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ABSTRACT

The Dynamic Effects of Personal and Corporate Income Tax Changes in the United States*

This paper estimates the dynamic effects of changes in taxes in the United States. We distinguish between the effects of changes in personal and corporate income taxes using a new narrative account of federal tax liability changes in these two tax components. We develop an estimator in which narratively identified tax changes are used as proxies for structural tax shocks and apply it to quarterly post WWII US data. We find that short run output effects of tax shocks are large and that it is important to distinguish between different types of taxes when considering their impact on the labor market and the major expenditure components.

JEL Classification: E20, E32, E62 and H30
Keywords: fiscal policy, measurement error, narrative identification and tax changes

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1 Introduction

This paper presents evidence on the aggregate effects of changes in tax policy in the US in the post WWII sample. Exogenous changes in taxes are identified in a vector autoregressive model by proxying latent tax shocks with narratively identified tax liability changes. We discriminate between the effects of changes in average personal income tax rates (APITRs) and the effects of changes in average corporate income tax rates (ACITRs). We find large short run effects on aggregate output of unanticipated changes in either tax rates. Cuts in personal income taxes lead to a fall in tax revenues while corporate income tax cuts on average have little impact on tax revenues. Cuts in APITRs raise employment, consumption and investment. Cuts in ACITRs boost investment but instead lower private consumption and have no immediate effects on employment.

The key issue in estimating the impact of economic policies is identification. In the case of tax policy shocks this is particularly challenging both because of endogeneity of policy variables and because of the diversity of policy instruments. The literature has often concentrated on exogenous changes in *total* tax revenues. There is little reason to expect that the many types of taxes available to governments all have the same impact on the economy and therefore can be summarized in a single tax measure. We deviate from the literature and look instead at two broad groupings of taxes, personal income taxes and corporate income taxes. In total these two types of taxes account for more than 90 percent of total federal tax revenues and we argue that the tax categories are individually sufficiently homogeneous that one can more meaningfully estimate their impact.

Endogeneity has been addressed in alternative ways. One line of papers uses the narrative approach to identify exogenous tax changes and estimates their effects by regressing observables on the narratively identified policy shocks, e.g. Romer and Romer (2010). An attractive feature of this approach is that the narrative record summarizes the relevant features of a potentially very large information set. On the other hand, a concern with the existing literature is that the narratively identified exogenous changes in policy instruments are implicitly viewed as mapping one-to-one into the true structural shocks. In practice there is good reason to expect that narratively identified shocks suffer from measurement errors as historical records rarely are sufficiently unequivocal that calls of judgment can be avoided. Another approach adopts structural vector autoregressions (SVARs) and achieves identification by exploiting institutional features of tax and transfer systems, see e.g. Blanchard and Perotti (2002), or by introducing sign restrictions derived from economic theory, see Mountford and Uhlig (2009). This approach has the advantage that VARs provide a parsimonious characterization of the shock transmission mechanism but identification requires parameter restrictions that may be questioned.

In this paper we develop an estimation strategy that exploits the attractive features of both SVARs and the narrative method but at the same time addresses their main weaknesses. The approach exploits the informational content of narrative measures of exogenous changes in taxes for identification in an SVAR framework. The key identifying assumptions that we propose are that narrative measures correlate with latent tax shocks but are orthogonal to other structural shocks. The main idea is to complement the usual VAR residual covariance restrictions with these moment conditions to achieve identification without having to make further assumptions on structural parameters as is required in standard SVAR approaches. The resulting structural model can be estimated using a simple three step procedure and is straightforward to implement. We show that the estimator effectively extends the use of the narrative approach to cases in which narrative shock series is measured with error and that it produces an estimate of the reliability of the narrative making it possible to judge its quality.

Given our focus on disaggregated taxes, we construct a new narrative account of shocks to average personal and corporate tax rates for the United States. This narrative is developed from Romer and Romer's (2009a) account of changes in federal US tax liabilities which we decompose into changes in personal and corporate income tax liabilities. We use only those tax changes that Romer and Romer (2009a) classify as exogenous. Following Mertens and Ravn (2011a), we also exclude those changes with implementation lags exceeding one quarter to remove anticipation effects.

Based on this methodology we provide new estimates of the impact of tax policy shocks in the US. We find that a one percentage point cut in the APITR raises real GDP per capita on impact by 0.8 percent and by up to 1.6 percent after six quarters. A one percentage point cut in the ACITR raises real GDP per capita on impact by 0.5 percent and by up to 0.7 percent after five quarters. Cuts in personal income taxes lower tax revenues while cuts in corporate taxes have no significant impact on revenues because of a very elastic response of the tax base. Translating into multipliers, the maximum personal income tax multiplier is 2.3, whereas the corporate income tax multiplier is very large given our finding that there is on average little impact on tax revenues from changes in corporate tax rates.

Changes in both types of taxes have important but distinct effects on other macroeconomic aggregates. A cut in the APITR raises employment, lowers the unemployment rate and increases hours worked per worker. A cut in the ACITR, on the other hand, has no immediate impact on either employment or hours per worker. Both cuts in the APITR and in the ACITR lead to increases in nonresidential investment and personal savings rates, but only cuts in personal income taxes stimulate private consumption. Cuts in corporate income taxes instead discourage private consumption in the short run. We find no signs of any significant change in government spending or nominal interest rates following tax shocks. The differences in the size and signs of the responses to the two types of taxes illustrates the necessity of discriminating between different types of taxes.

Our estimation approach produces a measure of the reliability of the narratively shock measures that may be of independent interest. In the benchmark model this measure has the interpretation of the squared correlation between the measure and the latent tax shock. We estimate a correlation between the narrative personal income tax shock measure and the latent tax shock of 77 percent while the corresponding estimate for the corporate tax is 48 percent. Thus, the narratives contain valuable information for identification purposes but measurement errors are nonetheless a relevant concern in practical applications.

The empirical findings support several conclusions relevant to the ongoing debate on fiscal policy. Given the currently available evidence on the multipliers associated with US government spending, see Ramey (2011b) for a recent review, our estimates indicate that the federal tax multipliers are likely to be larger than those associated with federal government purchases. If policy objectives include short run job creation and consumption stimulus, then cuts to personal income taxes are much more effective than cuts to corporate profit taxes. If the objective is to raise tax revenues, increases in personal income taxes are effective, but the costs in terms of job and output losses are relatively large. Increases in corporate profit taxes are not likely to raise significant revenues.

The remainder of the paper is organized as follows. Section 2 presents the estimation procedure. In Section 3 we present the narrative series on personal income and corporate income tax changes and the benchmark estimates. This section also provides a robustness analysis. Section 4 examines the wider macroeconomic impact of tax changes. Section 5 provides some concluding remarks.

2 Estimation and Identification

This section presents our estimation procedure. The main idea of our approach is to exploit narrative accounts of policy changes to identify structural fiscal shocks in an SVAR framework. We first describe the formal econometric framework and its relationship to existing approaches. We also provide a measurement error interpretation of our identification approach and propose measures of statistical reliability to quantify the quality of identification.

2 .1 General Methodology

Let Y_t be an $n \times 1$ vector of stationary observables. We assume that the dynamics of the observables are described by a system of linear simultaneous equations,

$$\mathcal{A}Y_t = \alpha' X_t + \varepsilon_t \tag{1}$$

where $X_t = [Y'_{t-1}, ..., Y'_{t-p}]'$ is the $np \times 1$ vector of lagged observations on the vector of observables, α is an $np \times n$ matrix of coefficients, $\mathcal{A}$ is an $n \times n$ nonsingular matrix of coefficients, and ε_t is an $n \times 1$ vector of structural shocks with $E[\varepsilon_t] = 0$, $E[\varepsilon_t \varepsilon'_t] = I_n$, $E[\varepsilon_t \varepsilon'_s] = 0$ for $s \neq t$. The specification in (1) omits deterministic terms and exogenous regressors for notational brevity. An equivalent representation of the dynamics of Y_t is

$$Y_t = \delta' X_t + \mathcal{B} \varepsilon_t \tag{2}$$

where $\mathcal{B} = \mathcal{A}^{-1}$, and $\delta' = \mathcal{A}^{-1} \alpha'$.

In the SVAR literature ε_t is treated as a vector of latent variables that are estimated on the basis of the prediction errors of Y_t conditional on the information contained in the vector of lagged dependent variables X_t, and by imposing identifying assumptions. Let the $n \times 1$ vector u_t denote the reduced form residuals which are related to the structural shocks by,

$$u_t = \mathcal{B} \varepsilon_t. \tag{3}$$

Since $E[u_t u'_t] = \mathcal{B}\mathcal{B}'$, an estimate of the covariance matrix of u_t provides $n(n+1)/2$ independent identifying restrictions. However, identification of the elements of at least one of the columns of $\mathcal{B}$ requires more identifying restrictions. The fiscal SVAR literature has accomplished this task in a variety of ways. For instance, Blanchard and Perotti (2002) exploit institutional features of the US tax system and policy reaction lags to impose restrictions on $\mathcal{B}$. Alternatively, Mountford and Uhlig (2009) impose sign restrictions on the impulse response functions implied by (2).

We propose instead to make use of proxies for the latent shocks. Let m_t be a $k \times 1$ vector of proxy variables that are correlated with the structural shocks of interest but orthogonal to other shocks. We make no requirement that the proxies coincide exactly with the true latent structural shocks but as long as they are orthogonal to other shocks, they contain information that can be exploited for identification purposes.[1]

Consider the partition $\varepsilon_t = [\varepsilon'_{1t}, \varepsilon'_{2t}]'$, where ε_{1t} is the $k \times 1$ vector containing the shocks of interest and the $(n-k) \times 1$ vector ε_{2t} contains all other $n-k$ shocks.[2] Without loss of generality we assume that $E[m_t] = 0$. The proxy variables can be used for identification of ε_{1t} and the associated impulse response functions as long as the following conditions are satisfied,

$$E[m_t \varepsilon'_{1t}] = \Phi \,, \tag{4}$$

$$E[m_t \varepsilon'_{2t}] = 0 \,, \tag{5}$$

$$E[m_t X'_t] = 0 \,, \tag{6}$$

where Φ is an unknown nonsingular $k \times k$ matrix. The first condition states that the proxy variables are correlated with the shocks of interest. The second condition requires that the proxy variables are uncorrelated with all other shocks. These two conditions are the key identifying assumptions. The third condition requires that the proxy variables are orthogonal to the history of Y_t. This assumption can be relaxed, because when a candidate proxy vector $\tilde{m}_t$ is correlated with X_t, m_t can be constructed by projecting $\tilde{m}_t$ on X_t and defining m_t as the projection error.

Estimation of the structural parameters can be accomplished as follows. Consider the following partitioning of $\mathcal{B}$,

$$\mathcal{B} = \begin{bmatrix} \underset{n\times k}{\beta_1} & \underset{n\times(n-k)}{\beta_2} \end{bmatrix} \,,\; \beta_1 = \begin{bmatrix} \underset{k\times k}{\beta'_{11}} & \underset{k\times(n-k)}{\beta'_{21}} \end{bmatrix}' \,,\; \beta_2 = \begin{bmatrix} \underset{(n-k)\times k}{\beta'_{12}} & \underset{(n-k)\times(n-k)}{\beta'_{22}} \end{bmatrix}' \,,$$

[1] Our approach is related to Nevo and Rosen (2010) who use weaker covariance restrictions in an IV framework to achieve partial identification, and Evans and Marshall (2009) who identify shocks in VARs with the aid of auxiliary shock measures derived from economic models.

[2] We assume that m_t and ε_{1t} are of the same dimension k. The case where multiple proxy variables are available, i.e. $dim(m_t) > k$, can be dealt with using factor analytic techniques.

with nonsingular β_{11} and β_{22}. Equations (2) through (6) imply that

$$\Phi\beta_1' = \Sigma_{mu'} \; , \tag{7}$$

where henceforth we use the notation $\Sigma_{AB} \equiv E[A_t B_t]$ for any random vector or matrix A_t and B_t. The matrix in (7), which is of dimension $n \times k$, provides additional identifying restrictions but also depends on the k^2 unknown elements of Φ. Because we do not wish to make any assumptions on Φ, equation (7) provides really only $(n-k)k$ new identification restrictions that exploit condition (5). Partitioning $\Sigma_{mu'} = [\Sigma_{mu_1'} \;\; \Sigma_{mu_2'}]$, where $\Sigma_{mu_1'}$ is $k \times k$ and $\Sigma_{mu_2'}$ is $k \times (n-k)$ and using (7), these identifying restrictions can be expressed as

$$\beta_{21}\beta_{11}^{-1} = (\Sigma_{mu_1'}^{-1}\Sigma_{mu_2'})' \; , \tag{8}$$

where the right hand side is a function only of moments of observable variables and therefore independent of Φ. Our approach is based on estimating the matrix $\beta_{21}\beta_{11}^{-1}$ and use it for identification of the objects of interest. In practice, estimation can proceed in three stages:

- **First Stage**: Estimate the reduced form VAR by least squares
- **Second Stage**: Regress the reduced form VAR residuals on m_t and premultiply the estimated coefficients of the last $n-k$ equations by the inverse matrix of estimated coefficients from the first k equations to get an estimate for $\beta_{21}\beta_{11}^{-1}$.
- **Final Stage**: Use the estimates from the previous stages to estimate the objects of interest, if necessary in combination with further identifying assumptions.

A key requirement is the availability of proxies that satisfy the conditions in equations $(4)-(6)$. We propose to use narratively identified measures of exogenous shocks to fiscal variables as proxies for the structural fiscal shocks. The use of narrative accounts has a long standing tradition in macroeconomics in the estimation of the effects of, for instance, fiscal and monetary policy shocks.[3]

[3] Examples include Romer and Romer (1989, 2010), Ramey and Shapiro (1998), Burnside, Eichenbaum and Fisher (2004), Cloyne (2010) and Ramey (2011a).

Existing applications of narrative accounts typically estimate the response to structural innovations by regressing the observables on distributed lags of the narratives or by adding them as variables in a VAR. In most of these applications, the interpretation of the results relies on implicit assumptions on Φ, the covariance between the narratives and the latent structural innovations. Our approach differs in that it does not require assumptions on Φ other than nonsingularity. Contrary to most existing narrative studies, this allows for the possibility of measurement error, which is discussed next.[4]

2 .2 Measurement Error and Reliability

A useful interpretation of the proxy variables is as imperfect measurements of (linear combinations of) latent structural shocks. Such an interpretation is natural in applications where the proxies are specified as narratively identified monetary or fiscal policy changes. Narratives of economic policy are constructed from historical sources that are used to summarize information about the size, timing, and motivation of policy interventions. But historical records can sometimes contradict each other and calls of judgment are in practice impossible to avoid. The likely presence of measurement error invalidates the use of the narratives as direct observations of structural shocks and neglecting measurement error typically results in biased estimates.

Consider an augmented system consisting of the SVAR in (2) and the following system of linear measurement equations,

$$m_t = \Phi \varepsilon_{1t} + \upsilon_t \ , \tag{9}$$

where υ_t is a $k \times 1$ vector of measurement errors with $E[\upsilon_t] = 0$, $E[\upsilon_t \upsilon_t'] = \Sigma_{\upsilon\upsilon'}$ and $E[\upsilon_t \upsilon_s'] = 0$ for $s \neq t$.[5] Note that (9) allows for two types of measurement error: the additive noise υ_t and the fact that m_t can be arbitrarily scaled.

[4]Moreover, our approach offers a more parsimoniously parametrized alternative for narrative measures with relatively few nonzero observations (which is the norm in the literature). In addition, the estimator that we propose identifies not only impulse response functions, but also the entire realized shock sequence in the sample of observations for Y_t and thus permits for instance forecast error variance decompositions.

[5]Depending on the nature of the proxy variables, e.g. discrete versus continuous, it is possible to adopt different specifications for the measurement error equation.

Combining (9) with the SVAR in (2) results in a system of structural equations with latent variables, as discussed in Bollen (1989). Rewrite the model as:

$$Y_t = \theta' X_t^* + w_t \ , \tag{10}$$

where $X_t^* = [Y_{t-1}', ..., Y_{t-p}' \ , \varepsilon_{1t}']'$, $\theta = [\delta', \beta_1]'$ and $w_t = \beta_2 \varepsilon_{2t}$. X_t^* is not fully observable because it contains ε_{1t}. The enlarged system is a measurement error model of the form

$$Y_t = \gamma' \bar{X}_t + z_t \ , \tag{11}$$

$$\bar{X}_t = \Omega X_t^* + \Upsilon_t \ , \tag{12}$$

where $\bar{X}_t = [Y_{t-1}', ..., Y_{t-p}' \ , m_t']'$ and

$$\theta = \Omega' \gamma \ , \ \ w_t = z_t + \gamma' \Upsilon_t \ , \ \ \Omega = \begin{bmatrix} \underset{np \times np}{I} & \underset{np \times k}{0} \\ \underset{k \times np}{0} & \underset{k \times k}{\Phi} \end{bmatrix} \ , \ \Upsilon_t = \begin{bmatrix} \underset{np \times 1}{0} \\ \underset{k \times 1}{\upsilon_t} \end{bmatrix} \ .$$

From $\Sigma_{\bar{X}w'} = 0$, we obtain the standard measurement error formula, see for instance Gleser (1992),

$$\theta = \Omega' \Lambda_{\bar{X}}^{-1} \Sigma_{\bar{X}\bar{X}'}^{-1} \Sigma_{\bar{X}Y} \ ,$$

where $\Lambda_{\bar{X}} = \Sigma_{\bar{X}\bar{X}'}^{-1} (\Sigma_{\bar{X}\bar{X}'} - \Sigma_{\Upsilon\Upsilon'})$ is the reliability matrix of $\bar{X}_t$. Most existing narrative studies estimate a version of (11), often also including lags of m_t. But unless there is no measurement error, the resulting naive estimator $\Sigma_{\bar{X}\bar{X}'}^{-1} \Sigma_{\bar{X}Y}$ is biased. The elements of θ reduce to

$$\delta = \Sigma_{XX'}^{-1} \Sigma_{XY'} \ ,$$

$$\beta_1' = \Phi^{-1} \Sigma_{mY'} \ ,$$

and since $\Sigma_{mY'} = \Sigma_{mu'}$, the three stage procedure described above is equivalent to estimating a measurement error model in which Y_t has perfect reliability and m_t is measured with error.

An advantage of imposing more structure by adopting the measurement error equation (9) is that it allows the use of the statistical reliability of m_t as a diagnostic tool. The $k \times k$ reliability matrix of m_t is given by

$$\Lambda = (\Phi\Phi' + \Sigma_{\upsilon\upsilon'})^{-1}\Phi\Phi' \ , \tag{13}$$

which is a generalization of the reliability ratio of a scalar measurement. When $k = 1$, Λ is the fraction of the variance in the measured variable that is explained by the variance of the latent variable or equivalently the squared correlation between the measure and the true structural shock of interest. When $k \geq 1$, the smallest eigenvalue of Λ corresponds to the smallest scalar reliability of any linear combination of m_t, see Gleser (1992). When an estimate of Λ is available, it can be used for testing the hypothesis that some linear combination of m_t has scalar reliability zero. It provides a metric for evaluating how closely the proxy variables are related to the true shocks, and therefore for the estimability of the structural parameters and the quality of identification. SVAR shocks are sometimes criticized for being at odds with historical events or descriptive records, see for instance Rudebusch (1998). The reliability of proxies constructed from the historical record of policy changes quantifies the extent to which this criticism applies.

Within the SVAR framework it is feasible to identify the reliability matrix. In the case of a single shock $k = 1$, this is always possible without further identifying assumptions. This is because it can be shown that

$$\beta_{11} = \sqrt{\Sigma_{11} - \left(\Sigma_{21} - \beta_{21}\beta_{11}^{-1}\Sigma_{11}\right)' \Gamma^{-1} \left(\Sigma_{21} - \beta_{21}\beta_{11}^{-1}\Sigma_{11}\right)} \tag{14}$$

where $\Gamma = \beta_{21}\beta_{11}^{-1}\Sigma_{11}(\beta_{21}\beta_{11}^{-1})' - \left(\Sigma_{21}(\beta_{21}\beta_{11}^{-1})' + \beta_{21}\beta_{11}^{-1}\Sigma_{21}'\right) + \Sigma_{22}$ and the Σ_{ij}'s are the elements of the appropriate partitioning of the covariance matrix $\Sigma_{uu'}$ of the reduced form VAR residuals. Since the right hand side of (14) involves only observable data moments, it is possible to estimate β_{11} and identify Φ from (7). With an estimate $\hat{\Phi}$ in hand, the scalar reliability of a (mean zero) proxy

m_t can be estimated in a sample of length T by (for $k = 1$),

$$\hat{\Lambda} = \left(\hat{\Phi}^2 \sum_{t=1}^{T} \mathbf{1}_t \varepsilon_{1t}^2 + \sum_{t=1}^{T} \mathbf{1}_t (m_t - \hat{\Phi}\varepsilon_{1t})^2 \right)^{-1} \hat{\Phi}^2 \sum_{t=1}^{T} \mathbf{1}_t \varepsilon_{1t}^2 \ . \tag{15}$$

where $\mathbf{1}_t$ is an indicator function for a nonzero observation of m_t. This estimator always lies in the unit interval. We will also consider specifications with $k > 1$, but we defer the discussion of identification for this case to the relevant section.[6]

3 Do Tax Cuts Stimulate Economic Activity?

This section presents our estimates of the impact of exogenous tax shocks on economic activity in the United States. Here we concentrate on the impact of tax shocks on output and devote special attention to analyzing the robustness of the results. The subsequent section provides evidence for a broad set of macroeconomic aggregates.

Our empirical analysis differs from existing estimates of the effects of unexpected changes in tax policy in three ways. First, we apply the SVAR estimator presented above and identify tax policy shocks with narrative data in a way that is robust to measurement error. Second, we take several steps to ensure that our estimates are not affected by anticipation effects. Third, while much of the macro literature has estimated the impact of changes in the average total tax rate (or in total tax revenues), we investigate the impact of changes in more disaggregated average tax rates. Ideally, one would like to examine the changes in very narrowly defined tax instruments. However, there are practical limits to the level of disaggregation determined by data availability. We concentrate on changes to two tax categories, personal income and corporate income taxes. In our sample, personal income tax revenues (we include contributions to social insurance in our definition of personal in-

[6] An alternative estimator is $\bar{\Lambda} = (N-1)\left(\sum_{t=1}^{T} \mathbf{1}_t m_t^2\right)^{-1} \hat{\Phi}^2$ where $N = \sum_{t=1}^{T} \mathbf{1}_t$. This estimate of the reliability has the disadvantage that in practice it is not necessarily bounded by one since no orthogonality is imposed between υ_t and ε_t such that in finite samples the covariance between the latent shock and the measurement error will be nonzero. Moreover, the proxy variable m_t may have many zeros, which we treat as missing observations so that ε_{1t} will not have unit variance in the subsample of nonzero observations. In our application we typically found the two estimators to be similar.

come taxes) have accounted for on average 74.2 percent of total federal tax revenues while corporate income taxes have accounted for 16.4 percent. Thus, the two components comprise the bulk of total federal tax revenue generation.

The literature instead often distinguishes between labor and capital income taxes, see e.g. Mendoza, Razin and Tesar (1994) or Jones (2002), which is appealing in terms of macroeconomic modeling. However, the division into personal and corporate income taxes corresponds more closely to the actual policy instruments and observed changes in federal tax liabilities can much more easily be assigned to one of these tax categories. The next subsection describes the proxies for each of the two types of tax shocks.

3 .1 A Tax Narrative for Personal and Corporate Income Taxes

We produce a narrative account of legislated federal personal and corporate income tax liability changes in the US for the sample period 1950Q1-2006Q4. The narrative extends Romer and Romer's (2009a) analysis by decomposing the total tax liabilities changes recorded by Romer and Romer (2009a) into the following subcomponents: corporate income tax liabilities (CI), individual income liabilities (II), employment taxes (EM) and a residual category with other revenue changing tax measures (OT). We discard the latter group because it is very heterogeneous.[7] The decomposition is based on the same sources as Romer and Romer (2009a) supplemented with additional information from sources such as congressional records, the Economic Report of the President, CBO reports, etc. whenever required. In an appendix available on our websites, we describe the construction of the data and the historical sources in detail.

To comply with condition (5), which requires that the proxies are orthogonal to all non tax structural shocks, we retain only those changes in tax liabilities that were unrelated the current state of the economy. To this end, we adopt Romer and Romer's (2009a) selection of exogenous changes

[7]They mostly include excise taxes, often targeted to specific industries (transportation) or goods (gasoline, automobiles, sporting goods,...), and gift and estate taxes. See the data appendix for details.

in tax liabilities, which is based on a classification of the motivation for the legislative action either as ideological or as arising from inherited deficit concerns. Another important issue is that many changes in the tax code are legislated well in advance of their scheduled implementation. In Mertens and Ravn (2011a) we distinguish between unanticipated and anticipated exogenous tax changes on the basis of the implementation lag. We find that around half of the exogenous changes in tax liabilities were announced at least 90 days before their implementation and that there is evidence for macroeconomic effects of legislated tax shocks prior to their implementation. These findings mean that condition (6) may fail to hold for the subset of preannounced tax changes. For that reason, we retain only those exogenous tax changes for which the legislation and implementation date are less than one quarter apart. After selecting all exogenous tax liability changes with implementation lags below one quarter, our tax shock measures contain 13 observations of individual income tax liability changes, 2 observations for employment tax liability changes and 16 observations for corporate income tax liability changes. Because there are too few observations for a separate employment tax category, we merge them with the individual income taxes into a personal income (PI) tax category. All our results are very similar if we instead leave out the employment taxes.

We convert the tax liability changes into the corresponding average tax rate changes as follows

$$\Delta T_t^{CI,narr} = \frac{\text{CI tax liability change}_t}{\text{Corporate Taxable Income}_{t-1}}$$

$$\Delta T_t^{PI,narr} = \frac{\text{II tax liability change}_t + \text{EM tax liability change}_t}{\text{Personal Taxable Income}_{t-1}}$$

We scale the tax liability changes by previous quarter taxable income, but our results are nearly identical if we instead scale by the contemporaneous or previous year taxable income. The resulting narrative measures are depicted in Figure 1 together with the average tax rates computed from the national income and product accounts (NIPA) tables. The average personal income tax rate (APITR) is the sum of federal personal current taxes and employee contributions to government social insurance divided by personal income less transfers plus employee contributions for social insurance. The average corporate income tax rate (ACITR) is constructed as federal taxes on corporate income excluding Federal Reserve banks as a ratio of corporate profits. The data appendix provides further details.

The two average tax rates display considerable variation over time. The average rates are very broadly defined and are affected by adjustments to tax rates, tax brackets as well as changes in tax expenditures. Romer and Romer (2009a) describe almost 50 legislative changes in the tax code over the sample period, many containing changes implemented at different points in time. Our narrative measures are a much smaller subset of all these legislated changes because we eliminate all endogenous and/or preannounced tax changes. The average tax rates also display endogenous movements unrelated to legislative changes to the tax code that occur for a variety of reasons, such as cyclical fluctuations in the administrative definition of taxable income versus NIPA income, tax progressivity and changes in the distribution of income, cyclical variations in tax compliance and evasion, etc. Even though total federal revenues as a share of GDP have remained fairly stationary around 18 percent, the APITR and ACITR measures both display trends over the sample. Figure 1 shows that the APITR has slowly risen from around 10 percent at the beginning of the sample to approximately 18 percent at the end of 2006. The two most significant exogenous changes in personal income taxes according to our narrative measure relate to the Revenue Act of 1964, which reduced marginal tax rates on individual income, and to the Jobs and Growth Tax Relief Reconciliation Act of 2003, which reduced marginal tax rates on individual income, capital gains and dividends and increased some tax expenditures. Each of these two pieces of legislation cut average personal income tax rates by more than one percentage point according to the narrative measure. The ACITR instead has fallen significantly over time from over 50 percent in the early 1950s to just above 20 percent at the end of the sample period. The narrative measure indicates several instances of sizeable changes in corporate income taxes, the biggest one being a large increase in corporate tax liabilities associated with the repeal of the investment tax credit included in the Tax Reform Act of 1986.

We use the new tax narratives depicted in Figure 1 as proxies for structural tax shocks. In the benchmark specification, the proxies are simply the demeaned narrative shocks. We checked whether lagged macro variables have predictive power for the narratively identified shocks but on the basis of standard Granger causality tests we found no such evidence.[8]

3 .2 Benchmark Specification

Our benchmark SVAR specifications include four variables in the vector of observables: $Y_t = [T_t^i$, $\ln(B_t^i)$, $\ln(G_t)$, $\ln(GDP_t)]$. T_t^i is the average tax rate of tax type $i = PI, CI$, i.e. federal personal and corporate income tax revenues as a fraction of the respective taxable income categories; B_t^i is the real per capita personal and corporate taxable incomes, respectively; G_t is real per capita government purchases of final goods; and GDP_t is real per capita gross domestic product. All fiscal variables are for the government at the federal level. Precise data definitions are provided in the appendix. When estimating the impact of changes in personal (corporate) income taxes we use the personal (corporate) income tax narrative described above as the proxy. Unless mentioned otherwise, our sample has quarterly observations for 1950Q1-2006Q4. On the basis of Akaike information criterion lag order selection tests, we include four lags of the endogenous variables, and also include a constant and linear/quadratic trend terms in all regressions. Our choice for a deterministic trend deserves some discussion, especially given the apparent nonstationarity in the average tax rates in Figure 1. In reality, the vast majority of legislative changes in our sample are intended by legislators to be permanent. With a deterministic trend, this is consistent with an interpretation of the structural tax shocks as random transitory fluctuations around a predictable long run trajectory. Alternatively, one may assume a stochastic trend and adopt a specification in first differences of Y_t to allow for permanent effects of tax shocks. We do this in the robustness section and find that this makes very little difference for the short run responses (within the first 2.5 years). Therefore we mostly present results for the case of a deterministic trend.

[8]Tests of the null hypothesis that the average tax rate, GDP, government spending and the tax base (deterministically detrended) do not Granger cause the narrative shock measure have p-values of 0.68 for the PI tax shock measure and 0.33 for the CI tax shock measure. Using first differences of the vector of observables increases the p-values to 0.79 and 0.69, respectively.

We report the impulse responses following 1 percentage point decrease in either of the two tax rates for the first 20 quarters along with 95% confidence intervals. The latter are computed using a recursive wild bootstrap, see Gonçalves and Kilian (2004), using 10,000 replications.[9] In each figure we also report the impact on tax revenues and estimates of the tax multipliers. The responses of tax revenues were computed as

$$\widehat{tr}_t = \frac{\hat{T}_t^i}{\bar{T}^i} + \hat{b}_t$$

where $\bar{T}^i$ is the mean average tax rate of type i in the sample, $\widehat{x}_t$ denotes the impulse response of x_t and lower case letters denote logged variables. Tax multipliers are simply rescaled versions of the the output response such that the tax cut reduces tax revenues by 1% of output.

3 .3 Benchmark Results

Figure 2 depicts the impact of a 1 percentage point decrease in the average personal income tax rate. After the initial cut, the APITR remains significantly below trend for the first 5 quarters and then gradually returns to trend. The cut in the APITR sets off a significant increase in the personal income tax base which initially rises approximately 0.5% and peaks at 1.2% above trend 7 quarters after the tax cut. Combining the responses of the tax base and the personal income tax rate, the decrease in the APITR implies a drop in personal income tax revenues of 5.5% upon impact. The fall in tax revenues remains significant for the first three quarters after the tax cut and turns into a small but insignificant increase in tax revenues 7 quarters after the cut in taxes. Thus, despite a substantial increase in the tax base we find that cuts in personal income taxes unambiguously lower tax revenues.

Cuts in average personal income taxes provide a short run output stimulus. We find that a one percentage point decrease in the APITR leads to an increase in aggregate output of 0.8% in the first quarter and a peak at 1.6% above trend 6 quarters after the tax cut. The confidence intervals for

[9] In every bootstrap sample we multiply every u_t and m_t with a random variable taking on values of -1 or 1 with probability 0.5. Thus, our bootstrap inference procedure also takes into account uncertainty about identification and measurement.

the output responses are relatively narrow and indicate a significant increase (at the 95% level) in output within a 3 year window after the initial tax cut. Translating these estimates of the output response into a personal income tax multiplier, we find an tax multiplier of 1.15 on impact rising to a maximum of 2.3 at the 6 quarter horizon.

Figure 3 shows the impact of a 1 percent decrease in the average corporate income tax rate. The cut in the ACITR is a little less persistent than the APITR cut and gives rise to a large and significant temporary increase in the corporate income tax base which rises by more than 3 percent in the first 6 months after the tax cut. The increase in the tax base is sufficiently large that the corporate income tax cut leads to a small decline in corporate income tax revenues only in the first quarter and a surplus thereafter. The response of corporate tax revenues is however insignificant at every horizon. Hence, we find that cuts in corporate income taxes are approximately self-financing. This suggest that, in contrast to personal income taxes, the US economy on average has been very close to the top of the corporate tax Laffer curve.[10]

The output effects of ACITR cuts are significant and sizeable. We find that a one percentage point decrease leads to a rise in aggregate activity of around 0.5% which increases slightly to a maximum of 0.7% in the 5th quarter. Since the impact on revenues is small, the implied corporate tax multiplier is very large. This is because multipliers express the impact of tax changes in terms of their revenue impact such that the multiplier is not well defined when there is little change in revenues.

In accordance with Romer and Romer (2009b), we find little impact of either tax shocks on government spending. Figure 2 shows that the response of government spending to an APITR tax cut is insignificantly different from zero at all forecast horizons at the 95% level. Similarly, there is no evidence that changes in the ACITR impact on government spending. This is reassuring since it refutes the possibility that the responses to tax shocks are confounded with changes in government

[10]See Trabandt and Uhlig (2009) for an argument based on a calibrated DSGE model that there is little scope for raising tax revenues with capital income taxes in the US.

spending.

The estimation procedure delivers an estimate of the reliabilities of the tax narratives. We find an estimate of the reliability of the personal income tax narrative of 0.60 with a 95% confidence interval of 0.39-0.66. This implies a point estimate of the correlation between the narrative and the estimated structural shock of 0.77. The reliability estimate for the corporate income tax rate is 0.23 with a confidence interval of 0.09-0.46, which indicates a correlation between the narrative and the structural shock of 0.48. Therefore, we find a somewhat weaker relationship between the structural shock and the narrative for corporate taxes than for personal taxes. One likely reason for this finding is that changes affecting average corporate income tax rates tend to be more heterogenous in nature than those affecting average personal income tax rates. Nonetheless, the corporate income tax narrative is still informative about the latent structural shock. Thus, in both cases there is a reasonably strong connection between the SVAR shocks and historically documented legislative changes to the tax code.

Perhaps the most important result that we uncover is that the estimated short run output effects of tax changes are relatively large, either when measured as output semi-elasticities or multipliers. There are relatively few studies which we can use for direct comparison, as most macro estimates are for shocks to total taxes. A noteable exception is Barro and Redlick (2011), who estimate the impact of changes in a measure of taxes related to our APITR variable. Using annual data, they consider the output response to changes in average marginal income tax rates (AMTRs) which includes state taxes, excludes most forms of capital income taxes, and makes no adjustment for anticipation effects. In contrast, our measure excludes state income taxes, includes capital income taxes that are not classified as corporate income taxes, and eliminates all anticipated tax changes. Identification in Barro and Redlick (2011) relies on using the year-aggregated Romer and Romer (2009a) series for exogenous total tax liability tax changes as an instrument for AMTR shocks. Based on annual data they find a tax multiplier of around 1.1. The first quarter output multiplier according to our estimates is 1.15 and the rising profile of the tax multiplier means that the average over the first 4 quarters is

1.66. Thus, our estimates are somewhat higher, although the Barro and Redlick (2011) estimate is within our 95% confidence interval. One possible explanation for our higher estimates, for which we provide evidence in Mertens and Ravn (2011c), is that failure to exclude preannounced tax changes leads to a downward bias in the estimated tax multipliers. This is because forward looking agents and intertemporal substitution motives generate a tendency for preannounced cuts in personal income taxes to lower output prior to implementation.[11]

Blanchard and Perotti (2002) estimate the impact of shocks to total tax revenues using an SVAR estimator. They find an impact multiplier of 0.69 and a peak multiplier of 0.78 in quarterly US data for the sample period 1947-1997. Even though they include tax revenues at all levels of government, our estimates suggest significantly larger aggregate tax multipliers than their estimates. Mertens and Ravn (2011c) provide a detailed analysis of this result and argue that the key discrepancy relates to the elasticity of tax revenues to output and that the Blanchard-Perotti estimates suffer from a negative endogeneity bias.[12] Mountford and Uhlig (2009) also analyze shocks to aggregate tax revenues identified using sign restrictions. In response to a deficit financed tax cut, they estimate multipliers of 0.29 on impact, 0.93 after one year and up to 3.41 at twelve quarters. These numbers are much larger at longer horizons, but similar to Blanchard and Perotti (2002) in the short run. This contrasts with our finding of large output effects in the shorter run.

Romer and Romer (2010) estimate the impact of innovations to their aggregate tax liability narrative and find that a one percent drop in legislated tax liabilities relative to GDP leads to an increase in GDP of less than half a percent on impact growing steadily to a 3% increase at the 10 quarter horizon. Again, these estimates are not directly comparable to ours since we consider disaggregated taxes, but as with the SVAR based estimates the main difference with our findings is the large output effects in the short run. In order to provide a more direct comparison between our results and

[11] See Yang (2005), Mertens and Ravn (2011a,b) and Leeper, Walker and Yang (2011) for theory and evidence.

[12] Blanchard and Perotti (2002) calibrate the output elasticity of tax revenues to 2.08 while we estimate a larger elasticity. Mertens and Ravn (2011c) show that (i) the lower elasticity produces simultaneity bias, and (ii) that the Blanchard-Perotti approach delivers a tax multiplier practically identical to our estimate when this elasticity is adjusted to be consistent with narrative data.

those of standard narrative approaches, we estimate the impact of a changes in taxes based on the assumption that the narratively identified shocks map one-to-one into structural shocks. We report results based on the following two specifications:

$$\Delta \ln(GDP_t) = \alpha + \sum_{s=1}^{K} \beta_s \Delta T_{t+s-1}^{i,narr} + e_t \qquad (16)$$

$$Y_t = \delta' X_t + \gamma \Delta T_t^{i,narr} + u_t \qquad (17)$$

where $\Delta T_t^{i,narr}$ $(i = PI, CI)$ are the narratively identified tax changes. The first of these specifications is a simple regression of output growth on the contemporaneous and lagged narrative, which is the approach of Romer and Romer (2010). The second specification in (17) adopts a reduced form VAR that includes the narrative as an exogenous regressor, as in for instance Favero and Giavazzi (2011). When estimating (16) we set $K = 12$. Figure 5 illustrates the resulting impulse response functions to one percentage point cuts in $\Delta T_t^{i,narr}$ together with our benchmark results.

The specifications in $(16) - (17)$ imply substantially smaller point estimates of the output effects of tax changes than our benchmark. This is particularly evident for the corporate income tax cut where the output responses to a tax cut derived from (16) and (17) are close to zero at all forecast horizons and significantly smaller than our benchmark estimates during the first 7 quarters after the tax cut. For the personal income tax, the output responses produced by (17) are smaller (but insignificantly so) than the benchmark estimates at all forecast horizons. Specification (16) also delivers estimates of the impact of cuts in the personal income tax that are considerably smaller at all horizons.

There are two reasons for why we find a larger impact of tax cuts on output than would be implied by standard narrative approaches. First, there is an important difference in the scaling of the shocks since we scale the shocks by their impact on actual average tax rates while the Romer and Romer (2010) multiplier estimates are based on *projected* tax liability calculations which are in turn typically based on the assumption that output (and other determinants of tax revenue) does not respond to changes in taxes. Since we find that taxable income expands following a tax cut, the tax changes implicit in $\Delta T_t^{i,narr}$ are smaller than those assumed in the structural estimates we report. Secondly, as we discussed above, our estimator allows for the presence of additive measurement error in the narrative accounts. Ignoring this type of measurement error typically yields attenuation bias which manifests itself in smaller estimated output responses.[13] The fact that the output response is more severely downward biased for the corporate income tax cut is consistent with our finding that the ACITR narrative has lower reliability. Interestingly, Perotti (2011) updates the Romer and Romer (2009a) series with the aim to improve measurement and as a result also finds tax multipliers that are relative larger.

3 .4 Robustness

We investigate the robustness of our main results with respect to several issues. First, we show how the estimates depend on our assumption of trend stationarity. Second, we extend the information set to include a number of variables that are informative about future changes in fiscal policy. Third, we adopt a specification that takes into account a nonzero correlation between innovations in APITRs and in ACITRs. Finally, we examine whether expanding the vector of observables with real government debt makes a difference.

A. Stochastic Trend Given that most of the tax changes that underly our narrative series are intended to be permanent changes to the tax code, it is not a priori clear that our specification should not allow for permanent effects of tax shocks. SVAR results can be somewhat sensitive to assumptions about trends, as in for instance Blanchard and Perotti (2002). Figure 4 shows the results for a VAR that includes the vector of observables in first differences. Unlike the benchmark specification, both the APITR (left panel) and ACITR (right panel) shocks now lead to permanent changes in average tax rates. The long run decrease in the APITR is smaller than the initial shock, whereas the long run change in the ACITR is identical to the initial cut. Despite the difference in the response of

[13]One should not jump to the conclusion that all narrative results in the literature are downward biased because of measurement error. When lags of narrative measure are included on the RHS of a regression, measurement error does not necessarily lead to attenuation. Moreover, some studies, such as Ramey (2011a), do rescale the impulse responses according to the impact on one of the observables. This can substantially mitigate the problem.

the average rates, essentially none of our conclusions regarding the short to medium run effects of tax shocks for the other variables are affected. Figure 4 shows output responses that are remarkably similar to the benchmark estimates for at least the first 10 quarters. At longer horizons, a 1% cut in the APITR leads to permanent increase in output of 1.5%, while a 1% cut in the ACITR raises output permanently by 0.5%. Our primary focus here is on the short run effects of tax shocks and these do not depend much on trend assumptions.[14]

B. Controlling for Expected Future Tax Rates To avoid anticipation effects, we have eliminated all tax liability changes that were implemented more than 90 days after the relevant tax changes became law. In Mertens and Ravn (2011a) we find no significant effects in the quarters leading up to aggregate tax changes that we classified as unanticipated. One might still worry that we do not fully address the possibility of tax foresight as tax changes may have been anticipated even before legislation. The mistiming of shocks and/or the omission of an important variable can potentially lead to misleading results, see Leeper, Walker and Yang (2011), Ramey (2011a) and Mertens and Ravn (2010).

We address this issue by extending our benchmark analysis with a measure of expected future taxes derived from the municipal bond prices obtained from Leeper, Walker and Yang (2011). Municipal bonds are exempt from federal income taxation in the US and the spread between the yields on municipal bonds and similar nonexempt bonds may therefore contain information about the market expectation of the present value of income taxes over the maturity of the bond. Indeed, several authors have demonstrated that the municipal bond spread has predictive power for income tax changes, see e.g. Poterba (1988) and Fortune (1996). A measure of implicit expected future taxes can be derived from yield spreads and a no arbitrage assumption, see Leeper, Walker and Yang (2011) for details. We use their measure for bonds with maturity of one year.

Figure 6 depicts the impact on GDP of one percentage point cuts in the APITR and ACITR when

[14] In terms of economic theory, however, whether displacements in tax rates are perceived by agents as permanent or transitory does matter importantly, see for instance Chetty et al. (2011).

we extend the vector of observables with this measure of expected future tax rates. Because data for this variable is only available since 1953Q2, the sample was shortened correspondingly. For comparison, we also show the benchmark impulses with their confidence bounds. The output response to a cut in the APITR is very similar to the benchmark and well within the 95% confidence interval. Including the measure of expected tax rates matters more for the impact of the ACITR cut, which is now significantly larger than the benchmark estimates. This perhaps reflects Miller's (1977) arguments that the marginal bond holder is taxed at the corporate tax rate.

In Figure 7 we report the results from an alternative exercise that aims at eliminating any remaining predictable components of our tax narratives.[15] To this end, we first regressed the nonzero observations of our narrative tax measures on two lags of the implicit expected tax rate variable and then use the residuals as the proxies for the structural shocks. The right panel of Figure 7 shows that projecting the ACITR narrative on the implicit tax rate produces output responses that are nearly identical to the benchmark estimates. For the personal income tax cut, the output response is now somewhat smaller, though still well within the 95% bounds of the benchmark estimates. Overall, we find no evidence that the finding of large output effects of tax cuts is affected by further controlling for tax foresight.

C. Controlling for Defense Stock Prices and Defense News Anticipation effects may be relevant not only for tax changes but also for government spending. While our principal interest is in estimating the impact of tax shocks, preannounced changes in government spending that are not controlled for may also give rise to problems of omitted variable bias and misalignment of the information sets of the econometrician and economic agents. Ramey (2011a) for instance argues that anticipation effects are crucial for the identification of government spending shocks.

We address this concern in two alternative ways. First, we extend the vector of observables with

[15]We also conducted standard Granger causality tests for the entire tax narrative series. Tests of the null hypothesis that the measure of expected tax rates do not Granger cause the narrative shock measure have p-values of 0.64 and 0.86 for the personal income and corporate income narratives, respectively.

an asset price that is likely to contain information about future changes in government spending. In particular, we include a defense sector stock returns variable, which is a series for the accumulated excess returns of large US military contractors constructed by Fisher and Peters (2010). Alternatively, we include Ramey's (2011a) defense spending news variable in the vector of observables. This narrative is based on professional forecasters' projections of the path of future military spending and therefore contains information about anticipated changes in government spending.

The results are shown in Figure 6. We find that including information about future changes in government spending matters little for our estimates of the impact of changes in the APITR and ACITR on output. Not only are the output responses within the 95% confidence intervals of the benchmark, but they are also very close to the benchmark point estimates.

D. Allowing for Correlation Between APITR and ACITR Innovations One potential complication with the interpretation of the structural impulse responses in the benchmark specifications is possible contemporaneous correlation between the tax narratives. Our estimates are derived from regression models that consider the impact of ACITR changes and APITR changes separately. In practice, several legislative actions involve changes in multiple tax instruments. In our sample of nonzero observations of the narrative tax shocks, the correlation between the two tax series is 0.42. This correlation is natural for a number of reasons. The tax narratives record changes in tax liabilities for which the historical documents indicate that they were not explicitly motivated by countercyclical considerations. Yet they of course still occurred with certain objectives in mind, typically related to longer run goals for economic growth or debt reduction. When both personal and corporate income taxes are adjusted simultaneously, it is therefore not surprising that they are often adjusted in the same direction. Also, given that the tax narratives are based on actual legislative actions, the fixed costs of passing legislation naturally imply a temporal correlation of the changes in different types of taxes.

The interpretation of our benchmark results as capturing purely the effect of a single tax type depends on how one interprets the correlation between changes in the APITR and in the ACITR. Ideally one would like to identify a structural relationship between the measured changes in both tax categories. This would however require, in our view, arbitrary assumptions on how for instance personal income taxes respond contemporaneously to unanticipated changes in corporate taxes and vice versa. Instead we follow an alternative route which is to estimate the effects of 'orthogonalized' tax shocks. By this we mean shocks which affect the average tax rate of one type while leaving the other unchanged in cyclically adjusted terms. To this end, we estimate a VAR which, on top of GDP and government spending, now includes both the ACITR and the APITR. The vector of observables is therefore $Y_t = [T_t^{PI}, T_t^{CI}, \ln(G_t), \ln(GDP_t)]$. We posit the following structural relationship between the VAR residuals and the structural tax shocks:

$$\begin{aligned} u_t^T &= \eta_G \sigma_G \varepsilon_t^G + \eta_Y u_t^{GDP} + \Sigma_T \varepsilon_t^T , \\ u_t^G &= \gamma_T' \Sigma_T \varepsilon_t^T + \gamma_Y u_t^{GDP} + \sigma_G \varepsilon_t^G , \\ u_t^{GDP} &= \zeta_T' u_t^T + \zeta_G u_t^G + \sigma_Y \varepsilon_t^{GDP} , \end{aligned} \tag{18}$$

where u_t^T and ε_t^T are the 2×1 vectors of reduced form and structural tax rate innovations, respectively, and Σ_T is a 2×2 matrix with potentially nonzero off-diagonal elements. The parameters η_Y and γ_Y measure the cyclical sensitivity of the average tax rates and spending respectively; η_G, γ_T and the off-diagonal elements of Σ_T capture the interdependence between fiscal instruments; and ζ_T and ζ_G parametrize the contemporaneous dependence of economic activity on fiscal policy. In the appendix we show that under the additional identification assumption that $\gamma_Y = 0$, i.e. government spending does not respond contemporaneously to output, it is possible to identify all parameters and the reliability matrix except for the elements of Σ_T. This suffices to estimate the impulse responses to tax shocks that move the cyclically adjusted tax rate of one category while leaving the other unchanged. Note that the responses to such orthogonalized tax shocks are lower bound estimates of the impact of a tax change of a certain type since they more than likely entail a partially offsetting change in the tax rate of the other type.

Figure 8 plots the response to the orthogonalized tax shocks. The results are qualitatively very similar to the benchmark estimates. Quantitatively, the impact on output of a one percentage cut in the APITR is smaller than in the benchmark model but still comfortably within the 95% confidence interval of the benchmark estimates. The attenuating effect on the output effects of an ACITR cut is more pronounced, yielding estimates that in the medium run are up to 50% smaller than the benchmark estimates. Nonetheless, the estimates remain within the 95% benchmark confidence interval. The finding of smaller output effects to orthogonalized tax shocks is not too surprising given that we force the response of the other cyclical adjusted tax rate to be zero.[16] The fact that this exercise produces estimates of the output effects of tax changes that that are reasonably similar to the benchmark gives us greater confidence in our benchmark results.

E. Controlling for Debt The final robustness exercise examines whether our results are sensitive to omitting government debt from the vector of observables. One might argue that government debt is an important variable since any change in taxes eventually must be accompanied by (future) adjustments in the fiscal instruments. Especially if the reaction to debt is strong and relatively fast, it might be inappropriate not to explicitly allow for feedback from debt to taxes and spending. To examine this issue we extend the vector of observables with real government debt (nominal debt deflated by the implicit GDP deflator and per capita). Figure 9 illustrates the output impact of decreases in the APITR and in the ACITR for this specification. For the APITR we find little relevance of controlling for government debt. The estimated impulse responses are as good as identical to the benchmark estimates. For the ACITR we find instead somewhat larger output effects when controlling for government debt. In any case, we find no signs that our finding of relatively large short run output effects of tax changes is altered by controlling for government debt.

[16] The reliability matrix has eigenvalues of 0.35 and 0.61. The appendix provides more details on how we identify this matrix in this case.

4 The Wider Macroeconomic Effects of Tax Changes

One advantage of the narrative identification approach is that it is straightforward to estimate the effects of shocks on other macroeconomic variables. Looking beyond the impact of tax changes on output or revenues allows us to gain further insight into how tax changes are transmitted to the economy and into possible differences between both tax components.

4 .1 Do Tax Cuts Create Jobs?

The labor market often takes center stage in discussions on fiscal policy. Romer and Bernstein (2009), for example, argue that "*Tax cuts, especially temporary ones, and fiscal relief to the states are likely to create fewer jobs than direct increases in government purchases.*" However, systematic empirical evidence on the dynamic effects of fiscal interventions on employment is surprisingly scarce. Ravn and Simonelli (2007) and Monacelli, Perotti and Trigari (2010) find that positive shocks to government spending impact negatively on the unemployment rate, but the response is very slow. Monacelli, Perotti and Trigari (2010) investigate the effects of tax shocks on unemployment and other labor market variables and find that tax cuts lead to delayed but sizeable reductions in unemployment.

To investigate the impact of tax changes on the labor market we extend the vector of observables with the log of total employment per capita, the log of hours worked per worker and the log of the labor force relative to population, all for the aggregate business, government (including military) and non profits sectors (see the appendix for precise data definitions). Combining these variables, we can also derive estimates of the impact of tax shocks on the unemployment rate. Figure 10 depicts the impact of a one percent cut in the APITR (left column) and in the ACITR (right column). The first row shows the output responses which are very similar to the benchmark. We will therefore concentrate on discussing the labor market effects.

Cuts in personal income taxes boost employment and do so relatively quickly. A one percentage point decrease in the APITR leads to an rise in employment per capita of 0.3% on impact, although the response becomes only statistically significant after the first quarter. The employment response peaks at around 0.9% above trend 6 quarters after the tax stimulus. The labor input response to an APITR tax cut is however not restricted to the extensive margin. Hours per worker also rises significantly on impact by 0.4% and peaks in the fourth quarter at around 0.75%. In contrast to the fairly elastic short run responses of labor input at both the intensive and extensive margins, we find no evidence for effects on labor force participation at any horizon. This is perhaps not surprising given that, at least under the assumption of trend stationary tax rates, the reduction in the APITR is fairly transitory, and therefore probably provides only limited incentives to enter the labor market. The increase in employment and lack of any effect on participation together imply a decrease in the unemployment rate of 0.2% on impact and a maximum decrease of 0.6% in the fifth quarter after the tax cut.

The results for the ACITR depicted in the right column of Figure 10 indicate that changes in corporate taxes have much less pronounced effects on the labor market. In contrast to the personal income tax cut, there is no evidence that a cut in corporate taxes is associated with any significant impact on employment, despite the considerable and significant immediate increase in output. Instead, there is a gradual rise in employment that becomes statistically significant in the fifth and sixth quarters before reverting to trend. The maximum increase in employment after a one percent cut in the ACITR is 0.5%. Another difference with the cut in personal income taxes is that there is no significant impact on hours per worker at any horizon. As was the case with the APITR cut, labor force participation is unaffected. We find that a cut in corporate taxes eventually does lower the rate of unemployment, but the effect is very gradual and only marginally statistically significant approximately one year and a half after the tax stimulus.

We draw two conclusions from our study of the labor market effects of tax changes. First, there are important differences in how personal and corporate income tax changes affect the labor market. Studies that focus exclusively on total average tax rates or revenues are therefore only of limited use for assessing the ability of tax policy to affect employment at various horizons. The second conclusion is that when the prime policy objective is to create jobs relatively fast, cuts in personal income taxes are probably the best fiscal instrument.[17] The employment effects of cuts in corporate taxes are delayed and less certain. The studies cited above suggest that the same is true for government spending increases.

4 .2 Spending and Saving

Changes in taxes are often implemented with the aim of stimulating private consumption or of setting the economy on a path of higher investment and higher prosperity in the long run. Thus, it is interesting to examine how tax changes affect private sector spending and savings.

Figure 11 shows the responses of private consumption expenditure of nondurable goods and services, nonresidential investment, and the personal savings rate following a one percent point cut in the APITR (left column) and in the ACITR (right column), respectively.[18] In response to a cut in the APITR, consumption jumps by 0.3% on impact and subsequently increases gradually to a peak response of just above 0.6% around 2.5 years. The consumption response appears roughly consistent with permanent income predictions for persistent changes in disposable income: it is more muted and smoother relative to the response of personal income (shown in Figure 2). This is also evident from the response of the personal savings rate, which is positive for the first year after the shock and statistically significant on impact. The positive consumption response to an APITR cut contrasts sharply with the response to a cut in the ACITR, which induces a decline in consumption that is statistically significant for roughly the first year. Since a corporate tax cut more or less directly increases the return on saving, the consumption decline is indicative of substitution effects dominating income effects. Not surprisingly, the personal savings rate also clearly responds positively to the

[17]Monacelli, Perotti and Trigari (2011) also separately estimate the effects of business and labor taxes. When expressed in terms of multipliers, our results are entirely consistent with their finding that the effects of business taxes on employment are larger than those of labor taxes. Relative to their estimates, our results imply larger effects on unemployment which in the case of labor taxes are also more immediate.

[18]The precise definitions and sources of each of these variables are listed in the appendix. Following the approach in Burnside, Eichenbaum and Fisher (2004), we estimate these responses by adding each variable separately to the vector of observables considered in the benchmark VAR specifications.

corporate tax cut.

The impact on private nonresidential investment is more uniform across the two tax components. A one percentage point cut in the APITR sets off a 1.2% increase in nonresidential investment in the quarter of the tax cut rising to a maximum of 4% after five quarters. The corresponding numbers for the ACITR are an impact increase in nonresidential investment of 1% and a peak increase of 2.5% after one year. In terms of output elasticities, these numbers imply roughly similar effects of changes in APITR and ACITR on investment.

In summary, changes in taxes impact importantly on key spending components but again there is an important difference between personal and corporate income taxes. Changes in either type of taxes stimulate nonresidential investment but only personal income tax cuts have short run positive effects on consumption, whereas corporate tax cuts lower consumption.

4 .3 Interest Rates and Inflation

Changes in taxes may impact on costs of production and, to the extent that cost changes are passed into prices, affect inflation. The sign of the inflation response is directly informative for whether the expansionary effects of tax cuts are primarily derived from increased demand or supply for final goods. The impact of taxes on inflation is also important because it may lead to monetary policy adjustments that in theoretical models are typically very important in determining the ultimate effects of fiscal shocks. In Figure 12 we show the impact of tax changes on inflation and the nominal rate rate on the 3 month T-Bill. Our measure of inflation is based on the business sector output price deflator as opposed to for instance the CPI, as in the latter the prices of imports and of the services provided by owner occupied homes are particularly important (see the data appendix).

A cut in the APITR is mildly disinflationary in the short run but inflationary at longer horizons. We find a stronger negative impact of a cut in the ACITR on the inflation rate in the short run and, in contrast to the results for the APITR, the decline in inflation is statistically significant at the 95%

percent level in the first two quarters. The significant and immediate short run drop in inflation after a corporate tax cut is consistent with a fall in marginal costs and dominating supply side effects. The evidence for changes in personal income taxes is less conclusive.

We find no evidence that changes in either of the two tax rates impact significantly on the short term nominal interest rate. For the APITR this result is not too surprising given that we do not find any significant impact on the inflation rate. For the ACITR instead, the short run decline in the inflation rate following a tax cut might instead have been expected to trigger a monetary policy accommodation. There are various possible explanations including that the drop in inflation is accompanied by an increase in aggregate activity and that the impact on inflation is very transitory. In any case, our results indicate little short run interaction between monetary and discretionary tax policies.

5 Concluding Remarks

Our analysis shows that changes in taxes have important consequences for the economy. This is important given the current debate on the efficacy of fiscal policy and on the possible consequences of the fiscal consolidation that is bound to take place over the coming years. The evidence we contribute in this paper is supportive for (i) relatively large and immediate output effects following changes in average tax rates (ii) tax multipliers that are larger than most estimates of government spending multipliers (iii) personal income tax cuts being more effective in creating jobs and stimulating consumption in the short run than cuts to corporate profit taxes and (iv) changes in corporate tax rates being approximately revenue neutral.

A key finding is that there are important differences in the effects on various macroeconomic aggregates after distinguishing between different types of taxes. Studies that focus on changes in total tax revenues alone can therefore only provide limited insight into a complex tax transmission mechanism and offer little guidance for judging the relative merits of different types of tax changes. On the other hand, the shocks to average tax rates that we identify still reflect changes to marginal tax rates, tax brackets as well as tax expenditures, all of which in principle have distinct influences on economic decisions. The main benefit of such aggregation is that it allows for controlling for macroeconomic conditions as traditionally emphasized in the macro literature. This approach is complementary to single event studies of macro data, such as House and Shapiro (2006) or Chetty, Guren, Manoli and Weber (2011), that do not explicitly control for macroeconomic conditions but can incorporate much greater legislative detail.

There are several interesting avenues for future research. First, we believe that it would be interesting to apply the methodology to data from other countries. Tax narratives are becoming increasingly available, see e.g. Cloyne (2010) for a UK tax narrative and the International Monetary Fund (2010) for a tax narrative for a broad selection of countries. It is likely that measurement errors are systematic features of these accounts making our approach attractive. Secondly, it would be interesting to confront the evidence that we have uncovered with macroeconomic models in order to examine its congruence with economic theory. Third, the methodology that we propose lends itself to applications to government spending and monetary policy where narrative policy measures are available. The methodology can also be used without availability of narrative measures as long as other proxies are available. Such applications could be very helpful in bringing about further evidence about the impact of structural shocks.

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A Data Definitions and Sources

Benchmark Variables

Output is GDP in line 1 from NIPA Table 1.1.5; **Government spending** is Federal Government Consumption Expenditures and Gross Investment in line 6 from NIPA Table 3.9.5; The **personal income tax base** is NIPA personal income (Table 2.1 line 1) less transfers (Table 2.1 line 16) plus employee contributions for social insurance (Table 2.1 line 24); The **corporate income tax base** is NIPA corporate profits (Table 1.12 line 13). These series are all deflated by the GDP deflator in line 1 from Table 1.1.9 and by the total population over age 16 obtained from Francis and Ramey (2009) (*nipop16*); The **average personal income tax rate** is the sum of federal personal current taxes (Table 3.2 line 3) and employee contributions to government social insurance (Table 2.1 line 24) divided by personal income less transfers plus employee contributions for social insurance; The **average corporate income tax rate** is NIPA federal taxes on corporate income excluding Federal Reserve banks (Table 3.2 line 9) divided by corporate profits.

Other Variables

The **implicit tax rate** is based on the 1 year municipal and treasury bond spread and is described in Leeper, Walker and Yang (2011); **Defense returns** are excess stock returns of the largest military contractors and is described in Fisher and Peters (2010); **Defense news** is the narrative series for defense spending described in Ramey (2011a); **Employment/Population** is the sum of total civilian employment (Bureau of Labor Statistics, series LNS12000000) plus military employment, measured as the difference between the total population over age 16 and the civilian population over age 16 (Bureau of Labor Statistics, series LNU00000000), divided by population ; The **Labor Force/Population** is the sum of the total civilian labor force (Bureau of Labor Statistics, series LNS11000000) and military employment divided by population; **Hours per worker** is the total hours worked (*tothrs*) series from Francis and Ramey (2009), which includes the government and non-profits sectors, divided by employment.

Inflation is the annualized quarterly percentage change in the implicit price deflator for the non-farm business sector (FRED ID: IPDNBS); The **nominal interest rate** is the interest rate on the 3 month Treasury Bill; **Consumption** is the aggregated chained nondurable and services consumption (*rcndsv*) obtained from the online data appendix to Ramey (2011a); **Nonresidential investment** is the quantity index (Table 1.1.3 line 9) divided by the population; The **personal savings rate** is household net saving (NIPA Table 5.1 line 8) as a ratio of personal income. **Debt** is real consolidated public debt held by the public (excluding trust funds) per capita, constructed by multiplying the Debt/GDP series from Favero and Giavazzi (2011) by nominal GDP (line 1 from NIPA Table 1.1.5) and dividing by the GDP deflator and the population.

B Identification of Orthogonalized Tax Shocks

In this appendix, we discuss identification of the system in (18) with multiple tax shocks. From $\Sigma_{uu'} = \mathcal{B}\mathcal{B}'$, we have

$$\begin{aligned}
\beta_{11}\beta_{11}' &= \Sigma_{11} - \left(\Sigma_{21} - \beta_{21}\beta_{11}^{-1}\Sigma_{11}\right)'\Gamma^{-1}\left(\Sigma_{21} - \beta_{21}\beta_{11}^{-1}\Sigma_{11}\right) \\
\beta_{22}\beta_{22}' &= \Gamma + \left(\Sigma_{21} - \beta_{21}\beta_{11}^{-1}\Sigma_{11}\right)\left(\beta_{21}\beta_{11}^{-1}\right)' + \left(\beta_{21}\beta_{11}^{-1}\right)\left(\Sigma_{21} - \beta_{21}\beta_{11}^{-1}\Sigma_{11}\right)' \\
&\quad + \left(\beta_{21}\beta_{11}^{-1}\right)\left(\Sigma_{21} - \beta_{21}\beta_{11}^{-1}\Sigma_{11}\right)'\Gamma^{-1}\left(\Sigma_{21} - \beta_{21}\beta_{11}^{-1}\Sigma_{11}\right)\left(\beta_{21}\beta_{11}^{-1}\right)'
\end{aligned}$$

where $\Gamma = \beta_{21}\beta_{11}^{-1}\Sigma_{11}(\beta_{21}\beta_{11}^{-1})' - \left(\Sigma_{21}(\beta_{21}\beta_{11}^{-1})' + \beta_{21}\beta_{11}^{-1}\Sigma_{21}'\right) + \Sigma_{22}$. Therefore, since estimates for $\beta_{21}\beta_{11}^{-1}$ and $\Sigma_{uu'}$ are available, we also know $\beta_{11}\beta_{11}'$ and $\beta_{22}\beta_{22}'$. Imposing the assumption that $\gamma_Y = 0$, β_{22} is lower triangular and hence obtainable through the Cholesky decomposition of $\beta_{22}\beta_{22}'$. Next we can obtain β_{12} through

$$\beta_{12} = \left(\Sigma_{21} - \beta_{21}\beta_{11}^{-1}\Sigma_{11}\right)'\Gamma^{-1}\left(\Sigma_{21} - \beta_{21}\beta_{11}^{-1}\Sigma_{11}\right)(\beta_{21}\beta_{11}^{-1})'(\beta_{22}^{-1})' + \left(\Sigma_{21} - \beta_{21}\beta_{11}^{-1}\Sigma_{11}\right)'(\beta_{22}^{-1})'$$

and since $\beta_{12}\beta_{22}^{-1} = [\eta_G \; \eta_Y]$ we have also identified the vectors η_G and η_Y. Next, we have

$$\beta_{11}\Sigma_T^{-1} = \left(I_k - \begin{bmatrix} 0 & \eta_Y \end{bmatrix}\beta_{21}\beta_{11}^{-1}\right)^{-1}, \quad \beta_{21}\Sigma_T^{-1} = \beta_{21}\beta_{11}^{-1}\left(I_k - \begin{bmatrix} 0 & \eta_Y \end{bmatrix}\beta_{21}\beta_{11}^{-1}\right)^{-1}$$

which gives us the impact of orthogonalized tax shocks. Although it is not possible to identify Φ without further assumptions, the reliability matrix can be estimated by the sample equivalent of

$$\Lambda = \Sigma_{mm'}^{-1}\Sigma_{mu_1'}(\beta_{11}\beta_{11}')^{-1}\Sigma_{mu_1'}' .$$

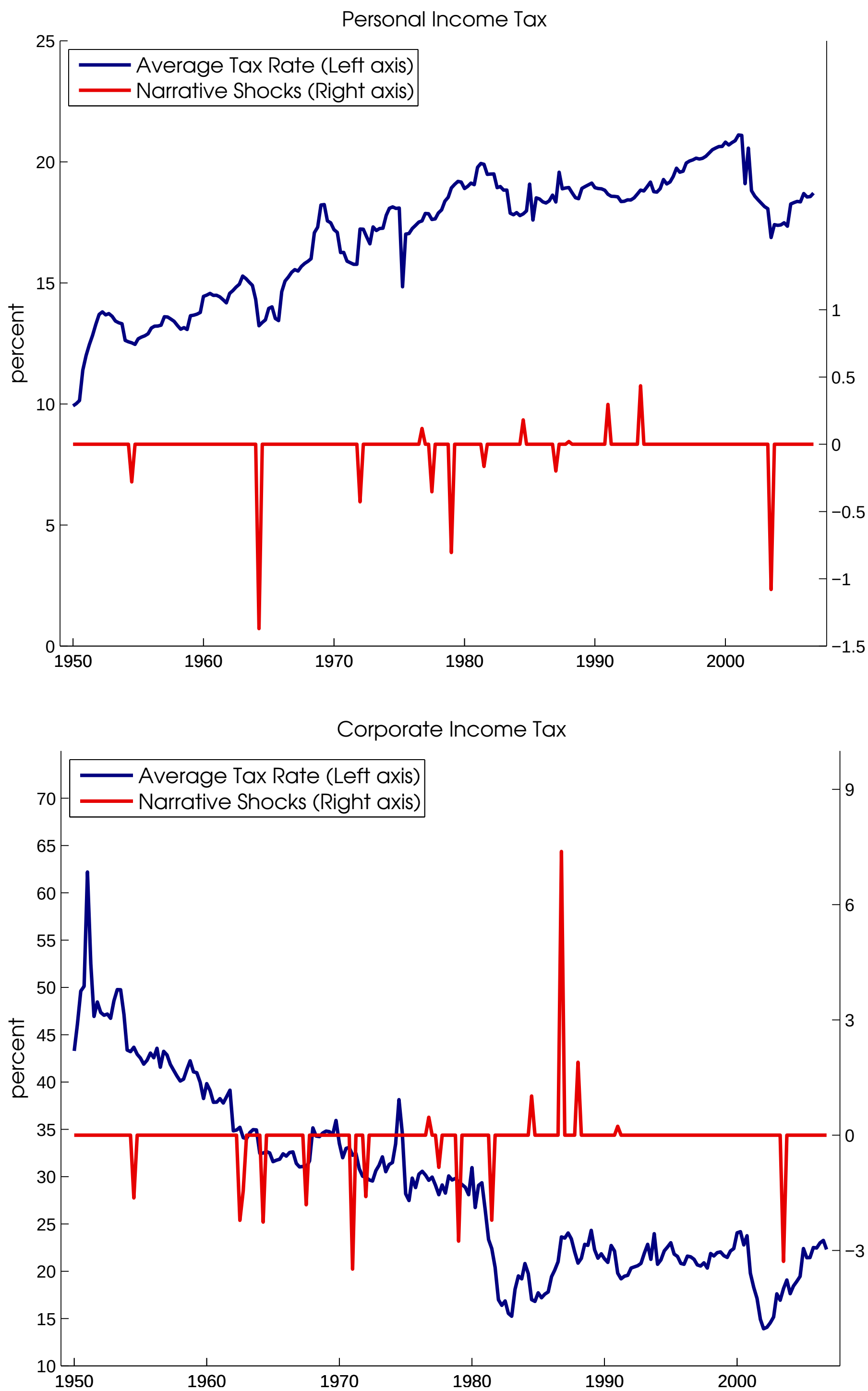

Figure 1 Average Tax Rates and Narrative Shock Measures for the US 1950Q1-2006Q4

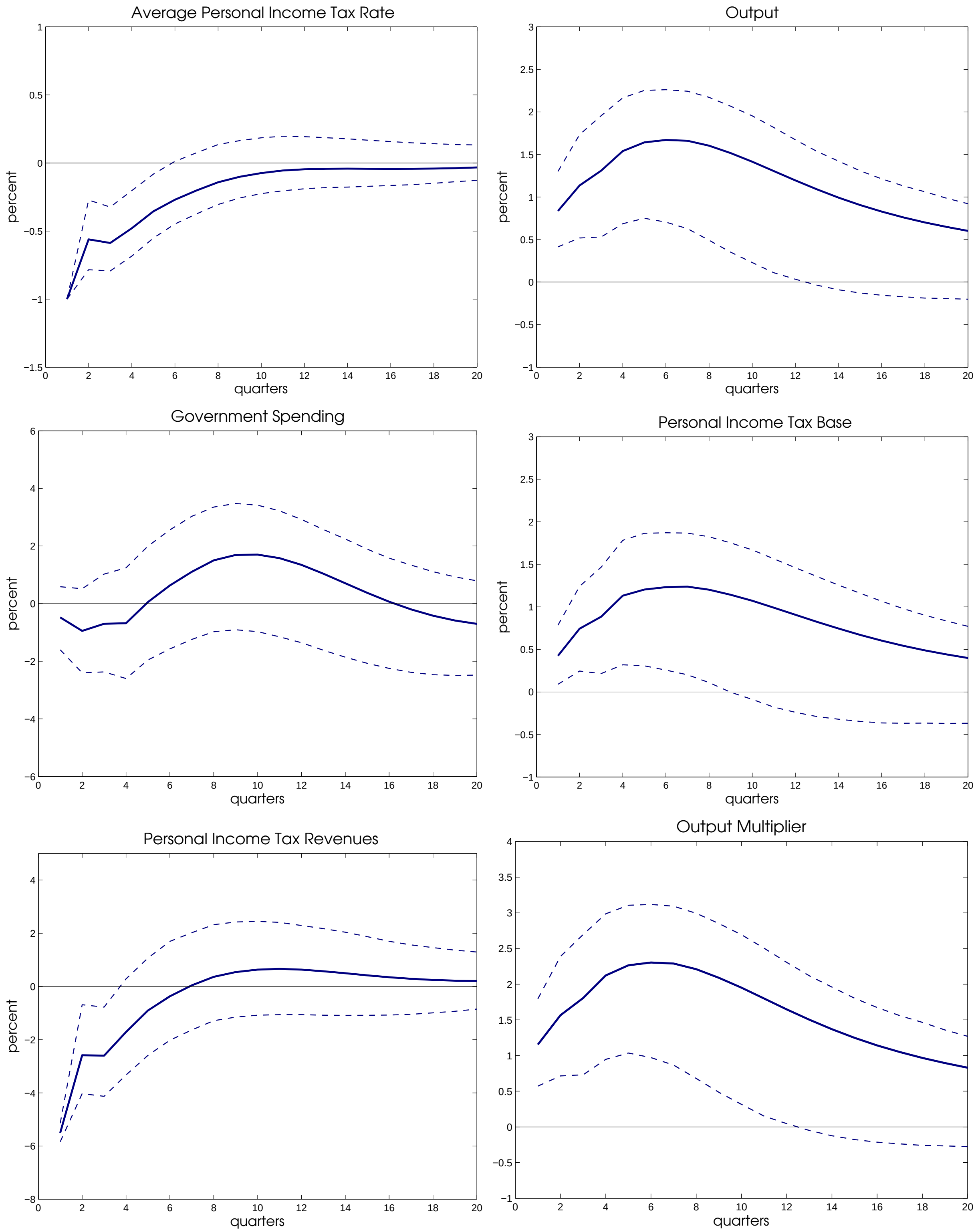

Figure 2 Benchmark Specification: Response to 1% Cut In **Average Personal Income Tax Rate**. Broken lines are 95% intervals.

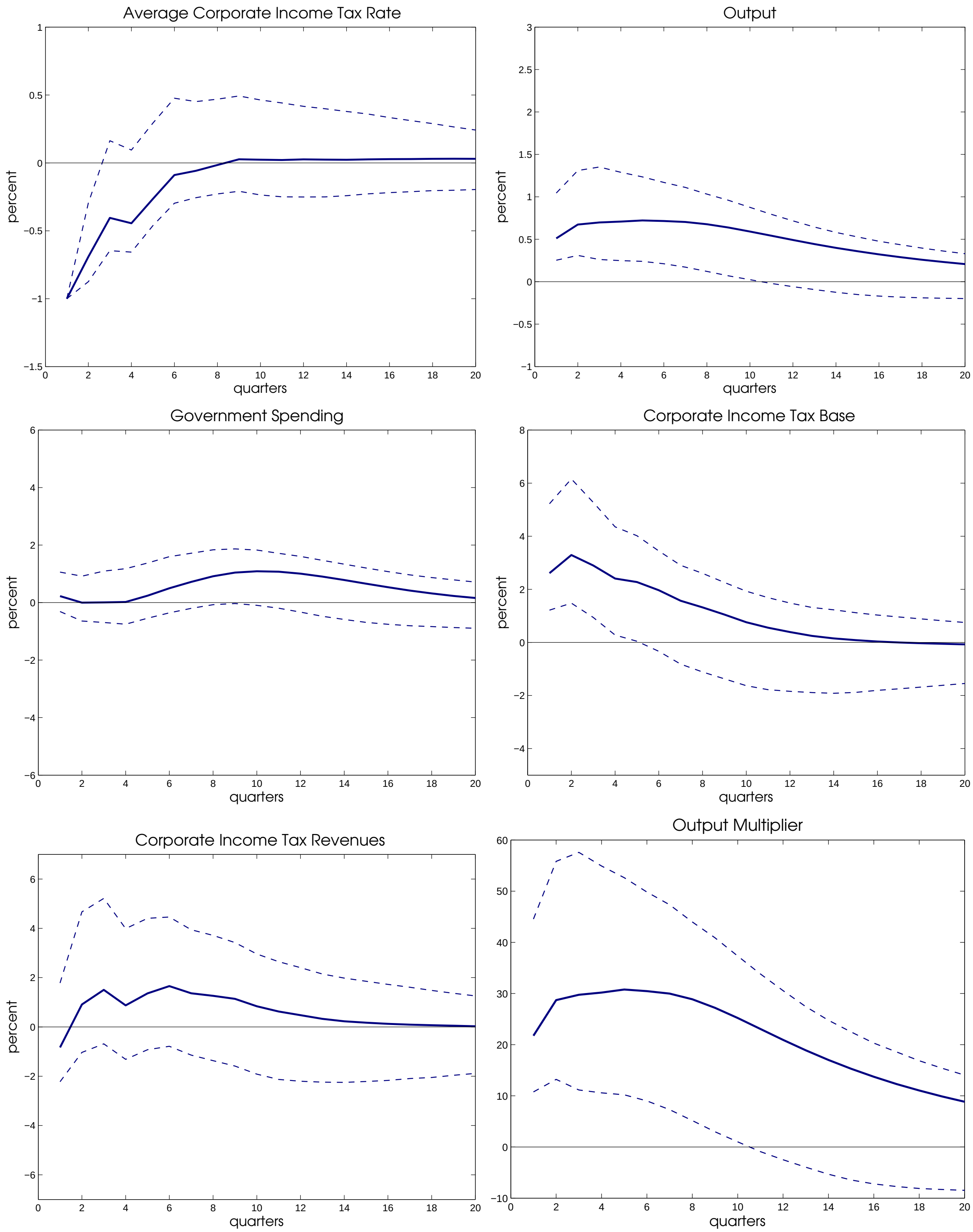

Figure 3 Benchmark Specification: Response to 1% Cut In **Average Corporate Income Tax Rate**. Broken lines are 95% intervals.

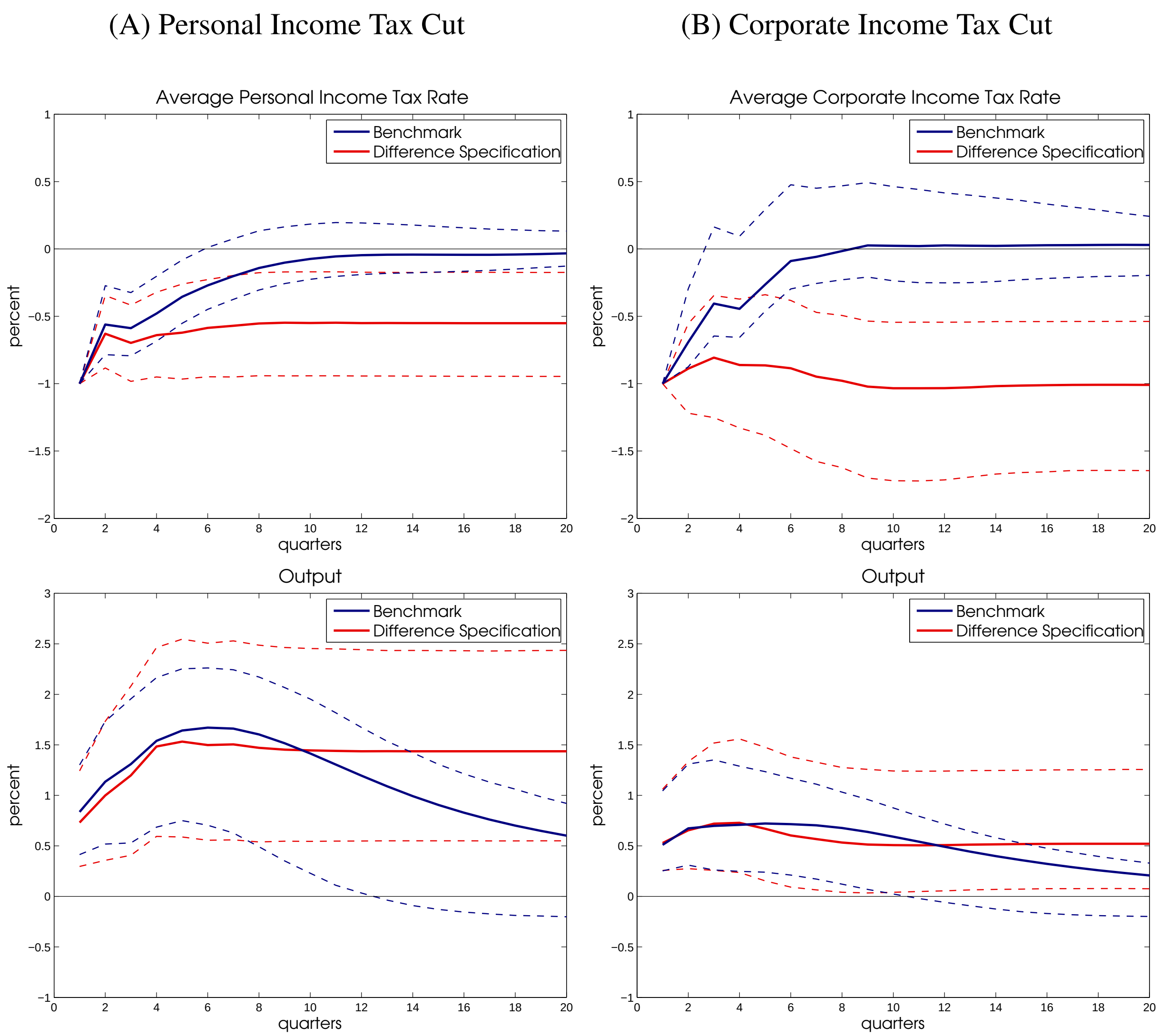

Figure 4 Comparing to a Difference Specification. Broken lines are 95% intervals.

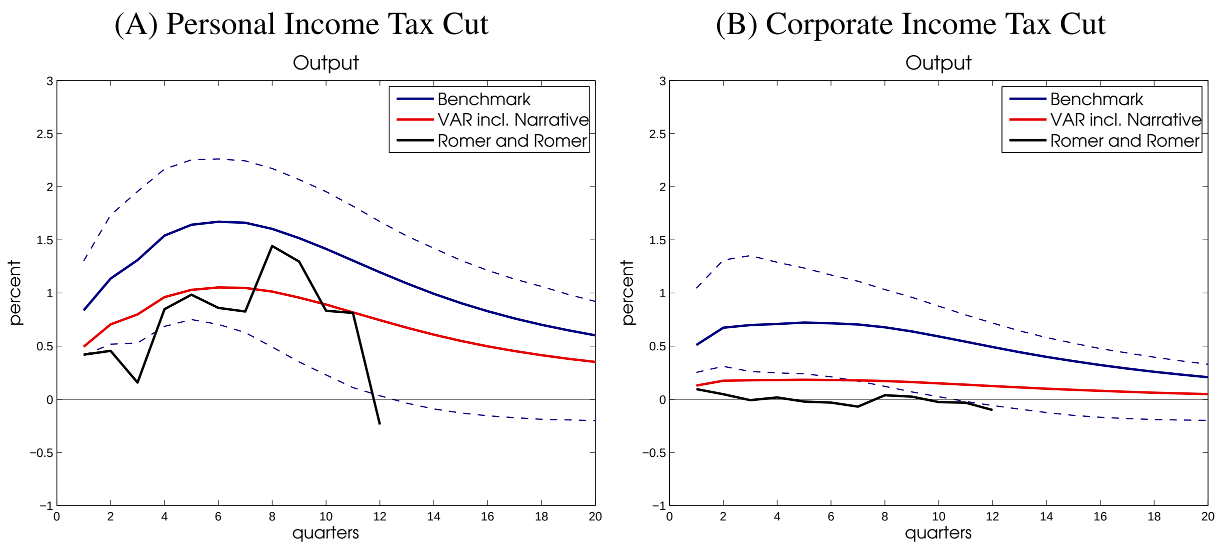

Figure 5 Comparing to **Alternative Specifications**

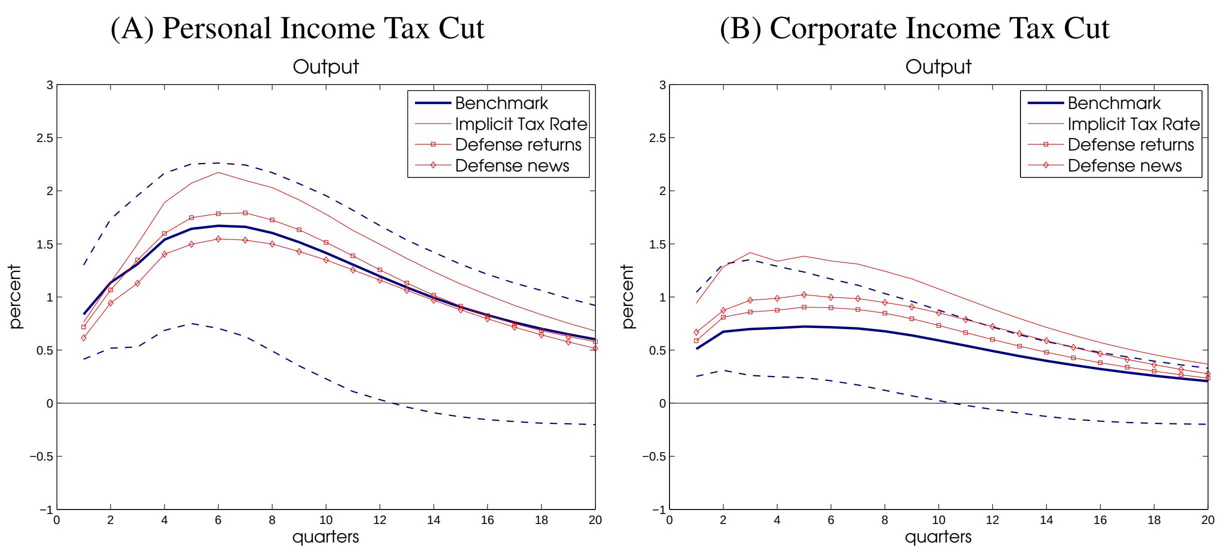

Figure 6 Including Fiscal Foresight Variables. Response to 1% Cut In Average Tax Rate. Broken lines are 95% intervals of the benchmark specification.

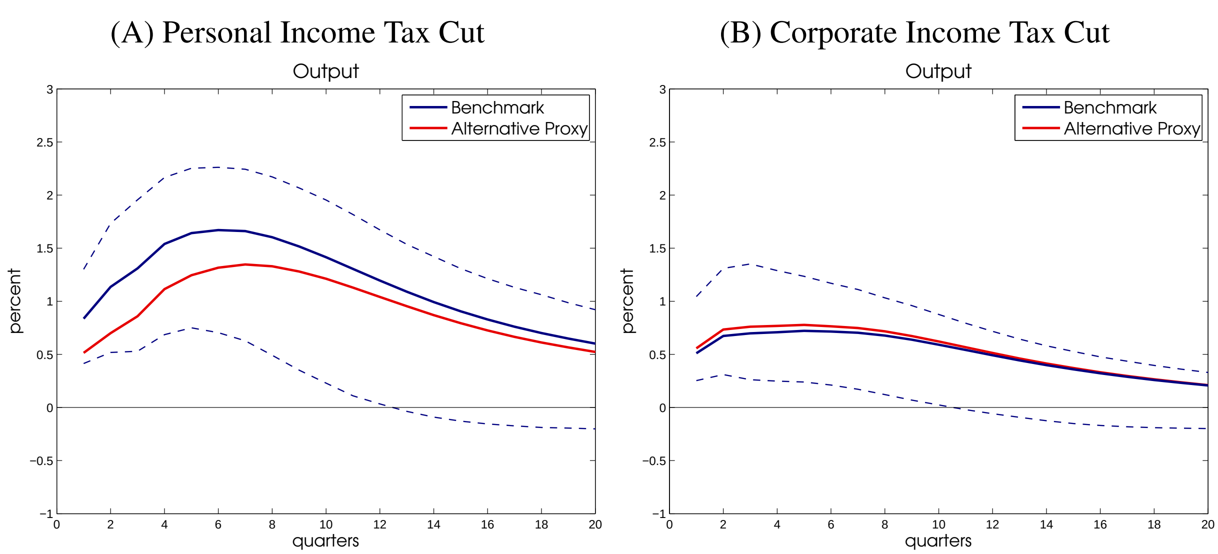

Figure 7 Regressing Narrative Shocks on the Implicit Tax Rate. Response to 1% Cut In Average Tax Rate. Broken lines are 95% intervals of the benchmark specification.

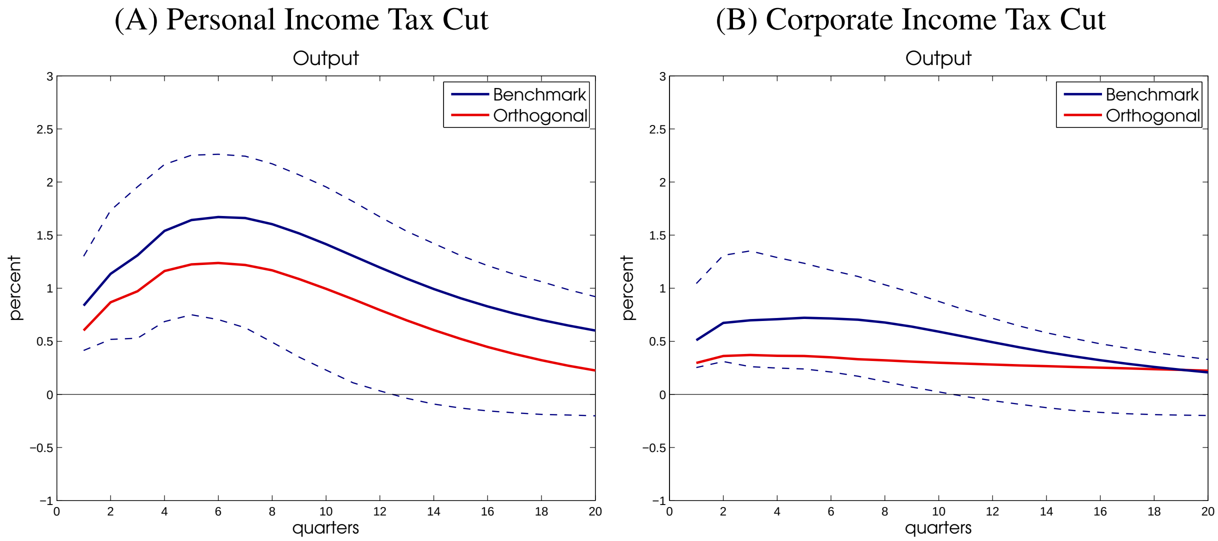

Figure 8 Response to **Orthogonalized 1% Cut In Average Tax Rates**. Broken lines are 95% intervals of the benchmark specification.

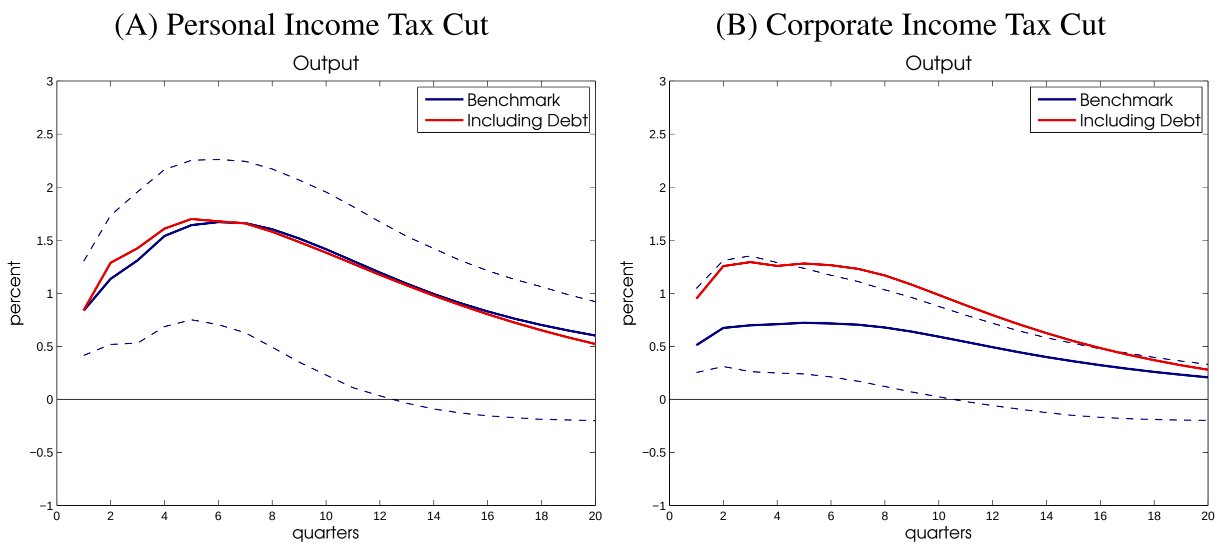

Figure 9 Response to **Controlling for Debt**. Broken lines are 95% intervals of the benchmark specification.

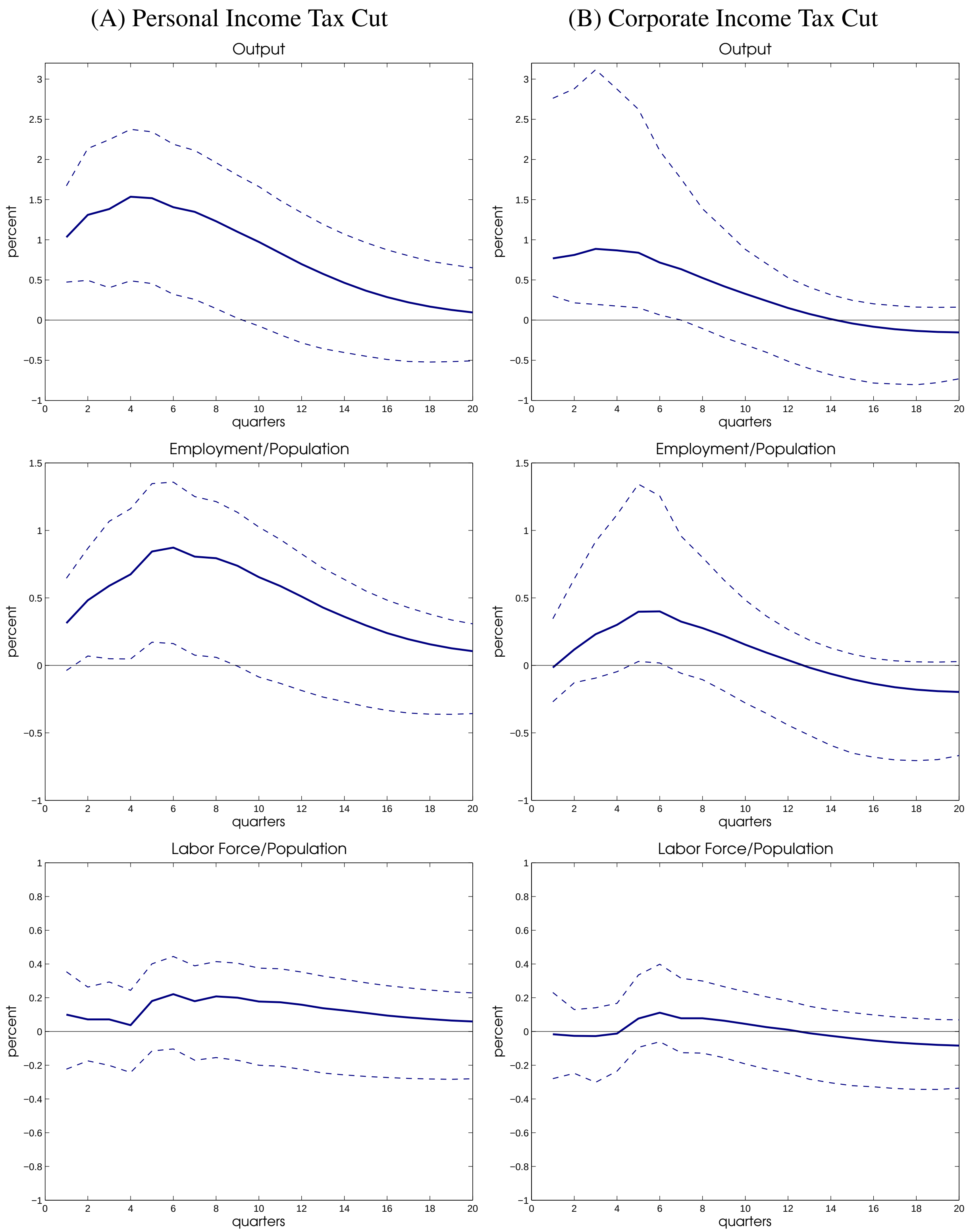

Figure 10 Labor Market. Response to 1% Cut In Average Tax Rates. Broken lines are 95% intervals.

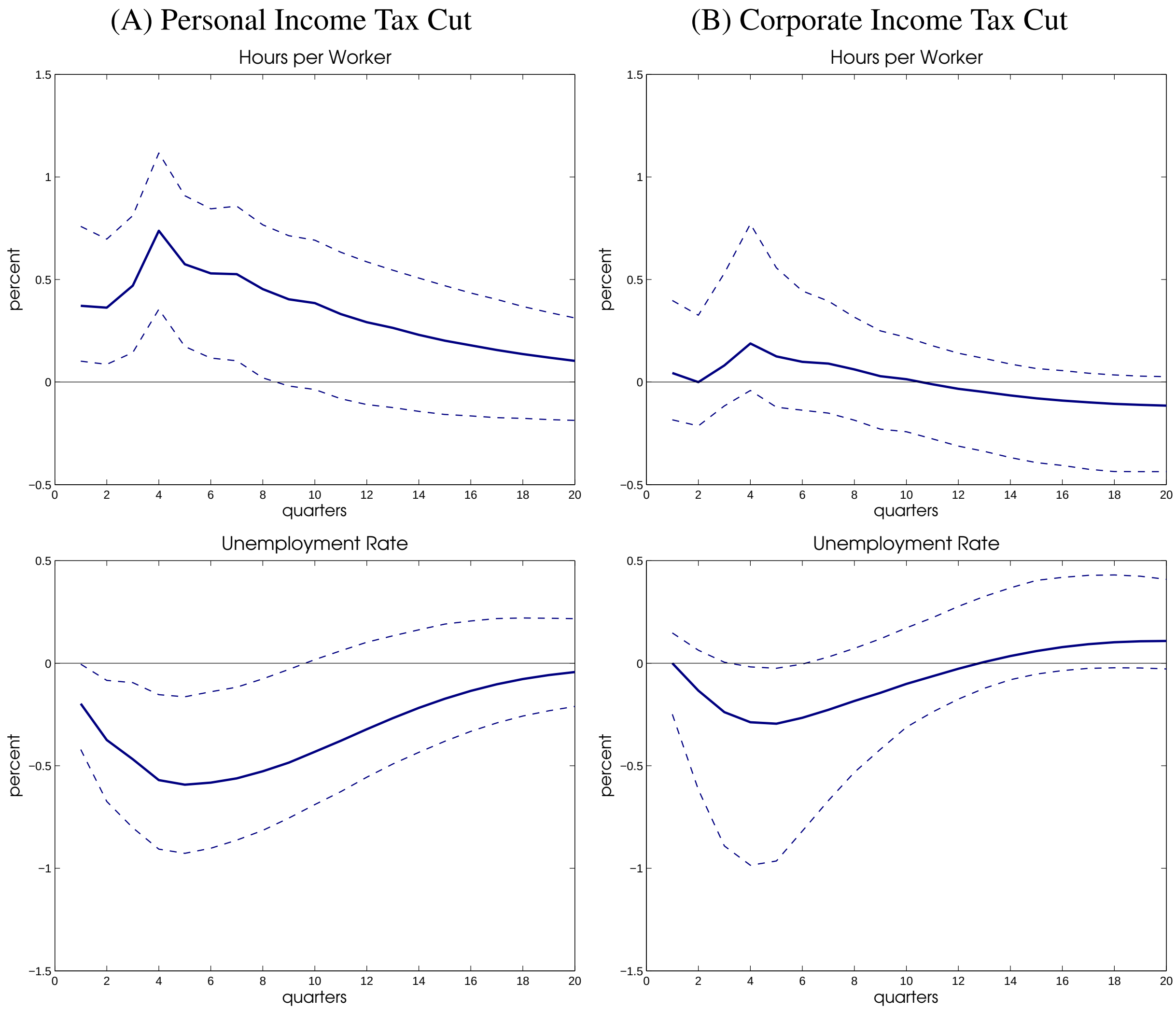

Figure 10 Labor Market (Continued). Response to 1% Cut In Average Tax Rates. Broken lines are 95% intervals.

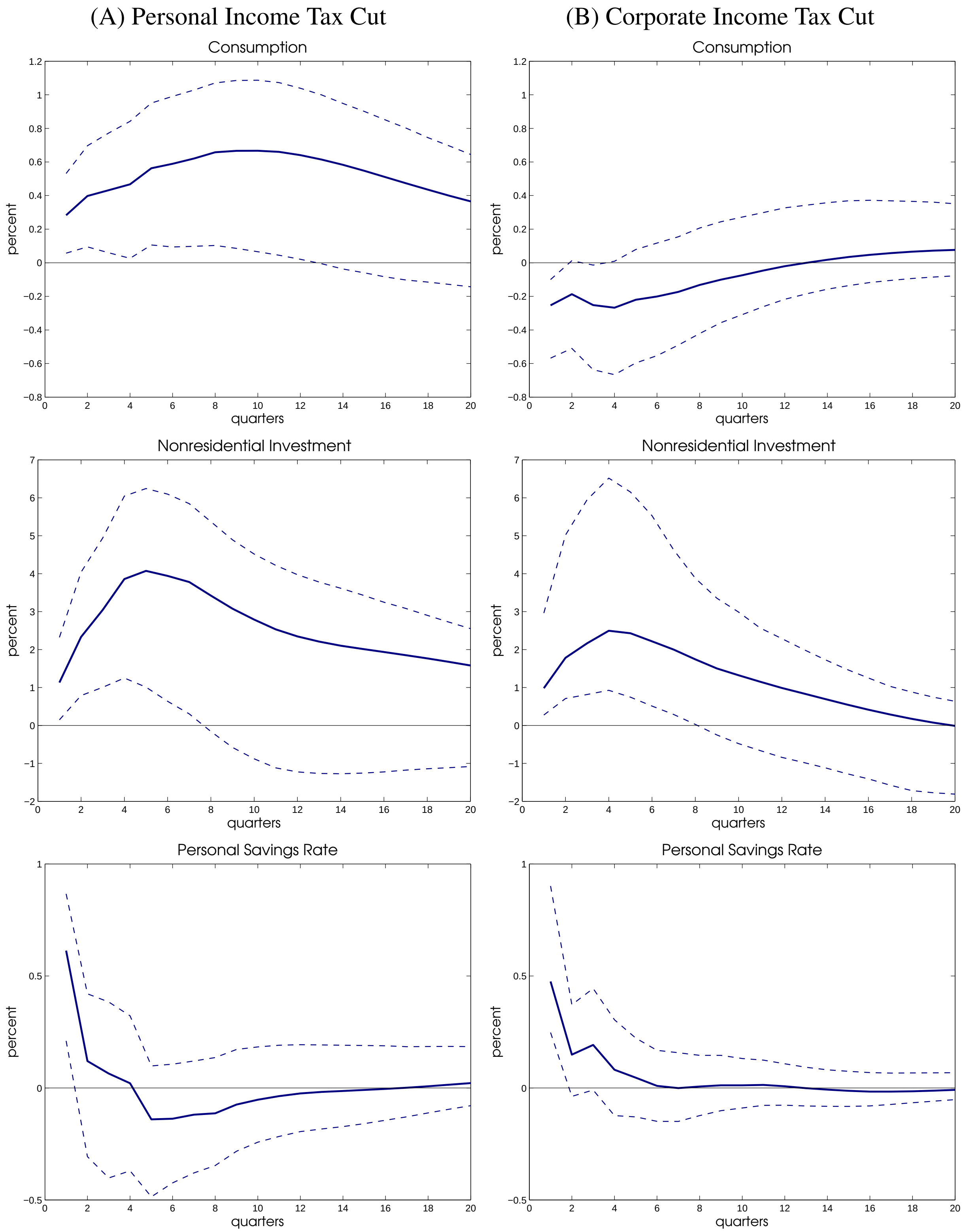

Figure 11 Response to 1% Cut In Average Tax Rates. Broken lines are 95% intervals.

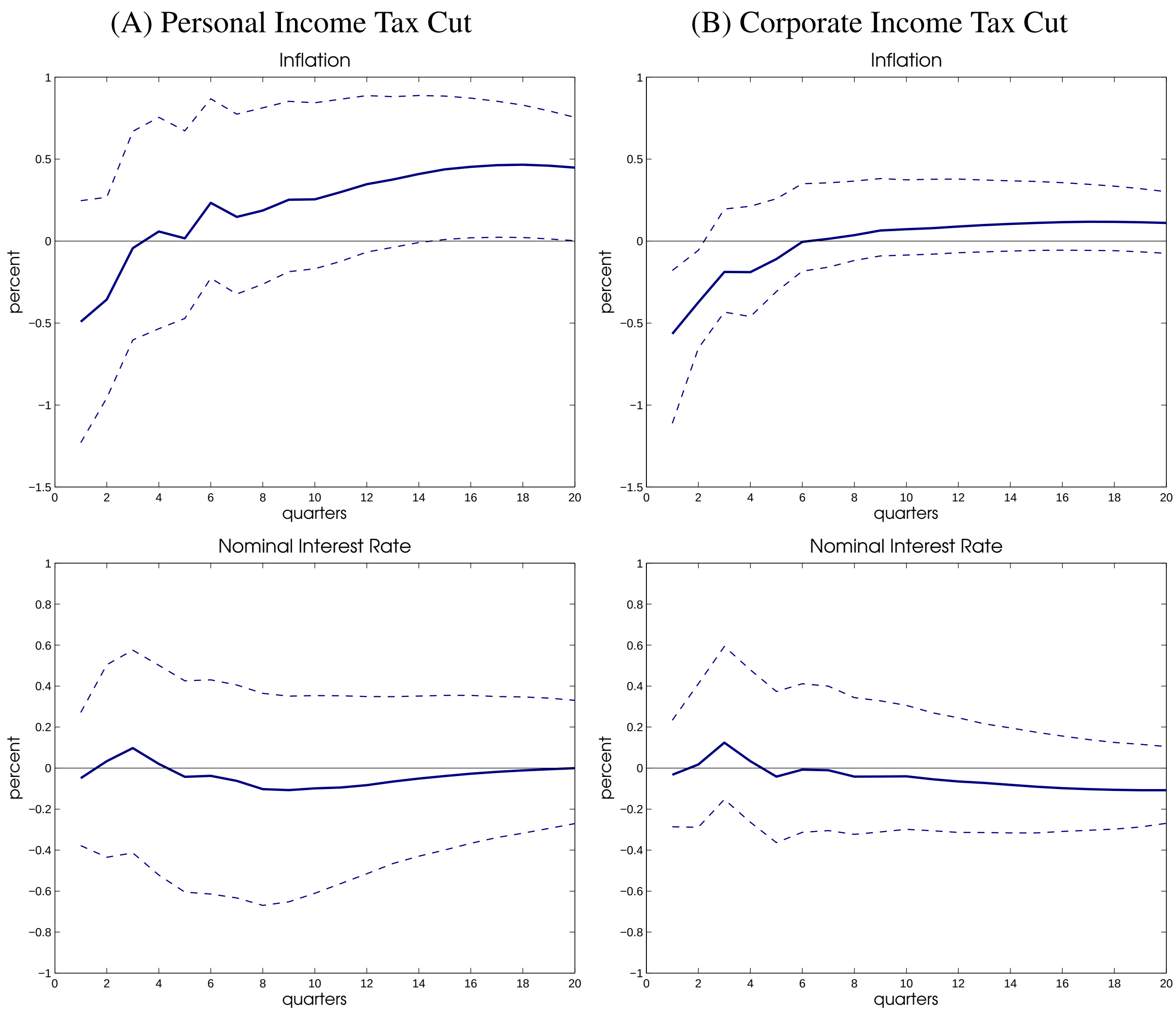

Figure 12 Response to 1% Cut In Average Tax Rates. Broken lines are 95% intervals.