

# Empirical Evidence on the Aggregate Effects of Anticipated and Unanticipated U.S. Tax Policy Shocks\*

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## Abstract

We provide empirical evidence on the effects of tax liability changes in the United States. We introduce a timing-based distinction between surprise and anticipated tax shocks. We find that surprise tax cuts have expansionary and persistent effects on output, consumption, investment and hours worked. Prior to their implementation, anticipated tax liability cuts give rise to contractions in output, investment and hours worked. In contrast, there are no significant anticipation effects on aggregate consumption. Once the anticipated tax liability cuts are implemented, they stimulate the economy in a fashion similar to the impact of surprise tax cuts. The results are shown to be very robust. We argue that tax shocks are empirically important impulses to the U.S. business cycle and that anticipation effects have been important during several business cycle episodes.

Key words: Fiscal policy shocks, tax liabilities, anticipation effects, business cycles.

JEL: E20, E32, E62, H30

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# 1 Introduction

This paper estimates the aggregate dynamic effects of tax shocks in the United States for the post World War II period. We distinguish between the impact of anticipated and unanticipated changes in taxes. This distinction is important both in theory and in practice. According to economic theory, agents base their decision rules upon their current information about variables relevant for the decision problem. Unanticipated changes in taxes affect behavior when the tax changes are implemented, while agents may wish to adjust their plans ahead of the implementation date in the case of anticipated tax changes. The idea that anticipated policy shocks impact on the economy prior to their implementation has been explored extensively in the literature on fiscal policy, see e.g. Hall, 1971, Auerbach, 1989, Yang, 2005, and House and Shapiro, 2006, for the case of tax shocks, or Blanchard, 1981, Taylor, 1993, and Ramey, 2008, for the case of government spending shocks. Taking into account that some changes in taxes are known before their implementation is also empirically relevant given that tax laws often specify changes in taxes that take place well out in the future.

Our approach to estimating the impact dynamic of tax policy shocks is geared towards assessing the impact of both unanticipated and anticipated tax shocks. We base the measurement of tax shocks upon Romer and Romer's (2007, 2008a) narrative account of legislated federal tax liability changes. Using information obtained from sources such as the Economic Reports of the President, presidential speeches and Congressional documents, these authors provide a detailed account of all significant federal tax bills during the post WWII period. We study the impact of those tax changes that according to Romer and Romer (2008a) can be assumed to be exogenous, i.e. tax changes that were not introduced due to government concerns about the state of the economy or concerns about current spending plans. This selection leaves us with 70 tax liability changes that derive from 35 different federal tax policy acts.

These data allow us to make a simple yet informative classification of tax changes into anticipated and unanticipated tax shocks. We accomplish this by introducing a timing convention. For each tax liability change we define its announcement date and its implementation date. The announcement date is assumed to correspond to the date at which the policy intervention became law (was signed by the President), while the implementation date is defined as the date at which the tax liability changes were to be implemented according to these laws. When these dates are sufficiently close together, we

classify the corresponding tax liability change as an unanticipated tax shock, while anticipated tax shocks are those changes in taxes for which the two dates differ significantly. Specifically, changes in tax liabilities that were to take place within (later than) 90 days of the law being signed by the President are classified as unanticipated (anticipated). In practice, this means that all anticipated tax shocks were implemented at least two quarters after the tax change became law. Based on this measurement, 37 out of the 70 tax liability changes are categorized as anticipated and the median anticipation horizon amongst these tax shocks is 6 quarters.

In order to derive estimates of the impact of tax shocks, we embed the tax shocks in a vector autoregression assuming that the tax liability changes are exogenous. We allow surprise tax changes to affect the endogenous variables from their implementation date, while anticipated tax changes are allowed to impact on the endogenous variables from the announcement date onwards. Our approach is akin to analyses that have examined the impact of government spending shocks using the narrative approach (e.g. Ramey and Shapiro, 1998, Burnside, Eichenbaum and Fisher, 2004, or Ramey, 2008) but with the extra feature that we distinguish between anticipated and unanticipated fiscal shocks. We study the impact of the tax shocks on aggregate output, consumption, investment, hours worked and real wages. The key findings of our analysis are:

- An unanticipated tax cut gives rise to significant increases in output, consumption, and investment which peak around 2.5 years after the introduction of the tax cut. The largest response relates to investment, which at peak increases by approximately 10 percent following a one percent cut in tax liabilities. Hours worked also increases but only gradually over time. Real wages rise persistently.
- Assuming that anticipated tax shocks are announced 6 quarters before their implementation, an anticipated tax cut is associated with a pre-implementation drop in output and investment while consumption remains roughly constant during the pre-implementation period. Once the tax change is implemented, it is associated with a stimulating effect on the economy. There is also a significant pre-implementation drop in hours worked, while real wages increase during the pre-implementation period.
- Unanticipated and anticipated tax shocks have contributed importantly to the U.S. business

cycle. The anticipation effects associated with the Social Security Amendments of 1977 and the Economic Recovery Tax Act of 1981 explain a significant proportion of the 1981-82 recession and the mid-1980's boom in the U.S. The Bush tax initiatives in the early 2000's also had important anticipation effects. Altogether, anticipated and unanticipated tax shocks account for around 20-25 percent of the volatility of output at business cycle frequencies.

These results provide novel insights on the impact of fiscal policy. First, we find a somewhat larger impact of tax shocks on aggregate output than Blanchard and Perotti (2002). This is important in terms of evaluating the effectiveness of fiscal policy and in terms of accounting for the effects of fiscal policy shocks in macroeconomic models.

Secondly, our approach provides a methodological innovation to the problem of estimating anticipation effects. Blanchard and Perotti (2002) apply an alternative approach. They identify fiscal shocks by exploiting the existence of decision lags assuming that fiscal policy variables do not adjust within a quarter to other unanticipated shocks. In order to allow for anticipation effects within such a framework, these authors point out that one needs to introduce even longer decision lags. Since the median implementation lag in our data is 6 quarters, this approach would be very problematic when estimating the dynamic impact of unanticipated and anticipated tax shocks.<sup>1</sup>

Third, we provide direct empirical evidence on the importance of anticipation effects. Preexisting evidence on such pre-implementation effects is scarce and does not provide much support of the existence of anticipation effects. A number of papers have studied the consumption response to pre-announced changes in personal taxes. Poterba (1988) tests whether aggregate U.S. consumption reacts to announcements of future tax changes and fails to find robust evidence in favor of this hypothesis.<sup>2</sup> Heim (2007) studies data from the Consumer Expenditure Survey (CEX) and tests for announcement effects of state tax rebates. He finds no significant household consumption response to rebate announcements. Parker (1999) and Souleles (1999, 2002) also study CEX data and test

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<sup>1</sup>Mountford and Uhlig (2005) estimate the impact of pre-announced fiscal policy shocks using a structural VAR approach where identification is obtained by imposing sign restrictions. They do not show, however, whether anticipation effects have been important quantitatively for fluctuations in the U.S. economy.

<sup>2</sup>Poterba (1988) identifies five such episodes: February 1964, June 1968, March 1975, August 1981, and August 1986.

whether household consumption responds to actual changes in taxes when these were known in advance of their implementation.<sup>3</sup> They find that consumption plans adjust only when tax changes are implemented. Our results agree with these studies but indicate that other key macroeconomic variables do display significant anticipation effects.

How does the anticipation lag matter for the dynamic adjustment to pre-announced tax shocks? We find that when the implementation lag is very short there are little effects of pre-announced tax changes prior to their implementation. However, anticipation effects are significant for the median implementation lag in the data. Within limits, longer implementation lags give rise to more pronounced anticipation effects on aggregate output. This insight reconciles our results with those of Blanchard and Perotti (2002) who find little evidence of anticipation effects when assuming a one-quarter implementation lag for tax shocks.

Romer and Romer (2008b) also apply a timing based distinction between anticipated and unanticipated tax shocks. They examine if news about the present value of future tax liability changes affect output and find little evidence for any such news effects. There are several differences between their estimation approach and ours. First, Romer and Romer (2008b) constraint the post-implementation impact of anticipated and unanticipated tax changes to be identical while we make no such assumption. Secondly, our analysis allows us to trace out the impact of anticipated tax shocks on the endogenous variables from the announcement date onwards, taking into account the length of the announcement horizon. In contrast, Romer and Romer's (2008b) analysis examines the impact of future tax changes irrespective of the announcement horizon. Our results show that the impact of anticipated tax shocks depend on the announcement horizon and that there are significant news effects on output, investment and hours worked when the announcement horizon corresponds to the median horizon in the data.

We carry out an extensive robustness analysis. We first examine the reliability of the timing based measurement of anticipated tax shocks. The key assumption of our analysis is that the tax liability changes became common knowledge when the tax act was signed by the President. If agents have a precise idea about tax changes before they become law, one might expect our measurements to

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<sup>3</sup>Parker (1999) examines the impact of Social Security changes during the 1980's while Souleles (2002) investigates the Reagan tax cut of the early 1980's.

be imprecise. To check this, we compare the estimated impact of pre-announced tax liability changes with the impact of leads of surprise changes in tax liabilities. If the public were well informed about tax changes before they become law, then we should find that surprise tax shocks affect the observables before their implementation. We show that this is not the case, thus giving support to our timing based distinction between anticipated and unanticipated tax shocks.

Next, we examine the sensitivity of our results to the types of tax changes that we include in the tax measure. We look at two aspects, the persistence of the tax liability change and the motivation for its introduction. The persistence of the tax liability change is an issue because our benchmark results are based on a tax shock measure that includes both tax changes that were meant to be permanent and tax changes that were temporary in nature. We show that our results are extremely robust to the elimination of tax shocks that were meant to be temporary: When we examine only permanent tax changes we find larger output effects of implemented tax shocks and even more pronounced anticipation effects of pre-announced tax changes.

The benchmark results include tax liability changes that according to Romer and Romer (2008a) were exogenous in the sense that their introduction was based either on ideological grounds or based on concerns about inherited government deficits. The latter of these motivations may be considered to be partially endogenous while the former relates more closely to a standard notion of exogenous policy changes. Therefore, we reexamine our results when we include only those changes in tax liabilities that were introduced for ideological reasons. The results estimated from this specification are very similar to the benchmark results.

Third, we investigate whether the estimation results hinge critically on particular tax laws. If this is the case, our results may simply be due to special circumstances surrounding particular fiscal interventions in the economy. The Reagan tax cut is a particular worry as it not only coincided with Volcker's chairmanship at the Fed, but also is by far the largest sequence of anticipated tax shocks in the sample that we study. We show that the results are extremely robust to removing the Reagan tax cut. The same robustness holds also if we eliminate the Kennedy or the Bush tax acts, both of which also incorporated substantial anticipated tax shocks.

Finally, we ask whether our results are contaminated by lack of control for monetary policy and for fiscal spending. This is a concern only to the extent that the tax shocks are non-orthogonal

to other structural shocks. In an infinite sample, such lack of control for other structural shocks would not be a problem, but non-orthogonality between shocks may be a concern in a finite sample. Although we study almost 60 years of quarterly data, one may thus worry that small-sample problems makes it problematic to leave out monetary policy shocks, government spending shocks, etc. We find that our results survive, and to some extent become even stronger when controlling for monetary policy shocks or for government spending shocks. Moreover, our finding that the anticipation effects associated with the Reagan tax cut were partially responsible for the early 1980's recession in the US hold true even when we control for monetary policy shocks.

The remainder of this paper is structured as follows. The next section describes our estimation approach and discusses the tax data that we use for identification of the fiscal shocks. Section 3 contains the main results regarding the dynamic effects of tax shocks. In Section 4 we carry out a substantial robustness analysis. Section 5 contains our analysis of the role of tax shocks for US business cycles. Finally, Section 6 concludes and summarizes.

## **2 Estimation Approach**

In this section we discuss how we measure tax shocks and our approach to the estimation of their impact on macroeconomic aggregates.

### **2.1 Identification**

Key to any estimation of the dynamic impact of fiscal policy shocks is the identification strategy. Recent contributions to the fiscal policy literature have adopted either structural VAR approaches or have relied upon the narrative approach. A central issue in our analysis is that we wish to control for the possible presence of anticipation effects. Blanchard and Perotti (2002) point out that allowing for anticipated shocks in a structural VAR framework where identification of fiscal policy shocks rests upon decision lags implies the need for using instrumental variables estimators and for assuming potentially very long decision lags. Specifically, identification requires decision lags to be at least as long as anticipation lags. In the data that we study, the median anticipation horizon is sufficiently long that such an estimation approach becomes very unattractive due to implausibly long decision lags. This not only means that this estimation approach is not suitable for estimating the effects

of tax shocks, but also, that if the anticipated shocks are neglected, the resulting moving average representation of the VAR may become nonfundamental (see Leeper, Walker and Yang, 2008).

We adopt instead a narrative approach to the measurement of tax shocks. The key advantage of the narrative approach is that it allows us to make a distinction between anticipated and unanticipated tax shocks based on timing assumptions. Specifically, we make use of Romer and Romer’s (2007) (updated in Romer and Romer, 2008a) narrative account of U.S. federal tax liability changes. Based on official government documents, presidential speeches, and Congressional documents, Romer and Romer (2007) identify 51 significant legislated federal tax acts in the period 1947-2006 and a total of 110 separate changes in tax liabilities. This account is therefore much richer than the standard narrative account of fiscal shocks (Ramey and Shapiro, 1998).

Some of these tax liability changes were introduced to address concerns about the state of the economy or were motivated by the need to finance of government spending plans while other changes in tax liabilities were exogenous in nature. We focus on the tax liability changes that Romer and Romer (2007) classify as exogenous since “(t)he changes were not motivated by current or projected economic conditions” (Romer and Romer, 2007, p.3). This includes tax liability changes that Romer and Romer (2007) classify as either “exogenous due to long-term growth objectives” or exogenous due to “deficit concerns”. The former of these are tax changes that were introduced with no explicit concerns about the current state of the economy while the latter are tax changes introduced to address *inherited* budget deficits. Romer and Romer (2007) make sure that the latter type of tax changes were not implemented to address concerns about the current or future state of the economy (or current or future spending plans). While this still leaves it possible that the tax changes were forecastable on the basis of past information, we find no such signs in the data that we study.

Therefore, we assume that these tax liability changes are exogenous in the sense that they are orthogonal to the current realizations of other structural shocks. This selection leaves us with 70 tax liability changes deriving from 35 different federal tax policy acts listed in Table A.1.

## **2.2 Distinguishing Between Anticipated and Unanticipated Tax Shocks**

We adopt a timing based distinction between unanticipated and anticipated tax shocks. For each tax liability change we define two dates. The first date is the announcement date which we assume

corresponds to the date at which the tax legislation became law, i.e. when it was signed by the President. The second date is the implementation date - the date at which, according to the legislation, the tax liability changes were to be introduced.<sup>4</sup> We refer to the difference between these two dates as the implementation lag.

We define a tax liability change as anticipated if *the* implementation lag exceeds 90 days. We use a 90 day window because it strikes a balance between robustness of the results to the date within a quarter that a tax change became law, and the ability to measure anticipation effects. Using a much shorter window makes the classification too sensitive to timings within quarters while a much longer window makes the distinction between surprise and anticipated shocks less lucid. Moreover, the results are robust to moderate changes in the size of this window. Figure 1 illustrates the histogram of the implementation lag (in days) for the data that we examine. As is clear, the implementation lag is twin peaked with the peaks occurring at 0-30 days and at more than 151 days. Only 18 of the 70 tax liability changes have implementation lags in the 30-150 days interval. Therefore, there is little impact on our results of minor changes in the size of the window that we use to distinguish between surprise tax changes and anticipated tax changes.

Based on our use of a 90 day window, 36 out of the 70 of the tax liability changes are anticipated while 34 tax liability changes are defined as surprise tax shocks. The resulting tax shocks are illustrated in Figure 2 in percentages of annual current price GDP. The top panel shows the unanticipated shocks, the middle panel shows the anticipated shocks dated by the quarter of implementation, and the bottom panel reports the anticipation horizon of the anticipated tax shocks (truncated at 4 years).

As is evident, tax shocks are quite frequent and some of these changes are quite large. The Reagan tax initiative, the Economic Recovery Tax Act of 1981 (ERTA 1981 from now on) , signed by Reagan in August 1981, contained a substantial amount of pre-announced tax changes. It consisted of five separate changes in tax liabilities due in 1981:3, 1981:4, 1982:1, 1983:1, and 1984:1. The first two tax changes are defined as surprise changes according to our taxonomy while the last three initiatives are

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<sup>4</sup>In cases where the tax bill is advanced by the President, the bill becomes law when passed by the Congress. This date may occur slightly earlier than the bill being signed by the President. However, given that we introduce a 90 day minimum anticipation horizon, this detail should not affect our results.

defined as anticipated policy changes. The change in tax liabilities of these latter three anticipated changes corresponds to approximately 4.5 percent of annual GDP. This sequence of tax cuts as a whole constitutes by far the largest anticipated tax changes in the sample that we study.

The median implementation lag in the data is 6 quarters. The longest implementation lag is associated with the Social Security Amendments of 1983 signed by Reagan in April 1983 which had tax liability changes being introduced as far out in the future as 1990. Most other pre-announced tax changes, however, had implementation lags close to the median lag. Out of the 36 anticipated tax liability changes, 27 have anticipation horizons within 4 quarters of the median anticipation lag and only 5 of them have anticipation lags longer than 4 years.

An important assumption for our analysis is that the date at which the public becomes informed about the changes in tax liabilities coincides with the date at which the legislations are signed by the President. This is also the approach taken in the microeconomic literature that has measured the consumption impact of pre-announced tax changes and in Poterba's (1988) analysis of the consumption response to pre-announced tax changes. Potentially, one might think of more sophisticated approaches to the measurement of the announcement dates taking into account promises made during election campaigns, lags in the democratic process of passing tax legislations, etc. However, the advantage of the approach that we take here is that the measurement is consistent across all tax liability changes and it is easily reproducible. Nevertheless, in the robustness analysis we take a closer look at its plausibility by examining if there are signs that the unanticipated tax shocks have any impact before their implementation. We do not find any signs of such anticipation effects of the surprise tax shocks.

Another issue relates to the timing of the announcements of anticipated tax shocks. If the announcements are systematically related to the state of the business cycle, it indicates endogenous timing irrespective of whether the tax changes as such were introduced for ideological reasons. We check this by examining how the announcement dates relate to business cycle turning points. Specifically, we calculate the average number of quarters between announcements of future tax changes and the closest NBER business cycle turning point. This statistic is 5.7 quarters in the data. Assuming that tax announcements are random so that they occur with the same probability at all dates gives an expected value of this statistic of 5.8 quarters. Thus, we find no signs of a systematic relationship

between announcements of future tax changes and business cycle turning points.

### 2.3 Measurement of Tax Shocks

We measure surprise tax liability changes in terms of the implied dollar change in tax liabilities as a percentage of current price GDP at the implementation date. This tax measure aggregates tax liability changes over many different types of taxes. However, it is a meaningful measure since it translates the federal tax liabilities of the U.S. economy into an average tax rate. It would potentially be interesting to translate the tax liability changes into changes in specific taxes but here we concentrate on the coarser measure since it gives us a rich amount of data to examine. We denote the resulting surprise tax changes measure by  $\tau_t^u$ .

Anticipated tax changes are distinguished by size, the announcement date, and by the anticipation horizon. Let  $s_t^{a,i}$  denote tax liability changes that were signed by the President at date  $t$  and had an anticipation horizon of  $i$  quarters measured as a percentage of GDP at the implementation date. Ideally, we would like to allow for differential effects of tax liabilities that had different anticipation horizons. For example, this would allow one to distinguish between a tax liability change announced at date  $t$  with  $i$  quarters of anticipation and a tax change announced at date  $t + 1$  with  $i - 1$  quarters of anticipation. Directly adopting this approach implies that the information set at date  $t$  needs to include the vector of anticipated tax shocks  $\left[ s_t^{a,i=1,\dots,M}, s_{t-1}^{a,i=2,\dots,M}, s_{t-M}^{a,M} \right]$  where  $M$  denotes the largest implementation lag in the data. The large dimension of this vector makes estimation difficult due to the implied loss of degrees of freedom. For example, for  $M = 10$ , 55 parameters pertaining to the anticipated tax shocks need to be estimated for each of the endogenous variables (and for  $M = 20$ , the largest anticipation horizon in the data, 210 parameters need to be estimated).

To address this issue, we distinguish between anticipated tax shocks on the basis of their *remaining* anticipation horizon. We define the following anticipated tax shocks:

$$\tau_{t,i}^a = \sum_{j=0}^{M-i} s_{t-j}^{a,i+j} \quad (1)$$

Thus,  $\tau_{t,i}^a$  measures the sum of all anticipated tax liability changes known at date  $t$  to be implemented at date  $t + i$ . Using this definition of the anticipated tax shocks implies that the number of anticipated tax variables that enter the information set at date  $t$  is equal to  $M$ , making estimation

feasible. We later investigate the sensitivity of the results to assumptions regarding the maximum anticipation horizon.

## 2.4 Estimating the Impact of Tax Liability Changes

Given the estimates and classification of the tax shocks, we estimate the impact of tax shocks on the basis of impulse response functions. These impulse response functions are derived from the following regression model:

$$X_t = A + Bt + C(L)X_{t-1} + D(L)\tau_t^u + F(L)\tau_{t,0}^a + \sum_{i=1}^K G_i\tau_{t,i}^a + e_t \quad (2)$$

where  $X_t$  is a vector of endogenous variables,  $A$  and  $B$  control for a constant term and a linear trend,  $C(L)$  is  $P$ -order lag polynomial, and  $D(L)$  and  $F(L)$  are  $(R+1)$ -order lag polynomials.<sup>5</sup> We allow the maximum anticipation horizon in equation (2),  $K$ , potentially to differ from the maximum implementation lag observed in the data,  $M$ .

This regression model can be viewed as a vector autoregression for  $X_t$ , treating the tax variables as exogenous shocks. Since we do not include actual tax rates in the vector  $X_t$ , in order to allow for persistence in the tax liability changes the VAR includes moving average terms of *implemented* tax liability changes,  $\tau_t^u$  and  $\tau_{t,0}^a$  (the  $D(L)$  and  $F(L)$  lag polynomials).<sup>6</sup>

The anticipation effects of pre-announced tax liability changes are introduced through the terms  $G_1 - G_K$ . Notice that, since these coefficients enter on the tax liability changes that are part of the information set at date  $t$  but not yet implemented, these terms directly measure news effects associated with anticipated tax shocks. Therefore, the empirical model allows us to trace out the dynamics of the economy from when tax changes are announced.

Our treatment of the tax shocks contrasts with the standard “dummy variable” measurement of the policy interventions usually adopted in the narrative approach, see e.g. Ramey and Shapiro

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<sup>5</sup>The results are robust to allowing for a break in the trend in 1973:2, see Ramey and Shapiro (1998) and Burnside, Eichenbaum and Fisher (2004). The results are also robust to first differencing the  $X_t$  vector.

<sup>6</sup>Alternatively, one might include estimates of actual tax rates in the VAR, see e.g. Burnside, Eichenbaum and Fisher (2004), but in our application this would require one to introduce a mapping between estimates of average marginal tax rates and tax liability changes.

(1998) or Burnside, Eichenbaum and Fisher (2004).<sup>7</sup> The advantage of our approach is that it allows us to aggregate the evidence across different episodes of tax liability changes. This seems a sensible strategy given that we have quite a large number of tax shocks, and given that their measurement can be interpreted in terms of changes in average taxes. Romer and Romer (2008b) adopt the same strategy to the measurement of the tax policy shocks.

One motivation for the formulation of the regression model is that it can be viewed as an approximation to the dynamics of the observables in a dynamic stochastic general equilibrium model with stochastic shocks to tax rates, see Mertens and Ravn (2009). The approximation consists of restricting  $D(L)$  and  $F(L)$  to be finite order polynomials (as opposed to infinite order polynomials). Mertens and Ravn (2009) show that as long as  $R$  is sufficiently high, the approximation is quite precise and that the VAR provides a very good fit of the dynamics of a DSGE model.

We study U.S. quarterly data for the sample period 1947:1 - 2006:4. In all our experiments, the vector  $X_t$  includes the following variables:

$$X_t = \begin{bmatrix} y_t, & c_t, & i_t, \end{bmatrix}'$$

where  $y_t$  denotes the logarithm of U.S. GDP per adult in constant (chained) prices,  $c_t$  is the logarithm of the real per capita private sector consumption expenditure,  $i_t$  is the logarithm of real aggregate per capita gross private sector investment (see Appendix 2 for precise definitions and data sources). We also estimate the impact of the tax shocks on hours worked and on real wages by adding these variables sequentially to the vector of observables. This strategy requires the estimation of a fewer number of parameters than estimating the larger dimensional VAR directly, see also Burnside, Eichenbaum and Fisher (2004).<sup>8</sup>

The VAR above assumes that the tax shocks have persistent but non-permanent effects on the vector of observables (under the condition that the lag-polynomial  $C(L)$  does not contain unit roots). We also checked the results when allowing for permanent effects of the tax shocks using a VAR in first differences. The results are very similar to those that we derive with the VAR in equation (2) and are therefore not reported.

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<sup>7</sup>See Perotti (2007) for an insightful discussion of the narrative approach to fiscal policy.

<sup>8</sup>Mertens and Ravn (2008) estimate a larger dimensional VAR which also includes consumer durables. The results are for all intents and purposes identical to those reported here.

### 3 Empirical Results

We assume that  $K = 6$  which corresponds to the median implementation lag in the data that we study, that  $R = 12$ , and that  $P = 1$  (the results are robust to assuming longer lag structures). We report the impulse response functions to a 1 percent decrease in the tax liabilities (relative to GDP) along with 68 percent non-parametric non-centered bootstrapped confidence intervals computed from 10000 replications. The impulse response functions are shown for a forecast horizon of 24 quarters for unanticipated tax liability shocks, and for 6 quarters before its implementation, to 24 quarters thereafter in the case of anticipated shocks.

#### 3.1 Benchmark Results

Figure 3 reports the benchmark results. The left column shows the impact of an unanticipated tax liability cut. The tax cut sets off a major expansion of the economy. According to our estimates, a 1 percent tax cut is associated with a 2 percent peak increase in GDP per capita. The increase in output occurs gradually over time and the peak is reached 10 quarters after the cut in taxes. Thereafter, output remains above trend for an extended period (that goes beyond the 6 years forecast horizon illustrated in Figure 3). The increase in output is significantly positive at all but the second and third quarter and the confidence interval is narrow at all forecast horizons.

Private sector consumption also increases and the size of its peak response is similar to the size of the peak response of output, corresponding to a 2 percent increase above trend. Consumption, however, tends to increase slightly faster than output, and 6 years after the tax cut, consumption is still around 0.75 percent above trend. The increase in consumption is significantly positive at all forecast horizons.

By far the most elastic response is associated with private sector investment, which we estimate to increase by as much as 10 percent relative to its trend. As for output, the peak increase occurs 10 quarters after the cut in taxes. This indicates a large supply side impact of tax changes. One noticeable feature of the investment dynamics is that while its peak response is much larger than that of output and consumption, the response is also significantly less persistent.

The impact on hours worked, instead, is estimated to be close to zero until around a year and a half after the change in taxes. After that, hours worked increases gradually and its peak response,

which occurs 10 quarters after the tax cut, corresponds to approximately a 1 percent increase above trend. This gradual increase in hours worked indicates a much less significant labor market impact of tax shocks than the impact on goods markets. Another aspect of the labor market impact is the effect on real wages. The real wage response to fiscal policy shocks is often seen as a litmus test of business cycle theories because it reflects the movements in labor supply relative to labor demand. We find that (before tax) real wages rise gradually but very persistently after a cut in taxes.

Our estimates of the impact of unanticipated tax liability changes on output are similar to the univariate results of Romer and Romer (2008b) who find large and protracted output responses to changes in tax liabilities. The shape of the responses are similar to the impact of a “basic government revenue shock” estimated by Mountford and Uhlig (2005). Relative to the estimates of Blanchard and Perotti (2002), the response of output to tax liability shocks occurs more gradually than the output response to the tax shocks that these authors identify with a structural VAR approach. However, our results are similar to theirs in terms of the persistence of the output response.

The right column of Figure 3 illustrates the impulse responses to an anticipated tax cut (assuming a 6 quarter anticipation period). We find strong evidence of anticipation effects. During the pre-implementation period, an anticipated tax cut gives rise to significant declines in output, investment, and in hours worked. The drop in aggregate output reaches its maximum 1 year before the implementation of the tax cut where output is estimated to be 1.5 percent below trend. The most dramatic anticipation effects relate to the impact on aggregate investment. The pre-implementation impact on aggregate investment 4 quarters before the tax is implemented corresponds to a 4 percent drop below trend. The anticipation effect on hours worked is also very pronounced, with hours worked dropping by up to 2 percent below trend and remaining below trend throughout the pre-implementation period.

The impact of the tax cut announcement on consumption and real wages are quite different from the effects on output, investment and hours worked. We find that the real wage if anything increases during the pre-implementation period, while consumption remains roughly unaffected by the tax announcement apart from a temporary drop 5 quarters before the tax cut is implemented. The impact on real wages makes it unlikely that the estimates are significantly contaminated by the lack of control for other structural shocks such as monetary policy shocks, a point that we will return to.

Once taxes are cut, the dynamics of the variables resemble the dynamics that follow after an unanticipated tax cut. Peak effects, however, occur slightly earlier for anticipated tax cuts. Output starts rising 3 quarters after the tax cut is implemented and reaches a peak increase that corresponds to a 1.5 percent rise above trend 8 quarters after the implementation of the tax cut. Investment starts rising 4 quarters before the tax cut and reaches a 10 percent peak rise over trend 9 quarters after the implementation date. Hours worked rise slowly but, eventually, about a year after the tax cut is implemented, hours worked expands 1 percent above trend while the increase in the real wage during the pre-implementation period persists after taxes are cut.

The lack of an anticipation effect on consumption is in line with previous contributions to the literature. Poterba (1988) and Heim (2007) fail to derive a significant consumption response to announced future tax cuts while Parker (1999) and Souleles (2002) find that consumption reacts to the implementation of pre-announced tax changes.

However, as we have shown, the lack of anticipation effects on consumption is not indicative of the absence of adjustments to pre-announced changes in future taxes. Instead, we find strong pre-implementation effects on output, investment, and hours worked. Romer and Romer (2008b) examine whether the expected present value of future not yet implemented tax changes affects the current level of aggregate GDP. They find only mild evidence in favor of expectational effects. An advantage of our approach is that we analyze the full path of the adjustment of the economy from the time when the tax liability changes are announced until several quarters after its implementation. This gives us a sharp characterization of the anticipation effects. In contrast, Romer and Romer's (2008b) analysis examines whether the announcement of future tax changes (regardless of the anticipation horizon) have an impact on output before their implementation. They find only mild evidence of news effects whereas our approach shows quite clearly that there are important anticipation effects when we take into account the anticipation horizon.<sup>9</sup>

Mountford and Uhlig (2005) identify the impact of a pre-announced government revenue shock

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<sup>9</sup>There are several other differences between our analysis and that of Romer and Romer (2008b). We control for the interrelationships between the vector of endogenous variables, while Romer and Romer (2008b) carry out univariate regressions. Our analysis allows for a different impact of implemented changes in unanticipated and anticipated tax shocks while Romer and Romer (2008b) restrict these to have identical impact apart from the possible impact of lagged news.

using an “ex-post” identification approach based on sign restrictions. In particular, they examine the impact of a government tax revenue shock that takes place one year out in the future with the restriction that the shock is orthogonal to “business cycle” shocks and monetary policy shocks. In contrast to our findings, they find that a pre-announced revenue increase is associated with a pre-implementation increase in output, while their estimates of the impact on investment agree with our results. Their identification strategy is fundamentally different from ours as they do not include currently available information about future tax liability changes. For that reason, it is perhaps not surprising that they find a different impact of pre-announced fiscal policy shocks.<sup>10</sup>

Our results appear consistent with strong supply side effects of tax changes. The strong decline in investment and the drop in hours worked in response to a pre-announced tax cut is consistent with the idea that future lower taxes motivate firms to delay purchases of capital goods and gives rise to intertemporal substitution of labor supply. Indeed, Mertens and Ravn (2009) show that a DSGE model can account quite precisely for the dynamics of output, investment, and hours worked that follow after unanticipated and anticipated changes in taxes. Their analysis also shows that the lack of a strong consumption response to a pre-announced tax cut is consistent with a model with no liquidity constraints when one takes into account complementarities between consumption of nondurables and services and consumption of durable goods.

### 3.2 Sensitivity to the Anticipation Horizon

The above analysis assumes pre-announced tax changes can impact on  $X_t$  from a maximum of 6 quarters before their implementation. This length of the anticipation period corresponds to the median anticipation lag in the data. However, there is quite some variation in the anticipation lags and it is therefore interesting to examine how the results depend on the assumed anticipation horizon.

Figure 4 illustrates the impact of an anticipated tax liability cut on aggregate GDP when we vary the maximum anticipation horizon, between 4 and 10 quarters. In each case, we reestimate equation

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<sup>10</sup>Moreover, as discussed by Leeper, Walker and Yang (2008), their identification is applied to government tax revenue rather than to tax liabilities relative to GDP. They assume that government revenue remains constant from the announcement date until the implementation date. Thus, to the extent that tax revenue is derived from income taxation, the pre-implementation increase in output that they estimate in response to a future tax revenue increase implies that tax rates must adjust during the pre-implementation period.

(2) for  $K = 4, 6, 8, 10$ , and then plot the resulting impulse response function. Regardless of the assumed anticipation horizon, the pre-implementation period is characterized by a drop in output and, once the tax cut is implemented, the tax cut stimulates the economy. However, the depth of the pre-implementation downturn and the size of the post-implementation expansion are sensitive to  $K$ . In particular, the longer the assumed maximum anticipation horizon (within reasonable limits), the deeper is the pre-implementation downturn and the milder is the post-implementation expansion.<sup>11</sup>

The sensitivity of the anticipation effects to the assumed length of the maximum anticipation horizon reconciles our findings with those of Blanchard and Perotti (2002) who find little evidence of anticipation effects, but allow only for a one quarter anticipation horizon. Our results indicate that for longer, and empirically relevant anticipation horizons, there are significant pre-implementation effects of pre-announced tax liability changes.

### 3.3 Anticipation Effects of Surprise Tax Changes

Our distinction between anticipated and unanticipated tax shocks is based on the difference in the timing between tax laws being signed by the President and the implementation of the tax liability changes according to these laws. The advantage of this approach is that it is simple and easily reproducible. On the other hand, the identification scheme may be viewed as too simplistic since it does not take into account that the private economy might have perceived the introduction of tax bills even before they became law. Such perceptions might occur due to promises made during election campaigns, commitments made in speeches, policy announcements etc. If such considerations are significant, our results regarding the existence of anticipation effects and the differential impact of anticipated and unanticipated tax shocks would be questionable.

To investigate this issue, we examine whether there is any evidence of systematic responses to leads of surprise tax shocks. If the public had good information about these shocks before their implementation, then we should find that these “anticipated surprise tax shocks” lead to much the same adjustment of the economy as the tax shocks that we identify as anticipated on the basis of

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<sup>11</sup>We find roughly the same results alternatively estimate the anticipation effects for, say,  $K = 10$ , and plot the impulse responses for anticipated shocks under alternative assumptions regarding their anticipation lag.

the timing convention. We estimate the following model:

$$X_t = A + Bt + C(L)X_{t-1} + D(L)\tau_t^u + \sum_{i=1}^K G_i\tau_{t,i}^a + \sum_{i=1}^K H_i\tau_{t+i}^u + e_t \quad (3)$$

This specification allows for the possibility that there are anticipation effects of surprise tax shocks. We estimate this relationship assuming, as above, that  $K = 6$ . Figure 5 illustrates the impact of a one percentage point decrease in  $\tau_{t+6}^u$  on output and on investment. For comparison, this figure also shows the estimates of the impact of anticipated tax changes from this regression.

We note first that the estimates of anticipated tax shocks are for all intents and purposes identical to the baseline results. But most importantly, leads of unanticipated tax shocks have no effects upon output or investment and the dynamics of these two variables differ markedly from their responses to anticipated tax shocks during the pre-implementation period. Thus, there is no evidence to support the view that surprise tax changes were systematically anticipated by the private sector. This evidence is supportive of our timing based assumptions regarding the measurement of anticipated tax shocks.

## 4 Robustness Analysis

In this section we investigate the robustness of the findings of the previous section. We examine whether the results hinge critically on particular tax acts, whether the results depend on the perceived persistence of the tax shocks, and whether the results are sensitive to the inclusion of tax shocks that Romer and Romer (2007, 2008a) categorize as exogenous due to “deficit concerns”. Most importantly, we also investigate whether the results derive from the fact that we do not control for other structural shocks.

### 4.1 Stability Across Tax Acts

One possible weakness of our estimation approach is that the results may derive from particular tax acts rather than being robust across the tax acts. This is an important issue since high sensitivity to particular tax acts would imply that the results cannot be viewed as general, but derive from features special to particular tax acts such as the economic circumstances under which they were introduced.

Under the maintained assumption that the tax shocks are exogenous, we can address this issue in a straightforward manner. In particular, if the tax shocks are exogenous, the estimates of the impact of tax shocks should be insensitive to the elimination of particular tax acts from the data. There are three tax acts that stand out as particularly important: The Kennedy tax initiative (the Revenue Act of 1964, RA 1964 in short), the Reagan tax act (ERTA 1981), and the Bush tax initiatives (the Economic Growth and Tax Relief Reconciliation Act of 2001 and the Jobs and Growth Tax Relief Reconciliation Act of 2003, EGTRRA 2001 and JGTRRA 2003).

We evaluate the stability of the results by reestimating the impact of tax shocks from the following VAR:

$$X_t = A + Bt + C(L)X_{t-1} + D(L)\hat{\tau}_t^u + F(L)\hat{\tau}_{t,0}^a + \sum_{i=1}^K G_i\hat{\tau}_{t,i}^a + e_t$$

where  $\hat{\tau}_t^u$  and  $\hat{\tau}_{t,i}^a$  denote the tax shocks when we eliminate the tax liability changes (i.e. when we set these tax shocks equal to 0) associated with either the Kennedy tax act, the Reagan tax act, or the Bush tax acts.

The results for output, investment and real wages are reported in Figure 6. The results are very robust. Exclusion of RA 1964 or ERTA 1981 has little effect at all. In particular, the resulting impulse responses of output and investment are all well inside the confidence interval of the benchmark estimates, with the exception of the response of output to unanticipated tax shocks when we eliminate ERTA 1981. When the latter tax act is excluded, we find a smaller output response to unanticipated tax shocks but the remaining results are not affected. Eliminating EGTRRA 2001 and JGTRRA 2003 has little consequence for the estimates of the effects of anticipated tax shocks but gives rise to somewhat larger impact of unanticipated tax shocks on aggregate output and investment.

Thus, our results regarding anticipation effects are robust to the elimination of specific tax acts. We do find some sensitivity of the size of the impact of surprise tax shocks to the elimination of specific tax acts, but qualitatively our results are very stable.

Figure 6 also reports the impact on the real wage. We note that we find a consistent tendency for the real wage to increase during the pre-implementation period in response to an anticipated tax cut. This result is important because it addresses indirectly a concern that the pre-implementation drop in activity that we estimate derives from lack of control for monetary policy variables (an issue that we examine in detail below). In particular, since the ERTA 1981 coincides with the Volcker disinflation,

one might worry that our results are sensitive to the omission of monetary policy variables. However, not only do we find that the results are robust to eliminating this tax act, but the rise in the real wage also appears at odds with this hypothesis. The reason is that standard monetary models would predict that the real wage should decline in response to a contractionary monetary policy shock (such as the Volcker experiment) due to the ensuing increase in the markup. We find instead a rise in the real wage which lends no support to this hypothesis.

## 4.2 Permanent vs. Temporary Tax Changes

The benchmark results reported in the previous section aggregates together tax liability changes that were meant to permanent and tax changes meant to be temporary. In theory one would like to treat these tax changes separately although the distinction in practice is less clear-cut since tax changes that were meant to be permanent have occasionally been reversed subsequently (and in some cases, tax changes meant to be temporary have subsequently been extended).

Nevertheless, it is of interest to examine whether this distinction matters for the results. Table A.1 records the persistence of the tax liability changes and we note that eliminating temporary tax changes corresponds to removing in total 10 tax liability changes from the sample out of which the Jobs and Growth Relief Tax Reconciliation Act of 2003 is the most important one. Figure 7 reports the responses of aggregate output, consumption and investment to the tax shocks when we consider only those tax changes that were explicitly meant to be permanent. The results are qualitatively very similar to those that we reported in Section 3. Quantitatively, we find a larger response of output and its components to surprise tax changes when we consider only permanent tax changes, a finding that appears natural. The impact of anticipated tax shocks is also quite similar to the benchmark results, although we now find some evidence to suggest a pre-implementation drop in private sector consumption. Nevertheless, as in the benchmark results, the anticipation effects are much more pronounced for output and investment than for consumption.

Thus, we find no evidence to suggest that our results are driven by the inclusion of temporary tax changes.

### 4.3 The Impact of Long-Term Growth Objective Tax Shocks

Throughout we have made the assumption that the tax liability changes can be considered exogenous. As pointed out earlier, there is no systematic relationship between the announcements of future tax changes and the state of the business cycle. However, one may still wonder whether the tax liability changes that we assume exogenous may be partially endogenous. We now wish to examine this issue in some more detail.

The tax liability changes that form the basis of our benchmark estimates consist of changes in tax liabilities that according to Romer and Romer (2007, 2008a) were “exogenous due to long-term growth objectives” or exogenous due to “deficit concerns”. We believe that there is little reason to question the exogeneity of the former of these, while the latter may be more susceptible. In particular, it is possible that tax shocks exogenous due to “deficit concerns” may not be orthogonal to past values of the vector of observables. We find no direct evidence of such predictability but this might be due to non-linearities that linear probability tests fail to uncover. Therefore, to examine whether endogeneity may be driving our results, we simply repeat our analysis using only tax liability changes that were “exogenous due to long-term growth objectives” (see Table A.1 for the classification of the tax shocks according to the motivation of their introduction).

Figure 8 shows the impulse responses for this alternative tax shock measure. When we eliminate tax liability changes that were due to “deficit concerns”, we find a slightly smaller impact of surprise tax changes although the estimates are within the confidence intervals of the benchmark estimates at most forecast horizons.

Perhaps more importantly, the finding that anticipated tax shocks give rise to a pre-implementation drop in output and in investment is, if anything, even stronger when we consider only tax shocks that were introduced for long-term growth objectives. We now find a 7 percent drop in aggregate investment 4 quarters before the implementation of the tax cut as compared to 4 percent in the benchmark case. These results are very important as they indicate that the anticipation effects do not arise from endogenous fiscal policy responses to the state of the economy. Thus, we conclude that the results regarding the presence of anticipation effects are very robust.

## 4.4 Other Structural Shocks

Finally, we consider whether our results are affected by the lack of control for other structural shocks. We wish to emphasize that lack of control for other structural shocks is only a concern due to small sample considerations. In particular, given the exogeneity assumption regarding the tax shocks, the benchmark results should not be contaminated in an infinite sample. However, in a small sample, it is possible that tax shocks may not be orthogonal to other structural shocks even if these shocks are orthogonal in the population.

### 4.4.1 Monetary Policy Shocks

The concern about the sensitivity of our results to the omission of other structural shocks is probably especially important in terms of monetary policy shocks. In particular, one of the largest sequence of tax shocks in our sample, the Reagan tax cuts, coincides with an important monetary policy invention, the Volcker disinflation. According to Goodfriend and King (2005), the deliberate disinflationary period of the Volcker chairmanship took its course during the period November 1980 to June 1982. During this period, interest rates were hiked and by mid-1981 the federal funds rate peaked at 19 percent while the Tbond rate peaked a few months later at more than 15 percent. Thus, when Reagan signed the ERTA in August 1981, it was in the midst of the Fed's attempts to stabilize inflation.

Therefore, it is possible that the anticipation effects that we have estimated arise from the lack of control for monetary policy. We have already provided evidence above in Section 4.1 that eliminating ERTA 1981 from the sample does not substantially alter the results. Therefore, we are skeptical about contamination during this particular experiment, but this does not exclude the possibility that there is a more systematic problem when omitting controls for monetary policy.

In order to address this issue we augment the VAR with variables that allow us to control for monetary policy shocks and introduce identifying assumptions that allow us to derive estimates of the monetary policy shocks and their effects. We estimate the following VAR:

$$\tilde{X}_t = A + Bt + C(L)\tilde{X}_{t-1} + D(L)\tau_t^u + F(L)\tau_{t,0}^a + \sum_{i=1}^K G_i\tau_{t,i}^a + e_t \quad (4)$$

where  $\tilde{X}_t$  consists of the following vector of variables:

$$\tilde{X}_t = [y_t, i_t, z_t, r_t, n_t]'$$

$z_t$  is a vector of variables consisting of the (annualized) inflation rate, and the commodity price inflation rate,  $r_t$  is the federal funds rate, and  $n_t$  denotes non-borrowed reserves (see the Appendix for precise definitions and sources). Given lack of data for non-borrowed reserves for the earliest part of the sample, we restrict the sample period to 1959:1-2006:4. This VAR is estimated using 4 lags of the vector of endogenous variables. This introduces a large number of parameters to be estimated and for that reason we eliminate consumption from the VAR. For the same reason, we set  $R = 6$ .

We assume that the interest rate is affected contemporaneously by all the variables in the VAR apart from non-borrowed reserves. We also allow the interest rate to react contemporaneously to innovations to the tax shocks. The variables ordered before the interest rate in the VAR (i.e. all but non-borrowed reserves) are assumed not to react contemporaneously to monetary policy shocks while non-borrowed reserves is not constrained this way. These assumptions identify the monetary policy shock as the innovation to the federal funds rate given a Cholesky decomposition of the covariance matrix of  $e_t$ , see Christiano, Eichenbaum and Evans (1996).

Figure 9 illustrates the impact of the tax shocks on output, investment, inflation, and the federal funds rate. The results are very similar to the benchmark estimates. A surprise tax cut is associated with an expansion in aggregate output, consumption, and investment. Interestingly, we find that the federal funds rate initially decreases in response to the surprise cut in taxes but then eventually returns to its normal level 1.5-2 years after the tax cut. The initial fall in the interest rate is due to the associated initial fall in the inflation rate which outweighs the expansion of output as far as monetary policy is concerned.

As in the benchmark estimates, the anticipated tax cut gives rise to a pre-implementation drop in aggregate output and investment as well as in the inflation rate and the nominal interest rate. The associated drop in aggregate investment during the pre-implementation period is slightly larger than in the benchmark estimates of Section 3 while the output drop is slightly smaller. Once taxes are eventually cut, we find that the tax cut stimulates aggregate output and investment. Relative to the benchmark estimates, the expansions in output and investment are slightly smaller and a bit less persistent when we control for monetary policy shocks.

All in all, we find a remarkable degree of robustness of the benchmark results. Thus, it does not appear to be the case that our estimates of the impact of tax policy shocks are contaminated by the lack of control for monetary policy variables.

#### 4.4.2 Government Spending

Our analysis considers changes in taxes but one might worry about possible correlations with other fiscal policy shocks. In particular, it is possible that the results are sensitive to in-sample correlation with shocks to fiscal spending. We address this issue in the same fashion as above by controlling directly for government spending shocks.

We estimate the following VAR:

$$\widehat{X}_t = A + Bt + C(L)\widehat{X}_{t-1} + \sum_{i=1}^4 H(L)W_{it} + D(L)\tau_t^u + F(L)\tau_{t,0}^a + \sum_{i=1}^K G_i\tau_{t,i}^a + e_t \quad (5)$$

where  $\widehat{X}_t$  is the following vector of variables:

$$\widehat{X}_t = [g_t, v_t, y_t, c_t, i_t]'$$

The variable  $g_t$  denotes gross federal purchases of goods and services, and  $v_t$  denotes federal revenues (both measured in real per capita terms, see the appendix for precise definitions). As in Burnside, Eichenbaum and Fisher (2004) and Ramey (2008), the fiscal VAR includes controls for (scaled) Ramey and Shapiro (1998) war dummies through  $W_{it}$  augmented with a 9/11 dummy as in Ramey (2008). These dummies control for changes in federal military spending associated with the Korean war, the Vietnam war, the Carter-Reagan military buildup, and 9/11. The war dummies are equal to zero at all dates apart from 1950:3 for  $D_{1t}$ , 1965:1 for  $D_{2t}$ , 1980:1 for  $D_{3t}$  and 2001:3 for  $D_{4t}$ . We normalize  $D_{1,1950:3}$  to be equal to 1 while following Ramey (2008), we set  $D_{2,1965:1} = 0.3$ ,  $D_{3,1980:1} = 0.1$  and  $D_{4,2001:3} = 0.1$ , see also Burnside, Eichenbaum and Fisher (2004).

Government spending shocks are here measured by the war dummies which provides a symmetric treatment to the narrative approach of the measurement of the tax shocks that we adopted in Section 2. One slight worry is changes government spending are also associated with anticipation effects. It is beyond the scope of this paper to evaluate this problem but Ramey (2008) provides some evidence of the relevance of this issue.

The impacts of a 1 percent tax cut when we control for government spending are reported in Figure 10. The impact on output, consumption, and investment are practically identical to the benchmark case. In particular, a surprise tax cut is associated with significant increases in output, consumption and investment, which peak around 10 quarters after the tax cut. An anticipated tax cut gives rise to a pre-implementation drop in output and in investment while consumption is roughly constant throughout the pre-implementation period. As in the benchmark estimates, once taxes are eventually cut, there is a significant rise in output and its components. Thus, we find no evidence to suggest that our results are contaminated by lack of control for government spending shocks.

We also find that an unanticipated tax shock is associated with a temporary drop in government spending, a finding that would be consistent with a “starving the beast” hypothesis regarding the government spending effects of tax cuts. This result, however, is sensitive to the inclusion of changes in tax liabilities that are related to concerns about inherited government deficits. When we consider only changes in tax liabilities that were introduced to address long term growth concerns (ideological based tax liability changes), we find that government spending increases after a cut in taxes, a finding that is consistent with the estimates of Romer and Romer (2008c). The impacts of tax changes on output, consumption and investment when considering only ideologically based tax shocks in the VAR, however, are as good as identical to those that we report here.

## 5 Tax Shocks and the U.S. Business Cycle

The analysis of Sections 3 and 4 has established that tax shocks have a significant impact upon output, consumption, investment, hours worked, and on real wages. We now examine if the tax shocks have had important business cycle consequences. We carry out this analysis using a counterfactual framework. In particular, we simulate the estimated VAR process setting the vector  $\hat{e}_t$  equal to zero and using the point estimates of the estimated parameter vectors. The resulting time-series for the observables are then Hodrick-Prescott filtered (assuming a value of the smoothing parameter of 1600). We then compare these counterfactual time-series with the actual time series for the observables.

We base the counterfactual VAR analysis upon the benchmark estimates using the VAR in equation (2). However, we also compare the results of this exercise with those that derive from using the estimates of the VAR in equation (4) where we control for monetary policy.

The results from using the benchmark VAR are reported in Figure 11. We show the counterfactual time series for output, consumption and investment for three different simulations. In the first simulation we allow only for surprise tax shocks thus setting  $\tau_{t,i}^a = 0$  for all  $t$  and all  $i = 0, 1, \dots, K$ . These results are reported in panel A. In the second case we allow only for anticipated tax shocks thus setting  $\tau_t^u = 0$  for all  $t$ . These results are shown in panel B. Finally, in the third simulation, we allow for both types of tax shocks. In all cases, the counterfactual time series are shown with thick lines while the actual time series are illustrated with the lighter lines. We also indicate the NBER datings of the business cycle with grey areas illustrating recessions according to the NBER datings.

The results are very interesting. Both anticipated and unanticipated tax shocks have provided important impulses to the business cycle. Surprise tax changes were important impulses to the business cycle during three episodes. First, during the early to mid 1960's, the tax liability increase associated with the Revenue Act of 1962 led to a slow uptake in activity after the 1960-61 recession while the 2.55 percent tax liability cut contained in RA of 1964 provided a major stimulus to the economy which accounts for a large fraction of the boom in the U.S. economy during the mid 1960's. Secondly, the 1.23 percent tax cut contained in the Revenue Act of 1971 contributed to the pre-OPEC I boom of the U.S. economy in the early 1970's. Finally, the 2.86 percent tax liability cut associated with JGTRRA 2003 provided a major boost to the economy in the mid 2000's.

Anticipated tax liability changes were particularly relevant impulses to the business cycle during the early 1980's recession, the expansion that followed thereafter, and during the early 2000's. Particularly interesting is the 1980's episode where we find that ERTA 1981 and the Social Security Amendments of 1977 together had a large impact on the U.S. economy. The Social Security Amendments of 1977 (signed by Carter in December 1977) included a 0.56 percent tax increase implemented in 1981. This tax liability change had an expansionary effect on the economy prior to its implementation but provided a negative stimulus once implemented in 1981. ERTA 1981, signed by Reagan in August 1981, was associated with major tax cuts implemented gradually from 1982 to 1984. These anticipated tax cuts had a negative impact on the U.S. economy from late 1981 up till the end of 1983, the same time as the negative effects of the Social Security Amendments of 1977 were setting in. When the Reagan tax cuts were eventually implemented through 1982 to 1984, it provided a major stimulus to the economy during the mid 1980's. Together, these anticipated tax

cuts therefore stimulated the economy prior to 1981, gave rise to a contractionary effects from 1981 to late 1983, and helped the economy recover thereafter. Quantitatively, our results indicate that the early 1980's recession was to a large extent caused by fiscal policy rather than induced by tight monetary policy during the Volcker monetary regime.

Anticipation effects are also relevant in the case of EGTRRA 2001 and JGTRRA 2003 signed by Bush in June 2001 and in May 2003, respectively. The former introduced a 0.80 percent cut in tax liabilities in the first quarter of 2002 while the latter introduced anticipated tax increases in the third quarter of 2004 (a 1.70 percent increase) and in the first quarter of 2005 (a 0.56 increase). In agreement with the simulations of House and Shapiro (2006), we find that the anticipation effects of the first of these tax acts contributed to the slow recovery from the 2001 U.S. recession, while the implementation of the tax cut helped stimulate the economy from 2002 onwards. Ironically, the anticipation effects associated with the tax increases incorporated in JGTRRA 2003 further stimulated the economy during the pre-implementation period (2003q2 - 2004q3) until its implementation eventually starts having a negative impact from the end of 2004 onwards.<sup>12</sup>

When we allow for both types of tax shocks, see panel C, we find that they have accounted for around 20 percent of the in-sample variance of output and its components at business cycle frequencies. Moreover, the correlation between the counterfactual time-series and the actual time-series is high (around 56 percent for output, and 48 percent for consumption and investment). Thus, we conclude that tax shocks play a major role as business cycle impulses, a finding that is in line with McGrattan (1994).

Perhaps the most controversial result of this exercise is the importance of anticipated tax changes for the early 1980's recession. To establish the robustness of this result, Figure 12 reports the counterfactual time-series for output based on the VAR in equation (4) where we control for monetary policy. In this figure we also show the counterfactual output series that derives from allowing only for monetary policy shocks. Our results are robust. At the trough of the early 1980's contraction, output is 4.6 percent below trend. According to the benchmark estimates, the tax shocks by themselves give rise to a 3.3 percent drop in output and the anticipated tax cuts account for almost all of this drop.

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<sup>12</sup>Note that we exclude parts of the tax changes in EGTRRA 2001 since Romer and romer (2007) categorize the temporary tax cut as an endogenous response to the state of the business cycle.

When we reestimate the VAR allowing for monetary variables, tax shocks give rise to a 2.6 percent drop in output again with most of the action coming from anticipated tax shocks. By contrast, the monetary policy shocks account for a maximum one percent drop in output. Moreover, in the counterfactual experiment, the tax shocks can account for 27 percent of the output variance when we introduce monetary variables in the VAR, and estimate that is even higher than the benchmark estimate. Thus, the finding that tax shocks were key for accounting for the early 1980's recession is robust and is not due to lack of control for monetary policy shocks.

## 6 Conclusions

We have investigated the dynamic effects of tax liability changes in the U.S. during the postwar period. We distinguished between anticipated and unanticipated tax shocks on the basis of a timing convention. In particular, we assume that tax shocks that were introduced more than 90 days after they became law were anticipated while tax liability changes implemented within 90 days of becoming law are classified as surprise tax shocks. We have documented that unanticipated tax cuts give rise to a major stimulus to the economy which is reflected in persistent increases in output, consumption, investment and, to a smaller extent, hours worked. Anticipated tax cuts also stimulate the economy once taxes are cut, but during the pre-implementation period, we find that output, investment and hours worked drop while consumption is roughly unchanged. A number of earlier studies have documented little impact of tax announcements on consumption until taxes are eventually changed, a finding that is consistent with our results. The finding of important anticipation effects on output, investment and hours worked is new to the literature, however.

We carried out an extensive robustness analysis. First, we find robustness of the results to the elimination of particular tax acts including the largest three tax interventions in the sample. Secondly, we showed that the results are robust to considering only tax liability changes that were meant to be permanent, and to eliminating tax liability changes that were introduced due to inherited debt concerns. Third, our results also hold true if we control for other structural shocks in the form of monetary policy shocks and government spending shocks. We also demonstrated that our timing based measurement of anticipated tax shocks is meaningful in the sense that there are no signs that leads of surprise tax shocks have any impact on the economy.

Tax shocks have provided important impulses to the U.S. business cycle. In particular, we showed that the two tax shocks have been important for several business cycle episodes in the U.S. In particular, the anticipated tax cuts associated with the ERTA 1981 and the implementation of the announced tax increases embedded in the Social Security Amendments of 1977 combined to provide a negative stimulus to the economy throughout 1981 to 1983, when the Reagan tax cuts eventually were implemented, they provided a major stimulus during the mid-1980's. The tax shocks altogether can account for around 20-25 percent of the in-sample variance of (detrended) output, an estimate that is at least as large as the contribution of other popular candidates for business cycle impulses.

Our results regarding the presence of anticipation effects have important consequences. First, it provides some empirical evidence towards which theories of anticipation effects can be evaluated. Preexisting evidence has been scarce on this since it has mainly been concerned with consumption responses to pre-announced tax changes and has found little impact on consumption of such tax announcements (until they become implemented). Therefore, our results regarding the output, investment, and labor market impact of tax announcements provide some valuable findings that one can use for model evaluation. Mertens and Ravn (2009) carry out such an exercise. Secondly, our results may also have some relevance for the recent interest into the “news” view of business cycles, see e.g. Beaudry and Portier (2004, 2006), Cochrane (1994), Danthine, Donaldson and Johnsen (1998), den Haan and Kaltenbrunner (2006), or Jaimovich and Rebelo (2006). An important obstacle to empirical tests for a news driven business cycle theory is that expectations are inherently difficult to estimate as they are unobserved by the econometrician, but we overcome this difficulty in our application to fiscal policy. Our finding that a pre-announced tax cut gives rise to a pre-implementation contraction in the economy may be important for understanding how news shocks help shape fluctuations in the economy.

It would be interesting to extend our analysis to other parts of fiscal policy. Ramey (2008) provides some first evidence that anticipation effects may account for some of the more puzzling recent findings about the impact of government spending shocks that derive from VAR based measures of fiscal shocks. In particular, she shows that the fiscal shocks identified by VAR based methods (that rely on decision lags for identification) appear to be predictable on the basis of publicly available forecasts of changes in fiscal policy. Moreover, by simulating a DSGE model she shows that ignoring

such anticipation effects may lead to seriously biased estimates of the impact of fiscal policy shocks. It would therefore be interesting to examine also whether there are signs of such anticipation effects in the data using similar methods to those that we have proposed in this paper.

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## 8 Appendix 1: Data: Definitions and Sources

Table A.1: Tax Liability Changes

Name	Signed	Effective	Type	Persistence	Motivation	Size
1. Social Security Amendments of 1947	August 1947	1950 Q1	Anticipated	Permanent	Deficit	0.27
2. Revenue Act of 1948	April 1948	1948 Q2	Surprise	Permanent	Long run	-3.74
	April 1948	1948 Q3	Surprise	Permanent	Long run	1.83
3. Social Security Amendments of 1950	August 1950	1954 Q1	Anticipated	Permanent	Deficit	0.35
4. Expiration of Excess Profits and Temporary Income Tax	October 1951	1954 Q1	Anticipated	Permanent	Deficit	-0.35
5. Internal Revenue Code of 1954	August 1954	1954 Q3	Surprise	Permanent	Long run	-1.13
	August 1954	1954 Q4	Surprise	Permanent	Long run	0.72
6. Tax Rate Extension Act of 1958	June 1958	1958 Q3	Surprise	Permanent	Long run	-0.11
7. Social Security Amendments of 1958	August 1958	1960 Q1	Anticipated	Permanent	Deficit	0.36
8. Federal-Aid Highway Act of 1959	September 1959	1959 Q4	Surprise	Temporary	Deficit	0.12
9. Social Security Amendments of 1961	Jun 1961	1963 Q1	Anticipated	Permanent	Deficit	0.33
10. Changes in Depreciation Guidelines and Revenue Act of 1962	July 1962	1962 Q3	Surprise	Permanent	Long run	-0.69
	July 1962	1962 Q4	Surprise	Permanent	Long run	0.45
	October 1962	1962 Q4	Surprise	Permanent	Long run	-0.61
	October 1962	1963 Q1	Surprise	Permanent	Long run	0.45
11. Revenue Act of 1964	October 1962	1963 Q1	Surprise	Permanent	Long run	0.10
	February 1964	1964 Q2	Surprise	Permanent	Long run	-2.55
	February 1964	1964 Q3	Anticipated	Permanent	Long run	1.25
12. Excise Tax Reduction of 1965	February 1964	1965 Q1	Anticipated	Permanent	Long run	-0.65
	June 1965	1965 Q3	Surprise	Permanent	Long run	-0.24
13. Tax Adjustment Act of 1966	June 1965	1966 Q1	Anticipated	Permanent	Long run	-0.23
	March 1966	1966 Q2	Surprise	Permanent	Long run	0.12
14. Public Law 90-26	June 1967	1967 Q3	Surprise	Permanent	Long run	-0.66
	June 1967	1967 Q4	Anticipated	Permanent	Long run	0.46

Table A.1 continued

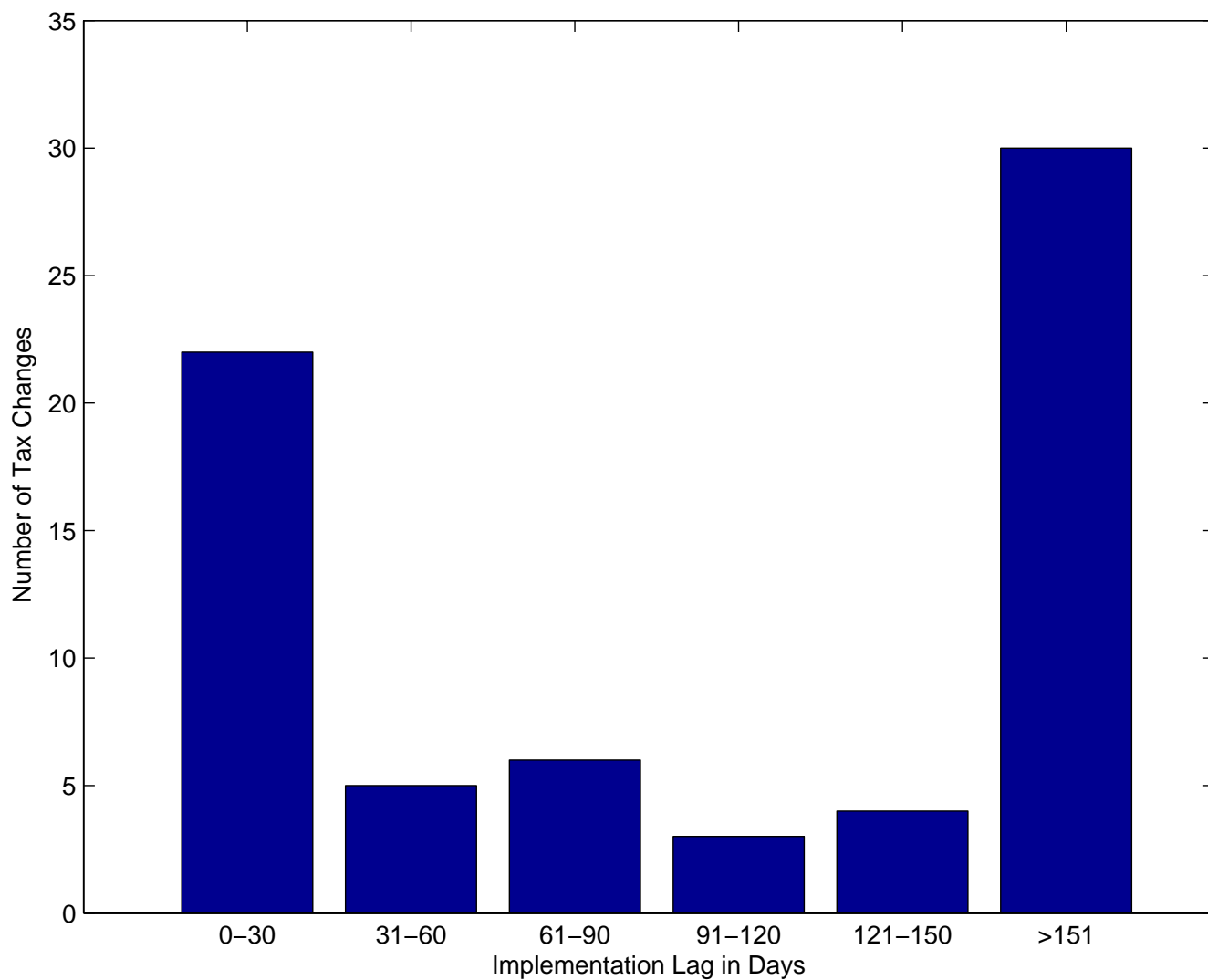
Name	Signed	Effective	Type	Persistence	Motivation	Size
15. Social Security Amendments of 1967	January 1968	1971 Q1	Anticipated	Permanent	Deficit	0.33
16. Tax Reform Act of 1969	December 1969	1971 Q1	Anticipated	Permanent	Long run	-0.09
	December 1969	1972 Q1	Anticipated	Permanent	Long run	-0.09
17. Reform of Depreciation Rules	January 1971	1971 Q1	Surprise	Permanent	Long run	-0.25
18. Revenue Act of 1971	December 1971	1972 Q1	Surprise	Permanent	Long run	-1.23
	December 1971	1972 Q2	Anticipated	Permanent	Long run	0.55
19. 1972 Changes to Social Security	October 1972	1978 Q1	Anticipated	Permanent	Deficit	0.13
20. Tax Reform Act of 1976	October 1976	1976 Q4	Surprise	Permanent	Long run	0.13
	October 1976	1977 Q1	Surprise	Permanent	Long run	-0.04
21. Tax Reduction and Simplification Act of 1977	May 1977	1977Q3	Surprise	Permanent	Long run	-1.02
	May 1977	1977Q4	Anticipated	Permanent	Long run	0.66
22. Social Security Amendments of 1977	December 1977	1979 Q1	Anticipated	Permanent	Long run	0.36
	December 1977	1980 Q1	Anticipated	Permanent	Long run	0.06
	December 1977	1981 Q1	Anticipated	Permanent	Long run	0.56
	December 1977	1982 Q1	Anticipated	Permanent	Long run	0.05
23. Revenue Act of 1978	November 1978	1979 Q1	Surprise	Permanent	Long run	-0.77
24. Crude Oil Windfall Profit Tax Act of 1980	April 1980	1980 Q2	Surprise	Temporary	Long run	0.30
	April 1980	1981 Q1	Anticipated	Temporary	Long run	0.13
	April 1980	1982 Q1	Anticipated	Temporary	Long run	0.13
25. Economic Recovery Tax Act of 1981	August 1981	1981 Q3	Surprise	Permanent	Long run	-0.84
	August 1981	1981 Q4	Surprise	Permanent	Long run	0.56
	August 1981	1982 Q1	Anticipated	Permanent	Long run	-1.53
	August 1981	1983 Q1	Anticipated	Permanent	Long run	-1.69
	August 1981	1984 Q1	Anticipated	Permanent	Long run	-1.28
26. Tax Equity and Fiscal Responsibility Act of 1982	September 1982	1983 Q1	Anticipated	Permanent	Deficit	0.78

Name	Signed	Effective	Type	Persistence	Motivation	Size
27. Social Security Amendments of 1983	April 1983	1984 Q1	Anticipated	Permanent	Deficit	0.32
	April 1983	1985 Q1	Anticipated	Permanent	Deficit	0.21
	April 1983	1986 Q1	Anticipated	Permanent	Deficit	0.10
	April 1983	1988 Q1	Anticipated	Permanent	Deficit	0.31
	April 1983	1990 Q1	Anticipated	Permanent	Deficit	0.18
28. Deficit Reduction Act of 1984	July 1984	1984 Q3	Surprise	Permanent	Deficit	0.20
29. Tax Reform Act of 1986	October 1986	1986 Q4	Surprise	Permanent	Long run	0.50
	October 1986	1987 Q1	Surprise	Permanent	Long run	-0.16
	October 1986	1987 Q3	Anticipated	Permanent	Long run	-0.42
	October 1986	1988 Q1	Anticipated	Permanent	Long run	-0.15
30. Omnibus Budget Reconciliation Act of 1987	December 1987	1988 Q1	Surprise	Permanent	Deficit	0.22
31. Omnibus Budget Reconciliation Act of 1990	November 1990	1991 Q1	Surprise	Permanent	Deficit	0.60
32. Omnibus Budget Reconciliation Act of 1993	August 1993	1993 Q3	Surprise	Permanent	Deficit	1.02
	August 1993	1993 Q4	Surprise	Permanent	Deficit	-0.59
	August 1993	1994 Q1	Anticipated	Permanent	Deficit	0.19
33. Tax Payer Relief Act and Balanced Budget Act of 1997	August 1997	2000 Q1	Anticipated	Permanent	Deficit	0.02
	August 1997	2002 Q1	Anticipated	Permanent	Deficit	0.01
34. Economic Growth and Tax Relief Reconciliation Act of 2001	June 2001	2002 Q1	Anticipated	Permanent	Long run	-0.80
35. Jobs and Growth Tax Relief Reconciliation Act of 2003	May 2003	2003 Q3	Surprise	Temporary	Long run	-2.86
	May 2003	2004 Q3	Anticipated	Temporary	Long run	1.70
	May 2003	2005 Q1	Anticipated	Temporary	Long run	0.56

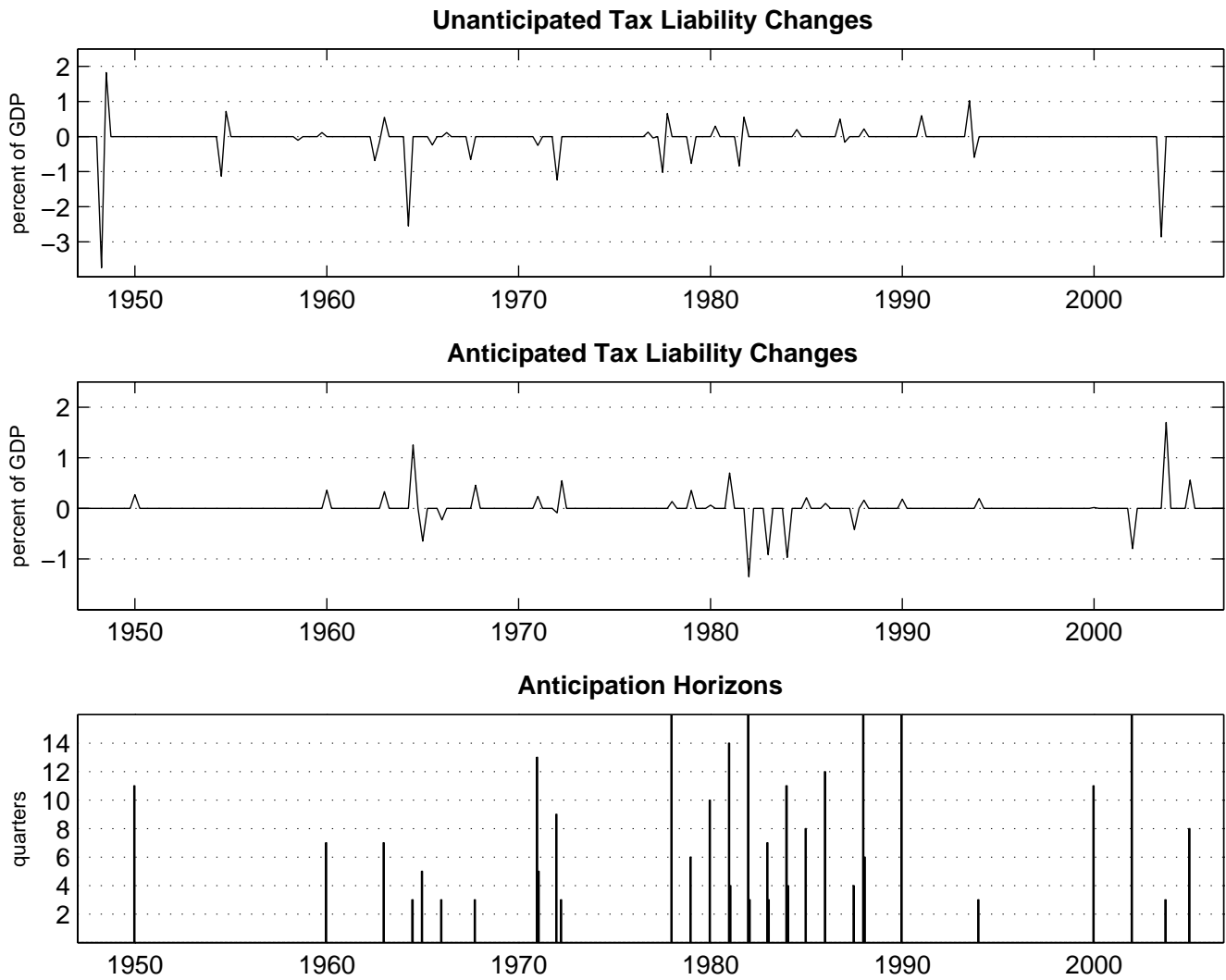
Source: Romer and Romer, 2007, 2008a and Bureau of Economic Analysis. Tax liability changes with more than 90 days difference between the signing of the legislation and their implementation are classified as anticipated tax liability changes. Sizes are measured by the implied tax liability impact divided by that quarter's current price GDP at the annual rate.

**Table A.2: Definitions of Variables**

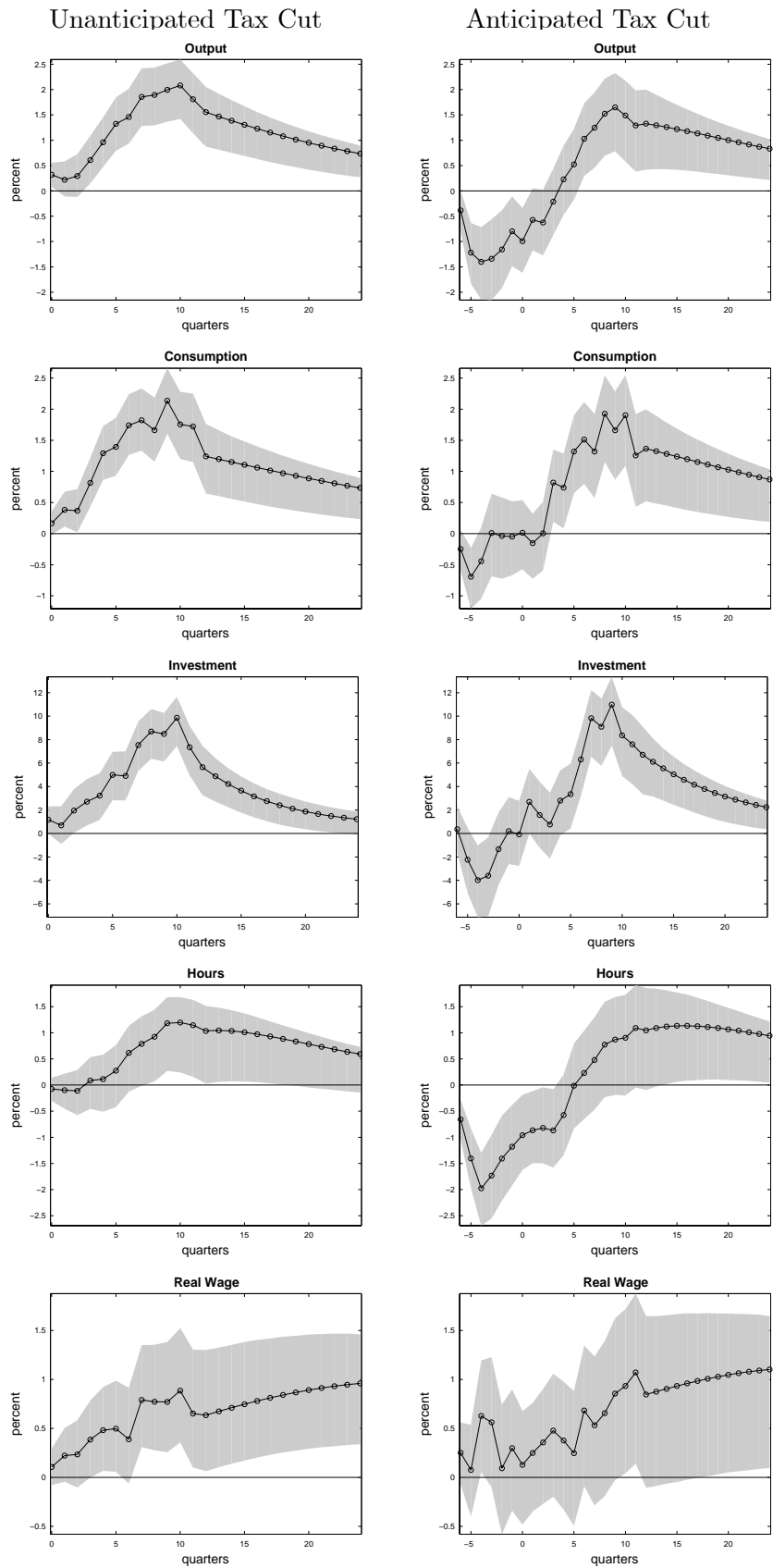
<b>Variable</b>	<b>Definition</b>	<b>Source</b>
Output	Nominal GDP divided by its implicit deflator and by population	Bureau of Economic Analysis
Consumption	Consumers nominal expenditure divided by its deflator and by population	Bureau of Economic Analysis
Investment	Private sector gross investment divided by its deflator and by population	Bureau of Economic Analysis
Hours worked	Product of hours per worker and civilian non-farm employment divided by population combined with Francis and Ramey (2002) hours worked series.	Bureau of Economic Analysis and Francis and Ramey (2002)
Population	Population above 16 years of age	Bureau of Labor Statistics
Federal funds rate	Effective funds rate (annualized)	Federal Reserve Bank of St. Louis
Inflation rate	Annualized quarter to quarter increase in implicit GDP deflator	Bureau of Economic Analysis
Nonborrowed reserves	Non borrowed reserves, final month of the quarter	Federal Reserve Bank of St. Louis
Commodity price inflation	Annualized quarter to quarter increase in the KR-CRB spot commodity price index	Commodity Research Bureau
Government spending	Sum of federal current expenditures, gross investment expenditures, capital transfer payments and net purchases of nonproduced assets divided by its implicit deflator and by population	Bureau of Economic Analysis
Government revenue	Total federal receipts divided by government spending deflator and by population	Bureau of Economic Analysis



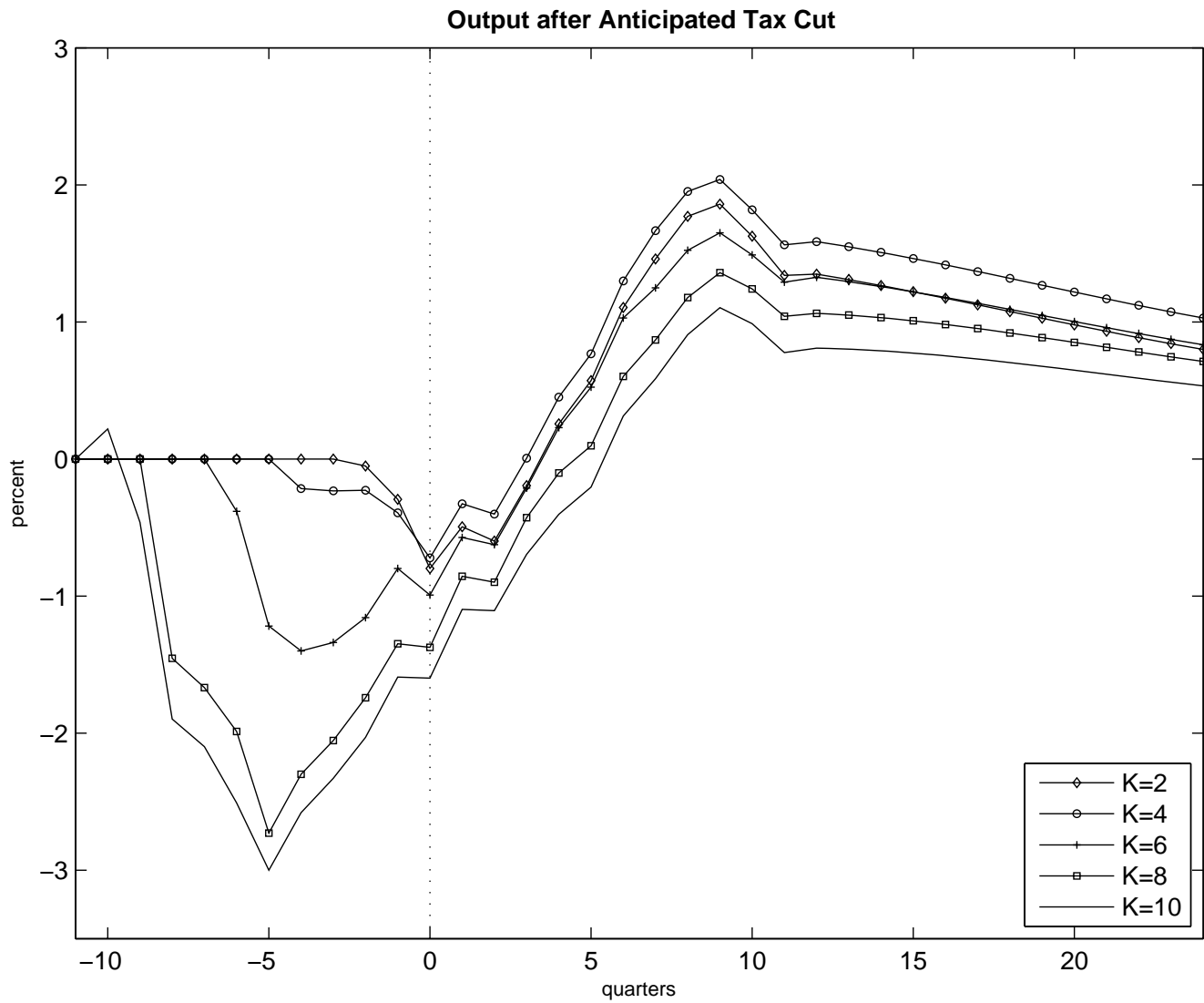
**Figure 1:** The Distribution of Implementation Lags Across Tax Liability Changes



**Figure 2:** Tax Liability Changes as Percent of Current Price GDP (the top panel illustrates unanticipated tax changes, the middle panel illustrates anticipated tax changes dated at the implementation date, the lower panel shows the implementation lag in quarters of the anticipated tax changes censored at 16 quarters)



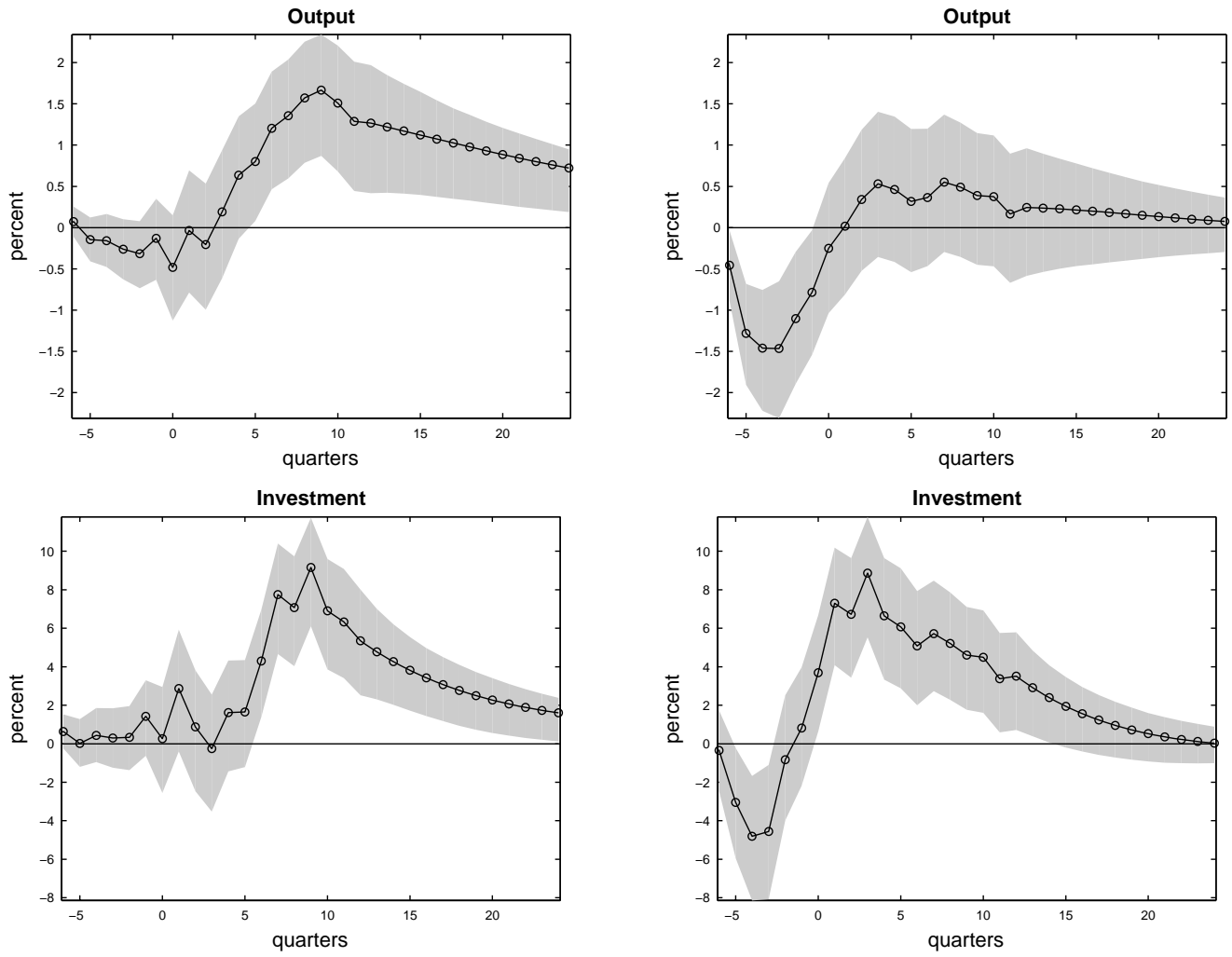
**Figure 3:** The Impact of a 1 percent Tax Liability Cut (lines with circles indicates point estimates, grey areas are 67 percent bootstrapped confidence intervals)



**Figure 4:** The Impact on Output of a 1 Percent Anticipated Tax Cut for Alternative Anticipation Horizons

### Leads of Unanticipated Tax Shocks

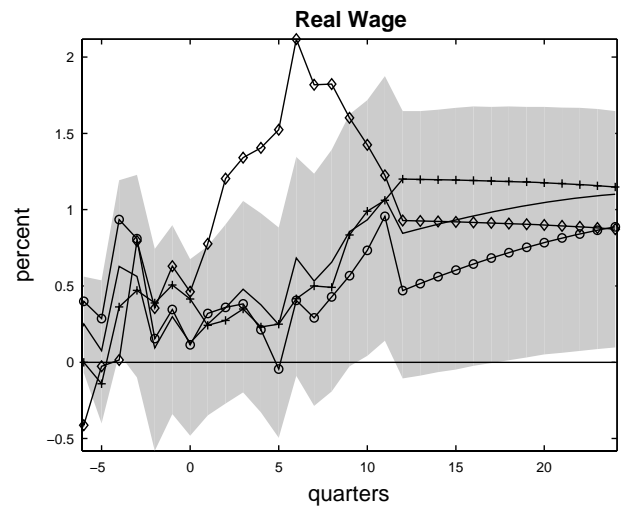
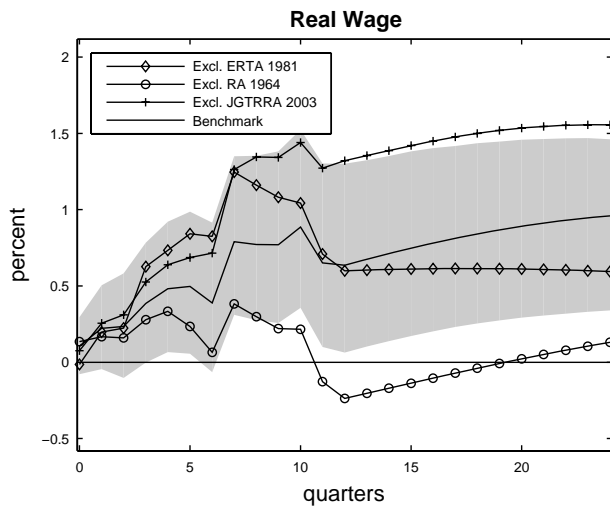
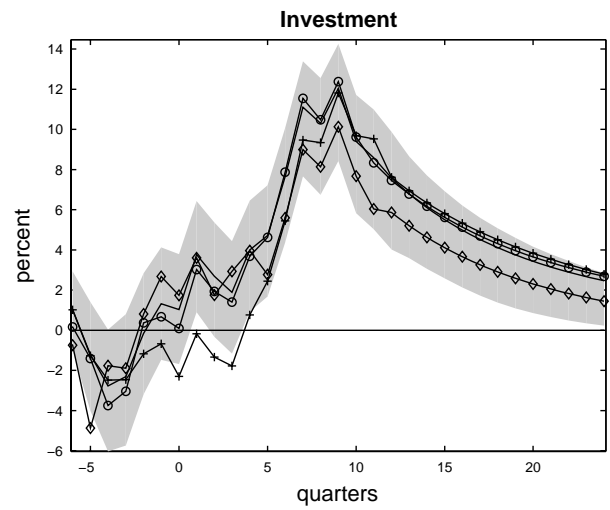
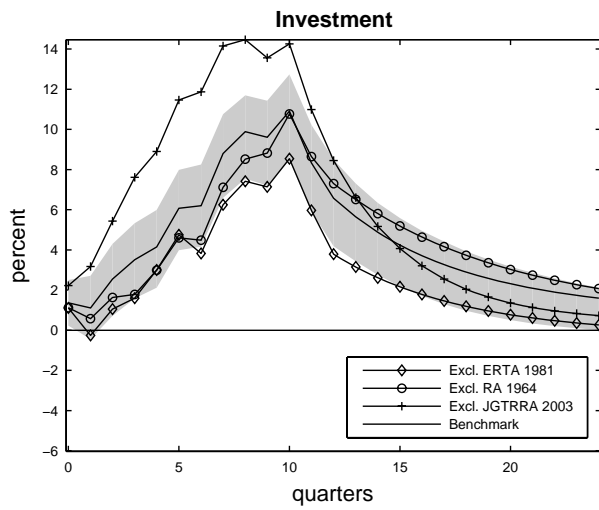
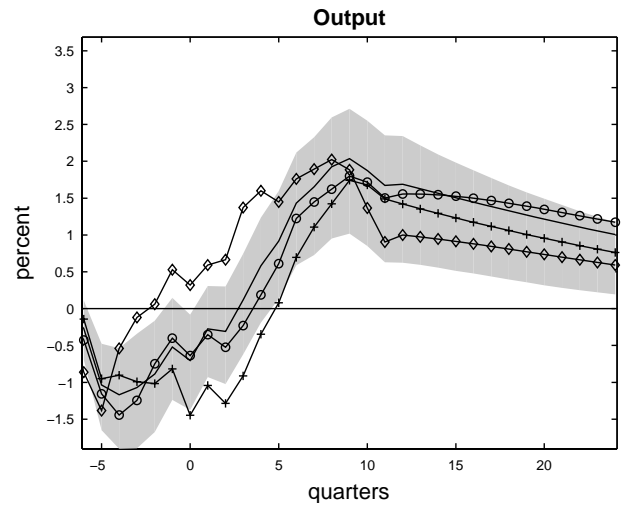
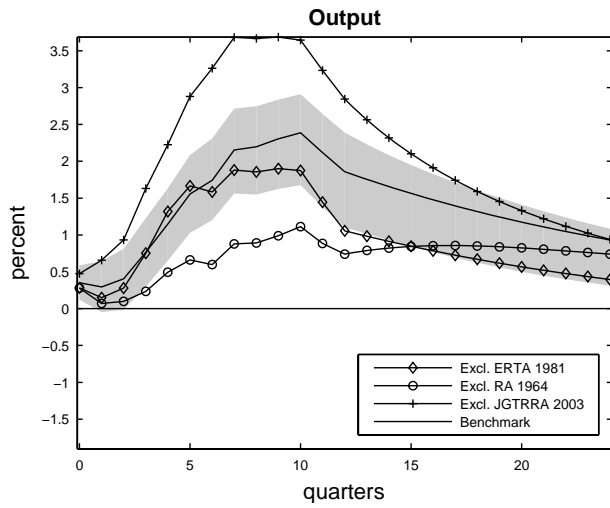
### Anticipated Tax Shocks



**Figure 5:** The Impact of a 1 Percent Tax Cut Allowing for Anticipation of Surprise Tax Shocks  
(the impulse responses are estimated from equation (3))

### Unanticipated Tax Cut

### Anticipated Tax Cut

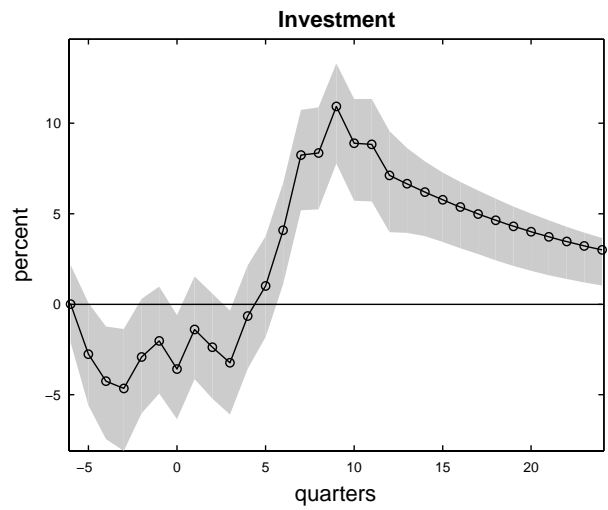
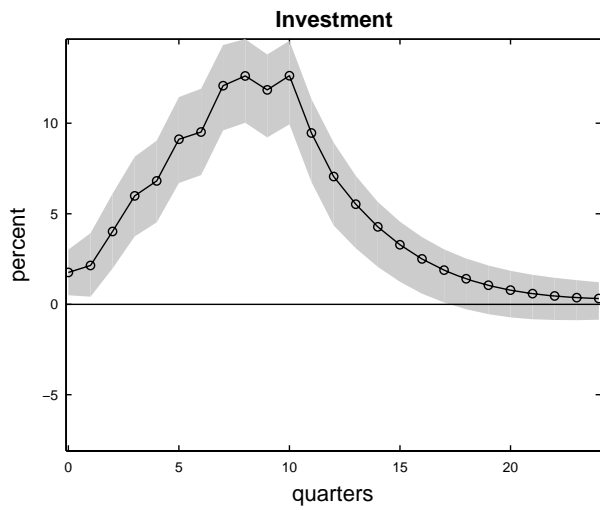
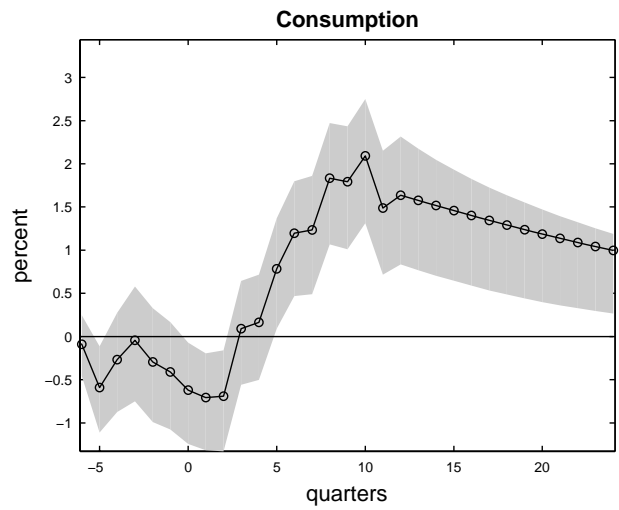
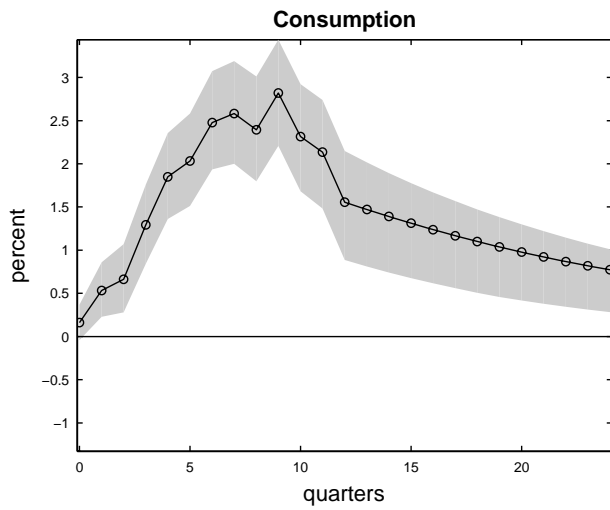
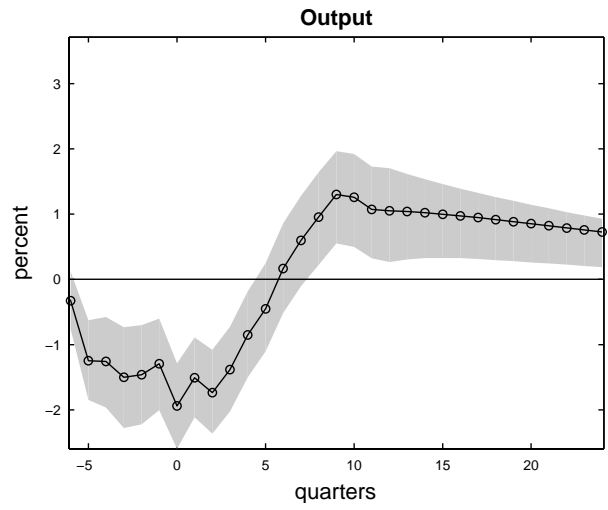
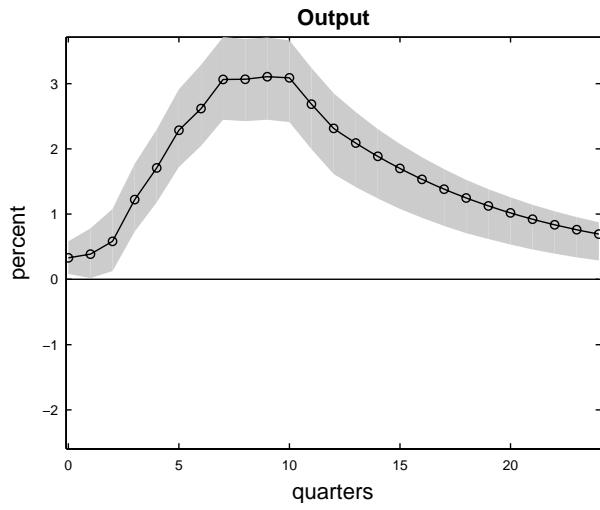


**Figure 6:** Stability of Estimates Across Tax Acts

(the diagrams show the impact of a 1 percent tax liability cut when eliminating RA 1964, ERTA 1981 or JGTRRA 2003 from the sample, respectively)

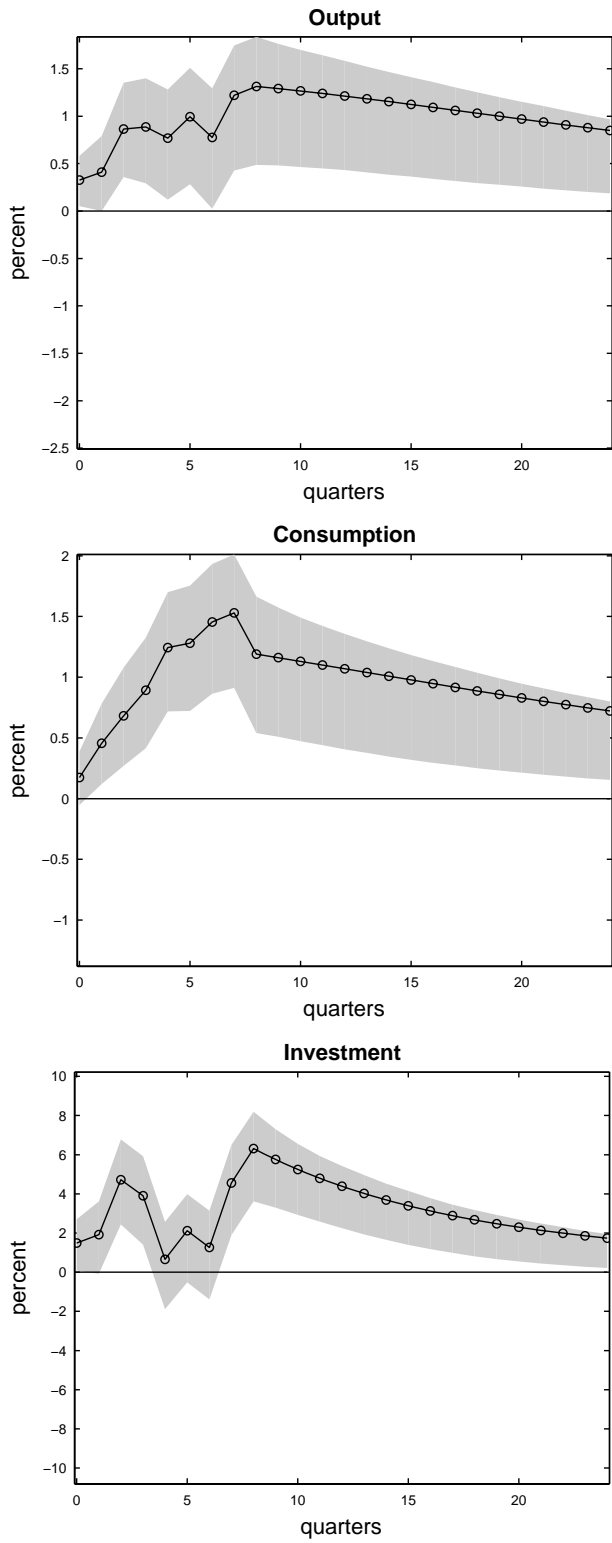
### Unanticipated Tax Cut

### Anticipated Tax Cut

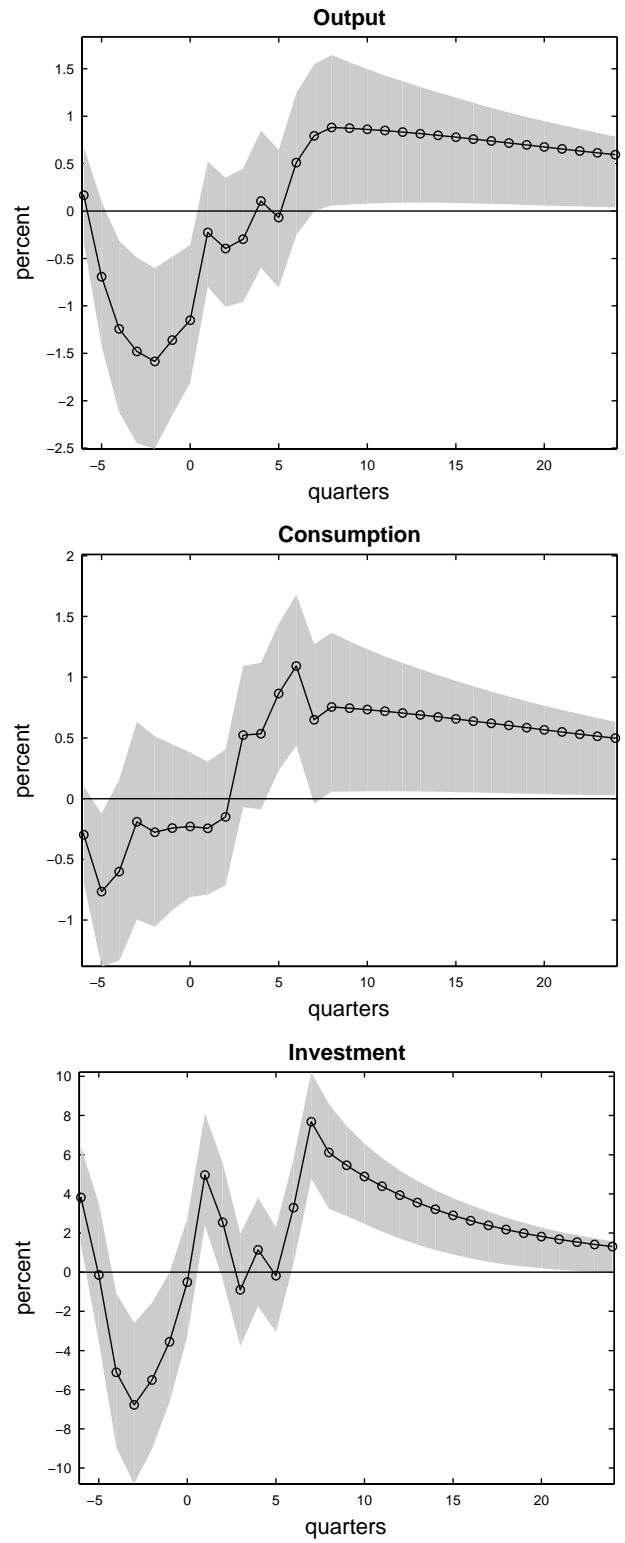


**Figure 7:** The Impact of a 1 Percent Permanent Tax Cut

### Unanticipated Tax Cut



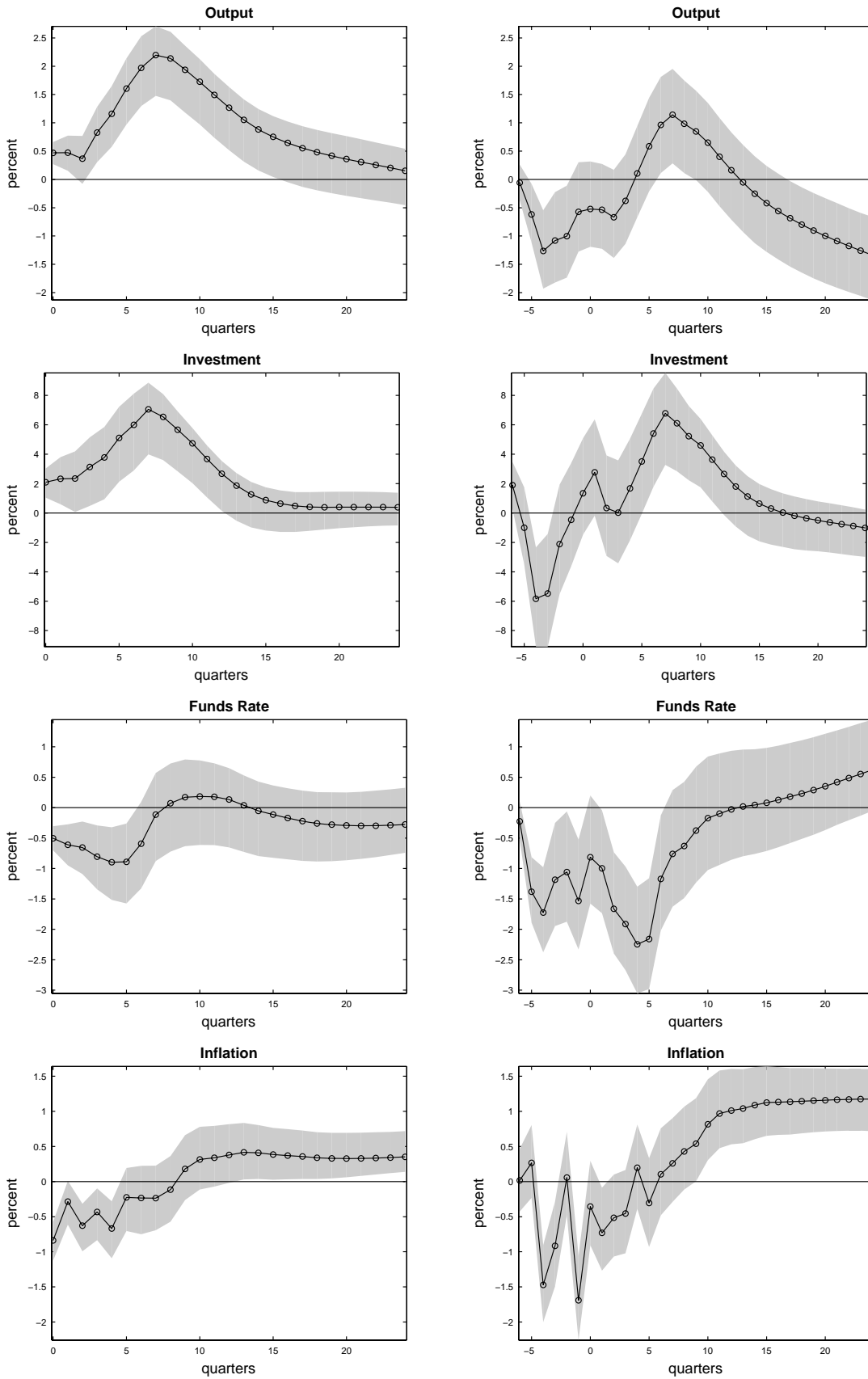
### Anticipated Tax Cut



**Figure 8:** The Impact of a 1 Percent Tax Cut Introduced for Long Run Growth Reasons

### Unanticipated Tax Cut

### Anticipated Tax Cut

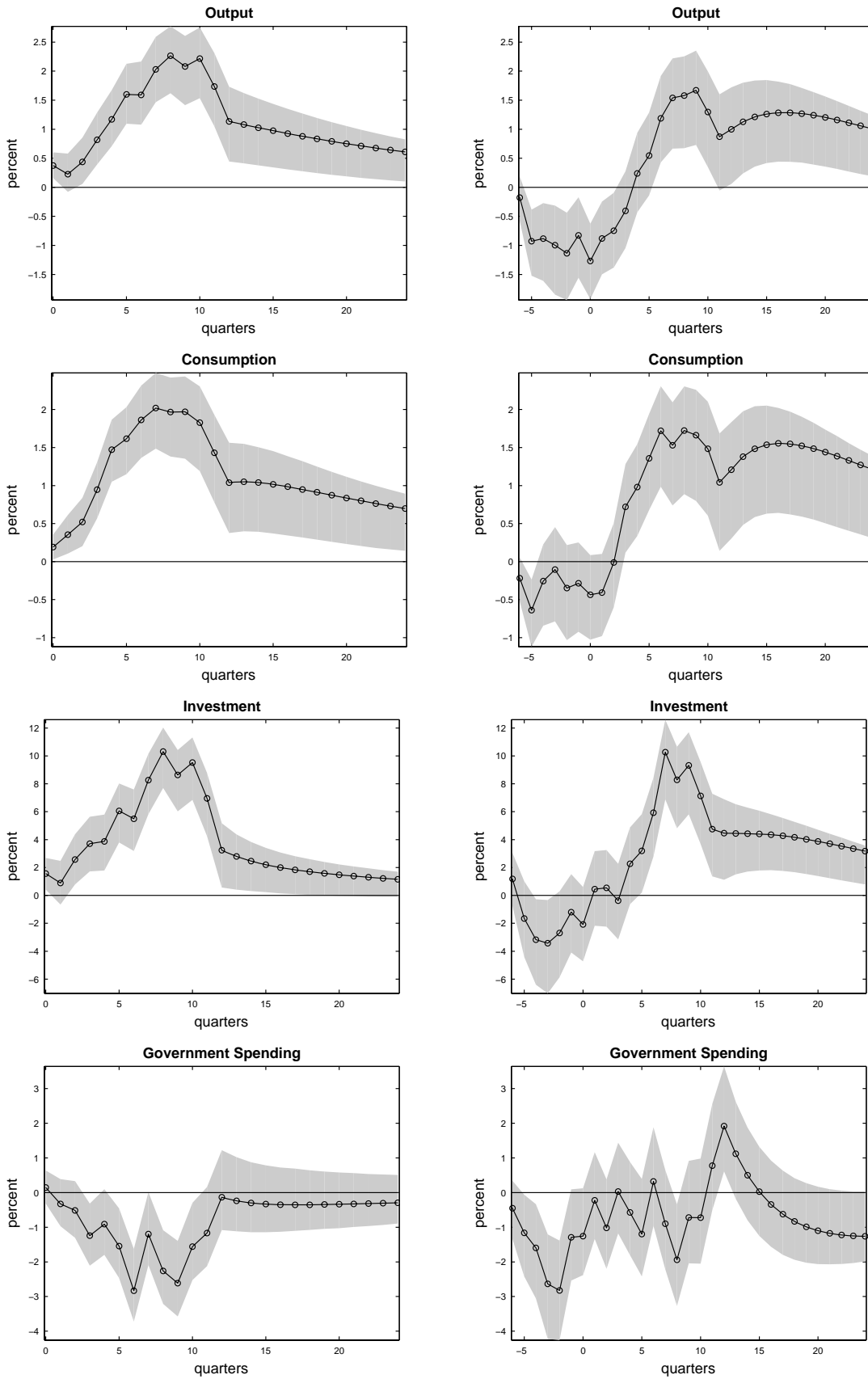


**Figure 9:** The Impact of a 1 Percent Tax Cut Controlling for Monetary Variables

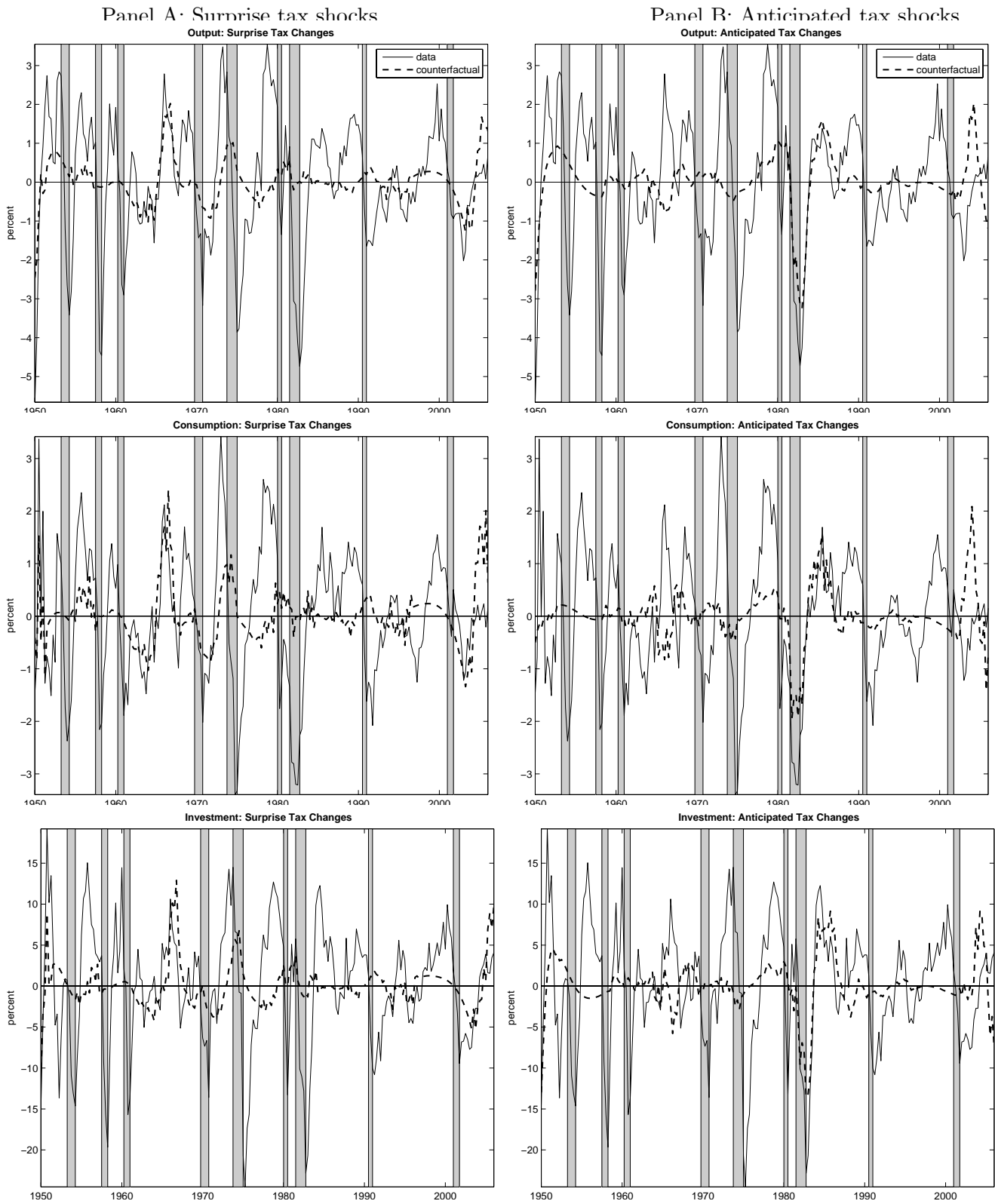
(the figure shows the estimates from equation 4)

### Unanticipated Tax Cut

### Anticipated Tax Cut



**Figure 10:** The Impact of a 1 Percent Tax Cut Controlling for Government Spending and Government Revenue (the figure shows the estimates from equation 5)



**Figure 11:** Counterfactual Analysis of US Business Cycles

(the figure illustrates HP-filtered actual and simulated output, consumption, and investment. In panel A, equation 2 is simulated setting all shocks by the surprise tax shock equal to zero; In panel B, equation 2 is simulated setting all but the anticipated tax shock equal to zero. Shaded areas indicate recessions according to the NBER datings)

### Panel C: All tax shocks

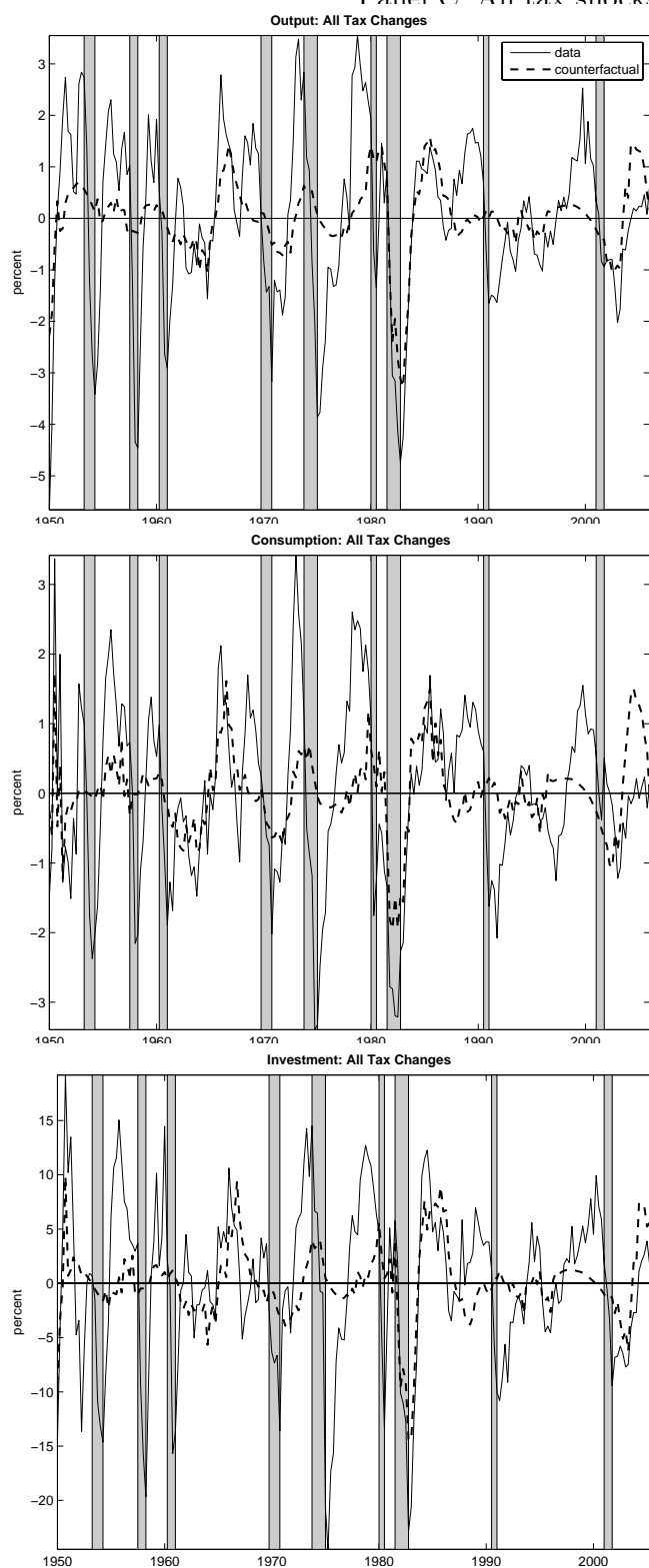
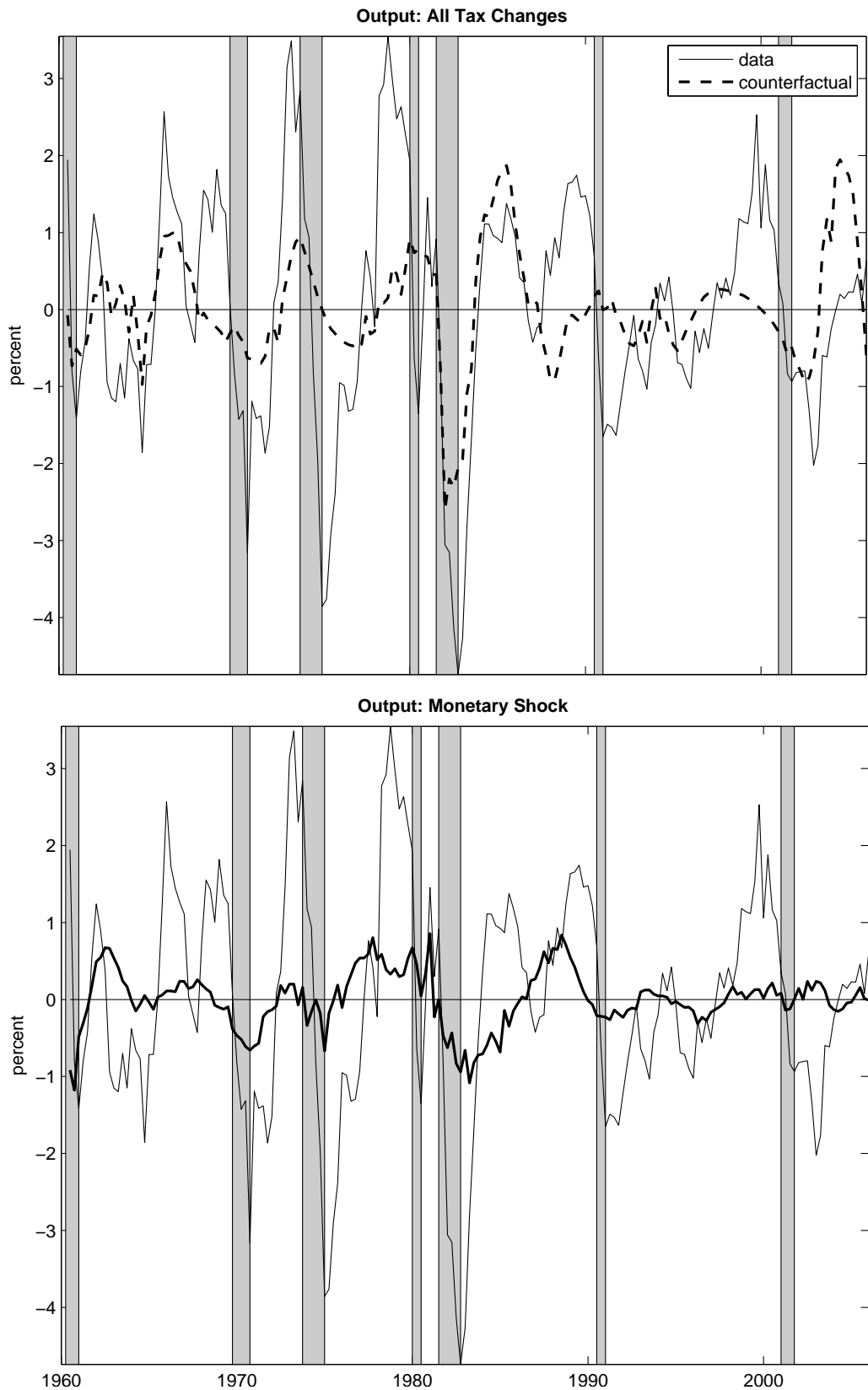


Figure 11, continued

(panel C illustrates the case where all shocks apart from the tax shocks are set equal to zero)



**Figure 12:** Counterfactual Output from the Monetary VAR

(the figure illustrates HP filtered actual and simulated output series; the top figure illustrates output when simulating the VAR in equation 4 allowing only for tax shocks; the bottom figure illustrates output when simulating the VAR in equation 4 allowing only for monetary policy shocks)