The Impact of Tax Changes on the Macroeconomy: A New Approach Using Failed Tax Changes*

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Abstract

We propose a simple way to address an endogeneity problem in tax multiplier studies. The endogeneity arises because lawmakers tend to propose and legislate tax cuts in anticipation of a slowing economy, making it difficult to identify the causal impact of tax changes on aggregate output. Although all proposed tax changes are likely to be correlated with the output expectations of lawmakers, only the legislated tax changes directly impact the economy. Hence, proposed tax changes that ultimately fail to become law can serve as a proxy for the unobserved output expectations of lawmakers. Using this proxy method and novel data on failed tax proposals, we obtain a tax multiplier of around -0.46 to -2.06 for the United States from 1975 to 2017. Our approach can have a wide variety of applications to other fiscal multiplier studies.

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1 Introduction

The magnitude and extent to which tax changes impact the aggregate economy is a widely studied question in macroeconomics. Despite this, estimating the causal impact of tax changes on the aggregate output has remained a challenge due to endogeneity concerns in econometric analyses. Since lawmakers tend to propose and legislate a tax cut in anticipation of slower output growth, tax changes are positively correlated with the output growth expectations of lawmakers which are unobserved by the econometrician. If this endogeneity is not addressed, the econometric analysis will result in an upward bias in the estimated impact of tax changes on the economy.

In this paper, we propose addressing the endogeneity in tax multiplier studies by using a newly constructed time series of *failed* tax changes - tax changes considered by Congress that ultimately fail to become law - as a proxy for the unobserved output expectations of lawmakers. Our approach is motivated by the finding that legislative bills aimed at stabilizing output are often delayed or fail entirely due to political reasons (e.g., Chappell and Keech (1986), Alesina and Drazen (1991), Alesina and Rosenthal (1994), Poterba (1994), Fatás and Mihov (2003)). If a substantial fraction of stabilizing tax proposals fail to pass for political reasons, then even the time series of failed tax changes is likely to have a positive correlation with the output expectations that affect legislated taxes. Moreover, unlike legislated taxes, failed taxes by definition cannot affect output directly[1]. Hence, in a tax multiplier study that regresses future output growth on legislated tax changes, including the failed tax change variable helps absorb the effect of the unobserved output expectations without affecting the causal relationship between legislated tax changes and output.

To illustrate our approach, we collect data on both legislated and failed tax revenue changes in the United States from 1975 to 2017. Among the 420 tax bills which have revenue estimates from the Joint Committee on Taxation, 103 bills (25%) eventually

[1] There may be anticipatory effects where the proposed tax changes cause individuals to change their behaviour thereby indirectly affecting output, but since 75% of the failed tax changes remain in discussion in Congress for less than a quarter, we ignore the anticipatory effects for now.

get legislated and 317 bills (75%) fail to become law. From these, we obtain our quarterly measures of legislated and failed tax revenue changes.

Consistent with our assumption that failed tax changes reflect the output expectations of lawmakers, the failed tax changes indeed predict future GDP growth. This is evidenced by a positive coefficient when regressing real GDP growth on contemporaneous and lagged failed tax changes over 8 quarters. This regression mimics the conventional time-series tax multiplier regression and we find that a 1% increase in the failed tax change as a fraction of GDP is associated with a 1.3% increase in the GDP growth over the next 8 quarters. Since failed tax changes do not affect GDP directly, this large "failed tax multiplier" of around 1.3 reflects that more tax cuts (increases) are proposed in anticipation of a slower (faster) output growth. Furthermore, we find that failed tax changes contain information about future output growth orthogonal to other potential predictors of output growth. That is, failed tax changes positively predict future GDP growth after controlling for lagged GDP growth and various survey forecasts.

Moving onto our main empirical approach, we illustrate how the failed tax variable helps correct the tax multiplier. We find that a naive regression that does not address endogeneity implies a positive tax multiplier of around 0.7. This small but positive value suggests that the endogeneity of lawmakers legislating more tax cuts in anticipation of a slowing economy overwhelms the potential direct effect of tax cuts stimulating the economy. On the other hand, once we control for failed tax changes as a proxy for the anticipated output growth, the legislated tax multiplier falls to around -0.9, which we argue is more likely to capture the causal effect of legislated tax changes on output. We also find this tax revenue estimate to be reasonably robust. Regardless of alternative specifications and data constructions, we obtain a tax multiplier of around -0.46 to -2.06.

To summarize, our contribution is to propose a simple proxy approach to dealing with the endogeneity issue in fiscal multiplier studies and to illustrate the approach in the context of tax multipliers. Other types of fiscal policy also have historical data on both the legislated and failed changes, so one can apply the proxy variable method to obtain the correct fiscal multiplier in other settings.

Related literature Our paper belongs to the large literature proposing alternative ways to obtain the correct fiscal multiplier.[2] Although various approaches have been proposed, we do not view this literature as crowded given the importance of estimating the correct fiscal multiplier and the wide range of the estimates found in the literature.

The structural VAR approach identifies the tax multiplier by imposing additional structures on the evolution of the economy.[3] For example, Blanchard and Perotti (2002) use elasticities inferred from institutional information about tax and transfer systems and assume that discretionary fiscal policy takes longer than one quarter to respond to news about the economy. Mountford and Uhlig (2002) impose restrictions on the sign of impulse responses. However, the structural VAR approach can be sensitive to the structural assumptions (Caldara and Kamps, 2012) and to assumptions about the implementation lag in the policy variable (Martens and Ravn (2010) and Favero and Giavazzi (2012)). The simple fiscal VAR has also been extended to incorporate key country characteristics that fiscal shocks depend on, such as the level of development, exchange rate regime, openness to trade, and public indebtedness (Ilzetzki, Mendoza, and Végh (2010)) and debt dynamics analysis (Ilzetzki, 2011).

The narrative approach identifies the principal motivation for policy actions from presidential speeches and Congressional reports to distinguish between "exogenous" and "endogenous" actions. Using this approach, Romer and Romer (2010) and Cloyne (2013) obtain a large GDP tax multiplier of around -2.5 to -3 in the U.S. and the U.K., respectively, whereas Ramey (2009) and Perotti (2012) obtain much smaller multipliers. The narrative approach is a departure from the earlier studies which focused on correcting for the relationship between output and revenues and the behavior of government spending to obtain an unbiased estimate of the tax multiplier (Romer

[2]The literature is too large to list here in a satisfactory manner. Ramey (2011a) is a recent survey paper on the topic.

[3]Examples are Perotti (1999), Fatas and Mihov (2001), Blanchard and Perotti (2002), and Mountford and Uhlig (2002) among others.

and Romer, 2010). However, the narrative approach tends to be time-consuming and subjective.

Others combine the VAR and narrative approaches or suggest an entirely new approach. Martens and Ravn (2014) use narrative measures as proxies for structural shocks to total tax revenues in an SVAR. Ramey and Shapiro (1998) and Ramey (2011b) use defensive spendings due to war events to gauge the government spending multiplier. Barro and Redlick (2011) use marginal tax rates series to estimate a tax multiplier but instrument the variation using the Romer-Romer tax dataset and find a negative multiplier of -1.1. However, they find that the "tax revenue" multiplier is negligible due to the substitution effect. Some others use the cross-sectional variation in fiscal shocks to identify their effect on macroeconomic variables (e.g., Johnson, Parker, and Souleles (2006), Chodorow-Reich, Feiveson, Liscow, and Woolston (2012), Parker Souleles, Johnson, and McClelland (2013), and Chodorow-Reich (2018) among others).

Some papers focus on reconciling the differences in the SVAR and narrative measures with the premise that the difference arises from either the identification assumptions of the SVAR or from the assumed reduced-form transmission mechanisms. Charhour, Schmitt-Grohe, and Uribe (2012) however reject this hypothesis and suggest instead that the observed differences are due to either both models failing to identify the same tax shocks or due to small-sample uncertainty. Favero and Giavazzi (2012) aim to reconcile the difference between Romer and Romer (2010) and Blanchard and Perotti (1991) by including narrative shocks in a VAR model. They create an encompassing model where the Romer-Romer taxes appear as a limited information approach since while it directly identifies tax shocks, it omits other sources of information that are included in the VAR approach. Perotti (2011) counters this by claiming that the Favero and Giavazzi (2012) multiplier is biased towards zero since the discretionary component of tax will have different effects compared to the automatic response of tax revenues to macroeconomic variables. Leeper, Walker, and Yang (2008) on the other hand argue that even the most creative identification schemes in a fiscal VAR cannot extract economically meaningful shocks to taxes because of the existence of the non-invertible moving average component in the equilibrium time series that results in biased tax multipliers. Furthermore, even narrative approaches that aim to identify fiscal foresight ex-ante will only be successful depending on the degree to which forecasted revenue changes reflect exogenous changes in taxes and the relative volatility of the random components of tax decisions.

Our approach is appealing in multiple ways. Unlike the structural VAR approach, we do not rely heavily on the structural assumption regarding the evolution of the economy. The assumption we do impose is that all tax proposals - legislated or failed - carry some information about the lawmakers' expectations of future economic activities. We test the validity of this assumption. Unlike the narrative approach, our method has less room for subjectivity and can be implemented quickly. The weakness of our approach is the assumption that the failed actions are determined by similar variables that determine the legislated actions. However, one can address this issue by presenting evidence consistent with the assumption as we do, based on the GDP predictability evidence.

2 The framework

We use a simple econometric model to describe why a naive regression of the output growth on the legislated tax changes is biased and how using failed tax changes solves this issue.

Endogeneity of legislated tax changes. We begin by highlighting how the endogeneity of legislated tax changes leads to a bias in the tax multiplier estimation. Suppose that the data-generating process for output growth at time $t+1$ is

$$\Delta Y_{t+1} = \beta \Delta T_t + g_t + \epsilon^Y_{t+1}, \tag{1}$$

where ΔT_t measures the change in legislated tax revenue, g_t is the deviation in the economic agent's expectation of the output growth from the stationary level of growth, and ϵ_{t+1}^Y measures other shocks to the economy that are independent of everything else. Importantly, the legislated tax revenue change at time t follows the data generating process,

$$\Delta T_t = f(g_t) + \epsilon_t^T, \tag{2}$$

where ϵ_t^T is a measurement error that is independent of everything else. If lawmakers legislate tax cuts when anticipating a recession, then $\frac{df}{dg} > 0$. For simplicity, we suppose $f(g) = \gamma_1 g_t$, where $\gamma_1 > 0$.

The problem is that the econometrician does not observe g_t. Hence, a naive tax multiplier regression estimates the following model:

$$\Delta Y_{t+1} = b\Delta T_t + e_{t+1}^Y \tag{3}$$

This leads to a bias $b > \beta$ because $Cov(\Delta T_t, g_t) > 0$. Intuitively, if lawmakers anticipate a recession and legislate tax cuts, then a naive econometrician observes a low output growth after tax cuts and erroneously conclude that tax cuts reduce the future economic growth.

Failed tax changes as a proxy for g_t. Our approach is to use additional information contained in changes in failed tax revenues. Because failed tax revenue changes do not become law, they do not directly enter into the data generating process for the output growth. Instead, they load on g_t. Specifically, we assume that the failed tax revenue change at time t follows the following data generating process:

$$\Delta U_t = h(g_t) + \epsilon_t^U \tag{4}$$

where ϵ_t^U is a measurement error that is independent of everything else. If lawmakers propose tax cuts when anticipating a recession, then $\frac{dh}{dg} > 0$. For simplicity, we assume linearity $h(g) = \gamma_2 g_t$.

We model $f(g_t)$ and $h(g_t)$ separately because a legislated tax bill may have more components than a failed tax bill. For example, lawmakers may add "pork barrel" components - components that help their constituents for political reasons- into a tax bill as the bill goes through the legislative process (e.g., passing the House, resolving the difference between the House and the Senate). In this case, since a legislated tax bill goes through more steps in the legislative process than a failed tax bill, we would expect $f(g) > h(g)$ for the same g.

Solving for g_t, we have

$$g_t = \frac{\Delta U_t - \epsilon_t^U}{\gamma_2} \tag{5}$$

Plugging g_t into the output growth data generating process, we have

$$\begin{aligned} \Delta Y_{t+1} &= \beta \Delta T_t + \tfrac{\Delta U_t - \epsilon_t^U}{\gamma_2} + \epsilon_{t+1}^Y \\ &= \beta \Delta T_t + \tfrac{\Delta U_t}{\gamma_2} + (\epsilon_{t+1}^Y - \tfrac{\epsilon_t^U}{\gamma_2}) \end{aligned} \tag{6}$$

Because ϵ_{t+1}^Y and ϵ_t^U are independent of everything else, we can correctly estimate the tax multiplier β now.

3 Data

Legislated and failed tax revenue changes. We collect data on revenue estimates for tax proposals in the U.S. during the period 1975-2017. We begin with the universe of revenue estimates available on the Joint Committee on Taxation (JCT) website since the JCT provides revenue estimates for all tax proposals (bills) considered by Congress since July 1974. To obtain revenue estimates for tax proposals, we apply two criteria. First, we require that the title of the revenue estimate document contains the bill identifier information (e.g., House bill ”H.R. 4”). This discards revenue estimates that are not specific to any specific tax bill (e.g., an overview of tax expenditures in a given year). Second, we require that the document contains a table with the revenue estimates to minimize errors in the digitization process. We have widened our dataset and incorporated a few revenue estimates that were available only in text form into our analysis[4]. This leaves us with 816 JCT revenue estimates on 517 distinct tax bills. Some tax bills have multiple JCT estimates since Congress may revise the proposal as the bill progresses through the legislative rounds. Excluding 127 bills with zero revenue estimates and taking the latest revenue estimate, we obtain 420 proposed tax changes.[5] 1 summarizes these 420 proposed tax changes by the last congressional action on the bill. We find that the number of bills that do not pass either of the chambers of the Congress, those that pass at least one chamber but fails to pass the other chamber, and those that successfully become law make up 48%, 27%, and 25% of all proposed tax changes.

By matching tax revenue estimates with legislative records on the U.S. Congress website, we obtain the dates when the bill was last considered in Congress. For legislated bills, this is the day when the bill was legislated, and for failed tax bills, this is the date when the bill was last discussed in Congress. However, we need to

[4]However, we concede that there may be other revenue estimates that may have been missed in the digitization process.

[5]If there are multiple JCT revenue estimate documents for the latest date associated with the bill, we assume that they are estimates for different provisions of the bill and take a sum over those estimates. We show in our robustness section that taking an average leads to similar results.

find the actual and supposed implementation date of the tax change for the legislated and failed bills. Mertens and Ravn (2008) report that the median lag between the legislation date and the implementation date is 6 quarters. Assuming a similar lag, we add 4 and 6 quarters to the last record date to obtain the implementation quarter for legislated and failed tax changes, respectively. That is, we assume a "legislation lag" (the time it would have taken for a failed tax to pass) of 2 quarters and an "implementation lag" (the time it would have taken for a legislated tax to be implemented) of 4 quarters. However, we consider alternative lags and find that our numbers do not change significantly. Following Romer and Romer (2010), we focus on the effect of the initial change in the tax policy. We do this by constructing our series based on JCT's estimate of tax revenue change to the first year of implementation[6], assuming that the tax revenue changes in the following years merely reflect the continuation of the same policy change[7].

By aggregating all the tax changes in a given quarter and dividing the resulting sum by nominal GDP, we obtain the time series of legislated and failed tax changes. These are plotted in the 2 series plot. Both legislated and failed tax change proposals tend to be sparse and smaller in magnitude between 1985 and 2000, whereas they are more frequent before 1985 and after 2001 although they are predominantly negative in the second half of the sample. Although this is consistent with having more tax cut proposals around economic downturns, it could also lead the two tax change series to act as a (-1 times) dummy variable on the time period 1985-2000[8]. This could result in a bias if the GDP growth rate has slowed down over time or was low following the financial crisis of 2008-2009. We address this concern in two ways. First, we include a dummy variable for the time period 1985-2000, which we interact with the control variable, thereby using only the variation within each half of the sample. Second, we

[6] We include a robustness check which includes the tax revenue estimate for the second year

[7] We allocate this tax revenue change in the first year to the supposed quarter of implementation although we do a robustness check with involves spreading the tax revenue change across different quarters.

[8] One reason for the sparse tax changes in the 1990s is the rule we apply to obtain JCT tax revenue estimates. Some of the large tax proposals in the 1990s did not have accompanying JCT tax revenue estimates in a table format, making it difficult to digitize the information.

also repeat our regressions using the pre-crisis sample of 1975-2007 and report it as one of our robustness specifications.

Macroeconomic variables. The following data are from the National Income and Product Accounts: Nominal GDP from Table 1.1.5, Real GDP from Table 1.1.3 (Index : 2012=100), Price Indices for GDP from Table 1.1.4 and Government spending from Table 3.1. Government Current Receipts and Expenditures. All of these are provided in billions of dollars and are seasonally adjusted at annual rates. The data on the three-year bond rate are from the Board of Governors of the Federal Reserve System, series H15/H15/RIFLGFCY03_N.M. All the above data were last revised on October 26, 2018.

GDP forecast data. In the next section, we evaluate the ability of failed tax changes to predict output growth beyond survey forecasts. The forecast variables we look at are from the Survey of Professional Forecasters (SPF), the Livingstone Survey (Livingstone), the Survey of Consumers (SC) and Fed Staff's Greenbooks (Greenbooks). Apart from the Survey of Consumers which uses the level value of real GDP, all the other datasets provide the growth rates of the real GDP forecasts. Data is available from 1975 to 2018 for all except the Survey of Consumers and the Fed Staff's Greenbooks. While the Survey of Consumers misses data from 1975 and is only available from 1978, the Fed Staff's Greenbooks provide data only until 2012. Another key distinction is that while most of the forecast variables are available at a quarterly rate, the Livingstone forecasts are available only at a semi-annual rate[9].

[9] The forecast data from the Survey of Consumers is available at a monthly rate and was transformed into a quarterly forecast.

4 Failed tax changes as a proxy for unobserved growth expectations

In this section, we provide evidence consistent with our conjecture that failed tax changes are positively correlated with the unobserved output expectations. If failed tax changes load positively on output expectations that are on average correct and if failed tax changes cannot affect future output directly, we would expect the multiplier on failed tax changes to be positive.

To check this, we regress real GDP growth on the contemporaneous and lagged failed tax changes (up to 8 quarters) to infer the coefficients on those tax changes over the time period 1975-2017:

$$\Delta Y_t = \alpha + \sum_{i=0}^{8} \beta_i \Delta U_{t-i} + \epsilon_t \tag{7}$$

where ΔY_t denotes real GDP growth in quarter t and ΔU_{t-i} denotes the failed tax change in quarter $t-i$. In other words, the cumulative GDP response $\sum_{i=0}^{8} \beta_i$ represents the "failed tax multiplier" coming purely from failed tax changes being positively correlated with the lawmakers' expectations of future GDP growth and not from any causal effect on GDP.

Figure 3 shows that the failed tax multiplier is positive. That is, output growth tends to be faster (slower) following failed proposals to increase (cut) taxes. Moreover, the magnitude of the miltiplier is large. A one-percentage increase in the failed tax change proposals as a fraction of GDP is associated with around 1.3 percentage point (pp) increase in the GDP growth rate over the next 8 quarters following the supposed implementation. This suggests that failed tax increases (cuts) tend to be proposed in anticipation of higher (lower) output growth rate, consistent with the premise of our proxy approach.

Table1 shows that the finding is robust to controlling for other potential predictors of output growth and to alternative assumptions about the number of quarters it would have taken for the failed tax proposals to be implemented. This suggests that the output expectations of lawmakers contained in failed tax change proposals are not fully captured by other time-series variables. It is also reassuring that the results are not sensitive to the assumption about the number of quarters between the last congressional action on failed tax bills and the supposed date of implementation.

5 The legislated tax multiplier

Using failed tax changes as a proxy for the growth expectations of lawmakers, we show how the proxy variable addresses the endogeneity problem in tax multiplier studies. In all analyses, the baseline specification is 4 quarters' implementation lag (the lag between legislation and implementation) and 2 quarters' legislation lag (the time it would have taken for a failed tax bill to pass).

5.1 The naive multiplier

We begin by estimating the naive multiplier. This is useful in highlighting the presence of endogeneity in a tax multiplier regression and serves as a benchmark for the next section when we address the endogeneity problem with a proxy variable. To do this, we regress real GDP growth on the contemporaneous and lagged legislated tax changes (up to 8 quarters) as well as lagged GDP growth to infer the coefficients on the tax changes:

$$\Delta Y_t = \alpha + \sum_{i=0}^{8} \beta_i \Delta T_{t-i} + \sum_{j=1}^{8} \eta_j \Delta Y_{t-j} + \epsilon_t \tag{8}$$

where ΔT_{t-i} denotes the failed tax change in quarter $t-i$.

Figure 4 shows that the naive tax multiplier is slightly positive (0.66) rather than negative for the baseline sample of 1975-2017, contrary to the notion that a tax increase (cut) has a contractionary (expansionary) effect on GDP.[10] This points to the

[10] In the specification without lagged GDP, the "tax multiplier" would simply be the sum of the betas $\sum_{i=1}^{8} \beta_i$. For the specification with lagged GDP, the tax multiplier is the dynamic tax multiplier that accounts for the feedback effect between ΔT and ΔY as in Romer and Romer (2010). The multiplier represents the effect of the legislated tax change on GDP 12 quarters or 3 years after legislation

endogeneity problem that motivates our study. Even if legislated tax changes have a negative causal effect on future output, they are likely to be positively correlated with the component of future output observed by lawmakers, leading to an upward bias. The conclusion is similar when we include a structural break dummy for the time period 1985-2000. In this case, we get a small negative naive multiplier of -0.03.

5.2 The proxy variable approach

We offer a simple remedy to the endogeneity problem illustrated above. Since failed tax changes are also likely to be positively correlated with the output expectations of lawmakers, one can include failed tax changes as a proxy variable for those unobserved expectations:

$$\Delta Y_t = \alpha + \sum_{i=0}^{8} \beta_i \Delta T_{t-i} + \sum_{j=0}^{8} \gamma_j \Delta U_{t-j} + \sum_{k=1}^{8} \eta_k \Delta Y_{t-k} + \epsilon_t \tag{9}$$

As explained in 2, this corrects for the bias arising from the omitted variable if failed tax changes have a non-zero loading on the unobserved growth expectations, even if the loading is different from that of legislated tax changes.

Figure 5 shows that the tax multiplier on legislated tax changes is now positive and significant at -0.88 as opposed to 0.66 in the naive approach. This is more in line with the notion that the causal effect of tax changes on GDP is negative. This suggests that the positive correlation between legislated tax changes and output expectations is now absorbed by failed tax changes, leaving the coefficients on legislated tax changes to only reflect the causal effect on future output. In terms of the magnitude, cutting tax revenues by 1% in the legislated tax change as a fraction of GDP is associated with around 0.9pp increase in the GDP growth rate over the next 8 quarters. The conclusion is again similar in the alternate specification where we include a structural break dummy variable which is also interacted with the failed tax changes. In this case, the implied tax multiplier is -2.06 instead of -0.03 from the naive approach.

Table 2 summarizes our result by comparing regression models (8) and (9) under different assumptions about the number of lags between legislation and implementation (baseline: 4 quarters) as well as the number of lags between last congressional action on failed bills and supposed legislation (baseline: 2 quarters). This translates to a 4 quarter lag for legislated tax changes and a 6 quarter lag for failed tax changes. Across all specifications, the proxy variable approach implies a tax multiplier of around -2.06 to -0.54, whereas the naive approach leads to a tax multiplier of around -0.45 to 0.87[11].

6 Robustness

We study how the resulting tax multiplier estimate changes with a battery of robustness checks. The results are summarized as 3.

First, tax increases may be legislated to offset more government spending. If government spending positively affects the future GDP growth, then not controlling for government spending may result in a bias. To address this concern, We add the change in total government expenditures divided by nominal GDP as a control. The results are almost identical to the baseline specification.

How does dropping lagged GDP affect our result? Although we believe it prudent to include lagged GDP as additional potential determinants of future GDP, it is useful to know how the result changes. In this case, the tax multiplier goes from 0.77 to -0.80 when we switch from the naive approach to our proxy variable approach.

We also repeat our analysis using the pre-crisis sample of 1975-2007 instead of the full sample period of 1975-2017. In this case, we obtain a similar conclusion. Not including the proxy variable implies a naive tax multiplier of 0.52, whereas controlling for the proxy implies a multiplier of -1.90.

As mentioned in 3, the dominant bill may have multiple JCT revenue estimates on the last congressional action date. In this case, we took a sum over all revenue estimates since for most bills, the different revenue estimates are different components

[11] Since many failed tax changes are small, the overall lag on the failed tax changes may be smaller than the lag on the legislated tax changes.

of the bill. Under the alternative approach of taking an average, the tax multiplier estimate becomes smaller and insignificant at -0.69.

Our baseline approach assumes that the tax revenue change happens instantly in the implementation quarter. Alternatively, we could split the tax revenue evenly across the four quarters starting with the implementation quarter and normalize the resulting tax revenue by the GDP in the corresponding quarter. The tax multiplier in this case goes from a positive 0.66 estimated under the naive approach to a negative estimate of -0.54.

The baseline specification only considers the year 1 revenue estimates for each tax bill. An alternate specification of including year 2 estimates which are to be implemented exactly 4 quarters after the first tax changes were implemented results in negative estimates under both approaches. Upon controlling for the expectations of GDP under the proxy approach, the multiplier becomes significant and more negative at -1.42.

People may follow the permanent income hypothesis and respond to not just the immediate change in tax but also respond to news about the future changes. To address this concern, we add the net present value of tax changes, similar to Romer and Romer (2010). In this case, the tax multiplier estimate becomes more negative at -1.12.

Another concern could be that political parties in power may bias the results as discussed in the introduction. So, we add a dummy variable for the political party in power and test the regression models. Similarly, since there is usually a flurry of new legislation at the beginning of a new Presidency, in another specification, we add a dummy variable for the first quarter of every year post an election. In both cases, the multipliers are identical to the baseline.

7 Conclusion

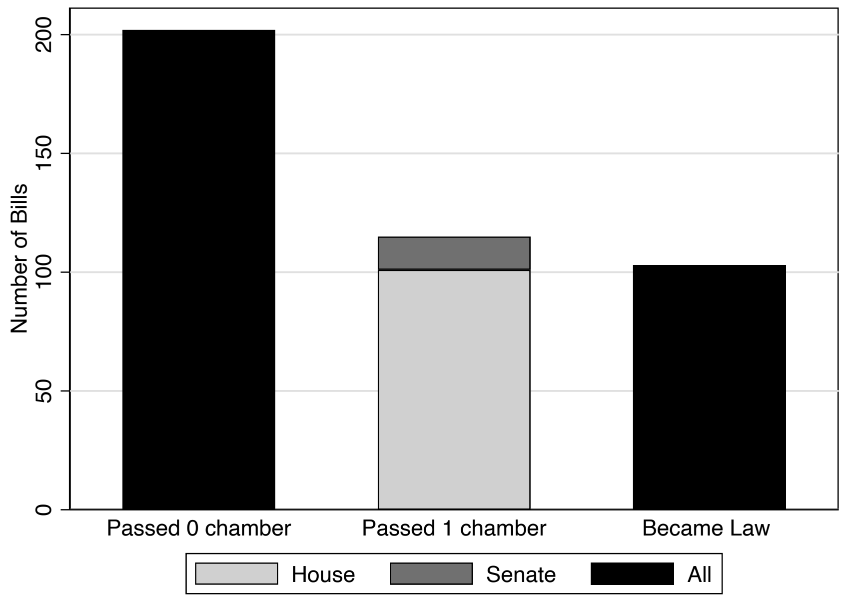
In this paper, we propose correcting for the bias in tax multiplier studies using a failed tax change series as an additional control. Using this approach, we obtain a tax multiplier of around -0.46 to -2.06 in the recent U.S. sample of 1975-2017.

We believe our approach can have fruitful applications. Since it uses readily available information about failed bills, one can apply our method to other fiscal multiplier estimations. It would also be possible to collect state-level legislation information to study local fiscal multipliers. These extensions are left to future studies.

8 Appendix

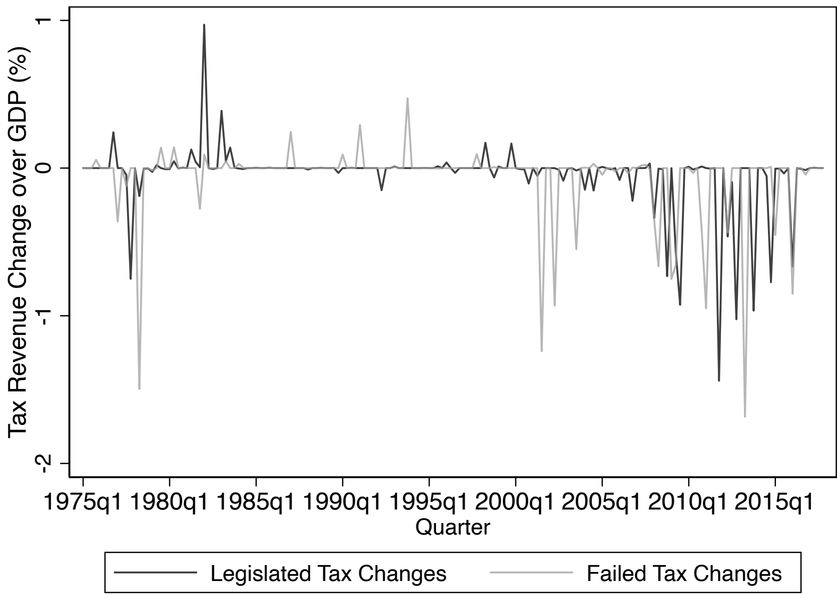
8.1 Figures and Tables

Figure 1: Distribution of Proposed Tax Bills by Last Legislative Action



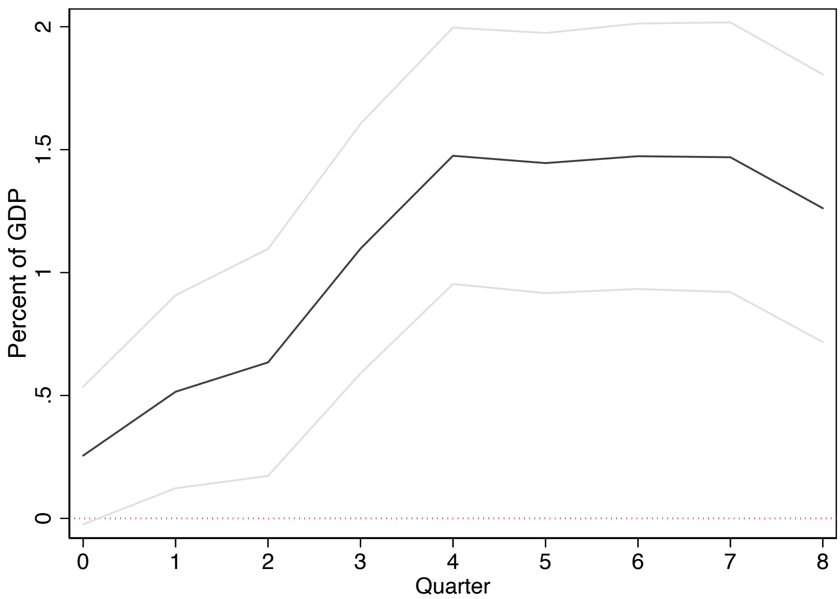
Notes: The figure reports the number of tax bills in our legislated and failed tax revenue change data by three mutually exclusive categories: passed 0 chamber before failing; passed at least 1 chamber before failing; and became law.

Figure 2: Legislated and Failed Tax Changes



Notes: The figure plots the quarterly legislated and failed tax revenue changes over 1975-2017. Each series is normalized by the GDP. The correlation between the two series is 0.11 with a p-value of 0.14.

Figure 3: The Failed Tax Multiplier: Estimated Change in GDP Associated with a Failed Tax Increase of 1 Percent of GDP



Notes: Gray lines denote the one standard deviation confidence band. They are computed by taking 10,000 draws of the coefficient vector from a multivariate normal distribution with mean and variance-covariance matrix equal to the point estimates and variance-covariance matrix of the regression coefficients.

Figure 4: The Naive Tax Multiplier on Legislated Tax Changes: Estimated Change in GDP Associated with an Legislated Tax Increase of 1 Percent of GDP

Figure 4a. Baseline specification

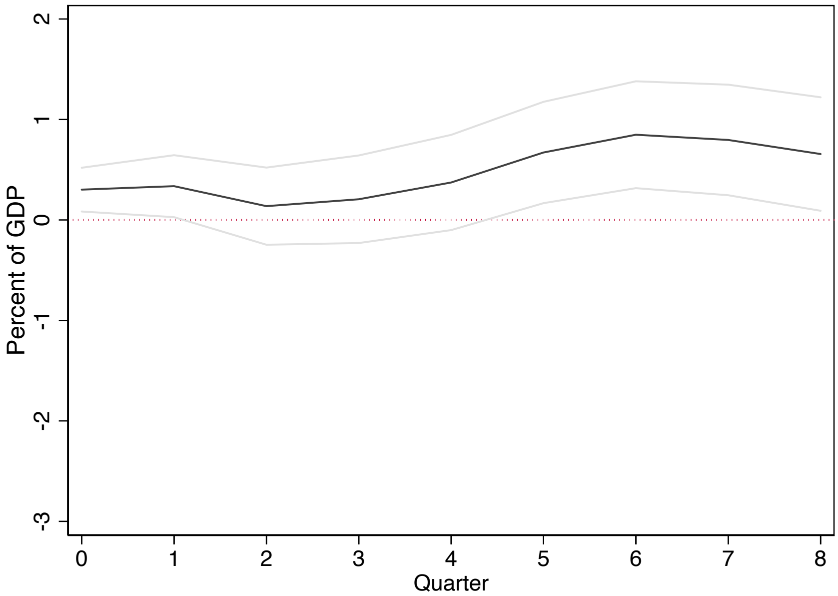
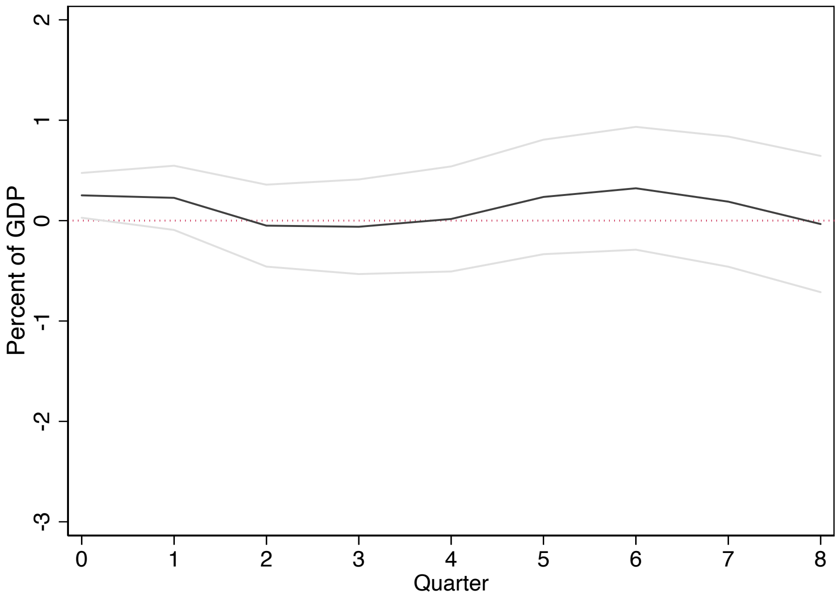


Figure 4b. With a dummy variable for the 1985-2000 sample



Notes: Gray lines denote the one standard deviation confidence band. They are computed by taking 10,000 draws of the coefficient vector from a multivariate normal distribution with mean and variance-covariance matrix equal to the point estimates and variance-covariance matrix of the regression coefficients.

Figure 5: Tax Multiplier Based on the Proxy Approach: Estimated Change in GDP in Response to a Tax Increase of 1 Percent of GDP

Figure 5a. Baseline specification

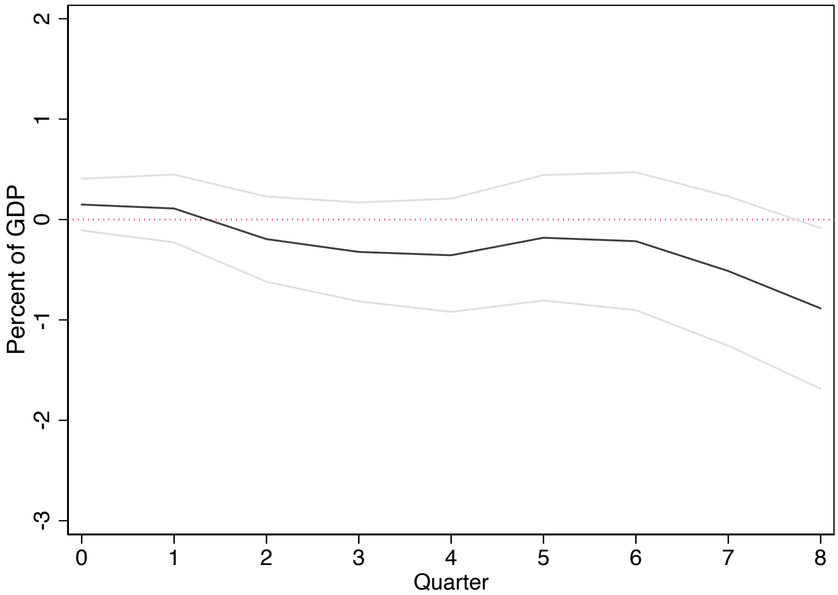
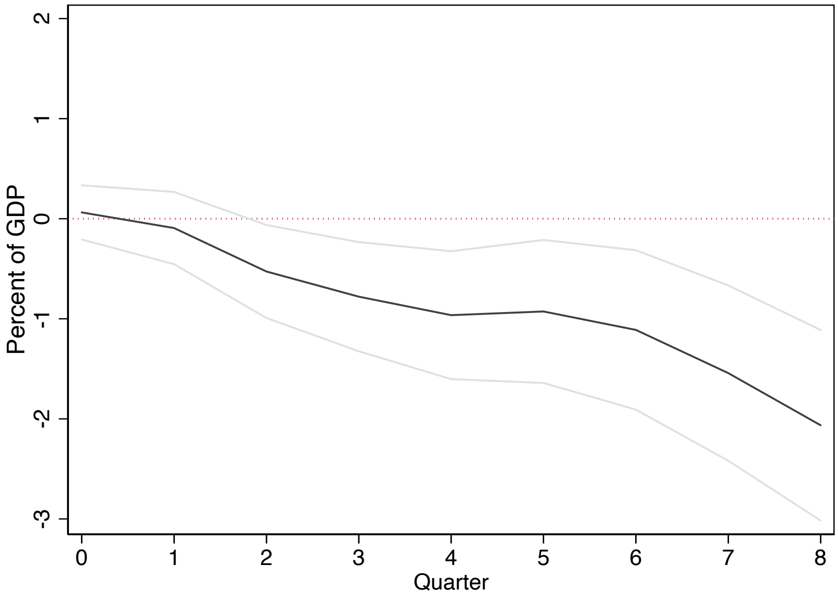


Figure 5b. With a dummy variable for the 1985-2000 sample



Notes: Gray lines denote the one standard deviation confidence band. They are computed by taking 10,000 draws of the coefficient vector from a multivariate normal distribution with mean and variance-covariance matrix equal to the point estimates and variance-covariance matrix of the regression coefficients.

Table 1: Predicting GDP Growth Using Failed Tax Changes

Lag			Controlling for other predictors				
(quarters)	Baseline	Dummy	Lagged GDP	SPF	SC	Greenbooks	Livingstone
2	1.49***	1.07*	1.28**	1.15***	0.42	0.64*	1.27**
	(0.54)	(0.57)	(0.56)	(0.41)	(0.52)	(0.46)	(0.54)
4	1.42***	1.29**	1.50***	1.00**	0.45	0.86*	1.17**
	(0.55)	(0.58)	(0.56)	(0.41)	(0.52)	(0.46)	(0.55)
6	1.26**	1.14*	1.38**	0.94**	0.49	0.88*	1.26**
	(0.54)	(0.58)	(0.56)	(0.41)	(0.52)	(0.49)	(0.53)
8	1.32**	0.92*	1.14**	0.96**	0.59*	0.78*	0.93*
	(0.56)	(0.59)	(0.57)	(0.40)	(0.52)	(0.54)	(0.56)

Notes: The Lag corresponds to the legislation plus implementation lag measured in quarters. The standard errors are reported in parenthesis. They are computed by taking 10,000 draws of the coefficient vector from a multivariate normal distribution with mean and variance-covariance matrix equal to the point estimates and variance-covariance matrix of the regression coefficients. The standard errors reported correspond to the cumulative (contemporaneous plus 8 lags) tax multiplier. Greenbooks data is available between the time period 1975-2012 while the Livingstone data is available between the time period 1975-2017 but the data is semi-annual. ICS data is only available for the time period 1978-2017 and is comparable to the baseline which is quarterly data from 1975-2017. *- significant at 32%; ** - significant at 5%; *** - significant at 1%.

Table 2: Tax Multiplier on Legislated Tax Changes

Lag on legislated tax changes:	4				6			
Lag on failed tax changes:	2	4	6	8	2	4	6	8
Naive tax multiplier	—— 0.66* ——				—— 0.55 ——			
	(0.56)				(0.56)			
Tax multiplier (corrected using the proxy approach)	-0.94*	-1.06*	-0.88*	-0.46	-0.90*	-0.95*	-0.75	-0.62
	(0.81)	(0.82)	(0.80)	(0.73)	(0.77)	(0.81)	(0.80)	(0.80)

Notes: The lag on legislated and failed tax changes is measured in quarters. The lag on legislated tax changes is the duration of a tax from the time it is legislated to the time it is implemented. The lag on the failed tax change includes the above defined implementation lag as well as the legislation lag which is the duration of an failed tax between its last known congress date and its expected enactment date. The standard errors are reported in parenthesis. They are computed by taking 10,000 draws of the coefficient vector from a multivariate normal distribution with mean and variance-covariance matrix equal to the point estimates and variance-covariance matrix of the regression coefficients. The standard errors reported correspond to the cumulative (contemporaneous plus 8 lags) tax multiplier. * - significant at 32%.

Table 3: Robustness Checks

	Naive Tax Multiplier	Corrected Tax Multiplier (Proxy Approach)
Baseline specification	0.66	-0.88*
	(0.56)	(0.80)
Structural break dummy	-0.03	-2.06*
	(0.68)	(0.95)
Government spending	0.63*	-0.92*
	(0.58)	(0.81)
Without lagged GDP	0.77*	-0.80*
	(0.57)	(0.82)
Pre-crisis sample	0.52	-1.90*
	(0.81)	(0.86)
Averaging JCT estimates	0.87*	-0.69
	(0.62)	(0.90)
ΔT spread over 1 year	0.66*	-0.54
	(0.61)	(0.92)
Including year 2 estimates	-0.45	-1.42*
	(0.76)	(1.31)
Using net present value	-0.14	-1.12*
	(0.64)	(0.88)

Notes: The standard errors are reported in parenthesis. They are computed by taking 10,000 draws of the coefficient vector from a multivariate normal distribution with mean and variance-covariance matrix equal to the point estimates and variance-covariance matrix of the regression coefficients. The standard errors reported correspond to the cumulative (contemporaneous plus 8 lags) tax multiplier. Time period of the regression is 1975-2017. The legislated and failed tax changes are expected to be enacted 4 and 6 quarters after their last congress discussion date. * - significant at 32%.

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