

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 unlegislated tax proposals, we obtain a tax multiplier of -1.1 in our baseline specification 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

To what extent tax changes impact the aggregate economy is a central question in macroeconomics. Despite this, estimating the causal impact of tax changes on the aggregate output remains a challenge due to endogeneity. Since lawmakers tend to propose and legislate a tax cut in anticipation of a slower output growth, tax changes are positively correlated with the output growth expectations of lawmakers which are unobserved by the econometrician. When unaddressed, this leads to 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 using time series of *unlegislated* tax changes—tax changes considered by the Congress but 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 unlegislated tax changes is likely to have a positive correlation with the output expectations that affect legislated taxes. Moreover, unlike legislated taxes, unlegislated taxes by definition cannot affect output directly. Hence, in a tax multiplier study that regresses future output growth on legislated tax changes, including the unlegislated 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 unlegislated tax revenue changes in the United States from 1975 to 2017. Among the 268 tax bills with revenue estimates from the Joint Committee on Taxation, 74 bills (28%) eventually get legislated and 194 bills (72%) fail to become law. From these we obtain our quarterly measures of legislated and unlegislated tax revenue changes.

Consistent with our assumption that unlegislated tax changes reflect the output expectations of lawmakers, they predict future GDP growth with a positive coefficient. When regressing real GDP growth on contemporaneous and lagged unlegislated tax changes over 12 quarters to mimic the conventional time-series tax multiplier regression, we find that a 1% increase in the unlegislated tax change as a fraction of GDP is associated with around 1.5% increase in the GDP growth over the next 12 quarters. Since unlegislated tax changes do not affect GDP directly, this large “unlegislated tax multiplier” of around 1.5 reflects that more tax cuts (increases) are proposed in anticipation of a slower (faster) output growth. Furthermore, we find that unlegislated tax changes contain information about future output growth orthogonal to other potential predictors of output growth. Unlegislated 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 unlegislated tax variable helps correct the tax multiplier. We find that a naive regression which does not address endogeneity implies a positive tax multiplier of around 0.1. 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 unlegislated tax changes as a proxy for the anticipated output growth, the legislated tax multiplier falls to around -1.1 , 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 -0.8 to -1.6 .

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 unlegislated 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.¹ 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.² 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) imposes 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 and Romer, 2010). However, the narrative approach tends to be time-consuming and subjective.

¹The literature is too large to list here in a satisfactory manner. Ramey (2011a) is a recent survey paper on the topic.

²Examples are Perotti (1999), Fatas and Mihov (2001), Blanchard and Perotti (2002), and Mountford and Uhlig (2002) among others.

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 is included in the VAR approach. Perotti (2011) counters this by claiming that Favero and Giavazzi 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 on the evolution of the economy. The assumption we do impose is that all tax proposals—legislated or unlegislated—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 unlegislated 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 unlegislated 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

$$\Delta Y_{t+1} = \beta \Delta T_t + g_t + \epsilon_{t+1}^Y, \quad (1)$$

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,

$$\Delta T_t = f(g_t) + \epsilon_t^T, \quad (2)$$

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:

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

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.

Unlegislated tax changes as a proxy for g_t Our approach is to use additional information contained in changes in unlegislated tax revenues. Because unlegislated 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 unlegislated tax revenue change at time t follows the following data generating process:

$$\Delta U_t = h(g_t) + \epsilon_t^U \quad (4)$$

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 compo-

nents than an unlegislated 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 legislation 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 legislation process than an unlegislated tax bill, we would expect $f(g) > h(g)$ for the same g .

Solving for g_t , we have

$$g_t = \frac{\Delta U_t - \epsilon_t^U}{\gamma_2} \quad (5)$$

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

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

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 unlegislated tax revenue changes We collect data on revenue estimates for tax proposals in the U.S. over 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 the 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., overview of tax

expenditures in a given year). Second, we require that the document contains a table with revenue estimates to minimize errors in the digitization process.³ This leaves us with 514 JCT revenue estimates on 369 distinct tax bills. Some tax bills have multiple JCT estimates since the Congress may revise the proposal as the bill progresses through the legislative rounds.

Using the latest tax revenue estimates for all 369 distinct bills may result in double counting since the Congress often introduces different bills based on similar ideas. For example, Democrats and Republicans may propose two different versions of tax changes based on the same idea of offsetting an anticipated recession. To address this issue, we define a “peer group” for tax bills. First, we associate each tax bill with all “associated” JCT documents mentioning the bill at least once in the document. Then, we define a peer group as all tax bills with at least one common associated JCT document. In each peer group, we pick one bill that has the higher number of associated JCT documents and call it the “dominant” tax bill. If there is a tie in the number of associated JCT documents within a peer group, we break the tie randomly.

From this, we identify 309 dominant bills. Excluding 41 bills with zero revenue estimates and taking the latest revenue estimate, we obtain 268 proposed tax changes.⁴ [Figure 1](#) summarizes these 268 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 chamber 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 31%, 41%, and 28% of all proposed tax changes.

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

³We are revisiting this rule to incorporate a larger number of revenue estimates into our analysis.

⁴If 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.

date of the tax change for the legislated and unlegislated 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 unlegislated tax changes, respectively. That is, we assume a “legislation lag” (the time it would have taken for an unlegislated tax to pass) of 2 quarters and an “implementation lag” (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, assuming that the tax revenue changes in the following years merely reflect the continuation of the same policy change.⁵

Aggregating all tax changes in a given quarter and dividing the resulting sum by nominal GDP, we obtain the time series of legislated and unlegislated tax changes. These are plotted in [Figure 2](#). Both legislated and unlegislated tax change proposals tend to be sparse and smaller in magnitude in the early half of the sample, whereas they are more frequent and 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 second half of the sample.⁶ This could result in a bias if 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 second half of the sample of 1997-2017, thereby using only the variation within each half of the sample. Second, we also repeat our regressions using the pre-crisis sample of 1975-2007 and report it as one of our robustness specifications.

⁵We put this tax revenue change in the first year to the supposed quarter of implementation rather than spreading it out across different quarters. Our robustness check shows that spreading the change over four quarters generates similar results.

⁶One reason for the sparse tax changes in the 1980s is the rule we apply to obtain JCT tax revenue estimates. Some of the large tax proposals in the 1980s did not have accompanying JCT tax revenue estimates in a table format, making it difficult to digitize the information. We are currently addressing this problem by manually going through JCT tax revenue documents at the time again.

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 unlegislated 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.⁷

4 Unlegislated tax changes as a proxy for unobserved growth expectations

In this section, we provide evidence consistent with our conjecture that unlegislated tax changes are positively correlated with the unobserved output expectations. If unlegislated tax changes

⁷The forecast data from the Survey of Consumers is available at a monthly rate and was transformed into a quarterly forecast.

load positively on output expectations that are on average correct but cannot affect future output directly, we would expect the multiplier on unlegislated tax changes to be positive.

To check this, we regress real GDP growth on the contemporaneous and lagged unlegislated tax changes (up to 12 quarters) to infer the coefficients on those tax changes over 1975-2017:

$$\Delta Y_t = \alpha + \sum_{i=0}^{12} \beta_i \Delta U_{t-i} + \epsilon_t \quad (7)$$

where ΔY_t denotes real GDP growth in quarter t and ΔU_{t-i} denotes the unlegislated tax change in quarter $t-i$. In other words, the cumulative GDP response $\sum_{i=0}^{12} \beta_i$ represents the “unlegislated tax multiplier” coming purely from unlegislated 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 unlegislated tax multiplier is positive. That is, output growth tends to be faster (slower) following unlegislated proposals to increase (cut) taxes. The magnitude is large. A one-percentage increase in the unlegislated tax change proposals as a fraction of GDP is associated with around 1.5 percentage point (pp) increase in the GDP growth rate over the next 12 quarters following the supposed implementation. This suggests that unlegislated tax increases (cuts) tend to be proposed in anticipation of higher (lower) output growth rate, consistent with the premise of our proxy approach.

[Table 1](#) 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 unlegislated tax proposals to be implemented. This suggests that the output expectations of lawmakers contained in unlegislated 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 unlegislated tax bills and the supposed date of implementation.

5 The legislated tax multiplier

Using unlegislated 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 analysis, 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 an unlegislated 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 subsection, 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 12 quarters) as well as lagged GDP growth to infer the coefficients on the tax changes:

$$\Delta Y_t = \alpha + \sum_{i=0}^{12} \beta_i \Delta T_{t-i} + \sum_{j=1}^{12} \eta_j \Delta Y_{t-j} + \epsilon_t \quad (8)$$

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

Figure 4 shows that the naive tax multiplier is slightly positive (0.15) 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.⁸ This points to the 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 dummy variable for the second

⁸In the specification without lagged GDP, the "tax multiplier" would simply be the sum of the betas $\sum_{i=1}^{12} \beta_i$. 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).

half of the sample (1997-2017) to use only the variations within each half of the sample, in which case we get a small negative naive multiplier of -0.58 .

5.2 The proxy variable approach

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

$$\Delta Y_t = \alpha + \sum_{i=0}^{12} \beta_i \Delta T_{t-i} + \sum_{j=0}^{12} \gamma_j \Delta U_{t-j} + \sum_{k=1}^{12} \eta_k \Delta Y_{t-k} + \epsilon_t \quad (9)$$

As explained in [Section 2](#), this corrects for the bias arising from the omitted variable if unlegislated tax changes have a nonzero 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 -1.10 as opposed to 0.15 in the naive approach, 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 are now absorbed by unlegislated 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 1.1pp increase in the GDP growth rate over the next 12 quarters. The conclusion is again similar in the specification with a dummy variable for the second half of the sample. In this case, the implied tax multiplier is -1.67 instead of -0.58 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). Across all specifications, the proxy variable approach imply a tax multiplier of around -0.7 to -1.7 , whereas the naive approach leads to a tax multiplier of around -0.6 to 0.2 .

5.3 Robustness

We study how the resulting tax multiplier estimate changes with a battery of robustness checks. The results are summarized as [Table 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 0.74 to -0.71 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 -0.06 , whereas controlling for the proxy implies a multiplier of -0.82 .

Next, we consider an alternative definition of our proxy variable. It is possible that tax bills that are not consistent with the expectation of future economic activity fail earlier in the legislative process, whereas tax bills more consistent with the expectation lasts longer in the Congress. In this case, proposed tax changes that pass at least one chamber of the Congress but ultimately fail

(“barely failed” tax bills) may be a superior proxy for the expectation of future economic activity. However, using these subset of 110 tax bills to construct our unlegislated tax change variable leads to a tax multiplier of -0.83 .

As mentioned in [Section 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. Under the alternative approach of taking an average, the tax multiplier estimate is -1.18 .

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 becomes more negative at -1.31 .

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, similarly to Romer and Romer (2010). In this case, the tax multiplier estimate become more negative at -1.38 .

6 Conclusion

In this paper, we propose correcting for the bias in tax multiplier studies using an unlegislated tax change series as an additional control. Using this approach, we obtain a tax multiplier of around -0.7 to -1.7 in the recent U.S. sample of 1975-2017.

We believe our approach can have fruitful applications. Since it uses the 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.

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Figures and Tables

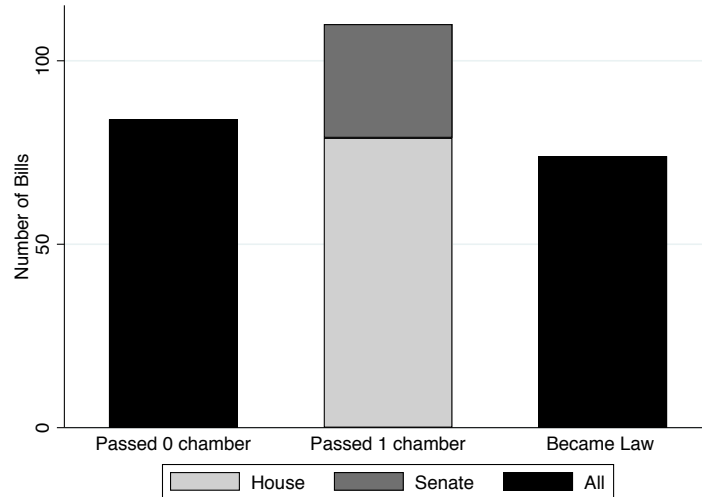


Figure 1: Distribution of Proposed Tax Bills by Last Legislative Action

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

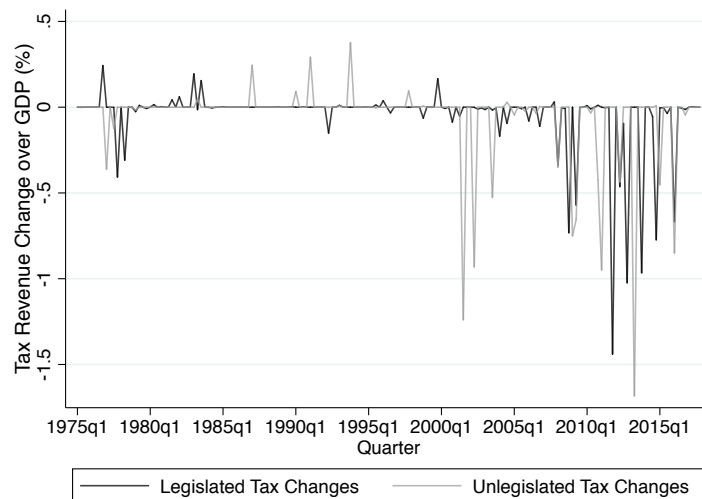


Figure 2: Legislated and Unlegislated Proposed Tax Changes

The figure plots the quarterly legislated and unlegislated 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.

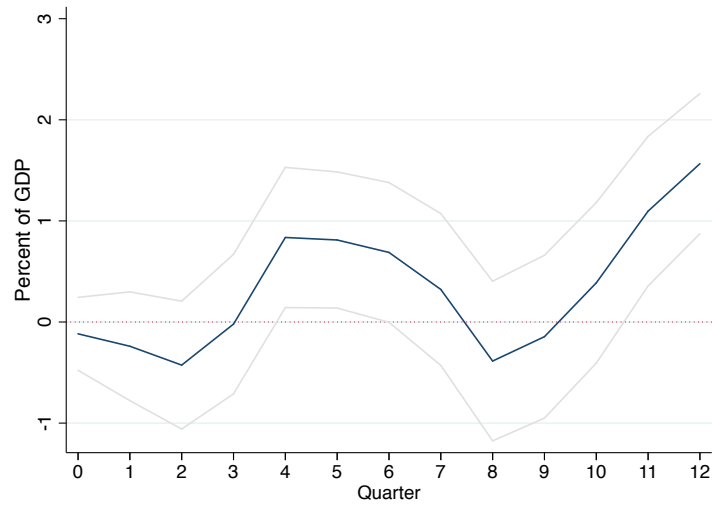


Figure 3: The Unlegislated Tax Multiplier: Estimated Change in GDP Associated with an Unlegislated Tax Increase of 1 Percent of GDP

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 4a. Baseline specification

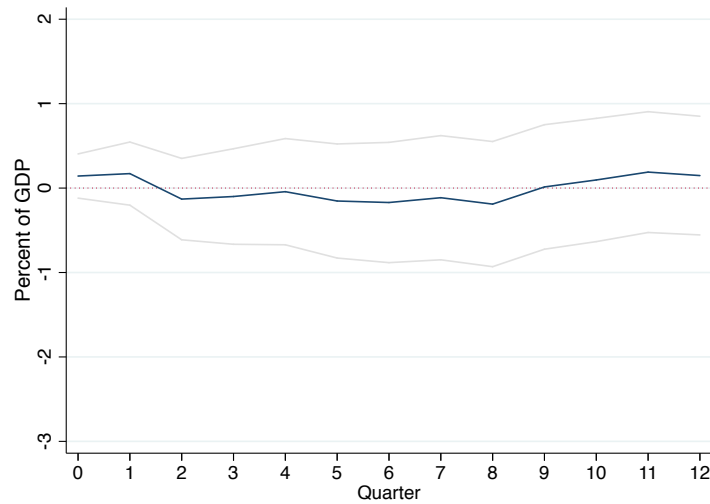


Figure 4b. With a dummy variable for the 1997-2017 sample

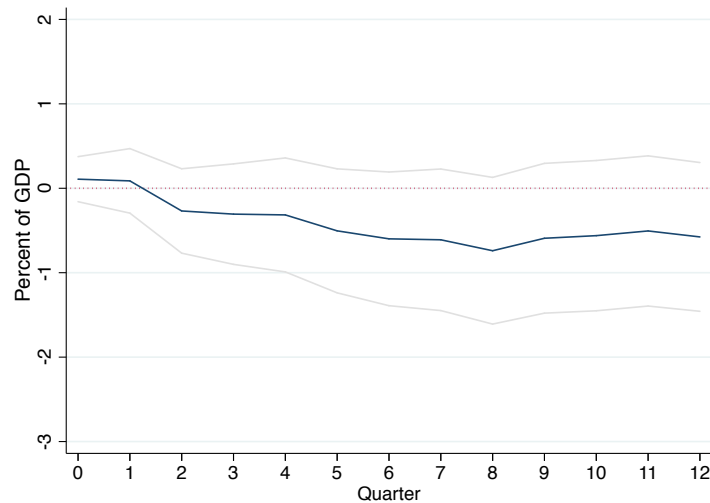


Figure 4: The Naive Tax Multiplier on Legislated Tax Changes: Estimated Change in GDP Associated with an Legislated Tax Increase of 1 Percent of GDP

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 5a. Baseline specification

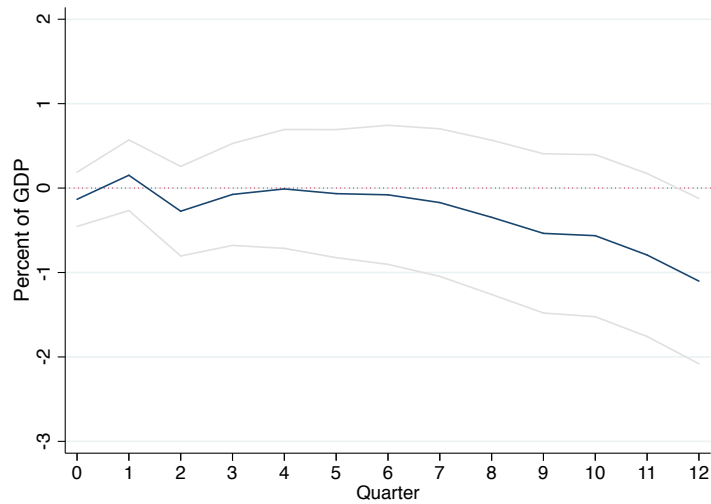


Figure 5b. With a dummy variable for the 1997-2017 sample

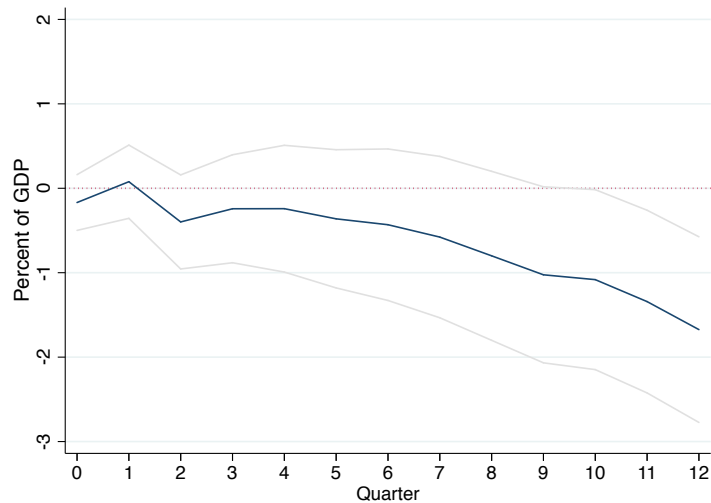


Figure 5: Tax Multiplier Based on the Proxy Approach: Estimated Change in GDP in Response to a Tax Increase of 1 Percent of GDP

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 Unlegislated Tax Changes

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 12 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%.

Legislation plus implementation lag (quarters)	Legislation plus		Controlling for other predictors				
	Baseline	Dummy	Lagged GDP	SPF	SC	Greenbooks	Livingstone
2	1.34* (0.69)	0.84* (0.78)	1.13* (0.69)	1.08** 0.51	0.38 (0.66)	1.69* (0.88)	1.13* (0.71)
4	1.41** (0.70)	0.49 (0.76)	0.71* (0.69)	1.22** (0.51)	0.71* (0.63)	1.50* (1.03)	1.54** (0.70)
6	1.57** (0.69)	0.65 (0.77)	0.81* (0.69)	1.35*** (0.51)	1.05* (0.63)	1.98* (1.12)	0.95* (0.72)
8	1.57** (0.71)	1.26* (0.76)	1.46** (0.70)	1.42*** (0.52)	1.15* (0.64)	1.83* (1.32)	0.92 (1.20)

Table 2: Tax Multiplier on Legislated Tax Changes

Implementation lag is the duration of a tax from the time it is legislated to the time it is implemented. Legislation lag is the duration of an unlegislated tax from 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 12 lags) tax multiplier. * - significant at 32%.

Implementation lag (quarters):	2			4			6		
	0	2	4	0	2	4	0	2	4
Naive tax multiplier	— 0.05 — (0.72)			— 0.15 — (0.70)			— 0.03 — (0.68)		
Tax multiplier (corrected using the proxy approach)	-1.02 (1.07)	-1.00* (0.99)	-0.87 (0.97)	-0.88 (1.02)	-1.10* (0.97)	-1.15* (0.92)	-1.35* (1.01)	-1.14* (0.94)	-0.77 (0.93)

Table 3: **Robustness Checks**

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 12 lags) tax multiplier. Time period of the regression is 1975-2017. The legislated and unlegislated tax changes are expected to be enacted 4 and 6 quarters after their last congress discussion date. * - significant at 32%.

	Naive Tax Multiplier	Corrected Tax Multiplier (Proxy Approach)
Baseline specification	0.15 (0.70)	-1.10* (0.97)
Post 1997 dummy	-0.58 (0.88)	-1.67* (1.10)
Government spending	0.14 (0.72)	-1.06* (0.97)
Without lagged GDP	0.74* (0.69)	-0.71 (1.04)
Pre-crisis sample	-0.06 (1.14)	-0.82 (1.00)
Using "barely failed" bills	0.11 (0.69)	-0.83 (1.02)
Averaging JCT estimates	0.23 (0.74)	-1.18* (1.14)
ΔT spread over 1 year	-0.12 (0.74)	-1.31* (1.12)
Using net present value	-0.06 (0.73)	-1.38* (1.00)