

DO TAX CUTS STARVE THE BEAST?
THE EFFECT OF TAX CHANGES ON GOVERNMENT SPENDING

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ABSTRACT

The hypothesis that decreases in taxes reduce future government spending is often cited as a reason for cutting taxes. However, because taxes change for many reasons, examinations of the relationship between overall measures of taxation and subsequent spending are plagued by problems of reverse causation and omitted variable bias. To derive more reliable estimates, this paper examines the behavior of government expenditures following legislated tax changes that narrative sources suggest are largely uncorrelated with other factors affecting spending. The results provide no support for the hypothesis that tax cuts restrain government spending; indeed, the point estimates suggest that tax cuts may increase spending. The results also indicate that the main effect of tax cuts on the government budget is to induce subsequent legislated tax increases. Examination of four episodes of major tax cuts reinforces these conclusions.

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In a speech urging passage of the 1981 tax cuts, Ronald Reagan made the following argument:

Over the past decades we've talked of curtailing government spending so that we can then lower the tax burden. Sometimes we've even taken a run at doing that. But there were always those who told us that taxes couldn't be cut until spending was reduced. Well, you know, we can lecture our children about extravagance until we run out of voice and breath. Or we can cure their extravagance by simply reducing their allowance (Address to the Nation on the Economy, 2/5/81, p. 2).¹

This idea that cutting taxes will lead to a reduction in government spending is often referred to as the “starve the beast” hypothesis: the most effective way to shrink the size of government is to reduce the revenues that feed it. This view has been embraced not just by politicians, but by distinguished economists from Milton Friedman to Robert Barro (see, for example, Friedman, 1967; Barro, 2003; and Becker, Lazear, and Murphy, 2003).

Of course, the starve the beast hypothesis is not the only view of how tax cuts affect expenditure. Another possibility is that government spending is determined with little or no regard to taxes, and thus does not respond to tax cuts. A third possibility is that tax cuts actually lead to increases in expenditure. One way this could occur is through the “fiscal illusion” effect proposed by Buchanan and Wagner (1977) and Niskanen (1978): a tax cut without an associated spending cut weakens the link in voters' minds between spending and taxes, and so leads them to demand greater spending. Another possible mechanism is “shared fiscal irresponsibility”: if supporters of tax reduction are acting without concern for the deficit, supporters of higher spending may do the same (see, for example, Gale and Orszag, 2004).

The question of how tax cuts affect government spending is clearly an empirical one. And, indeed, there have been attempts to look at the aggregate relationship between revenues and spending. However, such examinations of correlations cannot settle the issue of how changes in revenues affect spending. Revenues change for a variety of reasons. Many changes are legislated, but many others occur automatically in response to changes in the economy. And legislated tax changes themselves are

¹ Presidential speeches are from the database of presidential documents available from John Woolley and Gerhard Peters, *The American Presidency Project* (www.presidency.ucsb.edu).

motivated by numerous factors. Some, such as many increases in payroll taxes, are driven by increases in current or planned spending. Others, such as tax cuts motivated by a belief in the importance of incentives, are designed to raise long-run growth.

The relationship between revenues and spending is surely not independent of the causes of changes in revenues. For example, if spending-driven tax changes are common, a regression of spending on revenues will almost certainly show a positive correlation. But this relationship does not show that tax changes cause spending changes; causation, in fact, runs in the opposite direction. To give another example, if automatic and legislated countercyclical tax changes are common, one might expect to see government expenditures rising after declines in revenue, because spending on unemployment insurance and other relief measures typically rises in bad economic times. In this case, both revenues and spending are being driven by an omitted variable: the state of the economy. These examples suggest that looking at the aggregate relationship between revenues and spending without accounting for the causes of revenue changes may lead to biased estimates of the effect of revenue changes on spending.

This paper therefore proposes a test of the starve the beast hypothesis that accounts for the motivations for tax changes. In previous work (Romer and Romer, 2008), we identified all significant legislated tax changes in the United States since 1945. We then used the narrative record—presidential speeches, executive branch documents, Congressional reports, and records of Congressional debates—to identify the key motivation and the expected revenue effects of each action. In this paper, we use our classification of motivation to isolate tax changes that can legitimately be used to examine the effect of revenue changes on spending from those that are likely to give biased estimates. In particular, we focus on the behavior of spending following tax changes taken for long-run purposes. These are changes in taxes that are explicitly not tied to current spending changes or the current state of the economy. They are, instead, taken to promote various long-run objectives, such as spurring productivity growth, improving efficiency, or, as in the case of the Reagan tax cut discussed above, shrinking the size of government. Examining the behavior of government spending following these long-run tax changes should provide a relatively unbiased test of the starve the beast hypothesis.

We examine the relationship between real government expenditures and our new measure of long-run tax changes in a variety of specifications. We find no support for the hypothesis that a relatively exogenous decline in taxes lowers future government spending. In our baseline specification, the estimates in fact suggest a substantial and marginally significant positive impact of tax cuts on government spending. The finding of a lack of support for the starve the beast hypothesis is highly robust. The evidence of an opposite-signed effect, in contrast, is not particularly strong or robust.

The result that spending does not fall following a tax cut raises an obvious question: How does the government budget adjust in response to the cut? One possibility is that what gives is not spending but the tax cut itself. To investigate this possibility, we examine the response of both tax revenues and tax legislation to long-run tax cuts. We find that revenues fall in response to a long-run tax cut in the short run, but then recover after about two years. Most of this recovery is due to the fact that a large part of a long-run tax cut is typically counteracted by legislated tax increases within the next several years. As we discuss, the fact that policymakers are able to adjust on the tax side helps to explain why they do not adjust on the spending side.

While there are numerous long-run tax changes spread fairly uniformly over the postwar era, four stand out as the largest and most well known: the tax cut passed over Harry Truman's veto in the Revenue Act of 1948; the Kennedy-Johnson tax cut legislated in the Revenue Act of 1964; the Reagan tax cut contained in the Economic Recovery Tax Act of 1981; and the Bush tax cuts passed (along with some countercyclical actions) in 2001 and 2003. As a check on our analysis, we examine these four episodes in detail. We find that the behavior of spending and taxes in these extreme episodes is consistent with the aggregate regressions. Perhaps more importantly, we find that policymakers often did not even talk as if their spending decisions were influenced by revenue developments. They did, however, often invoke the tax cuts as a motivation for later tax increases. Finally, we find there were concurrent developments, namely wars, that account for some of the rise in spending in these episodes. But, there were other concurrent developments that caused measured spending changes to understate the effects of spending decisions that were taken in these episodes. As a result, it appears unlikely that failure of total

expenditures to fall after these tax cuts was due to chance or unobserved factors.

As mentioned above, ours is not the first study to investigate the starve the beast hypothesis. The most common approach is some variation of a regression of spending on lagged revenues; examples include Anderson, Wallace, and Warner (1986) and Ram (1988). Two more sophisticated versions of this methodology are pursued by Bohn (1991) and Auerbach (2000, 2003). Bohn, focusing on a long sample period that is dominated by wartime budgetary changes, examines the interrelationships between revenues and spending in a vector autoregression framework that allows for cointegration between the two variables (see also von Furstenberg, Green, and Jeong, 1986, and Miller and Russek, 1990). Auerbach, focusing on recent decades, studies the relationship between policy-driven changes in spending (rather than all changes in spending) and past deficits or projections of what future deficits would be if policy did not change (see also Calomiris and Hassett, 2002).

The results of these studies are mixed, but for the most part they suggest that tax cuts are followed by reductions in spending. None of these studies, however, consider the different reasons for changes in revenues, and thus none isolate the impact of independent tax changes on future spending. Indeed, our results point to a potentially important source of bias in studies using aggregate data. We find that the only type of legislated tax changes that are systematically followed by spending movements in the same direction are ones motivated by decisions to change spending. Since causation in these cases clearly does not run from the tax changes to the spending changes, this relationship is not informative about the starve the beast hypothesis. We also find that this type of tax change is sufficiently common to make the overall relationship between tax changes and subsequent spending changes substantially positive.²

The rest of the paper is organized as follows. Section I discusses the different motivations for tax

² One can also test the starve the beast hypothesis indirectly. Perhaps the best known study of this type is Becker and Mulligan (2003). They show that under appropriate assumptions, the same forces that would give rise to a starve the beast effect would cause a reduction in the efficiency of the tax system to reduce government spending. They therefore examine the cross-country relationship between the efficiency of the tax system and the share of government spending in GDP. While they find a strong positive relationship, the correlation between efficiency and spending, like that between taxes and spending, may reflect reverse causation or omitted variables. That is, countries may invest in efficient tax systems because they desire high government spending, or a third factor, such as tolerance of intrusive government or less emphasis on individualism, may lead both to a broader, more comprehensive tax system and to higher government spending.

changes, and identifies the tax actions best suited for testing the starve the beast hypothesis. Section II analyzes the relationship between tax changes and government expenditures, and includes a plethora of robustness checks. Section III examines how changes in taxes affect future tax revenues and tax legislation. Section IV discusses spending and taxes in the four key episodes. Section V presents our conclusions and discusses the limitations of our analysis.

I. THE MOTIVATIONS FOR LEGISLATED TAX CHANGES AND TESTS OF THE STARVE THE BEAST HYPOTHESIS

Legislated tax changes classified by motivation are a key input into our tests of the starve the beast hypothesis. Therefore, it is important to describe our classification of motivation, and to discuss which types of tax changes are likely to yield informative estimates of the effects of tax changes on government spending. It is also useful to provide a brief overview of our identification of the motivations for tax changes, and of our findings about the patterns of legislated tax changes in the postwar era.

A. Classification of Motivation

Our classification and identification of the motivations for postwar legislated tax changes are described in detail in Romer and Romer (2008). That paper shows that the motivations for almost all tax changes have fallen into four broad categories.

One type of tax changes are those motivated by contemporaneous changes in spending. Often, policymakers will introduce a new program or social benefit, and raise taxes to pay for it. This was true, for example, in the late 1950s when the interstate highway system was started, and in the mid-1960s when Medicare was introduced. The key feature of these changes is that the spending changes are the impetus for the tax changes. Typically such changes are tax increases, but spending-driven tax cuts are not unheard of.

A second type of tax changes are ones motivated by countercyclical considerations. These are changes made because policymakers believe that growth will be above or below normal, and therefore change taxes to try to keep growth at its normal, sustainable level. A classic example of such a countercyclical action is the 1975 tax cut. Taxes were reduced because the economy was in a severe recession and growth was predicted to remain substantially below normal. Countercyclical actions can be either tax cuts or tax increases, depending on whether they are designed to counteract unusually low or unusually high expected growth.

A third type of tax changes are those made to reduce an inherited budget deficit. By definition, these changes are all increases. A classic example of this type of change is the 1993 Clinton tax increase. This increase was undertaken not to finance a contemporaneous rise in spending, but to reduce a persistent deficit caused by past developments.

The fourth type of tax changes are ones intended to raise long-run growth. This is a broad category that includes changes motivated by a range of factors. What unites the changes is that they are all designed to improve the long-term functioning of the economy. The most common motivation is a belief that lower tax rates will improve incentives, and thereby raise long-run growth. Another motivation is a belief in the benefits of small government and a desire to return the people's money to them. A third motivation for changes in this category is a desire to improve the efficiency and equity of the tax system. Many of the most famous tax cuts, such as the 1964 Kennedy-Johnson tax cut and the Reagan tax cuts of the early 1980s, fall under the heading of tax changes to raise long-run growth. Most long-run tax changes are cuts, but some of the tax reforms included in this category increased revenues.

B. Which Tax Changes Are Useful for Testing the Starve the Beast Hypothesis?

This description of the different motivations for legislated tax changes makes it clear that some changes are much more appropriate for testing the starve the beast hypothesis than others. What we would like are tax changes that are not systematically correlated with other factors influencing government spending. An obvious implication is that spending-driven tax changes are not appropriate

observations to use. Causation in these episodes runs from desired changes in spending to changes in taxes. There is an omitted influence on spending—the prior decision to change spending—that is strongly correlated with these tax changes. Thus, if we have classified spending-driven tax changes correctly, there will be a positive correlation between these changes and spending changes by construction. Including spending-driven tax changes in a regression would therefore bias the results toward finding a starve the beast effect.

Similar reasoning suggests that examining spending changes following countercyclical and deficit-driven tax changes could also be problematic. In these cases, however, the likely bias is against the starve the beast hypothesis. In both cases, there may be spending changes that are negatively correlated with the tax changes but are not caused by them. Rather, both the tax and spending changes may be caused by a third factor.

In the case of countercyclical actions, the third factor is the state of the economy. In bad times, policymakers may cut taxes and increase spending as a way of raising aggregate demand. Also, some types of spending, such as unemployment compensation and public assistance, increase automatically in recessions. Thus, the relationship between taxes and spending in these episodes may reflect discretionary and automatic responses to the state of the economy, not a behavioral link between tax revenues and spending decisions.

In the case of deficit-driven tax changes, the unobserved third factor is a general switch to fiscal responsibility. Tax increases to reduce inherited budget deficits are often passed as parts of packages that include spending reductions. The spending reductions are not caused by the tax increases; rather, both are driven by a desire to eliminate the deficit. Inclusion of such packages in a regression will tend to bias the results away from supporting the starve the beast hypothesis. This concern may be more important in theory than in reality, however. Our narrative analysis of tax changes documents the spending reductions agreed to in conjunction with deficit-driven tax changes. In almost every case, the spending cuts were small relative to the tax increase. Therefore, while one may want to treat the behavior of spending following deficit-driven tax changes with caution, it may in fact yield relatively unbiased estimates.

The tax changes that are surely the most appropriate for testing the starve the beast hypothesis are those taken to spur long-run growth. As described above, these are tax changes not made in response to current macroeconomic conditions or in conjunction with spending changes. As a result, they are exactly the kind of changes that proponents of the starve the beast hypothesis believe are likely to alter government spending.

To the degree that focusing on this type of tax change may lead to bias, it is likely to be in the direction of finding a positive effect of taxes on spending. The ideal experiment for testing the starve the beast hypothesis is a tax change resulting from factors that have no direct impact on spending. Our long-run tax changes, however, include tax cuts for which the possible induced reduction in future spending is sometimes cited as a motivation. As a result, there is a potential correlation between spending and tax changes in these episodes driven by a third factor: a desire for smaller government. Policymakers, in addition to cutting taxes to starve the beast, may take other actions to achieve this goal. Because this possible omitted variable bias works in the direction of supporting the starve the beast hypothesis, a finding of a positive relationship between taxes and spending would have to be treated with caution. Since we in fact find a negative relationship, there is less cause for concern. Also, our narrative analysis suggests that this potential bias is likely to be small. The desire for smaller government is rarely the primary stated motivation for long-run tax changes; a belief in the incentive effects of lower taxes is considerably more common, for example.

C. Overview of the Narrative Analysis

The implementation and results of our narrative analysis of postwar tax changes are described in Romer and Romer (2008). We use a detailed examination of a wide range of policy documents to identify all significant legislated tax changes over the period 1945-2007. We then identify the motivations policymakers gave for each action. We find that policymakers are usually both quite explicit and remarkably unanimous in their stated reasons for undertaking tax actions. Only infrequently do they emphasize multiple motivations. In these cases, we divide the tax changes into pieces reflecting the

different motivations.

We also use the narrative sources to estimate the revenue impacts of the actions. Specifically, we determine how policymakers expected the actions to affect tax liabilities. Very often, tax bills change taxes in a number of steps. In these cases, our baseline revenue estimates show changes in each of the quarters the various provisions took effect.³ To make the revenue estimates comparable over time, we express them as a percent of nominal GDP.⁴

Figure 1 shows our measure of legislated postwar tax changes classified by motivation. Panel (a) shows long-run changes, which are the key actions for our purposes. The graph makes clear that the vast majority of long-run tax actions are cuts. The graph also makes clear that long-run tax changes have been fairly evenly distributed over the postwar era. The largest long-run tax changes were the 1948 tax cut, the Kennedy-Johnson tax cut in the mid-1960s, the Reagan tax cut in the early 1980s, and the two Bush tax cuts in the early 2000s.

Panel (b) shows the other types of tax changes. While there were a number of small deficit-driven increases in the first half of the postwar era, the vast majority took place in the 1980s and early 1990s. Most of the deficit-driven tax increases were passed to deal with the long-run solvency of the Social Security and Medicare systems. Spending-driven changes are typically tax increases, and were both frequent and relatively large in the first half of the postwar era. By far the largest spending-driven tax changes were those in the Revenue Act of 1945 following the end of World War II and those in the early 1950s to pay for the Korean War. Many of the other changes in this category were related to expansions of Social Security. Finally, explicitly countercyclical tax changes were confined to the fairly short period 1966-1975 until being resurrected as the reason for portions of the tax cuts in 2001 and 2002.

³ Tax actions are often retroactive for a quarter or two. When a change is retroactive in this way, it has a much larger effect on liabilities in the initial quarter than in subsequent ones. In differences, this results in a large movement in one direction in the initial quarter and a partially offsetting movement in the next quarter. For this study, which examines the longer-run responses of spending and future taxes, the short-run volatility caused by these changes may unnecessarily complicate the analysis. We therefore use the revenue estimates ignoring the retroactive changes as our baseline estimates. Using the estimates including the retroactive changes has almost no impact on any of the results.

⁴ The nominal GDP data are from the National Income and Product Accounts, Table 1.1.5 (www.bea.gov, downloaded 2/17/08). Quarterly nominal GDP data are only available after 1947. We therefore normalize the one tax change in 1946 using the annual nominal GDP figure.

II. THE EFFECT OF TAX CHANGES ON EXPENDITURES

The previous section describes our identification of legislated tax changes motivated by concern about long-run growth. This section investigates the relationship between these relatively exogenous tax changes and subsequent changes in government spending. It includes a detailed analysis of the robustness of the results. We also investigate the behavior of spending following other types of tax changes to see if there is evidence of bias when other tax measures are used.

A. Specification and Data

To estimate the effects of tax changes on government spending, we begin by estimating a simple reduced-form regression using quarterly data of the form:

$$(1) \quad \Delta E_t = a + \sum_{i=0}^N b_i \Delta T_{t-i} + e_t,$$

where ΔT is our measure of long-run tax changes (expressed as a percent of nominal GDP) and ΔE is the change in the logarithm of real government expenditure.

The key feature of our measure of long-run tax changes is that it is based on actions motivated by considerations largely unrelated to current spending, current macroeconomic conditions, or an inherited budget deficit. Our discussion of why such long-run changes provide the best test of the starve the beast hypothesis suggests that they are unlikely to have a substantial systematic correlation with other factors affecting spending. It is for this reason that our baseline specification includes no control variables. However, it is certainly possible that there are correlations in small samples, or that the dynamics of the relationship between tax changes and spending are more complicated than in equation (1). We therefore consider a wide range of control variables and a variety of more complicated specifications.

We include a number of lags of the tax variable to allow for the possibility that the response of spending to tax changes may be quite delayed or gradual. In our baseline specification we set the number

of lags to twenty, and so look at the response of spending over a five-year horizon. Because the starve the beast hypothesis does not make predictions about the exact timing of how spending responds to tax changes, we focus on the cumulative effect of a tax change on expenditures at various horizons. We summarize the regression results by reporting the implied impact of a tax cut of one percent of GDP on the path of expenditures (in logarithms). For our baseline specification, the cumulative impact after n quarters is just minus the sum of the coefficients on the contemporaneous value and first n lags of the tax variable. The starve the beast hypothesis predicts that tax cuts reduce spending. Therefore, the estimated cumulative impact of a tax cut on expenditures should be negative if the hypothesis is correct.

We use quarterly data on government expenditures from the National Income and Product Accounts. Our series on long-run tax changes refers only to federal legislation. Therefore, we consider only the behavior of federal expenditures. What the Bureau of Economic Analysis calls “total expenditures,” however, includes two components that are not appropriate in thinking about the response of spending to tax changes. One is a deduction for the consumption of fixed capital (that is, depreciation). This largely reflects spending decisions in the distant past, and so almost surely cannot show a starve the beast type of response. Thus, we do not subtract depreciation. The other component is interest payments on government debt. For a given interest rate, interest payments rise with the amount of debt. As a result, any tax cut that increases the deficit will almost certainly increase interest payments. We therefore exclude this type of spending. The resulting aggregate that we consider is thus total gross expenditures less interest. For simplicity, we refer to this as total expenditures in what follows.⁵

The NIPA expenditure data are expressed in nominal terms. For some components, such as defense and nondefense purchases, corresponding deflators exist. However, for some categories, especially those involving transfers, there are no associated deflators. We therefore deflate total gross

⁵ Data on total expenditures, consumption of fixed capital, and interest payments are from the National Income and Product Accounts, Table 3.2 (downloaded 2/17/08). Because the BEA does not have data on “net purchases of nonproduced assets” (which are normally a trivial component of total expenditures) until 1959Q3, before then we estimate total gross expenditures less interest as the sum of current expenditures, gross government investment, and capital transfer payments, minus interest payments.

expenditures less interest by the price index for GDP.⁶

Our data on tax changes begin in 1945Q1, and the data on expenditures begin in 1947Q1. Therefore, in the baseline specification, where we include twenty lags of the tax variable, the earliest starting date for the regression is 1950Q1. However, previous work has found some evidence that the behavior and effects of fiscal policy were unusual in the Korean War period (see, for example, Blanchard and Perotti, 2002, and Romer and Romer, 2009). We therefore also consider starting the estimation in 1957Q1. In both cases, we carry the regressions through 2007Q4.

B. The Effect of Long-Run Tax Changes on Total Expenditures

Table 1 shows the results of estimating equation (1) for total expenditures using twenty lags of the long-run tax variable over the full sample. The coefficient estimates for the individual lags fluctuate between positive and negative. As one would expect, few of the individual coefficients are statistically significant. The overall fit of the regression is modest ($R^2 = 0.20$).

Figure 2 summarizes the results by showing the implied response of total expenditures to a long-run tax cut of one percent of GDP, together with the one-standard-error bands. There is no evidence of a starve the beast effect. The cumulative effect is negative in the quarter of the tax cut and the subsequent three quarters, as the starve the beast hypothesis predicts, but very small, and the t-statistics do not rise above 0.6 in absolute value. After that, the estimated cumulative effect is positive at every horizon except quarters 9 and 10, suggesting fiscal illusion or shared fiscal irresponsibility.

The estimated positive impact of the tax cut on spending is often substantial. Since federal government spending averages roughly twenty percent of GDP in our sample, a tax cut of one percent of GDP is equal to about five percent of government spending. The point estimates suggest that a tax cut of one percent of GDP raises spending by four percent or more in quarters 13 through 20. That is, they suggest that spending eventually rises by almost the amount of the tax cut. However, the estimates are not very precise. The t-statistics for the cumulative impact of the tax cut on spending at horizons of more

⁶ The price index for GDP is from the National Income and Product Accounts, Table 1.1.4 (downloaded 2/22/08).

than three years are generally between 1.5 and 2, and exceed 2 for only one horizon (quarter 14, for which the t-statistic is 2.21).

C. Richer Dynamics

Our baseline results suggest that there is no discernable starve the beast effect, and some evidence of shared fiscal irresponsibility, over a five-year horizon. But, perhaps the main effects of tax changes occur with longer lags. Here we consider several approaches to allowing for more delayed effects.

Additional Lags. The most straightforward approach to examining whether tax changes have important effects at longer horizons is to include additional lags in equation (1). Of course, including more lags requires shortening the sample period and estimating additional parameters. The results of including forty lags of the tax variable in (1) and estimating the regression over the longest feasible sample (1955Q1–2007Q4) are shown in panel (a) of Figure 3. For horizons beyond five years, the estimated cumulative impact of a tax cut of one percent of GDP on total expenditures is always small, fluctuates between positive and negative, and is never remotely close to statistically significant. Thus, this change provides no evidence that tax cuts reduce government spending, but also fails to support the hypothesis that they increase it.

A Two-Variable VAR. Our second approach to allowing for more complicated and potentially longer-lasting dynamics is to estimate a vector autoregression (VAR) with our series for long-run tax changes and total expenditures. This approach allows spending to depend on its own lags as well as on the tax changes, and so allows for dynamics beyond the number of lags of the tax variable that are included.

For consistency with our regressions, we put the tax changes first and expenditures second, so tax changes can affect spending within the quarter. We enter expenditures in log levels; given the availability of the data, this allows us to include twelve lags while still using our baseline sample. Panel (b) of Figure 3 shows that the estimated response of spending to an innovation of minus one percent of GDP to our series on long-run tax changes is similar to the estimated effect of a long-run tax cut of one percent of

GDP in the baseline specification.⁷ The point estimates suggest that the tax cut reduces spending in the short run but then raises it, with a fairly large positive long-run effect. None of the estimated effects are statistically significant. Thus, again there is no support for the starve the beast hypothesis. Another finding from the VAR is that the estimated response of the tax series to an innovation to government spending is very small and highly insignificant at all horizons. This indicates that the actions we classify as long-run tax changes are not responses to spending developments.⁸

Larger Systems. Another way that a starve the beast effect could occur at longer horizons is if tax cuts affect other variables that in turn affect government spending. We therefore consider VARs with additional variables. However, including other variables in the VAR requires either estimating more parameters in each equation or including fewer lags. Thus, rather than just including a long list of variables that might be relevant, we consider a range of possibilities.

One way that tax cuts could gradually reduce government spending is by raising the level of government debt. Thus, our first multivariable VAR is a three-variable system with our series on long-run tax changes, log real spending, and log real debt.⁹

We also consider two four-variable VARs. In one, we add the log of real federal total receipts as the fourth variable, so that the system includes both the spending and revenue sides of the government budget. In the other, the fourth variable is log real GDP. Our reason for including this variable is that tax

⁷ Note that the experiment being considered here is slightly different from the experiment that we consider in summarizing the results from the baseline specification. There, we consider a one-time tax cut of one percent of GDP with no further tax changes. Here, following the innovation to our tax measure in the VAR, there are on average additional long-run tax cuts of about one-fifth of a percent of GDP over the next several years. We compute the standard errors by taking 10,000 draws of the vector of coefficient estimates from a multivariate normal distribution with mean and variance-covariance matrix given by the point estimates and variance-covariance matrix of the coefficient estimates, and then finding the standard deviation of the implied responses at each horizon.

⁸ We have also estimated the bivariate VAR with twenty lags for the period 1952Q1–2007Q4. The estimated effects of a tax cut on spending in this specification are even more consistently positive, and are marginally significant. The maximum effect is an increase of 3.97 percent after 18 quarters ($t = 1.93$).

⁹ From 1970Q1 to the end of the sample, we use quarterly data on the stock of federal debt held by the public. From the beginning of the sample to 1969Q4, we use the available series on gross federal debt held by the public for the second quarter of each year, and interpolate linearly between the annual observations. Both series are from the St. Louis Federal Reserve Bank's FRED database, series FYGFDPUN and FYGFDPUB (www.stls.frb.org, downloaded 3/24/08). We ratio splice the two series in 1970Q2, and deflate the resulting series by the price index for GDP. Note that since it is likely to be the level of debt that affects spending, the errors caused by the interpolation in the first part of the sample should have only minor effects on the estimates.

cuts have large short-run effects on output (Romer and Romer, 2009), and these output effects could have an important effect on the dynamics of spending in response to a tax cut.¹⁰

Finally, the nominal interest rate and inflation also enter the government budget constraint. Our last system is therefore a seven-variable VAR: our long-run tax series, log real spending, log real debt, log real revenues, log real GDP, the three-month Treasury bill rate, and the log of the price index for GDP.¹¹ In all of the VARs, we put the tax series first, so that it can affect the other variables within the quarter. We include twelve lags and use the full sample (1950Q1–2007Q4).

Figure 4 displays the response of government spending to an innovation of minus one percent of GDP to our series on long-run tax changes in each of the VARs.¹² The results consistently fail to support the starve the beast hypothesis. In every specification, there are only a few horizons at which the estimated effect of a tax cut on spending is negative. And in every case, those estimates are small and insignificant: there is never a single horizon where the t-statistic for the starve the beast effect is greater than -1 . Adding debt to the baseline VAR (panel a) in fact moves the estimates further in the direction of suggesting fiscal illusion. The estimated maximum effect of the tax cut is an increase in spending of 5.75 percent ($t = 2.12$) after seventeen quarters, and the estimated effect after ten years is an increase of 3.93 percent ($t = 1.70$). In the larger systems, the point estimates suggest a slightly weaker fiscal illusion effect, although it is more precisely estimated than in the two-variable VAR. In all three of the larger systems, the estimated maximum effect is an increase in spending between 3.6 and 3.9 percent after about four years (neglecting the spike to 4.6 percent after seven quarters in the seven-variable system). In the four-variable VAR with receipts (panel b), the effect is not significant ($t = 1.73$), but in the other two it is: the t-statistic for the maximum effect is 2.51 in the four-variable VAR with GDP (panel c), and 2.49 in

¹⁰ The specific receipts measure we use is the federal total receipts series from the National Income and Product Accounts, Table 3.2 (downloaded 4/6/09), deflated by the price index for GDP. Our real GDP series is the quantity index for GDP from NIPA Table 1.1.3 (downloaded 2/17/08).

¹¹ Data on the three-month Treasury bill rate are from the Board of Governors, series H15/H15/RIFSGFSM03_N.M (www.bog.gov, data for 2/15/08).

¹² In each VAR, following the innovation to the tax series, there are modest additional long-run tax cuts over the next year that are largely offset over the following few years. There is never an important response of the tax variable to the other variables.

the seven-variable VAR (panel d). Finally, in all three of these specifications, the estimated effect after ten years is in the direction predicted by fiscal illusion, but small and not significant.

D. Other Robustness Checks

The next step is to examine the robustness of the findings along other dimensions. The most important of these checks are summarized in Figure 5, which shows the implied response of total expenditures to a long-run tax cut of one percent of GDP for a number of variants of the baseline regression, equation (1). For comparison, panel (a) of the figure repeats the baseline estimates from Figure 2.

Sample Period and Outliers. One obvious concern is the possible importance of the sample period and outliers. As described above, fiscal policy was very unusual in the Korean War period. Panel (b) shows that considering the post-Korea sample weakens the evidence for a perverse effect of tax cuts on spending, but still yields no evidence of a starve the beast effect. The change in the sample makes the initial negative impact even smaller and more insignificant. The response in quarters 3 through 20 is always positive, but considerably smaller than for the full sample and not even marginally significant. To check more generally for the possible influence of outliers, we consider the effects of excluding each of the four large long-run tax cuts discussed in the case studies.¹³ In all four cases, the estimated effect of a tax cut on spending remains mainly positive and is never close to significantly negative at any horizon. Dropping the 1948 tax cut, however, renders the positive effect of tax cuts on spending small and insignificant.¹⁴

Military Actions. A second concern is the role of military actions. As discussed in the case studies, many of the largest long-run tax cuts were followed by wars. Such military actions could have

¹³ To exclude a tax cut, we set our series for long-run tax changes to zero from the first to the last quarter in which the bill changed taxes. We treat the 2001 and 2003 cuts as a single measure; thus in this case, we set our series to zero from 2002Q1 to 2005Q1.

¹⁴ A related exercise along these lines is to split the sample in 1980Q4. For the period 1950Q1–1980Q4, the estimates suggest a large and statistically significant positive effect of tax cuts on spending. For the period 1981Q1–2007Q4, the estimated effects are again virtually always positive, but consistently small and far from significant.

caused federal spending to rise after tax changes just by chance, and thus may have obscured a starve the beast effect that is actually present. To test for this possibility, we consider two alternative specifications of our baseline regression.

One alternative is to add an indicator of military actions to equation (1). Ramey (2008) suggests an updated list of the exogenous military actions identified by Ramey and Shapiro (1998) from narrative sources. This list dates the beginning of military actions in 1950Q3 (Korean War), 1965Q1 (Vietnam War), 1980Q1 (Carter-Reagan military build-up in response to the Soviet invasion of Afghanistan), and 2001Q3 (the wars in Afghanistan and Iraq following the September 11 terrorist attacks). We expand the baseline regression to include the contemporaneous value and twenty lags of a dummy variable equal to one in each of these four quarters. This specification shows the effect of tax cuts on total expenditures, allowing for the possibility that wars have a separate effect on spending.

The cumulative impact of a tax cut of one percent of GDP in this specification is shown in panel (c) of Figure 5. The estimates are very similar to those in the baseline specification. The effects of tax cuts on total spending controlling for military actions are largely positive, though not statistically significant. Thus, accounting for military actions does not reveal a starve the beast relationship. This is true even though wars exert a strong independent upward force on spending: the maximum cumulative impact of a military action on total expenditures is an increase of 15.83 percent ($t = 2.77$). The lack of a relationship between taxes and spending in this alternative specification is equally strong in the post-1957 sample.

A second alternative specification is to look only at the response of nondefense spending to long-run tax cuts. In particular, in place of our measure of the change in total federal expenditures in equation (1), we use the log difference in total expenditures less national defense purchases (deflated by the price index for GDP).¹⁵ This test will almost surely bias the results in favor of the starve the beast hypothesis, for two reasons. First, the case studies show that there is some correlation in our sample between

¹⁵ Data on national defense purchases are from the National Income and Product Accounts, Table 3.9.5 (downloaded 3/25/08).

supporters of tax cuts and supporters of shifting spending toward defense. Most notably, Ronald Reagan, who presided over the largest long-run tax cut in the postwar period, strongly advocated such a reallocation. Thus, we could see nondefense spending fall in the wake of long-run tax cuts not because of the effects of the cuts themselves, but because of other actions. Second, to the degree that defense spending rises following tax cuts because of wars, nondefense spending may decline for the same reason. Wartime tends to naturally lead policymakers to rearrange spending away from other purposes and toward defense. Therefore, chance correlation between wars and long-run tax cuts could cause the regression to find a starve the beast effect for nondefense spending when none actually exists.

The results of this exercise are shown in panel (d) of Figure 5.¹⁶ The point estimates are now generally negative, consistent with the starve the beast hypothesis. The effects are not statistically significant, however: the t-statistics for the cumulative impact are almost always less than -1 , and never greater than -1.3 . More importantly, the estimates are small and not robust. Total expenditures less defense account, on average, for about ten percent of GDP over our sample. Therefore, for a tax cut of one percent of GDP to reduce spending by the same amount, spending would need to decline by roughly ten percent. The estimated effect, however, is almost always a fall of less than four percent. And dropping the Reagan tax cut (where, as described above, there appears to have been an important omitted factor acting to directly reduce nondefense spending) yields estimates that fluctuate irregularly around zero. Similarly, either excluding the Korean War period or including the contemporaneous value and twenty lags of the dummy variable for military actions weakens the estimated effect considerably. Thus, there is little evidence that tax cuts have a noticeable negative effect even on nondefense spending.

Political Variables. A third robustness issue concerns the role of political variables. It is certainly possible that the party of the president or the existence of unified government has an influence on government spending. If such variables are correlated with our tax measure, the baseline regression could suffer from omitted variable bias. For this reason, we try adding a variety of political variables to

¹⁶ This panel is on a different scale than the others in Figure 5 because the dependent variable is now a percent of total expenditures less defense, rather than a percent of total expenditures.

our baseline specification. To give one example, panel (e) shows the effect of a tax cut on spending when a dummy variable for Democratic administrations is included in the regression. This regression asks whether tax cuts lower spending, taking into account that Democratic presidents may consistently spend more or less than their Republican counterparts. Adding this variable has very little effect on the estimates, although it strengthens the evidence for fiscal illusion or shared fiscal irresponsibility slightly. Both the estimated positive effects of tax cuts on spending and their statistical significance increase modestly. We also consider specifications including a dummy variable for whether the presidency and both houses of Congress are controlled by the same party, and including separate dummies for whether it is the first quarter of a new Republican or a new Democratic administration.¹⁷ Both specifications change the estimates only trivially, and neither provides support for the starve the beast hypothesis.

Alternative Tax Variable. A fourth concern involves the specification of our tax variable. Our baseline series dates revenue changes in the quarter that liabilities actually change. An alternative measure, which emphasizes expectational effects, calculates the present discounted value of all revenue changes called for by a given piece of legislation, and dates the revenue change in the quarter the law was passed.¹⁸ Panel (f) of Figure 5 shows that the starve the beast hypothesis fares even worse when this alternative tax measure is used. The estimated impact of a tax cut on spending is consistently in the opposite direction from the predictions of the starve the beast hypothesis, often quantitatively large, and sometimes marginally significant.

Alternative Spending Concepts. In our baseline specification, we use a NIPA measure of spending, on the grounds that it is available quarterly and is likely to correspond most closely with economic concepts of government spending. A natural alternative is to use the official budget numbers, which may be more closely tied to policymakers' actions. To do this, we aggregate our measure of long-run tax changes to construct a fiscal year measure, and then reestimate (1) using the change in the logarithm of real expenditures measured using the budget-based measure and the contemporaneous value

¹⁷ For the specification controlling for changes in the party of the president, we include both the contemporaneous value and fifteen lags of the new Republican and new Democratic dummy variables.

¹⁸ See Romer and Romer (2008) for a detailed description of how we calculate the present value of revenue changes.

and five annual lags of our measure of long-run tax changes.

For there to be a substantial starve the beast effect, tax cuts would almost certainly have to reduce not just discretionary spending, but also spending on entitlement programs. At the same time, because policymakers can change discretionary spending more quickly, it is interesting to ask whether there is a starve the beast effect for this type of spending. We therefore also examine the response of discretionary spending to long-run tax cuts, again using annual budget data and five annual lags of our tax measure.¹⁹

The results are shown in panels (g) and (h) of Figure 5. Once again, there is no support for the starve the beast hypothesis. The response of overall spending using the budgetary measure in panel (g) is quite similar to the response using the NIPA measure in panel (a). And, discretionary spending rises even more than overall spending following a tax cut, with a maximum increase of 11 percent after four years ($t = 2.23$).²⁰

Alternative Specifications of the Spending Variable. A final robustness issue involves the appropriate way to enter the spending variable. In all of the specifications discussed so far, we have looked at the response of the growth rate of real government expenditures to long-run tax changes. The cumulative impact therefore shows the effect of a tax change on the level of real expenditures relative to normal. We feel this is the appropriate measure for testing the starve the beast hypothesis: Does a tax cut change the spending decisions of policymakers? However, an alternative form of the hypothesis could be that a tax cut reduces expenditures as a percent of GDP. In this view, a tax cut could lower the share of spending in GDP not by changing policymakers' spending decisions, but by changing output growth.

To test this alternative version of the hypothesis, we re-estimate equation (1) using two different

¹⁹ The budget data are from the Budget of the United States Government: Historical Tables Fiscal Year 2009 (<http://www.gpoaccess.gov/usbudget/fy09/hist.html>, Tables 3.1 and 8.1, downloaded 3/16/09). Discretionary spending figures are only available beginning in 1962. For the years up through 1962, we estimate the growth rate of discretionary spending as the change in the log of total spending minus the sum of Social Security, income security, veterans benefits and services, agriculture, commerce and housing credit, net interest, and undistributed offsetting receipts. The estimates constructed in this way track the official estimates for the years immediately after 1962 quite well. In aggregating our measure of long-run tax changes to fiscal year values, we omit the transition quarter (1976Q3). We deflate both the overall spending measure and the discretionary measure by the price index for GDP.

²⁰ Note that panel (h), like panel (d), is on a different scale than the other panels.

specifications of the dependent variable. The more sensible of the two expresses real total expenditures as a percent of trend real GDP, where trend real GDP is calculated using a conventional Hodrick-Prescott filter, and then uses the change in this variable as the dependent variable in equation (1).²¹ Detrending real GDP is reasonable because, to the extent that a tax cut causes a temporary boom, it will inherently tend to reduce real expenditures as a percent of actual GDP in the short run. We do not believe that this is the mechanism proponents of even the alternative form of the starve the beast hypothesis have in mind. However, as a further robustness check, we also estimate equation (1) using the change in the ratio of total real expenditures to actual real GDP.

The results of these two exercises are shown in panels (i) and (j) of Figure 5.²² Panel (i) shows that the results using the change in spending as a share of trend GDP are very similar to the results using the percentage change in spending. A tax cut of one percent of GDP for the most part raises the share of spending in GDP. The estimated maximum effect is large (0.94 percent of GDP), but only marginally significant ($t = 1.92$). Thus, the results again fail to support the starve the beast hypothesis, and provide moderate support for the alternative view of fiscal illusion or shared fiscal irresponsibility.

Panel (j) shows that a tax cut does not even reduce spending as a share of actual GDP. The estimated effects fluctuate irregularly around zero. The estimates suggest a marginally significant starve the beast effect in a single quarter (quarter 9), but they are more often positive than negative, and the estimated long-run effect is positive, small, and very far from significant. That this second specification fails to support the starve the beast hypothesis is quite surprising. As discussed in Romer and Romer (2009), the short-run stimulatory effects of tax cuts on output are very strong. Yet even this rapid growth of output is not enough to generate a systematic fall in expenditures as a share of GDP.

The robustness checks yield two conclusions. First, and most important, the lack of support for the starve the beast hypothesis is very robust: with the possible exception of the examination of

²¹ We again calculate real expenditures by dividing nominal expenditures by the price index for GDP. Real GDP is constructed by dividing nominal GDP by the same price index. We fit a Hodrick-Prescott filter ($\lambda = 1600$) to log real GDP for the full sample (1947Q1–2007Q4).

²² These two panels are on a different scale than the others in Figure 5 because the dependent variable is now a percent of GDP, not a percent of total expenditures.

nondefense spending, which appears to be biased in favor of the starve the beast hypothesis and for which the results are mixed, none of the specifications we consider provide evidence that tax cuts reduce government expenditures. Second, the evidence for the alternative view of fiscal illusion or shared fiscal irresponsibility is only modest. The point estimates consistently suggest that tax cuts raise government expenditures, but they are only occasionally significantly different from zero, and then usually only marginally so.

E. The Relationship between Other Types of Tax Changes and Total Expenditures

As discussed above, we focus on the response of government spending to long-run tax changes because this is likely to provide the least biased test of the starve the beast hypothesis. Nevertheless, it is interesting to look at the behavior of spending following the other types of tax changes we have identified: deficit-driven, countercyclical, and spending-driven actions. This analysis can reveal if the feared biases from using these other types of tax changes to estimate the response of spending appear to be present. It can also provide an indirect check on our classification procedures. For example, if we have classified spending-driven tax changes correctly, they should be positively correlated with spending changes.

For this exercise, we estimate equation (1) using the contemporaneous value and twenty lags of a particular type of tax change as the independent variable. We estimate a separate regression for each type of tax change using the full postwar sample period. The results are again summarized by calculating the implied cumulative response of spending to a tax cut (of a given type) of one percent of GDP. These estimated effects for the three additional types of tax actions are given in Figure 6.²³ To facilitate comparisons, panel (a) of the figure repeats the results for long-run tax actions from Figure 2.

Deficit-Driven Tax Changes. Of the three additional types of tax changes, deficit-driven actions

²³ This way of summarizing the estimates is slightly less intuitive for deficit-driven and spending-driven tax changes than for our baseline case of long-run changes because deficit- and spending-driven tax changes are almost always tax increases. Nevertheless, the interpretation is the same as before: a negative response of spending to a tax cut is supportive of the starve the beast hypothesis; a positive response or no response is not.

are likely to be the most informative about the starve the beast hypothesis. Like long-run changes, these actions are not taken in response to current or prospective short-run macroeconomic conditions or because spending is moving in the same direction. The reason for excluding these changes from the baseline regression was that deficit-driven tax increases are often parts of budget packages that include spending reductions. These observations might therefore bias the results against the starve the beast hypothesis. The estimated impact of deficit-driven tax changes on total expenditures given in panel (b) shows this fear is somewhat justified. In the quarter of a deficit-driven tax cut and the subsequent two quarters, spending rises substantially. Or, to think about the more realistic case, following a deficit-driven tax increase, spending falls substantially. This is exactly the sort of inverse relationship one would expect if deficit-reduction packages were common. The effects, while large, are not precisely estimated. The t-statistic for the maximum impact is 1.98.

After the first few quarters, the estimated effects of a deficit-driven tax cut turn negative for several years, but return to being positive at distant horizons. None of these estimates are close to statistically significant, however. These results suggest that any spending cuts agreed to at the time of a deficit-driven tax increase disappear within the first year. The lack of a consistent pattern to the estimates at longer horizons suggests little ultimate impact of tax changes on expenditure. In this way, the results for deficit-driven tax changes echo those for long-run actions, and do not support the starve the beast hypothesis.

Countercyclical Tax Changes. Panel (c) shows the implied impact on spending of a countercyclical tax cut. We exclude such tax changes from our baseline regression because the state of the economy could tend to influence spending and taxes in opposite directions, and so again bias the estimates against the starve the beast hypothesis. The results suggest that this is somewhat the case. A countercyclical tax cut is associated with a persistent rise in spending. However, the standard errors are quite large, so it is impossible to reject the hypothesis of no relationship.

Spending-Driven Tax Changes. Panel (d) shows the behavior of government spending following a spending-driven tax cut. In this case, the relationship is negative, large in absolute terms, and

highly statistically significant.²⁴ This is exactly the result one would expect: if we classified spending-driven tax changes correctly, there should be a positive correlation between such actions and spending. That the relationship persists is consistent with the spending changes associated with these spending-driven actions being permanent. The findings for spending-driven tax changes both confirm our classification of these tax actions and illustrate the importance of controlling for motivation when testing the starve the beast hypothesis. Including spending-driven actions would clearly bias the results toward finding a positive correlation between spending changes and tax changes.

All Legislated Tax Changes. One way to see how much bias including spending-driven actions would cause is to define a tax variable that sums all four types of legislated tax changes and then use this as the explanatory variable in equation (1). Panel (e) of Figure 6 shows the implied impact of a legislated tax cut of any motivation of one percent of GDP on total expenditures. The estimated response is strongly negative, and often statistically significant, for the first three years after a tax cut. The point estimate for the maximum cumulative effect is -3.82 percent ($t = -2.41$). Since none of the other types of tax changes show a consistent negative response, this implied negative effect of the aggregate tax variable reflects the influence of the spending-driven tax changes.

To test this proposition more directly, we define a second composite tax variable that includes all legislated tax changes other than those motivated by spending changes. Panel (f) shows the implied cumulative response of total expenditures to a non-spending-driven legislated tax change of one percent of GDP. The effects are consistently positive, suggesting that, if anything, tax cuts appear to be followed by increases in government spending, not decreases as the starve the beast hypothesis predicts. And, for horizons beyond three years, these positive effects are significantly different from zero. The results suggest that the inclusion of spending-driven tax changes may explain why much of the previous literature has found evidence for the starve the beast hypothesis.

The Change in Cyclically Adjusted Revenues. This possibility can be investigated by

²⁴ These findings are somewhat sensitive to the sample period. Some of the largest spending-driven tax changes were during the Korean War. When the post-1957 sample period is used, the maximum impact of a spending-driven tax cut of one percent of GDP is large (-6.65 percent), but not significant ($t = -1.60$).

considering a more standard measure of tax changes. A typical test of the starve the beast hypothesis uses the change in cyclically adjusted revenues, which includes all changes in revenues not related to short-run fluctuations in income, as the measure of tax changes. Data on the change in cyclically adjusted revenues are available beginning in 1947Q2. We therefore investigate the effects of using the contemporaneous value and eleven lags of this variable as the tax measure for the period 1950Q1–2007Q4.²⁵ When we use this conventional tax variable, the results indeed seem to support the starve the beast hypothesis. Panel (g) of Figure 6 shows that the estimated cumulative effect of a decline in real cyclically adjusted revenues of one percent of GDP starts out positive, but then turns negative. The maximum impact is a change in government expenditures of -2.94 percent ($t = -2.04$).

Given that we have identified spending-driven tax changes, it is possible to subtract these actions from the change in cyclically adjusted revenues.²⁶ The results using this series, which are shown in panel (h), are dramatically different from those using all cyclically adjusted revenues. The estimated impact of a decline in cyclically adjusted revenues less spending driven changes of one percent of GDP is now strongly positive in the short run: the maximum impact is 3.63 percent ($t = 4.56$). It then gradually declines toward zero, but never turns negative over the eleven-quarter horizon we consider. Thus, the results provide no support for the starve the beast hypothesis, and, indeed, are somewhat supportive of shared fiscal irresponsibility. This suggests that the inclusion of spending-driven changes in conventional revenue measures is an important source of the finding that government spending moves in the same direction as tax revenues.²⁷

²⁵ For comparability with our tax measure, we use the change in real cyclically adjusted revenues as a percent of real GDP. See Romer and Romer (2009) for a more detailed discussion of the sources and derivation of this measure.

²⁶ Since both series are expressed as a percent of GDP, the spending-driven tax changes can be subtracted without further adjustment.

²⁷ The importance of spending-driven tax changes in causing bias is sensitive to the sample period used. Spending-driven changes were largest during the Korean War and tend to cause substantial bias in samples that include this period. In later sample periods, spending-driven changes are smaller and so are a less important source of bias. This may explain why studies such as Ram (1988), Miller and Russek (1990), and Bohn (1991), which use data from the Korean War period and before, find support for the starve the beast hypothesis, while those such as von Furstenberg, Green, and Jeong (1986), which use data starting in 1954, do not.

III. EFFECTS OF LONG-RUN TAX CHANGES ON FUTURE TAXES

Our analysis finds no evidence that tax cuts lead to reductions in government spending. This finding obviously raises another question: If tax cuts do not reduce government spending, how does the government budget adjust to the cuts? An obvious possibility is that the adjustment occurs on the tax side rather than on the expenditure side. To explore this possibility, we examine the response of both tax revenues and tax legislation to long-run tax changes.²⁸

A. Response of Tax Revenues

To investigate how revenues respond to long-run tax changes, we first estimate equation (1) using a measure of real tax revenues as the dependent variable. That is, we regress the percentage change in real revenues on a constant and the contemporaneous value and twenty lags of our measure of long-run tax actions. As in the VARs in Section II, we measure revenues using the federal total receipts series from the National Income and Product Accounts, deflated by the price index for GDP. We estimate the revenue response over both the full postwar sample period (1950Q1–2007Q4) and the post-Korean War sample (1957Q1–2007Q4).

Panels (a) and (b) of Figure 7 show the implied cumulative response of total receipts to a long-run tax cut of one percent of GDP in each sample period. Tax receipts decline strongly in the short run in response to a tax cut. The contemporaneous effect is a change in receipts of –1.90 percent in the full sample ($t = -2.00$) and –2.06 percent in the post-Korea sample ($t = -2.33$). Total receipts remain substantially below their pre-tax-cut path for the next year and a half.

In both samples, receipts then recover substantially. For the full sample, the rise in revenues two years after the tax cut is dramatic and marginally significant. This finding is largely driven by the Korean War. As described in Section IV, the large 1948 tax cut was followed roughly two years later by the

²⁸ Bohn (1991) also examines the degree to which deficits caused by tax cuts are eliminated by subsequent tax increases.

outbreak of the war. Three major tax increases were passed during the war, and the war was accompanied by rapid output growth. For this reason, the results for the full sample almost surely overstate the true tendency of revenue to rebound. For the post-Korea sample, receipts rise above their pre-tax-cut path seven quarters after the tax cut, but the effect is modest and the standard errors are large (the t-statistics on the positive effects do not rise above 1.00).

To investigate the response of receipts to tax shocks further, we also estimate a bivariate VAR. In particular, we estimate a system using our measure of long-run tax changes and the log of real total receipts. We include twelve lags of each series, so we can use our baseline sample period of 1950Q1–2007Q4. The response of real receipts to a long-run tax cut of one percent of GDP from this specification is shown in panel (c) of Figure 7. Receipts fall markedly following a long-run tax cut and the effects are significant, or nearly so, for the first year and a half. The effects then turn positive nine quarters after the shock. However, even though we are considering the full sample, the positive effects are extremely small in absolute terms and not statistically significant.²⁹

B. Response of Tax Legislation

To understand the behavior of revenues following a long-run tax cut, it is important to investigate the behavior of subsequent tax legislation. Do tax revenues recover because of unusually rapid growth or because policymakers legislate tax increases? Given that we have constructed measures of legislated tax changes classified by motivation, this is an issue we can investigate.

In our single-equation analyses of spending and revenues, we consider the experiment of a long-run tax cut that is not followed by any additional tax changes based on long-run considerations.

²⁹ The response of total receipts to a long-run tax cut is even more negative when the bivariate VAR includes twenty lags of each variable and is estimated over the shorter sample period 1952Q2–2007Q4. For this specification, tax revenues do not turn consistently positive until four years after the tax cut. The results from the multivariate VARs described in Section II are broadly similar to those from the bivariate VAR. For example, in the four-variable VAR including our measure of long-run tax changes, government expenditures, debt, and tax receipts, the effect of a long-run tax change of one percent of GDP on receipts is negative for the contemporaneous quarter and the six quarters after the shock, and then turns positive. The positive effects are somewhat larger than in the bivariate VAR, but still small in absolute terms and not significant.

Therefore, it does not make sense to ask how long-run tax changes respond to this experiment. But, it is reasonable to ask how other types of legislated tax changes respond to a long-run tax cut. Long-run tax cuts that do not lower spending, and so increase the deficit, might lead to tax increases designed to reduce an inherited budget deficit. Likewise, a long-run tax cut that gives rise to a short-run boom could lead to a countercyclical tax increase. A long-run tax cut could also lead policymakers to switch to a “pay-as-we-go” policy: a budget deficit resulting from a long-run tax cut may make policymakers unwilling to increase spending without increasing taxes. Therefore, one could also see an increase in spending-driven tax increases following long-run tax cuts.

Our basic empirical framework is again identical to that in equation (1), except that the dependent variable is now a measure of legislated tax changes. That is, we regress legislated tax changes of some motivation on a constant and the contemporaneous and lagged values of our measure of long-run tax changes. In our baseline specification we again use twenty lags. However, we experiment with longer lags. We estimate the responses over both the full postwar sample and the post-Korean War sample. As before, we summarize the results by examining the cumulative impact of a long-run tax cut of one percent of GDP. A positive impact implies that subsequent tax actions counteract the long-run tax cut. Because the other tax variables are also expressed as a percent of nominal GDP, the cumulative impact can be interpreted as the fraction of the long-run tax cut that is undone over the horizon considered.

The estimated impacts of a long-run tax cut of one percent of GDP on tax changes of various types are shown in Figure 8. Panel (a) shows the impact on deficit-driven actions. The effect is positive and highly statistically significant, suggesting that long-run tax cuts tend to be followed by deficit-driven tax increases. The cumulative impact is 0.23 percentage points ($t = 3.06$) after eight quarters, and 0.24 ($t = 2.39$) after sixteen.³⁰ This suggests that about a fifth of a long-run tax cut is undone by deficit-driven

³⁰ There is a substantial contemporaneous impact (0.11 with a t -statistic of 3.73). The most important observation behind this estimate is 1983Q1. A large part of the tax cuts in the Economic Recovery Tax Act of 1981 were scheduled to go into effect in 1983Q1. Concern about current and prospective deficits, however, led to passage of the Tax Equity and Fiscal Responsibility Act of 1982, which raised revenues mainly by modifying some features of the 1981 act that had already taken effect (Romer and Romer, 2008). Thus, although the long-run tax cut and the

tax increases within a few years. These results are highly robust. Starting the sample in 1957 has virtually no impact. And, increasing the number of lags to forty and carrying out the simulations for ten years strengthens the results. Ten years after the long-run tax action, 44 percent of the action has been undone by deficit-driven tax increases ($t = 2.53$).

Panel (b) shows the impact of a long-run tax cut on countercyclical tax actions. The estimated impact is moderate, but not close to significantly different from zero. After twenty quarters, countercyclical tax actions have counteracted 18 percent of a long-run tax cut ($t = 0.57$). Starting the sample in 1957 has no virtually impact because there were no countercyclical tax actions in the early 1950s. Including more lags suggests that the response diminishes at longer horizons. The estimated effect after ten years is 0.11 percentage points ($t = 0.21$).³¹

Panel (c) shows the impact of a long-run tax cut on spending-driven tax changes. In this case, the effects are virtually zero for the first nine quarters after the long-run tax cut, and then turn strongly positive. The maximum cumulative impact is 0.47 percentage points ($t = 2.53$) after fourteen quarters. The impact after twenty quarters is 0.36 ($t = 1.58$). This suggests that spending-driven tax increases occur after a long-run tax cut, and that they counteract close to half of the initial cut. Thus, long-run tax cuts may indeed give rise to more pay-as-we-go policies.

More so than with the other tax changes, there is reason to be concerned that the results for spending-driven actions are influenced by the observations from the Korean War. Starting the sample in 1957 does indeed weaken the link substantially. The strongest impact of a long-run tax cut is now a rise in spending-driven taxes of 0.14 percentage points after eight quarters ($t = 2.03$). Likewise, including forty lags reduces the impact substantially for the full sample, but this effect is due entirely to the required shortening of the sample period.

deficit-driven tax increase occurred simultaneously, there is a clear sense in which the deficit-driven increase was a response to the long-run cut.

³¹ We also experimented with leaving out the 1975 tax rebate, which is a huge outlier among countercyclical actions. It is peculiar because it mainly cut taxes dramatically in one quarter and then raised them dramatically in the next. Zeroing out this action reduces the response at medium horizons, but has almost no effect on the longer-run response. The main effect is to cut the standard errors by more than half.

Panel (d) shows the effect of a long-run tax cut on the other types of legislated tax changes combined. The effect is positive, large, and significant. The effect is 0.61 ($t = 2.08$) after twelve quarters, 0.81 ($t = 2.34$) after sixteen, and 0.74 ($t = 1.92$) after twenty. This suggests that roughly three-quarters of a long-run tax cut is typically undone by legislated tax increases of various sorts within five years.

Figure 9 shows three robustness checks for the effect of a long-run tax cut on this composite of other tax changes. Panel (a) shows the impact of starting the sample in 1957. Both the maximum impact and the statistical significance are somewhat reduced by this change. The impact now peaks at 0.60 percentage points ($t = 1.66$) after nineteen quarters. Panel (b) shows the effect of including forty lags of long-run tax changes. The required shortening of the sample reduces the estimated responses over the first twenty quarters somewhat. Thereafter, it moves irregularly upward. The response after 40 quarters is large (0.77), but not precisely estimated ($t = 1.39$). Though they weaken the evidence slightly, these two robustness checks suggest that a large fraction of a long-run tax cut is typically reversed by legislated tax increases within the next few years.

Our final robustness check allows for more complicated dynamics. To do this, we estimate a bivariate VAR including our measure of long-run tax changes and the composite measure of the three other types of legislated tax changes. We include twelve lags of each series and estimate it over our baseline sample period of 1950Q1–2007Q4.³²

The response of other legislated tax changes to a long-run tax cut of one percent of GDP in this specification is shown in panel (c) of Figure 9. The results are again very similar to those from the single-equation specification. The response of other tax changes is strongly positive: the maximum effect is 0.78 percentage points ($t = 2.22$) eighteen quarters after the shock. The effect diminishes slightly thereafter, but levels off at around 0.65. Thus, the VAR specification confirms that long-run tax cuts are

³² The experiment we can consider in this framework is again slightly different from that in the single-equation specification. When we look at the effect of an innovation to long-run tax changes in the VAR specification, we are no longer assuming that that the tax change is not followed by other long-run tax changes. Rather, we let the data say how long-run tax changes respond to the innovation. The cumulative response of long-run tax changes to a long-run tax cut of one percent of GDP levels off at around -1.2 . This suggests that a long-run tax change is typically followed by subsequent long-run tax changes in the same direction. This is consistent with the fact that many long-run tax changes are legislated to take effect in a series of steps.

substantially counteracted by other types of tax increases over the next several years.

C. Discussion

The fact that policymakers have been able to largely reverse tax cuts helps to explain why the cuts have not reduced spending.³³ To see this connection, note that there are two possible ways that a tax cut could reduce future spending. The first is through debt: by bequeathing greater debt to future policymakers, current policymakers restrict future policymakers' choice sets, which is likely to lead to some combination of higher taxes and lower spending. This is the mechanism emphasized in standard models of strategic budget deficits (for example, Tabellini and Alesina, 1990, and Persson and Svensson, 1989). The second is by leaving future policymakers with lower taxes. If increasing taxes is costly, this further reduces spending. This mechanism appears important in informal discussions of the starve the beast effect (see, for example, the quote from Ronald Reagan at the beginning of the paper, which seems to suggest a permanent reduction in government revenue).

If the costs of reversing a tax cut are small relative to the costs of cutting spending, then only the first channel is relevant. And, that channel is likely to be quantitatively small. Suppose, for example, a policymaker cuts taxes by two percent of GDP for five years. The result is a deficit that is higher than it otherwise would have been by about two percent of GDP for five years, and thus a stock of debt that is higher by about ten percent of GDP after five years. If the difference between the real interest rate and the economy's growth rate is two percentage points, then the interest costs associated with maintaining the debt-to-GDP ratio at its higher level are about 0.2 percent of GDP (2 percent times 10 percent). Thus, policymakers can keep the tax cut from raising the debt-to-GDP ratio further by first undoing the tax cut and then enacting a permanent spending reduction of 0.1 percent of GDP and an additional permanent tax increase of 0.1 percent of GDP. This corresponds to a spending reduction of about 0.5 percent—that is, only a quite small starve the beast effect.

If, however, undoing the tax cut is difficult, the effect is much stronger. In the extreme case

³³ We are grateful to Steven Davis for this point.

where none of the tax cut can be reversed, satisfying the government budget constraint requires a spending cut equal to the tax cut (or by even more if there is a delay between the tax cut and the spending reduction, so that the amount of debt increases). The result is a spending reduction of about 10 percent.

Our results about the behavior of tax legislation following tax cuts suggest that the truth is closer to the first case than to the second. This suggests a critical reason for our failure to find a substantial starve the beast effect: adjustment on the tax side, although presumably not costless, appears feasible, making large adjustment on the spending side unnecessary.

We also find that the overall revenue rebound exceeds the portion due to legislated changes. That is, at least some of the rebound in revenues is the result of non-legislated changes. The key source of the non-legislated changes in revenues is almost certainly the effect of the tax cut on economic activity. In Romer and Romer (2009), we find that a tax cut of one percent of GDP increases real output by approximately three percent over the next three years. Since revenues are a function of income, this growth raises revenues.

There is, however, an important caveat to this finding that tax cuts partially pay for themselves through more rapid growth: some of the output response is almost surely a transitory departure of output from normal, not a permanent change in the economy's normal level of output. To the extent that this is the case, some of the rebound in revenues is also temporary. As a result, in the absence of further legislated changes, there may be some long-run budgetary shortfall in the wake of the tax cut.

Because of these complications, our results do not allow us to describe with complete confidence how the government budget constraint adjusts following a tax cut. But, we can say that we find no evidence of adjustment on the spending side, and considerable evidence of substantial adjustment on the tax side.

IV. SPENDING AND TAXES IN FOUR KEY EPISODES

In this section, we examine the four episodes in our sample that stand out as having the largest long-run tax cuts. This examination serves several purposes. The first is to see whether the narrative record suggests that the tax cuts affected spending decisions. We examine the reasoning policymakers gave for their spending behavior, and so check whether tax cuts appear to have had an important effect on the decision-making process. To keep the narrative analysis manageable, we focus primarily on presidential documents and statements.³⁴ However, in cases where Congressional views appear to be central or at odds with those of the executive branch, we also examine Congressional documents.

The second purpose is to check whether the regression results reflect consistent patterns in the data. Specifically, we look at the behavior of overall spending and its two broad components, defense purchases and nondefense spending, in each episode. This allows us to investigate whether the relationships shown by the regressions appear in the key episodes.

Our third purpose is to examine whether there are omitted variables or idiosyncratic shocks that account for the failure of spending to fall. We ask whether there were unusual developments in the episodes that had important impacts on spending. This analysis can suggest whether the regression results may be overstating (or understating) the evidence against the starve the beast hypothesis.

The final purpose is to address a similar set of issues concerning the tax side of the episodes. We look at what tax actions were taken following the tax cuts, and thus again check whether the regression results reflect consistent patterns. Perhaps more importantly, we examine the reasons policymakers gave for those actions to see to what extent they appear to have been responses to the cuts. As with spending, we also check whether idiosyncratic factors were an important determinant of tax changes in the episodes.

³⁴ The key presidential documents that we use are the *Budget of the United States Government* (abbreviated as *Budget* in citations) and the *Economic Report of the President* (abbreviated as *Economic Report*). Presidential speeches are identified by their title and date as given in John Woolley and Gerhard Peters, *The American Presidency Project* (www.presidency.ucsb.edu).

A. The Revenue Act of 1948

The Revenue Act of 1948 was passed over Harry Truman's veto in April 1948. The bill reduced revenues by 1.9 percent of GDP beginning in 1948Q2. The primary motivation for the cut was a desire to improve economic efficiency by reducing marginal tax rates.³⁵

The tax cut was followed by a substantial reduction in revenues. Truman's view, however, was that government spending should be determined by considerations other than the level of revenues, and that tax policy should be adjusted accordingly. The 1950 *Economic Report* provided a clear statement of this belief:

In fields such as resource development, education, health, and social security, Government programs are essential elements of our economic strength. If we cut these programs below the requirements of an expanding economy, we should be weakening some of the most important factors which promote that expansion. Furthermore, we must maintain our programs for national security and international peace. ...

Government revenue policy should take into account both the needs of sound Government finance and the needs of an expanding economy (p. 8).

Consistent with this view, Truman's main response to the tax cut was to propose a counteracting tax increase. He argued, "In a period of high prosperity it is not sound public policy for the Government to operate at a deficit. ... I am, therefore, recommending new tax legislation to raise revenues by 4 billion dollars" (1950 *Budget*, p. M5). This increase would have offset 80 percent of the 1948 cut.

Nonetheless, the fall in revenues appears to have had a marginal effect on Truman's spending policies. In the 1949 *Midyear Economic Report of the President*, he explained, "When I submitted my budget for the fiscal year 1950 last January, the programs of expenditure that I then recommended were held to a minimum consistent with our basic needs in view of the inflationary strain upon materials and manpower then prevailing" (p. 7). Since Truman viewed the budget deficit as contributing to inflationary pressures (for example, Annual Message to the Congress on the State of the Union, 1/5/49, p. 3), this points to at least some effect of the tax cut on spending decisions.

On June 25, 1950, North Korea invaded South Korea. Once that occurred, taxes and the deficit

³⁵ Our descriptions in this section of the motivations for tax changes and our figures for their revenue effects are based on Romer and Romer (2008). The revenue estimates exclude the effects of retroactive features of the bills.

essentially disappeared from Truman's discussions of spending. Even more so than in peacetime, his view was that spending should be determined by the country's needs, and taxes adjusted accordingly. For example, in his budget message in January 1951, Truman described the spending side of the budget and then stated, "I shall shortly recommend an increase in tax revenues in the conviction that we must attain a balanced budget to provide a sound financial basis for what may be an extended period of very high defense expenditures" (1952 *Budget*, p. M6).

Finally, although Congress's view of the tax cut was obviously very different from Truman's, Congress does not appear to have sought lower spending than the president. For example, in August 1948, Truman reported that although Congress had not appropriated the full amount he had requested for fiscal 1948 and 1949, this shortfall was offset by two factors: some spending had been authorized but not yet appropriated, and several pieces of legislation had been enacted that would require higher spending, but no spending had yet been authorized. As a result, he expected spending in fiscal 1949 to be significantly higher than what he had requested in January (Statement by the President: The Midyear Review of the Budget, 8/15/48, p. 3). Thus, there is no evidence of a starve the beast effect operating through Congressional actions.

Panel (a) of Figure 10 shows the behavior of real government spending in this episode. It plots the logarithms of our measure of total expenditures and the two categories of spending, national defense purchases and nondefense spending. As in Section II, we define nondefense spending as the difference between our measure of total expenditures and national defense purchases; the two main components of this measure are nondefense purchases and current transfer payments. The vertical line shows the quarter in which the tax cut took effect. Several things are apparent. First, and most important, there was no discernable slowdown in overall spending or either of the components. Indeed, the growth of overall spending increased after the tax cut. Total expenditures, which had been essentially flat before the tax cut, rose by 16 percent in the two years between the cut and the start of the war.³⁶ Second, there was a substantial one-time spike in nondefense spending in 1950Q1. This rise reflected a one-time dividend

³⁶ Percentage changes are computed as changes in logarithms.

payment from the trust fund for National Service Life Insurance (the government insurance program for military personnel). These payments were the result of a large accumulation of assets in the trust fund, which could not be used for other purposes (Hines, 1943; *Survey of Current Business*, March 1950, pp. 1-3, and August 1950, p. 7). Third, there was a sharp rise both in defense spending and in overall spending after the outbreak of the war.

Both the National Service Life Insurance dividend payment and the increased military spending after the start of the war clearly reflected unusual developments, not just the normal response of spending to tax cuts. The occurrence of these idiosyncratic upward influences on spending works in the direction of causing the regressions to overstate the impact of tax cuts on subsequent spending increases.

There was also an important unusual development operating in the opposite direction. The Social Security Amendments of 1950 almost doubled Social Security benefits starting in September 1950 and substantially increased the coverage of the system beginning in January 1951 (*Social Security Bulletin*, October 1950, pp. 3-14). Because Social Security benefits were initially small, these changes had little immediate impact on overall spending. Nonetheless, the rise in the benefit base and the expansion of coverage contributed significantly to the growth of spending over time. The fact that these delayed spending effects are not captured in our regressions operates in the direction of causing the regressions to understate the impact of tax cuts on later spending increases.

On the tax side, the 1948 tax cut was followed by a series of tax increases that were largely spending-driven. The first, and least important, was an increase in Social Security taxes of 0.3 percent of GDP in 1950Q1 that had been legislated before the tax cut was passed. After that, there were larger tax actions. The Social Security Amendments of 1950 increased the base of the payroll tax from \$3000 to \$3600 effective at the beginning of 1951 and called for a gradual increase in the combined Social Security tax rate from 3 percent to 6½ percent over the next two decades (*Social Security Bulletin*, October 1950, pp. 3-14). And three bills in 1950 and 1951 to finance the Korean War increased taxes by a combined 4.2 percent of GDP.

The move to spending-driven tax increases in the early 1950s was clearly a policy decision. In

the case of Social Security, policymakers were grappling with the issue of how to finance the system. A special Congressional commission and the Social Security Administration both recommended that Social Security taxes be limited and that the system move toward increasing reliance on general revenues. Instead, however, the 1950 amendments repealed the provision of the Social Security Act that permitted financing from general revenues and made the system entirely self-financing (*Social Security Bulletin*, May 1948, pp. 21-28; February 1949, pp. 3-9; October 1950, pp. 3-14). However, there is no direct evidence that the 1948 tax cut played a causal role in this decision.

In the case of the Korean War, the extent of the government's reliance on contemporaneous tax increases is remarkable: total government expenditures as a share of GDP rose by 6.0 percentage points from 1950Q2 to its peak in 1952Q3, only moderately more than the expected revenue effects of the tax increases to finance the war. Moreover, Truman explicitly cited the deficit as a reason for this heavy reliance on tax finance. Soon after the start of the war, he wrote to Congressional leaders:

We embark on these enlarged expenditures at a time when the Federal budget is already out of balance. This makes it imperative that we increase tax revenues promptly lest a growing deficit create new inflationary forces detrimental to our defense effort.

We must make every effort to finance the greatest possible amount of needed expenditures by taxation (Letter to the Chairman, Senate Committee on Finance, on the Need for an Increase in Taxes, 7/25/50, p. 1).

Thus, the Korean War tax increases were in part a response to the 1948 tax cut.

B. The Revenue Act of 1964

Lyndon Johnson signed the Revenue Act of 1964 in February 1964. It reduced revenues by 1.3 percent of GDP in 1964Q2 and by another 0.6 percent in 1965Q1. The key motivation for the tax cut was a desire to increase long-run growth.

Because of very rapid growth, revenues recovered quickly after the tax cut. As a result, there were no immediate budget deficits to trigger a starve the beast type of response. Nevertheless, policymakers' statements and behavior can provide some evidence concerning this mechanism.

At almost the same time that he signed the tax bill, Johnson began to propose drastic increases in

spending. In February 1964 he gave a speech proposing federal hospital insurance for the elderly and other health initiatives (Special Message to the Congress on the Nation's Health, 2/10/64). In May 1964, he gave his speech on "the Great Society" that called for elimination of poverty, urban renewal, pollution reduction, and education expansion (Remarks at the University of Michigan, 5/22/64). Over the next year, a number of spending increases directed at achieving these goals were passed. The most significant was the dramatic expansion of benefits and the introduction of Medicare contained in the Social Security Amendments of 1965.

The administration believed that spending should be determined by necessity and efficiency. For example, the 1967 *Economic Report* stated: "most economists now agree that the selection of appropriate expenditure levels ... should be made in light of the relative merits of alternative programs, and of the benefits of added public expenditures, compared with private ones, at the margin. ... [I]t is preferable to emphasize changes in tax rates (suitably coordinated with changes in monetary policy) for stabilization purposes" (p. 68). The narrative record in this episode is striking in the degree to which revenues were not mentioned as a determinant of expenditures.

Defense spending increased substantially starting in mid-1965 because of escalation of the war in Vietnam. Johnson argued forcefully against allowing budgetary concerns to stop the rise in nondefense spending. He stated:

There are men who cry out: We must sacrifice. Well, let us rather ask them: Who will they sacrifice? Are they going to sacrifice the children who seek the learning, or the sick who need medical care, or the families who dwell in squalor now brightened by the hope of home? ...

I believe that we can continue the Great Society while we fight in Vietnam (Annual Message to the Congress on the State of the Union, 1/12/66, p. 2).

Congress went along with his calls for increased spending. For example, the Social Security Amendments of 1967 brought about another substantial increase in benefits and a significant increase in coverage. Thus, the rise in spending following the tax cut was in not just the consequence of the war.

Beginning in early 1966, policymakers began to worry that the economy was overheating. In addition, by late 1966, the deficit had increased substantially. Nevertheless, the administration did not

call for substantial spending reductions. Federal expenditures were expected to rise by \$15 billion in 1968 (1968 *Economic Report*, p. 54). Instead, the administration concluded: “the cost of meeting our most pressing defense and civilian requirements cannot be responsibly financed without a temporary tax increase” (1969 *Budget*, p. 8).

Over the president’s objection, Congress included a \$6 billion spending reduction (relative to projected) in the 1968 bill imposing a ten-percent temporary tax surcharge. Congress pressed for the spending cuts not because revenues had declined, but because it felt it was unfair to take all of the needed macroeconomic restraint in the form of higher taxes. A number of senators expressed sentiments similar to that of Senator Byrd, who stated: “Before any new tax burden ... is placed upon the American taxpayer, the executive branch and the legislative branch should reduce, and eliminate where possible, all nonessential expenditures” (*Congressional Record*, 90th Congress, 2^d Session, Volume 114—Part 7, 4/2/68, p. 8561). The tax cut was surely one factor contributing to the overheating that motivated the tax surcharge. Therefore, although policymakers did not draw a direct link between the tax cut and the spending cut, the spending reduction is the one development in this episode that could suggest some connection between tax cuts and subsequent spending decisions.

The actual behavior of spending following the 1964 tax cut is completely consistent with policymakers’ stated positions. Panel (b) of Figure 10 shows that total expenditures were basically constant during the first year after the tax cut, but then rose dramatically. Total expenditures increased by 27 percent in the five years after the tax cut. This is noticeably greater than the 18 percent growth in the five years before the cut. The rise in defense purchases was one source of the increase. However, nondefense spending, fueled by a large increase in transfer payments, increased even more rapidly.

Special factors clearly played a role in the behavior of spending. Much of the rise in defense expenditures was related to the Vietnam War. To the extent that defense spending truly was non-discretionary, some of the rise in spending reflects this exogenous shock rather than a failure of the starve the beast phenomenon. At the same time, the immediate increase in spending called for by the Social Security Amendments of 1965 and 1967 in a fundamental way underestimates the true rise in spending.

The creation of the Medicare program and the increases in Social Security benefits and coverage put in place an enormous stream of future spending. So, the present value increase in spending that was passed in the wake of the 1964 tax cut was unquestionably huge.

Policymakers' statements and actions on taxes in this episode are striking. In 1965, the Johnson administration proposed (and succeeded in passing) two significant tax actions. One was the Excise Tax Reduction Act of 1965. The administration viewed this tax cut as a continuation of the 1964 action. In this case, the serial correlation of tax changes reflected serial correlation in views about appropriate policy. The second was the Social Security Amendments of 1965, which included a substantial increase in payroll taxes to pay for an equally substantial increase in benefits, including hospital insurance for the elderly. This tax increase appears to have had little to do with the 1964 tax cut. Policymakers paid for the desired expansion of benefits through expansion of taxes because the decision had been made in the early 1950s that the Social Security system should be self-financing.³⁷

The overheating of the economy beginning in 1966 led policymakers to advocate tax increases. The Tax Adjustment Act of 1966 (enacted in March 1966) rescinded the excise tax reduction that had just occurred the previous January. Public Law 89-800 (enacted in November 1966) suspended the investment tax credit. Together, the two tax increases were expected to raise revenues by 0.3 percent of GDP.³⁸

By far the largest tax increase in the immediate post-1964 period was the 1968 surcharge. The administration first proposed a six-percent surcharge in January 1967. In August 1967, Johnson stated: "If left untended, this deficit could cause ... [a] spiral of ruinous inflation" and "[b]rutally higher interest rates" (Special Message to the Congress: The State of the Budget and the Economy, 8/3/67, p. 1). He requested that the surcharge be increased to ten percent, which was the level ultimately included in the

³⁷ The Social Security Amendments of 1967, enacted in January 1968, also raised taxes substantially to pay for another increase in benefits and coverage.

³⁸ Public Law 90-26 (enacted in June 1967) restored the investment tax credit. As discussed in Romer and Romer (2008), the motivation for this change involved the conditions in a particular sector (the capital goods market), and concern about longer-run incentives for investment. It does not appear to have been motivated by the 1964 tax cut or short-run macroeconomic conditions.

Revenue and Expenditure Control Act of 1968. The act increased taxes by 0.9 percent of GDP in 1968Q3 and by another 0.2 percent in 1969Q1. Johnson was explicit that the surcharge was undoing part of the 1964 tax cut. In the Statement by the President Upon Signing the Tax Bill, he stated: “This temporary surcharge will return to the Treasury about half the tax cuts I signed into law in 1964 and 1965” (6/28/68, p.1). This action, combined with the continued rise in expenditures, is a vivid example that what typically gives in response to a tax cut is not spending but the tax cut itself.

C. The Economic Recovery Tax Act of 1981

A very large long-run tax cut was enacted in August 1981, shortly after Ronald Reagan took office. The cut lowered taxes by a combined 4.5 percent of GDP in a series of steps.

Reagan was a strong advocate of spending reductions throughout his presidency. For example, in a speech presenting his economic program, he identified “reducing the growth in government spending and taxing” as a central goal, and he argued that “[s]pending by government must be limited to those functions which are the proper province of government” (Address Before a Joint Session of the Congress on the Program for Economic Recovery, 2/18/81, pp. 1, 5). Similarly, in his first budget message, in February 1982, he listed “[r]educing the growth of overall Federal spending by eliminating Federal activities that overstep the proper sphere of Federal Government responsibilities” as one of his fundamental economic goals (1983 *Budget*, p. M4).

The tax cut was followed by a substantial fall in revenues and a large rise in the deficit. As the deficit increased, Reagan often cited it as a further reason for restraining spending. For example, in his February 1986 budget message, he said, “there is a major threat looming on the horizon: the Federal deficit” (1987 *Budget*, p. M-4). He went on to say: “Spending is the problem—not taxes—and spending must be cut. The program of spending cuts and other reforms contained in my budget will lead to a balanced budget at the end of five years” (p. M-5). Similarly, his February 1988 budget message stated:

Last year, members of my Administration worked with the Leaders of Congress to develop a 2-year plan of deficit reduction—the Bipartisan Budget Agreement. ...
The Bipartisan Budget Agreement reflects give and take on all sides. I agreed to

some \$29 billion in additional revenues and \$13 billion less than I had requested in defense funding over 2 years. However, because of a willingness of all sides to compromise, an agreement was reached that pared \$30 billion from the deficit projected for 1988 and \$46 billion from that projected for 1989 (1989 *Budget*, p. 1-6).

Thus, the narrative record from this episode provides some evidence that revenue declines affected spending decisions.

Panel (c) of Figure 10 plots the behavior of government spending around the 1981 tax cut. The vertical line is drawn in 1981Q3, the date of the first of the series of cuts. Despite what is suggested by the narrative evidence, the growth of overall spending did not fall, but actually rose. In the five years following the tax cut, total expenditures grew by 23 percent, substantially above the 14 percent growth in the five years before the cut. This overall increase in spending growth reflects a combination of a large rise in the growth of defense spending and a more moderate rise in the growth of nondefense spending.

There were two important unusual spending developments in this episode. First, the tax cuts coincided with a shift in political power toward supporters of lower spending. Reagan's goal of restraining government spending was not shared by his predecessor. For example, in his final budget message, Jimmy Carter, while advocating "budget restraint," stated, "The growth of budget outlays is puzzling to many Americans, but it arises from valid social and national security concerns" (1982 *Budget*, pp. M4-M5). There was also a large swing in the balance of political power in Congress toward advocates of spending restraint at the time of Reagan's election. Thus, there was clearly an omitted variable acting to reduce spending in this episode.³⁹

Second, the heightening of the cold war acted to increase defense spending. Ramey and Shapiro (1998), for example, identify the Soviet invasion of Afghanistan at the end of 1979 as an exogenous positive shock to defense spending. This factor operates in the opposite direction of the political shift toward supporters of lower spending.

The tax cuts were followed by two types of tax increases. First, the Social Security Amendments

³⁹ Although Reagan supported spending reduction in general, he favored higher defense spending. He had campaigned on a need to rebuild the military and identified "strengthening the Nation's defenses" as one of his key goals (1983 *Budget*, p. M4).

of 1983 called for a series of tax increases from 1984 to 1990 to improve the solvency of the Social Security system. These increases appear to have been largely a continuing consequence of the 1950 decision to make the Social Security program self-financing.

Second, there was a series of tax increases that were explicitly motivated by a desire to reduce the budget deficits that developed followed the tax cuts. These included the Tax Equity and Fiscal Responsibility Act of 1982, which undid some of the provisions of the 1981 act; the Deficit Reduction Act of 1984; the Omnibus Budget Reconciliation Act of 1987; and the Omnibus Budget Reconciliation Act of 1990. For example, in a national address on the 1982 act, Reagan stated that it reflected a choice to “reduce deficits and interest rates by raising revenue from those who are not now paying their fair share,” rather than to “accept bigger budget deficits, higher interest rates, and higher unemployment” (Address to the Nation on Federal Tax and Budget Reconciliation Legislation, 8/16/82, p. 4). Similarly, the 1989 *Budget* reported that the 1987 act was enacted “[i]n conformance with the Bipartisan Budget Agreement” (p. 4-5), which, as described above, was motivated by concern about the deficit. The 1982 and 1984 actions alone increased taxes by 1.0 percent of GDP. Thus, these tax increases were a fairly direct response to the earlier tax cut.

D. The Economic Growth and Tax Relief Reconciliation Act of 2001 and the Jobs and Growth Tax Relief Reconciliation Act of 2003

Two long-run tax cuts were passed early in the administration of George W. Bush. The Economic Growth and Tax Relief Reconciliation Act of 2001, enacted in June 2001, included a long-run tax cut of 0.8 percent of GDP in 2002Q1, as well as a large countercyclical tax cut in 2001Q3. The Jobs and Growth Tax Relief Reconciliation Act of 2003, enacted in May 2003, included a long-run cut of 1.1 percent of GDP in 2003Q3.

These tax cuts do not appear to have had any substantial impact on the administration’s view of appropriate spending. Throughout the episode, both spending restraint and either preserving the surplus or reducing the deficit received some attention. But, the discussions of spending did not change

appreciably in response either to the tax cuts or to the subsequent deterioration of the budget situation.

The administration's first budget proposals, which predated the tax cuts, put some emphasis on spending restraint and on paying down debt. The president's first budget document, for example, stated that the budget would "Moderate Growth in Government and Fund National Priorities" and achieve "Debt Reduction" (A Blueprint for New Beginnings: A Responsible Budget for America's Priorities, 2/28/01, p. 7).⁴⁰ It also said that "[t]he President's Budget commits to using today's surpluses to reduce the Federal Government's publicly held debt so that future generations are not shackled with the responsibility of paying for the current generation's overspending" (p. 22), and that "we must ensure that we rein in excessive Government spending" (p. 23).

In the immediate aftermath of the attacks of September 11, 2001, discussions of budget policy placed less emphasis on spending restraint (see, for example, Address Before a Joint Session of the Congress on the State of the Union, 1/29/02, p. 4). Later presidential statements, however, returned to calls for spending restraint similar to those in 2001. For example, in his 2004 State of the Union Address, Bush stated: "I will send you a budget that funds the war, protects the homeland, and meets important domestic needs while limiting the growth in discretionary spending By doing so, we can cut the deficit in half over the next 5 years" (Address Before a Joint Session of the Congress on the State of the Union, 1/20/04, p. 5). Similarly, in the 2007 State of the Union Address, he said, "What we need is spending discipline I will submit a budget that eliminates the Federal deficit within the next 5 years" (Address Before a Joint Session of the Congress on the State of the Union, 1/23/07, p. 1). While these statements were very similar to those before the tax cuts, actual budget conditions had changed substantially: revenues had fallen and the overall budget had shifted from surplus to deficit. The similarity in the rhetoric despite the large changes in the deficit suggests that there was not a link between the level of revenues and the perceived need for spending restraint.

Panel (d) of Figure 10 plots the behavior of the major categories of spending in this episode. The

⁴⁰ This document was not part of the president's formal 2002 budget, which was not submitted until April 2001. However, it is included with the other 2002 budget documents on the Government Printing Office website. See <http://www.gpoaccess.gov/usbudget/fy02/index.html>.

two vertical lines show the dates that the two tax cuts first took effect. As in the other episodes, overall spending growth did not slow. In the five years following the first cut in 2001Q3, spending grew by 22 percent, substantially larger than the 14 percent growth in the five years before the cut. The growth in spending following the tax cut was greatest in defense: national defense purchases rose by 33 percent in the five years after the tax cut, while nondefense spending rose by 19 percent.

The events of September 11, 2001 were clearly an important outside influence on spending. Some of the behavior of total expenditures surely reflects the impact of this development rather than the effect of the tax cuts. On the other hand, there was one important spending action that is not well reflected in our spending measures. The addition of prescription drug coverage to Medicare, enacted in December 2003, was expected to have only a modest short-run effect on spending but to raise its path substantially over time. Thus, although the change was enacted soon after the tax cuts, most of its impact on spending will almost surely come after the period considered in our regressions.

One notable feature of this episode is that the tax cuts were not soon followed by counteracting tax increases. A modest countercyclical tax cut was enacted in March 2002, in the wake of the September 11 attacks. The only important tax increase was that the bonus depreciation provisions included in the 2002 bill, and then expanded and slightly extended as part of the 2003 tax bill, were allowed to expire as scheduled at the end of 2004. Thus the issue of how the government will eventually deal with the loss of revenues from the 2001 and 2003 tax cuts remains open.

E. Assessment

Examination of the four episodes of major long-run tax cuts reinforces the findings from the statistical work: there is little evidence of a starve the beast effect. The one aspect of the episodes that is somewhat consistent with the hypothesis that tax cuts reduce government spending is the narrative record of the budget process. Although the presidents in two of the episodes (Johnson and Bush) appear to have paid little attention to the impact of the tax cuts on revenues in formulating their budget policies, the presidents in the other two (Truman and Reagan) cited the level of revenues as a consideration in budget

policy. Even in these cases, however, other factors were clearly much more important, and to a considerable extent the concern over revenues led not to advocacy of spending reductions, but to support for (or acceptance of) tax increases.

The actual behavior of spending in all four episodes provides no support for the starve the beast hypothesis. In no episode was there a discernible slowdown in spending following the tax cut. Indeed, in all of the episodes, there was an acceleration of spending. This is similar to the overall statistical finding of a positive (though only marginally significant) effect of tax cuts on spending, and suggests that the regression results reflect a consistent pattern in the data rather than the effects of outliers.

Examination of other influences on spending in the episodes does not change these conclusions. On the one hand, there was an important external development in each episode that acted to raise defense spending. By itself, this pattern would suggest that the regressions might overestimate the positive effects of tax cuts on spending. Two considerations, however, point in the opposite direction.⁴¹ First, the largest tax cut (1981) coincided with the election of a president who had a strong commitment to reducing the size of government. This suggests that the positive impact of tax cuts on spending might be even larger than that implied by the regressions. Second, there were significant actions taken in the episodes to increase spending that had important effects after the five-year window considered in our baseline regressions. For example, in two of the episodes (1964 and 2001/03), the government enacted major changes in the provision of medical care for the elderly that had very large implications for the long-term path of government spending. Since our regressions miss much of the effects of these actions, this too suggests that the regressions may underestimate the extent to which tax cuts increase spending. Thus, examination of other factors affecting spending in the episodes suggests that on net the regressions do not overstate the evidence against the starve the beast hypothesis.

The behavior of subsequent tax changes in the episodes is consistent with the regression results. In three of the cases, there were substantial tax increases within five years that offset a substantial fraction

⁴¹ In addition, recall that our statistical results are robust to controlling for a measure of exogenous shocks to defense spending, and that even excluding defense spending entirely provides little evidence for the starve the beast hypothesis.

of the initial tax cut. Perhaps more striking is what policymakers said about the tax increases. In all three cases they referred directly to the need to raise taxes to counter the macroeconomic and budgetary effects of the original tax cuts. And in two cases (1948 and 1964), the president said explicitly that raising taxes was preferable to cutting spending.

V. CONCLUSIONS

The starve the beast hypothesis—the idea that tax cuts restrain government spending—is a central argument for tax reduction. Despite its importance, however, the hypothesis has been subject to few tests, and the tests that have been done have important limitations.

This paper tests the starve the beast hypothesis by examining the behavior of government spending following tax changes motivated by long-run considerations. Because these tax changes are not motivated by factors that are likely to have an important direct effect on government spending, they are the most appropriate for testing the theory. The results provide no evidence of a starve the beast effect: following long-run tax cuts, government spending does not fall. Indeed, if anything, spending rises, providing some support for the alternative view of fiscal illusion or shared fiscal irresponsibility. The lack of support for a starve the beast effect is highly robust. Detailed examination of the four largest postwar episodes of long-run tax cuts reinforces the statistical findings.

We also identify a potentially powerful source of bias in tests of the starve the beast hypothesis that use data on overall revenues and spending. Some tax changes are explicitly motivated by contemporaneous or planned changes in spending. Not surprisingly, these tax changes are followed by large spending changes in the same direction. Causation, however, runs from the decisions to raise spending to the tax changes. For the full postwar sample, this type of tax change is sufficiently common that it causes the overall relationship between tax revenues and spending to be significantly positive. Excluding these spending-driven changes makes the relationship negative and marginally significant.

The fact that tax cuts do not lead to spending reductions raises the question of how the government budget constraint is ultimately satisfied. We find that long-run tax cuts are offset by legislated and non-legislated tax increases over the next several years. The fact that policymakers are able to make changes on the tax sides helps to explain why they do not appear to make large changes on the spending side.

Of course, failing to find support for the starve the beast hypothesis is not the same as definitively refuting it. There are several ways in which our results are not inconsistent with the presence of at least some starve the beast effect. First, our failure to find a starve the beast effect for the postwar U.S. federal government does not mean it is not important in other times and places. Second, the case that focusing on tax changes taken for long-run purposes yields unbiased estimates is not airtight. As we describe, however, the most likely direction of bias is in favor of the starve the beast hypothesis, not against it. Third, because our estimates are not highly precise, the hypothesis that tax cuts exert some restraining influence on spending usually cannot be rejected. Fourth, some of our evidence (the statistical examination of nondefense spending and the narrative evidence for the 1948 and 1981 episodes) provides some hints of support for a small starve the beast effect.

Finally, although we find that the fall in revenues caused by a tax cut disappears after a few years, some of this disappearance is most likely the result of a temporary output boom. Thus, we do not completely resolve the issue of how the government restores long-run budget balance. Since the government's long-run budgetary situation deteriorated substantially over the period we consider, to some extent this limitation is inherent: not all of the offsetting actions have yet occurred. It is possible that some of the remaining adjustment will take place on the spending side.

Taken together, these caveats imply that one cannot necessarily conclude that tax cuts do not restrain government spending at all. But it remains the case that, over the period we consider, there is virtually no evidence of such an effect.

The finding that tax cuts do not appear to substantially restrain government spending could obviously have implications for policy. At the very least, policymakers should be aware that the

historical experience suggests that tax cuts tend to lead to tax increases rather than to spending cuts.

The finding also has implications for models that assume the existence of a starve the beast effect. For example, Bohn (1992) argues that one reason for Ricardian equivalence to fail is that a tax cut implies that government spending will be lower; as a result, a tax cut leads households to reduce their estimates of the present value of their present and future liabilities, and so to increase their consumption. Similarly, a restraining effect of tax cuts on government spending plays a central role in the theories of strategic debt accumulation of Persson and Svensson (1989), Tabellini and Alesina (1990), and others. If decision-makers understand that tax cuts do not in fact lead to substantial reductions in government spending, these mechanisms are much less important. Thus, better estimates of the effects of tax cuts on spending may require changes to the modeling of a wide range of issues.

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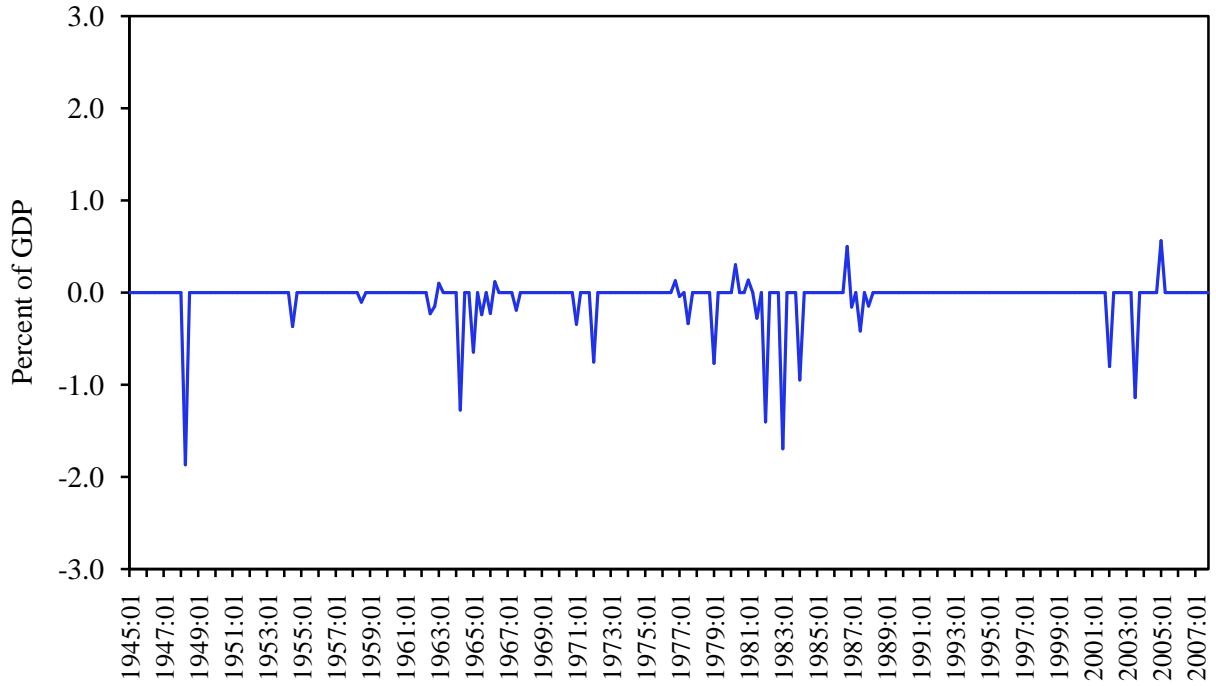
Table 1
Estimated Impact of Tax Changes on Total Expenditures

Variable	Coefficient
Constant	0.72 (0.25)
Tax Change:	
Lag 0	0.24 (0.85)
Lag 1	0.40 (0.85)
Lag 2	-0.11 (0.85)
Lag 3	-0.28 (0.83)
Lag 4	-0.92 (0.87)
Lag 5	-1.50 (0.87)
Lag 6	0.31 (0.87)
Lag 7	-1.42 (0.75)
Lag 8	2.63 (0.75)
Lag 9	2.52 (0.75)
Lag 10	-0.98 (0.75)
Lag 11	-1.53 (0.74)
Lag 12	-2.19 (0.76)
Lag 13	-2.13 (0.76)
Lag 14	-1.11 (0.76)
Lag 15	0.47 (0.76)
Lag 16	0.02 (0.76)
Lag 17	-0.11 (0.74)
Lag 18	0.51 (0.78)
Lag 19	0.86 (0.78)
Lag 20	0.20 (0.78)
 R ²	 0.20
D.W.	1.90
s.e.e.	2.72

Note: Estimates of equation (1) in the text using long-run tax changes and total gross expenditures less interest payments. The sample period is 1950Q1–2007Q4. The numbers in parentheses are standard errors.

Figure 1
Legislated Tax Changes Classified by Motivation

a. Long-Run Tax Changes



b. Spending-Driven, Countercyclical, and Deficit-Driven Tax Changes

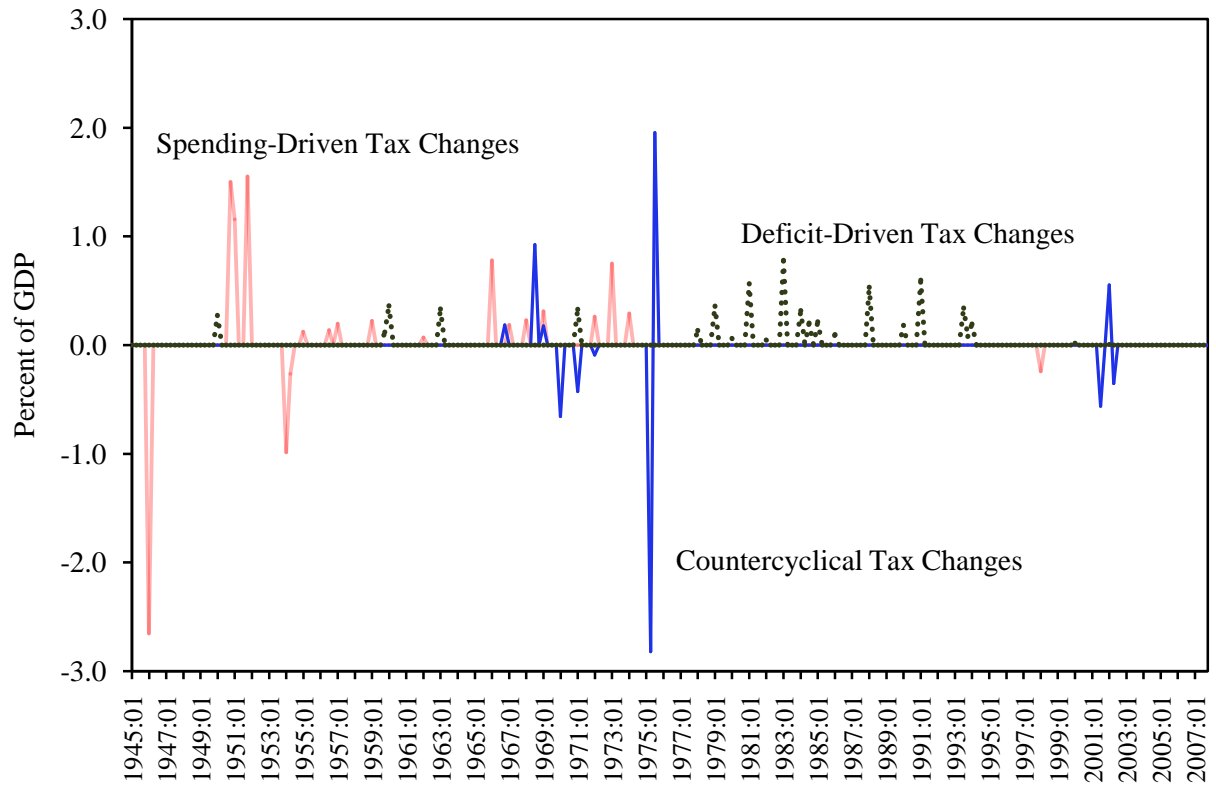


Figure 2
Estimated Impact of a Tax Cut of 1% of GDP on Total Expenditures
Baseline Specification

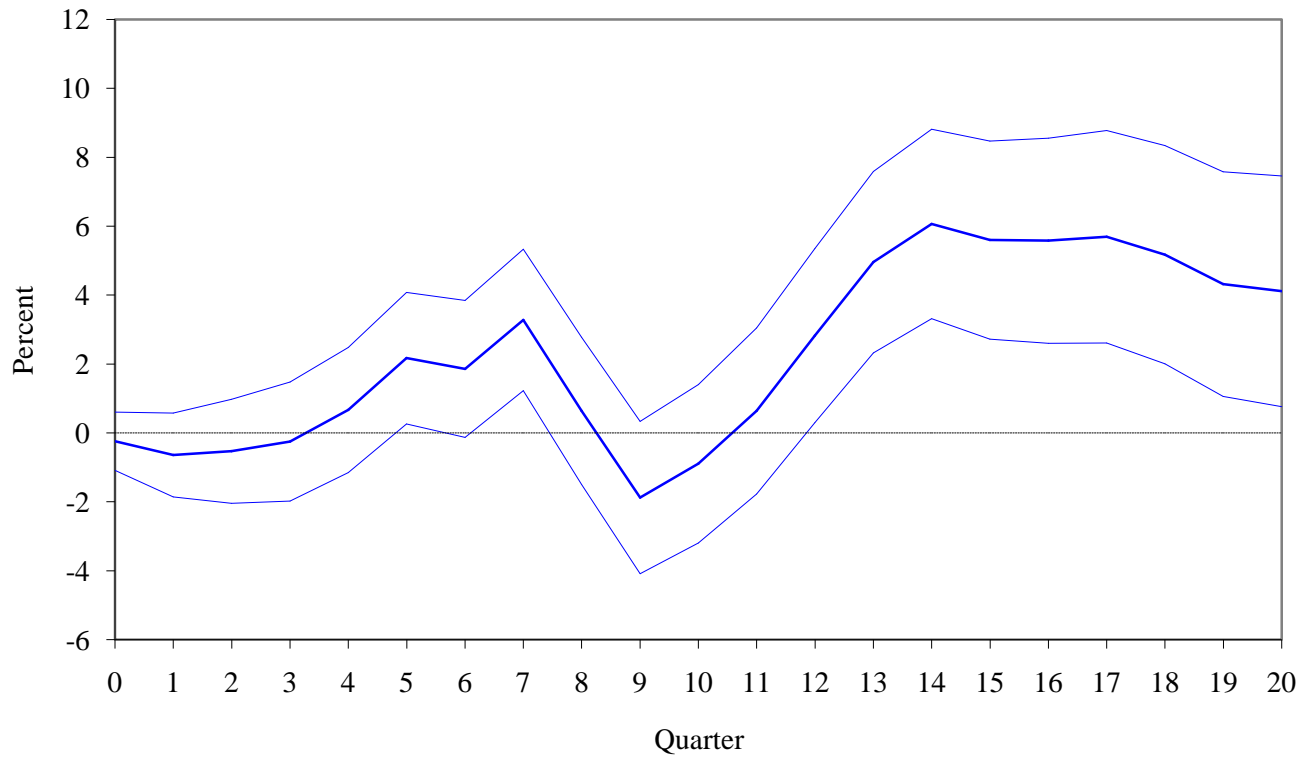
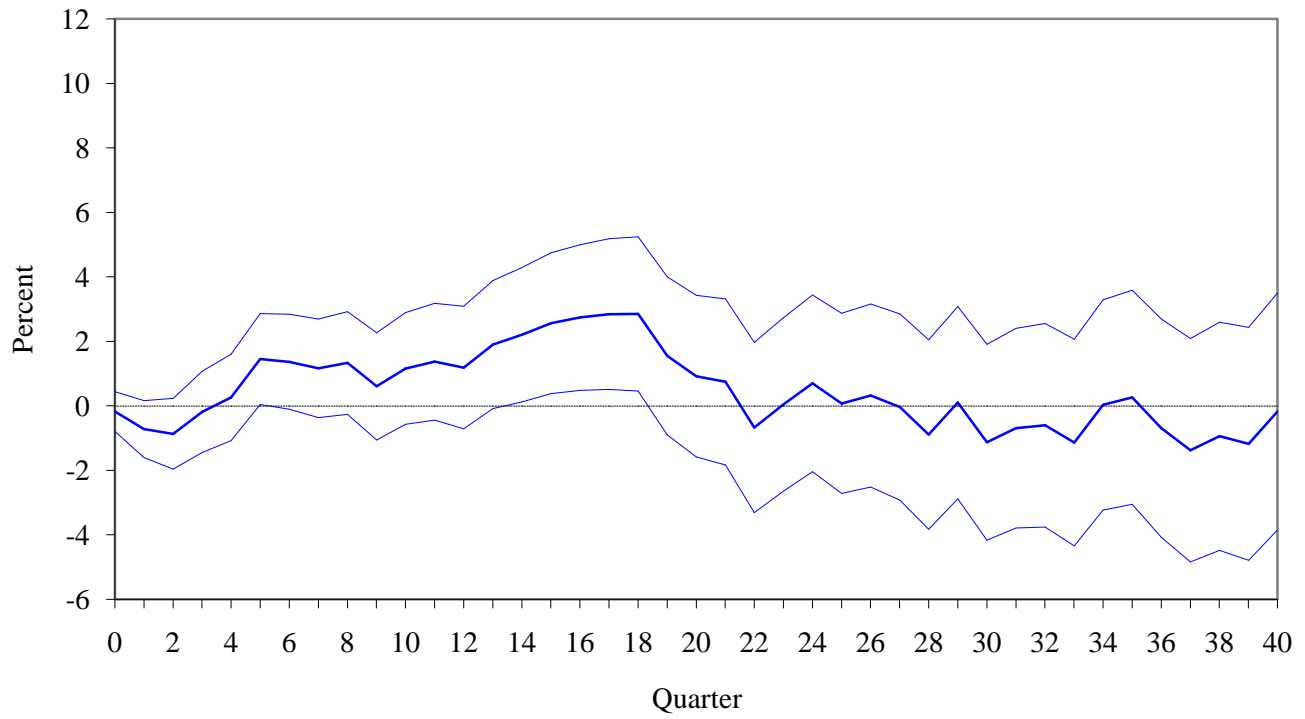


Figure 3
Estimated Impact of a Tax Cut of 1% of GDP on Total Expenditures
with Richer Dynamics

a. Including 40 Lags of Tax Changes



b. Using a Two-Variable VAR

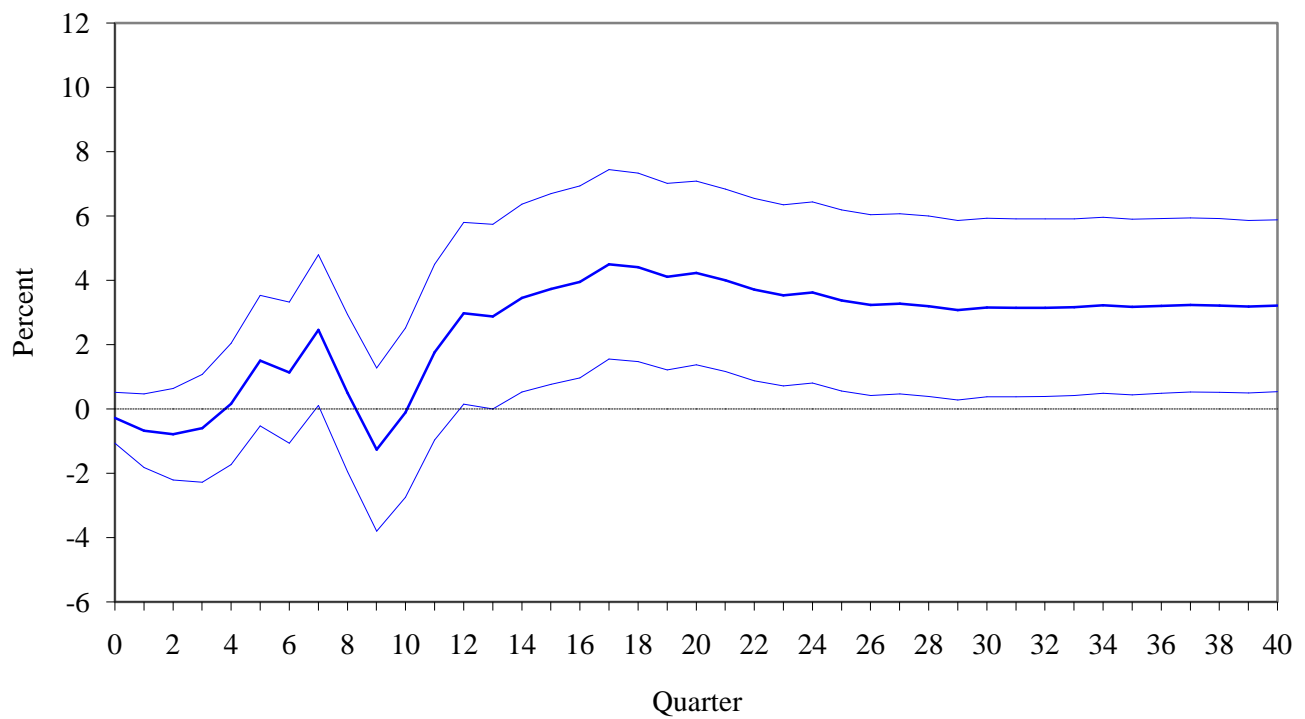
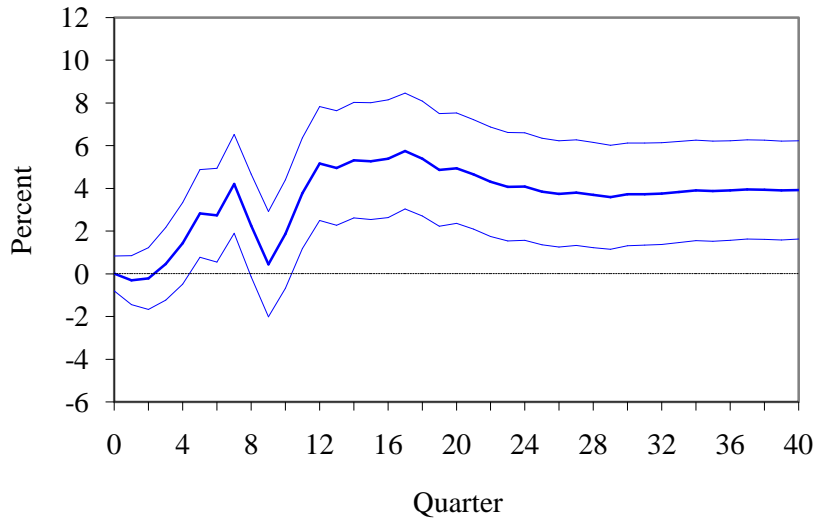
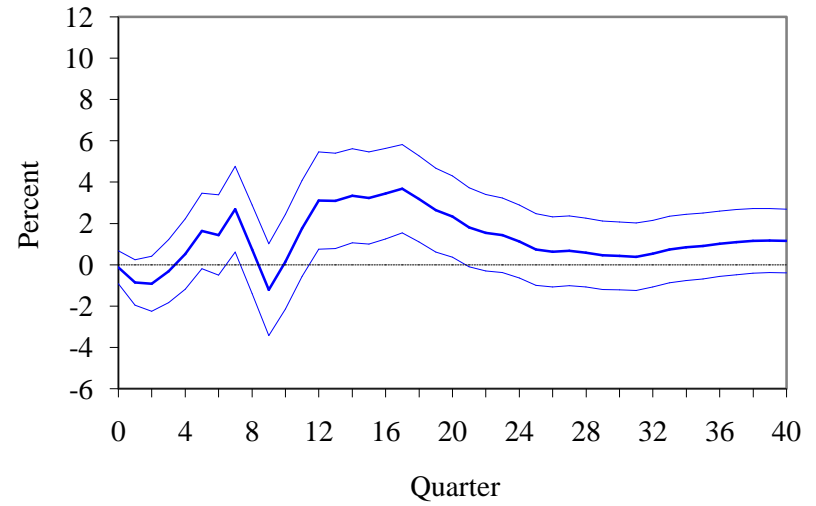


Figure 4
Estimated Impact of a Tax Cut of 1% of GDP on Total Expenditures in Multivariate VARs

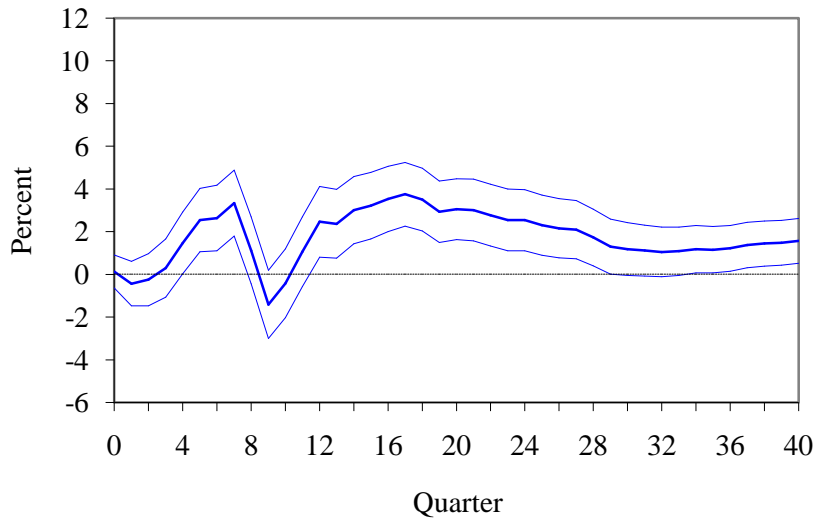
a. 3-Variable VAR (Taxes, Spending, Debt)



b. 4-Variable VAR (Taxes, Spending, Debt, Receipts)



c. 4-Variable VAR (Taxes, Spending, Debt, GDP)



d. 7-Variable VAR

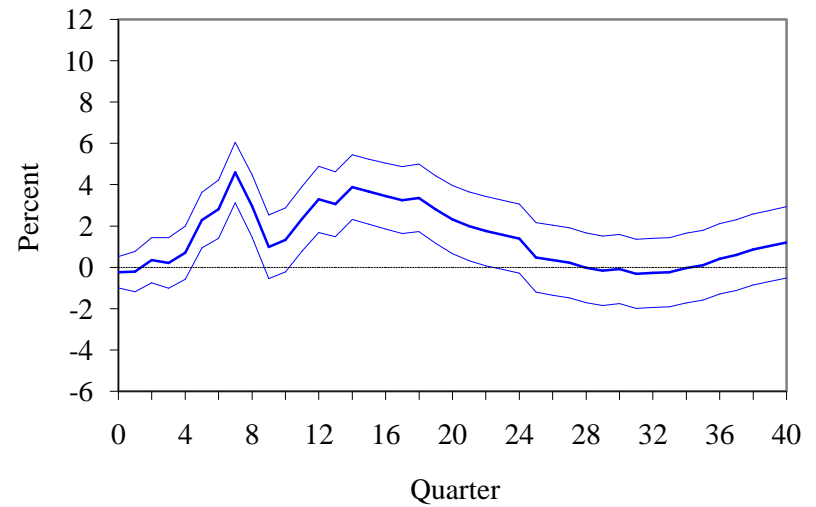
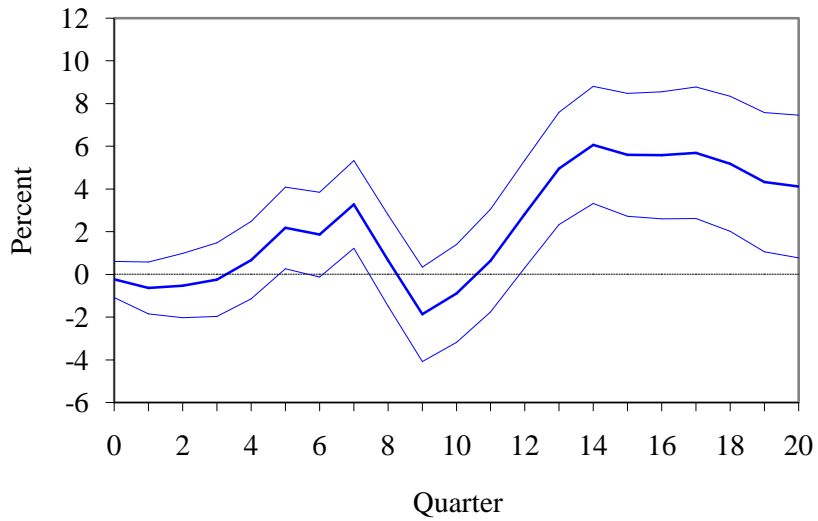
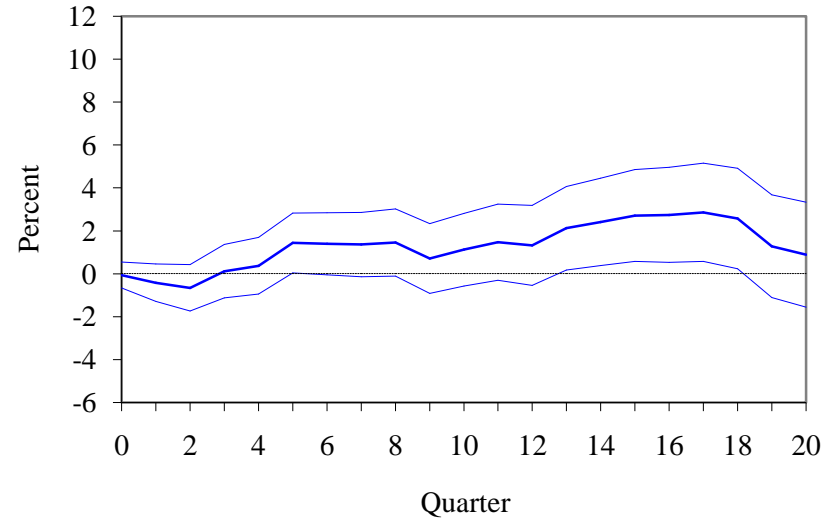


Figure 5
Estimated Impact of a Tax Cut of 1% of GDP on Total Expenditures Using Different Specifications

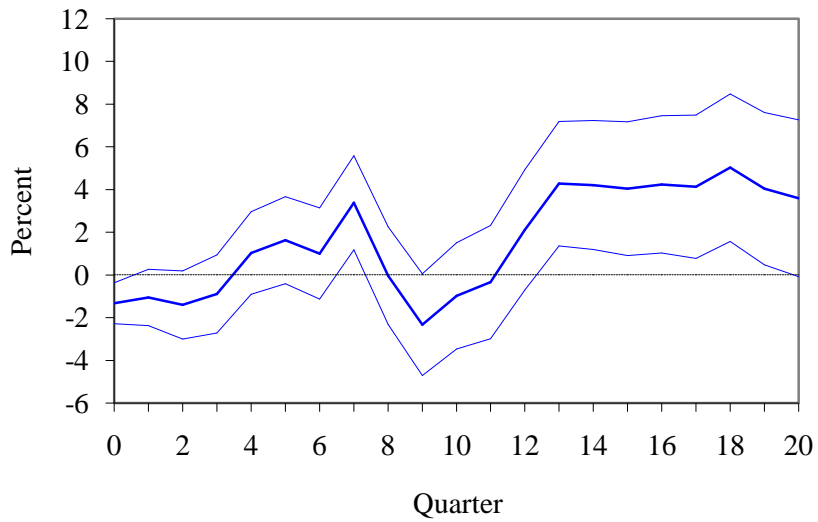
a. Baseline Specification



b. Starting Sample in 1957Q1



c. Including War Dummy Variable



d. Total Expenditures Less Defense

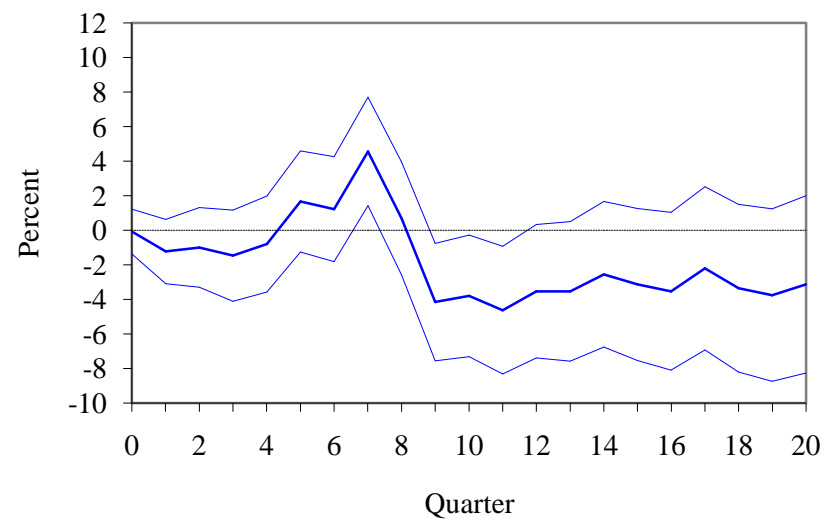
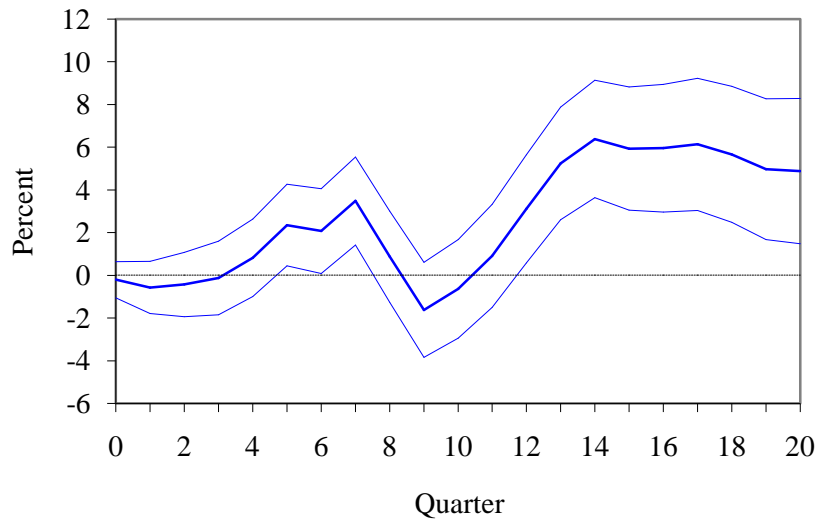
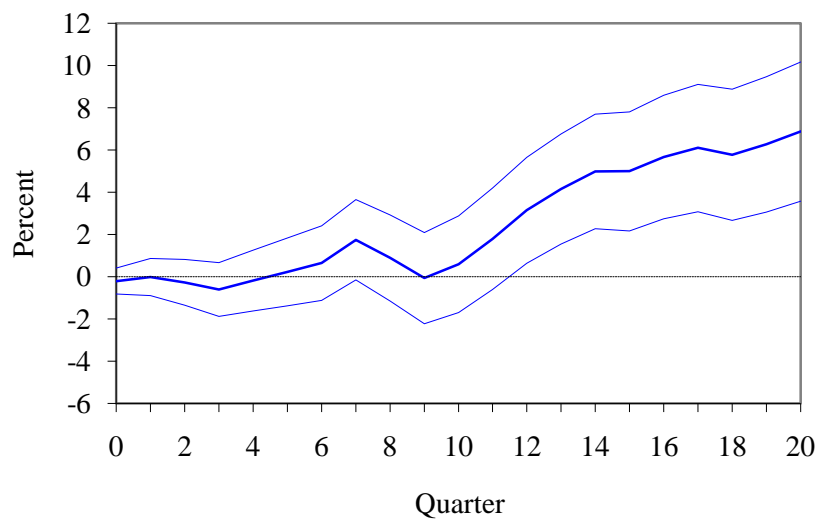


Figure 5 (continued)
 Estimated Impact of a Tax Cut of 1% of GDP on Total Expenditures Using Different Specifications

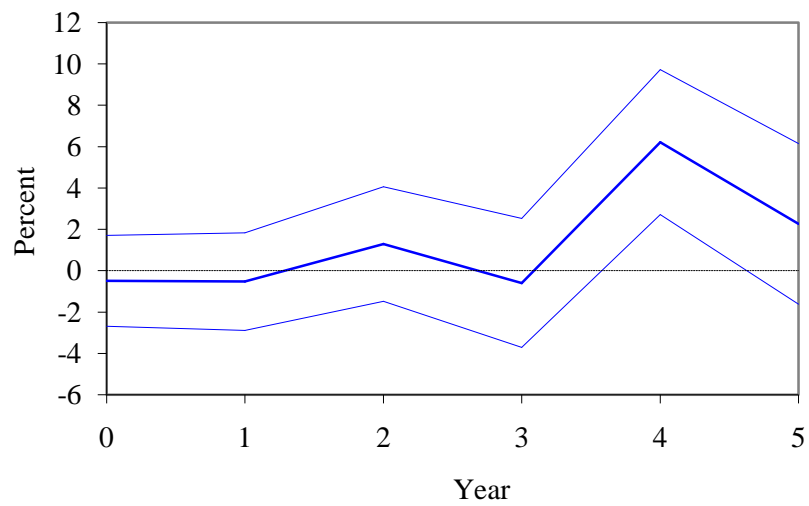
e. Including Democratic Dummy Variable



f. Using PDV Measure of Tax Changes



g. Budget-Based Spending Measure



h. Discretionary Spending

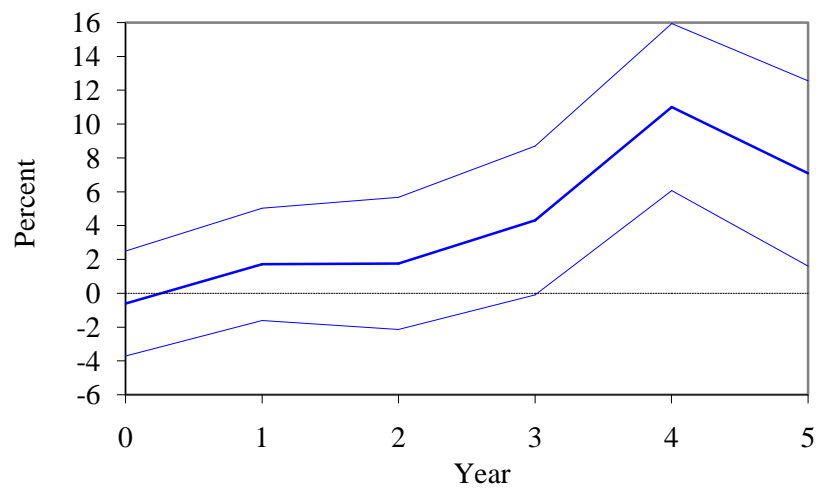


Figure 5 (continued)
Estimated Impact of a Tax Cut of 1% of GDP on Total Expenditures Using Different Specifications

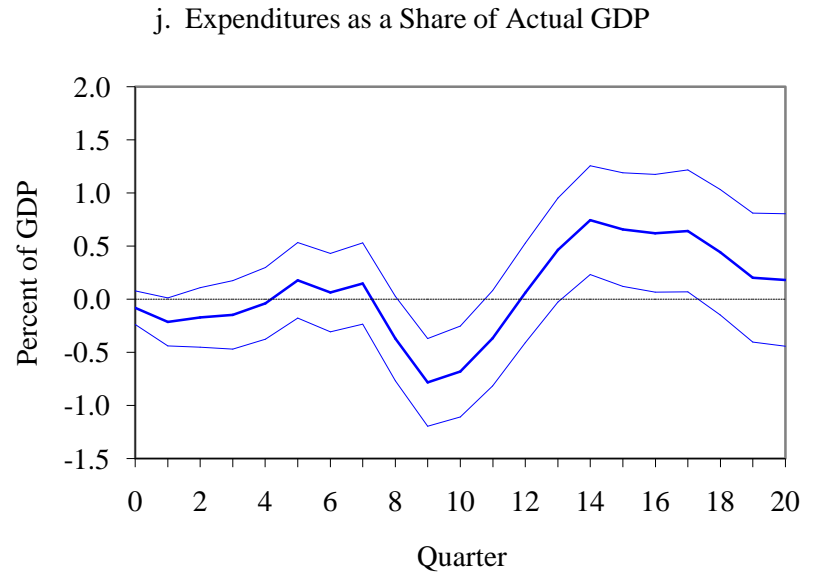
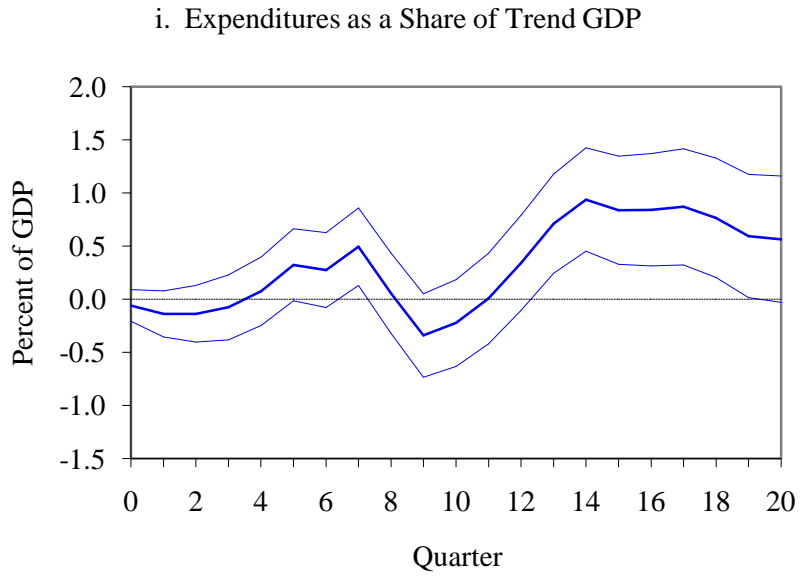
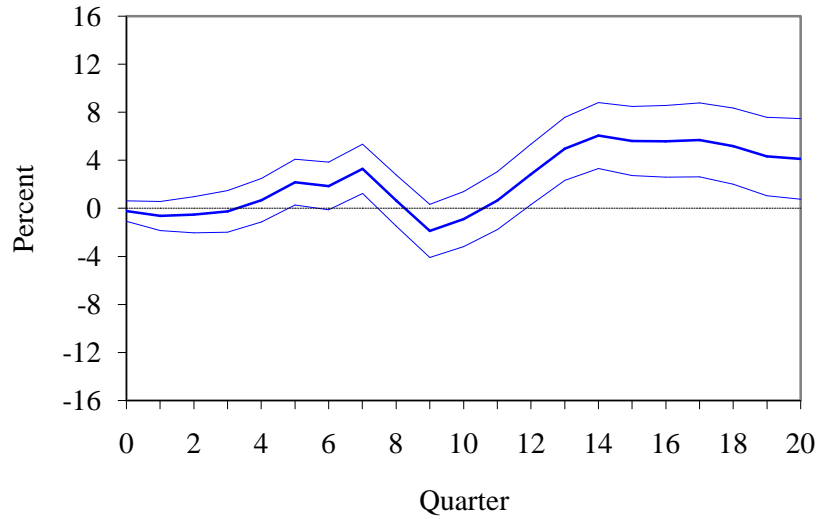
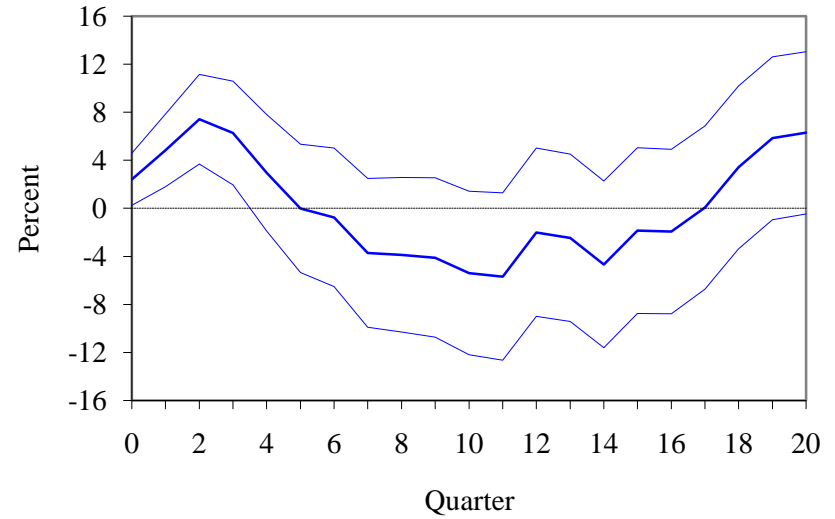


Figure 6
Estimated Impact of a Tax Cut of 1 % of GDP on Total Expenditures by Type of Tax Change

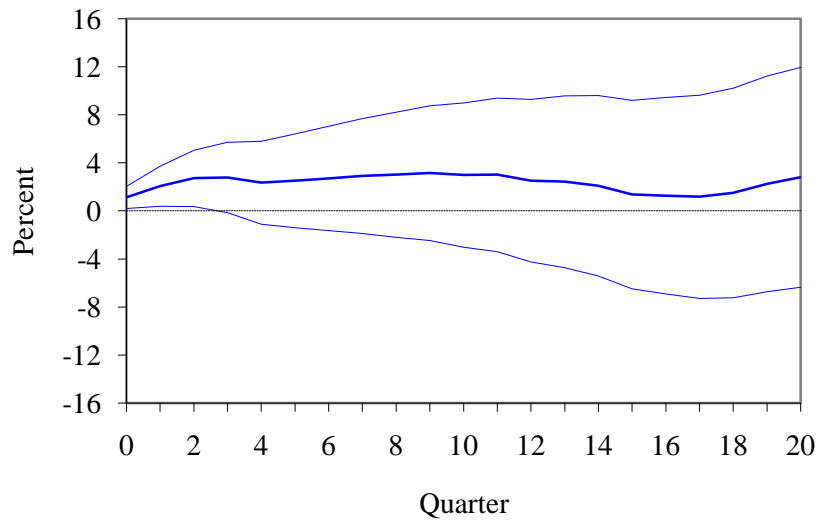
a. Long-Run Tax Changes



b. Deficit-Driven Tax Changes



c. Countercyclical Tax Changes



d. Spending-Driven Tax Changes

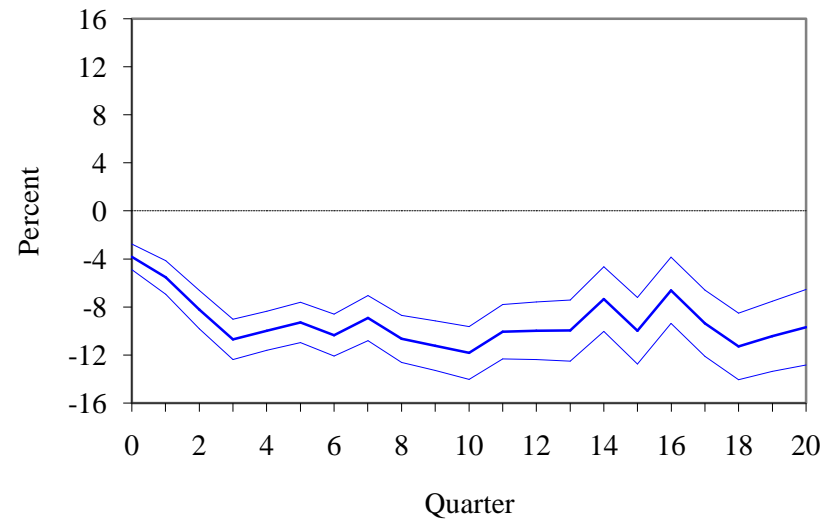
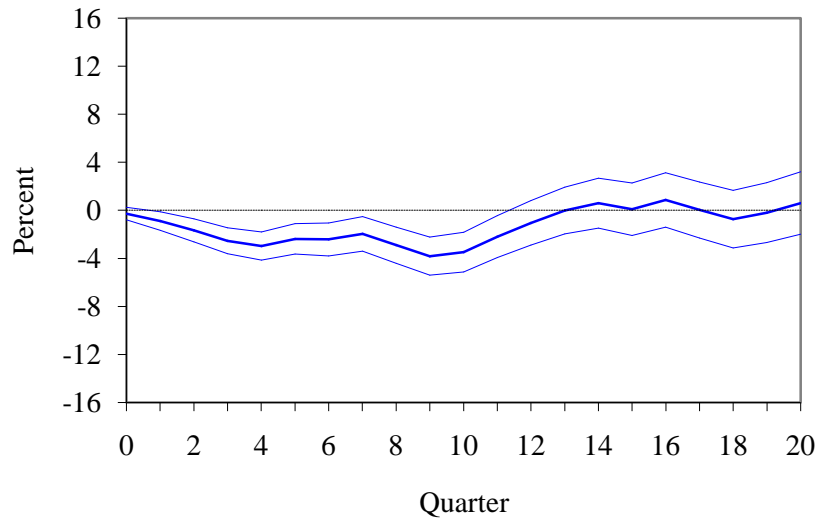
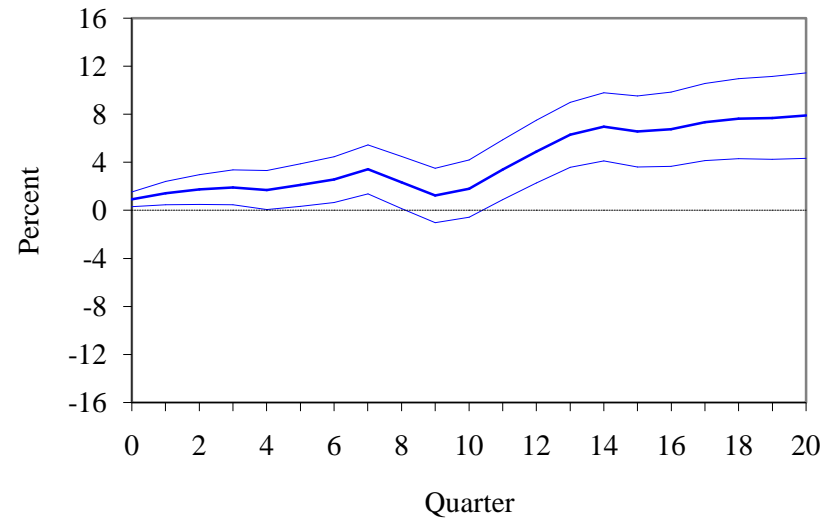


Figure 6 (continued)
Estimated Impact of a Tax Cut of 1% of GDP on Total Expenditures by Type of Tax Change

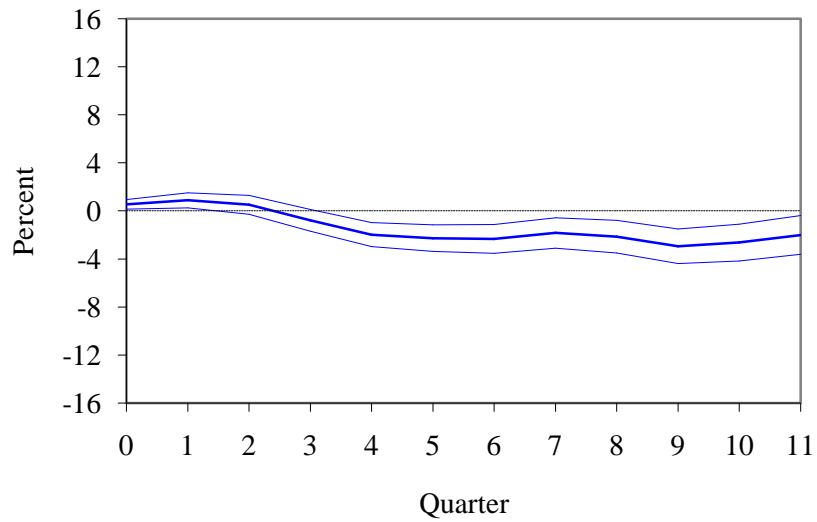
e. All Legislated Tax Changes



f. All Legislated Less Spending-Driven



g. Cyclically Adjusted Revenues



h. C. A. Revenues Less Spending-Driven

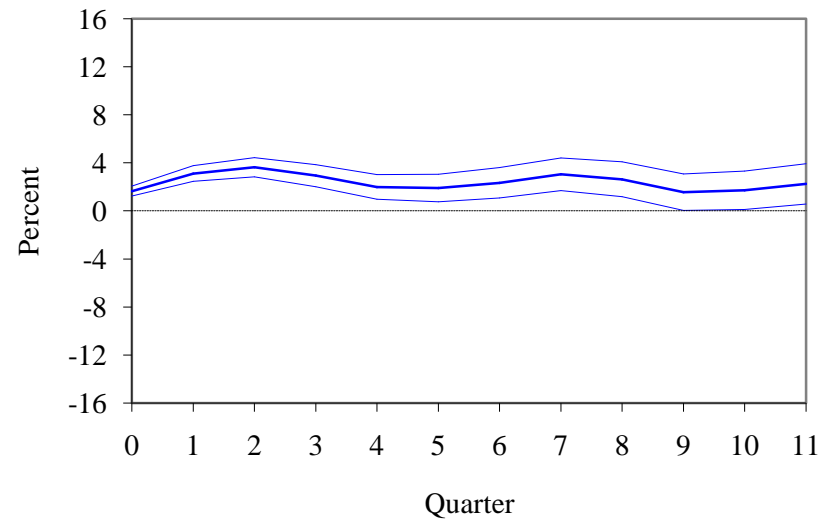
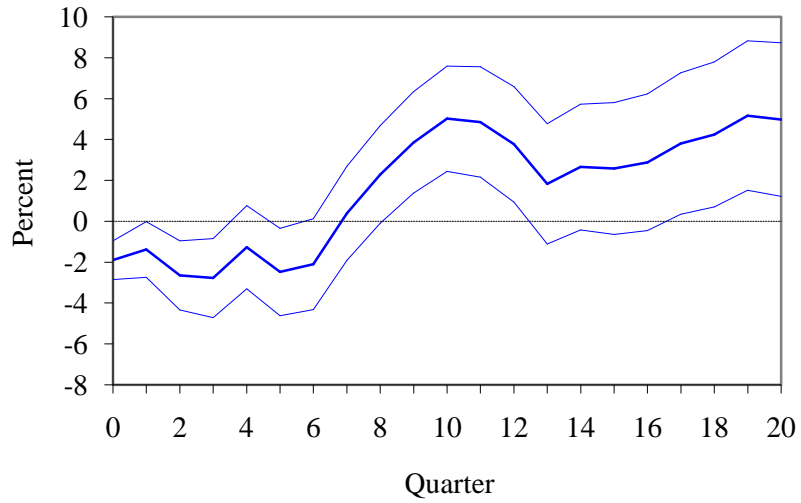
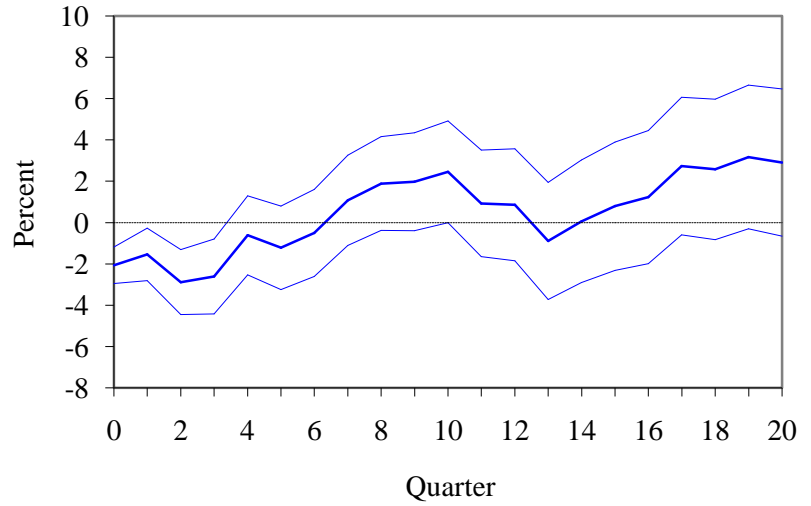


Figure 7
Estimated Impact of a Tax Cut of 1% of GDP on Total Receipts

a. Full Sample



b. Starting Sample in 1957Q1



c. In a Two-Variable VAR

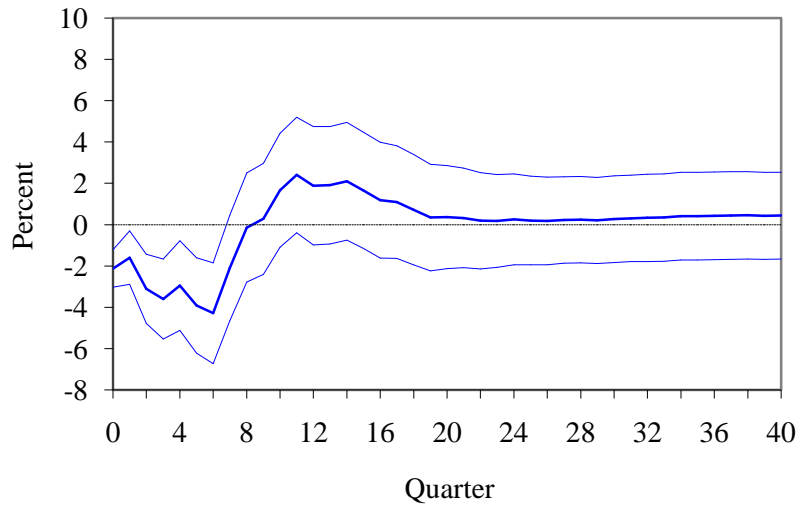


Figure 8
Estimated Impact of a Long-Run Tax Cut of 1% of GDP on Tax Changes by Type

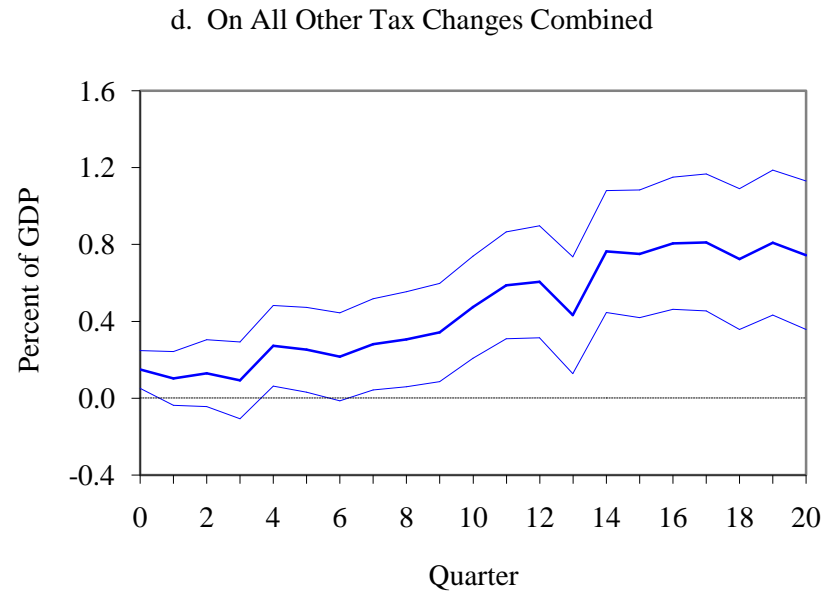
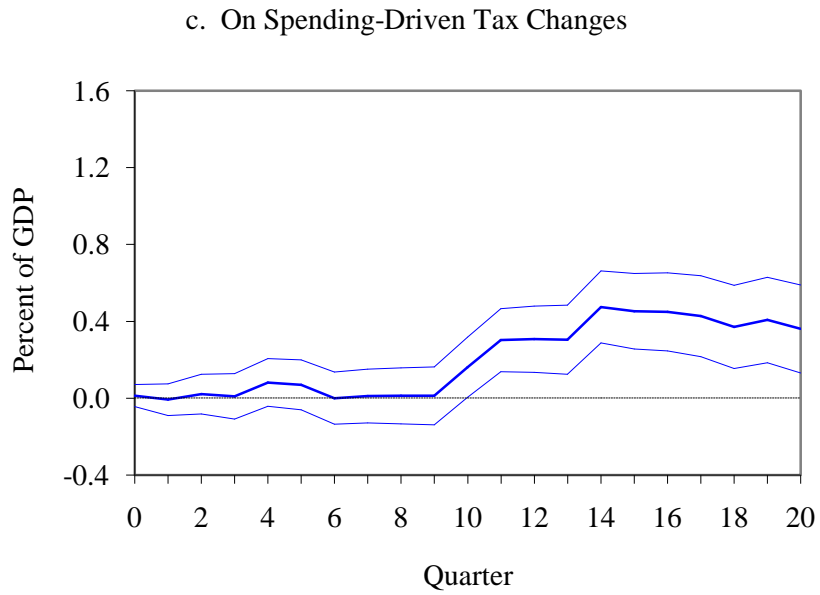
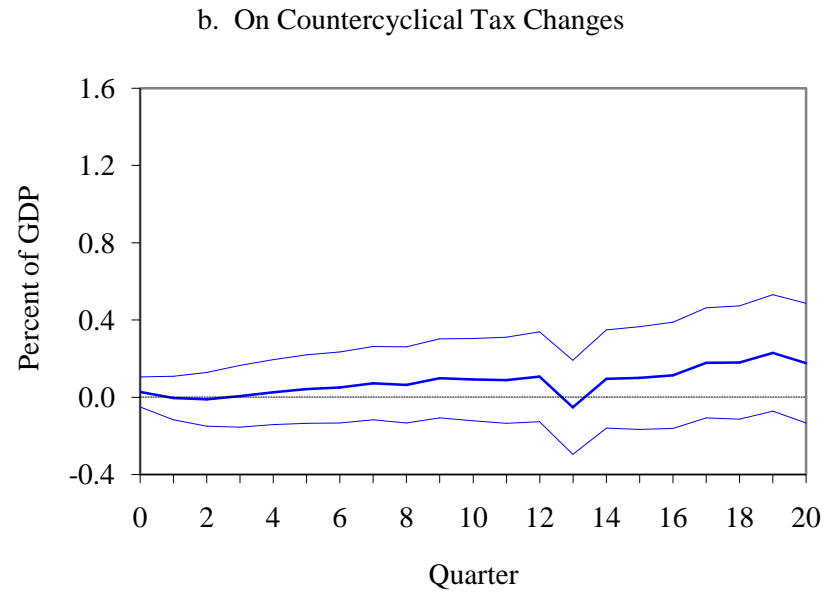
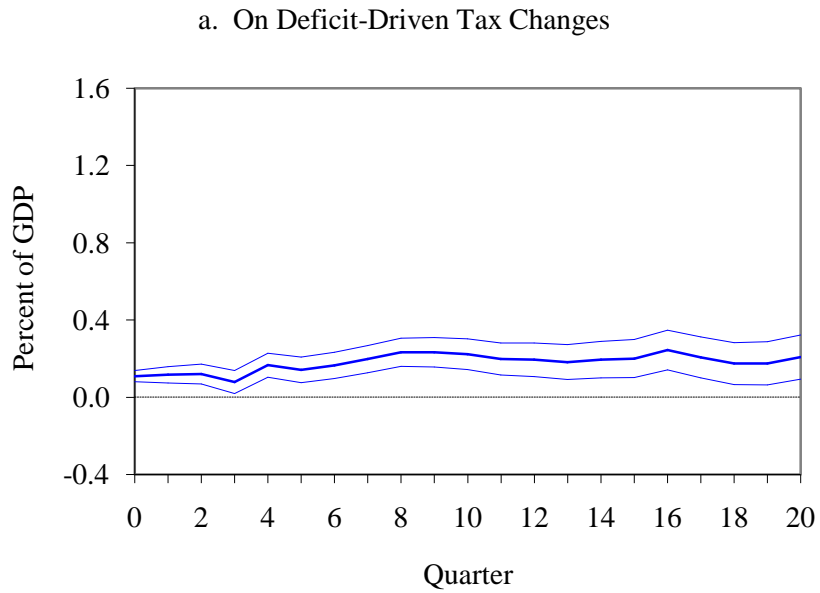
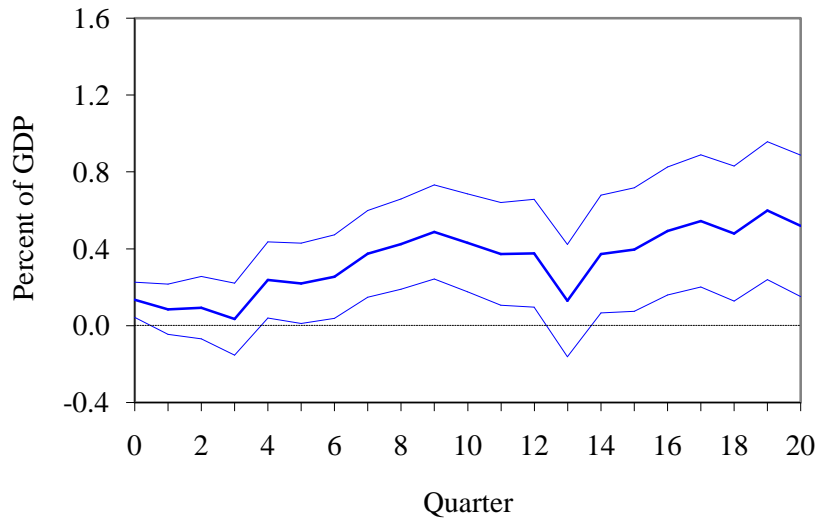
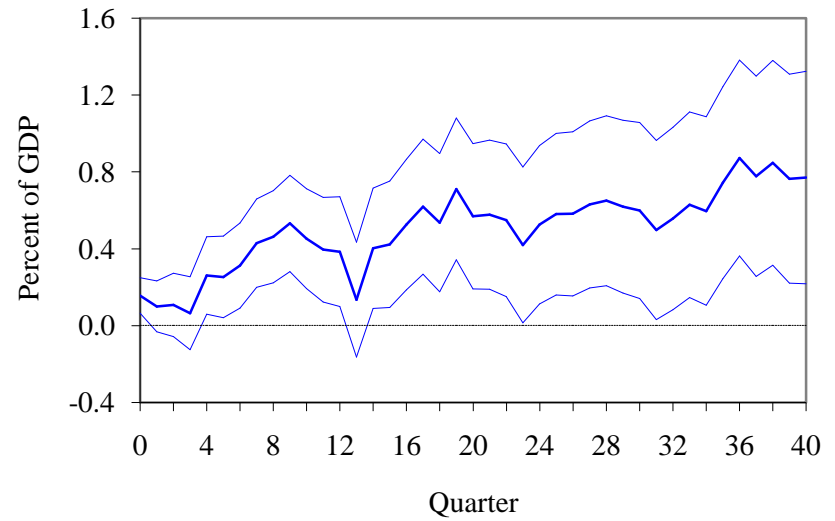


Figure 9
Estimated Impact of a Long-Run Tax Cut of 1% of GDP on All Other Tax Changes Combined Using Different Specifications

a. Starting Sample in 1957Q1



b. Including 40 Lags



c. In a Two-Variable VAR

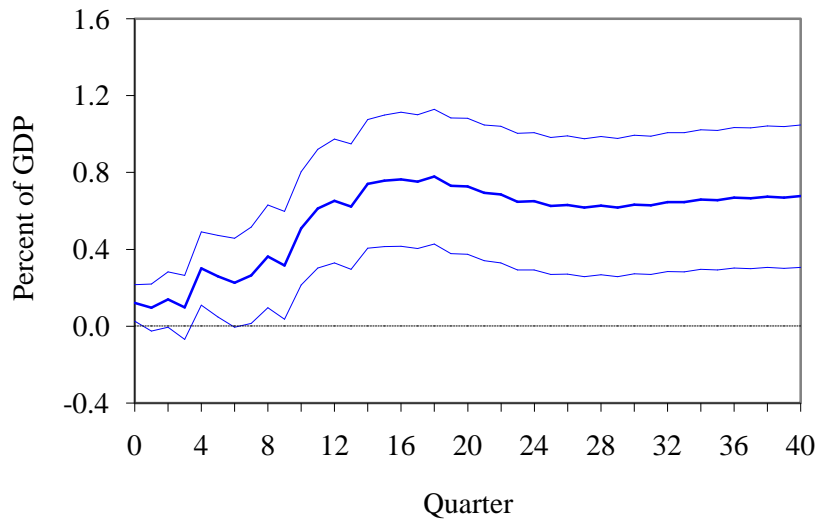
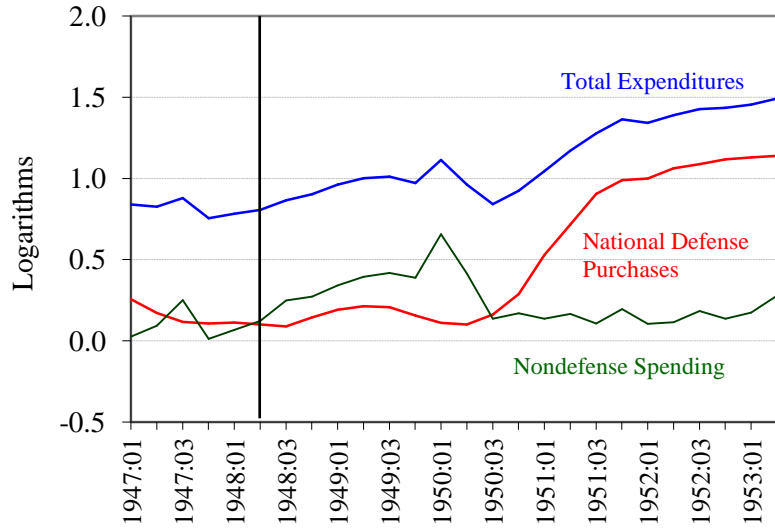
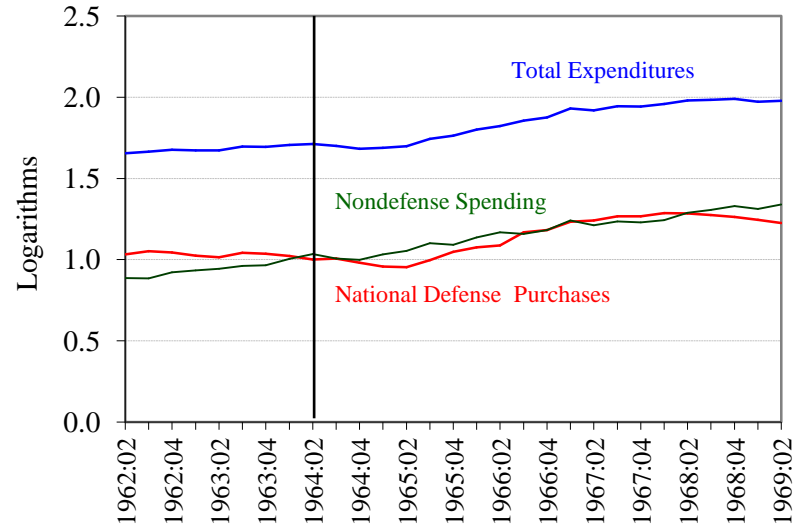


Figure 10
 Real Federal Government Expenditures after Four Key Long-Run Tax Cuts

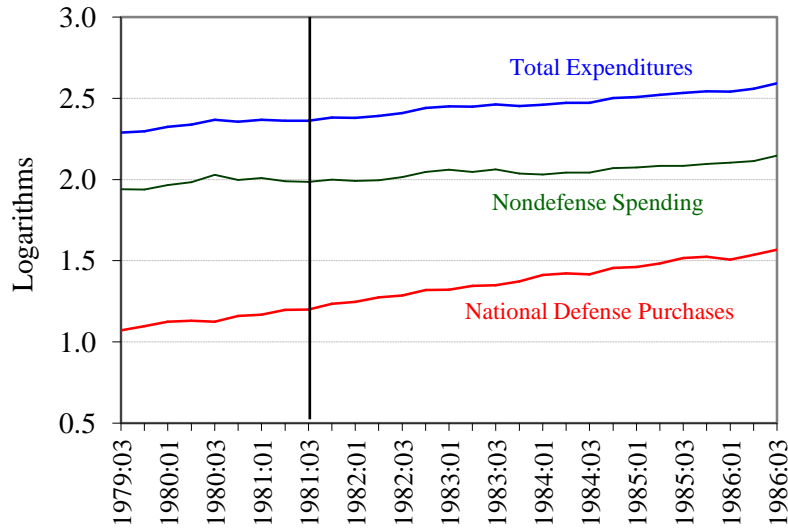
a. Revenue Act of 1948



b. Revenue Act of 1964



c. Economic Recovery Tax Act of 1981



d. EGTRRA of 2001 and JGTRRA of 2003

