Market Reactions as Macroeconomic Barometer: Quantifying the TCJA's Effect on GDP and Wages

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The Tax Cuts and Jobs Act (TCJA) is one of the most significant US tax reforms in 40 years. However, we know little about the TCJA's macroeconomic effects, presumably due to the difficulty in distinguishing the law's effects from other factors that affect the macroeconomy. In this paper, I create a new methodology that allows a researcher to use firms' market reactions to identify the effects of a macroeconomic shock on the broader economy. I apply this method to the TCJA to identify its effects on GDP and wages. I find that the TCJA increased GDP and total wages paid to employees by 2.2% and 3.4%, respectively. I find that the total wage increase was driven by a 1.7% increase in employment and a 1.3% increase in annual salaries. In other words, I find the TCJA created 2 million jobs and increased average annual salaries by $520.

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1. Introduction

The Tax Cuts and Jobs Act (TCJA) is one of the most significant United States tax reforms in nearly 40 years. The bill made major changes to both the corporate and personal income tax systems and was expected to reduce federal tax revenues by $1-$2 trillion over 10 years.[1] Nevertheless, proponents at the time argued that such a revenue reduction was justified, primarily because the revised tax code would increase GDP by 3.6%-5.6% and raise household income by $4,000 (Council of Economic Advisors, 2018). Policy experts and academics have since struggled to evaluate these forecasts because it is difficult to estimate the magnitude of the TCJA's effect on GDP and wages. It is not clear what fraction of such changes is due to the TCJA rather than any one of the numerous other contemporaneous shocks to the macroeconomy.[2] Furthermore, it is challenging to control for alternate shocks because the TCJA affected all firms and individuals in the economy, severely limiting the set of suitable control groups that were unaffected by the TCJA. This problem is not unique to the TCJA – many macroeconomic changes are notoriously difficult to empirically estimate because of contemporaneous confounds and the lack of adequate control groups.

In this paper, I overcome these challenges by creating a new methodology, which I call the stakeholder market reaction approach, that uses a market event study to identify the TCJA's effects on two stakeholder outcomes, GDP and wages.[3] I show theoretically and empirically that, under fairly straightforward assumptions, the market event study can be used to cleanly quantify the ex-post outcomes of major regime changes. The simple intuition of my approach is that

[1] https://www.taxpolicycenter.org/briefing-book/how-did-tcja-affect-federal-budget-outlook

[2] Examples of other potential confounding events that might have affected post-TCJA GDP and wages (positively or negatively) include the US-China trade war, the renegotiation of NAFTA, the general government deregulation in this period, and, in later years, the COVID-19 pandemic.

[3] I use the term stakeholder to refer to all individuals who receive a portion of a firms' contribution to GDP (called its gross value added or GVA). Under this definition, the three primary groups who make up the stakeholders of a firm are shareholders (in the form of dividends), workers (in the form of wages), and the government (in the form of taxes).

because market reactions indicate how a regime change affected *shareholder value,* they also contain information that can be used to estimate how a regime change affected *stakeholder value*.

Market event studies assume that 1) markets are efficient, and 2) the market response is not contaminated by confounding events (Kothari and Warner, 1997). The first assumption requires at least semi-strong form efficiency; to make the second assumption plausible, market event studies often analyze stock returns over relatively short windows, such as a few hours or days. If satisfied, these two assumptions allow researchers to identify the effects of an exogenous shock to firm value without a control group. That is, under these assumptions, market reactions to major regime changes (such as the TCJA) are plausibly exogenous from other shocks.

The key insight of this paper is that a given shock that affects shareholder value, as identified by market reactions, will typically have broader implications for other stakeholders. Therefore, by making basic statistical assumptions about how shareholders and stakeholders jointly share the consequences of a regime change, I can combine market reactions with broader stakeholder outcomes (i.e., GDP and wages) to quantify the regime change's effects on the macroeconomy. Essentially, the idea is a new application of the traditional notion of shared incidence, in which the burden of a tax shock is shared across many stakeholders.[4]

While Section 3 formalizes this method into a theoretical model, to develop intuition, consider the following example. Suppose there exist three islands, each of whom 1) reports their GDP annually, and 2) has a single representative firm with observable stock market returns every

[4] The economic incidence of a shock refers to how a shock changes the welfare distribution in an economy. For example, if a corporate tax increase reduces economic output, shareholders will receive fewer dividends and workers will receive lower wages. The magnitude by which wages and dividends decrease relative to one another is referred to as the incidence of the tax. Corporate tax incidence in particular has been widely studied for many decades (see Fullerton and Metcalf, 2002; Kotlikoff and Summers 1987; Harberger 1962).

day. Next suppose that each island implements the TCJA at the same time, and that firms' market reactions to the TCJA are both observable and identifiable with a market event study.

Let Islands, 1, 2, and 3, have stock market reactions of 3%, 5%, and 1%, respectively. Further let, Islands 1, 2, and 3 have a change in GDP from the year before the island enacts the TCJA to the year after they enact the TCJA of 7%, 11%, and 3%, respectively. Using this data, regress each island's change in GDP on their market reaction to the TCJA in the following regression $\Delta GDP_i = \beta_0 + \beta_1 TCJA\ Returns_i + \varepsilon_i$. Note that in this regression, $\overline{TCJA\ Returns} = 3\%$, $\beta_1 = 2$ and $\beta_0 = 1\%$. β_1 states how a change in $TCJA\ Returns_i$ is associated with a change in GDP. Therefore, by multiplying $\beta_1 * \overline{TCJA\ Returns}$, one can see that the average effect of the TCJA on the islands' GDP is 6%. Furthermore, β_0 captures all other factors that affected GDP that were not related to the TCJA. The model in Section 3 formalizes this intuition and shows that this method will recover the full effect of a regime change on GDP and wages.

To empirically implement this intuition, I begin by calculating firms' stock market reactions to the TCJA. Because there are relatively few days over which the information about the TCJA is released, it is unlikely that such returns are affected by other events, and thus the effect of the TCJA is relatively isolated in its market reaction. Next, I link those market reactions to other stakeholder outcomes, namely GDP and wages, which are reported by geography. I do so by computing the market reaction of a "representative firm" for each county. Specifically, I allocate firms' market reactions to the counties in which they operate based on the proportion of their employees who work in each county.[5]

[5] The use of a representative firm is a simplifying assumption that allows me to move between units of observation (i.e., from firm-level to stakeholder-level). There are some limitations to this assumption, namely potential bias in the choice of a representative firm, the lack of data for private firms, and potential spillover effects. I discuss these concerns later in the paper and provide some alternative analyses that aim to deal with them.

With that set up, I am ready to estimate the effect of the TCJA on important stakeholder outcomes, namely GDP and wages. Using a county's stock returns to the TCJA to capture its "exposure to treatment", I perform a fuzzy difference-in-differences analysis with county level aggregate wages and GDP as the dependent variables. I find that a 1% higher market reaction to the TCJA associates with a 0.41% and 0.64% increase in GDP and total wages paid, respectively. With an average market reaction of 5.23%, these results indicate that the TCJA increased GDP and aggregate wages by an economically significant, 2.2% and 3.4% respectively, on average. Prior to the TCJA, forecasters predicted the TCJA's effect of GDP using theoretical models. Model predictions ranged between -0.1% to 3.0%, meaning that when empirically well-identified, the effects of the TCJA on GDP and wages is actually in the upper quartile of most forecasts.

In my next set of analyses, I seek to better isolate the mechanism for these results. First, I assess whether the increase in total wages was due to an increase in worker salaries, new employment, or both. I find evidence that the TCJA increased the wage rate and employment by 1.7% and 1.3%, respectively. An increase of 1.7% in employment equates to a total of 2 million new jobs created, while a 1.3% increase in wages implies an increase of $520 per person.

Second, I perform a firm-level analysis using measures of profitability and investment that are likely related to the changes in GDP and wages. Specifically, I parallel the county level analysis, by using firms' market reaction to the TCJA as their measure of "exposure to treatment" in a fuzzy difference-in-differences analysis, with firms' financial statement line items as the dependent variables. In short, this test allows me to connect how firms' exposure to the TCJA manifested in their financial statements. I find that firms' revenues increased without a commensurate increase in expenses, which manifested as an increase in pretax income. The

increase in pretax income led to an increase in tax expense and net income.[6] I also find that the TCJA increased firms' total capital expenditures and cash spent on acquisitions, and that these are funded primarily through the sale of stock. Together these results are consistent with the TCJA having spurred additional profitability and investment.

While this study provides a unique and novel approach to isolating the impact of the TCJA and other shocks, it also faces some limitations. These include 1) stock market reactions are only available for public firms and may be imperfectly linked to macroeconomic outcomes, 2) stock returns may impound information about the TCJA outside of the dates chosen by prior literature, 3) firms' market reactions to the TCJA may be correlated with differential exposure to other future shocks, or 4) counties may have spillover effects on one another. I provide additional tests for each of these potential empirical threats and demonstrate that while my estimates are likely not a perfect point estimate, they at least serve as a lower bound for the effects of the TCJA on GDP and wages. Although these tests potentially lessen concerns about these limitations, they cannot be ruled out entirely.

This paper makes two primary contributions to the literature. First, this paper develops a novel research framework to analyze the ex-post consequences of macroeconomic shocks. Many important events, such as recessions, tax changes, wars, and changes in regulations are important because they affect all individuals and corporations in a given location. Unfortunately, by virtue of their large impact, these events make careful causal analysis about their macroeconomic implications difficult. The TCJA is one such event. Subsequent researchers can use this framework to study macroeconomic shocks that do not have readily identifiable treatment groups.

[6] See Section 7 for a discussion on why this does not imply that that decreasing corporate tax rates increases corporate tax revenues.

Second, this paper contributes to the growing literature on the effects of the TCJA. Because it is one of the most significant US tax reforms since the Tax Reform Act of 1986, researchers have published a variety of studies about the TCJA (Dyreng et al., 2020; Wagner et al., 2020; Eldar and Garber, 2019); however, examining its primary economic consequences has proven elusive because of empirical limitations (e.g., lack of a control group). This study contributes by documenting and quantifying the act's effects on aggregate wages and GDP. The TCJA's proponents strongly emphasized how the bill would increase both of these outcomes. This paper documents that while the bill's effects were large and positive, its proponents exaggerated its impact. Furthermore, this paper is timely in highlighting the potential negative consequences on economic growth of allowing many of the TJCA's provisions to expire in 2025.

2.0 Background

The TCJA made several changes to the personal and corporate tax systems. The bill reduced the top corporate statutory rate from 35% to 21%, limited interest deductibility, expanded bonus depreciation, introduced tax incentives to limit companies' abnormal foreign earnings (i.e., BEAT, FDII, and GILTI), and switched the US from a worldwide to a territorial system. On the personal side, the bill cut individual tax rates, altered tax brackets, eliminated personal exemptions, doubled the standard deduction, instituted qualified business income deductions for pass-throughs, and capped state and local tax deductions. In summary therefore, the bill made several significant changes to the US tax code.

Understanding the macroeconomic effects of tax change is a significant factor in setting tax policy. For example, Slemrod (2018) argues that the single most important economic factor in determining whether the TCJA passed were forecasts of how the bill would affect aggregate macroeconomic statistics like GDP and wages. Such forecasts are important because they affect

how a bill is expected to affect the federal budget deficit and therefore whether it can pass through budget reconciliation.

The White House Council of Economic Advisors (CEA) argued that the TCJA would increase GDP for several reasons (Council of Economic Advisors, 2018). First, corporate rate cuts and bonus depreciation allow firms to earn higher after-tax returns from their investments. An increase in the after-tax return on capital investment reduces firms' pretax cost of capital, not only making more projects positive NPV, but also attracting additional foreign direct investment. Second, to the extent that the corporate rate and base changes reduce firms' income shifting, it can create more efficient US investment[7]. Finally, because the individual income tax cuts increase workers' after-tax wages, they argue the individual income tax changes will increase labor supply. Combining these effects, the White House CEA estimates that the corporate tax cuts were expected to increase GDP by 2-4% by increasing investment while the individual tax cuts were expected to increase GDP by 1.6%.

Although not as large as the White House's claims that the TCJA would increase GDP by 3.6% - 5.6%, others also forecasted moderate increase in GDP. Slemrod (2018) lists a series of popular forecasts for how the TCJA would affect GDP (displayed in Table 1 Panel A). These forecasts had a relatively wide distribution, ranging from an increase of 0.1% on the low end to as much as 3.0% on the high end. For context, this range is greater than the average annual GDP growth of the US in this period.[8]

Although explicit forecasts were less common for wages, the academic literature has a similarly large range of estimates for how corporate taxes affect wages. The corporate tax

[7] These benefits may be offset by increased government debt or decreased government spending. As government debt increases it can crowd out private investment and when government spending decreases there may be fewer public goods available.
[8] GDP grew between 1.5%-2.7% between 2010-2016.

incidence literature has long recognized that although one party remits a tax, they may not bear the economic burden of a tax (Dyreng et al., 2022; Harberger, 1962). For example, if a corporation responds to a tax increase by lowering wages, workers may bear the economic burden of the corporate tax, even if companies pay the tax to the government. It is a subject of intense debate whether and to what extent corporations can shift the burden of corporate taxes from shareholders to workers. Table 1 Panel B lists a series of estimates for what fraction of the corporate tax burden is borne by workers in the form of lower wages. Although some papers argue that workers bear no burden (Clausing, 2013) or over 100% of the burden (Hassett and Mathur, 2006), most argue that workers bear somewhere between 40%-80% of the total corporate tax burden. Prior to its passage, it was therefore unclear not only how the TCJA affected GDP, but also how the law affected workers wages.

Furthermore, since the TCJA's passage, although subsequent literature has analyzed many facets of the TCJA's effects, there is relatively little evidence of its macroeconomic consequences. Most papers provide descriptive evidence on how firms changed before and after the act (Dyreng et al., 2020; Wagner et al., 2020) or causally analyze the effects of small well identified provisions such as opportunity zones (Eldar and Garber, 2019). Still others provide theoretical models (Furno, 2021) without empirical evidence. Some macro level evidence was provided by a very early Federal Reserve working paper by Kumar (2019) that uses the NBER TAXSIM methodology pioneered by Zidar (2019) to estimate how the change in personal income taxes in the TCJA affected employment and GDP.

In a contemporary working paper, Kennedy et al. (2022), use micro level tax return data to compare the differences in firm-level outcomes for S- and C-Corporations in the same size and industry group. Because C-Corporations received a relatively larger tax cut, the authors

attribute these results to the corporate tax change provision of the TCJA. They find that workers bear 44% of the federal corporate tax burden, most of which is concentrated among high income groups, and that for every $1 decrease in taxes, total firm output increased by $0.1.

This paper is different from Kennedy et al. (2022) in three ways. First, Kennedy et al. (2022) only examines the changes in marginal tax rates on business income while my results apply to the TCJA as a whole, including the relatively large individual tax changes and changes to the corporate tax base. Second, because no S-corporation is as large as the largest C-corporations, their paper does not document the effect on large firms. Third, while Kennedy et al. document micro-level effects, this paper documents macro-level effects. This difference is crucial because firm-level estimates underestimate the effects of spillovers (Armstrong et al., 2019; Glaeser & Guay, 2017; Angrist et al., 1996). For example, even if only C-corporations were given a tax cut, one would expect S-corporation wages to increase as the demand for labor increases. Because the control group is partially "treated", any firm level analysis would underestimate the effects on workers, especially when comparing firms in the same size and industry group, where firms are most likely to be competitors. Regardless, given the importance of the TCJA and difficulties in identifying its effects, triangulating the effects of the TCJA using many different methodologies and data sources is crucial to our understanding of the effects of the bill.

3.0 Market Event Studies and Identification

The TCJA made several changes to the tax code that may have affected GDP and wages. GDP and wages are however affected by many different factors in a given year. It is therefore difficult to distinguish whether a change in GDP or wages is due to the TCJA or some other change in the macroeconomy. Furthermore, it is challenging to control for these macroeconomic

changes because the TCJA affected nearly every individual and company in the US, severely limiting researchers' ability to identify a suitable control group.

Market event studies can identify the effects of the TCJA on shareholders without a control group (e.g., Kothari and Warner, 2007). In an event study, a researcher identifies a short time horizon, typically a few days or hours, in which information was released about an economic event. If market prices fully incorporate the new information, and no other information is released during the selected time horizon, then any stock market returns during that window are attributable to the economic event in question. These stock returns then measure how the market expects the event to affect shareholder welfare. In the introduction I presented an example with three islands to provide intuition for how identifying the effects of a shock on shareholder welfare can provide useful information for identifying its effects on stakeholder welfare. The following model formalizes that intuition.

Let there be a series of firms indexed by i.[9] Let Y_i denote a firm's gross value added (GVA), V_i denote the amount of gross value added remitted to shareholders (also called the gross operating surplus), and W_i all other factors included in gross value added (primarily consisting of worker's compensation). Then tautologically:

$$Y_i = V_i + W_i$$

Let there be some exogenous shock Z that affects all firms in an economy, then:

$$dY_i = \frac{\partial Y_i}{\partial Z} dZ + (dY_i)|_{non-shock\ factors} = \frac{\partial W_i}{\partial Z} dZ + \frac{\partial V_i}{\partial Z} dZ + (dY_i)|_{non-shock\ factors}$$

A regression of a firm's change on its gross value added would then yield a coefficient

[9] The term firm here is used for ease of intuition, but the model can equally apply to the geographies where GDP and wages are typically reported such as counties. In this case, it is the change in GVA and its subcomponents rather than the change in profits that are relevant.

$$\beta_1 = \frac{cov\left(dY_i, \frac{\partial V_i}{\partial Z} dZ\right)}{var\left(\frac{\partial V_i}{\partial Z} dZ\right)}$$

By assumption, let $cov\left((dY_i)|_{non-shock\ factors}, \frac{\partial V_i}{\partial Z} dZ\right) = 0$. That is to say, let stock market returns to the shock be exogenous from all other factors that affect gross value added. Then:

$$\beta_1 = \frac{cov\left(dY_i, \frac{\partial V_i}{\partial Z} dZ\right)}{var\left(\frac{\partial V_i}{\partial Z} dZ\right)} = \frac{cov\left(\frac{\partial W_i}{\partial Z} + \frac{\partial V_i}{\partial Z}, \frac{\partial V_i}{\partial Z}\right)}{var\left(\frac{\partial V_i}{\partial Z}\right)} = 1 + \frac{cov\left(\frac{\partial W_i}{\partial Z}, \frac{\partial V_i}{\partial Z}\right)}{var\left(\frac{\partial V_i}{\partial Z}\right)}$$

$$\beta_1 * \frac{\partial V_i}{\partial Z} dZ = \frac{cov\left(dY_i, \frac{\partial V_i}{\partial Z} dZ\right)}{var\left(\frac{\partial V_i}{\partial Z} dZ\right)} * \frac{\partial V_i}{\partial Z} dZ = dZ * \left(\frac{\partial V_i}{\partial Z} + \frac{\partial V_i}{\partial Z} * \frac{cov\left(\frac{\partial W_i}{\partial Z}, \frac{\partial V_i}{\partial Z}\right)}{var\left(\frac{\partial V_i}{\partial Z}\right)}\right)$$

This equation implies that since $\frac{\partial Y_i}{\partial Z} = \frac{\partial W_i}{\partial Z} + \frac{\partial V_i}{\partial Z}$, a necessary and sufficient condition for this regression to recover the average effect of the TCJA on gross value added is that $\frac{\partial V_i}{\partial Z} * \frac{cov\left(\frac{\partial W_i}{\partial Z}, \frac{\partial V_i}{\partial Z}\right)}{var\left(\frac{\partial V_i}{\partial Z}\right)} = E[\frac{\partial W_i}{\partial Z}]$. Now, let $\frac{\partial W_i}{\partial Z} = \alpha_i \frac{\partial V_i}{\partial Z}$, where α_i represents the fraction of the shock that is not borne by shareholders. Then, the necessary condition becomes $\frac{\partial V_i}{\partial Z} * \frac{cov\left(\alpha_i \frac{\partial V_i}{\partial Z}, \frac{\partial V_i}{\partial Z}\right)}{var\left(\frac{\partial V_i}{\partial Z}\right)} = \frac{\partial V_i}{\partial Z} E[\alpha_i] = E[\frac{\partial W_i}{\partial Z}]$. A sufficient condition is therefore for the non-shareholder share to be uncorrelated with firm's stock market reaction, i.e., $cov\left(\alpha_i, \frac{\partial V_i}{\partial Z}\right) = 0$.

In other words, there are two main conditions for the regression to recover the average effect of a shock on GVA. First, the stock market reaction to the shock must be uncorrelated with all other factors that affect GVA besides the shock. In the case of the TCJA, this means that I implicitly assume that the impact of the trade war and other changes on firms is not associated with firms' market reaction to the TCJA. Second, the labor share of the shock is uncorrelated

with the level of firms' market reaction to the shock. More specifically, for the estimates to be a lower bound, the correlation between labor share and market reaction cannot be positive. In other words, it cannot be the case that the firms with the highest market reaction to the TCJA are also the firms' whose shareholders received the smallest fraction of the benefits from the TCJA. The inverse is likely true, and the firms with the highest market reaction likely had shareholders who received the largest fraction of the benefits from the TCJA; therefore, my estimates likely represent a lower bound.

4.0 Measurement

To implement the empirical strategy discussed in Section 3, I must 1) identify firms' market reactions to the TCJA, 2) identify GDP and wages at some subnational geographic level (i.e., state, county, zip code, etc.), and 3) allocate firms' market reactions out to geographies to proxy for the market reactions of a representative firm for the geography. With those three components, I can regress a geography's GDP on its representative firm's returns, which can use the strategy discussed in Section 3 to recover the effects of the TCJA on GDP and wages.

I begin by identifying firms' market reactions to the TCJA. As discussed in Gaertner et al. (2020), Congress passed the TCJA over a relatively short period of time with clearly identifiable days on which information relevant to its passage was released. Specifically, on September 27, 2017, the White House and Republicans in Congress released the "United Framework for Fixing our Broken Tax Code". H.R. 1 was introduced in the House of Representatives by Kevin Brady of Texas on November 2, 2017, passed in the House on November 16, 2017, passed in the Senate on December 2, 2017, and ultimately signed by the President on December 22, 2017. Using Google Trends, Gaertner et al. (2020) identified these,

among others, as the days when information was released about the TCJA. I create the measure *Firm TCJA Returns* as firms' cumulative market return on these days.[10]

Next, the most granular data on GDP is presented at the county level. To allocate firms' stock returns to counties, I use the National Establishment Time Series (NETS). NETS is a database with information on companies' establishment location, number of employees, and sales in any given year. Not only has the database been used in prior literature to assess the impact of state taxes on firms (Garrett et al., 2021; Heider & Ljungqvist, 2015; Suarez Serrato, 2018; Farre-Mensa & Ljungqvist, 2016; Ljungqvist et al., 2017; Armstrong et al., 2019), but it is also used by the Bureau of Economic Analysis (BEA) in portions of its county-level GDP calculations.[11] I start by matching Compustat to NETS using company names.[12] This yields a NETS match for 47% of Compustat firms. I provide details on NETS and the matching process in Appendix B. After matching, I use the establishment address in NETS to identify the number of employees per county by GVKEY in Compustat.

For each county, I then take the natural log of employment weighted average TCJA returns for all firms located in that county (i.e.

$$TCJA\ Returns = ln\left(1 + \frac{\sum_{i=1}^{N} Firm\ TCJA\ Returns_i * Employees_{i,c}}{\sum_{i=1}^{N} Employees_{i,c}}\right)$$

where i denotes public firms and c denotes a county).[13] An example of how this is done is presented in Figure 2. To check whether these returns might be in any way spatially correlated, I

[10] In doing so, I implicitly assume that absent the TCJA, the average stock market return on these days would have been near 0.
[11] https://apps.bea.gov/scb/issues/2020/03-march/0320-county-level-gdp.htm
[12] I use 2013 as the base year throughout the paper to eliminate any concerns that the results may be driven by anticipation effects, but results are consistent using 2016 as the base year as well. I also eliminate 2017 from the sample to mitigate any anticipation effects.
[13] I use natural log so that the coefficient is interpretable as an elasticity.

map them in Figure 3. As can be seen, *TCJA Returns* has significant variation across counties, but this variation does not appear to be spatially correlated.

5.0 Empirical Design and Data

5.1 Data and Descriptives

After creating *TCJA Returns,* I use the Bureau of Labor Statistics' (BLS) Quarterly Census of Employment and Wages (QCEW) to calculate county-level total wages, and the BEA's CAGDP1 file to identify county-level real GDP. I take the natural log of these variables, and use them as the two primary dependent variables, *Log GDP* and *Log Wage*

Basic descriptive statistics are provided as of 2014 in Table 2 Panel A. As can be seen, the average county has *TCJA Returns* of 5.6%, GDP of $5.5 billion and total wages paid to employees of $2.2 billion. These wages can be decomposed to show that the average county has 44 thousand employees who make an average of $38 thousand. A correlation matrix of each county-level variable as of 2014 is shown in Table 2 Panel B and indicates that all macroeconomic variables are highly correlated. This is perhaps unsurprising in that the counties with the highest GDP also have the most employees and the highest total wages paid.

To make the correlation matrix more relevant to my empirical tests, I regress each macroeconomic variable on county and year fixed effects from 2014 to 2022. I then take the residuals from these regressions and display them in a correlation matrix (Table 2 Panel C) to get the within fixed effect correlations. They indicate that the within fixed effect correlations are still strong, but much weaker than those displayed in Panel B. For example, the correlation between the level of employment and GDP in Panel B was 0.99 while the within fixed effect correlation is 0.5.

5.2 Design

I operationalize the model in Section 3 by performing a fuzzy difference-in-differences at the county-year level around the TCJA, using *TCJA Returns* to measure the degree of treatment. Specifically, I run the following regression:

$$GDP_{c,t}(Total\ Wages_{c,t}) = \beta_0 + \beta_1 TCJA\ Returns_c * Post_t + \gamma_c + \eta_t + \varepsilon_{c,t} \quad (1)$$

Where γ_c and η_t denote county and year fixed effects, respectively. The objective of this paper is to assess the aggregate effects of the TCJA, not to assess the average effect of the TCJA on a given county. Therefore, in order to ensure that the coefficient more heavily weights counties with larger GDP, I use weighted least squares to weight the regression by GDP.[14] β_1 represents the elasticity between stock returns and GDP or wages. The sample period is 2014 to 2021, allowing for four years pre and post treatment.[15]

Importantly, the theory described in Section 3 implies that including firm-level determinants of *TCJA Returns* in equation (1) is not only unnecessary but will actively bias the results downward. This is because including firm-level controls will implicitly control for the mechanism through which the TCJA affects GDP and wages (see discussion on controls in Dyreng et al., 2010).[16,17]

[14] There are also technical reasons to avoid an unweighted county analysis. Small counties are much more likely to exhibit extremely large percentage increases in GDP and wages meaning the results in an equal weighted OLS are entirely driven by very small counties. Additionally, measurement error in how TCJA returns is allocated is likely much greater in small counties where NETS employment surveys are less likely to be accurate.

[15] TCJA Returns are not randomly determined but are a result of firm characteristics. The longer the pre-period, the more likely it is to be contaminated by shocks that affected firms with high returns differently from firms with low returns. To avoid this possibility, I limit the pre-period to four years.

[16] For example, suppose I interacted the average ETR of the firms in a county in 2016 with Post, effectively controlling for firms' pre-treatment ETR. A tax cut is more beneficial for high ETR firms than low ETR firms, meaning all else equal, we would expect a greater increase in GDP for counties with firms who have high ETRs compared to those with low ETRs. If ETR were controlled for, the beta coefficient would no longer capture this portion of effect of the TCJA on GDP. This logic holds similarly for firm size, leverage, and any other factor that may determine firms' returns to the TCJA. Taken to the extreme, one could control for all of the heterogeneity in firms' stock returns leaving no variation for identification. For this reason, I do not include firm-level controls in the main specification.

[17] There are two primary concerns for which I might wish to include controls. First, one might worry that firms with low market reactions to the TCJA may have different pre-trends than firms with high returns. This does not appear to be the case empirically. Second, one might worry that subsequent events affected firms with high market reactions differently than firms with low market reactions. This is possible, but if parallel trends is satisfied, the

When examining wages, equation (1) can also be used to make a statement about what fraction of the total change in GDP accrues to labor. Specifically, the formula for labor share of the TCJA is $\frac{\%\ Change\ Total\ Wages}{\%\ Chang\ \ GDP} \times \frac{Total\ Wages}{GDP}$. *Total Wages* divided by *GDP* (in the pre-period) is 0.41.[18] Because both are multiplied by market returns to calculate the percent change in wages and GDP, $\frac{\%\ Change\ Total\ Wages}{\%\ Change\ GDP}$ will be equal to the coefficient on the wage regression divided by the coefficient on the GDP regression.

6.0 Main Result

6.1 Main Test

The main tests of equation (1) are presented in Table 3, with *Log GDP* presented in columns (1) and (3) and *Log Total Wages* in columns (2) and (4). The results are presented both with and without fixed effects but are not sensitive to this choice. In the GDP regression, the main coefficient is 0.414 (t-statistic 2.32), which when multiplied by employment weighted average TCJA returns (5.2%) yields an increase in GDP of 2.2%. When spread over the four years for which we have data in the post period, this indicates that the TCJA increased GDP growth by approximately 0.5% per year. Meanwhile the wage regression has a coefficient of 0.641 (*t*-statistic 3.60). This represents a 3.4% increase in workers' wages, or an increase in wage growth of 0.8% over the four years for which we have data in the post period. Using the labor share formula mentioned earlier $\frac{\%\ Change\ Total\ Wages}{\%\ Chang\ \ GDP} \times \frac{Total\ Wages}{GDP}$, Table 3 indicates that the labor share is roughly 64.1%. Because *TCJA Returns* is subject to measurement error, these estimates likely represent a lower bound for the fraction of benefits given to workers. Combined,

subsequent events would have to be unusual compared to events prior to treatment because parallel trends implies high and low county returns were on the same trajectory prior to the enactment of the TCJA. I explore these in additional analyses.

[18] This is calculated by dividing the sum of QCEW total wages in 2014-2016 by the sum of GDP in 2014-2016. This is consistent with Federal Reserve Economic Data (FRED), which states that wages and salaries are 42% of GDP (https://fred.stlouisfed.org/series/A4102E1A156NBEA).

these results, suggest strong economic benefits from the TCJA that were, in major part, shared by workers.

6.2 Decomposing the Wage Regression

So far, I have shown that wages increased by approximately 3.4%; however, it does not necessarily follow that worker welfare has improved. Depending on the labor supply and demand elasticities faced by the firms who received the benefits of the tax cut, the total level of wages may have increased because more people were employed or because the average wage rate increased. To test this hypothesis, I repeat the regressions in Table 4, with *Log Employment* and *Log Annual Wage* in Table 4.

Table 4 corroborates the findings in Table 3. The increase in employment shows a coefficient of 0.258 (*t*-statistic 2.74), while the coefficient on annual wages is 0.316 (*t*-statistic 2.58). In terms of magnitudes, this indicates that employment increased by 1.7% while average annual wages increased by 1.3%. In the pre-treatment period, QCEW had average total employment of 120 million people with an average wage of $38,470, meaning that the TCJA increased employment by 2 million and average annual wages by $520.

6.3 Parallel Trends

I perform the test of parallel trends of equation (1) by graphing the β_t's from the following regression:

$$GDP_{c,t}(Wage_{c,t}) = \beta_0 + \beta_t TCJA\ Returns_c * \mathbb{I}(Year)_t + \gamma_c + \eta_t + Controls_{ct} + \varepsilon_{c,t} \quad (2)$$

The results of this test are presented in Figure 4 Panel A for GDP and Panel B for Wages, respectively. Prior to 2017, the β_t coefficients are roughly equal to 0. Both coefficients increase sharply between 2016 and 2018 and continue to increase. GDP appears to flatten out just above 0.4, while Total Wages appear to dip in 2020, only to return in 2021, ending at approximately

0.8. Figure 4 presents the test of parallel trends for employment and annual wage. Similar to the results in Figure 4, Figure 5 demonstrates that employment and annual wages were essentially 0 prior to 2017 and spiked in 2018. Employment appears to level out near 0.3 at the end of the sample period, while annual wages see a sharp uptick to 0.5 in 2021. It is unclear in both tests to what extent COVID 19 introduced noise in 2020 and 2021 that may have affected the results.

7.0 Mechanism

While county level analyses derive estimates for the aggregate effects of the TCJA, they do not reveal the mechanism through which it creates those effects. To better understand this mechanism, I use an exploratory firm level analysis. Specifically, I explore how the TCJA affected firms by decomposing financial statements into line items in Compustat and run the following regression:

$$Line\ Item_{i,t} = \beta_0 + \beta_1 Firm\ TCJA\ Returns_i * Post_t + \gamma_i + \eta_t + \varepsilon_{i,t} \quad (3)$$

Where γ_i and η_t are firm and year fixed effects, respectively, and *Line Item* is the inverse hyperbolic sine of each respective line item for each firm-year. I use the inverse hyperbolic sine rather than log because line items frequently take on negative values and inverse hyperbolic sine approximates logs when there are negative values (Glaeser & Omartian, 2022; Pence, 2006). The results of this test are reported in Table 5. Each panel of Table 5 examines a summary line item (i.e, net income), and a decomposition of that summary line item into various line items that sum to the summary line item.

Panel A reports the effects of the TCJA on the income statement. Net income increases were large, with total firm net income increasing by approximately 9.2% (coefficient of 1.872 × *Firm TCJA Returns* of 4.9%, all future percent changes in Table 5 are calculated similarly). This increase is driven by a 9.6% increase in pretax income that is offset by a 5.7% increase in tax

expense. Of note, this implies that the average effect on pretax income was so large that it offset the decrease in the tax rate.[19] The increase in pretax income is driven by a 2.7% increase in revenue, with a statistically insignificant increase of 1.2% in COGS. Note that because the coefficient when *Net Income* is regressed on *TCJA Returns* is greater than 1, this implies that realized earnings increased more than the expected increase in discounted future earnings. This could be because 1) the market underestimated how large the effect would be, 2) an unobservable increase in discount rates offset the increase in income, 3) I am only observing a short time window and the market expects these effects to attenuate over the long run (if for example the TCJA provisions were allowed to expire in 2025).

Panels B and C show the results for investing and financing cash flows, respectively. It demonstrates that while I could not detect a statistically significant change in investing cash flows, I find that capital expenditures increased by 1.5% and that total acquisitions increased by 3.5%. These increased capital expenditures and acquisitions appear to be funded by a 6.5% increase in financing cash flows. Specifically, the TCJA appears to have increased the sale of firm's stock by 3.3% and other financing activities by 4.0%. It also appears to have caused firms to refinance their debt, increasing debt issuances and debt reductions of 3.3% and 3.3%, respectively. Together these results are consistent with the hypothesis that the TCJA made more projects positive NPV, and so encouraged firms to increase their economic activity.

[19] This does not mean that decreases in corporate tax rates increase corporate tax revenues for several reasons. First, I only observe corporate tax expense for public companies, and the TCJA reduced rates for both corporations and individuals. Because the returns encompass both the corporate tax cuts and personal tax cuts (the latter of which were expected to increase *corporate* tax revenues), one cannot draw inferences about just the effects of the change in the corporate rate. Second, the firm level analysis does not calculate aggregate effects, only the average effect on each firm. Third, GAAP tax expense is not equivalent to corporate tax revenues. For example, because of one-time repatriations and revaluations of DTAs and DTLs, an increase in tax expense does not mean that the long-term impact of the TCJA was to increase corporate tax revenue Nevertheless, the increase in GAAP tax expense suggests that the elasticity of corporate taxable income to tax rates is likely very large, which is consistent with prior literature (Coles et al., 2022).

In summary therefore, the TCJA appears to allow firms to raise capital that is used to finance more acquisitions and to a lesser extent capital expenditures. At the same time, I also find an economically significant increase in firms' revenue and gross margin that leads to an increase in pretax income. Not only does this increase in pretax income mean that firms have a higher after-tax income, but in fact, the increase in pretax income is so large that firms appear to pay more in taxes despite the rate cut. This is consistent with the tax foundation's argument that, "the 2017 tax reform did not substantially reduce the revenue potential of the corporate tax", because of increased economic activity and base broadening.[20]

8.0 Additional Analysis

8.1 Measurement Error

A significant threat to my estimates' validity is measurement error. The model in Section 3 states that if I can identify the firms in a county's change in profits from a shock, then I can recover the effects of that shock on other macroeconomic outcomes. I proxy for this change in profits by using the stock returns of the public firms' whose employees are located in that county. This creates at least two potential sources for measurement error. First, private firms, who do not have observable stock returns, not only might react differently to a given shock, but in the case of the TCJA, were generally treated differently under the law because most are structured as pass-through organizations. Second, while Gaertner et al. (2020) already identified a series of dates on which information was released about the TCJA, it is possible that their list was not comprehensive. In the following sections, I attempt to address both of these concerns.

8.1.1 Private Firms

To address the concern that private firms do not have observable stock returns I conduct two separate tests. Both tests rely on the fact that for each county, I can observe the number of

[20] https://taxfoundation.org/corporate-tax-revenue-tcja/

employees reported by NETS, meaning that using information from QCEW, I can identify the fraction of a given county for which I have observable stock return and employment information.

My current main specification assumes that private firms in a given county have equal stock returns to public firms. This may not be the case. Because private firms are primarily pass-through organizations, instead of being taxed at the corporate rate, their income is taxed at the personal income tax rates of their owners. Therefore, rather than a rate reduction from 35% to 21%, they were affected by the tax cuts of their owners and the 20% deduction of qualified business income (QBI). It is therefore likely that on average, the marginal rate reduction for private businesses was smaller than those of corporations.[21] While this only introduces mean zero measurement error if private firms are randomly distributed across counties, if the distribution of private firms is associated with *TCJA Returns,* this could influence my estimates.

To account for this possibility, I perform a sensitivity analysis in which I show how the results change based on the assumed ratio between the unobservable average private firm returns in a county and the average public firm returns in a county. Specifically, I re-estimate equation (1) but calculate the market return of the county's representative firm as, $Alternate\ TCJA\ Returns = TCJA\ Returns \times (\text{NETS Employment Fraction} + (1 - \text{NETS Employment Fraction}) \times \text{Return Ratio})$, where *NETS Employment Fraction* is the number of employees in a county across all public firms in NETS divided by the number of employees in the county using QCEW, and *Return Ratio* is the assumed ratio of returns between firms not listed in NETS to those listed in NETS. Said differently, if public firms in a county have an average return of 5%, and *Return Ratio* is 2, then private firms are assumed to have a return of 10%. If the county's employment is 80% from public firms and 20% from private

[21] A reduction in the top marginal bracket from 39.6% to 37% plus a 20% qualified income deduction would mean that on a marginal dollar of income, total taxes were reduced roughly 25% (1-(80%*37%)/39.6%=25%) compared to the 40% reduction of the corporate rate (1-21%/35%=40%).

firms, the county's return would be assumed to be 6%, while the returns of a county with 20% employment from public firms and 80% from private firms would be assumed to be 9%.

The results of this sensitivity analysis are displayed in Table 6. They show that for a *Return Ratio* between 0 and 2, the results are strikingly similar. The estimates of GDP growth range from 1.9% to 2.8%, meaning the original estimate of 2.2% is towards the lower end of the set of estimates. With the exception of a *Return Ratio* of 0% (which yields a wage growth estimate of 1.7%), the estimates for wage growth are even more consistent, ranging from 3.2% to 3.5%. The results do indicate however that the implied labor share is increasing in *Return Ratio,* suggesting that if private firms had lower returns than public firms, the workers may have received a smaller fraction of the benefits of the TCJA.

Nevertheless, because the analysis in Table 6 still requires me to make assumptions about how private firm returns are related to public firm returns, I supplement this analysis by controlling for the presence of private firms in Table 7. Controlling for the presence of private firms, mitigates the concern that my results are driven by measurement error that is correlated with *TCJA Returns,* without having to make an assumption about private firm returns. The two specific controls I use are *NETS Employment Fraction,* and *% Pass-Through,* where *% Pass-Through* is the fraction of a county's AGI that is designated as pass-through income according to the IRS Statistics of Income (SOI) database. Across specifications, these controls do not seem to change inferences about the magnitude of the TCJA's effects. In summary, I therefore do not find evidence that my results are being driven by measurement error related to private firms.

8.1.2 Alternate Return Measurement

While Gaertner et al. (2020) identify a subset of days that were significant in the passage of the TCJA, there may be other days when markets recognized a tax cut became likely. If

returns on these unidentified days are perfectly autocorrelated with returns on the identified days then my inferences will be unchanged (the coefficient will be larger and the average returns smaller). If they are not autocorrelated however, there may be potentially biased measurement error. To mitigate such concerns, I supplement the Gaertner et al. (2020) return dates using betting markets. Specifically, using data from the online political betting market, PredictIt, I collect data on days where the probability that the TCJA would pass increased significantly. I then supplement the returns on these days and rerun the analysis. If the returns identified in Gaertner et al. (2020) captured most of the returns to the TCJA, then adding additional days identified by betting markets should not attenuate the estimates. The results of this analysis are presented in Table 8. The results are the same (or stronger) when using the betting market returns as when using the Gaertner et al. (2020) returns. This is consistent with Gaertner et al. (2020) measuring TCJA returns with mean zero or negative measurement error, attenuating the estimates.

8.2 Simultaneous Treatment

A common threat in any difference-in-differences design is that even if parallel trends appear to be satisfied, if another shock occurs that affects the treatment group in the post period, it may contaminate the inferences. In the case of the TCJA, a potential concern is other shocks such as COVID-19 or the China trade war could affect firms with high market reactions to the TCJA differently than those with low market reactions to the TCJA. It is clear from Figure 4 Panel A that most of the aggregate efficiency gains occurred in 2018 and 2019 (rather than 2020), meaning that COVID-19 is likely not a primary concern; however, the China trade war occurred in 2018.

To control for the possibility that my results could be driven by firms with high market reactions to the TCJA being less negatively affected by the trade war, I follow Amiti et al (2020) to identify firm level market reactions to the China trade war. I then allocate these returns out to counties in the same process as *TCJA Returns* to construct *China Trade War Returns*. Additionally, to control for other potential shocks, I take the firm level returns over the 15-month period from January 2017 to March 2018 and allocate them to counties to construct *15 Month Returns.* As mentioned previously, when including such controls there is a risk that I may control for the mechanism through which the TCJA affects GDP and wages; however, as can be seen in Table 9 my inferences are similar once these controls are included.

8.3 Spillovers

A significant potential concern in this study is the presence of negative spillovers. If the TCJA induced workers and capital to shift from counties who had positive market reactions to counties that had negative market reactions, then I may identify a positive relation between changes in GDP/wages and firms market reactions, but the total aggregate effect could be zero. To assess this possibility, I conduct two tests: 1) controlling for migration, and 2) testing for negative spillovers of adjacent counties.

First, a primary mechanism through which the TCJA may create negative spillovers is through immigration. To control for this possibility, I use IRS SOI migration data to assess, both the net number of returns that moved into a county and the net amount of AGI that moved into a county. I scale these by the number of people in the prior year who did not migrate to create *Fraction Migrants* and *AGI Migration*. I then repeat the analyses in Table 4 and 5 after controlling for these factors and the results are presented in Table 10.[22] All of the coefficients

[22] Note that because IRS immigration data is only available through 2020 that I lost one year of observations.

appear to mildly attenuate relative to Table 3 and 4, but remain statistically significant after controlling for migration.

As an additional test for negative spillovers, I examine adjacent counties. While I can observe and control for migration, I cannot control for the possibility that people continued to reside in one county and began working in an adjacent county. If this is the case, then as firms in a county have positive returns, its adjacent counties should see decreases in the GDP, employment, and wages. To test for this possibility, I collect the set of adjacent counties and construct a measure of the average *TCJA Returns* in adjacent counties (which I call *Adjacent TCJA Returns*). In Table 11, I then repeat the test in Table 4 and 5, testing whether the coefficient is negative. I find no evidence to support this, and in fact find some weak evidence to support the idea that there may have been positive spillovers (which would attenuate the estimates). This is especially true for the annual wage rate, where adjacent counties may have the ability to positively affect workers' labor supply elasticities. Together with the migration test, this mitigates concerns of negative spillovers.

9.0 Conclusion

Economic forecasters and prior literature agreed that the TCJA would increase wages and GDP, but strongly disagreed about the magnitude. Literature subsequent to the TCJA has similarly struggled to identify its effects. In this paper, I use a novel empirical method to identify the degree to which a county was treated by the TCJA. I then assess how changes in GDP and wages vary with counties' degree of treatment. I find that the TCJA led to an increase of at least 2.2% in GDP and 3.4% in total wages. This indicates that workers received at least 64.1% of the benefits from the TCJA. I find that the increase in wages was due to the creation of approximately 2 million jobs and an increase in average annual wages of $520.

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Appendix A: Variable Definitions

Variable	Description	Measured	Unit Of Observation	Data Source
Log GDP	Ln(1+GDP), GDP is in thousands of chained 2012 dollars, i.e. real GDP	Annually	County-Year	BEA CAGDP1 File
Log Total Wages	Ln(1+Total Wages) where Total Wages is measured at the county level. Total Wages=Employment*Annual Average Wage	Annually	County-Year	BLS QCEW File
Log Employment	Ln(1+Employment) where Employment is measured at the county level	Annually	County-Year	BLS QCEW File
Log Annual Wage	Ln(1+Annual Average Wage), where Annual Average Wage is measured at the county level	Annually	County-Year	BLS QCEW File
TCJA Returns	Ln (1+County level employment weighted average of Firm TCJA Returns)	2017	County	CRSP, NETS
% Pass-Through	The total amount of pass-through income in a county divided by the AGI in the county according to the IRS Statistics of Income (SOI) file in 2013	2013	County	IRS SOI
NETS Employment Fraction	The number of employees in a county from all NETS firms that could be matched to Compustat, divided by the number of employees listed in a county in 2013.	2013	County	BLS QCEW File, NETS, Compustat
Betting Market TCJA Returns	County level employment weighted average of firm level returns to the TCJA. Firm level returns are measured as the cumulative returns from the days on the interval from [0,1] from Gaertner et al. (2020) or identified by the betting market PredictIt as important. PredictIt days are identified as important if they had a probability of passage increase of 5% or more and a volume of over 100.	2017	County	Compustat, NETS
15 Month Returns	Ln (1+County level employment weighted average of Firm 15 Month Returns). Firm 15 Month Returns are the stock returns for a given firm from January 1, 2017 to March 31, 2018	2017	County	CRSP, NETS
China Trade War Returns	Ln (1+County level employment weighted average of Firm China Trade War Returns). Firm China Trade War Returns are the stock returns for a given firm on the interval [-1,5] around the dates identified in Amiti et al. (2020)	2018	County	CRSP, NETS

Fraction Migrants	The total number of inflow tax returns minus the number of outflow tax returns divided by the prior year's total number of non-migrant returns.	Annually	County-Year	IRS SOI Migration Database
AGI Migration	The total amount of inflow AGI from tax returns minus the total amount of outflow AGI from tax returns divided by the prior year's total amount of non-migrant AGI.	Annually	County-Year	IRS SOI Migration Database
Adjacent TCJA Returns	The weighted average TCJA Returns of adjacent counties. Weights are the number of employees in a county in the NETS database. Adjacent counties are identified using the Census adjacent counties datafile.	2013	County	CRSP, NETS, Census Adjacent County File
Firm TCJA Returns	The cumulative stock market reaction on the TCJA event days identified in Gaertner et al. (2020).	2017	Firm	CRSP

Appendix B: Additional Analyses

1.0 NETS

The NETS is a database containing information about establishment locations, their sales, and their number of employees on a year-by-year basis from 1990 to the present. While NETS itself provides very little information about their origin or process, Barnatchez et al. (2017) published a federal reserve working paper describing the data, its origin, and where it may be useful. Much of the information from this appendix stems from Barnatchez et al. (2017) and is included here to provide background on NETS.

To summarize their description, NETS is compiled by Dun & Bradstreet (D&B) who sell the information for a variety of purposes, including to help firms get credit ratings. They collect the information from a variety of sources, including yellow pages, credit inquiries, and directly calling the firm. D&B not only has a strong incentive to ensure the information's accuracy, but individual businesses who provide the information also have an incentive to truthfully report the information because it can affect their access to credit.

In addition to the above description, Barnatchez et al. (2017) also compare the NETS data to Census and BLS data resources. They find that NETS is strikingly similar to both resources, with strong correlations between official sources and NETS data. The authors do however, recommend that users take two precautions: 1) use only a static sample, and 2) avoid using small firms. The authors discuss how the problem with using a dynamic sample is that D&B must manually update changes at the establishment level year over year, which can lead to significant measurement error. To eliminate this concern, I fix the NETS sample in 2013, eliminating problems with business dynamics. Second, because small firms are measured with less precision, they are subject to significant errors from imputation. This concern is not applicable to this

setting, where I use exclusively public firms that are much larger than those Barnatchez et al. (2017) raise concerns about.

An additional difficulty with using NETS is to match the establishments names in NETS to the company names in Compustat. To this end, I start by fuzzy matching the headquarters name in NETS of all establishments that have self-identified as belonging to a public firm to Compustat. I then manually verify all matches. For unmatched headquarters names in NETS with large fractions of total NETS employment, I then match the data to Compustat by performing internet searches on whether the NETS headquarters name is a subsidiary of a Compustat firm. The match using 2013 NETS data yields the following: 1) 80% (81%) of NETS (employment-weighted) establishments have a matched in Compustat, 2) the matched NETS employees make up 24% of all employees in the United States, 3) 47% (42%) of (asset weighted) Compustat firms in 2013 with positive assets are matched to NETS. Appendix B Tables 1 demonstrates that compared to Compustat more broadly, NETS is more heavily represented in the Food, Retail, and Fabricated product industries, but less represented in the mining, utilities, and oil industries.

Finally, readers familiar with NETS may question why I chose to use employment weighting rather than sales weighting, given that NETS provides both. There are two main reasons. First, theoretically, because changes in wages and GDP are driven by the location of factors of production, not sales; employment weighting is a better theoretical construct. Second, NETS lists whether a given figure for an establishment is the exact figure or an estimate imputed using other data from D&B; while over 70% of employment data is the exact figure, less than 1% of sales data is exact. Therefore, to avoid potential measurement error, I use the employment data.

While the NETS database has several flaws that require adjustments on the part of a researcher, as mentioned in the body of this paper, not only has its reliability been established in prior literature, but the BEA even uses NETS for its calculation of county-level GDP. Additionally, its ability to link a very large subset of public firms to the geographic locations in which they operate allows for new empirical techniques that can estimate the causal impacts of macroeconomic events that do not have clearly identifiable control groups.

2.0 Firm Level Returns Determinants

The central premise of the identification strategy discussed in this paper is that any macroeconomic shock will have heterogeneous impacts on firms, and by extension firm owners. To explore this heterogeneity, in Appendix B Table 2, I display firms' market reactions to the TCJA on various firm level characteristics. Several associations become clear. First, firms size appears to be consistently important in explaining returns, with firms' market reactions declining with size. This fact is consistent with prior literature that documents that larger firms are less able to adapt to tax changes (Coles et al., 2022). Second, industry fixed effects appear to have large explanatory power, particularly when regressions are weighted by firms' total assets. Third, other predictors such as *ETR, Market-to-Book, Leverage, Foreign Income,* and *Change in MTR 2013 to 2019,* all appear to load in some specifications, but are sensitive to whether regressions are weighted and include industry fixed effects. When significant, these variables generally behave as theoretically predicted, with firms with higher effective tax rates, lower market-to-book (i.e., less expected future taxable income), and larger decreases in marginal tax rates all seeing generally higher returns. The one counterintuitive result is that market reactions appear to be positively correlated with leverage; however, this result is consistent with prior literature that has found that taxes depress borrowing (Ivanov et al., 2020).

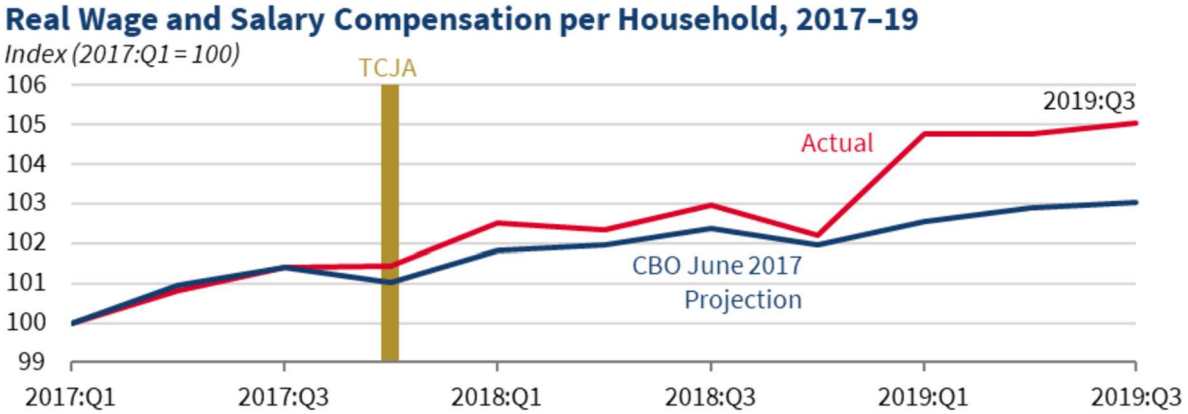
3.0 Additional Mechanism Analysis

The analysis documented in Table 5 decomposes firm financial statements into various line items and then estimates equation (3) to assess how the TCJA affected these line items at the firm level. While Table 5 presents line items from the income statement and investing and financing sections of the statement of cash flows, Appendix B Table 3 presents the results for the balance sheet and operating section of the statement of cash flows. Consistent previous results, this table highlights that firms with higher market reactions to the TCJA saw increases in their short- and long-term debt, and their shareholder's equity. These firms also saw an increase in their cash and other assets. Finally, confirming prior results, these firms saw an increase in their net income on the operating statement of cash flows.

Figure 1: Prior Estimates

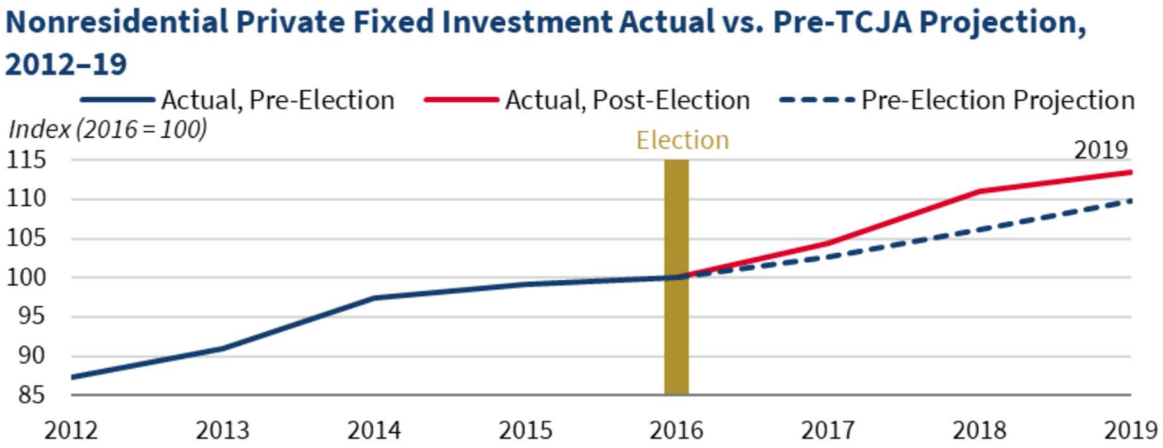
Figure 1 displays figures from an article published on December 20, 2019 by the Council of Economic Advisors under President Trump. Panel A displays actual real wage and salary compensation per household against 2017 CBO forecasts. Panel B displays nonresidential private fixed investment against the Blue-Chip consensus forecast in 2016. The article is found at: https://trumpwhitehouse.archives.gov/articles/two-years-tax-cuts-continue-boosting-united-states-economy/

Panel A: Real Wages



Sources: Bureau of Economic Analysis; Census Bureau; Congressional Budget Office; CEA calculations.
Note: Values are adjusted to real terms using the PCE chain price index. Values are indexed such that 2017:Q1 is equal to 100 to account for BEA annual revisions.

Panel B: Investment



Sources: Bureau of Economic Analysis; Blue Chip; CEA calculations.
Note: Pre-TCJA projection represents the Blue Chip consensus forecast from October 2016. 2019 value represents the annualized value of the first three quarters of available data.

Figure 2: Example of Allocating Market Reactions

Figure 2 presents an example of how firm-level stock returns are combined with establishment level data to measure county-level stock returns. In the following example, Firm A has 5 employees in County 1 and 70 employees in County 2, while Firm B has 20 employees in County 1 and 30 employees in County 2. County 1 therefore has a 18% weighted average return ($\frac{5}{25} * 10\% + \frac{20}{25} * 20\%$) and County 2 has a 13% weighted average return ($\frac{70}{100} * 10\% + \frac{30}{100} * 20\%$).

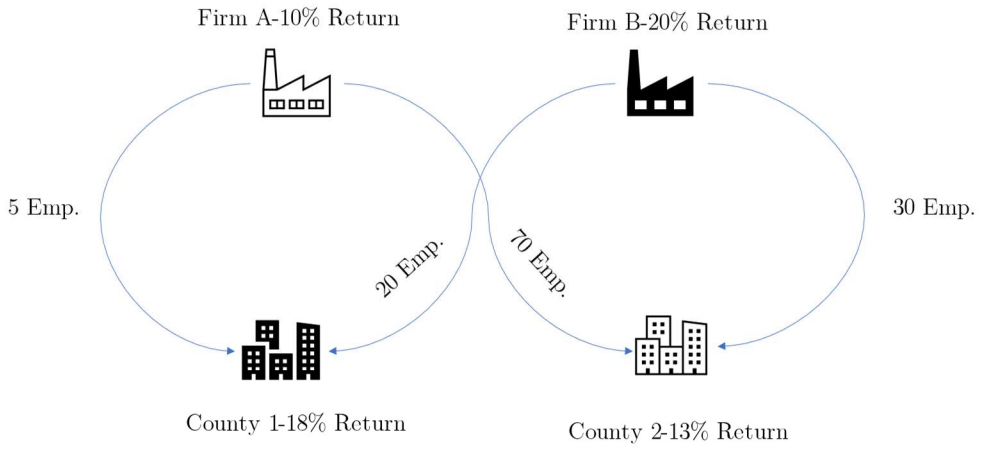


Figure 3: Choropleth of Market Reactions

Figure 3 presents a choropleth map of *TCJA Returns* by county, where *TCJA Returns* is defined as a county's employment weighted average of *Firm TCJA Returns,* and *Firm TCJA Returns* are calculated as the cumulative stock market reaction on the TCJA event days identified in Gaertner et al. (2020).

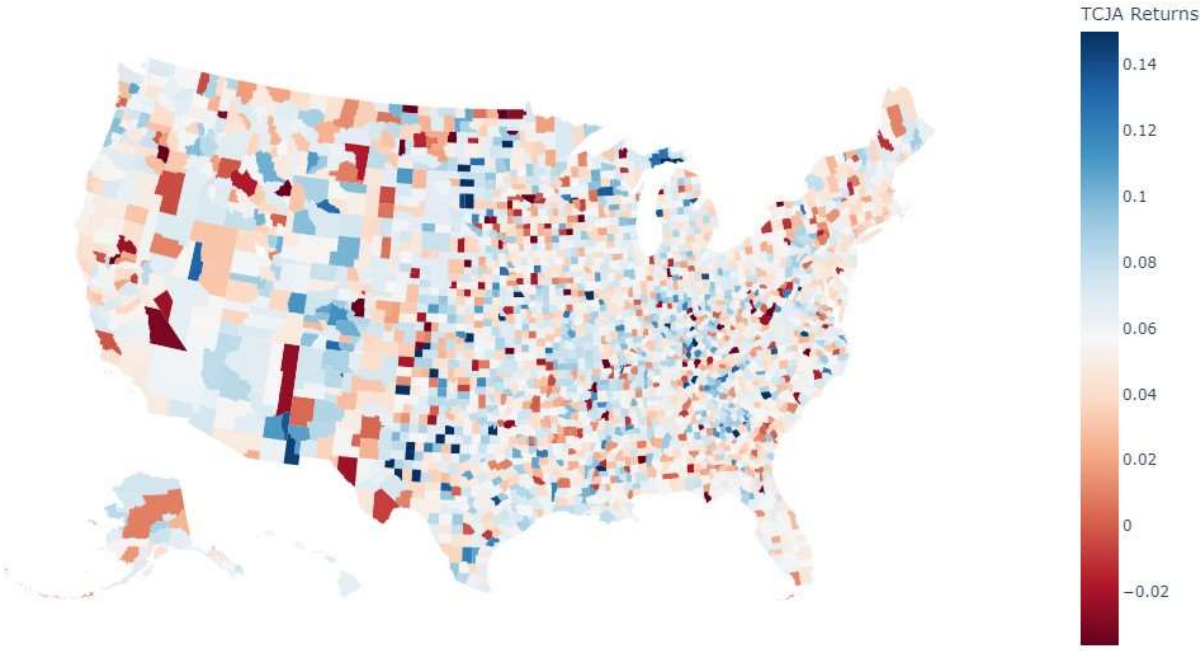
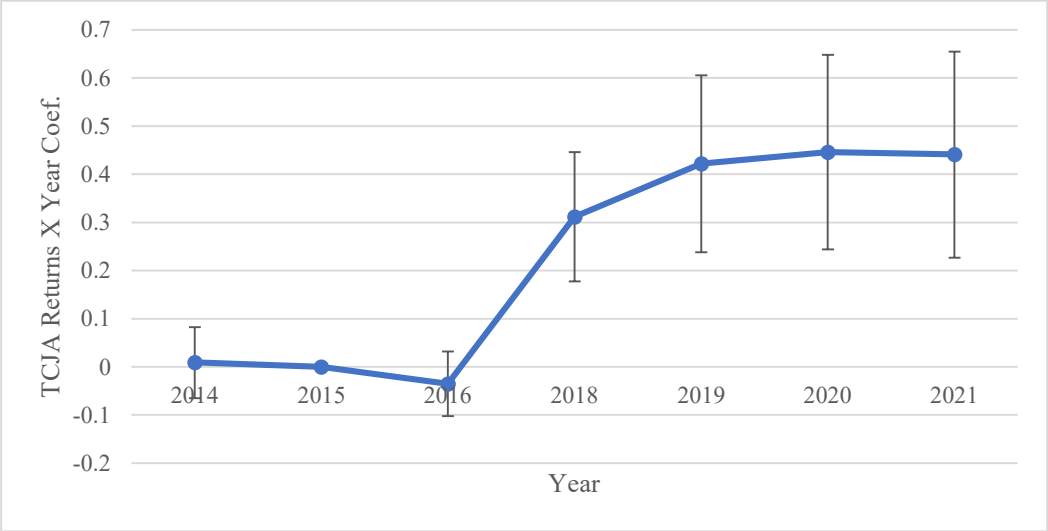


Figure 4: Parallel Trends

Figure 4 presents graphs of the β_t coefficients from the following regression: $GDP_{c,t}(Wage_{c,t}) = \beta_0 + \beta_t TCJA\ Returns_c * \mathbb{I}Year_t + \gamma_c + \eta_t + \varepsilon_{c,t}$, where $\mathbb{I}Year_t$ is a series of year fixed effects. Error bars are calculated from standard errors. The unit of observation is county-years between 2014 and 2021. Panel A presents the GDP result and Panel B presents the Wage result. *TCJA Returns* is defined as a county's employment weighted average of *Firm TCJA Returns*. *Firm TCJA Returns* are calculated as the cumulative stock market reaction on the TCJA event days identified in Gaertner et al. (2020). All regressions are weighted by 2013 GDP and clustered at the county-level. Regressions included year and county fixed effects.

Panel A: GDP



Panel B: Total Wages

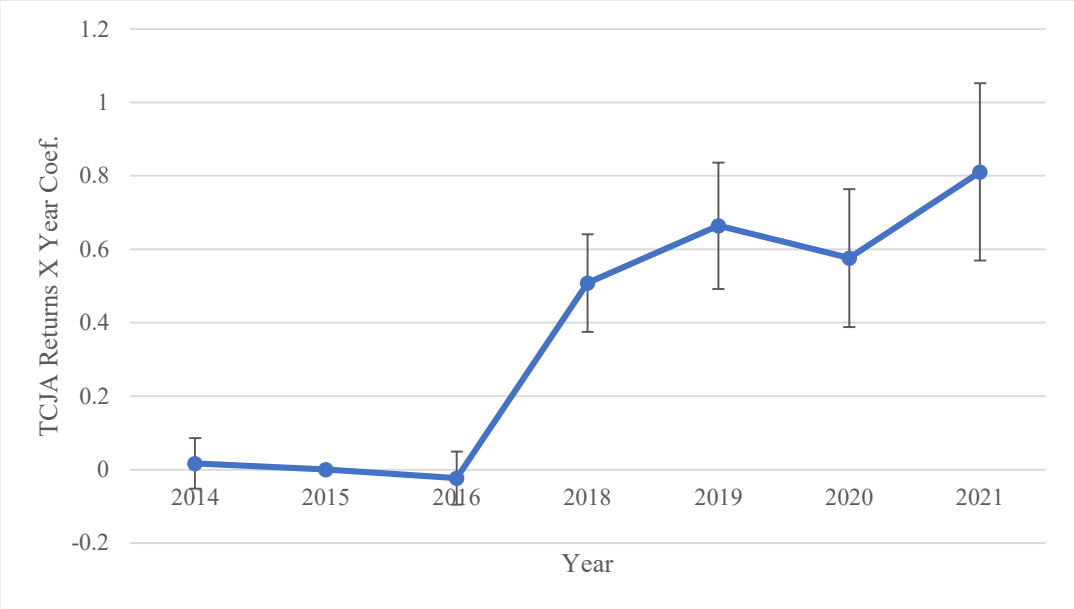
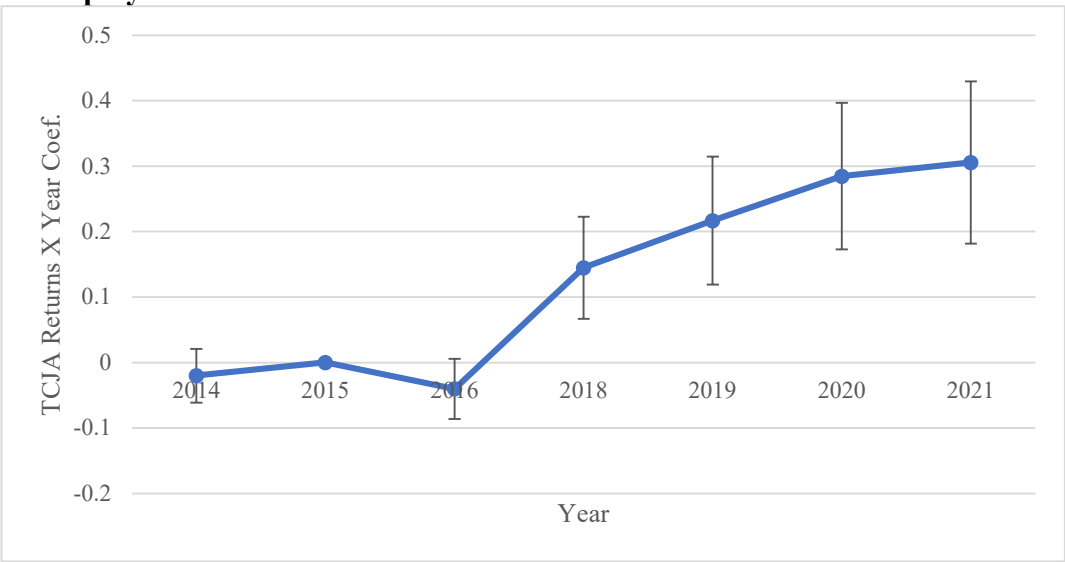


Figure 5: Parallel Trends Wage Decomposition

Figure 5 presents graphs of the β_i coefficients from the following regression: $Emplolyment_{c,t}(Annual\ Wage_{c,t}) = \beta_0 + \beta_i TCJA\ Returns_c * \mathbb{I}Year_t + \gamma_c + \eta_t + \varepsilon_{c,t}$, where $\mathbb{I}Year_t$ is a series of year fixed effects. Error bars are calculated from standard errors. The unit of observation is county-years between 2014 and 2021. Panel A (B) presents the result for Employment (Annual Wage). *TCJA Returns* is defined as a county's employment weighted average of *Firm TCJA Returns*. *Firm TCJA Returns* are calculated as the cumulative stock market reaction on the TCJA event days identified in Gaertner et al. (2020). All regressions are weighted by 2013 GDP and clustered at the county-level. Regressions included year and county fixed effects.

Panel A: Employment



Panel B: Annual Wage

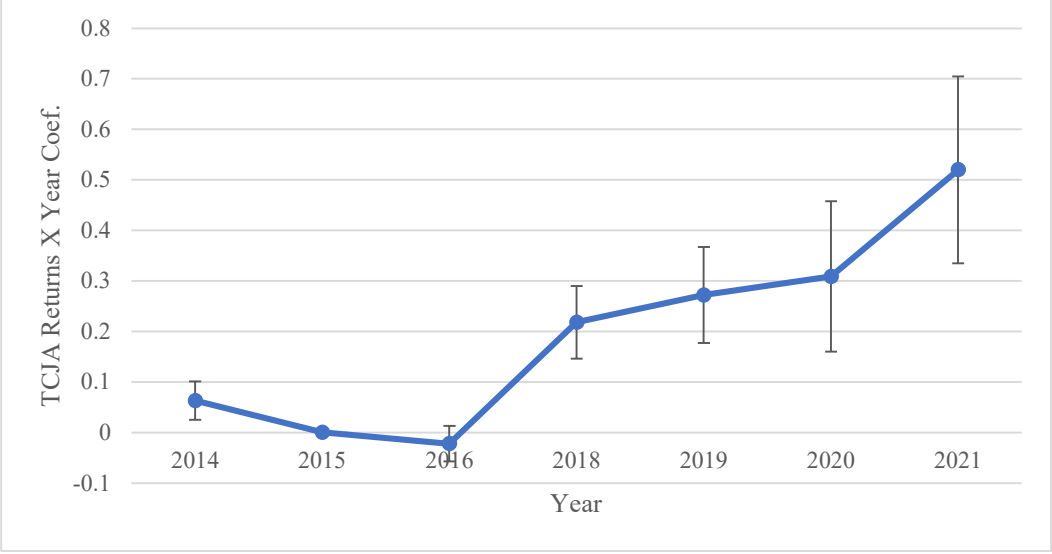


Table 1: Forecasts and Prior Literature

Table 1 Panel A lists a series of forecasts for the effect of the TCJA on GDP. The set of forecasts were compiled by the Congressional Budget Office (CBO) and Slemrod (2018). Table 1 Panel B lists a series of estimates for the fraction of corporate tax incidence that is borne by labor, as seen in Auerbach (2018) and in a review by the Tax Foundation.

Panel A: GDP Forecasts

Forecasting Group	Forecasted 10 Year GDP Level Increase
Economist Letter to Treasury Secretary	3%
Tax Foundation	2.90%
Penn-Wharton Model	0.6%-1.1%
Goldman Sachs	0.70%
Congressional Budget Office	0.60%
Moody's Analytics	0.40%
Joint Committee on Taxation	0.1%-0.2%
International Monetary Fund	-0.10%

Panel B: Labor Incidence

Paper	Labor Share of Incidence
Hassett & Mathur (2010)	2200%
Felix (2007)	400%
Arulampalam et al. (2012)	49%
Liu and Atshuler (2013)	40%-80%
Fuest et al. (2018)	40%
Suarez Serrato and Zidar (2016)	30%-35%
Congressional Budget Office	25%
Clausing (2013)	0%

Table 2: Descriptive Information

Table 2 presents descriptive information on counties. Panel A provides descriptive statistics, Panel B and C provides Pearson (Spearman) correlations below (above) the diagonal. In Panel A and B, *GDP, Total Wages, Employment,* and *Annual Wage* are presented logged and unlogged, and as of 2014 for interpretability. In Panel C, *GDP, Total Wages, Employment,* and *Annual Wage* are presented logged and within fixed effects for all years. All variables are winsorized at 1 and 99 percent. Variables are defined in Appendix A.

Panel A: Descriptive Statistics

	N	Mean	SD	Q1	Q2	Q3
TCJA Returns	2,994	5.6%	3.2%	4.2%	5.7%	7.2%
GDP	2,994	$5,529,724	$23,107,410	$391,618	$967,503	$2,776,165
Total Wages	2,994	$2,243,900	$10,033,103	$114,059	$310,723	$984,841
Employment	2,994	44,151	154,785	3,366	8,422	25,010
Annual Wage	2,994	$37,796	$8,718	$32,386	$35,905	$40,782

Panel B: Levels Correlations

		1	2	3	4	5
1	TCJA Returns	1	-0.01	0.00	0.00	-0.10
2	GDP	-0.03	1	0.97	0.96	0.65
3	Total Wages	-0.03	1.00	1	0.99	0.63
4	Employment	-0.03	0.97	0.96	1	0.55
5	Annual Wage	-0.03	0.44	0.45	0.43	1

Panel C: Within Fixed Effect Correlations

		1	2	3	4
1	Log GDP	1	0.52	0.49	0.29
2	Log Total Wages	0.50	1	0.83	0.61
3	Log Employment	0.46	0.87	1	0.21
4	Log Annual Wage	0.33	0.70	0.33	1

Table 3: Main Analysis

Table 3 presents the difference-in-differences specification listed in equation (1), with the dependent variable being *Log GDP (Wage)*. The unit of observation is county-years between 2014 and 2021. *TCJA Returns* is defined as a county's employment weighted average of *Firm TCJA Returns. Firm TCJA Returns* are calculated as the cumulative stock market reaction on the TCJA event days identified in Gaertner et al. (2020). *Post* is an indicator for if a year is after 2017. All regressions are weighted by 2013 GDP and clustered at the county-level. Regressions included year and county fixed effects where indicated. Variables are defined in Appendix A.

Dep. Var	Log GDP	Log Total Wages	Log GDP	Log Total Wages
	(1)	(2)	(3)	(4)
TCJA Returns*Post	0.420**	0.652***	0.414**	0.641***
	(2.355)	(3.645)	(2.321)	(3.596)
TCJA Returns	-17.930***	-20.263***		
	(-8.162)	(-8.518)		
Post	0.029***	0.105***		
	(3.124)	(9.222)		
Labor Share	64.2%		64.1%	
Year FE	No	No	Yes	Yes
County FE	No	No	Yes	Yes
Observations	20,956	20,956	20,956	20,956
R-Squared	0.042	0.048	0.999	0.999
Within R-Squared	0.042	0.048	0.004	0.009

Table 4: Wage Decomposition

Table 4 presents the difference-in-differences specification listed in equation (1), with the dependent variable being *Log Employment (Annual Wage)*. The unit of observation is county-years between 2014 and 2021. *TCJA Returns* is defined as a county's employment weighted average of *Firm TCJA Returns. Firm TCJA Returns* are calculated as the cumulative stock market reaction on the TCJA event days identified in Gaertner et al. (2020). *Post* is an indicator for if a year is after 2017. All regressions are weighted by 2013 GDP and clustered at the county-level. Regressions included year and county fixed effects where indicated. Variables are defined in Appendix A.

Dep. Var	Log Employment	Log Annual Wage	Log Employment	Log Annual Wage
	(1)	(2)	(3)	(4)
TCJA Returns*Post	0.269***	0.315**	0.258***	0.316**
	(2.838)	(2.571)	(2.744)	(2.575)
TCJA Returns	-17.075***	-3.501***		
	(-8.176)	(-7.476)		
Post	0.005	0.117***		
	(1.013)	(14.015)		
Year FE	No	No	Yes	Yes
County FE	No	No	Yes	Yes
Observations	20,956	20,956	20,956	20,956
R-Squared	0.04	0.147	0.999	0.982
Within R-Squared	0.04	0.147	0.004	0.008

Table 5: Mechanism

Table 5 presents the difference-in-differences specification listed in equation (3). The unit of observation is firm years between 2014 and 2021. The dependent variables are various firm-year financial statement line items. All line items are defined as the inverse hyperbolic sine of the corresponding line item in Compustat. *Firm TCJA Returns* are calculated as the cumulative stock market reaction on the TCJA event days identified in Gaertner et al. (2020). *Post* is an indicator for if a year is after 2017. All regressions include firm and year fixed effects, and are clustered at the firm-level.

Panel A: Income Statement

Decomposition:	Top Level	Net Income Decomposition			Pretax Income Decomposition						
Dep. Var	Net Income	Tax Expense	Minority Interest Income	Pretax Income	Revenue	COGS	SG&A	Depreciation	Non-Operating Income	Interest Expense	Special Items
Firm TCJA Returns * Post	1.872***	1.162***	0.288	1.958***	0.542***	0.254	0.274	0.039	0.189	-0.020	-0.403
	(2.865)	(3.103)	(1.505)	(2.903)	(2.751)	(1.421)	(1.481)	(0.271)	(0.514)	(-0.102)	(-1.118)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm
Observations	23,728	23,728	23,728	23,728	23,728	23,728	23,728	23,728	23,728	23,728	23,728
R-Squared	0.646	0.579	0.666	0.652	0.966	0.952	0.967	0.975	0.628	0.954	0.421

Panel B: Statement of Cash Flows – Investing

Decomposition:		Investing Cash Flow Decomposition						
Dep. Var	Investing Cash Flow	Increase in Investments	Sale of Investments	Change Short Term Investments	CAPX	Sale PP&E	Acquisitions	Other Investing Activities
Firm TCJA Returns * Post	-0.350	0.155	-0.075	-0.328	0.313*	-0.023	0.723**	-0.287
	(-0.672)	(0.573)	(-0.298)	(-1.307)	(1.830)	(-0.176)	(2.459)	(-0.759)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm
Observations	23,728	23,728	23,728	23,728	23,728	23,728	23,728	23,728
R-Squared	0.452	0.885	0.872	0.268	0.954	0.733	0.516	0.449

Panel C: Statement of Cash Flows – Financing

Decomposition:		Financing Cash Flow Decomposition							
Dep. Var	Financing Cash Flow	Sale of Stock	Tax Benefit of Stock Options	Purchase of Stock	Dividends	LTD Issuance	LTD Reduction	Change Current Debt	Other Financing Activities
Firm TCJA Returns * Post	1.325**	0.680**	0.203	0.418	-0.094	0.684**	0.671**	-0.029	0.805**
	(2.006)	(2.245)	(1.332)	(1.609)	(-0.424)	(2.022)	(2.149)	(-0.129)	(2.457)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm
Observations	23,728	23,728	23,728	23,728	23,728	23,728	23,728	23,728	23,728
R-Squared	0.450	0.635	0.458	0.743	0.912	0.697	0.791	0.152	0.569

Table 6: Private Firm Return Sensitivity Analysis

Table 6 re-estimates equation (1) using an alternate definition of *TCJA Returns.* Specifically I estimate $GDP_{c,t}(Wage_{c,t}) = \beta_0 + \beta_1\ Alternate\ TCJA\ Returns_c * Post_t + \eta_t + \varepsilon_{c,t}$, where *Alternate TCJA Returns = TCJA Returns× NETS Employment Fraction + (1- NETS Employment Fraction)×Return Ratio × TCJA Returns. Return Ratio* is the assumed ratio of returns between firms not listed in NETS to those listed in NETS. % $Change\ GDP\ (Wage)$ represents $\beta_1 * \overline{Alternate\ TCJA\ Returns} * Post_t$. *TCJA Returns* is defined as a county's employment weighted average of *Firm TCJA Returns. Firm TCJA Returns* are calculated as the cumulative stock market reaction on the TCJA event days identified in Gaertner et al. (2020). *Post* is an indicator for if a year is after 2017. *NETS Employment Fraction* represents the number of employees in a county across all public firms in NETS divided by the number of employees in the county using data from the Bureau of Labor Statistics. The unit of observation is county-years between 2014 and 2021. All regressions are weighted by 2013 GDP and clustered at the county-level. Regressions included year and county fixed effects where indicated.

Return Ratio	% Change GDP	% Change Total Wages	Labor Share
0	2.1%	1.7%	32.9%
0.25	2.8%	3.3%	48.9%
0.50	2.6%	3.5%	56.6%
0.75	2.3%	3.4%	61.1%
1	2.2%	3.4%	64.1%
1.25	2.1%	3.3%	66.2%
1.50	2.0%	3.2%	67.7%
1.75	1.9%	3.2%	69.0%
2	1.9%	3.2%	69.9%

Table 7: Private Firm Controls

Table 7 re-estimates equation (1) after controlling for private firm presence. The unit of observation is county-years between 2014 and 2021. *TCJA Returns* is defined as a county's employment weighted average of *Firm TCJA Returns. Firm TCJA Returns* are calculated as the cumulative stock market reaction on the TCJA event days identified in Gaertner et al. (2020). *NETS Employment Fraction* represents the number of employees in a county across all public firms in NETS divided by the number of employees in the county using data from the Bureau of Labor Statistics. *% Pass-Through* represents the fraction of the AGI in a county that is attributable to pass-through income. *Post* is an indicator for if a year is after 2017. All regressions are weighted by 2013 GDP and clustered at the county-level. Regressions included year and county fixed effects where indicated. Variables are defined in Appendix A.

Dep. Var	Log GDP	Log Total Wages	Log Employment	Log Annual Wage
	(1)	(2)	(3)	(4)
TCJA Returns*Post	0.498***	0.597***	0.343***	0.234**
	(2.714)	(3.259)	(3.458)	(2.020)
% Pass-Through*Post	0.051	-0.499**	-0.073	-0.236**
	(0.359)	(-2.546)	(-0.805)	(-2.044)
NETS Employment Fraction*Post	0.091*	-0.006	0.104***	-0.072*
	(1.939)	(-0.078)	(2.964)	(-1.926)
Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Observations	20,914	20,914	20,914	20,914
R-Squared	0.999	0.999	0.999	0.983
Within R-Squared	0.008	0.020	0.014	0.024

Table 8: Alternate Return Definitions

Table 8 re-estimates equation (1) using an alternate set of dates to identify market reactions to the TCJA. The unit of observation is county-years between 2014 and 2021. *Betting Market TCJA Returns* is defined as a county's employment weighted average of *Firm Betting Market TCJA Returns. Firm Betting Market TCJA Returns* are calculated as the cumulative stock market reaction on the TCJA event days identified in Gaertner et al. (2020) supplemented with days identified by the betting market PredictIt as important. All regressions are weighted by 2013 GDP and clustered at the county-level. Regressions included year and county fixed effects where indicated. Variables are defined in Appendix A.

Dep. Var	Log GDP	Log Total Wages	Log Employment	Log Annual Wage
	(1)	(2)	(3)	(4)
Betting Market TCJA Returns*Post	0.493***	0.639***	0.363***	0.309***
	(4.077)	(4.029)	(4.580)	(2.795)
Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Observations	20,956	20,956	20,956	20,956
R-Squared	0.999	0.999	0.999	0.982
Within R-Squared	0.011	0.015	0.013	0.013

Table 9: Simultaneous Treatment Robustness

Table 9 re-estimates equation (1) after controlling for potential subsequent events. The unit of observation is county-years between 2014 and 2021. *TCJA Returns, 15 Month Returns,* and *China Trade War Returns* are defined as a county's employment weighted average of their corresponding firm level measures. *Firm TCJA Returns* are calculated as the cumulative stock market reaction on the TCJA event days identified in Gaertner et al. (2020). *Firm 15 Month Returns* are calculated as a firm's cumulative stock market returns from January 1, 2017 to March 31, 2018. *Firm China Trade War Returns* are the stock returns for a given firm on the interval [-1,5] around the dates identified in Amiti et al. (2020). *Post* is an indicator for if a year is after 2017. All regressions are weighted by 2013 GDP and clustered at the county-level. Regressions included year and county fixed effects where indicated. Variables are defined in Appendix A.

Dep. Var	Log GDP	Log Total Wages	Log Employment	Log Annual Wage
	(1)	(2)	(3)	(4)
TCJA Returns*Post	0.436**	0.672***	0.296***	0.324***
	(2.527)	(3.914)	(3.219)	(2.844)
15 Month Returns*Post	0.043	-0.005	0.089***	-0.034
	(0.926)	(-0.103)	(3.949)	(-1.146)
China Trade War Returns*Post	0.126	0.430***	0.134**	0.251***
	(1.081)	(3.978)	(2.425)	(3.955)
Year FE			Yes	
County FE			Yes	
Observations	20,956	20,956	20,956	20,956
R-Squared	0.999	0.999	0.999	0.983
Within R-Squared	0.007	0.023	0.017	0.027

Table 10: Migration Controls

Table 10 re-estimates equation (1) after controlling for migration. The unit of observation is county-years between 2014 and 2021. *TCJA Returns* is defined as a county's employment weighted average of *Firm TCJA Returns. Firm TCJA Returns* are calculated as the cumulative stock market reaction on the TCJA event days identified in Gaertner et al. (2020). *Post* is an indicator for if a year is after 2017. *Fraction Migrants (AGI Migration)* is the net migration of individuals (amount of AGI) into a county scaled by the number of individuals (amount of AGI) in a county in the prior year. All regressions are weighted by 2013 GDP and clustered at the county-level. Regressions included year and county fixed effects where indicated. Variables are defined in Appendix A.

Dep. Var	Log GDP	Log Total Wages	Log Employment	Log Annual Wage
	(1)	(2)	(3)	(4)
TCJA Returns*Post	0.381**	0.521***	0.218**	0.225**
	(2.235)	(3.317)	(2.449)	(2.289)
Fraction Migrants	0.355	1.342***	0.500**	0.463**
	(1.245)	(2.767)	(2.093)	(2.168)
AGI Migration	0.059	0.212	-0.065	0.283***
	(0.433)	(1.353)	(-0.611)	(3.217)
Year FE			Yes	
County FE			Yes	
Observations	17,916	17,916	17,916	17,916
R-Squared	0.999	0.999	1.000	0.987
Within R-Squared	0.007	0.039	0.010	0.036

Table 11: Negative Spillover Falsification Test

Table 11 re-estimates equation (1) after controlling for adjacent county returns to test for negative spillovers in adjacent counties. The unit of observation is county-years between 2014 and 2021. *TCJA Returns* is defined as a county's employment weighted average of *Firm TCJA Returns. Firm TCJA Returns* are calculated as the cumulative stock market reaction on the TCJA event days identified in Gaertner et al. (2020). *Adjacent TCJA Returns* is the NETS employment weighted average *TCJA Returns* for adjacent counties. *Post* is an indicator for if a year is after 2017. All regressions are weighted by 2013 GDP and clustered at the county-level. Regressions included year and county fixed effects where indicated. Variables are defined in Appendix A.

Dep. Var	Log GDP	Log Total Wages	Log Employment	Log Annual Wage
	(1)	(2)	(3)	(4)
Adjacent TCJA Returns*Post	0.220	0.465	-0.002	0.592***
	(0.895)	(1.533)	(-0.012)	(2.976)
TCJA Returns*Post	0.343*	0.486***	0.263***	0.114
	(1.771)	(2.980)	(2.635)	(1.296)
Year FE			Yes	
County FE			Yes	
Observations	20,928	20,928	20,928	20,928
R-Squared	0.999	0.999	0.999	0.982
Within R-Squared	0.005	0.011	0.004	0.017

Appendix B Table 1: NETS Industry Coverage

Appendix B Table 1 the distribution of NETS coverage by industry. *NETS % Firms* represents the fraction of Compustat firms with data in NETS. *NETS % Assets* calculates this fraction of an industry's assets that are held by NETS firms. Industries are defined using Fama French 17 industries. Both NETS and Compustat use data from 2013.

Industry	NETS % Assets	NETS % Firms
Food	63.6%	59.7%
Mining and Minerals	17.1%	23.8%
Oil and Petroleum Products	32.5%	52.7%
Textiles, Apparel & Footware	91.2%	65.8%
Consumer Durables	26.3%	57.3%
Chemicals	39.0%	55.8%
Drugs, Soap, Prfums, Tobacco	61.0%	59.9%
Construction and Construction Materials	58.0%	68.5%
Steel Works Etc	39.9%	47.8%
Fabricated Products	82.8%	72.7%
Machinery and Business Equipment	59.4%	58.1%
Automobiles	28.8%	61.2%
Transportation	55.7%	48.6%
Utilities	40.4%	35.2%
Retail Stores	87.2%	77.9%
Banks, Insurance Companies, and Other Financials	39.8%	65.1%
Other	58.5%	57.8%

Appendix B Table 2: Firm Return Determinants

Appendix B Table 2 presents a firm-level regression of *Firm TCJA Returns* on potential market reaction determinants. The sample is composed of public Compustat firms who have data in NETS in 2013. *Firm TCJA Returns* are calculated as the cumulative stock market reaction on the TCJA event days identified in Gaertner et al. (2020). *ETR* is firms' cash ETR, *Market-To-Book* is firms' market value of equity divided by their book value of equity, *Size* is the natural log of firms' total assets, *Leverage* is firms' total liabilities divided by total assets, *Foreign Income* is firm's pretax foreign income divided by their total pretax income, and *Change MTR 2013 to 2019* is the change in firms' marginal tax rate from 2013 to 2019, where marginal tax rates use data from Graham (1996). All independent variables are defined as of 2013. Industry fixed effects are at the three-digit NAICS level. Regressions are weighted as labeled. Standard errors are robust but not clustered.

Dep. Var	Firm TCJA Returns							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ETR	0.009*	0.010*	-0.013	-0.022	0.018***	0.015**	0.007	0.003
	(1.755)	(1.867)	(-0.655)	(-0.962)	(2.637)	(2.178)	(0.307)	(0.163)
Market-To-Book	-0.000	-0.000	-0.002**	-0.001	-0.001**	-0.001	-0.002**	-0.001***
	(-0.810)	(-0.209)	(-2.055)	(-1.053)	(-1.967)	(-1.438)	(-2.536)	(-2.604)
Leverage	0.017**	0.003	0.037**	0.007	0.017**	0.002	0.057***	0.038*
	(2.532)	(0.351)	(2.253)	(0.390)	(2.045)	(0.165)	(3.516)	(1.946)
Size	-0.003***	-0.003***	-0.004**	-0.006***	-0.003***	-0.002**	-0.001	-0.006***
	(-4.194)	(-3.692)	(-2.226)	(-3.544)	(-2.619)	(-2.376)	(-0.431)	(-4.112)
Foreign Income	-0.008**	-0.006	0.006	0.008	-0.004	-0.001	-0.001	0.005
	(-2.021)	(-1.532)	(0.628)	(1.209)	(-0.922)	(-0.174)	(-0.078)	(0.695)
Change MTR 2013 to 2019					-0.026**	-0.011	-0.052*	-0.023
					(-2.377)	(-0.922)	(-1.692)	(-1.255)
FE	None	Industry	None	Industry	None	Industry	None	Industry
Weight	None		Total Assets		None		Total Assets	
Observations	3,317	3,306	3,317	3,306	1,906	1,901	1,906	1,901
R-Squared	0.012	0.107	0.024	0.248	0.018	0.167	0.089	0.496

Appendix B Table 3: Further Financial Statement Decomposition

Appendix B Table 3 presents the difference-in-differences specification listed in equation (3). The unit of observation is firm years between 2014 and 2021. The dependent variables are various firm-year financial statement line items. All line items are defined as the inverse hyperbolic sine of the corresponding line item in Compustat. *Firm TCJA Returns* are calculated as the cumulative stock market reaction on the TCJA event days identified in Gaertner et al. (2020). *Post* is an indicator for if a year is after 2017. All regressions include firm and year fixed effects, and are clustered at the firm-level.

Panel A: Operating Cash Flows

Decomposition:		Operating Cash Flow Decomposition											
Dep. Var	Operating Cash Flow	Net Income CF	Depreciation CF	Extraordinary Items	Deferred Taxes CF	Subsidiary Earnings CF	Sale of PP&E	Funds from Other Operations	Change AR	Change Inventory	Change Accrued Liab.	Change Accrued Taxes	Change Other A&L
Firm TCJA Returns * Post	0.098	1.933***	-0.148	0.012	0.989***	-0.478***	-0.359	-0.294	-0.694*	0.435	0.503	0.015	-0.335
	(0.205)	(2.930)	(-0.870)	(0.085)	(2.610)	(-2.711)	(-1.523)	(-0.682)	(-1.757)	(1.402)	(1.600)	(0.129)	(-0.707)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm
Observations	23,728	23,728	23,728	23,728	23,728	23,728	23,728	23,728	23,728	23,728	23,728	23,728	23,728
R-Squared	0.735	0.645	0.945	0.235	0.311	0.454	0.483	0.474	0.285	0.297	0.254	0.157	0.316

Panel B: Assets

Decomposition:		Assets Cash Flow Decomposition								
Dep. Var	Assets	Cash	Total Receivables	Inventory	Other Current Assets	PP&E Net	Other Investments	Equity Investments	Intangibles	Other Assets
Firm TCJA Returns * Post	0.194	0.327*	0.279	-0.107	0.135	0.209	-0.234	0.008	0.205	0.333*
	(1.352)	(1.884)	(1.572)	(-0.600)	(0.869)	(1.042)	(-0.825)	(0.034)	(0.759)	(1.712)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm
Observations	23,728	23,728	23,728	23,728	23,728	23,728	23,728	23,728	23,728	23,728
R-Squared	0.979	0.923	0.966	0.962	0.949	0.967	0.921	0.886	0.937	0.944

Panel C: Liabilities and Equity

Decomposition:		Equity Decomposition			Liabilities Decomposition						
Dep. Var	Stockholders' Equity	Preferred Stock	Common Equity	Total Liabilities	Current Debt	Accounts Payable	Taxes Payable	Other Current Liab.	Long Term Debt	Deferred Tax and Investment Tax Credit	Other Liabilities
Firm TCJA Returns * Post	0.321*	-0.158	0.829*	0.176	0.569*	0.234	-0.175	-0.047	0.577*	-0.565**	-0.248
	(1.672)	(-0.875)	(1.649)	(1.004)	(1.867)	(1.494)	(-1.096)	(-0.348)	(1.693)	(-2.258)	(-1.210)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm
Observations	23,728	23,728	23,728	23,728	23,728	23,728	23,728	23,728	23,728	23,728	23,728
R-Squared	0.680	0.785	0.791	0.970	0.810	0.972	0.877	0.971	0.897	0.904	0.954