Federal Reserve Bank of Dallas

Did Tax Cuts and Jobs Act Create Jobs and Stimulate Growth? Early Evidence Using State-Level Variation in Tax Changes

Anil Kumar

Working Paper 2001 **January 2020**

Research Department

https://doi.org/10.24149/wp2001

Did Tax Cuts and Jobs Act Create Jobs and Stimulate Growth? Early Evidence Using State-Level Variation in Tax Changes*

Anil Kumar†

November 15, 2019

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. Following recent pioneering work of Zidar (2019), I exploit plausibly exogenous state-level variation in tax changes and find that an income tax cut equaling 1 percent of GDP led to a 1 percentage point higher nominal GDP growth and about 0.3 percentage point faster job growth in 2018.

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

JEL Classification: E62, H30

---

* I thank Pia Orrenius, Keith Phillips, and Mike Weiss for helpful comments and Dan Feenberg for invaluable help with the NBER-TAXSIM program. All remaining errors are my own. The views expressed here are those of the author and do not necessarily reflect those of the Federal Reserve Bank of Dallas or the Federal Reserve System.

† 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applied to income over $600,000 for married filers—a higher threshold than $480,050 in 2017.[1] The new tax law also repealed personal and dependent exemptions, increased the amount of child tax credit and considerably reduced the scope of the Alternative Minimum Tax (AMT).[2]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, the immediate 2018 effect of the law change can be a useful lower bound on

[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 TCJA's expected future effects, as the impact tends to strengthen in subsequent periods. And finally, 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.

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 2018 state-level spending. I use panel data on growth rates and tax shocks in 2017 and 2018 and estimate models with state fixed effects and year effects—equivalent to regressing the *change* in growth rate on the *change* in tax shock in 2018, rather than using their 2018 *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.

TCJA tax shocks are calculated using 2016 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 2016 GDP, varied widely across states—from 0.4 percent of GDP in California to 1.5 percent of GDP in New Hampshire. 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 percentage points higher nominal GDP growth and 0.3 percentage point faster job growth in 2018.

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 follows recent work of Zidar (2019):

$$y_{st} = \alpha + \beta\left(\mathrm{D}\tau_{st}/GDP_{st}\right) + \gamma X_{st} + \kappa_s + \mu_t + \epsilon_{st}, \qquad (1)$$

where the subscript s indexes states, t stands for year and the dependent variable y_{st} is a measure of change in economic activity i.e., nominal GDP growth or job growth, for state s in year t. The key explanatory variable, $\mathrm{D}\tau_{st}/GDP_{st}$, is a measure of state-level tax shock, defined here as the annual change in state-level total income tax liabilities ($\mathrm{D}\tau_{st}$) as a share of 2016 state-level GDP.[5] Finally, X_{st} are controls for other covariates that vary across states as well as over time and may be correlated with both $\mathrm{D}\tau_{st}/GDP_{st}$ and y_{st}; κ_s and μ_t are state and year fixed effects, respectively.

The fixed-effects specification 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 shocks, $\mathrm{D}\tau_{st}/GDP_{st}$. To minimize the influence of such omitted factors, I control for lagged measures of GDP growth or job growth. Additionally, I control

[5] Although almost all of the variation in tax changes are driven by changes in federal income taxes, the state-level tax shock ($\mathrm{D}\tau_{st}$) 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.

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 a dummy for energy-intensive 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 for 2018, tax changes due to the TCJA can be approximated using 2016 SOI data, which provides information on the number of taxpayers and their tax filing characteristics for different income groups at the state level. Taxes for the average 2016 taxpayer in different income groups for each state are calculated under the

2017 and 2018 tax laws using the NBER-TAXSIM model.[6] 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 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 for every one of 10 income groups in 2016.[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. Summing 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 different measures of taxes—e.g. total taxes, tax/AGI, and tax/GDP—did not change from 2016 to 2017, they dropped significantly from 2017 to 2018. Taxes as percent of GDP, for example, declined from an average of 13.5 percent to 12.7 percent.

The two outcome variables are nominal GDP growth and job growth. Nominal GDP growth is based on state-level data on nominal GDP 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

---

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

[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] Although BEA does publish real GDP measured in chained 2012 dollars, I focus on nominal GDP because, as noted in Zidar (2019), inflation adjustment at the state level can be imperfect due to well-known limitations in state-level price indexes.

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 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. As further evidence, Figure A1 in the appendix plots such coefficients for years 2010 through 2017 and shows that almost all the coefficients on the TCJA tax shock variable in the pre-TCJA period are insignificant.

Impact of TCJA-induced income tax changes on growth

The main results from estimation of the econometric specification in equation (1) are presented in Table 3 and 4. Column (1) of Table 3 reports coefficients on tax shock (tax change/GDP) from a simple cross-section regression of payroll job growth on the tax shock for only 2018. This 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 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. While Table 2 and Appendix Figure A1 show that TCJA tax shocks are largely uncorrelated with prior economic growth, omission of a variety of other state-specific factors correlated with state-level growth could cloud estimates in column (1), with the net result that the estimate is of expected sign, but statistically insignificant.

Accounting for such factors, columns (2)-(4) of Table 3 report coefficients on tax shock (tax change/GDP) from fixed effects regressions of payroll job growth on the tax shock using data from 2017 and 2018. Such fixed effects regressions using two-period data are numerically equivalent to estimating a simple cross-section regression of the change in growth rate on the change in TCJA tax shock in 2018.

Column (2) reports results with only state and time-fixed effects and shows that a tax cut equaling 1 percent of GDP leads to 0.4 percentage point faster job growth and the effect is statistically significant at a 5 percent level. Column (3) adds lagged job growth to the simple fixed effects regression in column (2) and shows that the estimated effect is little changed. This is not surprising, as we saw before that TCJA tax shocks were largely uncorrelated with lagged growth. To account for key macro shocks that may affect states differently, column (4) 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 (4) is smaller than in column (3), but remains statistically significant.

Overall, Table 3 suggests that tax cuts led to faster job growth of about 0.2-0.4 percentage points in 2018. Isomorphic to Table 3, Table 4 presents estimated effects for nominal GDP growth. The pattern of results in Table 4 largely mirror those for job growth in Table 3. Although the results for GDP growth are more imprecise than those for job growth, they remain remarkably stable from column (2) through column (4), with the estimate in the richest specification in column (4) implying an impact of about 1 percentage point on GDP growth, statistically significant at the 10 percent level.

For the richest specification in Tables 3 and 4, I also present results from randomization inference in Appendix Figure A2, which plots the empirical CDF of the coefficient on $\mathrm{D}\tau_{st}/GDP_{st}$ from 1,000 random permutations of the variable across states. The figure shows that the actual tax shock coefficient for job growth (GDP growth) presented in column 4 of Table 3 (Table 4) lies in the extreme of this distribution, with less than 13 percent (8 percent) of coefficients in random permuted regressions of job growth (GDP growth) more negative than that on the actual tax shock.

Instrumental variable estimates

The fixed effects estimates presented in Tables 3 and 4 can still be biased and inconsistent if there are other omitted time-varying confounders that are correlated with tax shocks and that also determine growth rates. 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, β. In order to motivate the use of potential instruments it is useful to write (1) in the first differenced form:

$$\Delta y_{st} = \alpha + \beta\,\Delta(\mathrm{D}\tau_{st}/GDP_{st}) + \gamma\Delta X_{st} + \mu_{\mathrm{t}} + \Delta\epsilon_{st}, \qquad (2)$$

To account for any remaining endogeneity in $\Delta(\mathrm{D}\tau_{st}/GDP_{st})$, I use two instruments: (1) share of tax returns with AGI \$200,000 or higher in 2016 (*share200K+*) and (2) a dummy variable for the state with no state income tax (*nositax*).

The identifying assumption is that conditional on $\Delta(\mathrm{D}\tau_{st}/GDP_{st})$, *share200K+* and *nositax* do not directly affect the *change* in growth rates, Δy_{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. Validity of *share200K+* as an instrument for the change in tax shock is based on the argument that regional variation in income distribution is plausibly exogenous—an assumption also made in Zidar (2019). As for *nositax*, the implicit assumption for validity is that any differences in growth rates between states with and without a state income tax are constant over time, so that *nositax* can be excluded from (2).

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, $\mathrm{D}\tau_{st}/GDP_{st}$. They would also be highly correlated with the change in tax shock, $\Delta(\mathrm{D}\tau_{st}/GDP_{st})$, as $\mathrm{D}\tau_{st}/GDP_{st}$ is practically zero in years prior to the TCJA.

Estimates from IV regressions are reported in Table 5. 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 $\Delta(\mathrm{D}\tau_{st}/GDP_{st})$ with an F-stat well exceeding 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 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. IV estimates for job growth are larger than simple fixed effects estimates in Table 4. The estimate with full controls in column (6) imply that TCJA income tax cuts led to 0.4 percentage point faster job growth in 2018, almost twice the effect implied by fixed effects estimates in column (4) of Table 3.

IV estimates for GDP growth in columns (4) and (5) of Table 5 are very close to analogous estimates in column (2) and (3) of Table 3. IV estimates reported in column (6) appear highly sensitive to a full set of controls, as the estimated effect is not only substantially smaller than that in column (5) of Table 6, but also notably lower than the fixed effects estimates from the richest specification in column (4) of Table 4. Given the relative instability and imprecision of the IV estimate in column (6), it is useful to formally test whether it is statistically different from the corresponding first-differenced OLS estimate—or equivalently the fixed effects estimate—with a full set of controls.

A test for endogeneity of $\Delta(\mathrm{D}\tau_{st}/GDP_{st})$ for the specification in column (6) of Table 5 yields a p-value of 0.37, implying that the variable is not endogenous (Hausman, 1978). High p-values from similar tests for IV specifications across other columns in Table 5 also imply non-rejection of the null hypothesis that the change in tax shock is exogenous. Thus, under the assumption that the instruments are valid, there is no statistical evidence that fixed effects estimates reported in Table 3 are contaminated by endogeneity. Therefore, I continue to use the fixed effects estimates reported in Table 3 and Table 4 as my preferred set of estimates, which on average suggest that TJCA tax shocks contributed to about 1 percentage point stronger GDP growth and 0.3 percentage point faster job growth in 2018.

5. Conclusion

Using 2016 SOI tax return statistics for states and NBER-TAXSIM model, this paper exploits state-level variation in TCJA tax shocks as a source for identification and makes an initial attempt to measure the TCJA's impact on economic activity in 2018. Using fixed effects models as well as instrumental variables, I find that an income tax cut equaling 1 percent of GDP led to 1 percentage point higher nominal GDP growth and about 0.3 percentage point faster job growth in 2018. Given that TCJA reduced individual income tax liabilities by 0.8 percent of GDP on average across states, it likely boosted GDP growth by 0.8 percentage point and job growth by roughly 0.24 percentage point in 2018, The estimated impact is well within the range of economists' predictions of TCJA impact on GDP growth for 2018.

References

Baum, C.F., Schaffer, M.E., Stillman, S. 2010. ivreg2: Stata module for extended instrumental variables/2SLS, GMM and AC/HAC, LIML and k-class regression. http://ideas.repec.org/c/boc/bocode/s425401.html

Barro, R. J., & Redlick, C. J. (2011). Macroeconomic effects from government purchases and taxes. *The Quarterly Journal of Economics*, *126*(1), 51-102. https://doi.org/10.1093/qje/qjq002

Carlino, G., & DeFina, R. (1998). The differential regional effects of monetary policy. *Review of economics and statistics*, *80*(4), 572-587. https://doi.org/10.1162/003465398557843

Feenberg, D., & Coutts, E. (1993). An introduction to the TAXSIM model. *Journal of Policy Analysis and management*, *12*(1), 189-194. https://doi.org/10.2307/3325474

Gale, W., Gelfond, H., Krupkin, A., Mazur, M. J., & Toder, E. (2018). A Preliminary Assessment of the Tax Cuts and Jobs Act of 2017. *National Tax Journal*, 71(4), 589-611. https://doi.org/10.17310/ntj.2018.4.01

Hansen, L. P. (1982). Large sample properties of generalized method of moments estimators. *Econometrica: Journal of the Econometric Society*, 1029-1054. https://doi.org/10.2307/1912775

Heß, S. (2017). Randomization inference with Stata: A guide and software. *The Stata Journal*, 17(3), 630-651. https://doi.org/10.1177/1536867x1701700306

Hausman, J. A. (1978). Specification tests in econometrics. *Econometrica: Journal of the econometric society*, 1251-1271. https://doi.org/10.2307/1913827

Mertens, Karel. 2018. The Near Term Growth Impact of the Tax Cuts and Jobs Act. Research Department Working Paper 1803. Dallas: Federal Reserve Bank of Dallas. https://doi.org/10.24149/wp1803

Mertens, K., & Ravn, M. O. (2013). The dynamic effects of personal and corporate income tax changes in the United States. *American Economic Review*, *103*(4), 1212-47. https://doi.org/10.1257/aer.103.4.1212

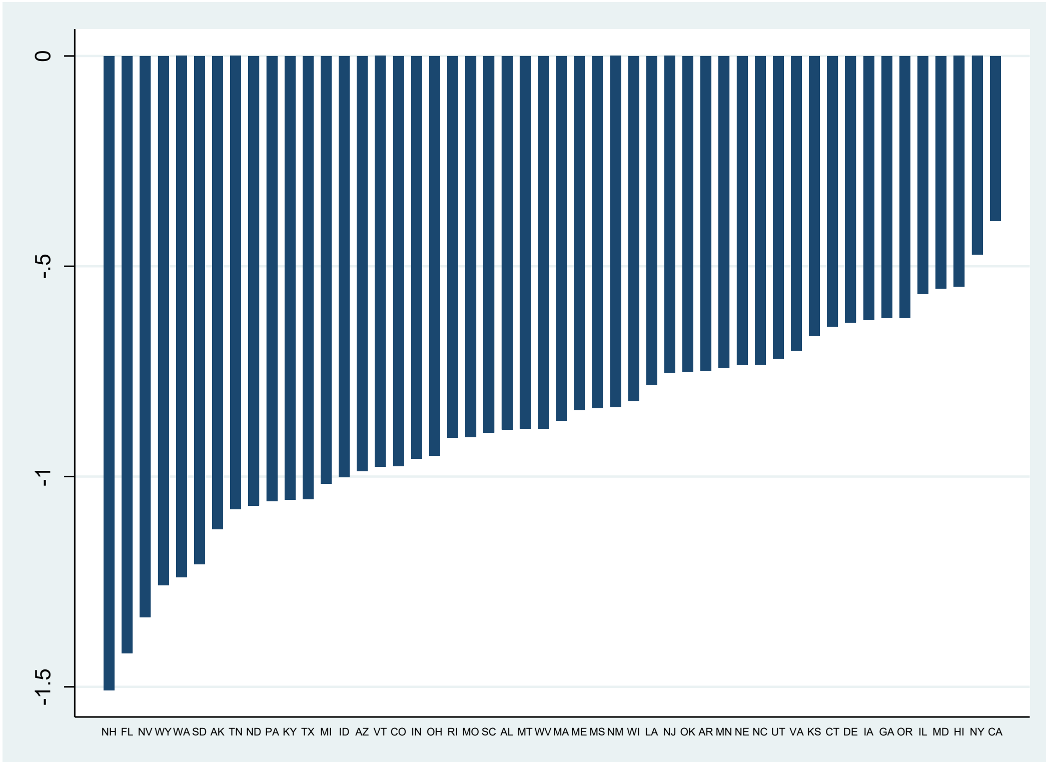
Olea, J. L. M., & Pflueger, C. (2013). A robust test for weak instruments. *Journal of Business & Economic Statistics*, *31*(3), 358-369. https://doi.org/10.1080/00401706.2013.806694

Romer, C. D., & Romer, D. H. (2010). The macroeconomic effects of tax changes: estimates based on a new measure of fiscal shocks. *American Economic Review*, *100*(3), 763-801. https://doi.org/10.1257/aer.100.3.763

Stock, J. H., Wright, J. H., & Yogo, M. (2002). A survey of weak instruments and weak identification in generalized method of moments. *Journal of Business & Economic Statistics*, *20*(4), 518-529. https://doi.org/10.1198/073500102288618658

Zidar, O. (2019). Tax cuts for whom? Heterogeneous effects of income tax changes on growth and mployment. *Journal of Political Economy*, *127*(3), 1437-1472. https://doi.org/10.1086/701424

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

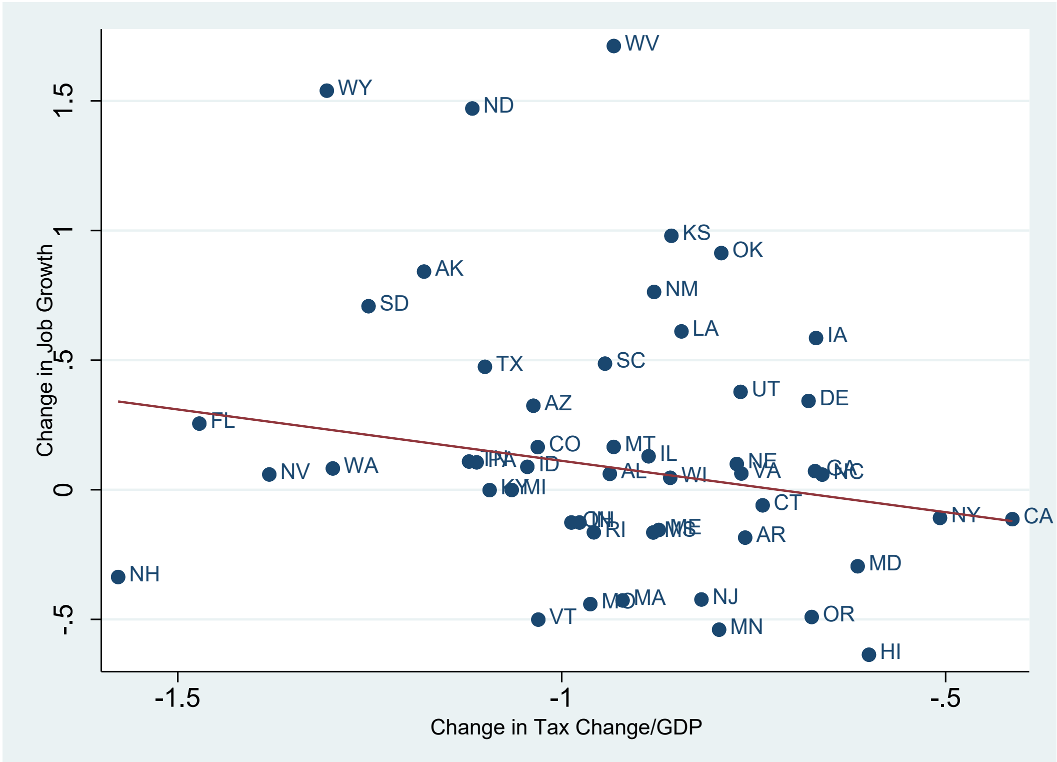


Source: 2016 SOI Tax Statistics; authors' calculations using NBER-TAXSIM.

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

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 across States



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.

Table 1: Summary Statistics

	Mean	SD	Median	Min	Max
			2016		
Tax (Billions)	119.11	111.97	73.58	4.3	381.55
Tax/AGI (Percent)	24.46	2.29	24.14	20.44	29.37
Tax/GDP (Percent)	13.44	1.61	13.6	9.95	18.13
Tax/Tax Return ('000)	16.79	3.87	14.83	10.41	27.25
Payroll Job Growth (Percent)	1.76	1.05	1.56	-4.21	3.5
GDP Growth (Percent)	2.72	1.98	3.08	-7.6	5.85
			2017		
Tax (Billions)	119.52	112.13	73.81	4.32	382.1
Tax/AGI (Percent)	24.56	2.3	24.23	20.52	29.52
Tax/GDP (Percent)	13.5	1.62	13.91	10.01	18.22
Change in Tax/GDP (Percent)	0.05	0.06	0.05	-0.07	0.32
Tax/Tax Return ('000)	16.86	3.89	14.89	10.45	27.39
Payroll Job Growth (Percent)	1.45	0.72	1.33	-1.28	3.3
GDP Growth (Percent)	4.27	1.48	4.19	1.28	6.81
			2018		
Tax (Billions)	113.32	109.16	67.89	3.87	371.67
Tax/AGI (Percent)	23.06	2.71	22.51	18.45	28.48
Tax/GDP (Percent)	12.67	1.73	12.86	8.88	17.58
Change in Tax/GDP (Percent)	-0.83	0.29	-0.84	-1.51	-0.39
Tax/Tax Return ('000)	15.87	4	14.11	9.72	26.42
Payroll Job Growth (Percent)	1.52	0.74	1.23	-0.44	3.36
GDP Growth (Percent)	5.42	1.29	5.22	2.76	8.22

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 ($D\tau_{st}/GDP_{st}$)	0.078	-0.527	0.169
	(0.488)	(1.473)	(0.361)
State Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Observations	100	100	100
R-Sq	0.240	0.316	0.021

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 TCJA-Induced Income Tax Changes on Job Growth

	(1)	(2)	(3)	(4)
Tax Shock ($D\tau_{st}/GDP_{st}$)	-0.622	-0.396**	-0.419**	-0.227**
	(0.625)	(0.092)	(0.128)	(0.105)
State Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	Yes	Yes
Lagged Job Growth	No	No	Yes	Yes
Other Controls	No	No	No	Yes
Observations	50	100	100	100
R-Sq	0.059	0.147	0.354	0.504

Note: * p<0.10, ** p<0.05. Column (1) of the table reports coefficient on tax shock (tax change/GDP) from a simple cross-section regression of job growth on the tax shock for the year 2018. Columns (2)-(4) reports coefficients on tax shock (tax change/GDP) from a fixed effects regression of job growth on the tax shock using data from years 2017 and 2018. 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 (4): 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 TCJA-Induced Income Tax Changes on GDP Growth

	(1)	(2)	(3)	(4)
Tax Shock ($D\tau_{st}/GDP_{st}$)	-0.910	-0.996	-0.967	-1.064*
	(1.132)	(0.754)	(0.737)	(0.577)
State Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	Yes	Yes
Lagged GDP Growth	No	No	Yes	Yes
Other Controls	No	No	No	Yes
Observations	50	100	100	100
R-Sq	0.042	0.657	0.664	0.747

Note: * p<0.10, ** p<0.05. Column (1) of the table reports coefficient on tax shock (tax change/GDP) from a simple cross-section regression of GDP growth on the tax shock for the year 2018. Columns (2)-(4) report coefficients on tax shock (tax change/GDP) from a fixed effects regression of GDP growth on the tax shock using data from years 2017 and 2018. 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 (4) 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.

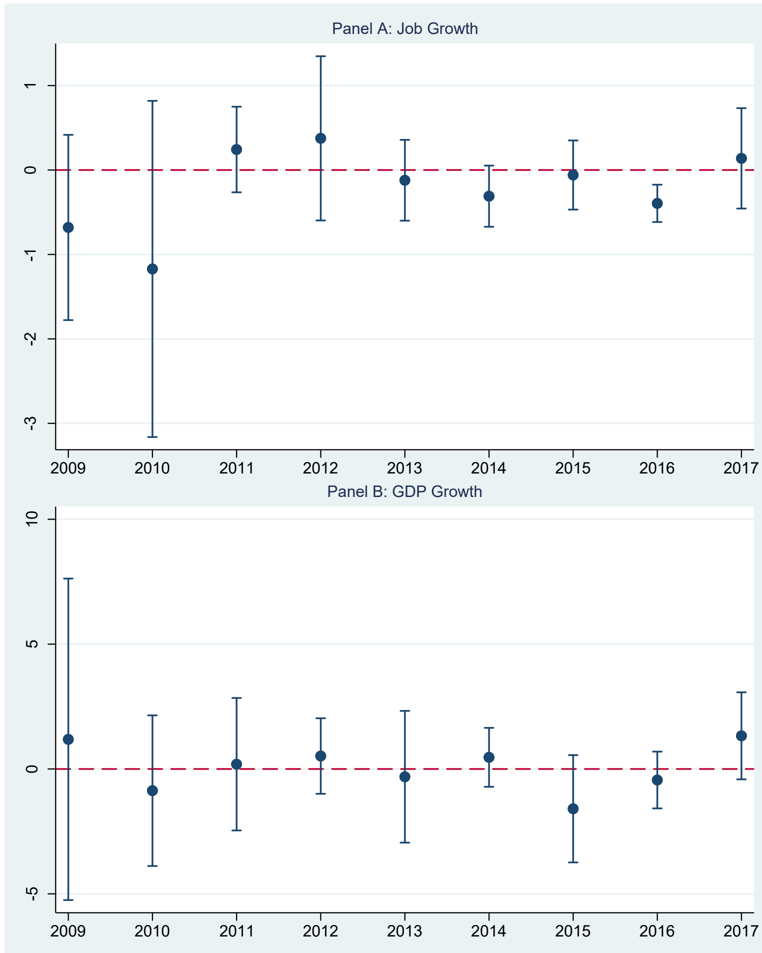
Table 5: Instrumental Variable Estimates of the Effect of Individual Income Tax Changes on Job Growth and GDP Growth

	(1)	(2)	(3)	(4)	(5)	(6)
	Change in Job Growth			***Change in GDP Growth***		
Change in Tax Shock ($\Delta(\mathrm{D}\tau_{st}/GDP_{st})$)	-0.637**	-0.631**	-0.387**	-1.003	-0.897	-0.498
	(0.212)	(0.208)	(0.173)	(0.866)	(0.881)	(0.971)
Change in Lagged Job Growth	No	Yes	Yes	No	No	No
Change in Lagged GDP Growth	No	No	No	No	Yes	Yes
Other Controls	No	No	Yes	No	No	Yes
Observations	50	50	50	50	50	50
R-Sq	0.073	0.298	0.472	0.104	0.123	0.317
First Stage Partial F on IV (homoscedastic)	29.890	29.447	16.257	29.890	29.591	15.698
Effective-F (Olea and Pflueger, 2013)	23.528	22.639	12.469	23.528	22.376	11.697
Critical Value for Worst Case Bias>10%	8.789	8.600	8.905	8.876	8.802	9.953
P-value on Over-identification Test	0.218	0.180	0.111	0.751	0.714	0.866
P-value on Test of Endogeneity	0.103	0.265	0.428	0.973	0.815	0.382

Note: * p<0.10, ** p<0.05. The table reports coefficients on change in tax shock (i.e. tax change/GDP) from a first-differenced IV regression of change in job growth (left panel) and change in GDP growth (right panel) on the change in tax shock (i.e. tax change/GDP) for the year 2018. IV used are share of tax returns with income greater than $200,000 and a dummy for no state income tax. Robust standard errors reported in parenthesis and estimates weighted by state-level number of tax returns. Other controls included in the regression in column (3) and (6) are: change in lagged job/GDP growth, 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).

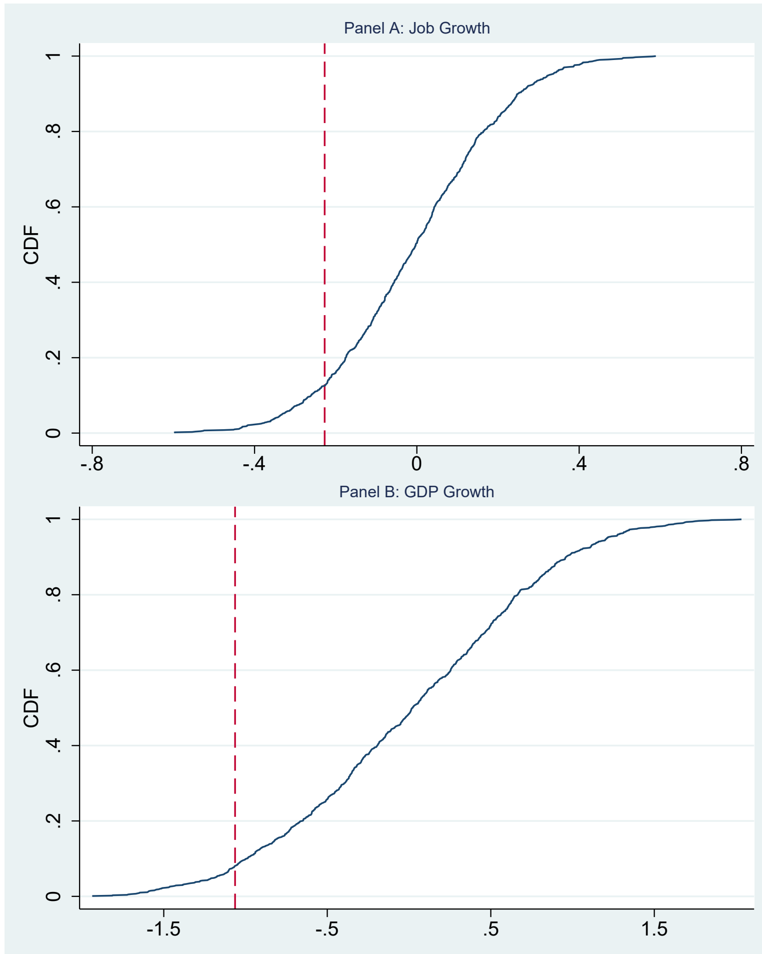
Appendix A

Figure A1: Estimated Effect of 2018 TCJA-Tax Change on Growth Rates Prior to 2018



Note: The figure plots coefficients on change in tax shock (i.e. tax change/GDP) from a first-differenced regression of change change in job growth (Panel A) and in GDP growth (Panel B) for each year from 2010 to 2017 on the 2018 change in tax shock (i.e. tax change/GDP). Other controls include—change in lagged outcome variable, cyclicality quantile, a dummy variable for energy state, 2016 manufacturing share, and a dummy for Republican control. The figure shows that, in almost all cases, TCJA tax shocks do not predict GDP growth or job growth in years prior to the TCJA.

Figure A2: Empirical Distribution of Estimated Coefficients by Randomly Permuting TCJA-Tax Changes across States



Note: The figure plots the empirical CDF of the coefficient on change in tax shock (i.e. tax change/GDP) from a first-differenced regression of change in job growth (Panel A) change in GDP growth (Panel B) on the change in tax shock (i.e. tax change/GDP) from 1000 random permutations of the 2018 change in tax shock across states. Other controls include—change in lagged outcome variable, cyclicality quantile, a dummy variable for energy state, 2016 manufacturing share, and a dummy for Republican control. The red dashed line denotes the coefficient on the 2018 actual tax shock. The figure shows that the actual tax shock coefficient lies in the extreme of this distribution, with less 13 percent (8 percent) of coefficients in random permuted regressions of job growth (GDP growth) more negative than that on the actual tax shock. The permutation test was conducted using software available from Heß (2017).

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[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 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.