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Have the Growth Effects of Tax Policy Changed Over Time?

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ABSTRACT

We extend Romer and Romer's (RR, 2010) classic narrative analysis of exogenous tax shocks to cover data through 2019 and identify an exogenous tax increase in 2013 and an exogenous tax cut in 2018. We find that tax cuts have the same substantial effects on growth over the 1950-2019 period as RR found for 1950-2007. Several major events circa 1985 – the indexing of the income tax for inflation, the onset of the Great Moderation, and changes in monetary policy – could plausibly have changed the effects of tax shocks. We test this idea and find that although tax cuts substantially boosted growth in 1950–1984, they had minimal effects on GDP in 1985-2019. Changes in monetary policy before and after 1985 – specifically, in the federal funds rate's response to tax changes – are qualitatively consistent with the changing GDP response to taxes. Rough estimates of the share of the GDP response due to monetary policy span a wide range.

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I. Introduction

The effect of tax policy on economic growth is one of the most enduring, controversial, and important topics in both macroeconomics and public finance. The issue has become even more salient in recent years due to major tax legislation in 2017 and 2025 and growing awareness that tax increases may need to be part of the policy response to the federal government's long-term fiscal shortfalls.

A particular challenge is to determine the direction of causation because taxes affect the economy, but economic activity affects tax revenues and marginal tax rates. To untangle this problem, Romer and Romer's (RR, 2010) "narrative" analysis identifies exogenous policy shifts through close examination of government documents to determine the primary reasons for each legislative act.[1] Acts that were undertaken for reasons unrelated to the current state of the economy – for example, those intended to address long-term growth, inherited deficits, or fairness, or those based on ideology – are considered exogenous with respect to the current economy and therefore valid for purposes of identifying the causal impact of tax changes on growth. In contrast, acts that are motivated by cyclical concerns or to pay for a spending increase are labelled endogenous and discarded from the analysis. Using quarterly U.S. data from 1950 to 2007, RR generated several key results, most notably that exogenous tax cuts boost economic growth. Specifically, a tax cut that reduced federal revenues by 1% of GDP upon impact (which we will call a "standardized tax cut") raised GDP by a peak amount of about 3% over 2-3 years, with strong impacts on consumer durables and investment. Over the past 15 years, the narrative approach has become the standard method for evaluating the causal effect of tax changes and has been employed in numerous analyses of the U.S. and other countries, with a broad and general

[1] Blanchard and Perotti (2002) and Auerbach and Gorodnichenko (2012) propose other ways to establish causation.

conclusion that cuts in tax revenues and/or marginal rates spur economic growth.[2]

This paper builds on the narrative analysis literature. We extend the RR data through 2019 (ending before the onset of the COVID pandemic, to avoid the complications of modelling the major fiscal and economic shocks that took place then). We identify an exogenous tax increase that takes effect in 2013 (the expiration of the high-end Bush tax cuts) and an exogenous tax cut that takes effect in 2018 (the Tax Cuts and Jobs Act of 2017). We find that the effect of exogenous tax changes on GDP over 1950-2019 is almost identical to the effects in the original RR sample period through 2007. (To avoid repetition going forward, we note that all the tax changes in this paper are exogenous, as defined by the RR framework, unless specifically noted otherwise.)

But there is no reason to expect the effects of tax policy to be constant over time. Tax cuts differ – in terms of how much they reduce average versus marginal rates, how permanent they are, and who they benefit. Even the same tax cut implemented at different times can have varying effects because of differences in the cyclical state of the economy, credit markets and borrowing constraints, the level of marginal tax rates or public debt, the monetary policy stance, international institutions and the openness of trade, or the age distribution of the population. All of these determinants have varied over time. In any case, examining the effect of taxes over the entire 1950–2019 period gives the same weight to recent tax changes as it does to changes that occurred under circumstances that may not accurately reflect the last few decades.

[2] For U.S. analysis, see Barro and Redlick (2011), Favero and Giavazzi (2012), Ferraro and Fiori (2020), Fritsch and Mazlish (2026), Mertens and Montiel Olea (2018), Mertens and Ravn (2012, 2013), Romer and Romer (2016), and Zidar (2019). For analysis of other specific countries, see Cloyne (2013) and Hayo and Uhl (2014). For cross-country analysis, see Alesina et al. (2017), Alesina, Favero, and Giavazzi (2019a, 2019b), Auerbach and Gorodnichenko (2011), Bhasin and Loungani (2026), Dabla-Norris and Lima (2023), Guajardo, Leigh and Pescatori (2014), Gunter et al.(2021), Jordà and Taylor (2016), Kawano et al. (2025), and Riera-Crichton, Vegh, and Vuletin (2016). See Balasundharam et al. (2023) and Gale, Berlin, and Thorpe (2025) for surveys.

With these considerations in mind, we highlight several key trends and events that occurred around 1985 and that could plausibly affect the impact of tax changes. The volatility of many macroeconomic aggregates declined – a phenomenon colloquially termed the "Great Moderation" (Stock and Watson 2002, Bernanke 2004). The income tax was first indexed for inflation in 1985 (Joint Committee on Taxation 1981). Monetary authorities abandoned reserve management in the early 1980s and gradually moved toward a focus on interest rate adjustments to stabilize the economy (Meyer 2001). Finally in 1986, top marginal rates were slashed; they have fluctuated since then but have remained permanently lower than before (Auerbach 2026).

Given these changes, we test for differences in tax effects on GDP starting in 1985. Our central result is that, although the effects of tax cuts on GDP were quite large between 1950 and 1984, the effects almost vanished between 1985 and 2019. A standardized tax cut raised GDP by as much as 3.65% in the earlier period, but the peak effect was an order of magnitude smaller, 0.37%, in the later period. The difference in peak effects in the two periods is thus approximately 3.3% of GDP. The peak *difference* is about 3.6% of GDP, occurs after 9 quarters, and represents GDP increases of 3.65% in the earlier period and just 0.07% in the later period. Using a variety of robust methods for calculating standard errors, we show that the difference across the time periods is statistically significant.

We provide a variety of sensitivity analyses. First, we show that the results are not due to our extension of the data to 2019. Very similar results emerge for the sample that ends in 2007, the period used in RR. Second, we show that the response of GDP to tax policy stabilized starting in 1985. Testing for time-varying effects that emerge after 1985 generates stable and small effects of taxes on GDP through 2019. In contrast, testing for time-varying effects that begin before 1985 generate results that change considerably with the starting date of the break. Third, the results are robust to including dummy variables that control for unusual economic

episodes – the turbulent period around 1981, the Great Financial Crisis, or the period in 2008-2015 when the Federal Funds rate was constrained by the zero lower bound. Finally, we report results using local projections methods that corroborate the findings reported above, which employ ordinary least squares/distributed lag models.

Turning to components of GDP, durable consumption, non-residential investment, and consumption of nondurables and services responded more positively to a standardized tax cut in the earlier period than in the later period, by about 14%, 12%, and 2%, respectively, at the 9-quarter horizon (when the difference in the GDP response peaks). These differences are generally statistically significant. Together, these three components account for more than 100% of the difference in the GDP response to taxes over the two sub-periods. The point estimates for residential investment show large (8%) time-varying differences in responses to tax changes but are imprecisely estimated. Tax cuts are associated with lower net exports in both periods, consistent with economic theory.

Tax cuts are also associated with a more positive change in government purchases in the later period than in the earlier period. This implies (and we show) that the difference in the response of private economic activity (defined as GDP less government purchases) to tax cuts over the two periods is even larger than the difference in the response of overall GDP. Tax cuts had no positive effect on private economic activity in the 1985-2019 period.

Other evidence supplements these results. A standardized tax cut reduced federal revenue by almost 1% of GDP in the later period, suggesting little economic response. In contrast, a standardized tax cut reduced revenues by only about 0.4% of GDP in the earlier period, suggesting a more robust economic reaction to tax changes. We also find that the peak difference across the two periods in the unemployment rate's response to tax changes is just over 1 percentage point. This difference is imprecisely estimated but is loosely consistent with an

Okun's Law coefficient around 3.

Given these results, a natural question is why the GDP response to taxes changed. Although there are many possible reasons, in this draft we focus on one, key potential factor – monetary policy. Time-varying differences in the federal funds rate response to tax changes are qualitatively consistent with the GDP results and highly statistically significant. In the earlier period, the funds rate tended to amplify the effects of tax changes. In the later period, it tended to offset tax changes. These results hold even more strongly after (a) adding a dummy variable to control for the turbulent period around 1981, and (b) substituting a Wu-Xia (2016, 2022) "shadow rate" for the federal funds rate when the zero lower bound applied in 2008-2015.

We provide some very rough calculations of the share of the changing GDP response to taxes that is due to monetary policy. These estimates range from 25% to more than 100%, but we highlight that they are uncertain for several reasons.

The paper proceeds as follows. Section II explains our econometric framework and data extensions. Section III analyzes the effects of tax changes on GDP over the RR sample period and 1950-2019. Section IV examines the time-varying response of GDP to tax cuts. Section V reports time-varying effects of taxes on other aggregate measures. Section VI explores the role of monetary policy. Section VII concludes.[3]

II. Framework and Data

A. One Caveat

At the outset, we note that a reasonable qualification to our analysis, and of the entire

[3] Appendix A describes the construction of our data set, including the methodology and results for characterizing tax changes between 2007 and 2019. Appendix B reports local projections results that are analogous to the ordinary least squares results shown in the main text. Appendix C explores previous work and our estimates relating to several issues in the literature: the effects of growth-oriented versus deficit-driven tax changes; the effects (and definition) of anticipated versus unanticipated tax changes, and the use of narrative tax shocks as instruments rather than explanatory variables.

narrative literature, is that there is no such thing as a truly "exogenous" macroeconomic tax policy change. This applies both in the sense that policies are not conducted as randomized control trials that can impose an exogenous treatment, and in the sense that a tax change is never truly independent of the state of the economy.[4] Nevertheless, within the normal range of economic activity, legislators do appear to have clear motivations for different tax changes, and the narrative identification process has proven to be quite informative in a large number of analyses.

B. Basic Regression Specification

We estimate the distributed lag regression given by

$$\Delta Y_t = a + \sum_{i=0}^{M} b_i \, \Delta T_{t-i} + \sum_{j=1}^{N} c_j \, \Delta Y_{t-j} + e_t, \qquad (1)$$

where the dependent variable, ΔY_t, represents the percentage change in real GDP and ΔT_t reports the size and sign of exogenous tax changes divided by GDP, as derived by RR and in our Appendix A. We follow RR in using quarterly data and setting $M = 12$ and $N = 11$. Including lagged output helps control for the normal dynamics of output as well time patterns in other variables that are correlated with GDP. The impulse response, $\sum_{i=0}^{h} \hat{b}_i$, measures the cumulative effects of a tax shock h quarters after implementation.

To examine whether the response of GDP to tax policies changes over time, we estimate

[4] For example, a tax cut that is motivated by the goal of boosting long-term growth would probably not be enacted if the current and projected growth rate of the economy were already extraordinarily high (say, 15%). Likewise, a tax increase to deal with an inherited budget deficit would probably never be enacted if the inherited deficit were already projected to turn quickly to high and rising surpluses. In these examples, the existence of the growth-motivated tax cut is in fact related to the current state of the economy – namely, to the fact that the current growth rate is not already 15% – and the existence of a tax increase to deal with inherited deficits is also related to the current state of the economy – namely, the fact that it is not changing in a way that would allow the budget to move quickly to surplus status.

$$\Delta Y_t = a + \sum_{i=0}^{M} b_i \, \Delta T_{t-i} + \sum_{j=1}^{N} c_j \, \Delta Y_{t-j} + \alpha D_t + \sum_{i=0}^{M} \beta_i \, (D_t \times \Delta T_{t-i}) + \sum_{j=1}^{N} \delta_j \left(D_t \times \Delta Y_{t-j}\right) + e_t \quad (2)$$

where the variables are defined as before and D_t is a dummy variable that takes the value one if time t is 1985:I or later. We test two null hypotheses: whether each of the interaction coefficients is zero; and whether the sum of the interaction coefficients is zero, given respectively by:

(A) $\beta_i = 0 \; \forall i$

(B) $\sum_{i=0}^{12} \beta_i = 0$

Finally, to examine changes over time in how tax changes affect other variables Z – including components of GDP, federal revenues, the unemployment rate, the federal funds rate, and the 10-year Treasury rate – we estimate equations of the form:

$$\Delta Z_t = a + \sum_{i=0}^{M} b_i \, \Delta T_{t-i} + \sum_{j=1}^{N} c_j \, \Delta Z_{t-j} + \sum_{j=1}^{N} d_j \, \Delta Y_{t-j} + \alpha D_t + \sum_{i=0}^{M} \beta_i \, (D_t \times \Delta T_{t-i}) + \sum_{j=1}^{N} \gamma_j \left(D_t \times \Delta Z_{t-j}\right) + \sum_{j=1}^{N} \delta_j \left(D_t \times \Delta Y_{t-j}\right) + e_t \quad (3)$$

where the variables are defined as before and *Y* still represents GDP.

B. Testing for time-varying differences

The natural way to test hypotheses (A) and (B) using equations (2) and (3) is through F-tests. The conventional F-test, however, relies on homoskedasticity assumptions (Wooldridge 2010, Schmidt and Sickles 1977, Toyoda 1974) that require the unexplained variation in quarterly GDP to be constant throughout the sample. This assumption is violated in our data. The residual variance of quarterly GDP growth from estimating (2) is 4.66 times as large in the 1950-84 period as in the 1985-2019 period. This result reflects the onset of the Great Moderation and

is consistent with other studies.[5] As a result of this difference, a conventional F test would be overly conservative and would reject the null hypotheses less often than its nominal level.[6]

We therefore conduct tests based on three alternatives. The first is the feasible generalized least-squares (FGLS) procedure. FGLS reweights each observation by its subperiod's estimated residual standard deviation and then runs a conventional F-test on the reweighted regression (Wooldridge 2010). The validity of this procedure leans on large-sample approximations, however, which may not be valid given our sample size.

Our second correction uses the wild bootstrap procedure (Gonçalves and Kilian 2004), which avoids large-sample approximations entirely. We resample observations by multiplying estimated residuals by an independent random sign (-1 or +1) while leaving their magnitude unchanged. Because the magnitudes are preserved, the resampled datasets from the bootstrap procedure retain the decline in volatility post-1984, therefore validating this resampling scheme even in the presence of volatility break.

The third alternative correction is to use Newey-West (1987, 1994) standard errors. This is potentially quite important because, as discussed below, we detect serial correlation of the error terms in equations for GDP and several other variables, and the Newey-West calculations correct for both heteroskedasticity and serial correlation. Bandwidth is selected (automatically via a data-driven procedure) to minimize the mean-squared error of the resulting long-run-variance estimate.

C. Extending the sample to 2019

[5]See, for example, Blanchard and Simon (2001), Gordon (2005), McConnell and Perez-Quiros (2000), and Stock and Watson (2002).

[6] The reason the conventional F test would be overly conservative is that the post-1984 tax response is identified only from data in the later subperiod, whereas the conventional test judges how precisely the post-1984 tax response is identified using variance estimated over the 1950-2019 sample. The latter measure is too large for the post-1984 estimates, so the standard errors on the post-1984 interaction coefficients would be overstated.

We extend the RR sample through the end of 2019, stopping at that point to avoid the complications of modelling the economy during the COVID-19 pandemic. For standard variables like GDP, its components, the unemployment rate, and federal revenues, we extend the sample drawing on FRED. We describe the collection of data on the federal funds rates and 10-year Treasury rates in Section VI.

Appendix A describes how we classify tax changes enacted between 2008 and 2019. Of the several legislated tax acts over this period, we identify two as exogenous. The first is a tax increase: the expiration of the Bush tax cuts for high-income households, which took effect in 2013:I. The second is a tax cut: the Tax Cut and Jobs Act of 2017, which took effect in 2018:I. Revenue estimates were obtained from the Obama Administration (for 2013) and the Joint Committee on Taxation (for 2018).

While the exogeneity of every narratively-determined tax policy shock cannot be proven with finality, we find that these shocks are not Granger-caused by output growth or other exogenous tax changes (Appendix Table A1).

D. Overall sample statistics

Table 1 reports that from 1950 to 2019, there were 46 quarters with exogenous changes, 29 between 1950 and 1984 and 17 between 1985 and 2019. There were 23 tax cuts, averaging just over 0.5% of GDP, with more tax cuts in the first period (18) than in the second (5) – though the cuts were larger on average in the second period (0.78% of GDP) than in the first (0.45% of GDP). The 23 tax increases were roughly, evenly split across the periods and averaged just over one-quarter of 1% of GDP in each period.

III. Replication and Extension of GDP Results

Before turning to the regression results, we first present scatter plots to summarize basic descriptive patterns in the data. Figure 1A plots, for the 1950-2019 period, the relationship

between (a) real GDP growth over the 9 quarters following tax changes and (b) the size and sign of those tax changes.[7] The 9-quarter horizon was chosen because the peak effects estimated in many of the regressions below occur 9 quarters after the enactment of a tax change. The Figure shows a positive relationship between tax cuts and growth, with a standardized tax cut associated with a 1.92% (s.e. = 0.99%) increase in GDP after 9 quarters. There is wide variation in both tax changes and GDP changes. In particular, there are several tax increases associated with higher-than-average growth and several tax cuts associated with lower-than-average growth.

Figure 2 shows results from estimating equation (1). Over the 1950-2007 period, the effects of tax changes on GDP replicate RR's results very closely (though not exactly because some of the data were revised after publication of their article). Extending the sample to 2019 generates very similar effects. In both cases, a standardized tax cut raises GDP by about 3.0% (t=2.9 in each case) after 10 quarters. As RR point out, technically, the graph shows the effect of a 1%-of-GDP tax *increase* and can be read as "tax increases reduce growth." But to conform with common discussion, both they and we describe what the figures imply in terms of the effects of tax cuts, rather than increases.

IV. Time-varying effects of tax changes on GDP

A. Background

As noted in the Introduction, there is no reason the impact of taxes on GDP or other variables should be constant over all time periods. For the several reasons laid out, 1985 is a natural point to examine for changes in the effects of tax cuts. First, it is approximately the beginning of the "Great Moderation," which saw substantial reductions in the variance of GDP

[7] The Tax Cuts and Jobs Act (TCJA) of 2017 took effect in 2018:I. Nine quarters later, by 2020:II, the COVID pandemic had begun and GDP had cratered. To avoid attributing that decline to TCJA, when we calculate the growth of GDP over 9 quarters, we replace the actual growth rate for 2020:I and 2020:II with the average GDP growth rate over the last two quarters of 2019.

and other macro aggregates, a change that could affect the impact of tax policy.[8]

Second, 1985 marks the first year in which the income tax was indexed for inflation. The 1981 tax act indexed the personal exemption, standard deduction, and tax brackets for inflation, but the implementation was delayed until 1985. Indexing eliminated the automatic annual tax increases and bracket creep induced by inflation, therefore altering how taxes shift in years without legislated changes. As a result, this could also plausibly affect the impact of newly legislated tax cuts.

Other important and lasting changes in monetary and fiscal policy further marked the post-1984 period as different from the pre-1985 period. After targeting reserves in 1979-1982, monetary authorities gradually moved back toward a focus on managing interest rates starting in 1983, though not reaching completion until a few years later (Meyer 2001). In 1986, marginal tax rates for high-income households were cut substantially, a change that has more or less persisted (Auerbach 2026), as the income tax base was broadened. While this would not have influenced activity in 1985, it could have helped to preserve a new status quo in later years.

RR examine results for sample splits starting in 1981 and find that the GDP effect of tax cuts is smaller in a post-1980 sample but positive and large in both periods.[9] Splitting the sample

[8] Although several papers provide an exact quarter for the beginning of the Great Moderation – 1984:I for Kim and Nelson (1999) and McConnell and Perez-Quiros (2000) and 1983:II for Stock and Watson (2002) – there is naturally considerable uncertainty about when such a broad phenomenon began. Stock and Watson (2002) estimate that a 67% (95%) confidence interval for the structural break in the variance of GDP covers the period from 1982:IV to 1985:III (1982:I to 1989:IV), and they find that only about one-quarter of the 168 time-series evaluated have structural breaks in the 1983-1985 period. Blanchard and Simon (2001) argue that GDP variance changed as part of a longer-term trend; when they isolate a precise date, they find a break at 1986:I. Hall (2003), using five-year variance measures, argues for a later date than the mid-1980s.

[9] RR find the pre-1981 peak effect is 4.29% (t=2.19) of GDP. The post-1980 peak effect is 3.08% (t=3.61) of GDP. Cloyne (2013), using narrative techniques and U.K. data, obtains similar results, finding that tax cuts had peak GDP effects of 3.6% between 1955 and 1979 and 2.5% between 1980 and 2008. Favero and Giavazzi (2012) extend the data in Blanchard and Perotti (2002) to cover data through 2001. They find similar results during 1960-1980, estimating that a standardized tax cut raised output by 2.6%. In the 1980-2001 period, however, the find that tax cuts had negative effects on output. For other papers that discuss how the effects of tax changes vary based on economic conditions, see Agostini, Asatryan, Bach, et al. (2026), Hsieh, Sanz-Maldonado, and Slemrod (2023), and Slemrod and Kopczuk (2002).

at that point, however, may be problematic. The years just before and after 1981 featured a variety of turbulent economic events, including an oil crisis, stop-and-go monetary policy changes, high and volatile interest rates, two sharp recessions, a major tax cut, and then several tax increases. Our split in 1985 avoids the turbulence of the previous several years and coincides with what was essentially the onset of a new era in the economy.

B. Central Results

As before, we begin by summarizing the basic patterns in the data. Figures 1B and 1C show the relationship between 9-quarter GDP growth and the size and sign of tax changes in the earlier (1950-1984) and later (1985-2019) periods, respectively. In the earlier period, a standardized tax cut is associated with GDP that is higher by 3.00% (s.e. = 1.55) – a larger response than in the overall 1950-2019 period shown in Figure 1A.

In the later period, however, there is a noticeable flattening out of the relationship, with a standardized tax cut generating only 0.11% in higher GDP, an effect that is insignificantly different from zero. In particular, the Figure shows that none of the 2001, 2003, and 2017 tax cuts – which averaged more than 1% of GDP – are associated with high subsequent growth. This is particularly notable for the Tax Cuts and Jobs Act of 2017, which was the largest of the three Acts (as a share of GDP) and which cut corporate tax rates substantially and provided strong investment incentives.[10]

[10] As a share of GDP, the tax cuts equaled 0.80% (2001), 1.14% (2003) and 1.37% (2017). Growth was slow after the 2001 and 2003 tax cuts, despite simultaneous increases in government spending. Gale and Potter (2002) and Gale and Orszag (2005) estimate that the 2001 tax cut ended up raising the cost of capital for investments in residential housing, sole proprietorships, and corporate structures because the impact of higher deficits on interest rates outweighed the impact of lower marginal tax rates. Yagan (2015) presents empirical evidence that the 2003 tax cut had little impact on investment or employment. Furman (2020) notes that the growth rate was the same in the 8 quarters before and after TCJA was enacted, despite higher government spending and easing Fed policy. Gale, Hoopes, and Pomerleau (2024) provide evidence relating to the level and composition of aggregate investment that suggests that TCJA had a small aggregate impact.

Figure 3 shows the main result in the paper, estimating the time-varying response of the real quarterly change in GDP to tax changes using equation (2). The response of GDP to standardized tax cuts in 1950-1984 looks like the response over the whole 1950–2019 period shown in Figure 2: a standardized tax cut raises GDP by 3.65% (t = 3.07) after 9 quarters. But the peak effect of a standardized tax cut on GDP in 1985–2019 is only one tenth as large (0.37% of GDP (t=0.16)) after 8 quarters. The difference in the tax effect on GDP, measured at the peak effects for each period, is 3.27%. The maximum difference occurs at 9 quarters out, where the GDP effects are 3.65% and 0.07%, yielding a difference of 3.58%.

Table 2 reports p-values for tests of whether the interaction coefficients shown in Figure 3 are significantly different from zero.[11] A Breusch (1978)-Godfrey (1978) test reveals serial correlation in the GDP equations, making the Newey-West standard errors particularly salient for assessing the statistical significance of the results.

Considering first the tests reported in the bottom line of the table – which use data from all 12 lags – we find mixed results. The null hypothesis that each interaction coefficient is zero is rejected at the 5% level in two of the three tests and at the 10% level in the third. The null hypothesis that the sum of the interaction coefficients is zero survives better. Only one of the three tests rejects the null and only at the 10% level. The explanation for the latter results can be seen from Figure 3. After peaking in quarter 9, the effect of taxes on GDP falls in the earlier period in quarters 10-12, reducing the difference between the earlier-period and later-period

[11] The standard error bands in Figure 3 report how precisely each period's response is estimated and so use an unrestricted wild bootstrap procedure, i.e., no null hypothesis is imposed in generating resampled data. In contrast, Table 2 examines tests of the null hypothesis that the pre-1985 and post-1984 responses are equal, and so reports bootstrap standard error estimates where resampled data are generated under that null. Gonçalves and Kilian (2004) establish that both variants are valid under conditional heteroskedasticity and autoregressive processes. RR (2010, footnote 15) note that their "...standard errors are computed by taking 10,000 draws of the coefficient vector from a multi-variate normal distribution with mean and variance-covariance matrix equal to the point estimates and variance-covariance matrix of the regression coefficients." When we follow the same procedure, the p-values are 0.198 for hypothesis A and 0.169 for hypothesis B. For reasons noted in the text, we believe those p-values are misleadingly high

results. The gap between the two periods in the impact of taxes on GDP is 3.6 percentage points (pp) after 9 quarters but only 2.7 pp after twelve, while the standard error rises from 1.6 pp to 1.8 pp. This raises the issue that the 12-quarter results may not be representative of results at other lag lengths.

We therefore perform similar tests for each lag length from 7 to 11. These tests show overwhelming rejection of the first null – 13 of the 15 tests reject at the 5% level and the other two reject at the 10% level. The tests also reject the second null, though not in quite as overwhelming a fashion – with 7 of the 15 rejecting at the 5% level and 6 more rejecting at the 10% level. Notably, the Newey-West tests, which address the serial correlation found in the estimating equation, reject both nulls in 11 of 12 reported tests at a 5% level and the twelfth test at a 10% level. Taken together, we view the tests reported in Table 2 as providing strong evidence that the time-varying differences in the effect of taxes on GDP are statistically significant.

C. Sensitivity Analysis

We offer five sensitivity analyses, each of which supports the results above. First, we highlight that the small effects of tax changes on GDP in the 1985–2019 period are not an artifact of the years we added to the data, 2008–19. Figure 3 shows that a very similar pattern holds for 1985–2007, where the maximum impact of a standardized tax cut on GDP in this period is just 0.6%. This period is of interest for at least two reasons. It covers the Great Moderation period until just before the Global Financial Crisis, so it contains no major economic crises. Also, it implies that the RR results for the 1981-2007 period are due to the inclusion of data from 1981-1984.

Second, precisely because the exact date of the beginning of the Great Moderation is uncertain, we report results with different starting dates for the "later period." Figure 4 shows the

effects of taxes on GDP in the later period when we vary the start date to cover the first quarter of each year from 1981 to 1989. Table 3 provides estimates of the peak effects in each combination of earlier period and later period. Figure 4 shows that 1985 is when the estimated effects of taxes on GDP stabilize, strengthening our main results. The tests for time-varying effects where the later period starts in 1985 or subsequent years yield stable (and small) estimates of the effects of tax cuts on GDP. In contrast, when the later period is defined to begin before 1985, the estimates change substantially with the starting date chosen. For instance, starting the later period in 1981 – and thus adding just 4 years to the data – changes the 9-quarter effect from essentially zero (0.07%) for the sample starting in 1985 to 2.57% for the sample starting in 1981.

The pattern shown in the Figure – stable, small estimates when the post-break window opens at or after 1985, growing estimates when the window starts progressively earlier than 1985 – is consistent with a structural break in 1985. Moreover, the results highlight the special nature of the period surrounding 1981, which as noted earlier, was especially turbulent. These results are consistent with Coibion's (2012) findings that estimates of the impact of monetary policy are sensitive to the treatment of the period between 1979 and 1982. Indeed, if anything, the extreme sensitivity of the results to including 1981-1984 highlights both the atypicality of the turbulence and policy choices during that period, as well as the sharp contrast with specific long-lasting changes – in the economy as a whole, monetary policy, income tax indexing, and marginal tax rates – that took effect around 1985 and plausibly continue to influence the results going forward.

As a third sensitivity test, we provide dummy variables to control for three unusual economic episodes – the economic and policy turbulence surrounding 1981 (1979:IV to 1983:IV), the Global Financial Crisis (2008:III to 2009:II), and the period when the Federal Funds rate was at the zero lower bound (2008:IV to 2015:IV). In each case, we re-estimate the

equation shown in Figure 3, with time-varying effects beginning in 1985:I. The results, in Appendix Figures A1, A2, and A3, show that the patterns shown in Figure 3 are robust to these controls. The statistical significance (not shown) of the difference between the pre-1985 and post-1984 patterns is about the same – or if anything, slightly enhanced – relative to the results without the dummy variables shown in Table 2.

The fourth sensitivity test involves re-estimating these results using local projections methods with similar results, as described in Appendix B. The fifth test is simply that a Chow test that accounts for autocorrelation and heteroskedasticity, finds that the null of no break at 1985:I is strongly rejected ($p = 0.001$). (This does not hold with the classical Chow tests that do not adjust for heteroskedasticity or serial correlation.)

V. Time-Varying Tax Effects on Other Macro Aggregates

To provide a more complete picture, we examine the time-varying impact of tax changes on major components of GDP, federal revenues as a share of GDP, and the unemployment rate, following equation (3). Using Breusch-Godfrey tests again, we detect serial correlation in the equations for consumption of durable goods, federal revenues/GDP, and the unemployment rate, making the analysis based on Newey-West standard errors particularly relevant.[12]

Figure 5 shows the time-varying effects of tax changes on components of GDP, with results summarized in Table 4. Consumption of nondurables and services (Figure 5A) rose by 1.8%, 9 quarters after standardized tax cuts in the earlier period, but was essentially flat with respect to tax cuts in the later period. Consumption of nondurables and services account for 58% of GDP (Table 4), meaning that although the percentage difference in response to tax changes

[12] For the components of GDP other than net exports, the dependent variable is measured as the log change in the real dollar value. For federal revenues as a share of GDP, the unemployment rate, and net exports/GDP, the dependent variable is measured as the quarterly change.

over the two periods is small, the impact accounts for a significant share of the changing GDP response to tax cuts. The largest percentage difference over time comes from the response of durable consumption to tax changes (Figure 5B). In the earlier period, a standardized tax cut raised durable consumption by a little more than 10% over 9 quarters. In the later period, durable consumption actually fell by about 4% over 9 quarters following a standardized tax cut. A similar difference occurs for nonresidential investment (Figure 5C) which rose by about 6% following a standardized tax cut in the earlier period but fell by about the same percentage after a standardized tax cut in the later period.

Taken together, the differential responses of these three GDP components 9 quarters after a tax change (when the GDP difference peaks), account for a 3.8 pp difference in the GDP response to tax cuts in the two periods, more than 100% of the 3.6 pp difference in the GDP response documented earlier. Table 4 shows that null hypothesis A is strongly rejected for these components, but null hypothesis B cannot be rejected.

Turning to the other components, the difference in the response of residential investment to tax cuts is 8.4% in the two periods (Figure 5D), consistent with durable consumption and non-residential investment. But the effect is estimated imprecisely because of the large variation over time in residential investment. Net exports responded to tax changes essentially the same way in the two periods (Figure 5E).

Government purchases responded in the opposite way as GDP in the two sub-periods (Figure 5F). This implies that the response of private economic activity – defined as GDP minus government purchases – to taxes varies more in the two time periods than does GDP. This is borne out in Figure 5G, which shows that standardized tax cuts raised private economic activity by a peak of 4.64% in the 1950-1984 period (compared to 3.65% for GDP) and reduced private economic activity (by a peak of about 1%) in the 1985-2019 period. Not surprisingly, both null

hypotheses are decisively rejected for private economic activity (not shown).

Figure 6 examines the effects of tax cuts on the change in the ratio of federal revenues as a share of GDP. In the earlier period, revenues fell by about 0.4% of GDP in the first two years following a standardized tax cut and not at all in the third year. In the later period, revenues fell by 0.9% of GDP in the first year following a standardized tax cut and by about 1% of GDP in the second and third year. These results are qualitatively consistent with a stronger economic response to tax cuts in the earlier period but could also be due to other factors. For example, the fact that the income tax was not indexed for inflation in the earlier period implies that in the years following any tax cut, the automatic annual tax increases due to bracket creep would increase revenue and thus offset some of the original revenue loss. These differences are of mixed statistical significance – the first null is significant using both wild bootstrap and Newey-West standard errors; the second null is not significant using either (Table 5).

Figure 7 shows that after 9 quarters, tax cuts reduced the unemployment rate by 0.57 percentage points in the earlier period but raised it by 0.45 percentage points in the later period, for a 1.02 percentage point difference. Neither null hypothesis is rejected (Table 5) but the time patterns are consistent with the GDP results. Given the 3.58% difference in GDP effects noted above, the results for unemployment are loosely consistent with Okun's Law, which historically found coefficients around 3. More recent papers, though, have found Okun's Law coefficients closer to 2.0 or 2.5 (For example, Ball, Leigh, and Loungani 2017).

VI. Monetary Policy and Interest Rates

A. Federal Funds Rate and Shadow Rate

Monetary policy can serve to amplify or offset the effects of tax cuts.[13] We use the federal

[13] There is an enormous longstanding literature on interactions between monetary and fiscal policy. We highlight a number of recent papers. Chodorow-Reich (2019) surveys the empirical evidence on fiscal policy shocks in and

funds rate (with some adjustments) as a measure of monetary policy. Figure 8 plots the series on a quarterly basis, showing that it peaked during the early Volcker years and hit the zero lower bound (ZLB) during and after the Global Financial Crisis.[14]

The funds rate is an imperfect indicator of monetary policy (Coibion 2012, Romer and Romer 2004). In the 1950s and 1960s, the Fed often focused on changes in reserve requirements and credit restrictions as well as or instead of the funds rate. During the early part of Paul Volcker's term as chair of the Federal Reserve Board, between 1979 and 1982, the Fed abandoned funds rate targeting and instead targeted non-borrowed reserves. Over the next few years, the monetary authorities moved gradually back to a focus on interest rates (Meyer 2001). Under Alan Greenspan's term as Fed Chair, starting in 1987, the funds rate was the primary instrument used to stabilize the economy. During and after the Global Financial Crisis – between late 2008 and 2015 – the funds rate was at the ZLB and Fed policy focused on quantitative easing, forward guidance, and other unconventional tools. Despite these concerns, a very large literature – surveyed in Coibion (2012) and including subsequent contributions by Romer and Romer (2016, 2023) – uses the funds rate as a measure of the stance of monetary policy.

Figure 9A shows the results of estimating equation (3) with the quarterly change (averaged over monthly data) in the federal funds rate as the dependent variable. In the 1950–

outside of monetary unions, suggesting a 'no-monetary-policy-response' multiplier of approximately 1.7. Several papers examine the impact of fiscal policy under the zero lower bound (e.g., Ramey and Zubairy, 2018; Miyamoto, Nguyen, and Sergeyev, 2018) or under distinct exchange rate regimes (e.g., Corsetti et al., 2012; Born et al., 2024). Cloyne, Jordà, and Taylor (2023) use state-dependent local projections to estimate how monetary policy accommodation affects the impact of fiscal shocks in an international context. Their results suggest that fiscal multipliers can vary widely (from 0 to 2) depending on the degree of monetary policy accommodation.

[14] The FFR series is sourced from the Romer-Romer dataset (FYFF column), which extends FRED series DFF (effective federal funds rate, quarterly average, available from 1954Q3) back to 1950Q1 using Martens (1958). Taking the first difference and applying 11 AR lags pushes the effective regression start to 1953Q1. Data source: FRED DFF, https://fred.stlouisfed.org/series/DFF, downloaded 06/02/26; observations for 1950-I-1954-II from Martens (1958) as recorded in Romer-Romer (2010) Excel Sheet 2 (FYFF column).

1984 period, the funds rate fell quickly after a standardized tax cut and remained at the changed level for the next two years – which in turn would naturally amplify the effect of tax cuts. The rate fell by as much as 1.45 percentage points after 8 quarters following a standardized tax cut. In contrast, in the 1985-2019 period, standardized tax cuts led to a very slight increase (0.16 percentage points in quarter 8) in the funds rate on average. In short, in response to a standardized tax cut, the funds rate fell by 1.61 percentage points more in the first period than in the second period. This difference is highly statistically significant and is consistent with the time-varying effects of tax cuts on GDP, and other macroeconomic aggregates reported above.

To address the two extreme periods for the funds rate, we adjust the regressions as follows. First, Coibion (2012) shows that the estimated effects of monetary policy are sensitive to the treatment of the early Volcker years when reserve-targeting was the focus on Fed policy and the funds rate experienced elevated levels and wide swings. To address this period in a straightforward manner, we include an indicator variable for 1979:III through 1983:IV.

Second, for 1990 to 2019, we replace the funds rate with the Wu-Xia (2016, 2022) shadow rate, whose values over time are shown in Figure 8. The shadow rate is essentially equal to the funds rate when the latter is positive. When the funds rate is at the ZLB, the shadow rate is designed to capture the true stance of Fed policy, considering tools such as quantitative easing and forward guidance. It can be interpreted as a measure of what the funds rate would be if the ZLB constraint did not exist, and thus provides an indicator of monetary policy that is more consistent across periods. The shadow rate mirrors the funds rate from 1990 until the onset of the ZLB, at which point it is negative before recovering in the mid-2010s.[15]

[15] The Wu-Xia shadow rate time series is highly correlated with other efforts to measure similar concepts, including Bauer and Rudebusch (2013), Bullard (2012), Kim and Singleton (2012), and Krippner (2013). We have not estimated the effects of using any of these series.

Figure 9B shows the effects of these two adjustments. In the earlier period, the addition of the Volcker indicator slightly increased the amount of amplification of tax cuts, with a standardized tax cut leading to a funds rate reduction of 1.93 percentage points (after 8 quarters). In the later period, the substitution of the shadow rate for the funds rate leads to a higher offset of tax policy – a tax cut leads to an increase in the funds rate of 0.56 percentage points after 8 quarters. In short, in response to a standardized tax cut, the funds rate fell by about 2.5 percentage points more in the first period than in the second period. This effect is also highly statistically significant (Table 5).

B. The Role of Monetary Policy in the Changing GDP Response to Taxes

Ideally, we would like to determine the share of the changing GDP response that can be explained by changes in monetary policy. This is an inherently difficult task, however, for several reasons. It is unclear which horizon of estimates from Figure 9B is the most appropriate to employ and how permanent a funds rate response should be considered. In addition, there are different estimates of the impact a policy rate change has on the economy. In light of these issues, we adopt a simple and transparent approach, acknowledging the uncertainties involved. We use the maximum funds rate difference in the two periods – 2.5 percentage points and provide two (very rough) estimates, based on studies that provide broad perspectives on the literature.

Carpenter et al. (2025) employ several leading models and find that a surprise 1 percentage point increase in the funds rate leads to a peak GDP decline of 0.4% after 6-8 quarters. [16] Combining this estimate with our peak 2.5 pp difference in the federal funds rate

[16] Cloyne and Hürtgen (2016) finds that, in the U.K., a 1 percentage point contractionary shock to the policy target rate leads to a peak decline of 0.6% in IIP and 0.5% of GDP.

response to taxes over time implies that the time-varying monetary policy response to tax cuts can explain 1.0 pp – or just over a quarter – of the 3.6 pp difference in the GDP response.

Alternatively, Coibion (2012) surveys and reconciles three distinct approaches in the literature. His central estimates (page 4) imply that a 1 percentage point rise in the funds rate reduces the index of industrial production (IIP) by 2–3%.[17] Industrial production, of course, is more variable than GDP over the business cycle. We estimate that, after detrending, the standard deviation of IIP is about 2.5 times as large as GDP in the 1985-2019 period.[18] If so, then we can roughly approximate that Coibion's (2012) 2-3% effect on IIP implies approximately a 0.83% to 1.25% effect on GDP. This suggests that the difference in the federal funds rate responses to tax cuts in the two periods – 2.5 pp – would reduce the GDP response to tax changes by between 2.1-3.75 pp in the 1985-2019 period. Under this estimate, changes in the federal funds rate response to tax cuts can explain a sizable share – from more than half to more than 100% – of the 3.6% peak difference in the GDP response to taxes reported in Figure 3.

Taken together, the estimates based on Coibion (2012) and Carpenter et al. (2025) are consistent with the argument that shifts in monetary policy play a significant role in the changing GDP response to tax cuts, though it is difficult to pin down a precise effect.[19]

C. 10-year Treasury Rates

[17] Coibion also finds that a 1 pp increase in the policy rate raises unemployment by about 0.5 percentage points. Combined with our 2.5 pp difference, this would imply that unemployment would vary by 1.25 pp over the two time periods, which is similar to our 1.01 pp estimate based on Figure 7.

[18] IIP is published monthly and in index points, while real GDP is quarterly and in billions of chained dollars. To ensure comparability, IIP is converted to quarterly data, and both series are placed on a log scale. We apply the Hodrick-Prescott (HP) filter (using the standard quarterly parameter) and extract the cyclical component. We then compute the standard deviations of the cyclical components of log real GDP and log IIP, with relative volatility measured as the ratio of the IIP standard deviation to the GDP standard deviation.

[19] We are unable to address the Nakamura and Steinsson (2018) critique that funds rate announcements include not only an indication of policy but also information about what the Fed believes about the strength of the economy.

The 10-year Treasury yield is the benchmark long-term, risk-free interest rate in the United States. Many borrowing rates relevant for investment – such as those on corporate bonds or commercial mortgages – tend to move closely with the 10-year Treasury. Given the time-varying effects of tax cuts on investment as noted above, it is natural to examine the time-varying impact of tax cuts on the 10-year Treasury rate.[20] Figure 8 plots the time path of the 10-year Treasury rate.

Figure 10 shows the results of estimating equation (3) using the change in the 10-year Treasury rate as the dependent variable. The Figure reveals that while the differences over time are not as substantial as the funds rate, the general pattern for the 10-year Treasury rate is consistent with the funds rate analysis. In the earlier period, a tax cut leads to a substantial reduction in the 10-year rate – approaching 1 percentage point – over the third to eighth quarters, a response that amplifies the effect of the tax cut. In the later period, the 10-year rate shows little response to tax cuts, implying that the previous amplification is not present. These differences are statistically significant at a 10% level under the first null but not under the second (Table 6).

VII. Conclusion

We revisit the long-standing question of how tax cuts affect economic growth. We extend the Romer and Romer (2010) sample by twelve years and categorize tax changes in the interim period using narrative techniques. We show that extending the sample to 2019 preserves the main result in Romer and Romer (2010) that exogenous tax cuts raise economic growth.

We argue that 1985 is a natural point at which to search for time-varying effects, since it

[20] The 10-year Treasury constant maturity yield is sourced from the Romer-Romer dataset (CM10 column), which extends FRED series GS10 back to 1947Q1 using rates on Treasury bonds with maturities over ten years. This recovers the full pre-1985 sample of 1950Q1–1984Q4 (N=140). Data sources: FRED GS10, http://fred.stlouisfed.org/series/GS10, downloaded 06/02/26; observations for 1947-I-1953-I from rates on Treasury bonds with maturities over ten years, as recorded in Romer-Romer (2010) Excel Sheet 2 (CM10 column).

coincides with a series of durable changes to the economy that could plausibly affect the impact of tax cuts. These changes include the onset of the Great Moderation, a change in the focus of Federal Reserve policy, and the indexing of the income tax for inflation.

Our central result is that although tax cuts had substantial effects on growth in the 1950-1984 period, they had negligible effects between 1985 and 2019. Private economic activity (defined as GDP minus government purchases) especially did not respond positively to tax cuts in the latter period. These findings are robust to several sensitivity tests. Moreover, time-varying effects of tax cuts on major components of GDP, federal revenues, and the unemployment rate are consistent with the GDP effects.

We also show that changes over time in monetary policy – specifically, in how the federal funds rate responds to tax changes – are qualitatively consistent with the GDP changes and statistically significant. Rough calculations suggest that changes in monetary policy can explain a significant share of the time-varying response of GDP to tax cuts, but the range of plausible estimates is quite large – between 25% and more than 100% – and the estimates themselves are uncertain.

These results generate important policy implications. First, and most obviously, the claim that tax cuts raise growth is not universally true and depends, at the very least, on monetary policy and potentially on a host of other factors. Historical evidence on multipliers that does not consider these issues may prove to be an unreliable guide for current or future policy debates. Second, as policymakers search for ways to reduce the federal government's long-term fiscal shortfall, they should be aware that tax cuts can help in that regard without jeopardizing economic growth, under certain circumstances.

The analysis here leaves an ambitious agenda for future research. From a methodological perspective, supplementing the analysis above with vector autoregression (VAR) or structural

VARs could help reveal richer relationships between the variables. The key substantive issue is understanding the source(s) of the changing GDP response to taxes. We have identified monetary policy as one factor, but there are likely to be others. For example, the results for consumption and investment suggest that changes in the evolution of credit markets or borrowing constraints could be a factor. Societal aging (Ferraro and Fiori 2020) or higher government debt (including higher public awareness of government debt) could dampen the stimulative effect of tax cuts. The nature of tax changes themselves could also have changed – either in terms of which groups they affect or the extent to which they reduce average versus marginal tax rates (Mertens and Montiel Olea 2018 and Zidar 2019). Individuals' and firms' responsiveness to tax rules may have changed as well. In particular, the substantial and lasting reduction in the highest marginal tax rates introduced in the Tax Reform Act of 1986 could have muted the impact of further changes for high-income groups. All these hypotheses are worth exploring in future work, although some of them would be hard-pressed to explain a sharp change that takes effect in 1985 as opposed to a gradual shift over time.

One factor that could help explain the abrupt shift in 1985 is the indexing of the income tax for inflation, beginning in that year. Indexing creates automatic annual tax cuts relative to the unindexed system that existed before 1985. These cuts are exogenous in the Romer and Romer (2010) sense – they are legislated tax cuts that are not designed in response to current economic conditions or new spending. Moreover, they are anticipated – and there is evidence that anticipated tax cuts reduce economic activity prior to their implementation (Appendix C). And they can be substantial.[21] Thus, indexing could plausibly affect the impact of newly legislated tax

[21] The Joint Committee on Taxation (1984) estimated that indexing would reduce annual revenues by $52.8 billion in fiscal year 1989, or just under 1% of GDP, a figure that is substantially larger than the average exogenous tax cut shown in Table 1 of this paper. See also Garcia-Miralles et al. (2025), who find a revenue elasticity of 1.8 with respect to income growth in European income tax systems, which are similar in key respects to the American system. Auerbach and Feenberg (2000) discuss several matters related to indexing.

cuts. Nevertheless, Romer and Romer (2010) do not include the automatic annual tax cuts generated by indexing in their construction of exogenous tax changes, and we have not estimated them here. Explicitly considering the role of indexing in explaining the results in this paper seems like a high priority for future research.

Another issue is the relation between tax policy and the Great Moderation. Previous research attributes the Great Moderation to a variety of factors, including changes in inventory investment (e.g., McConnell and Perez-Quiros 2000) and other structural factors, fewer and less disruptive economic shocks (e.g., Stock and Watson 2002, Gordon 2005), and better monetary policy (e.g., Bernanke 2004). There has been little attention given to the role of tax policy in general – and inflation indexing in particular – in bringing about or supporting the Great Moderation, despite the similar timing of inflation indexing and reduction in volatility of macroeconomic aggregates. One exception is Keinsley (2016), who notes that indexing the income tax for inflation could reduce the negative impact of supply shocks. For example, if an oil price increase led to general inflation, the economy would suffer two short-term hits (higher oil prices and higher taxes) if the income tax were not indexed, but only one if the income tax were indexed.

All these issues should be high priority for researchers, in light of policymakers' continuing interest in tax policy and, in particular, in the need to think seriously about how to resolve the nation's fiscal shortfalls.

Table 1

Summary Statistics: Exogenous Tax Changes

	1950–2019	1950–1984	1985–2019
# of exogenous tax changes	46	29	17
# of tax cuts	23	18	5
Average cut (percent of GDP)	0.525	0.454	0.777
# of tax increases	23	11	12
Average increase (percent of GDP)	0.271	0.256	0.283

Source: Romer and Romer (2009), and authors' calculations (Appendix A of this paper).

Notes: The Table reports the frequency and magnitude of exogenous tax changes.

Table 2

Robustness with respect to number of lags

for tests of no change in the tax impulse response post-1984: GDP

	Hypothesis A: Interaction Coefficients Jointly Zero ($\beta i = 0\ \forall i$)			**Hypothesis B: Sum of Interaction Coefficients Zero ($\Sigma \beta i = 0$)**		
Lags	FGLS	Wild bootstrap	Newey-West	FGLS	Wild bootstrap	Newey-West
0–7	0.041**	0.023**	0.029**	0.090*	0.094*	0.014**
0–8	0.064*	0.034**	0.034**	0.099*	0.096*	0.019**
0–9	0.041**	0.018**	0.013**	0.035**	0.030**	0.005***
0–10	0.048**	0.015**	0.009***	0.083*	0.064*	0.019**
0–11	0.065*	0.021**	0.011**	0.136	0.102	0.035**
0–12	0.089*	0.036**	0.006***	0.157	0.129	0.074*

Source: Authors' calculations

Notes: The table reports p-values for tests of no change in the tax impulse response post-1984, corresponding to Figure 3, across alternative lag specifications. "Lags" denotes the range of TIME × tax interaction coefficients included in the test. Hypothesis A tests whether the TIME × tax coefficients are jointly zero, while Hypothesis B tests whether they sum to zero. "FGLS" is Feasible Generalized Least Squares (Wooldridge 2010). "Wild bootstrap" is the restricted wild bootstrap described in the text, with 9,999 draws. The final column presents results using Newey-West standard errors. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 3

9-Quarter Effects (t-statistics) for various breakpoints

measuring the changing effect of exogenous tax changes on GDP

Early period		**Later period**	
Years	9-quarter effect (t-statistic)	Years	9-quarter effect (t-statistic)
1950-1980	3.54 (2.44)	1981-2019	2.57 (1.54)
1950-1981	3.99 (2.88)	1982-2019	1.91 (1.06)
1950-1982	3.76 (2.39)	1983-2019	1.69 (0.82)
1950-1983	3.40 (2.53)	1984-2019	0.72 (0.30)
1950-1984	3.65 (3.07)	1985-2019	0.07 (0.03)
1950-1985	3.65 (3.09)	1986-2019	-0.02 (0.01)
1950-1986	3.58 (3.06)	1987-2019	0.04 (0.02)
1950-1987	3.47 (2.99)	1988-2019	0.11 (0.04)
1950-1988	3.44 (2.98)	1989-2019	0.15 (0.06)

Source: Authors' calculations

Notes: Each row reports the 9-quarter cumulative effect of an exogenous tax change on real GDP (with t-stats in parentheses) for the early and later periods as defined by the denoted breakpoint. Effects are estimated per equation (2), with the full model including the current value and 12 lagged values of exogenous tax changes, 11 lags of GDP growth, a post-break dummy (TIME), and the interactions between TIME and the tax variables. The early-period effect is the baseline IRF evaluated at h = 9; the later-period effect adds the TIME interaction coefficients. The fifth row (1950-1984 / 1985-2019) corresponds to the baseline specification in Figure 3.

Table 4

Estimated effects of exogenous tax changes and tests of the null hypothesis of no change in the tax impulse response post-1984: Components of GDP

Component	Avg. Share of GDP, 1985–2019 (%)	Peak Difference, Two Periods (pp)	9-quarter Difference, Two Periods (pp)	Implied Difference in GDP Response, 9 Quarters (pp)	Hypothesis A: Interaction Coefficients Jointly Zero ($\beta i = 0\ \forall i$)		Hypothesis B: Sum of Interaction Coefficients Zero ($\Sigma \beta i = 0$)	
					Wild bootstrap	Newey-West	Wild bootstrap	Newey-West
Nondurable Consumption + Services	58.0	−1.98	-1.82	−1.06	0.051*	0.00***	0.326	0.232
Durable Consumption	8.1	−14.36	-14.36	−1.16	0.003***	0.001***	0.310	0.243
Nonresidential Investment	12.9	−12.26	-12.26	−1.58	0.052*	0.053*	0.114	0.102
Residential Investment	4.3	−8.37	-8.37	−0.36	0.786	0.306	0.913	0.908
Net Exports	−2.9	1.96	1.09	1.09	0.279	0.033**	0.912	0.912
Government Purchases	19.3	3.16	3.16	0.61	0.018**	0.001***	0.420	0.416

Source: Authors' calculations.

Notes: The first column reports the component's share of GDP from 1985 to 2019 (US. Bureau of Economic Analysis (2026), Table 1.1.5). The second column reports the difference in peak interaction coefficients of the regression based on equation (3) that uses the quarterly log change in the component/GDP as the dependent variable (except for net exports, where the dependent variable is the quarterly change in net exports/GDP). The third column reports the 9-quarter difference in the interaction coefficients of the regression based on equation (3). The fourth column multiplies the first and third column to obtain an approximation of the peak difference as a share of GDP (except for net exports, which is already reported as a share of GDP). The IRF is propagated through tax changes and own-component lags only. The p-values are reported for the wild bootstrap and Newey-West procedures. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 5

Tests of the null hypothesis of no change in the tax impulse response post-1984:

Revenues, Unemployment, and Interest Rates

	Hypothesis A: Interaction Coefficients Jointly Zero (βi = 0 ∀i)		**Hypothesis B: Sum of Interaction Coefficients Zero (Σβi = 0)**	
Dependent Variable (Quarterly Change)	Wild bootstrap	Newey-West	Wild bootstrap	Newey-West
Federal Revenues/GDP	0.065*	0.031**	0.524	0.542
Unemployment Rate	0.379	0.481	0.268	0.231
Federal Funds Rate (FFR)	0.004***	0.247	0.035**	0.04**
Spliced FFR/Wu-Xia shadow rate	0.006***	0.212	0.023**	0.072*
10-year Treasury yield	0.059*	0.071*	0.766	0.718

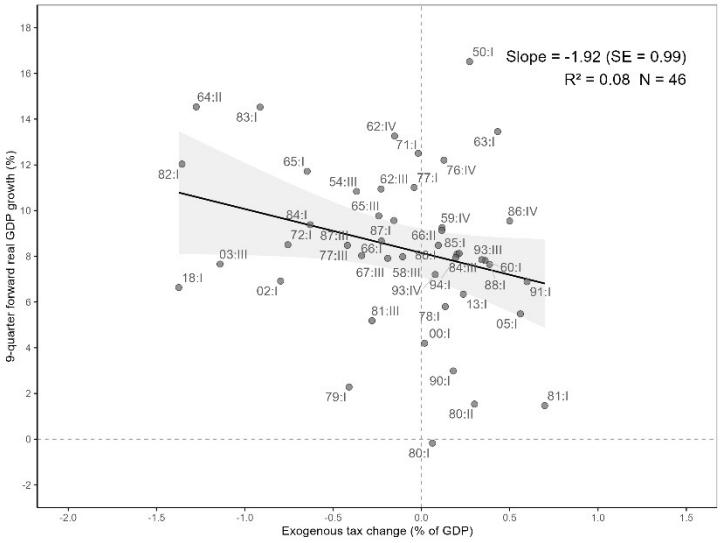
Source: Authors' calculations

Notes: The table reports p-values for the null hypothesis that the response to tax changes of federal revenues/GDP, the unemployment rate, the federal funds rate, a hybrid federal funds rate and shadow rate series, or the 10-year Treasury rate does not vary across the pre-1985 and post-1984 subsamples. The OLS regressions in Figures 6, 7, 9A, 9B, and 10 use the fully interacted distributed-lag regression in equation (3) in the text: $\beta_i = 0 \forall i$ is the joint test that all thirteen post-1984 interaction coefficients are zero; $\sum_{i=0}^{12} \beta_i = 0$ is the single restriction that their sum is zero. "Wild bootstrap" is the restricted wild bootstrap described in the text, with 9,999 draws. Newey-West standard errors are also presented, for reasons described in the text. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

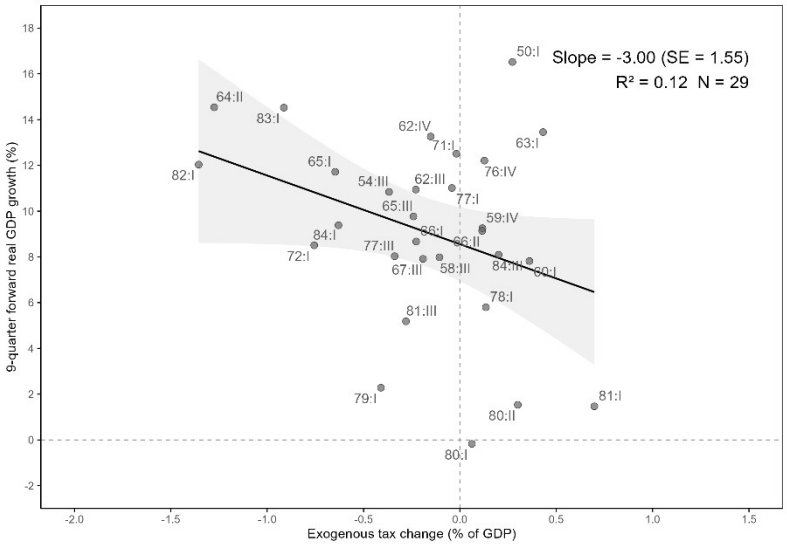
Figure 1

Exogenous Tax Changes and 9-quarter GDP Growth

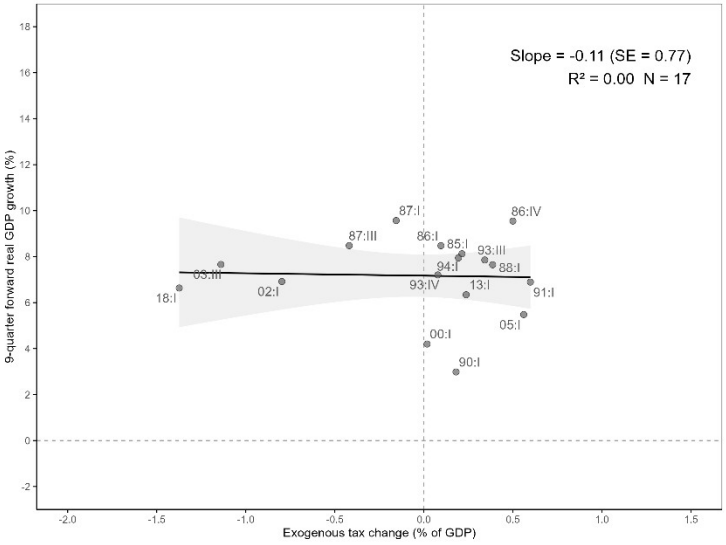
Panel A: 1950-2019



Panel B: 1950-1984



Panel C: 1985-2019

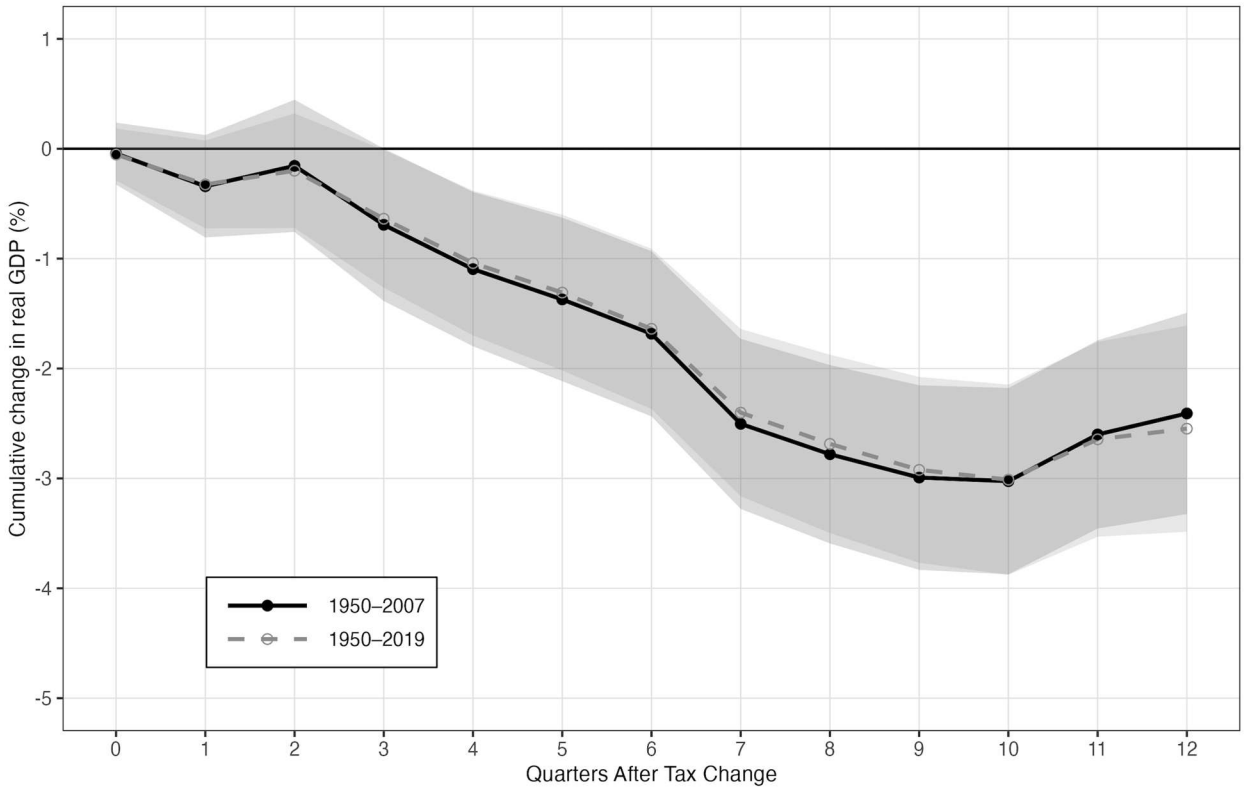


Source: Authors' calculations

Notes: The Figures plot the 9-quarter cumulative growth rate in GDP following exogenous tax changes against the size and direction of those exogenous tax changes over the 1950-2019, 1950-1984, and 1985-2019 sample periods. N=46, N=29, and N=17, respectively. To avoid attributing COVID-related GDP declines in 2020:I and 2020:II to TCJA, we replace actual GDP growth in both quarters with the average growth rate in the last two quarters of 2019.

Figure 2

Effects of Exogenous Tax Changes on Real GDP, 1950-2007 and 1950-2019

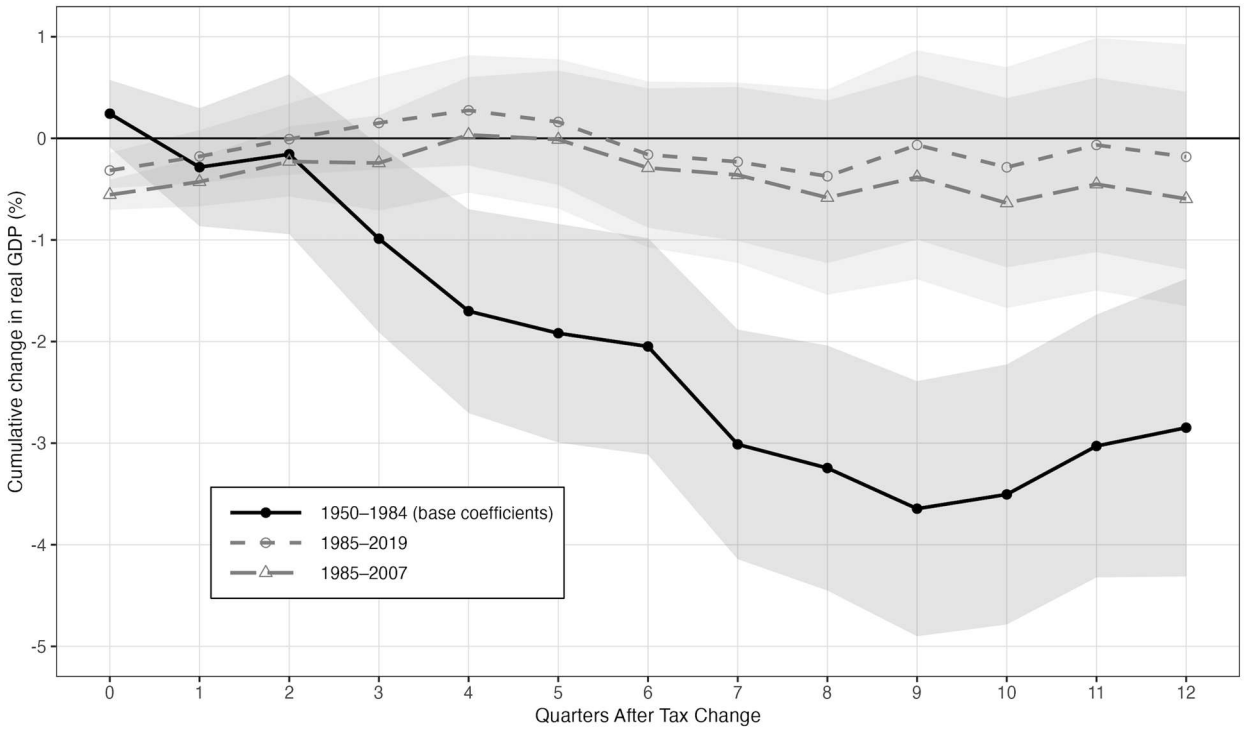


Source: Authors' calculations

Notes: The Figure reports the results of estimating equation (1) over different sample periods. The dependent variable is the quarterly growth rate of real GDP, the independent variables are current and 12 lagged values of exogenous tax changes and 11 lags of the dependent variable. N=232 (1950-2007) and N = 280 (1950–2019). The shaded bands show ±1 standard error computed via the unrestricted wild bootstrap with 9,999 Rademacher replications.

Figure 3

The Changing Effect of Exogenous Tax Changes on GDP, 1950-1984 and 1985-2007 and 1985-2019

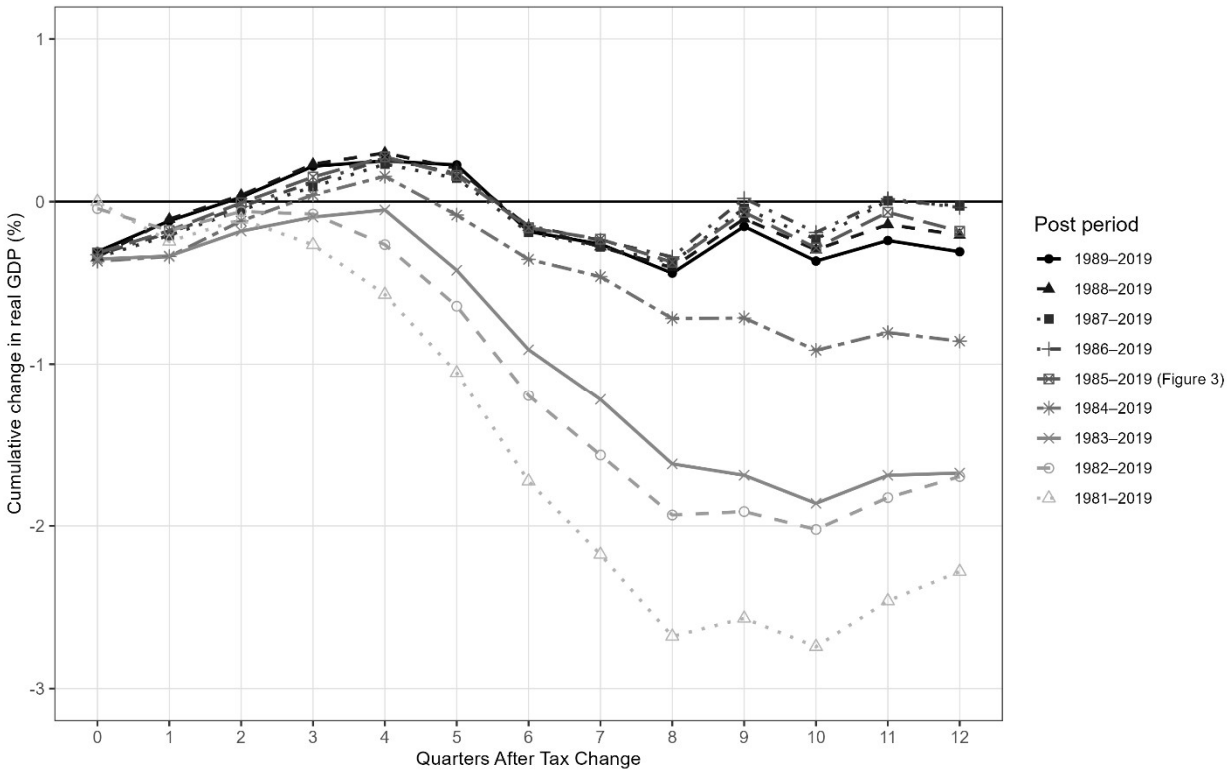


Source: Authors' calculations

Notes: The Figure reports the results of estimating equation (2) twice: once for 1950–2019 and once for 1950–2007. The dependent variable is the quarterly growth rate of GDP, the independent variables are current and 12 lagged values of exogenous tax changes (ETCs), 11 lags of the dependent variable, a post-1984 dummy (TIME = 1 if 1985–2019 and 1985–2007, respectively), and the interaction between TIME and ETCs. N = 280 (1950–2019 estimation) and N = 232 (1950–2007 estimation). Because the pre-1984 base coefficients are numerically identical across both estimations, a single 1950-1984 line is shown. The shaded bands show ±1 standard error computed via the unrestricted wild bootstrap with 9,999 Rademacher replications.

Figure 4

Effects of Exogenous Tax Changes on GDP: Second period effects only, across various start dates



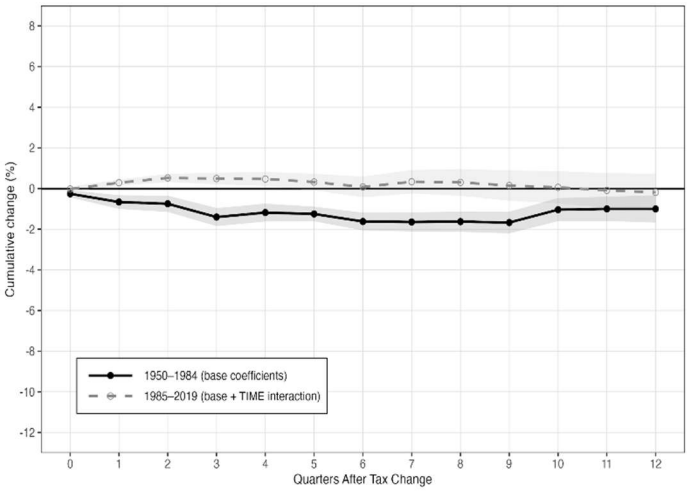
Source: Authors' calculations

Notes: The Figure reports the post-period impulse response functions from estimating equation (2) for 1950-2019, varying the start date of the post-period. Each line displays the cumulative response of real GDP (%) to a standardized exogenous tax change in the indicated post-period, with the pre-period always ending one quarter before the post-period begins. The dependent variable is the quarterly growth rate of GDP, the independent variables are current and 12 lagged values of exogenous tax changes (ETCs), 11 lags of the dependent variable, a post-1984 dummy (TIME= for the indicated post-period), and the interaction between TIME and ETCs. The line labelled "1985-2019 (Figure 3) corresponds to the baseline specification in Figure 3.

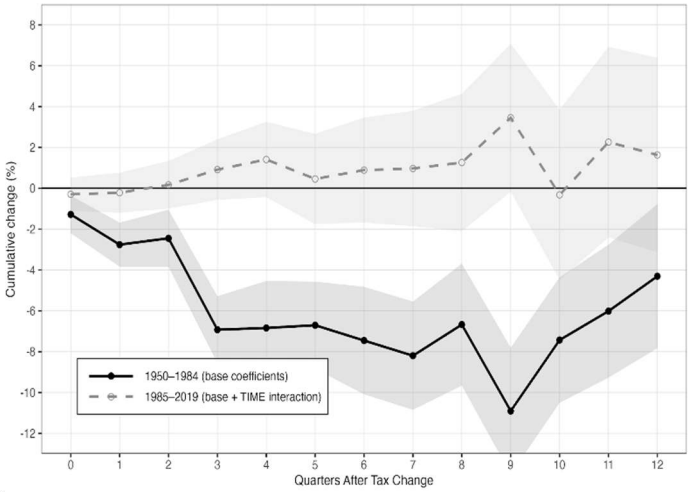
Figure 5

The Changing Effects of Exogenous Tax Changes on Components of GDP

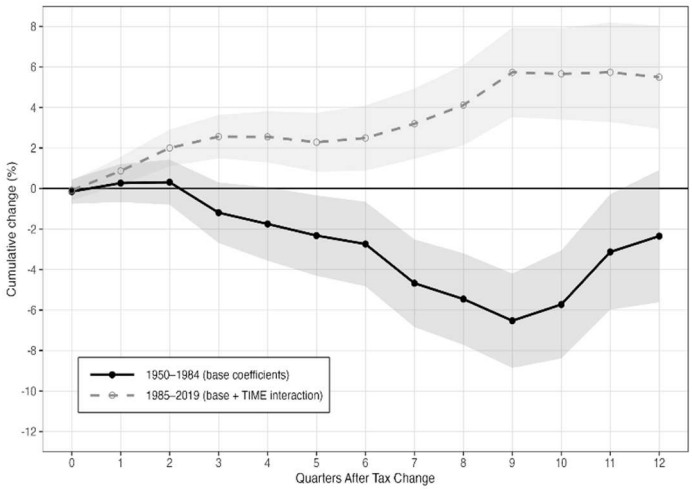
Panel A: Nondurable consumption and services



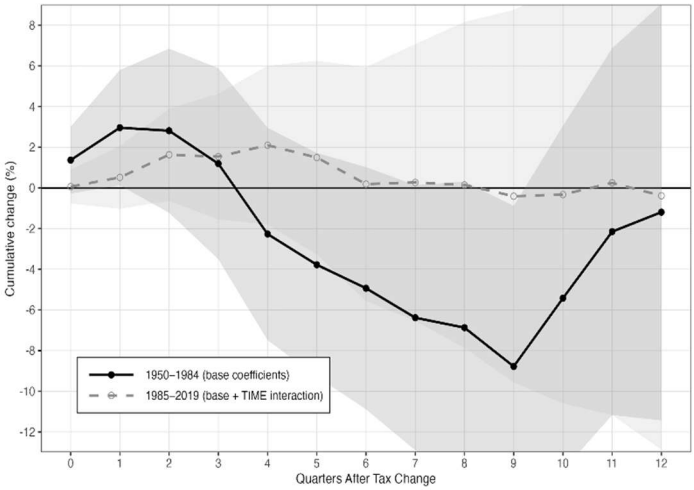
Panel B: Durable goods consumption



Panel C: Nonresidential investment



Panel D: Residential investment

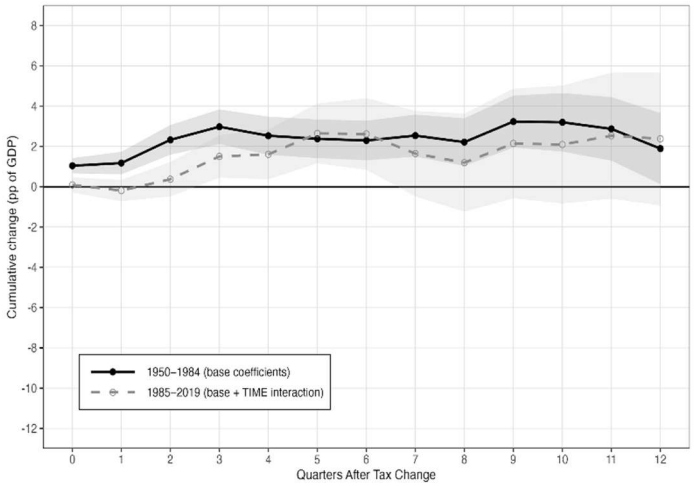


Source: Authors' calculations.

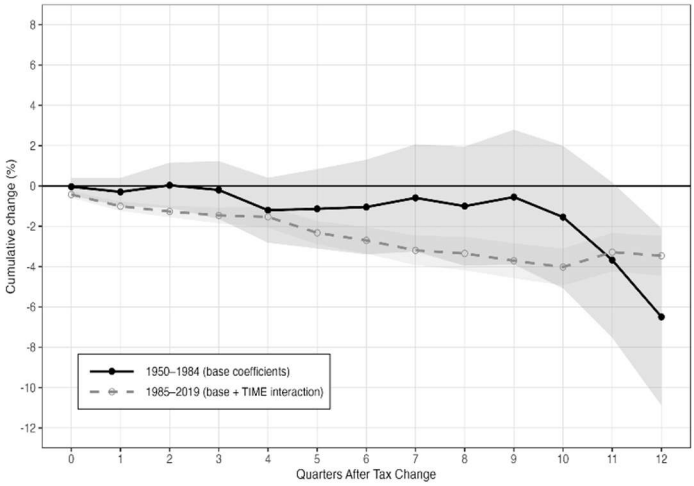
Notes: Figures 5A-5D show the results of regressing the quarterly change (log growth rate) for four components of GDP on current and 12 lagged values of exogenous tax changes, 11 lags of the dependent variable and real GDP growth, a post-1984 dummy (TIME= 1 if 1985–2019), and full interaction of the TIME dummy with other variables. The impulse response function (IRF) is based on the coefficients of current and lagged exogenous tax changes and the lagged dependent variable, as well as the sums of those coefficients and their interactions with the TIME dummy for 1985-2019. The shaded bands show ±1 standard error computed via the unrestricted wild bootstrap with 9,999 Rademacher replications.

Figure 5 (cont.)

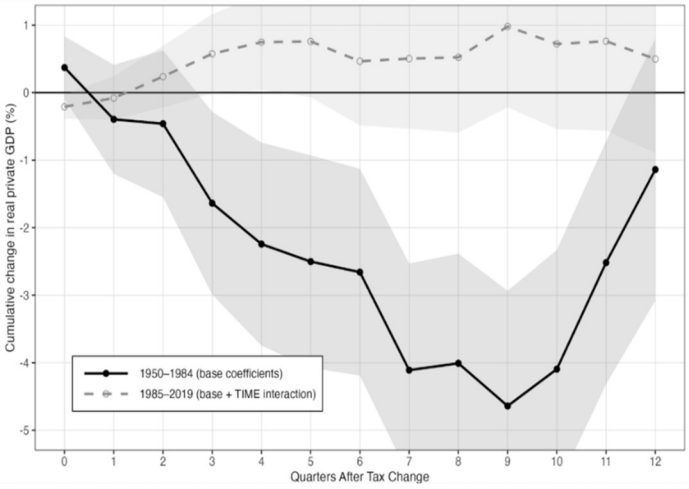
Panel E: Net exports



Panel F: Government purchases



Panel G: Private Economic Activity (GDP – Government Purchases)

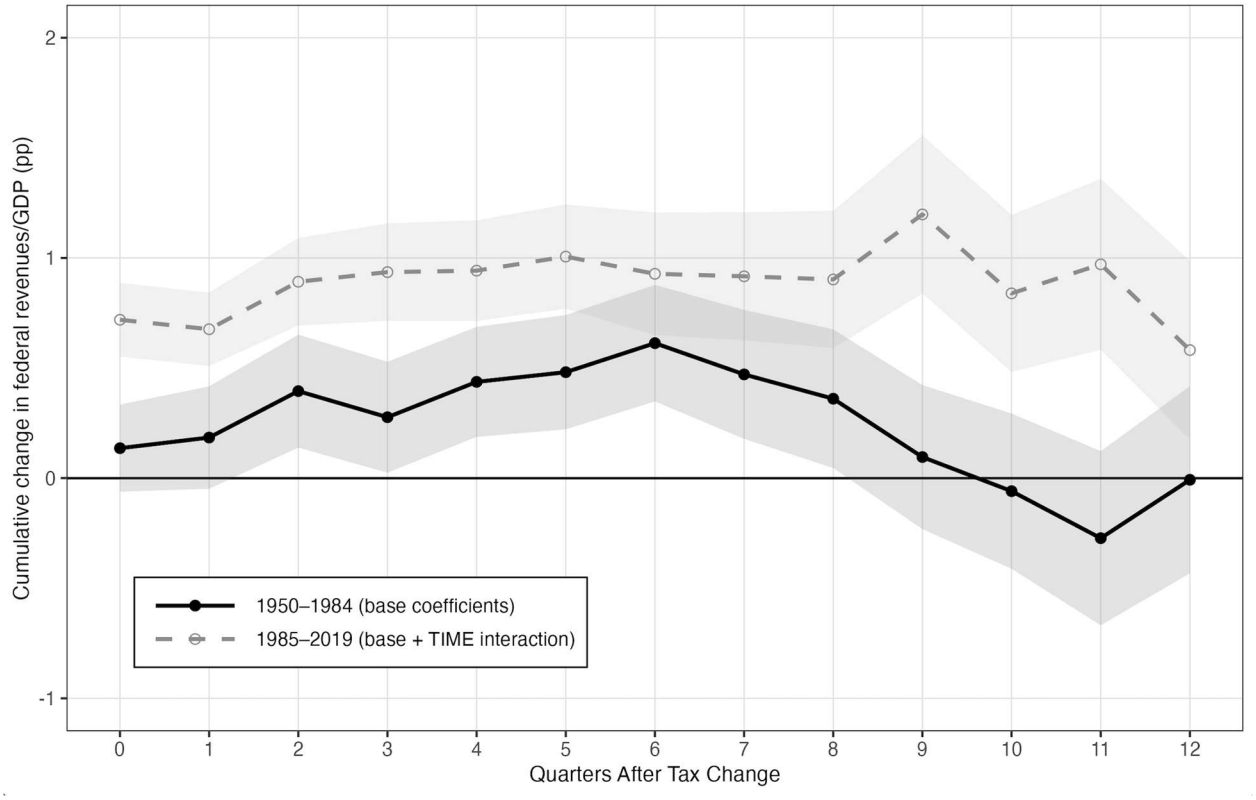


Source: Authors' calculations.

Notes: Figures 5E and 5F show the results of regressing change in net exports as a share of GDP and quarterly change in the log of government purchases (log growth rate), respectively, on current and 12 lagged values of exogenous tax changes, 11 lags of the dependent variable and real GDP growth, a post-1984 dummy (TIME= 1 if 1985–2019), and full interaction of the TIME dummy with other variables. Figure 5G reports the results of estimating equation (2) for 1950-2019, with the dependent variable as the quarterly growth rate of real private economic activity (= GDP - government purchases). The IRF specifications and shaded bands follow from the notes for Figures 5A-5D.

Figure 6

The Changing Effects of Exogenous Tax Changes on Federal Revenues

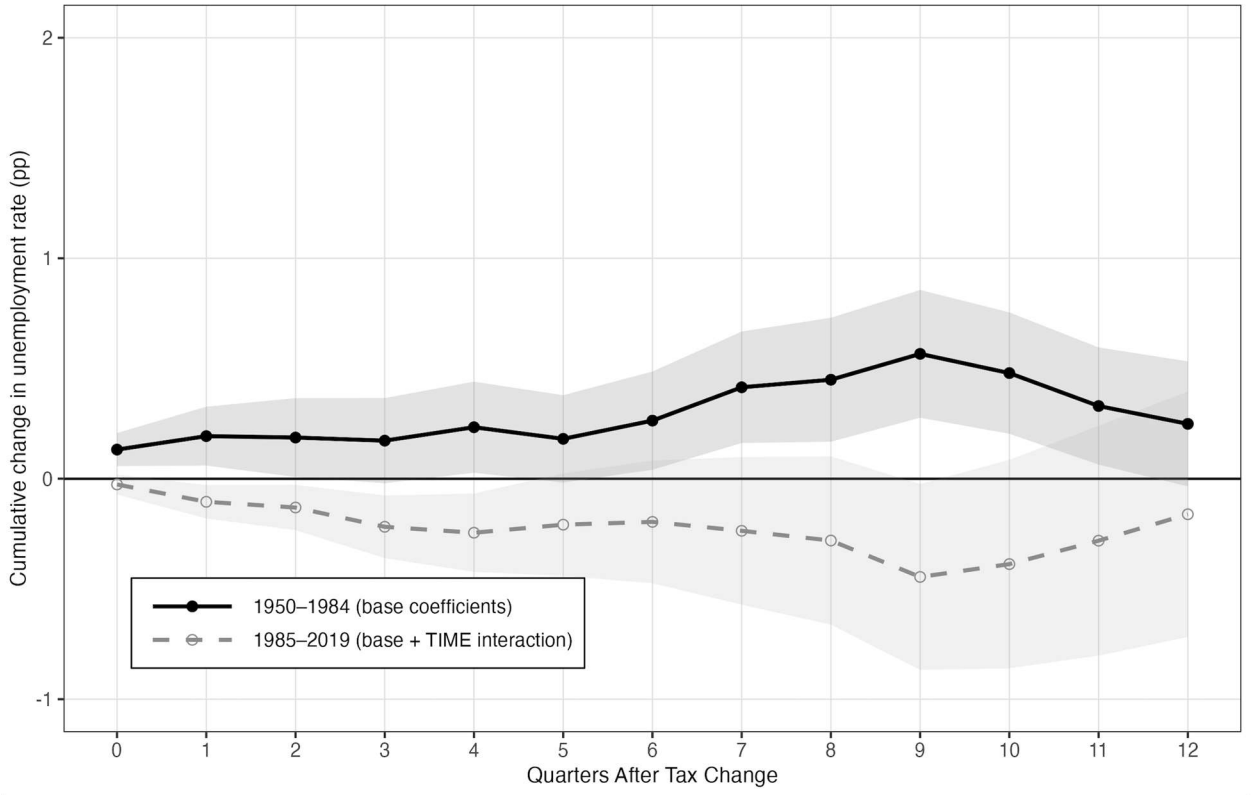


Source: Authors' calculations.

Notes: The Figure shows the results of regressing the quarterly change in the ratio of federal revenues as a share of GDP on current and 12 lagged values of exogenous tax changes, 11 lags of the dependent variable and real GDP growth, a post-1984 dummy (TIME= 1 if 1985–2019), and full interaction of the TIME dummy with other variables. The impulse response function (IRF) is based on the coefficients of current and lagged exogenous tax changes and the lagged dependent variable, as well as the sums of those coefficients and their interactions with the TIME dummy for 1985-2019. The shaded bands show ±1 standard error computed via the unrestricted wild bootstrap with 9,999 Rademacher replications.

Figure 7

The Changing Effect of Exogenous Tax Changes on the Unemployment Rate

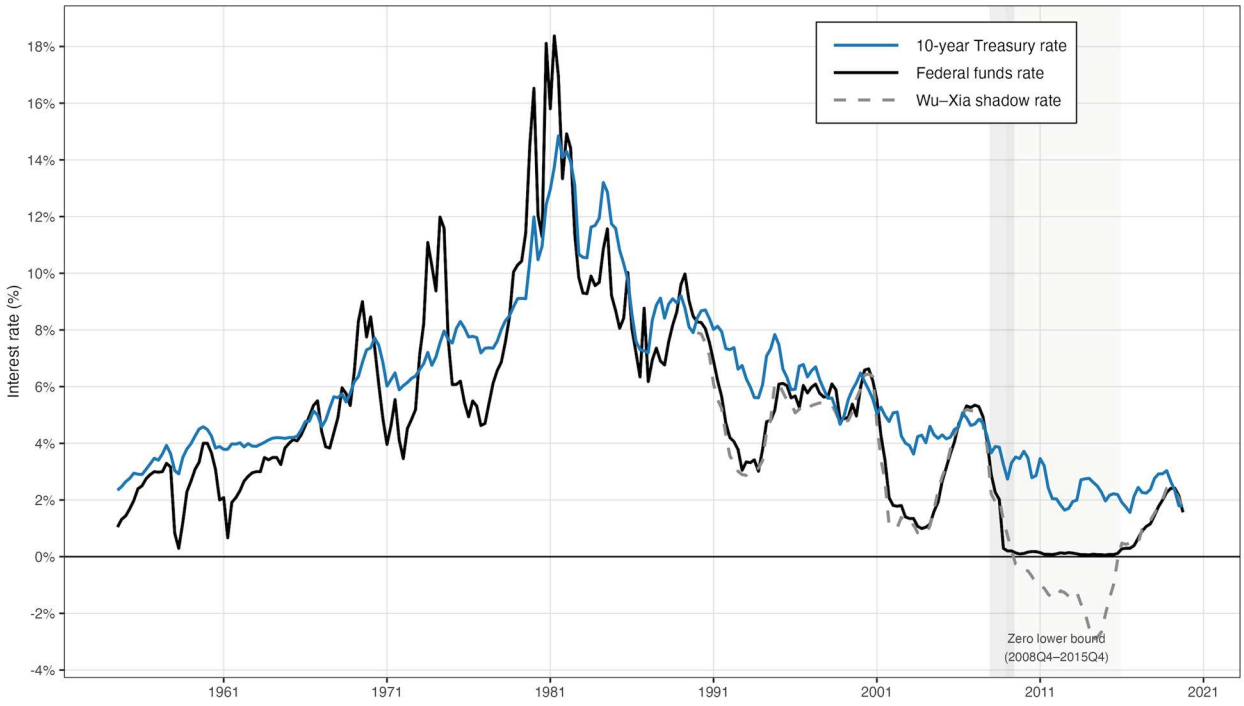


Source: Authors' calculations.

Notes: The Figure shows the results of regressing the quarterly change in the unemployment rate on current and 12 lagged values of exogenous tax changes, 11 lags of the dependent variable and real GDP growth, a post-1984 dummy (TIME= 1 if 1985–2019), and full interaction of the TIME dummy with other variables. The impulse response function (IRF) is based on the coefficients of current and lagged exogenous tax changes and the lagged dependent variable, as well as the sums of those coefficients and their interactions with the TIME dummy for 1985-2019. The shaded bands show ±1 standard error computed via the unrestricted wild bootstrap with 9,999 Rademacher replications.

Figure 8

Federal Funds Rate, Wu-Xia Shadow Rate, and 10-Year Treasury Rate

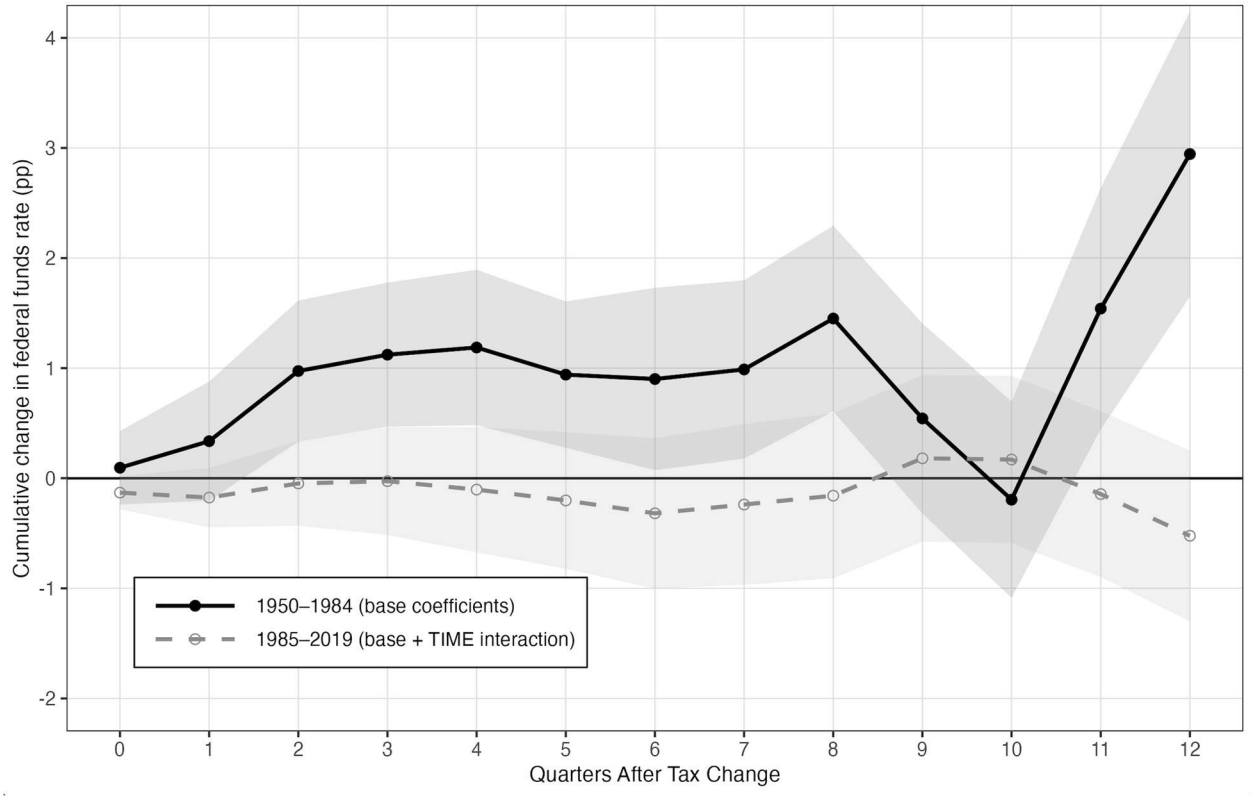


Source: Romer and Romer (2010), which extended the FRED DFF series; Wu-Xia (2016, 2022); Board of Governors of the Federal Reserve System (2026).

Notes: The Figure shows the evolution over time of the Federal Funds rate, a hybrid series that combines the Federal Funds Rate and the Wu-Xia (2016) shadow rate, and the 10-year Treasury yield.

Figure 9A

The Changing Effects of Exogenous Tax Changes on the Federal Funds Rate

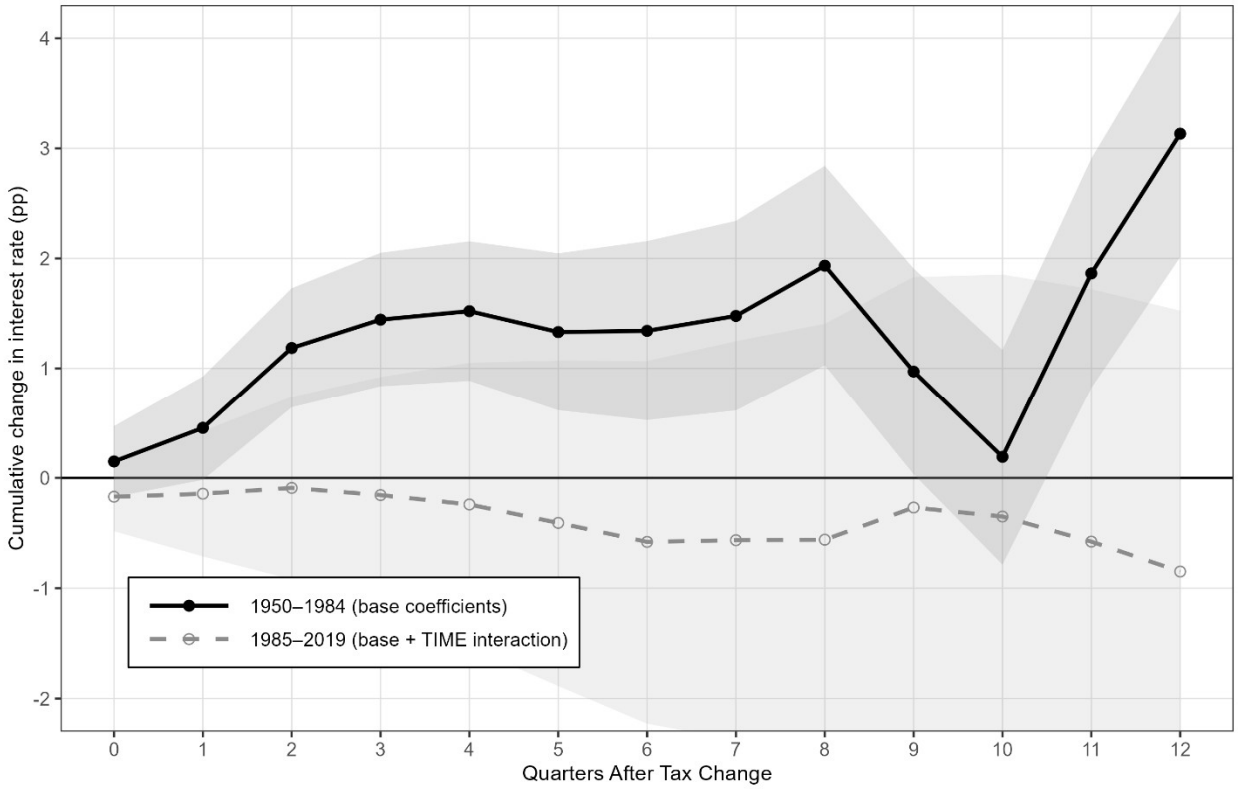


Source: Authors' calculations.

Notes: The Figure shows the results of regressing the quarterly change in the nominal federal funds rate (FFR) on current and 12 lagged values of exogenous tax changes, 11 lags of the dependent variable and real GDP growth, a post-1984 dummy (TIME= 1 if 1985–2019), and full interaction of the TIME dummy with other variables. The impulse response function (IRF) is based on the coefficients of current and lagged exogenous tax changes and the lagged dependent variable, as well as the sums of those coefficients and their interactions with the TIME dummy for 1985-2019. The p-value for the F-test that all the coefficients on the TIME*ETC variables are zero is 0.002. N=268. The shaded bands show ±1 standard error computed via the unrestricted wild bootstrap with 9,999 Rademacher replications.

Figure 9B

The Changing Effects of Exogenous Tax Changes on the Federal Funds Rate and Shadow Rate (with Volcker Indicator)

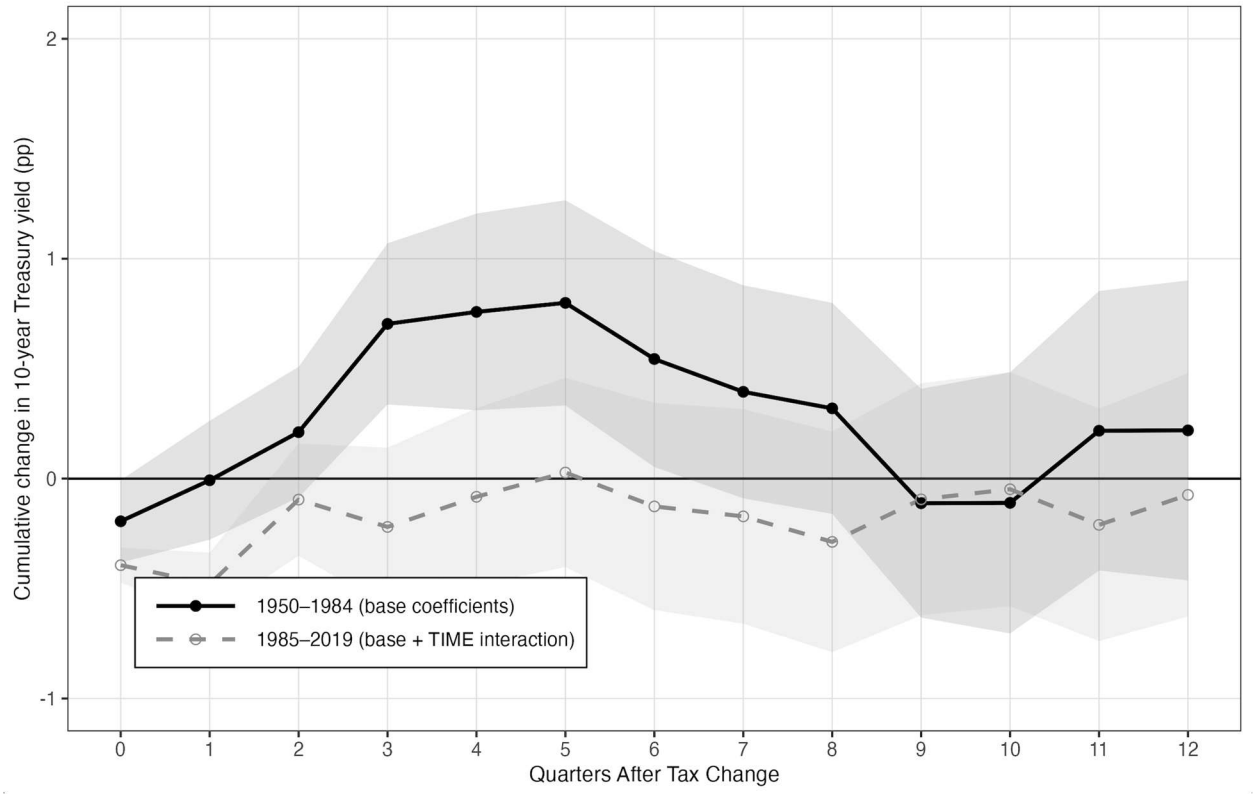


Source: Authors' calculations.

Notes: The Figure shows the results of regressing the quarterly change in the nominal federal funds rate (FFR), replaced by the Wu-Xia shadow rate for 1990-2019, on current and 12 lagged values of exogenous tax changes, 11 lags of the dependent variable and real GDP growth, a post-1984 dummy (TIME= 1 if 1985–2019), full interaction of the TIME dummy with other variables, and a Volcker indicator variable that equals 1 from 1979:III to 1984:IV to account for the unusual turbulence of monetary and fiscal policy and outside economic events during that period. The impulse response function (IRF) is based on the coefficients of current and lagged exogenous tax changes and the lagged dependent variable, as well as the sums of those coefficients and their interactions with the TIME dummy for 1985-2019. The Volcker indicator serves only as a control and does not directly affect the IRF. N=268. The shaded bands show ±1 standard error computed via the unrestricted wild bootstrap with 9,999 Rademacher replications.

Figure 10

The Changing Effects of Exogenous Tax Changes on the 10-Year Treasury Rate



Source: Authors' calculations.

Notes: The Figure shows the results of regressing the quarterly change in the 10-year Treasury rate on current and 12 lagged values of exogenous tax changes, 11 lags of the dependent variable and real GDP growth, a post-1984 dummy (TIME= 1 if 1985–2019), and full interaction of the TIME dummy with other variables. The impulse response function (IRF) is based on the coefficients of current and lagged exogenous tax changes and the lagged dependent variable, as well as the sums of those coefficients and their interactions with the TIME dummy for 1985-2019. N=280. The shaded bands show ±1 standard error computed via the unrestricted wild bootstrap with 9,999 Rademacher replications.

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