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

Anil Kumar

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

Anil Kumar†

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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 1.3 percentage point faster job growth and nearly 1.5 percentage points higher GDP growth. The impact on growth was the strongest in the year of the tax change, with much smaller effects in the following two years. The estimates imply a tax cut multiplier of around 1.5 and a cost per job of $105,000. Moreover, they also suggest that TCJA-related income tax cuts of 0.8 percent of GDP led to 1 percentage point stronger job growth in 2018, which translates to about 1.5 million jobs at a cost of nearly $158 billion.

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

JEL Classification: E62, H30

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†Anil Kumar, Research Department, Federal Reserve Bank of Dallas, 2200 N. Pearl St., Dallas, TX 75201, anil.kumar@dal.frb.org.

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. Among the most far-reaching changes, 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applied to income over $600,000 for married filers—a higher threshold than $480,050 in 2017.[1] The new tax law also repealed personal and dependent exemptions, increased the amount of child tax credit, and considerably reduced the scope of the Alternative Minimum Tax (AMT).[2]

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.

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 fully dynamic effects of the TCJA, following the recent pioneering work of Zidar (2019), the immediate short-term effect of the TCJA can be identified using spatial variation in tax changes. There are at least three reasons why such an exercise is worthwhile.

First, an estimate using actual TCJA-induced variation in tax cuts can provide a more accurate measure of the TCJA impacts than projections based on estimated effects of prior tax reforms. Secondly, there remains significant divergence in the estimates of tax multipliers from

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[1] These individual income tax changes are set to expire after 8 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.
[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.
[3] See Mertens, K. (2018) and Gale et. al. (2018) for a review of estimated effects of TCJA.

the previous literature with recent estimates ranging from less than 1 to as large as 3.5. And finally, there is also some debate about the timing of the impact of tax changes on growth. Previous research on the immediate impact of tax changes is mixed at best. While Romer and Romer (2010) and Mertens and Ravn (2013) found evidence of impact starting in the first year of the tax change, Zidar (2019) found insignificant first-year effect.[4]

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. These 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. According to the widely used characterization in Romer and Romer (2010), tax shocks driven by spending changes or "countercyclical" tax cuts in response to concerns of a likely downturn are potentially endogenous. On the other hand, exogenous tax changes are not motivated by the desire to temporarily return output to normal, but rather to reduce the federal deficit or to raise the long-run growth rate of potential output.

Using the criteria outlined in Romer and Romer (2010), the TCJA tentatively fits the definition of an exogenous tax change. Just after the TCJA was signed into law, the 2018 Economic Report of the President noted that "*The U.S. economy experienced a strong and economically notable acceleration in 2017, with growth in real gross domestic product exceeding expectations and increasing to 2.5 percent, up from 1.8 percent during the four quarters of 2016.*" Thus, weak economic activity does not appear to be a motivation for the tax reform. Furthermore, in remarks

[4] Romer and Romer (2010) estimate that a 1 percent of GDP tax increase starts having significant negative impact on real GDP after three quarters, peaking to almost 3 percent after 10 quarters. Mertens and Ravn (2013) find that a 1 percentage point cut in average personal income tax rate raises per-capita real GDP by 1.4 percent in the first quarter, with the maximum impact rising up to 1.8 percent after three quarters. Barro and Redlick (2011) find that a 1 percentage point cut in average marginal tax rate raises per-capita GDP by 0.5 percentage points after one year.

before the TCJA became law, the Treasury Secretary stated that "*Lackluster growth below 2 percent has sometimes been referred to as the New Normal*" and observed that the proposed tax reform plan would help "*create sustained growth of 3 percent or higher.*" Additionally, the 2018 tax cuts do not appear motivated by changes in government spending.

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 and changes in state-level spending. I use panel data on growth rates and tax shocks from 2016-2020 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.

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 percent of GDP, varied widely across states—from 0.3 percent of GDP in California to 1.6 percent of GDP in Florida. Figures 2 and 3 show that while the *change* in TJCA tax shock was uncorrelated with the *change* in job growth in 2017 (Figure 2), the shocks shared a strong negative relationship with the *change* in state-level job growth in 2018 (Figure 3). A similar pattern held for the tax shock's relationship with GDP growth.

The 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. 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 the strongest in the year of the tax change, with much smaller effects in the following two years.

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 specifications closely follow recent work of Zidar (2019) and Nakamura and Steinsson (2014):

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

where the subscript s indexes states, t stands for year, and the dependent variable $\Delta^m y_{st}$ is the m period change in economic activity, i.e., GDP growth or job growth, for state s in year t. The key explanatory variable, $Tax\ Shock_{st}^m$, is a measure of state-level tax shock over m periods, defined here as the m period change in state-level total income tax liabilities ($D\tau_{st}^m$) as a share of state-level GDP in period t.[5] Finally, X_{st} are controls for other covariates that vary across states as well as over time and may be correlated with both $Tax\ Shock_{st}$ and $\Delta^m y_{st}$; κ_s and μ_t are state and year fixed effects, respectively.

[5] Although almost all of the variation in tax changes are driven by changes in federal income taxes, the state-level tax shock ($D\tau_{st}$) is based total taxes—including federal, state, and payroll tax liabilities. Because all taxes are calculated using 2016 SOI statistics, I normalize tax variables by the 2016 state-level GDP. Normalizing with current GDP yielded almost identical results.

Following Zidar (2019) and Nakamura and Steinsson (2014), I first focus on the cumulative impact of 2-year tax change on 2-year change in economic activity. In this specification, the left hand side in (1) is $\Delta^2 y_{st} = (y_{st} - y_{st-2})/y_{st-2}$ and the 2-year tax shock measure on the right hand side is calculated as $Tax\ Shock_{st}^{m} = (\sum_{k=0}^{2} \mathrm{D}\tau_{st-k})/GDP_{t-2}$.

In addition to the 2-year change specifications, I also track the evolution of the tax change impact before and after the tax shock by estimating impulse responses at different time horizons using local projections (LP) specifications similar to Zidar (2019) and Jorda and Taylor (2017). For estimating the impact on growth at horizon h, the LP specification can be written as:

$$y_{s,t+h} - y_{s,t-1} = \alpha + \beta^h Tax\ Shock_{st} + \gamma^h X_{st} + \kappa_s^h + \mu_t^h + \epsilon_{st+h} \tag{2}$$

In LP specifications, I allow h to vary from -3 to 2 to estimate the impact not only after the tax shock but also any anticipation effects before the tax change.

The fixed-effects specification in (1) and (2) 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 is that, conditional on X_{st}, any state-by-time effects, ϵ_{st}, are random and uncorrelated with $Tax\ Shock_{st}$. 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. However, I do not account for the corporate tax cuts from TCJA, so identification rests on the assumption that TCJA corporate tax cuts were uncorrelated with TCJA income tax shocks at the state level.

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 a dummy for energyintensive states. Previous research has found that states differ in how sensitive they are to interest rate changes and that the sensitivity varies strongly with share of the manufacturing sector in states' economies (Carolino and DeFina, 1998). Therefore, I include an interaction between 2016 manufacturing share of employment and the federal funds rate. Following Zidar (2019), I also control for state-level cyclicality-quintile-specific year effects. 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. The robustness of fixed-effect estimates to these additional confounders further reinforces the view that TCJA tax changes were mostly exogenous. All estimates are weighted by the number of state-level tax returns to obtain nationally representative estimates. To account for serial correlation in errors, I throughout use clustered standard errors 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.[6] Key input variables and sample calculations using the NBER-TAXSIM model

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

for representative taxpayers in various Adjusted Gross Income (AGI) groups for Texas and California are presented in Appendix Table A1.

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.[7]

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.

The two outcome variables are real GDP growth and job growth. GDP growth is based on state-level data from the Bureau of Economic Analysis (BEA).[8] Job growth is calculated from nonfarm payroll employment data from the Current Establishment Statistics (CES) of the Bureau of Labor Statistics (BLS). I define energy states as those in which mining share of total state employment in 2016 exceeds 1 percent. Manufacturing share of employment is also based on CES data. Data on cyclicality quantile of states is from Zidar (2019). Data on political control of state

[7] 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.
[8] 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).

government is from National Council of State 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. Furthermore, if state-level TCJA-induced tax shocks are indeed exogenous, then at the very minimum they should not predict GDP/job growth in the years prior to the TCJA and current spending. Table 2 reports coefficients on the tax shock variable from fixed effects regressions of one-year lagged job growth, one-year lagged GDP growth, and current spending growth on the tax shock and shows that none of the three coefficients is significant. Analogous regressions (not reported) revealed that current tax shocks are uncorrelated with even longer lags of job growth and GDP growth in pre-TJCA period.

Impact of TCJA-induced income tax changes on growth

The main results from estimation of the econometric specification in equation (1) are presented in Tables 3 and 4. Using data from 2016 through 2020, Column (1) of Table 3 reports coefficients from an OLS regression of 2-year payroll job growth on the 2-year tax shock measure.[9] 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

[9] Note that while data from 2016 to 2020 is used, because regressions are based on 2-year change in growth and 2-year tax shock measure, the estimation sample effectively consists of years 2018-2020.

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 3 reports coefficients from fixed effects regressions of payroll job growth on the tax shock measure. Estimates indicate that a tax cut equaling 1 percent of GDP leads to a 2.8 percentage point faster job growth and the effect is statistically significant at a 5 percent level. 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: interaction between cyclicality quantile and year effects, interaction between a dummy variable for energy state and oil prices, interaction between 2016 manufacturing share and the federal funds rate, and an interaction between a dummy for Republican control and year effects. The estimated effect in column (3) loses statistical significance but remains consistent with findings in columns (1) and (2) that tax cuts led to faster job growth. The point estimates indicate that a tax cut equaling 1 percent of GDP leads to a 1.3 percentage point faster job growth.

Overall, Table 3 suggests that tax cuts led to faster job growth after TCJA. Isomorphic to Table 3, Table 4 presents estimated effects for real GDP growth. The pattern of results in Table 4 largely mirrors those for job growth in Table 3, with the estimate in the richest specification in column (4) implying an impact of about 1.5 percentage points on GDP growth, though results are imprecise.

Instrumental variable estimates

The fixed effects estimates presented in Tables 3 and 4 can still be biased and inconsistent if there are other state by time confounders that are correlated with both tax shocks and economic growth rates. Furthermore, the tax shock variable also contains some measurement error. To mitigate these concerns, it is necessary to use instrumental variables (IV) to identify the effect of the tax shock. I use two instruments plausibly correlated with the tax shock: (1) the share of tax returns with AGI $200,000 or higher in 2016 (*share200K+*) and (2) maximum state income tax rate (*maxsitax*). To motivate the use of these instruments, we first write equation (1) in the first differenced form, eliminating the fixed effects (κ_s):

$$\Delta\Delta^m y_{st} = \alpha + \beta\,\Delta Tax\ Shock_{st}^m + \gamma\Delta X_{st} + \mu_{\mathrm{t}} + \Delta\epsilon_{st} \qquad (3)$$

The identifying assumption is that conditional on $\Delta Tax\ Shock_{st}^m$, *share200K+* and *maxsitax* do not directly affect the *change* in m period growth rates, $\Delta\Delta^m y_{st}$. I do not rule out the possibility that the two variables may be correlated with the *level* of growth in economic activity, just that they are uncorrelated with the *change* in growth rates. The validity of *share200K+* as an instrument for the change in tax shock rests on the argument that regional variation in income distribution is plausibly exogenous—an assumption also made in Zidar (2019). As for *maxsitax*, the implicit assumption for validity is that any differences in growth rates between states with differing top state income tax rates are constant over time, so that *maxsitax* can be excluded from the first differenced equation.

Due to the nature of TCJA tax changes, which altered taxes differentially across the income distribution and introduced caps on state and local tax deductions, both of these variables should be strongly correlated with the tax shock, $Tax\ Shock_{st}^m$. It turns out that they are also highly correlated with the change in the tax shock, $\Delta Tax\ Shock_{st}^m$.

Estimates from IV regressions controlling for first differences of covariates (ΔX_{st}) are reported in columns (2) and (4) of Table 5 for job growth and GDP growth, respectively. OLS estimates from first-differenced regressions are also presented in columns (1) and (2) for comparison. The bottom panel of the table presents diagnostics examining the properties of the two IVs. Assuming homoscedasticity, the high partial F-statistic for the joint significance of IVs in the first stage suggests that they are strongly correlated with $\Delta Tax\ Shock_{st}^{m}$ with an F-stat well nearly equaling 10—the rule of thumb suggested in Stock, Wright, and Yogo (2002). Because that rule-of-thumb is not valid under heteroscedasticity, the bottom panel of Table 5 also reports the "effective F-statistic" proposed in Olea and Pflueger (2013). The "effective F-statistic" is larger than the critical values reported in the paper and presented in the next row, indicating that the instruments are not weak. In addition to the instrument's relevance, it is reassuring to note that the p-value on the test of overidentifying restrictions using Hansen's J-statistic suggests that overidentifying restrictions are not rejected, i.e., the additional instrument is valid (Hansen, 1982).

Results in Table 5 reaffirm the findings in Tables 3 and 4 that TCJA tax cuts had a positive effect on the pace of economic activity. The first-differenced OLS estimates in column (1) and (3) of Table 5, for job growth and GDP growth, respectively, are similar to the corresponding fixed effects estimates reported in column (3) of Tables 3 and 4. The IV estimates in Table are smaller than the both the fixed effects and the first-differenced OLS estimates. The IV estimate in column (2) implies that TCJA income tax cuts led to 0.8 percentage point faster job growth.

Analogous IV estimates for GDP growth in columns (4) of Table 5 are very close to analogous fixed effects estimates in column (3) of Table 4 as well as the first-differenced OLS estimates in column (3) of Table 5. Given the imprecision of IV estimates, it is useful to formally test whether it is statistically different from the corresponding first-differenced OLS estimate.

A Hausman test for endogeneity of $\Delta Tax\ Shock_{st}^{m}$ in both columns of Table 5 yields a p-values of 0.48 and 0.95, respectively, implying that the change in tax shock variable is not endogenous (Hausman, 1978). Thus, under the assumption that the instruments are valid, there is no statistical evidence that the first-differenced estimates are contaminated by endogeneity. Given the first differenced and fixed effects estimates are similar in spirit, this conclusion should likely extend to IV versus the fixed effects estimates as well. Therefore, I continue to use the fixed effects estimates reported in column (3) of Table 3 and Table 4 as my preferred set of estimates.[10]

Cost per Job

These estimates can be interpreted as tax multipliers because the tax shock is measured as percent of GDP. Thus, the preferred estimates from column (3) of Tables 3 and 4 imply a tax multiplier of around 1.3-1.5. A multiplier of around 1.5 is towards the lower end of the range of recent estimates of between 0.8 to 3.5.[11] The coefficient of 1.3 in the job growth regression essentially implies that a tax cut worth 1% of GDP ($210 billion in 2018) led to 1.3 percentage point faster job growth, i.e., approximately 2 million jobs (in 2018), 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.

Local Projection Estimates

Dynamic impulse responses around the timing of the tax change from local projection specifications of equation (2) are presented in Figure 4, which plots the h-period impulse responses from LP specifications for job growth in Panel A and GDP growth in Panel B, with h ranging from

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[10] The results for job growth using state-level data reported in Tables 3 and 5 are broadly consistent with those using county-level data, reported in Appendix Tables 2 and 3, though the estimates from county-level data are more precise due to more variation. Notably, the exogeneity of first different tax shock measure is rejected at the county level.

[11] 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).

-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 the strongest in the year of the tax change and dissipated in the following two years. It is comforting to note that estimated impulse responses for years before the tax shocks are not statistically different from zero, suggesting little anticipation affects. The main takeaway from Figure 4 is that much of the response from TCJA was concentrated during the first year of the law change, with the growth response dissipating in the following two years.

5. Conclusion

Using SOI tax return statistics for states from 2016 to 2020 and 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 1.3 percentage point faster job growth and 1.5 percentage point 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. 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 contributed to a 1 percentage point stronger job growth in 2018, 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 a see 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.

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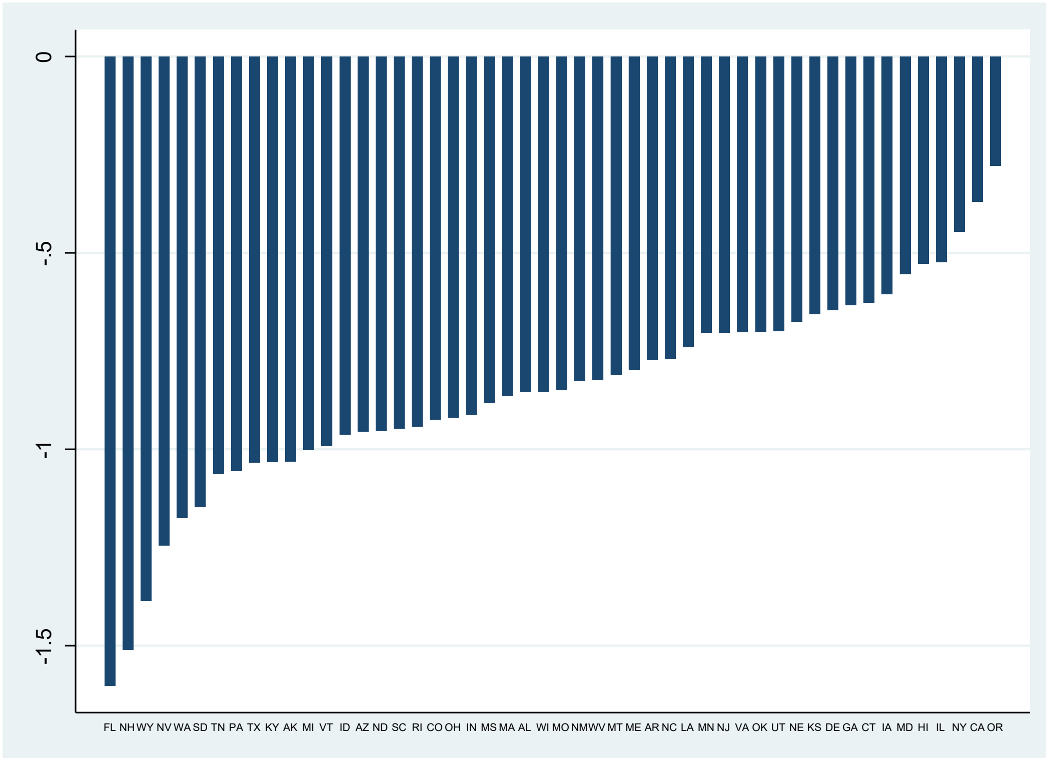
Figure 1: TCJA-Induced Change in Income Tax as Share of GDP across States

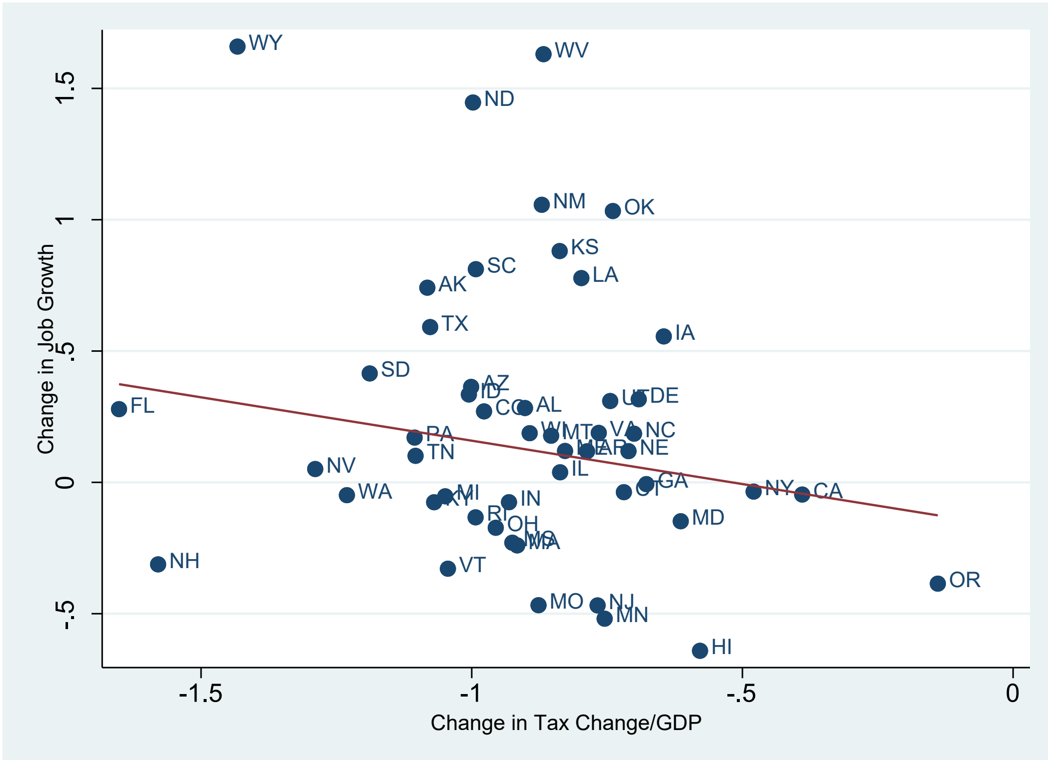


Source: SOI Tax Statistics; authors' calculations using NBER-TAXSIM.

Figure 2: Relationship between Change in Lagged Job Growth and Change in Tax Shock

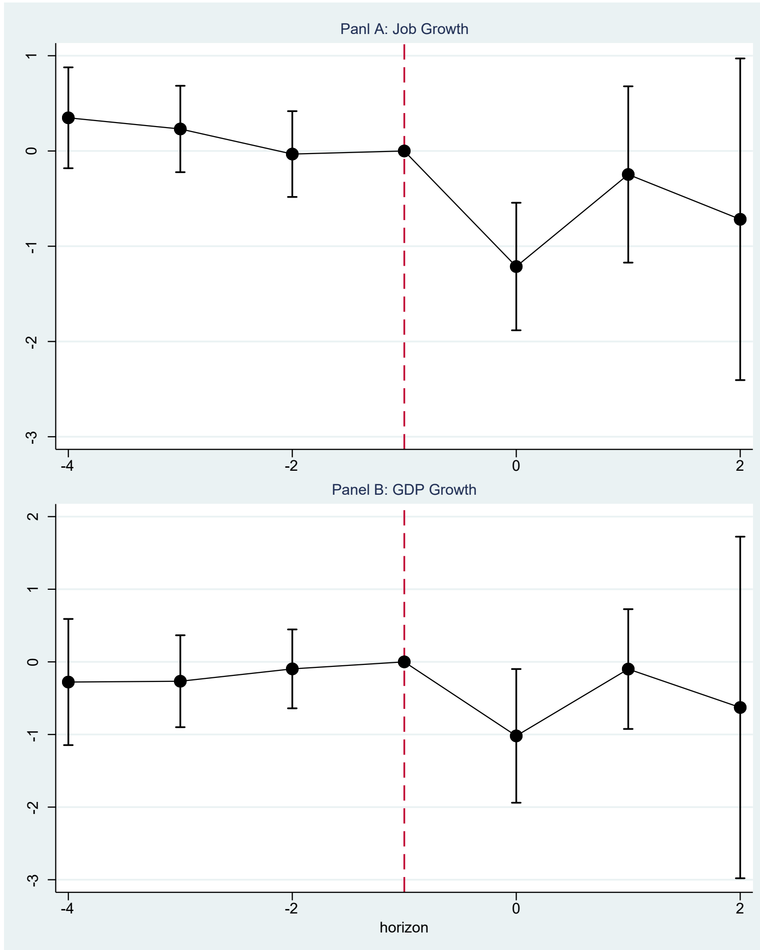
The figure plots change in change in one-year-lagged job growth from 2017 to 2018, i.e. change in job growth from 2016 to 2017 (Y-axis) against change in tax shock from 2017 to 2018 (X-axis). The linear fit is based on a linear regression of change in lagged job growth on change in tax shock, weighted by number of state-level tax returns.

Figure 3: Relationship between Change in Job Growth and Change in Tax Shock



The figure plots change in change in job growth from 2017 to 2018 (Y-axis) against change in tax shock from 2017 to 2018 (X-axis). The linear fit is based on a linear regression of change in job growth on change in tax shock, weighted by number of state-level tax returns.

Figure 4: Impulse Reponses from TCJA Tax Shocks



Note: * p<0.10, ** p<0.05. The dependent variable is job growth from year t to $t+h$ in Panel A and GDP growth from year t to $t+h$ in Panel B, and the Tax Shock/GDP variable is measured as tax change as share of GDP. Estimation sample consists of 50 states and years 2018-2020. The table presents impulse responses of job growth from tax shock using Local Projection regression. Standard errors clustered at the state level reported in parenthesis and estimates weighted by state-level number of tax returns. Other controls include dare: interaction between cyclicality quantile and year effects; interaction between a dummy variable for energy state and oil prices; interaction between 2016 manufacturing share and federal funds rate; and interaction between dummy for Republican control and year effects.

Table 1: Summary Statistics

	Mean	SD	Median	Min	Max
			2016		
Tax (Billions)	120.19	113.01	73.00	4.47	376.33
Change in Tax (Billions)	-0.30	0.36	-0.20	-1.21	0.35
Change in Tax/GDP (Percent)	-0.03	0.03	-0.03	-0.32	0.16
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)	119.47	113.21	73.81	4.32	382.10
Change in Tax (Billions)	0.40	0.52	0.29	-0.38	2.59
Change in Tax/GDP (Percent)	0.05	0.06	0.04	-0.14	0.31
Payroll Job Growth (Percent)	1.45	0.71	1.37	-1.23	3.20
GDP Growth (Percent)	2.29	1.53	1.95	-3.49	5.36
			2018		
Tax (Billions)	127.55	123.36	71.68	4.45	411.03
Change in Tax (Billions)	-6.79	5.51	-4.55	-18.70	-0.33
Change in Tax/GDP (Percent)	-0.82	0.33	-0.85	-1.60	-0.28
Payroll Job Growth (Percent)	1.56	0.75	1.37	-0.49	3.32
GDP Growth (Percent)	2.77	1.33	2.71	-1.63	6.79

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

Table 2: Relationship between TCJA-induced Tax Change and Lagged GDP/Job growth and Spending Growth/GDP

	(1) Lagged Job Growth	(2) Lagged GDP Growth	(3) Change in Spending/GDP
Tax Shock/GDP	0.114	0.128	-0.082
	(0.227)	(0.442)	(0.227)
State Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Observations	200	200	250
R-Sq	0.162	0.195	0.190

Note: * $p<0.10$, ** $p<0.05$. The table reports coefficients on tax shock (tax change/GDP) from a fixed effects regression of specified dependent variables on the tax shock variable. Standard errors are clustered at the state level and regression is weighted by state-level number of tax returns.

Table 3: Estimated Impact of Income Tax Changes on 2-year Job Growth

	(1)	(2)	(3)
Tax Shock/GDP	-1.680**	-2.756**	-1.295
	(0.560)	(1.298)	(0.828)
State Fixed Effects	No	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Other Controls	No	No	Yes
Observations	150	150	150
R-Sq	0.832	0.950	0.975

Note: * p<0.10, ** p<0.05. The dependent variable is 2-year job growth, and the Tax Shock/GDP variable is measured as the 2-year tax change as share of GDP. Estimation sample consists of 50 states and years 2018-2020. Column (1) of the table reports coefficient on tax shock from a simple cross-section OLS regression of job growth on the tax shock. Columns (2)-(3) reports coefficients on tax shock from a fixed effects regression of job growth on the tax shock. Standard errors clustered at the state level reported in parenthesis and estimates weighted by state-level number of tax returns. Other controls included in the regression in column (3): interaction between cyclicality quantile and year effects; interaction between a dummy variable for energy state and oil prices; interaction between 2016 manufacturing share and federal funds rate; and interaction between dummy for Republican control and year effects.

Table 4: Estimated Impact of Income Tax Changes on 2-year GDP Growth

	(1)	(2)	(3)
Tax Shock/GDP	-0.446	-2.181**	-1.460
	(1.212)	(0.706)	(0.890)
State Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Other Controls	No	No	Yes
Observations	150	150	150
R-Sq	0.601	0.928	0.934

Note: * p<0.10, ** p<0.05. The dependent variable is 2-year GDP growth, and the Tax Shock/GDP variable is measured as the 2-year tax change as share of GDP. Estimation sample consists of 50 states and years 2018-2020. Column (1) of the table reports coefficient on tax shock from a simple cross-section regression of GDP growth. Columns (2)-(3) report coefficients on tax shock from a fixed effects regression of GDP growth on the tax shock using. Standard errors clustered at the state level reported in parenthesis and estimates weighted by state-level number of tax returns. Other controls included in the regression in column (3) are: interaction between cyclicality quantile and year effects; interaction between a dummy variable for energy state and oil prices; interaction between 2016 manufacturing share and federal funds rate; and interaction between dummy for Republican control and year effects.

Table 5: OLS and Instrumental Variable Estimates of Income Tax Changes on Growth using First-Differenced Regressions

	(1)	(2)	(3)	(4)
	Job Growth	Job Growth	GDP Growth	GDP Growth
	OLS	IV	OLS	IV
Tax Shock/GDP	-1.818**	-0.795	-1.719**	-1.624
	(0.741)	(1.294)	(0.675)	(1.545)
Other Controls	Yes	Yes	Yes	Yes
Observations	100	100	100	100
R-Sq	0.936	0.933	0.870	0.870
Craig-Mcdonald-F		9.432		9.432
F_eff		24.718		8.579
c_TSLS_10		7.871		8.397
P-val-overid		0.144		0.934
P-val-Endog-Test		0.362		0.957

Note: * p<0.10, ** p<0.05. The dependent variable is first differenced 2-year job growth/GDP growth, and first differenced 2-year tax change as share of GDP. Estimation sample consists of 50 states and years 2019-2020. The table reports coefficients on change in tax shock from a first-differenced OLS regressions in columns (1) and (3) and IV regressions in columns (2) and (4). IV used are share of tax returns with income greater than $200,000 and maximum state income tax rate. Robust standard errors reported in parenthesis and estimates weighted by state-level number of tax returns. Other controls included in the regression are: cyclicality quantile; a dummy variable for energy state; 2016 manufacturing share; and a dummy for Republican control. IV regressions estimated using STATA ivreg2 software from Baum et. al. (2010).

Appendix Table A1: Sample NBER-TAXSIM Input and Output Variables based on Averages from SOI 2016 Data

State	AGI Group (Thousands)	Number of Returns	Filing Status	Deps$^{\Psi}$	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: $^{\Psi}$ 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 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.

Appendix Table 2: Estimated Impact of TCJA on 2-year Job Growth using County-level Data

	(1)	(2)	(3)
Tax Shock/GDP	-0.679**	-1.449**	-1.268**
	(0.207)	(0.288)	(0.322)
County Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Other Controls	No	No	Yes
Observations	9262	9262	9262
R-Sq	0.233	0.431	0.448

Note: * p<0.10, ** p<0.05. The dependent variable is 2-year job growth, and the Tax Shock/GDP variable is measured as the 2-year tax change as share of GDP. Estimation sample consists of US counties from and years 2018-2020. Column (1) of the table reports coefficient on tax shock from a simple cross-section OLS regression of job growth on the tax shock. Columns (2)-(3) reports coefficients on tax shock from a county fixed effects regression of job growth on the tax shock. Standard errors clustered at the state level reported in parenthesis and estimates weighted by county-level number of tax returns. Other state-level controls included in the regression in column (3): interaction between cyclicality quantile and year effects; interaction between a dummy variable for energy state and oil prices; interaction between 2016 manufacturing share and federal funds rate; and interaction between dummy for Republican control of state government and year effects.

Appendix Table A3: Instrumental Variable Estimates of the Effect of TCJA on 2-year Job Growth using County-level Data

	(1)
Tax Shock/GDP	-1.091**
	(0.522)
Other Controls	Yes
Observations	6174
R-Sq	-0.201
Craig-Mcdonald-F	41.682
F_eff	14.574
c_TSLS_10	14.284
P-val-overid	0.731
P-val-Endog-Test	0.009

Note: * p<0.10, ** p<0.05. The dependent variable is first differenced 2-year job growth/GDP growth, and first differenced 2-year tax change as share of GDP. Estimation sample consists of US counties from years 2019-2020. The table reports coefficients on change in tax shock from a first-differenced OLS regressions in columns (1) and (3) and IV regressions in columns (2) and (4). IV used are share of tax returns with income greater than $200,000 and maximum state income tax rate. Robust standard errors reported in parenthesis and estimates weighted by county-level number of tax returns. Other state-level controls included in the regression are: cyclicality quantile; a dummy variable for energy state; 2016 manufacturing share; and a dummy for Republican control. IV regressions estimated using STATA ivreg2 software from Baum et. al. (2010).