Did the Tax Cuts and Jobs Act Create Jobs and Stimulate Growth?

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Did the Tax Cuts and Jobs Act Create Jobs and Stimulate Growth?*

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Abstract

The Tax Cuts and Jobs Act (TCJA) of 2017 is the most extensive overhaul of the U.S. income tax code since the Tax Reform Act of 1986. Existing estimates of TCJA's economic impact are based on economic projections using pre-TCJA estimates of tax effects. I exploit plausibly exogenous state-level variation in tax changes from TCJA and find that an income tax cut equaling 1 percent of GDP led to a 1.2-percentage-point faster job growth and nearly 1.5 percentage points higher GDP growth over two years following the law change. While the estimates are imprecise, the overall pattern suggests that the TCJA stimulated economic growth. The estimates imply a two-year tax cut multiplier of 1.5 and a cost per job of $105,000. The estimated growth effect was driven by a nearly 1.3-percentage-point increase in the labor force participation rate.

Keywords: Taxes and Economic Growth, Tax Cuts and Jobs Act

JEL Classification: E62, H30

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1. Introduction

In the most extensive overhaul of the U.S. tax code since the Tax Reform Act (TRA) of 1986, the Tax Cuts and Jobs Act (TCJA) of 2017 made extensive changes to both individual income and corporate tax codes. The TCJA lowered tax rates and broadened most tax brackets. The top individual income tax rate fell from 39.6 percent to 37 percent and was applied to income over $600,000 for married filers—a higher threshold than $480,050 in 2017.[1] The new tax law also repealed personal and dependent exemptions, almost doubled the standard deduction, capped the itemized deductions for state and local taxes to $10,000, restricted mortgage interest deductibility, expanded the child tax credit, and considerably reduced the scope of the Alternative Minimum Tax (AMT).[2]

These individual income tax changes are set to expire after eight years, in 2025, unless extended by Congress. In addition to the individual income tax changes, the 2017 tax law cut the top corporate tax rate permanently from 35 percent to 21 percent and made far-reaching changes to the treatment of foreign source income and international financial flows.

Lower taxes are expected to positively affect the economy in the short term by boosting consumer spending on the demand side and by increasing labor force participation, hours worked, saving, and investment on the supply side. The economic stimulus from the TCJA is widely believed to have contributed to stronger economic activity in 2018.

[1] While these rate reductions were not as extensive as TRA-1986, particularly the top income tax rates, which fell from 50 to 28 percent after TRA-1986, the average marginal income tax rates still declined appreciably after TCJA. According to calculations available at https://taxsim.nber.org/marginal-tax-rates/index.html (accessed 5/14/2023), average federal marginal tax rates on wages declined from 25.8 percent in 1985 to 21.9 percent in 1988 after TRA-1986. In comparison, the average marginal tax rate declined from 22.7 percent in 2017 to 20.7 percent in 2019, following TCJA. Average federal tax rates fell 1.7 percentage points from 20.7 percent in 2017 to 19 percent in 2019, with practically all income groups receiving a cut in average tax rates (Joint Committee on Taxation, 2019).

[2] For more details, see The Tax Policy Center's Briefing Book, retrieved from https://www.taxpolicycenter.org/sites/default/files/briefing-book/bb_full_2018_1.pdf.

However, almost all existing estimates of the TCJA's effect on the economy are based on economic projections using pre-TCJA estimates of tax effects.[3] While more data in the post-TCJA period is needed to estimate the fully dynamic effects of the TCJA, following the recent pioneering work of Zidar (2019), the immediate short-term impact of the TCJA can be identified using spatial variation in tax changes.

In this paper, I exploit plausibly exogenous state-level variation in tax shocks and, using econometric specifications similar to the recent literature, estimate the impact of the TCJA-induced tax cuts on GDP and job growth in 2018 and beyond. Additionally, I delve into the mechanisms behind the stimulative effects of the tax cuts. My estimates would be credible only if the TCJA were an exogenous tax change, i.e., if it was uncorrelated with factors affecting current economic activity. A primary advantage of using state-level variation in tax changes is that it accounts for purely time-varying national confounders, such as monetary policy changes that may be endogenous to economic activity.

Nonetheless, state-level differences in tax changes may still be correlated with other factors likely also driving state-level economic growth. To mitigate this concern, I show that TCJA tax shock measures are uncorrelated with lagged economic growth. I use panel data on growth rates and tax shocks from 2014-2019 and estimate models with state fixed effects and year effects—equivalent to regressing the *change* in growth rate on the *change* in tax shock rather than using their *levels*. The empirical framework is similar in spirit to standard difference-in-differences designs with continuous treatment, comparing GDP and job growth in states with smaller TCJA tax shocks to those with larger tax shocks before vs. after the TCJA.

[3] See Mertens (2018) and Gale et. al. (2018) for a review of estimated effects of TCJA.

In the absence of individual income tax data, TCJA tax shocks are calculated using 2017 state-level statistics on tax returns from the Statistics of Income (SOI) division of the Internal Revenue Service (IRS) in combination with the NBER-TAXSIM model. Using these data, Figure 1 shows that the TCJA tax shocks, i.e., tax cuts as a percent of GDP, varied widely across states—from 0.3 percent of GDP in Oregon to 1.6 percent in Florida. It is evident from Figure 1 that the primary source of tax variation stems from limits on state and local income tax deductions.[4] Figure 2 shows that while the *change* in TCJA tax shock was uncorrelated with the *change* in job growth in 2017 (Panel A), the shocks shared a strong negative relationship with the *change* in state-level job growth in 2018 (Panel B). A similar pattern held for the tax shock's relationship with GDP growth (Panels C and D).

My main finding is that tax shocks equaling 1 percent of GDP led to around 1.3 percentage points faster job growth and 1.5 percentage points higher GDP growth—implying an estimated cost per job of $105,000 (in 2018 dollars) and a multiplier of around 1.5. While the point estimates lack statistical significance, the overall pattern of results suggests that the TCJA stimulated economic growth. Nonetheless, these implied multipliers from TCJA are towards the lower end of estimated tax multipliers from the previous literature. I also find that the TCJA's impact on growth was stronger in 2019, i.e., the year following the tax change, with smaller effects detected in 2018. A key contributor to the estimated growth effect was a rise in the labor force participation rate by

[4] To identify the main factors behind the variation, I performed a regression analysis of the tax shock measure on potential influencers that could explain post-TCJA tax changes. The regression yielded an R-square value of 0.61, where the dummy variable for a state having an income tax accounted for over 60% of it. The proportion of taxpayers itemizing taxes emerged as the next significant driver, contributing to 11% of the R-square. The combination of mortgage interest deductions, the child tax credit, and property taxes yielded a modest contribution of 22%. Differences in income distribution explained only about 3% of the R-square, while the qualified business income deduction accounted for a mere 1.6%. The R-square decomposition was conducted using Shorrocks-Shapely decomposition (Chavez Juarez, 2012; Shorrocks, 1982).

close to 1.3 percentage points. In contrast, the influence on consumer spending was relatively minor and lacked statistical significance.

Undertaking this research is worthwhile for several reasons. Firstly, utilizing actual variations induced by TCJA to estimate its impacts may offer a more precise measure than projections based on the assessed effects of prior tax reforms. Secondly, there is a substantial divergence in the tax multiplier estimates from the preceding literature, with recent figures ranging from less than 1 to as high as 3.5. Thirdly, the debate surrounding the timing of tax change impacts on growth remains unresolved. Previous studies examining the immediate impact of tax changes yield mixed results. Romer and Romer (2010) and Mertens and Ravn (2013) found evidence of impact starting in the first year of the tax change, whereas Zidar (2019) reported an insignificant effect in the first year.

And lastly, a central motivation for this paper is that all tax changes are not identical, and the type of tax reform can greatly influence the magnitude and duration of its economic impact. Zidar (2019) pooled multiple exogenous tax changes and assessed their impact using state-level data. However, tax changes vary significantly in their objectives. Some, like TRA 1986, aimed at revenue neutrality, while it is well-known that TCJA is deficit-financed. The economic growth impact of tax changes is fundamentally influenced by their financing method. Deficit-financed tax cuts tend to exert smaller long-term growth effects due to their investment crowding-out effect and the rise in interest rates. Estimates suggest that TCJA will inflate the deficit by $1-1.4 trillion over a decade.[5] This enlarged deficit could dampen the act's long-term growth effect, although short-term effects may persist.

[5] Joint Committee on Taxation (2017) projected that the individual income tax reforms will add an estimated $1.1 trillion to the deficit, while the business tax reforms will add another $650 billion. In contrast, international tax reforms will generate additional revenue of $324 billion.

The consequences of tax changes also hinge on whether they entail a reduction in tax rates or an expansion of the tax base. Like the TRA 1986, the TCJA both lowered tax rates and broadened the tax base, though TRA 1986 did so to a far greater extent. Nevertheless, as outlined in Slemrod (2018), while the TCJA lowered tax rates across the board, its base-broadening elements were far less extensive than those in TRA 1986.[6] According to Gale and Samwick (2017), while base broadening measures can reduce deficit impacts and enhance economic efficiency, they can also dampen the direct short-term stimulative effects on the economy. Therefore, the balance of these two components plays a critical role in the overall economic impact of such tax reforms.

This paper does not attempt to estimate the impact on long-term growth, as it is too premature to do so. Nevertheless, the short-term regression estimates presented in this paper do provide insight into the stimulative effects of tax changes. Previous research suggests that most of the effects of tax cuts materialize within two years of the tax change. Additionally, because the TCJA is deficit-financed, the increased deficit may cancel out some of the longer-term growth effects. According to Barro and Furman (2018), the long-term effects of TCJA are projected to be small—GDP would be just 0.2 percent higher in 2027 with the law as it stands and 1 percent higher if TCJA's temporary provisions are extended. Consequently, the short-term growth effects might serve as a useful upper bound for the law's long-term impact.

It's important to emphasize that this paper estimates the local multiplier. As noted in Chodorow-Reich (2019), this can differ from the national multiplier due to several factors. One of

[6] One significant base-broadening measure was capping itemized state and local income tax deductions at $10,000, coupled with the further devaluation of itemized deductions through the doubling of the standard deduction. According to the Joint Committee on Taxation (2017), rate reduction measures will cost the government $1.2 trillion over ten years, with the expanded standard deduction adding another $720 billion to this tally. However, the base-broadening features cannot fully offset the revenue lost due to rate-reducing measures. Among the major base-broadening measures, the repeal of personal exemptions will counterbalance $1.2 trillion of the deficit, and the new limits on itemized deductions will recover another $668 billion. Still, these base-broadening measures are not enough to completely counteract the cost of the tax cuts.

these is the absence of a monetary policy response at the state level. Another is local spending on products from other regions, influenced by higher prices and income. Lastly, the in-migration of workers in response to a stronger economy is another factor that contributes to this difference. Collectively, the influence of these factors results in the local multiplier serving as a lower bound estimate for the national multiplier.

The remainder of the paper is organized as follows. Section 2 lays out the econometric framework, section 3 describes the data used and TCJA tax shock calculations, section 4 discusses results, and section 5 concludes.

2. Econometric Framework

The econometric specification closely follows recent work on estimating multipliers using local projections (LP) regressions, which provides a simple way to track the evolution of the tax change impact before and/or after the change by estimating impulse responses at different time horizons.[7] For estimating the impact on growth at horizon h, the LP specification can be written as:

$$\Delta^h y_{st} = \alpha + \beta^h \Delta\tau_{st} + \gamma^h X_{st} + \kappa_s^h + \mu_t^h + \epsilon_{st+h}, \qquad (1)$$

where the subscript s indexes state, t stands for year, h denotes horizon, and the dependent variable $\Delta^h y_{st} = (Y_{st+h} - Y_{st-1})/Y_{st-1}$, the change in economic activity for state s from year $t-1$ to $t+h$, i.e., real GDP growth, job growth, change in the labor force participation rate (LFPR), or growth in real personal consumption expenditure (PCE). The key explanatory variable, $\Delta\tau_{st}$, is a measure of the state-level tax change in year t, defined here as the change in state-level total income tax

[7] See Jorda and Taylor (2016), Chadorow-Reich (2019), and Zidar (2019), among many others.

liabilities as a share of state-level GDP in period t—$(T_{st} - T_{st-1})/GDP_{st-1}$.[8] Finally, X_{st} are controls for other covariates that vary across states as well as over time and may be correlated with both $\Delta\tau_{st}$ and $\Delta^h y_{st}$; κ_s and μ_t are state and year fixed effects, respectively. Since I use data through 2019, which is two years following TCJA, I am primarily interested in estimating the effect of the 2018 tax change in years 2018 (i.e., $h = 0$) and 2019 (i.e., $h = 1$).

Following a string of recent studies on multipliers, an important object of interest is the cumulative impact of year *t* tax shock on the current and future economic activity. A key question is, what was the cumulative impact of the 2018 TCJA-induced state-level tax change on economic activity in the years 2018 and 2019? In the LP framework, this is easily accomplished by adding up coefficients β^0 and β^1 after estimating equation (1) for $h = 0$ and $h = 1$, respectively. However, a more convenient way to estimate the cumulative impulse response with associated standard errors in just one step is to use the sum of the outcome variable of equation 1 on the left-hand side:

$$\sum_{h=0}^{1} \Delta^h y_{st} = \alpha + \beta^h \Delta\tau_{st} + \gamma^h X_{st} + \kappa_s^h + \mu_t^h + \epsilon_{st+h} \qquad (2)$$

One complication with estimating equations (1) or (2) is that the actual tax change $\Delta\tau_{st}$ is almost certainly endogenous, as it depends on changes in actual income, which are correlated with measures of economic growth, and hence with ϵ_{st+h}. To solve the endogeneity problem, I instrument $\Delta\tau_{st}$with $\Delta\hat{\tau}_{st}^{TCJA}$—a measure of TCJA-induced tax shock holding income constant at the year before the tax change.

[8] Although almost all of the variation in tax changes are driven by changes in federal income taxes, the state-level tax change ($\Delta\tau_{st}$) is based on total taxes—including federal, state, and payroll tax liabilities. I normalize tax variables by the lagged state-level GDP. Normalizing with the current GDP yielded almost identical results.

The instrumental variable (IV) coefficient in this just-identified case is simply the reduced form coefficient scaled by the coefficient on $\Delta\hat{\tau}_{st}^{TCJA}$ from the first stage regression of $\Delta\tau_{st}$on $\Delta\hat{\tau}_{st}^{TCJA}$. The reduced form regression corresponding to equation (3), and analogously for equation (1), is as follows.

$$\sum_{h=0}^{1}\Delta^{h}y_{st} \;= \alpha + \delta^{h}\Delta\hat{\tau}_{st}^{TCJA} + \gamma^{h}X_{st} + \kappa_{s}^{h} + \mu_{\mathrm{t}}^{\mathrm{h}} + \epsilon_{st+h}, \qquad (3)$$

The fixed-effects specification in (3) accounts for all state-specific factors (e.g., right-to-work states or low-cost states) and purely macroeconomic shocks (e.g., oil prices and interest rates) potentially correlated with state-level growth rates. Like standard difference-in-differences (DID) designs, the key identifying assumption[9] is that conditional on X_{st}, any state-by-time effects, ϵ_{st+h}, are random and uncorrelated with $\Delta\hat{\tau}_{st}^{TCJA}$.

To minimize the influence of such omitted factors, I control for other macroeconomic shocks—such as oil prices, interest rates, and political party control of government—that may have differential effects across states. To account for the possibility that positive oil price shocks in 2018 may have benefitted states with large energy sectors, I control for the interaction between oil prices and lagged mining share of employment. Previous research has found that states differ in how sensitive they are to interest rate changes and that the sensitivity varies strongly with the share of the manufacturing sector in states' economies (Carolino and DeFina, 1998). Therefore, I include an interaction between lagged manufacturing share of employment and the federal funds rate.

[9] Given that the treatment is continuous with no clear control group, an additional identifying assumption—that the tax change effect is homogeneous across states—is required to estimate the causal effect in the two-way-fixed-effects model, as per Sun and Shapiro (2022) and de Chaisemartin and D'haultfoeuille (2018). To relax this assumption, it would be necessary to have at least some states that are unaffected by the tax change. However, in this context, such states are not present, limiting my ability to relax this assumption.

Finally, to account for the possibility that state-level tax shocks may be correlated with the party in power at the state level, I include a dummy for Republican control of government. Additionally, as is typical in LP regressions, I also include one lag of $\Delta\tau_{st}$ and GDP growth in the richest specifications. As shown in the results section, the robustness of fixed effect estimates to these additional confounders further reinforces the view that TCJA tax changes were mostly exogenous.

Because the object of interest here is the local multiplier, capturing the causal effect of tax changes on economic growth, and not some nationally representative multiplier, I report unweighted estimates. Moreover, estimates indicate that the impact of the tax shock does not significantly differ by states' population. To account for serial correlation in errors, I report robust standard errors clustered at the state level when needed.

3. Data

In the absence of individual income tax return data at the state level, I calculate tax changes using SOI data, which provides information on the number of taxpayers and their tax filing characteristics for different income groups at the state level. For example, to construct the tax shock measure due to TCJA in 2018, I use the 2017 SOI data to estimate income tax liabilities for an average taxpayer in each income group under both the 2017 and 2018 tax laws using the NBER-TAXSIM model.[10] Key input variables and sample calculations using the NBER-TAXSIM model for representative taxpayers in various Adjusted Gross Income (AGI) groups for Texas and California are presented in Appendix Table 1.

[10] All tax calculations were done using NBER-TAXSIM model available from https://www.nber.org/taxsim/ and documented in Feenberg and Coutts (1993).

The NBER-TAXSIM model calculates taxes based on a series of input variables, the most important of which are income, tax-filing status, number of dependents, and deductions such as mortgage interest and property taxes. Each of these input variables for the average taxpayer in an income group is set to the state-level average.[11]

While not exact, the difference between 2018 and 2017 taxes thus calculated is a good proxy for changes due to the TCJA at the state level. Aggregating tax changes across income groups for each state and expressing it as a percent of the state's GDP yields the state-level measure of tax shock used in estimation. Summary statistics presented in Table 1 show that while state-level income tax liabilities changed little from 2016 to 2017, they dropped significantly from 2017 to 2018. The focal tax shock measure—tax change as percent of GDP—averaged across states declined from 0.05 percent in 2017 to -0.82 percent in 2018.

My measure of GDP growth is based on state-level data from the Bureau of Economic Analysis (BEA).[12] Job growth is calculated from nonfarm payroll employment data from the Current Establishment Statistics (CES) of the Bureau of Labor Statistics (BLS). Data on the state-level labor force participation rate comes from Local Area Unemployment Statistics (LAUS) published by the BLS, and I use real consumption expenditure data from the BEA. Manufacturing and mining share of employment are based on CES data. Data on cyclicality quantile of states is from Zidar (2019). Data on political control of state government is from National Council of State

[11] For example, taxes for a representative taxpayer in the $75,000-$100,000 income group in a state are calculated for the average AGI within each AGI group, with filing status set to married if the share of married filers was 50% or higher and set to single otherwise. Number of dependents was set to the group-level average (rounded to the nearest integer), and deductions were set to the average for that group in SOI data. The calculation does not account for capital gains income and assumes that all income comes from wages. Therefore, estimated taxes and tax rates for high-income returns are likely overestimated.

[12] BEA's estimate of real GDP is measured in chained 2012 dollars. Results based on real GDP come with the caveat that inflation adjustment at the state level can be imperfect due to well-known limitations in state-level price indexes (Zidar, 2019).

Legislatures (NCSL), and data on state-level spending is from National Association of State Budget Officers (NASBO).

4. Results

Informal evidence on identifying assumptions

Similar to standard DID designs, a key identifying assumption is that counterfactual trends in economic growth be similar in states with low exposure to TCJA tax shocks relative to those with high exposure. As tentative evidence of this, if state-level TCJA-induced tax shocks are genuinely exogenous, they should not, at the bare minimum, forecast GDP/job growth in the years preceding the TCJA. Exploring this further, Figure 2 plots the relationship between change in the key dependent variables—two-year cumulative job/GDP growth from 2017 to 2019—on the y-axis against the TCJA tax shock from 2017 to 2018 on the x-axis, along with their linear fits. To account for state-level confounders, the two variables are plotted in changes. The figure shows that while there exists a robust negative relationship between the TCJA tax shock and current values of the change outcome variable (Panels B and D), there is practically no relationship between the tax shock and lagged values of the change in outcome variables (Panels A and C). Analogous regressions (not reported) revealed that current tax shocks are uncorrelated with current spending growth.

Reduced form estimates

Before examining the IV estimates of equation (1) and (2), it is useful to look at the reduced form estimates of the TCJA tax shock ($\Delta\hat{\tau}_{st}^{TCJA}$) using estimates of equation (3). Before delving into the fixed effects specification, I will first present the simple cross-section specification, limiting the sample to the year 2018. Column (1) of Table 2 reports coefficients from the regression of cumulative 2-year change in key outcome variables ($\sum_{h=0}^{1} \Delta^h y_{st}$) on $\Delta\hat{\tau}_{st}^{TCJA}$ using a cross-sectional regression similar to Chodorow-Reich (2019).[13] Estimates in panel A suggest that the 2018 TCJA tax cut had a large positive effect on job growth, and the effect is statistically significant. A similar relationship emerges for GDP growth and PCE growth, although the estimate for GDP growth is imprecise. The tax shock had a somewhat muted effect on labor force participation in the cross-sectional specification, one that is also imprecise.

However, this simple cross-state regression cannot account for pre-existing differences in growth rates, which may be correlated with exposure to TCJA tax shocks. Estimates could be upward biased in magnitude if, for example, high-growth states such as Texas received more generous TCJA tax breaks relative to states such as California and New York, which also tend to grow more slowly. There could also be other state-specific omitted variables confounding estimates in column (1).

Accounting for such state-specific factors, columns (2) of Table 2 reports coefficients from fixed effects regressions of the key outcome variables on the tax shock measure using panel data from 2014 to 2019. This specification is identical to a difference in differences regression, with the reported coefficients capturing the differential effect on states more exposed to 2018 TCJA tax shocks relative to states less exposed before versus after the onset of TCJA. Estimates indicate that a TCJA tax cut equaling 1 percent of GDP led to faster job growth and GDP growth, although the estimated effects are not statistically significant at a 5 percent level. Compared to the cross-

[13] The covariates used in the cross-sectional specification are a measure of states' cyclical sensitivity from Zidar (2019), a republican dummy, lagged mining share of employment, lagged manufacturing share of employment, average age, share white and share with a college degree.

sectional estimates, panel fixed effects estimates suggest that the TCJA led to a sizeable and statistically significant increase in the labor force participation rate. However, the effects on PCE became less pronounced and imprecise.

The simple fixed-effects model in column (2) still omits other covariates which are correlated with growth and vary both across states and over time. For example, if states with Republican control of government received larger tax cuts and for other unknown reasons also grew more slowly in the post-TCJA period, then the coefficient on the tax shock variable in column (2) would be upward biased.

To address such concerns, column (3) of Table 3 includes the following additional covariates: the interaction between log oil price and lagged mining share of employment, an interaction between log federal funds rate and lagged manufacturing share of employment, and a dummy for republican control. The estimated effect in column (3) is little changed from column (2). Finally, as is typical in LP regressions, column (4) augments the fixed effects specification to include lagged tax change as a percent of GDP and lagged GDP growth. Once again, the results are qualitatively indistinguishable from columns (2) and (3).

Overall, the reduced form estimates in Table 2 suggest that a 1 percent of GDP tax cut from TCJA led to 1.4 percent faster job growth and 1.8 percent stronger GDP growth in the first two years after the reform. While results for job growth and GDP growth appear robust across fixed effects specifications, they are imprecise. Faster growth appears to be driven by a statistically significant 1.5 percentage point increase in the labor force participation rate, with the impact on consumer spending being rather small and insignificant.

Instrumental variable estimates

The instrumental variable estimates of actual tax changes, reported in Table 3, largely mirror the reduced form results in Table 2. IV estimates show that, except for the labor force participation rate, panel fixed effects estimates are typically smaller than cross-sectional estimates. With richer specifications, the IV estimates in Table 3 converge to the reduced form estimates. The reason is that the first-stage estimates presented in Panel D of Table 3 is very close to 1 for the richest specification in column (4). Notably, a comparison of OLS and IV estimates for the richest specification presented in Appendix Table 1 shows that instrumenting matters, as the IV estimates are of opposite signs of those from analogous OLS specifications. The wrong sign of the OLS estimates reaffirms the need to use instruments to circumvent the endogeneity of actual tax changes.

Local Projection Estimates

Table 4 tracks the evolution of the TCJA-induced increase in economic growth using separate LP regressions by time horizon for the reduced form specification in column (4) of Table 2. IV estimates by horizon, presented in Appendix Table 2, were almost identical to the reduced form estimates. The effect on job growth, GDP growth, and the labor force participation rate in the period after the initial tax shock (2019) was larger than the impact in the year of the tax sock (2018). Like previously reported estimates, only the labor force participation effect is estimated precisely.

Dynamic impulse responses around the timing of the tax change from LP regressions are presented in Figure 3, which plots the h-period impulse responses for each of the outcome variables, with impulse responses for job growth in Panel A and for GDP growth in Panel B. I allow the horizon (h) to range from -3 to 2. The h-period growth is measured relative to the year before the tax change, the response for which is set to zero. The results suggest that the tax shock impact was rather modest in the year of the tax shock and strengthened in the following year. It is comforting to note that estimated impulse responses for years before the tax shocks are not statistically different from zero, suggesting little anticipation effects.

These estimates are comparable with previous projection-based estimates, though there are some differences. According to an analysis by Mertens (2018), a 1.1 percentage point tax cut (measured as a decrease in revenue) would have an impact on real GDP of 1.2 percentage points in 2018 and 0.4 percentage points in 2019. This adds up to a total cumulative effect of 1.6 percentage points over two years. The effect in 2020 would be minimal. Consequently, the implied two-year multiplier is (1.6/1.1 ≈ 1.5), aligning with my estimates.[14] These short-term projections are on the higher end of the spectrum when compared to other estimates outlined in Gale et al. (2018), which range from 0.4 to 0.9 percent, averaging 0.7 percent.

Cumulative Multipliers

I have so far focused on the impulse responses for the tax change in year t on economic activity in years t and $t+1$ and their sum. These impulse responses provide an estimate of the impact multiplier and the peak multiplier of tax changes in economic activity. Following the work of Ramey Zubairy (2018), several recent papers estimate cumulative multipliers of cumulative tax changes over multiple periods.

$$\sum_{h=0}^{1} \Delta^h y_{st} = \alpha + \beta^h \sum_{h=0}^{1} \Delta^h \tau_{st} + \gamma^h X_{st} + \kappa_s^h + \mu_t^h + \epsilon_{st+h}, \quad (2)$$

[14] Additionally, Mertens (2018) calculates the impact of a 2.75 percentage point cut in Average Marginal Tax Rates (AMTR) following the TCJA. Here, a one percentage point AMTR cut equates to a 0.5 percentage point rise in GDP, yielding an implied real GDP growth of 1.3 percentage points in 2018 and 1 percentage point in 2019. This accumulates to a total effect of 2.3 percentage points.

where $\Delta^h \tau_{st}$ is defined analogously to $\Delta^h y_{st}$ as $\Delta^h \tau_{st} = \sum_{h=0}^{1} \frac{(T_{st+h} - T_{st-1})}{GDP_{st-1}}$ –the two-year cumulative tax change normalized by GDP. The coefficient β^h from an IV regression using $\Delta\hat{\tau}_{st}^{TCJA}$ as the instrument provides an estimate of the cumulative multiplier, which is presented in Table 5. The estimates of cumulative multipliers display a pattern like the two-year sum of impulse responses from the initial tax shock presented in column (4) of Table 2. Showing results for GDP growth, column (2) of Table 5 suggests that the two-year cumulative multiplier of the TCJA tax shock is 1.5, which is at the lower end of the range of recent estimates of between 0.8 to 3.5.[15]

The two-year cumulative estimate of 1.2 in the job growth regression essentially implies that a tax cut worth 1 percent of GDP ($210 billion) over two years led to 1.2 percentage points faster job growth, i.e., approximately 2 million jobs over two years, at a cost per job of $105,000 in 2018 dollars ($210 billion tax cut divided by 2 million jobs). This cost-per-job estimate is significantly higher than the estimate in Zidar (2019) of $35,000 from previous tax changes.

Mechanisms

My analysis reveals that an important mechanism for TCJA's stimulative effect was labor force participation rather than consumer spending. This might seem surprising at first, given that tax changes' supply-side effects typically take time to materialize. So, why am I observing significant labor supply responses? One explanation is that the TCJA comprehensively reduced the average and marginal tax rates (Appendix Figure 1). Labor force participation largely depends on average tax rates. I demonstrate significant variation across states in both marginal and average tax rates (as shown in Appendix Figure 2). Furthermore, the pronounced effect on labor force participation

[15] Recent estimates of tax multipliers include 0.8 in Blanchard and Perotti (2002), 1.1 in Barro and Redlick (2011), 2.5 in Mertens & Ravn (2013), to 3.5 in Zidar (2019).

isn't surprising, as TCJA was not a revenue-neutral tax reform. As noted by Gale and Samwick (2017), a revenue-neutral reform would generally diminish the impact on labor supply due to a smaller substitution effect.

In terms of the weak consumption response, the most likely explanation is that state-level consumption responses are harder to pin down due to spillover to other states. Also, households with relatively lower incomes—those with the largest marginal propensities to consume—received smaller tax cuts from the TCJA.

5. Conclusion

Using SOI tax return statistics for states from 2014 to 2019 and the NBER-TAXSIM model, this paper exploits state-level variation in TCJA tax shocks as a source for identification and measures the TCJA's impact on economic activity after 2017. Using fixed effects models, I find that income tax cuts equaling 1 percent of GDP contributed to about 1.2 percentage points faster job growth and 1.5 percentage points stronger GDP growth after TCJA, so the implied tax multiplier is around 1.5, which is towards the lower end of the range of recent estimates of the stimulative effects of tax changes. While the broader results suggest that the TCJA likely stimulated economic growth, it is important to emphasize that these point estimates carry significant uncertainty and come with wide confidence intervals. These estimates imply a cost per job of $105,000—nearly three times as high as the cost per job estimate for prior tax changes in Zidar (2019). These estimates suggest that the TCJA tax cut equaling 0.8 percent of GDP, on average in 2018 and 2019, contributed to a 1 percentage point stronger job growth, creating about 1.5 million jobs at a cost of nearly $158 billion.

A likely explanation for a relatively modest tax multiplier from TCJA is that these tax cuts were implemented while the economy was still booming; it is well-known that multipliers are typically higher for stimulus during periods of economic slack, which was not the case for TCJA. Another factor is that nearly 70 percent of households in the lowest income quintile did not see a tax cut from the TCJA (Sammartino, Stallworth, and Weiner 2018), and as found in Zidar (2019), stimulative effects of tax changes are mostly driven by tax cuts for lower income groups. The short-run effects could be an upper bound for the long-term effects of TCJA, because the tax cut was financed by higher budget deficits, which generally tend to blunt the long-term effects of tax cuts.

A potential limitation of my analysis is that it largely focuses on the effects of changes in personal income tax, while considerable changes in corporate taxes within the TCJA have been overlooked. Such an omission could bias the paper's estimates, especially if the tax relief from corporate tax cuts varies by state and correlates with income tax cuts received by states' workers.[16] Nevertheless, there are several reasons why the bias may be limited. For instance, prior research suggests the multiplier effect of corporate tax cuts is typically less than that of income tax cuts (Mertens and Ravn, 2013). Moreover, this has been particularly true for the TCJA, which had a modest corporate tax cut multiplier, far smaller than the corporate tax cuts of the Kennedy administration in 1966 (Furno, 2021).[17] Even with the modest stimulative effects stemming from TCJA's corporate tax cuts, it is plausible that some bias remains. As highlighted by Wagner,

[16] As Wagner, Zeckhauser, and Ziegler (2020) note, corporate tax cuts had complex effects on firms, creating winners and losers. However, evidence is scarce on whether these impacts differ among states.

[17] Additionally, a recent study by Kennedy et al. (2022) revealed that TCJA's corporate tax cuts had a modest stimulative effect—every $1 of corporate tax rate cut led to a $0.1 increase in output. This study also found that the majority of the gains from these corporate tax cuts accrued to the top 10% of earners. This evidence aligns with Zidar's (2019) finding that tax cuts for the rich stimulate less than those for the non-rich, thereby further substantiating the claim that TCJA's corporate tax cuts had limited stimulative effect. Therefore, the primary impact I'm capturing in this paper likely stems from the individual income tax cuts.

Zeckhauser, and Ziegler (2020), the corporate tax cuts had complex effects across firms, creating winners and losers. Still, evidence is scarce on whether these impacts differ among states or if they are correlated with individual income tax shocks. Accordingly, a more thorough exploration of this issue is left for future research.

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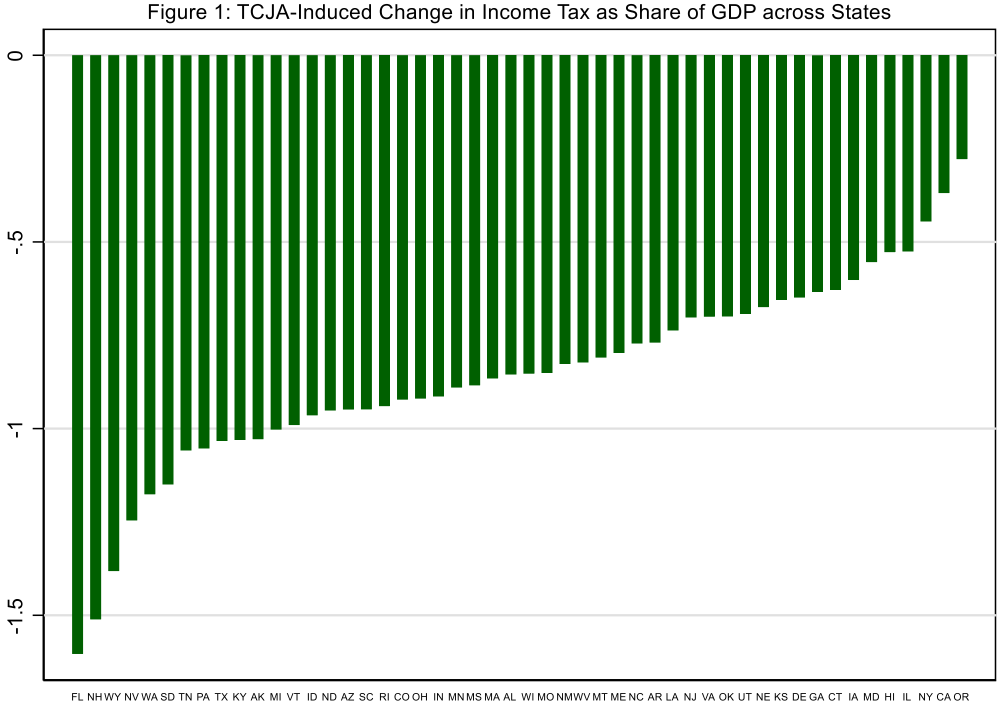
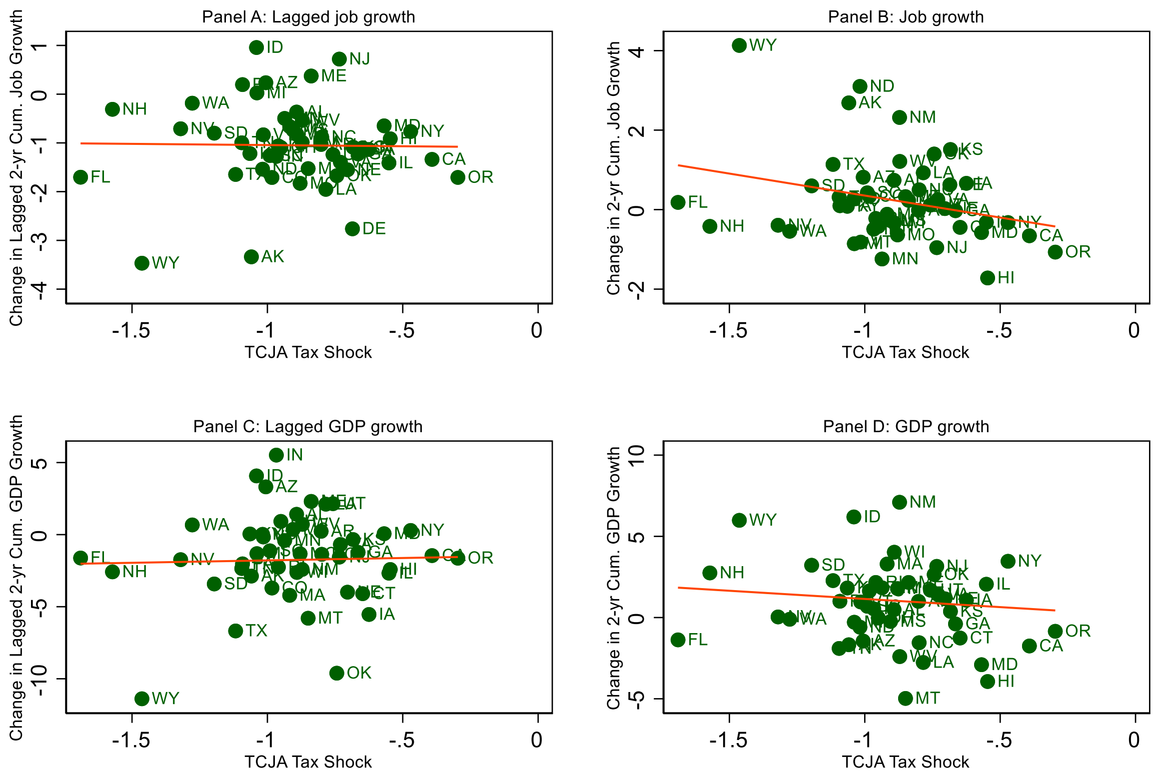
Figure 1: TCJA-Induced Change in Income Tax as Share of GDP across States



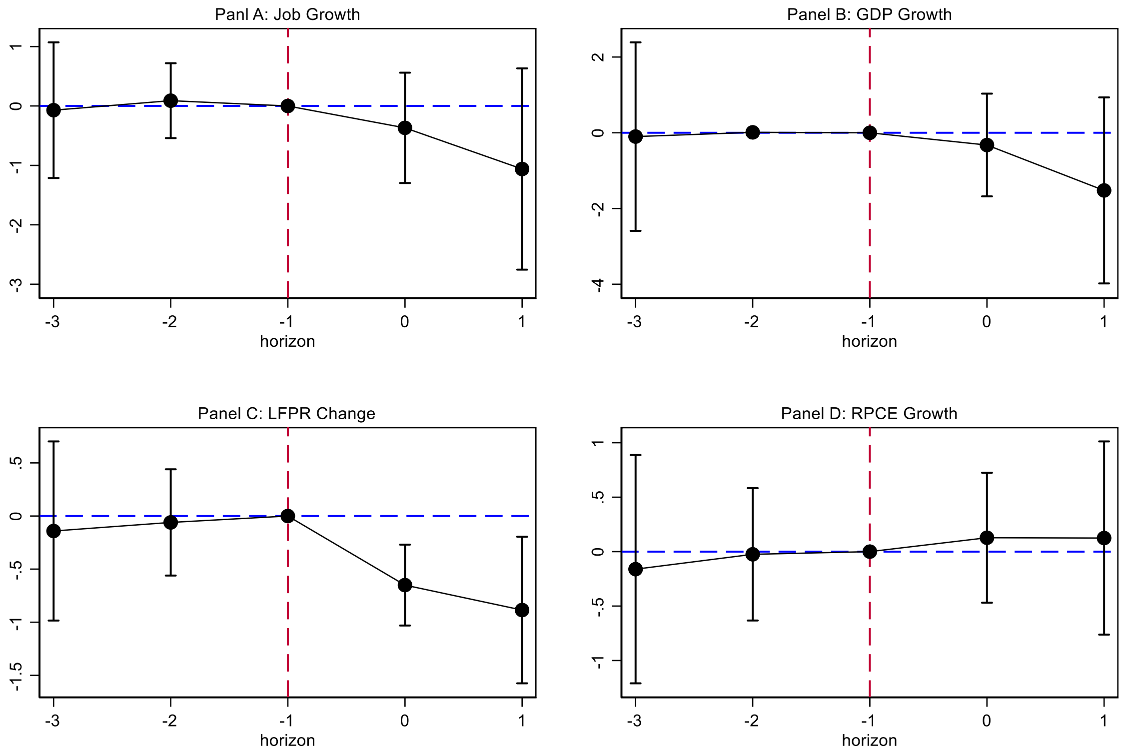
Source: SOI Tax Statistics; authors’ calculations using NBER-TAXSIM.

Figure 2: TCJA Tax Shocks and Economic Activity



Note: The figure plots the relationship between change in the key dependent variables—two-year cumulative job/GDP growth from 2017 to 2019—on the y-axis against the TCJA tax shock from 2017 to 2018 on the x-axis. Panel A and Panel C plot the relationship for change in the lagged outcome variable, while Panels B and D use change in current values of the dependent variable on the y-axis. The linear fit is based on a linear regression of change in the current/lagged outcome variable on the TCJA tax shock. The figure shows that while there exists a robust negative relationship between the TCJA tax shock and current values of the change outcome variable (Panels B and D), there is practically no relationship between the tax shock and lagged values of the change in outcome variables (Panels A and C).

Figure 3: Impulse Responses from TCJA Tax Shock



Note: The figure plots the impulse response of key measures of economic activity to a 1 percentage point TCJA tax shock in year t (i.e., 2018) using local projection regressions, with the same covariates as in column (4) of Table 2, for time horizons ranging from $t-3$ (2015) to $t+1$ (2019). The dependent variable in each regression is the h-period growth/change, $y_{t+h} - y_t$ for $h = -3 \ldots 1$. The figure shows that while the impulse responses of all outcome variables in years 2015-2017 to the 2018 TCJA tax shock are close to zero, the impulse responses in 2018 and 2019 are negative for job growth, GDP growth, and change in the labor force participation rate (LFPR) and zero for real personal consumption expenditure (RPCE). 90 percent confidence intervals are based on standard errors clustered at the state level.

Table 1: Summary Statistics

	Mean	SD	Median	Min	Max
			2016		
Tax (Billions)	120.20	113.00	73.00	4.47	376.33
Change in Tax (Billions)	-0.30	0.36	-0.20	-1.21	0.35
Tax Shock/GDP (Percent)	-0.03	0.03	-0.03	-0.32	0.16
Actual Tax Change/GDP (Percent)	-0.03	0.54	0.08	-1.63	1.27
Payroll Job Growth (Percent)	1.74	1.06	1.54	-4.19	3.49
GDP Growth (Percent)	1.71	1.54	1.86	-6.31	4.40
			2017		
Tax (Billions)	119.48	113.20	73.81	4.32	382.10
Change in Tax (Billions)	0.40	0.52	0.29	-0.38	2.59
Tax Shock/GDP (Percent)	0.05	0.07	0.04	-0.14	0.31
Actual Tax Change/GDP (Percent)	1.44	0.90	1.21	0.19	4.36
Payroll Job Growth (Percent)	1.45	0.71	1.37	-1.23	3.20
GDP Growth (Percent)	2.20	1.52	1.70	-4.20	5.18
			2018		
Tax (Billions)	127.55	123.36	71.68	4.45	411.03
Change in Tax (Billions)	-6.80	5.50	-4.55	-18.70	-0.33
Tax Shock/GDP (Percent)	-0.82	0.33	-0.85	-1.60	-0.28
Actual Tax Change/GDP (Percent)	0.29	0.66	0.29	-1.17	1.36
Payroll Job Growth (Percent)	1.56	0.75	1.37	-0.49	3.32
GDP Growth (Percent)	2.77	1.42	2.77	-1.75	6.85

Notes: All summary statistics are weighted by the state-level number of tax returns. All state-level tax measures are inclusive of federal, state, and payroll tax liabilities.

Table 2: Reduced Form Estimates

	(1)	(2)	(3)	(4)
Panel A: Job Growth				
TCJA Tax Shock	-2.805**	-1.598	-1.502	-1.429
	(1.215)	(1.442)	(1.438)	(1.590)
R-Sq	0.323	0.825	0.829	0.834
Panel B: GDP Growth				
TCJA Tax Shock	-2.737	-1.825	-1.897	-1.846
	(2.455)	(1.933)	(2.107)	(2.238)
R-Sq	0.248	0.600	0.601	0.623
Panel C: Change in LFPR				
TCJA Tax Shock	-0.950	-1.368**	-1.422**	-1.535**
	(0.775)	(0.636)	(0.662)	(0.640)
R-Sq	0.095	0.369	0.377	0.379
Panel D: Consumption Growth				
TCJA Tax Shock	-2.550*	0.117	-0.162	0.253
	(1.309)	(0.661)	(0.789)	(0.885)
R-Sq	0.424	0.850	0.867	0.871
Observations	50	250	250	250
Year Fixed Effects	No	Yes	Yes	Yes
State Fixed Effects	No	Yes	Yes	Yes
Other Controls	Yes	No	Yes	Yes
Lags	No	No	No	Yes

Note: The table reports the coefficient on TCJA tax shock from regressions of two-year cumulative growth/change in outcome variables, as indicated, on the TCJA tax shock. Column (1) presents results using cross-section regression for the year 2018, while columns (2)-(4) report results using state-level panel data from 2014 to 2018. The covariates used in the cross-sectional specification in column (1) are a measure of states' cyclical sensitivity from Zidar (2019), a republican dummy, lagged mining share of employment, lagged manufacturing share of employment, average age, share white and share with a college degree. Controls in columns (2)-(4) include the interaction between log oil price and lagged mining share of employment, an interaction between log federal funds rate and lagged manufacturing share of employment, a dummy for republican control. In column (4), lags include lagged tax change as a percent of GDP and lagged GDP growth.

Table 3: Instrumental Variable Estimates

	(1)	(2)	(3)	(4)
Panel A: Job Growth				
Tax Change/GDP	-2.668*	-1.215	-1.118	-1.443
	(1.366)	(1.169)	(1.109)	(1.555)
Panel B: GDP Growth				
Tax Change/GDP	-2.603	-1.387	-1.413	-1.863
	(2.295)	(1.540)	(1.593)	(2.163)
Panel C: Change in LFPR				
Tax Change/GDP	-0.904	-1.039	-1.059	-1.550*
	(0.749)	(0.634)	(0.651)	(0.799)
Panel C: Consumption Growth				
Tax Change/GDP	-2.425*	0.089	-0.120	0.255
	(1.326)	(0.435)	(0.523)	(0.761)
Panel D: First Stage				
TCJA Tax Shock	1.051**	1.316**	1.343**	0.991**
	(0.216)	(0.418)	(0.418)	(0.262)
Craig-Mcdonald-F	25.376	22.795	23.410	14.720
KP-F	23.658	9.927	10.322	14.302
underid_pval	0.009	0.033	0.032	0.007
Observations	50	250	250	250
Year Fixed Effects	No	Yes	Yes	Yes
State Fixed Effects	No	Yes	Yes	Yes
Other Controls	Yes	No	Yes	Yes
Lags	No	No	No	Yes

Note: * $p<0.10$, ** $p<0.05$. The table shows the coefficients from a regression of two-year cumulative growth in measures of economic activity on tax change/GDP, using TCJA tax shock in year t as an instrument. The dependent variable in each regression is $\sum_{h=0}^{1} y_{t+h} - y_{t-1}$, for h=0 and 1, where y denotes growth in job/GDP/labor force participation/consumption expenditure. The coefficients reported represent the two-year cumulative impulse response, i.e., the sum of impulse responses in year t and year $t+1$, from a 1 percent of GDP tax change in year t. Standard errors in parenthesis are clustered at the state level. IV regressions were estimated using STATA ivreg2 software from Baum et al. (2010). The covariates used in the cross-sectional specification in column (1) are a measure of states' cyclical sensitivity from Zidar (2019), a republican dummy, lagged mining share of employment, lagged manufacturing share of employment, average age, share white and share with a college degree. Controls in columns (2)-(4) include the interaction between log oil price and lagged mining share of employment, an interaction between log federal funds rate and lagged manufacturing share of employment, a dummy for republican control. In column (4), lags include lagged tax change as a percent of GDP and lagged GDP growth.

Table 4: Reduced Form Estimates of Impulse Responses from TCJA Tax Shock

	(1)	(2)	(3)
	Year 0	Year 1	Year 1 + Year 2
Panel A:Job Growth			
TCJA Tax Shock X Post	-0.368	-1.061	-1.429
	(0.566)	(1.033)	(1.590)
Observations	250	250	250
R-Sq	0.756	0.863	0.834
Panel B: GDP Growth			
TCJA Tax Shock X Post	-0.323	-1.523	-1.846
	(0.827)	(1.498)	(2.238)
Observations	250	250	250
R-Sq	0.469	0.692	0.623
Panel C: Change in Labor Force Participation			
TCJA Tax Shock X Post	-0.650**	-0.885**	-1.535**
	(0.232)	(0.421)	(0.640)
Observations	250	250	250
R-Sq	0.290	0.406	0.379
Panel D: Consumption Growth			
TCJA Tax Shock X Post	0.128	0.125	0.253
	(0.364)	(0.541)	(0.885)
State Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Controls (with Lags)	Yes	Yes	Yes
Observations	250	250	250
R-Sq	0.825	0.885	0.871

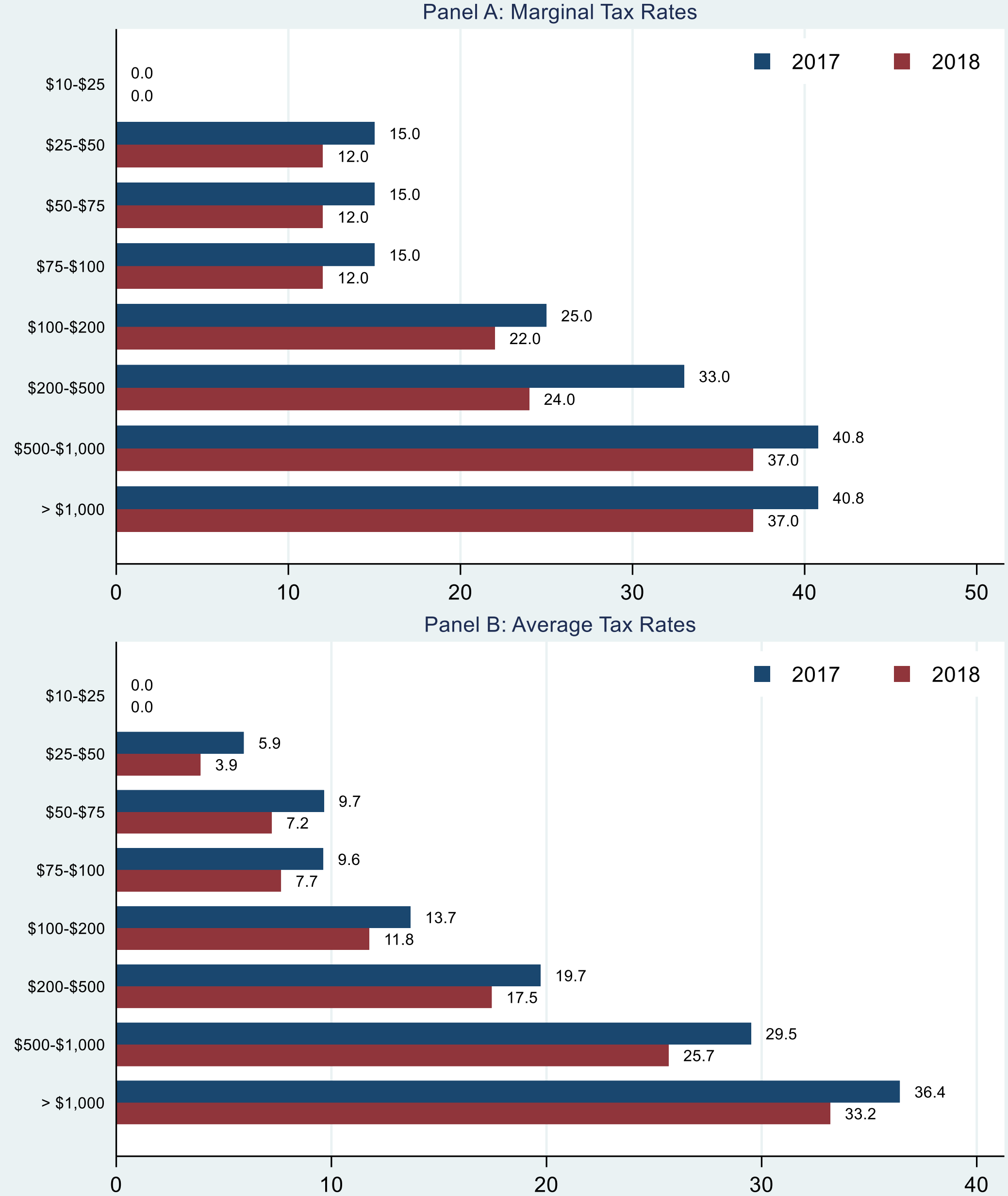
Note: * p<0.10, ** p<0.05. The table shows the impulse response of key measures of economic activity to a one percentage point TCJA tax shock in year t, (i.e., 2018) using local projection regressions, with the same covariates as in column (4) of Table 2, for time horizons ranging from year t (2015) to $t+1$ (2019). The dependent variable in each regression is the h-period growth/change, $(Y_{t+h} - Y_{t-1})/Y_{t-1}$ for h=0 and 1 in columns (1) and (2), respectively. Impulse responses for year t (2018) are in column (1) and for year $t+1$ (2019) in column 2. Column (3) presents the estimated coefficient on the TCJA tax shock when the dependent variable is $(\sum_{h=0}^{1} Y_{t+h} - Y_{t-1})/Y_{t-1}$, i.e., two-year cumulative growth in job/GDP/labor force participation/consumption expenditure. The estimate in column (3) equals the sum of impulse responses in year t and year $t+1$. Standard errors in parenthesis are clustered at the state level.

Table 5: IV Estimates of Two-Year Cumulative Multipliers

	(1)	(2)	(3)	(4)
	Cum 2-yr Jobs	Cum 2-yr GDP	Cum 2-yr LFPR	Cum 2-yr PCE
Cum 2-yr Tax Shock	-1.188	-1.534	-1.276	0.210
	(1.607)	(2.094)	(0.916)	(0.583)
Year Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
Controls (with Lags)	Yes	Yes	Yes	Yes
Observations	250	250	250	250
R-Sq	0.695	0.524	-0.263	0.880
Craig-Mcdonald-F	7.728	7.728	7.728	7.728
KP-F	3.377	3.377	3.377	3.377
underid_pval	0.078	0.078	0.078	0.078

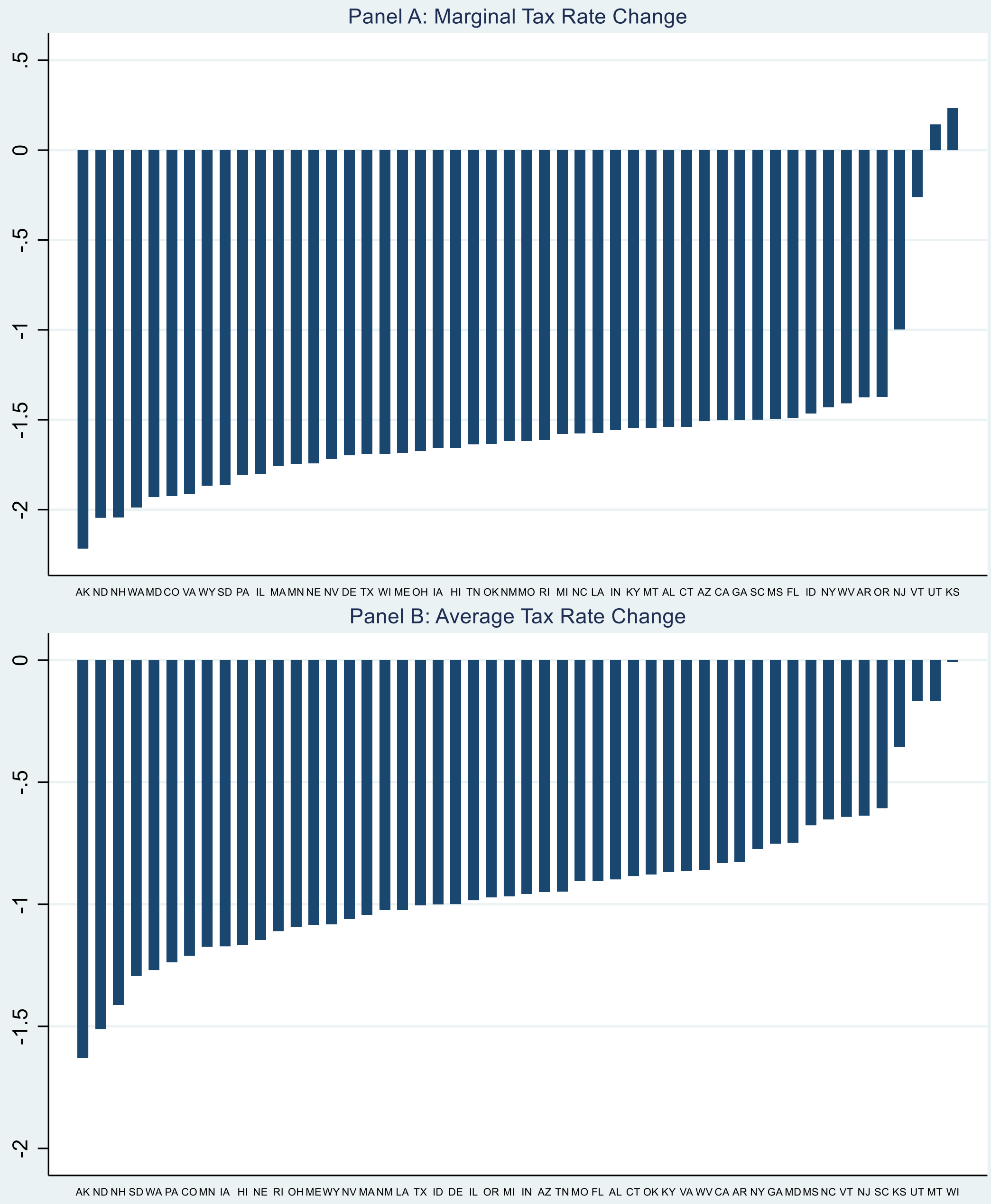
Note: * p<0.10, ** p<0.05. The table shows the two-year cumulative effects for key measures of economic activity from a one percentage point change in two-year cumulative actual taxes/GDP in year t and $t+1$, using TCJA tax shock in year t as an instrument, including the same covariates as in column (4) of Table 2, for time horizons ranging from year t (2015) to $t+1$ (2019). The dependent variable in each regression is the is $\left(\sum_{h=0}^{1} Y_{t+h} - Y_{t-1}\right)/Y_{t-1}$, i.e., two-year cumulative growth in job/GDP/labor force participation/consumption expenditure and the key right hand said variable is $\left(\sum_{h=0}^{1} \tau_{t+h} - \tau_{t-1}\right)/GDP_{t-1}$, i.e., two-year cumulative change in actual taxes normalized by GDP. Standard errors reported in parenthesis are clustered at the state level. IV regressions were estimated using STATA ivreg2 software from Baum et al. (2010).

Appendix Figure 1: Changes in Federal Income Tax Rates before vs. after TCJA



Notes: Income groups on y-axis in thousand dollars; SOI idnividual income tax return data; Author's calculations using NBER-TAXSIM

Appendix Figure 2: State-level Variation in Federal Income Tax Rate Change



Appendix Table 1: Sample NBER-TAXSIM Input and Output Variables based on Averages from SOI 2016 Data

State	AGI Group (Thousands)	Number of Returns	Filing Status	Deps[Ψ]	Average AGI	Average Property tax	Average Other Itemized Deductions*	2016 Federal Income Tax	2017 Federal Income Tax	2018 Federal Income Tax
CA	$0 or less	282380	Single	0	0	0	0	0	0	0
CA	$0.001- $10	2171950	Single	0	5389	147	709	-412	-412	-412
CA	$10- $25	3804250	Single	1	17308	209	1101	0	0	0
CA	$25-$50	4168190	Single	1	36159	506	2832	2151	2139	1407
CA	$50-$75	2328840	Single	1	61434	1250	6276	5943	5930	4440
CA	$75-$100	1497060	Married	1	86638	2137	9612	8212	8218	6636
CA	$100-$200	2422130	Married	1	137787	3890	14502	17007	16980	16412
CA	$200-$500	925170	Married	1	286927	7804	22839	54062	53752	49060
CA	$500-$1,000	145880	Married	1	672146	14379	32110	175611	175454	172492
CA	$1,000 or more	71290	Married	1	3514985	31546	242056	1101583	1101433	1146663
TX	$0 or less	162530	Single	0	0	0	0	0	0	0
TX	$0.001- $10	1677390	Single	0	5320	78	402	-407	-407	-407
TX	$10- $25	2860440	Single	1	17152	124	808	0	0	0
TX	$25-$50	2961660	Single	1	36162	385	2615	2152	2139	1407
TX	$50-$75	1556440	Single	1	61270	1044	5351	5918	5905	4420
TX	$75-$100	957550	Married	1	86662	1822	7423	8359	8339	6638
TX	$100-$200	1405640	Married	1	135697	3730	11286	18336	18266	15952
TX	$200-$500	436180	Married	1	285125	8381	20699	55207	55001	49141
TX	$500-$1,000	66720	Married	1	672133	14431	34704	195291	194805	171528
TX	$1,000 or more	31810	Married	1	2958385	26070	183843	1062253	1061739	962260

Notes: [Ψ] Number of dependents. *Average Other Itemized Deductions exclude state income taxes, as they are calculated separately based on actual state income tax calculations. The AGI group $0 or less includes returns with negative incomes; the average AGI for this group is set to zero. Filing status is set to married if the share of married filers is 50% or higher and set to single otherwise. The number of dependents was set to the group-level average (rounded to the nearest integer), and deductions were set to the average for that group in SOI data.

Appendix Table 2: Comparison of OLS vs. IV Estimates of Two-Year Cumulative Impulse Responses

Panel A: OLS Estimates				
	Cum 2-yr Jobs	Cum 2-yr GDP	Cum 2-yr LFPR	Cum 2-yr PCE
Tax Change/GDP	1.035*	1.877**	0.166	0.894*
	(0.538)	(0.915)	(0.125)	(0.455)
Observations	250	250	250	250
R-Sq	0.848	0.644	0.363	0.883
Panel B: IV Estimates				
Tax Change/GDP	-1.443	-1.863	-1.550*	0.255
	(1.555)	(2.163)	(0.799)	(0.761)
Year Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
Controls (with Lags)	Yes	Yes	Yes	Yes
Observations	250	250	250	250
R-Sq	0.756	0.556	0.022	0.877
Craig-Mcdonald-F	14.720	14.720	14.720	14.720
KP-F	14.302	14.302	14.302	14.302
underid_pval	0.007	0.007	0.007	0.007

Note: * p<0.10, ** p<0.05. The table shows the coefficients from an OLS regression of two-year cumulative growth in measures of economic activity on current year tax change/GDP in Panel A and from an instrumental variable regression, using TCJA tax shock in year t as an instrument in Panel B. All specifications include the same set of covariates as in column (4) of Table 2. The dependent variable in each regression is $\left(\sum_{h=0}^{1} Y_{t+h} - Y_{t-1}\right)/Y_{t-1}$, i.e., two-year cumulative growth in job/GDP/labor force participation/consumption expenditure. The reported coefficients represent the two-year cumulative impulse response, i.e., the sum of impulse responses in year t and year $t+1$, from a 1 percent of GDP tax change in year t. Standard errors in parenthesis are clustered at the state level. IV regressions were estimated using STATA ivreg2 software from Baum et al. (2010).

Appendix Table 3: IV Estimates of Impulse Responses

	Year 0	Year 1	Year 1 + Year 2
Table 4: Panel A: Job Growth			
Tax Change/GDP	-0.372	-1.071	-1.443
	(0.539)	(1.024)	(1.555)
R-Sq	0.695	0.781	0.756
Panel B: GDP Growth			
Tax Change/GDP	-0.326	-1.537	-1.863
	(0.747)	(1.498)	(2.163)
R-Sq	0.438	0.611	0.556
Panel C: Change in Labor Force Participation			
Tax Change/GDP	-0.656**	-0.894*	-1.550*
	(0.312)	(0.497)	(0.799)
R-Sq	-0.101	0.120	0.022
Panel D: Consumption Growth			
Tax Change/GDP	0.129	0.126	0.255
	(0.313)	(0.466)	(0.761)
State Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Controls (with Lags)	Yes	Yes	Yes
Observations	250	250	250
R-Sq	0.832	0.889	0.877
Craig-Mcdonald-F	14.720	14.720	14.720
KP-F	14.302	14.302	14.302
underid_pval	0.007	0.007	0.007

Note: * p<0.10, ** p<0.05. The table shows the impulse response of key measures of economic activity to a 1 percentage point change in actual taxes/GDP in year t, using TCJA tax shock in year t as an instrument in local projection regressions, including the same covariates as in column (4) of Table 2, for time horizons ranging from year t (2015) to $t+1$ (2019). The dependent variable in each regression is the h-period growth/change, $(Y_{t+h} - Y_{t-1})/Y_{t-1}$ for h=0 and 1 in columns (1) and (2), respectively. Impulse responses for year t (2018) are in column (1) and for year $t+1$ (2019) in column 2. Column (3) presents the estimated coefficient on actual tax change when the dependent variable is $(\sum_{h=0}^{1} Y_{t+h} - Y_{t-1})/Y_{t-1}$, i.e., two-year cumulative growth in job/GDP/labor force participation/consumption expenditure. The estimate in column (3) equals the sum of impulse responses in year t and year $t+1$. Standard errors in parenthesis are clustered at the state level. IV regressions were estimated using STATA ivreg2 software from Baum et al. (2010).