, \text{ D.W.} = 1.62.$$

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 $\{\Delta R_t - [\Delta(\ln R_t - \ln C_t)]R_t\}/\text{Real GDP}_t$. Our constructed estimate for the period 1947:II to 1960:I is therefore $(\Delta R_t - Z_t R_t)/\text{Real GDP}_t$, where Z_t is the fitted value from the regression.

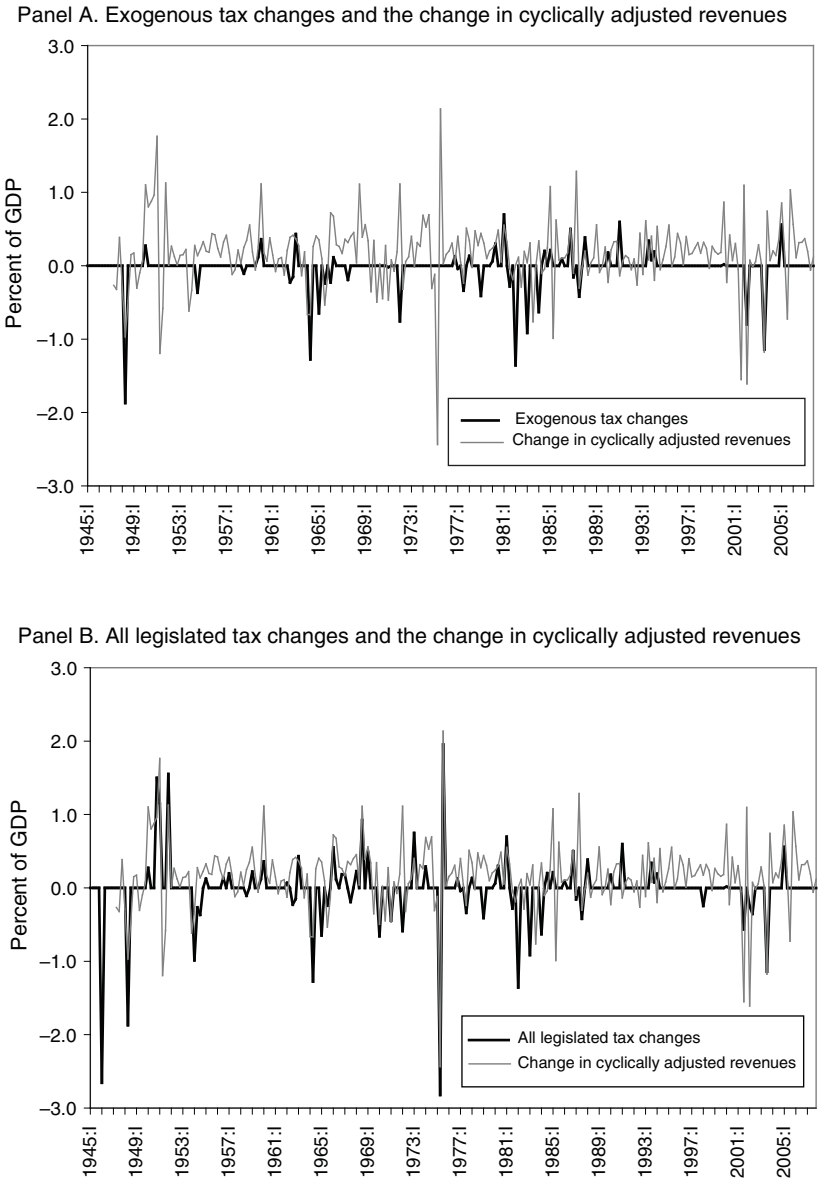

FIGURE 3. COMPARING NEW MEASURE OF FISCAL SHOCKS AND CYCLICALLY ADJUSTED REVENUES

At the same time, however, there are clearly important differences between the two series. In terms of average changes, the mean of the change in cyclically adjusted revenues is decidedly positive (0.16 percent of GDP), while that of exogenous tax changes is slightly negative. The standard deviation is 0.47 percentage points, almost exactly twice that of exogenous changes. Thus, there is substantial variation in the change in cyclically adjusted revenues that is not in our new measure of fiscal shocks.

Some of the largest movements in cyclically adjusted revenues that have no counterpart in our series correspond to the 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. But

many of the changes in cyclically adjusted revenues that do not appear in our series reflect 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. 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, 50–52). The late 1970s are another period when nonpolicy factors were important. Rapid inflation resulted in substantial bracket creep, and hence unlegislated, noncyclical increases in revenue. These differences show that nonlegislated factors are an important source of movements in cyclically adjusted revenues.[12]

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 the 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 real output in the near term. 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:

$$\Delta Y_t = a + \sum_{i=0}^{M} b_i \Delta T_{t-i} + e_t, \tag{6}$$

where Y is the logarithm of real output and ΔT 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:

$$\Delta Y_t = a + \sum_{i=0}^{M} b_i \Delta T_{t-i} + \sum_{j=1}^{N} c_j \Delta Y_{t-j} + e_t. \tag{7}$$

[12] Alan J. Auerbach (2000) stresses that many nonpolicy 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.

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 important, 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 and prospective macroeconomic conditions, perhaps the democratic process causes such changes to be correlated with economic performance. For example, candidates advocating tax cuts might be more likely to be elected 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 output using the chain-type quantity index for GDP.[13] ΔT is our measure of exogenous tax changes as a percent of GDP.[14] The data are quarterly. Our tax measure is available beginning in 1945:I, and real GDP is available beginning in 1947:I. To allow for a substantial number of lags, we begin our estimation in 1950:I. The final observation is 2007:IV. In estimating equation (6), we include 12 lags of the tax variable. In estimating (7), we again include 12 lags of the tax variable but add only 11 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 12 lags and keep the baseline sample period.

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,

[13] The quantity data are from the National Income and Product Accounts, Table 1.1.3 (accessed February 17, 2008).

[14] In our basic specifications, we use the version of the series that does not account for retroactive features of tax actions (see n. 6). 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 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.

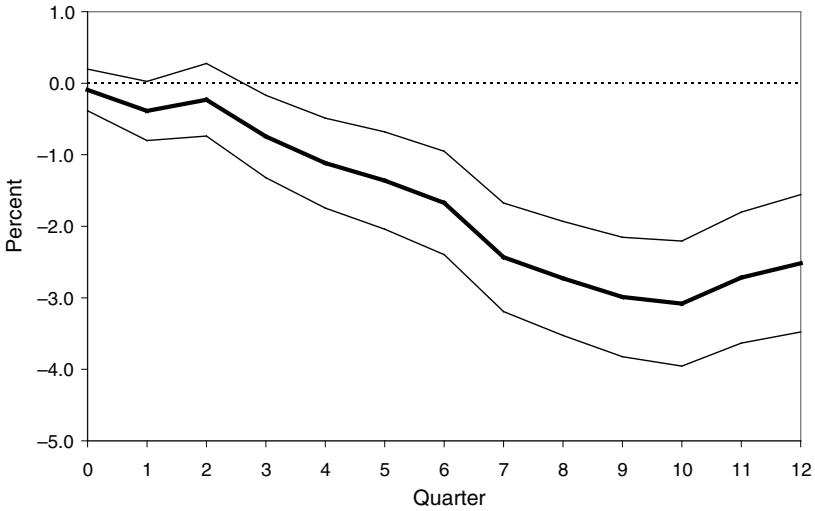

Figure 4. Estimated Impact of an Exogenous Tax Increase of 1 Percent of GDP on GDP
(*Single equation, no controls*)

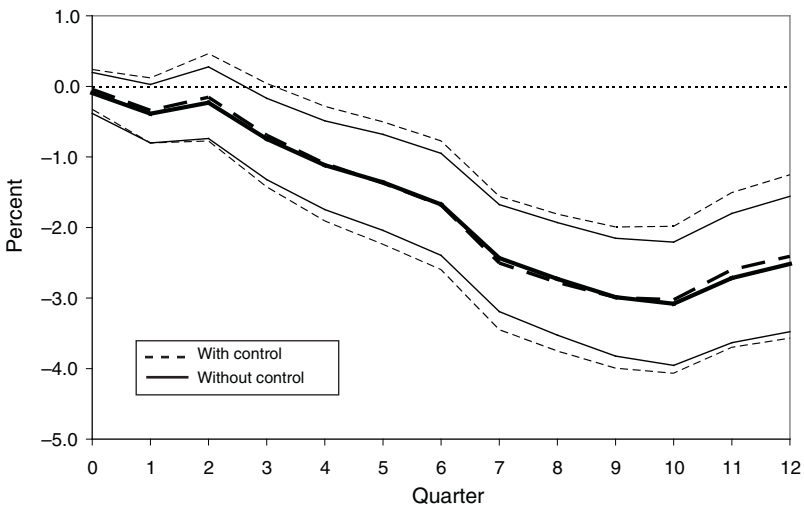

Figure 5. Estimated Impact of an Exogenous Tax Increase of 1 Percent of GDP on GDP
(*Single equation, controlling for lagged GDP growth*)

but also the effects working through lagged output.[15] 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

[15] 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.

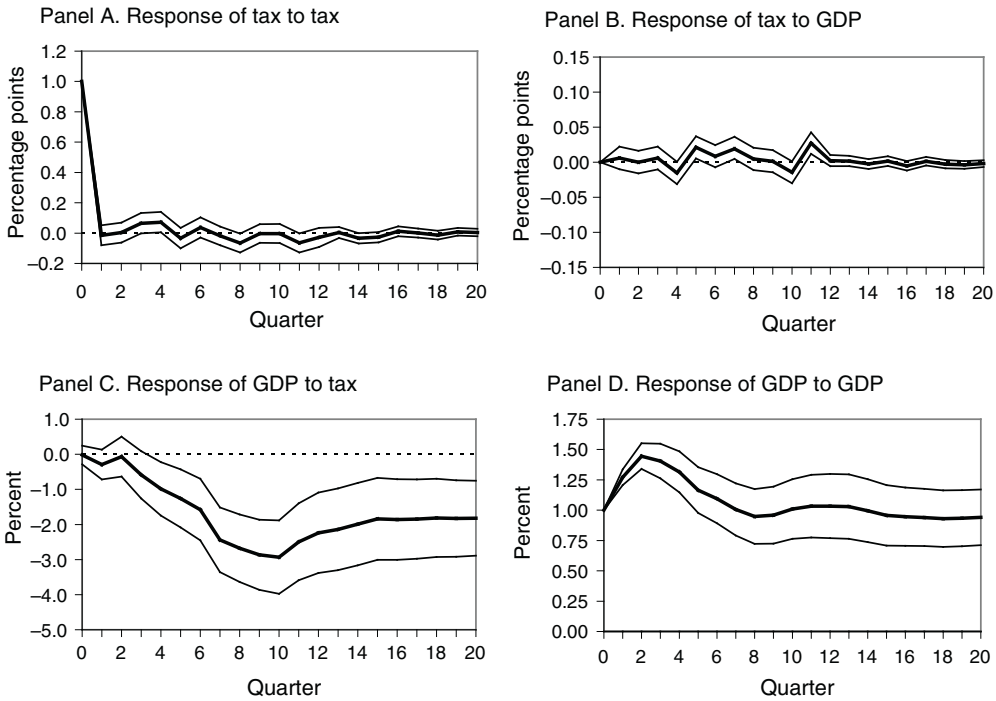

FIGURE 6. RESULTS OF A TWO-VARIABLE VAR FOR EXOGENOUS TAX CHANGES AND GDP

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.[16]

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.[17] 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 past output movements.

[16] The specification including lagged growth allows for the possibility of further effects of tax changes on output beyond 12 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.

[17] The standard errors are computed in the same way as those for the regression that includes lagged GDP growth.

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 15 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.

C. *Comparison with 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. As in Section III, we consider two broader measures: the change in real cyclically adjusted tax revenues as a percent of real GDP, and our measure of all legislated tax changes (as a percent of nominal GDP).

Panel A of Figure 7 shows the implied impact of a tax change on real GDP from the simple 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 measure of exogenous tax changes. The results show that the estimates based on the broader measures 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 -1.10 percent ($t = -2.27$) 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 nonpolicy, noncyclical factors present in cyclically adjusted revenues.

The results from the simple 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 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 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. The panels also repeat the VAR results for our 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. 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 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 broader measures than using the new measure. The maximum impact is a decline of -1.21 percent ($t = -2.08$) using the change in cyclically adjusted

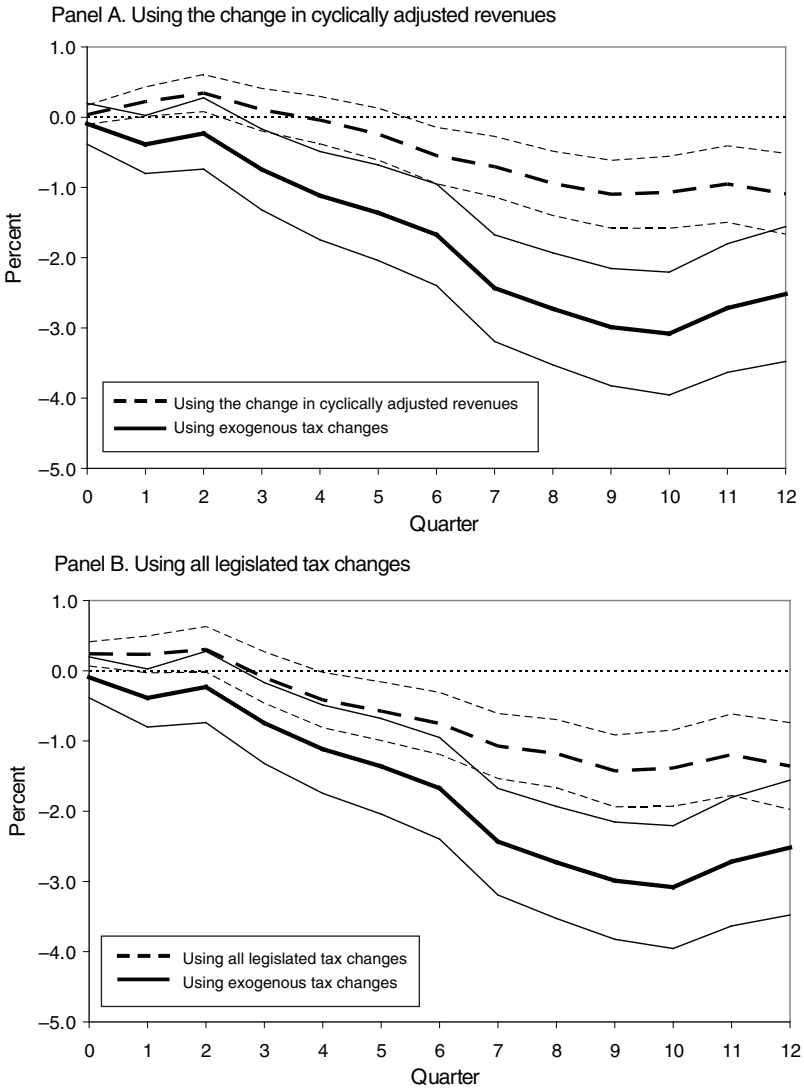

FIGURE 7. ESTIMATED IMPACT OF A TAX INCREASE OF 1 PERCENT OF GDP ON GDP USING BROADER MEASURES
(*Single equation, no controls*)

revenues; −1.86 percent ($t = -2.74$) using all legislated tax changes; and −2.93 percent ($t = -2.80$) using exogenous tax changes. 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 nonpolicy, noncyclical factors that are 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 measure of exogenous tax changes comprises tax changes with two categories of motivations: those taken to deal with an inherited budget deficit and those taken

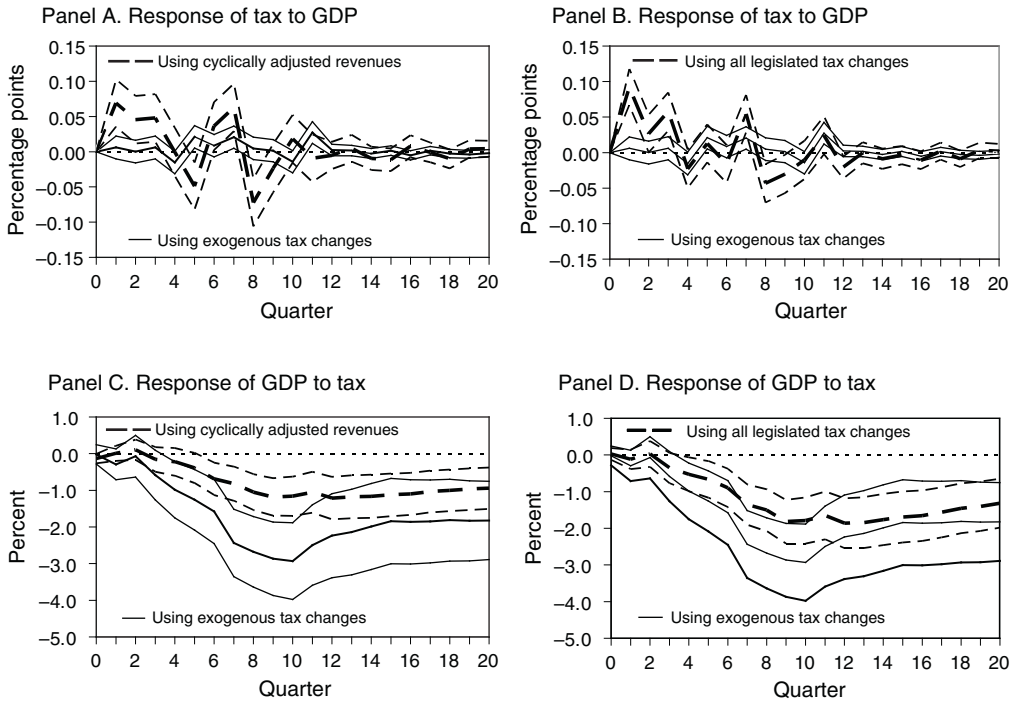

FIGURE 8. RESULTS OF TWO-VARIABLE VARS FOR BROADER MEASURES OF TAX CHANGES AND GDP

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.

Panel C shows that the output effect of a long-run tax increase of one percent of GDP is virtually identical to that of 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. Panel B shows that these 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 fairly long lags, suggests that the tax changes are unlikely to be highly correlated with other developments affecting output in the future.

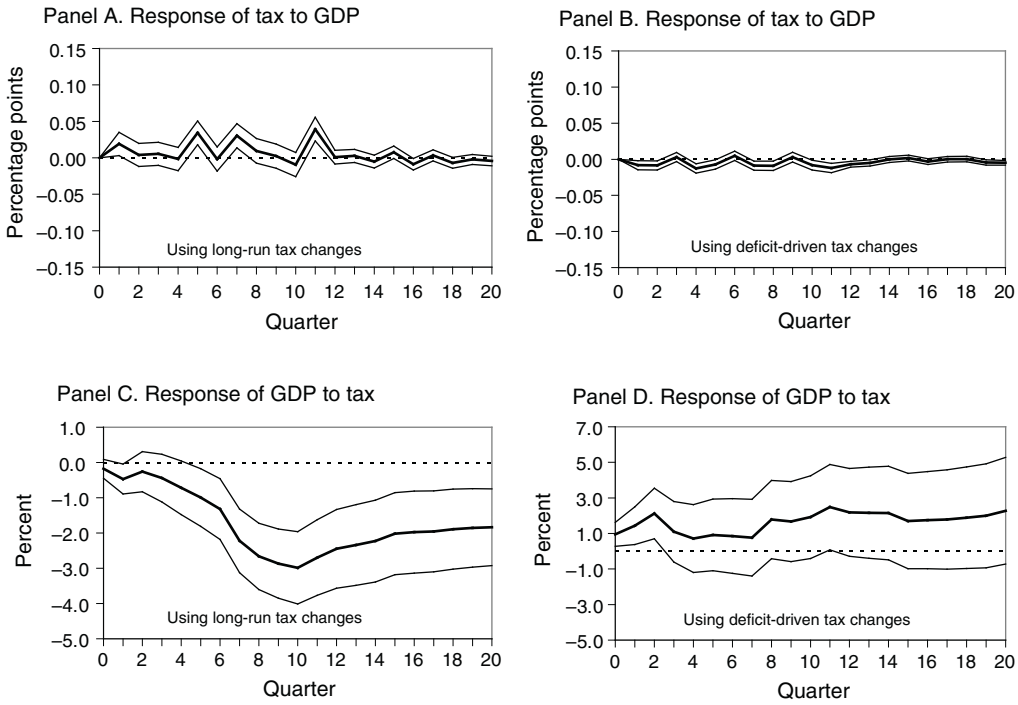

FIGURE 9. RESULTS OF TWO-VARIABLE VARS FOR THE TWO TYPES OF EXOGENOUS TAX CHANGES AND GDP

Panel D shows 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.[18]

V. Robustness

In this section, we test the robustness of the finding that changes in taxes have large effects on output. We investigate the sensitivity of the results to outliers and the sample period. We also examine the effects of including numerous control variables.

[18] 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 (see n.4). 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.

A. *Outliers and Sample Period*

While large exogenous tax changes are surely reasonable observations to consider, it is natural to ask whether they are driving the results. We therefore 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 Revenue Act of 1964, the Economic Recovery Tax Act of 1981, and the Economic Growth and Tax Relief Reconciliation Act of 2001 together with the Jobs and Growth Tax Relief Reconciliation Act of 2003.[19]

The results suggest that the estimates are quite durable. For two of the actions, excluding the extreme observations has very little impact. The maximum output 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 the cut was followed by the Korean War and the associated wartime boom. Another way to address the possible importance of the extreme observations in the early postwar era is to start the sample in 1955:I. Figure 10 shows the cumulative impact of an exogenous tax increase on GDP for this case. The estimated maximum output effect of a tax increase of one percent of GDP is now -2.63 percent ($t = -2.68$), just slightly smaller than the maximum impact for the full sample.

Shortening the sample 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 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 -1.57 percent ($t = -2.93$). 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 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 output effect is -2.36 percent ($t = -3.55$). 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 nonpolicy, noncyclical factors does, however.

B. *Controlling for Government Spending*

Government spending, especially large military actions, can have a powerful impact on output. In constructing our measure of exogenous tax changes, we exclude tax changes motivated by a change in government spending. As a result, our tax measure should not be correlated with this other determinant of output, and our VAR should yield unbiased estimates of the effects of tax changes. But chance correlation is always a possibility. And, to the degree that

[19] Specifically, we set our series on exogenous tax changes to zero in all quarters from the first to the last quarter affected by the 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.

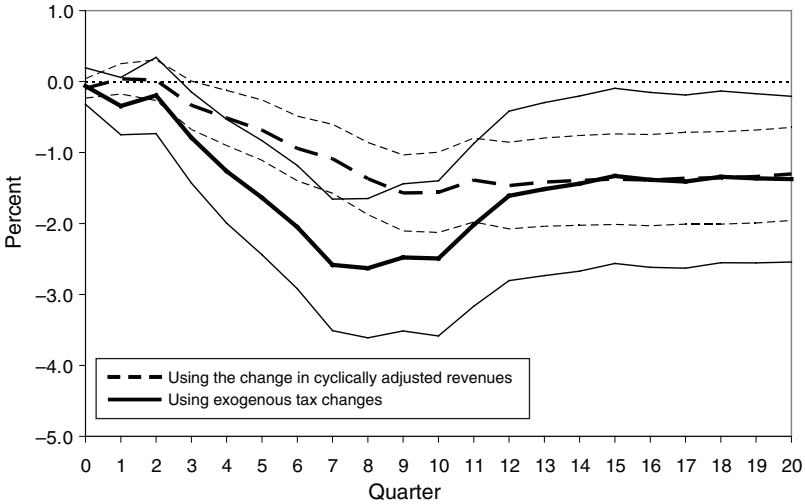

FIGURE 10. ESTIMATED IMPACT OF A TAX INCREASE OF 1 PERCENT OF GDP ON GDP, EXCLUDING KOREA (*Two-variable VAR*)

the government budget constraint binds, there may be positive correlation between tax changes and spending, even if the link is not explicitly mentioned in the narrative record. For these reasons, it is prudent to test the robustness of our findings to including a measure of government spending.

We control for government spending by estimating a VAR with three variables: our measure of exogenous tax changes, log GDP, and log real federal total gross government expenditures less interest payments.[20] The expenditure data start in 1947:I. As before, we include 12 lags.

Figure 11 shows that controlling for government spending has little impact on the estimated effects of exogenous tax changes. The maximum output effect of a tax increase of one percent of GDP is -2.75 percent ($t = -2.53$), which is just slightly smaller than the estimate from our baseline VAR. Moreover, there is no evidence that our measure of exogenous tax changes responds to spending changes: the p-value for the test of the null hypothesis that spending does not enter the equation for our tax series is virtually one.

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.

[20] 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. Because data on net purchases of nonproduced assets (which are a minor component of federal total gross expenditures) are not available until 1959:III, for the period 1947:I–1959:II we estimate total gross expenditures less interest as current expenditures plus gross government investment plus capital transfer payments, minus interest payments.

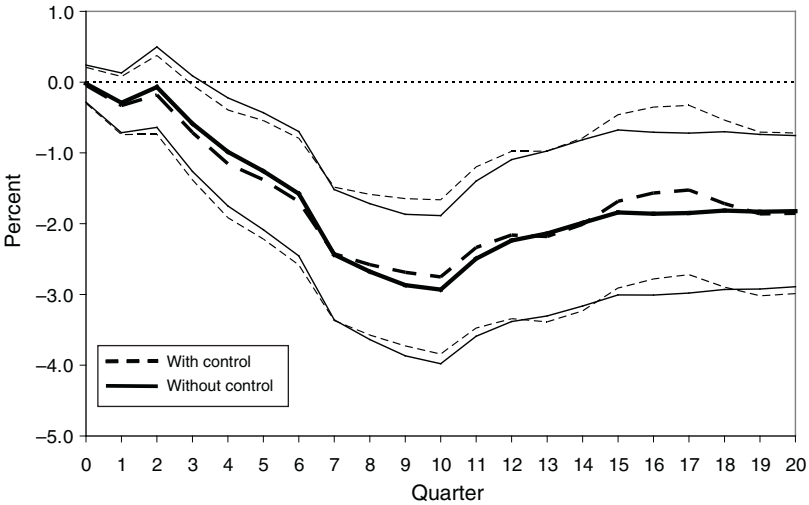

FIGURE 11. ESTIMATED IMPACT OF AN EXOGENOUS TAX INCREASE OF 1 PERCENT OF GDP ON GDP, CONTROLLING FOR GOVERNMENT EXPENDITURES (*Three-variable VAR*)

The changes are quite small, however, and the estimates remain substantially smaller than those estimated using the exogenous tax changes.[21]

C. *Controlling for Other Variables*

While government spending is perhaps the most obvious omitted variable to consider, it is by no means the only one. Myriad other factors can affect output and could be correlated with our measure of exogenous tax changes. Such correlation could be present just by accident in small samples, or it could be causal if there are systematic imperfections in the narrative record or in our interpretation of it. For this reason, we experiment with augmenting our VAR of exogenous tax changes and output with a range of third variables. We follow this sequential approach because data limitations do not allow us to estimate VAR systems with many variables using the large number of lags necessary to capture the dynamic response of output to tax changes. The three-variable specification allows us to continue to include 12 lags.

We consider several additional variables. One is a measure of supply shocks. In particular, we include the relative price of crude oil in the VAR.[22]

We also consider three measures of monetary policy. One is simply the federal funds rate. This series is a standard indicator of monetary policy and is available for almost our entire sample period.[23] 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

[21] The maximum contractionary impact of a tax change of one percent of GDP on output in the three-variable VAR is −1.27 percent ($t = -2.05$) when the change in cyclically adjusted revenues is used as the tax variable, and −2.01 percent ($t = -2.73$) when all legislated tax changes is used.

[22] 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.

[23] The data for the funds rate from 1954:III to 2007:IV 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 Edward J. Martens (1958).

Table 1—Effect of Including Additional Variables in the VAR

Third variable in VAR (sample period)	Maximum contractionary impact on GDP of a tax increase of 1% of GDP (standard error)	Maximum impact in VAR without third variable (standard error)	*p*-value for exclusion of third variable in tax equation
Government spending (1950:I–2007:IV)	−2.75% (1.07)	−2.93% (1.05)	1.000
Relative price of oil (1950:I–2007:IV)	−2.54 (1.07)	−2.93 (1.05)	0.896
Romer and Romer dummy (1950:I–2007:IV)	−2.32 (0.96)	−2.93 (1.05)	0.792
Federal funds rate (1953:I–2007:IV)	−2.18 (0.80)	−2.76 (1.52)	0.023
Romer and Romer shock (1972:I–1996:IV)	−3.61 (0.90)	−2.72 (1.42)	0.004
Republican president dummy (1950:I–2007:IV)	−3.07 (1.00)	−2.93 (1.05)	0.008

Notes: All VARs include the new measure of exogenous tax changes and log real GDP. See text for the description and data source for the various third variables. The VARs include 12 lags.

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 1969:I–1996:IV.[24]

Finally, we consider a political indicator. It is possible that political ideology affects tax policy and also independently affects macroeconomic developments through other channels. For example, perhaps Republican administrations consistently cut taxes for philosophical reasons and also lessen regulation. We proxy for politically correlated other factors with a simple dummy variable for whether the president is a Republican.

Table 1 reports the bottom line of these robustness exercises. In each case, it gives the maximum contractionary impact of an exogenous tax increase of one percent of GDP on GDP both with and without the control variable. The results for the no-control baseline vary slightly because the sample period has to be adjusted to reflect the availability of the control variable. For completeness, the table also reports the results including government spending described above.

The table shows that including the control variables has little effect on the estimated impact of our new tax variable on GDP. As already discussed, including government spending has virtually no effect. Including the relative price of crude oil reduces the contractionary impact of a tax increase slightly. Including a dummy variable for whether the president is a Republican increases the impact slightly. The effect of including the monetary policy controls varies with which series is used. Including the federal funds rate or the Romer and Romer dummy variable lessens the contractionary impact of a tax increase by about 20 percent relative to the baseline effect in the same sample period. Including the continuous Romer and Romer shock series increases the

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

impact by about 30 percent. In all cases, the effect of tax changes on output remains large and highly statistically significant.

Thus, the finding that tax changes have substantial impacts on output appears to be very durable. That including controls for known output shocks has little effect on the estimated impact of tax changes is important indirect evidence that our new measure of fiscal shocks is not correlated with other factors affecting output.

More direct evidence on this can be found in the test for the predictive power of each control on exogenous tax changes. The final column of Table 1 shows the p-value for the test that all of the lagged values of the control variable are zero in the equation for our measure of exogenous tax changes. Recall that we have already shown that the p-values for both output and government spending are very large; neither variable predicts our measure of fiscal shocks. The table shows that the p-values are also very high for the relative price of oil and the Romer and Romer dummy variable for shifts to anti-inflationary policy. Thus, the new tax series is not predictable from supply shocks or this particular measure of monetary shocks.

The new fiscal shock series does appear to be somewhat predictable from the two monetary shock series based on the funds rate. 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, came soon after the dramatic shift to tight monetary policy under Paul Volcker, which is when these 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. 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.

The new series is also quite predictable using the political dummy variable. However, the sign of the coefficient on the individual lags of the dummy variable switches between positive and negative, suggesting that the effect is not straightforward. More importantly, since our measure of exogenous tax changes is designed to be driven primarily by fundamentals, such as ideology, rather than by economic conditions or changes in government spending, predictability by political variables is not only not worrisome, but to be expected.

VI. Extensions

In this section, we extend our results in three directions. First, we examine whether the enactment of tax legislation has an important impact on output through expectations, or whether the response is mainly associated with the change in taxes 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. The reason for using this approach as our baseline is that, 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 effects of tax increases through incentives 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. This initial reaction could then trigger additional dynamics through such channels as habits and effects on other sectors. But, there should be no additional reaction associated with the time when tax liabilities actually change. In this view, the correct approach is to relate output movements to current and lagged values of a measure of news about tax changes.

An obvious intermediate possibility is that some consumers follow the permanent income hypothesis, while others do not respond until taxes actually change (as in John Y. Campbell and N. Gregory Mankiw 1989). This suggests that output responds to current and lagged values of both news about tax changes and the actual changes.

A specification that encompasses both the extremes and the intermediate possibility is:

$$\Delta Y_t = a + \sum_{i=0}^{M} b_i \Delta T_{t-i} + \sum_{j=0}^{M} c_j NEWS_{t-j} + \sum_{k=1}^{N} d_k \Delta Y_{t-k} + e_t. \tag{8}$$

Here, ΔT_t is our baseline measure of tax changes, which dates changes at the time of implementation, and $NEWS_t$ is news about the present value of future changes in tax liabilities. By construction, the news tax variable forecasts the implementation tax variable. As a result, in a VAR including both variables, the impulse response function of output to an innovation to the news series includes both the effects through news and the effects through the usual behavior of actual liabilities following a movement in the news series. Thus, it does not provide information on the separate effects of the two series. For this reason, we focus on the one-equation specification including lagged output changes as a control.

Models emphasizing substitution rather than income effects suggest a similar specification. News of a future tax increase, holding actual taxes constant, would cause intertemporal substitution toward income generation before the increase. Thus, in contrast to models based on the permanent income hypothesis, one would expect the $c_j's$ to be positive. When the tax increase is implemented, incentives to work fall, and so output falls. That is, one would expect the $b_i's$ to be negative.

Finally, news of a tax increase may improve people's expectations about the government's fiscal health. This could have a positive impact on confidence, and hence on spending and output. Like the intertemporal substitution effect, this effect goes in the opposite direction from the permanent income effect.

To investigate the effects of news about future taxes, ideally one would 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 its passage. That is, we take the stream of tax changes called for in a bill and discount them back to the quarter 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.

The calculation of the present values is straightforward. We date an action in the quarter the bill was signed. We discount the future changes using the three-year Treasury bond rate. When the individual actions for a given act have multiple motivations, we calculate a separate present value for each motivation.[25]

[25] The data on the three-year bond rate 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 April 1953. We extend the series back to 1945:I using the three-month Treasury bill rate (series H15/H15/RIFSGFSM03_N.M). The two interest rates differ by only 0.3 percentage points in April 1953. Romer and Romer (2009) provide additional details about the calculation of the present-value measure.

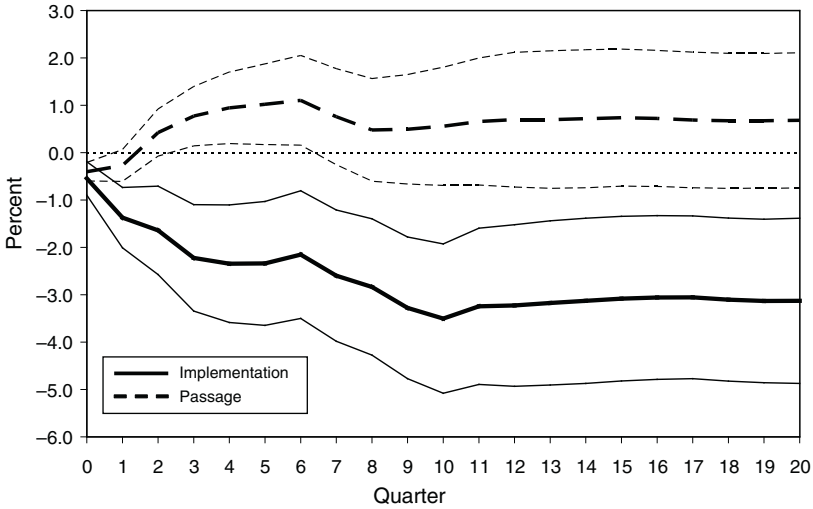

FIGURE 12. ESTIMATED IMPACT OF AN EXOGENOUS TAX INCREASE OF 1 PERCENT OF GDP ON GDP, INCLUDING TAX CHANGES DATED AT BOTH TIME OF IMPLEMENTATION AND TIME OF PASSAGE
(*Single equation, controlling for lagged GDP growth*)

Armed with the present values, we estimate equation (8). For both tax variables, we use only our exogenous tax changes. As before, we include 12 lags of the tax variables and 11 lags of output growth. Identification comes largely from the fact that the lag between passage and implementation differs across tax changes, so that the ΔT and *NEWS* variables are not perfectly correlated. Thus, we are asking whether the timing of the output responses is more closely tied to the implementation of the tax changes or to their passage.[26]

We estimate the regression over the full postwar sample. We then consider two experiments: 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 the liabilities series.

The results are shown in Figure 12. The solid line shows the impact on GDP of a tax increase of one percent of GDP dated in the quarter the change was implemented; 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 of implementation 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 are implemented.

The impact of a present-value tax increase dated in the quarter of passage is initially small, negative, and marginally significant. This could suggest some immediate impact of tax news on GDP in the direction suggested by the permanent income hypothesis. Two quarters after the change, however, the effects turn consistently positive, though they are not significantly different from zero. The maximum impact is 1.10 percent ($t = 1.17$). That the 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 suggests that

[26] The supplemental materials to the paper at http://www.aeaweb.org//articles.php?doi=10.1257/aer.100.3.763 include a slightly expanded discussion of the logic behind equation (8).

the main effects of news may operate through intertemporal substitution or confidence rather than permanent income.

These results suggest that our baseline method of dating tax changes is largely appropriate. The relationship between exogenous tax increases dated when liabilities actually change and output 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*

Panel A of Figure 13 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 separately over the periods 1950:I–1980:IV and 1981:I–2007:IV. The results suggest that the effects of tax changes may have become smaller over time. For the early period, the maximum estimated effect 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. In addition, the estimated effects are considerably more rapid for the earlier period. For example, the effect after a year is −3.69 percent for the first sample and −0.81 percent for the second. The estimates for both periods are statistically significant. For the earlier period, the t-statistic for the estimated maximum effect is −2.19; for the later period, it is −3.61. However, the standard errors are large enough that the evidence of a change over time is only modest. For example, the two-standard-error confidence interval for the response for the early period easily includes the point estimates for the later period.[27]

There are several reasons that the effects of tax changes might have become weaker. First, the Federal Reserve's response to tax changes may have become stronger over time. 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 through confidence. And third, the increasing depth and scope of financial markets may have caused the dependence of consumer spending on disposable income to decline over time. On the first two of these dimensions, there were significant changes around 1980; it is for this reason that we split the sample at the end of 1980.

To investigate whether changes in monetary policy have contributed to the apparent decline in the effects of tax changes after 1980, we examine the response of the federal funds rate to our tax measure. Panel B of Figure 13 shows the response of the funds rate in the two periods to an exogenous tax increase of one percent of GDP in a VAR that adds the funds rate to our baseline specification.

The results provide mild support for the view that monetary policy now responds more aggressively to changes in fiscal policy. For the earlier period, the estimated response fluctuates between positive and negative and is never significant. For the later period, the response over the first three years is generally a fall of one-half to one percentage point, and is often significantly different from zero. At longer horizons, however, the estimates turn positive, though not significantly so. Thus, the results suggest that there has been a stronger monetary policy response to fiscal policy in the later period.[28]

[27] 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 1955:I, the estimated effect after a year increases to −4.09 percent, and the maximum impact is −4.73 percent after seven quarters.

[28] These results are somewhat sensitive to the inclusion of the period of highly volatile interest rates during the Volcker disinflation. Ending the first sample period in 1979:III has little impact on the estimates. Starting the later sample in 1984:I, however, makes the estimates smaller and never more than marginally significant.

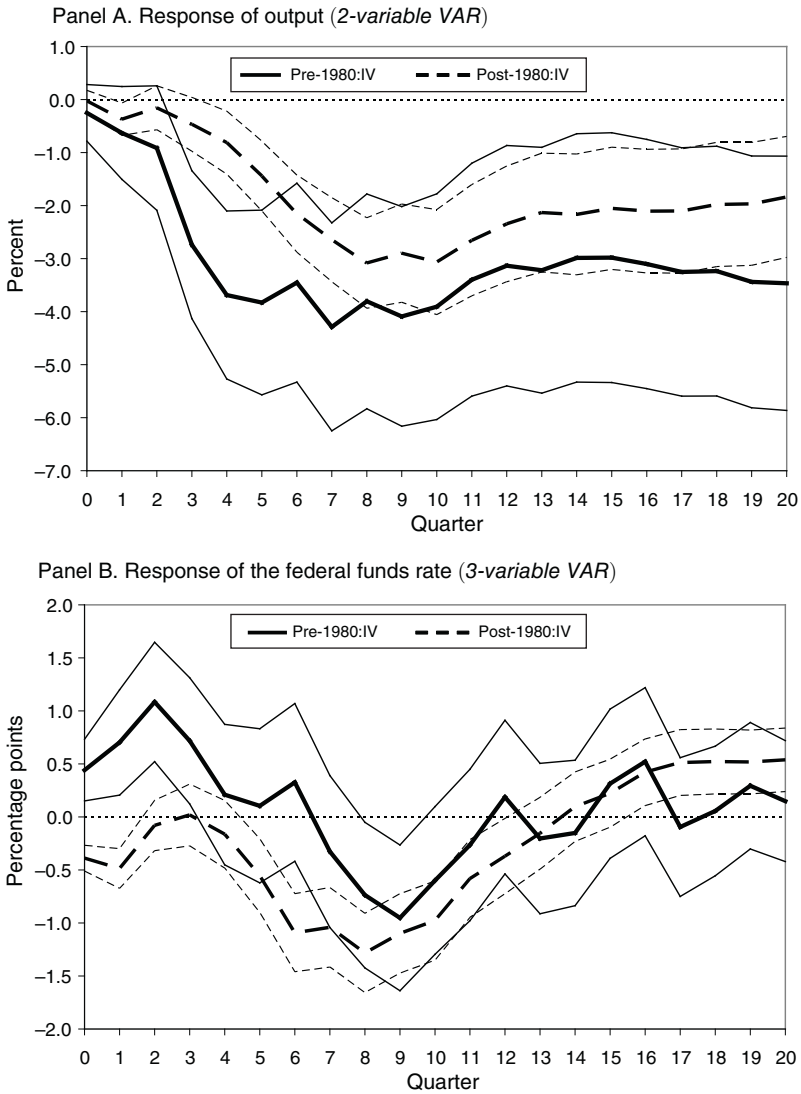

Figure 13. Changes in the Impact of an Exogenous Tax Increase of 1 Percent of GDP over Time

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

We find 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 measure of exogenous tax changes.[29]

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

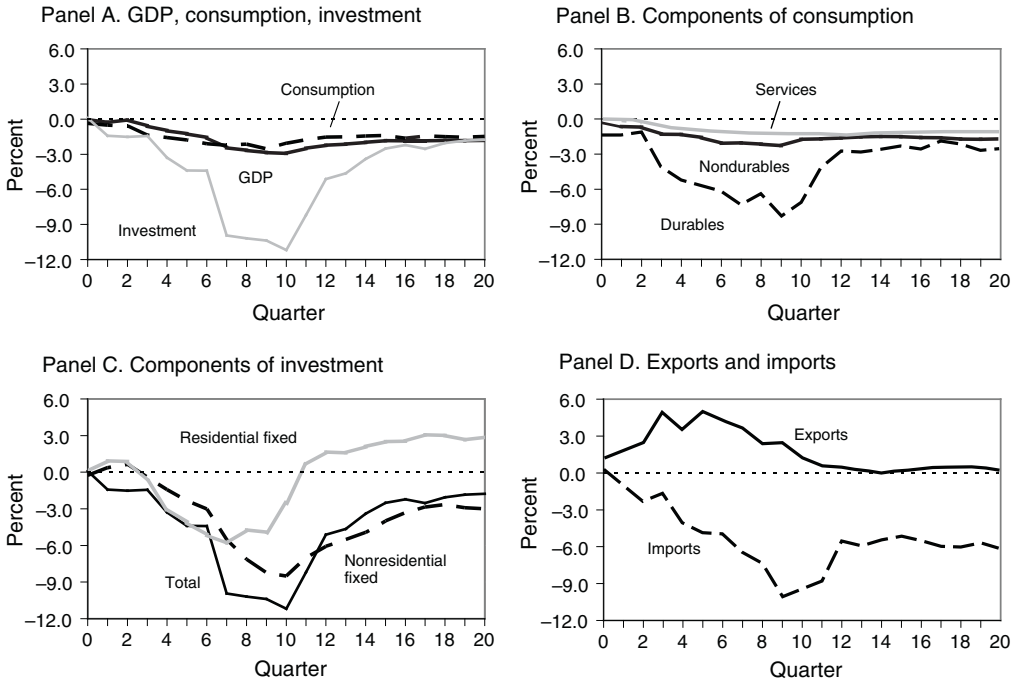

FIGURE 14. ESTIMATED IMPACT OF AN EXOGENOUS TAX INCREASE OF 1 PERCENT OF GDP ON THE COMPONENTS OF GDP (*Three-variable VARs*)

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 a major component of real GDP. As before, we include 12 lags and focus on the full postwar sample (1950:I–2007:IV).

The results are presented in Figure 14. Panel A shows the estimated responses of consumption and investment to an exogenous tax increase. For comparison, it also repeats the estimated response of 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 ($t = -3.35$).[30]

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. 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, Andrew B. Abel and Blanchard 1986; Steven M. Fazzari, R. Glenn Hubbard, and Bruce C. Petersen 1988; and Stephen Oliner, Glenn Rudebusch, and Daniel Sichel 1995).[31]

[30] Blanchard and Perotti (2002) 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.

[31] Our series on exogenous tax changes is not well suited to measuring the impact of tax changes on long-term interest rates. Long-term rates are likely to respond to news about future tax changes, and even the present-value measure from Part A of this section is a highly imperfect measure of news. Nonetheless, we find some evidence that tax increases reduce long-term rates. In a three-variable VAR with the present-value variant of our series of exogenous tax changes, log real GDP, and the ten-year government bond rate, the contemporaneous impact of legislation raising taxes by one percent of GDP on the ten-year bond rate is a fall of 0.20 percentage points ($t = -2.44$). (The data for the ten-year

Another candidate explanation of the strong investment response is that our exogenous tax changes sometimes include features that directly affect incentives for investment. However, cases of substantial exogenous tax changes accompanied by important changes in tax incentives for investment are relatively uncommon. Nonetheless, 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 in periods when there were significant changes in the tax treatment of investment.[32] This change in fact strengthens the estimated investment response. Thus, direct investment incentives do not appear to be the source of the large estimated investment effects.

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 three percent. An important part of that effect appears to be due to the procyclical behavior of investment.

Panel B of Figure 14 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 percent ($t = -2.87$) after eight quarters and then at -1.42 percent ($t = -2.48$) after 12. 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.[33] 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 exports and imports are shown in panel D. 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's 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.

VII. Conclusions

This paper investigates the causes and consequences of changes in the level of taxation in the postwar United States. We find that despite the complexity of the legislative process, most

bond rate are from the Board of Governors of the Federal Reserve System, series H15/H15/RIFLGFCY10_N.M. The data do not begin until April 1953. We extend the series back to 1945:I using the rate on bonds with maturities over ten years, series H15/discontinued/H1.RIFLGFY10P_N.M. Both series downloaded 10/9/08.)

[32] Romer and Romer (2009) describe the nature of the tax changes, including whether the act included changes in investment incentives, in each episode. The observations we exclude are 1962:III–1963:I (Changes in Depreciation Guidelines and Revenue Act of 1962), 1967:III (Public Law 90-26, Restoration of the Investment Tax Credit), 1971:I (Reform of Depreciation Rules), 1972:I (Revenue Act of 1971), and 1981:III–1984:I (Economic Recovery Tax Act of 1981 and Tax Equity and Fiscal Responsibility Act of 1982). There were other changes in investment tax incentives in exogenous tax actions, such as in the Tax Reform Act of 1969 and the Jobs and Growth Tax Relief Reconciliation Act of 2003, but the overall change in revenue in these cases went in the opposite direction of the revenue impact of the change in investment incentives. As a result, these observations could not be driving the results, and so we do not exclude them.

[33] The BEA does not provide a chain-type quantity index for real inventory investment.

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.

Our results indicate that tax changes have very large effects on output. Our baseline specification implies 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. In addition, we 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.

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. Finally, we find suggestive evidence that tax increases to reduce an inherited budget deficit do not have the large output costs associated with other exogenous tax increases.

Our results are largely silent concerning whether the output effects operate through incentives and supply behavior or through disposable income and demand stimulus. The persistence of the effects is suggestive of supply effects. But other studies have found that monetary policy, which necessarily works through demand, also has highly persistent output effects (for example, Ben S. Bernanke and Ilian Mihov 1998; and Romer and Romer 2004). The speed of the effects is suggestive of demand effects. But rapid supply responses are not out of the question.[34]

Similarly, our results do not speak to the issue of whether taxes are a more powerful tool of fiscal policy than government purchases. The fact that our estimates of the effects of tax changes are larger than conventional estimates of the effects of changes in purchases is of little relevance: conventional estimates of the effects of purchases, like conventional estimates of the effects of taxes, almost surely suffer from omitted variable bias. And changes in purchases resulting from military developments and other forces likely to be unrelated to other factors affecting output, such as those identified by Ramey and Shapiro (1998) and Ramey (2008), are usually accompanied by substantial changes in taxes (and sometimes by other policy changes), and so cannot easily be used to isolate the effects of government purchases alone.

It is also 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 nevertheless 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

[34] An earlier version of our paper (Romer and Romer 2007) presented evidence that exogenous tax increases are followed by substantial falls in inflation and substantial rises in the unemployment rate, both of which fit with demand effects. However, although the general direction of these effects is robust across specifications, the magnitudes and statistical significance are not. Thus, this evidence too is no more than suggestive.

increasing the precision of the estimates of the macroeconomic effects of tax changes by finding more observations 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 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 how far in advance it is expected, its perceived permanence, its impact on marginal tax rates, and how it affects the tax treatment of investment. For example, supply-based theories of the effects of tax changes imply that the output effects depend mainly on the impact of the changes on marginal rates, while demand-based theories imply that they depend mainly on the revenue effects. By systematically gathering information about characteristics 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 features as well. Doing so would provide insights into both the transmission mechanism of tax changes and the properties of the macroeconomy.

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