Corporate Tax Cuts, Firm Growth, and Workers' Earnings

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Abstract

We study the effects of the largest corporate income tax cut in U.S. history on firms and workers. To identify causal effects, we use employer-employee matched tax records and event studies comparing similarly sized firms in the same industry that faced divergent tax changes due to their pre-existing legal status. Tax cuts cause increases in firms' investment, sales, profits, employment, and payrolls, with earnings gains concentrated among highly paid workers. In the short-run, 87% of private income gains flow to the top 10% of the income distribution.

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1 Introduction

We study the effects of corporate income taxes on firms and workers in the United States, where in 2017 Congress enacted the largest corporate tax cut in American history. Commonly known as the Tax Cuts and Jobs Act (TCJA), the legislation introduced sweeping changes to corporate tax rates, investment incentives, and taxation of foreign income, among several other provisions of the tax code. Collectively, these reforms constituted the first major overhaul of U.S. business taxation since the Tax Reform Act of 1986, providing a rare and sharp natural experiment to shed light on research and policy debates.

Even as governments around the globe have steadily reduced corporate income tax rates since the 1980's, policymakers and researchers today continue to debate the costs and benefits of declining corporate tax burdens. Advocates for tax cuts argue that lower rates increase investment, growth, and workers' living standards, while critics argue they do little to boost growth and primarily benefit the wealthy.

In this paper we bring new evidence to these debates. Our empirical analysis studies the core provisions of TCJA affecting firms' marginal income tax rates, using an employer-employee matched panel dataset constructed from large random samples of de-identified firm- and worker-level federal tax records. These data allow us to observe a holistic set of firm outcomes — including capital investment, sales, profits, and shareholder payouts— and to merge them with worker-level data on employment and annual labor earnings.

To identify causal effects, we use an event study design comparing the outcomes of similarly sized firms in the same industry that faced divergent changes in their tax treatment following the reform. In particular, TCJA cut the top marginal tax rate for a legal entity type of firms known as C corporations from 35% to 21% (a 40% reduction). At the same time, TCJA cut the implied top marginal tax rate for a distinct legal entity type of firms known as S corporations from 39.6% to 37% (about a 7% reduction), and also introduced a new tax deduction that, for some of these firms, further reduced the top marginal rate to 29.6% (for a cumulative 25% reduction). C corporations and S corporations operate in the same industries, overlap in their firm size distributions, and otherwise faced broadly similar tax incentives, inviting a natural comparison between the two.

We exploit that the average C corp received a substantially larger tax cut than the average S corp to study how the corporate tax cuts affected firms' real decisions, including capital investment and labor demand, as well as payments to shareholders and workers. As in Yagan (2015), the identifying assumption of this research design is not random assignment of C or S status, but rather that outcomes for C and S corps would have trended similarly in the absence of the tax cuts. Outcomes of comparable C and S corps were on similar trends prior to TCJA, and we implement extensive robustness checks to validate that our estimates are driven by changes in tax rates rather than other features of the law, tax shifting behaviors, or unrelated shocks differentially affecting C and S corps at the same time as the reform.

The event studies, which compare trends in outcomes of C versus S corps controlling for

firm and industry-size-year fixed effects, indicate that corporate tax cuts cause economically and statistically significant increases in firms' capital investment, payrolls, employment, sales, profits, and dividend payouts to shareholders. Responses are larger among capital-intensive and small firms, particularly for payroll outcomes, and we interpret the evidence as consistent with both user cost and liquidity channels.

Our benchmark estimate of the federal corporate elasticity of taxable income (ETI), a common parameter for measuring the magnitude of tax distortions, is 0.70 (s.e.=0.16). This elasticity is smaller than most estimates from small open economies, but larger than most estimates from personal income taxes. Since businesses are less mobile within US boundaries than among states, municipalities, or small countries, and since a large literature documents that individual labor supply elasticities tend to be small, we interpret this evidence as consistent with the conventional view in public finance that tax distortions vary proportionally with factor mobility. The corresponding implied marginal value of public funds (MVPF) is 1.47, indicating that $1 in foregone corporate tax revenue generates around $1.47 in aggregate private income. This MVPF point estimate is likely an upper bound, since allowing for general equilibrium effects that dampen aggregate responses for both firm types would attenuate welfare gains.

Moving to the evidence on workers, we study impacts on firm wage quantiles and find that annual earnings do not change differentially among C and S corp workers in the bottom 90% of the within-firm distribution, but do increase for C corp workers in the top 10%, and increase particularly sharply for firm managers and executives. Executive pay increases are only modestly correlated with changes in firms' sales, profits, or sales growth relative to other firms in the same industry, suggesting compensation increases are not tightly linked to stronger firm performance among firms in our sample in the short run.

Models of perfect labor market competition predict that workers who compete in integrated labor markets may benefit indirectly from corporate tax cuts via general equilibrium effects that broadly increase labor demand and wages. In this case, firm-level research designs could mask earnings gains for lower-income workers, which would be absorbed by industry-size-year fixed effects in the empirical specifications. To explore this channel, we aggregate worker-level earnings data at the market level — where markets are defined as states or as industry-state pairs — and estimate median earnings growth among workers with varying market-level exposures to the tax cuts. The resulting elasticities are economically small and centered around zero, suggesting that the corporate tax cuts did not significantly increase median earnings over our time horizon.

To assess holistic distributional impacts, we estimate the incidence of the corporate tax cuts on several key factor groups — firm owners, executives, and high- and low-paid workers — as the share of the change in total income accruing to each factor. Combining the reduced form elasticities with key moments from the tax data, we find that 60% of gains flow to firm owners, 8% flow to executives, 32% flow to high-paid workers, and 0% flow to low-paid workers. We further estimate effects across the income distribution, accounting for the empirical fact that many workers are also firm owners (that is, they hold equity portfolios) and many firm owners also work. Using data on

the distribution of capital ownership, we estimate that 87% of the gains from tax cuts accrue to the top 10% of earners and 13% of gains flow to the bottom 90%. Leveraging the empirically observable geographic distribution of workers and income, we show that these benefits are disproportionately concentrated in the Northeastern and Western regions of the United States, particularly among workers in large, high-income cities.

This paper builds on research that studies the effects of corporate taxes on firms and workers. Early studies used aggregate or firm-level panel data and estimate two-way fixed effect models to study policy variation across countries or industries (Cummins et al. 1994, 1996; Hall and Jorgenson 1967). More recent contributions use detailed administrative microdata and modern econometric methods to exploit geographic policy variation in tax rates (Link et al. 2024; Duan and Moon 2026; Garrett et al. 2020; Giroud and Rauh 2019; Fuest et al. 2018; Suárez Serrato and Zidar 2016; Heiser et al. 2025), industry-level variation in exposure to federal tax deductions or credits (Curtis et al. 2021; Ohrn 2023; Dobridge et al. 2021; Ohrn 2018; Zwick and Mahon 2017; House and Shapiro 2008), and firm-level policy variation induced by plausibly arbitrary legal or circumstantial distinctions (e.g., Moon 2022; Carbonnier et al. 2022; Bachas and Soto 2021; Risch 2024; Yagan 2015).

Despite advances in recent research, there are natural reasons to question whether existing evidence is generalizable to understanding the effects of corporate tax cuts in the context of TCJA. Evidence from subnational governments, small developing countries, or small firms may have limited applicability to major reforms in a large advanced economy such as the United States (Auerbach 2018). This concern is especially salient with respect to the U.S. federal corporate income tax, where the tax base is broader, top tax rates and revenues are orders of magnitude larger, and factors of production are considerably less mobile. Moreover, economic theory predicts that alternate tax instruments — such as dividend taxes, capital gains taxes, or targeted deductions and credits — have different effects than the corporate income tax (Auerbach 2002). In this light, it is not surprising that debates over the effects of corporate tax cuts remain contested by researchers and policymakers (Barro and Furman 2018).

Empirical evidence on the effects of the federal corporate tax has remained scarce for three reasons. First, federal tax reforms are rare historical events, leaving limited policy variation for researchers to study. Second, administrative microdata was previously unavailable to researchers, constraining the scope and precision of empirical analyses. Third, even when countries do change their tax rates, it is difficult for researchers to establish counterfactuals for causal inference given the challenges of identifying a credible control group.

This paper overcomes these limitations to provide extensive evidence on the effects of corporate tax cuts on firms and workers. In doing so, we make four main contributions.

First, we study a rare policy change that generated historically large within-country variation in federal corporate income tax rates, and moreover generated variation even across similarly sized firms in the same industry. As a share of GDP, the TCJA tax cut is an order of magnitude larger than previous studies that focus, for example, on changes in state or local corporate taxes, which

tend to have lower rates and a smaller tax base (e.g., Giroud and Rauh 2019; Fuest et al. 2018; Suárez Serrato and Zidar 2016). The large magnitude of the tax cut is relevant on theoretical grounds under the conventional view that economic distortions are non-linear (e.g., proportional to the square of the tax rate, as in Harberger 1964), or under the view that rationally inattentive agents facing adjustment costs may be less responsive to small tax changes (e.g., as in Chetty 2012). The large magnitude of the tax cut is also relevant on purely empirical grounds, since ex-ante it is unclear whether existing evidence can be extrapolated to the case of an outlier.

Second, we provide more complete and nuanced estimates of corporate tax incidence relative to prior studies. We build on research that uses employer-employee data to assess incidence on different worker types (Risch 2024; Carbonnier et al. 2022; Dobridge et al. 2021; Fuest et al. 2018), and provide novel estimates of incidence over the full distribution of workers' earnings and income. The richness of the data allows us to assess shareholder incidence using fewer assumptions than are required when data availability are more limited, in contrast to studies that do not observe firms' profits (e.g., Fuest et al. 2018; Suárez Serrato and Zidar 2016). We further show how incidence varies across U.S. regions and contributes to geographic disparities.

Third, we contribute to the literature on TCJA and its effects on the U.S. economy. Researchers have studied impacts on macroeconomic performance (Gale and Haldeman 2021; Barro and Furman 2018), international and intertemporal profit shifting (Clausing 2020; Dowd et al. 2020), pass-through businesses (Goodman et al. 2025), executive compensation (De Simone et al. 2022), and capital structures (Carrizosa et al. 2023). Our study differs from existing research in that we specifically study the effects of TCJA's corporate income tax cuts on firm- and worker-level outcomes using administrative microdata and a quasi-experimental research design leveraging cross-firm policy variation. Contemporaneously, Chodorow-Reich et al. (2025) use confidential tax records to study the effects of TCJA, with a primary focus on capital investment. We obtain similar investment elasticities to theirs, despite using a different identification strategy and a different sample of firms.

Finally, we contextualize our findings from this historical episode in debates about efficiency and progressivity in the broader national tax and transfer system (Carbonnier et al. 2022; Bachas and Soto 2021; Risch 2024; Hendren and Sprung-Keyser 2020; Fuest et al. 2018; Suárez Serrato and Zidar 2016). With respect to efficiency, our results imply that corporate taxes impose significant distortions on the real economy, and that reducing corporate tax burdens causes firms to grow and expand. With respect to progressivity, our results contrast with much existing research in that we find the incidence of the corporate tax falls heavily on capital and highly paid workers. Taken together, the findings imply that policymakers face an equity-efficiency tradeoff when setting corporate tax policy. We note that the results estimate short-run responses, do not capture effects for extremely large public firms (which are exclusively C corporations), and do not account for potential changes in government spending or other fiscal externalities, all of which are important considerations for policymakers but beyond the scope of this research.

2 The Tax Cuts and Jobs Act

2.1 Legislative History

In 2017 Congress reformed federal business tax policy, with the stated aims of increasing capital investment, economic growth, and international competitiveness. Following several months of political negotiations and policy proposals, in December 2017 Congress and the President enacted Public Law 115-97, more commonly known as the Tax Cuts and Jobs Act, or TCJA. Our aim below is not to exhaustively detail TCJA's numerous reforms — for reviews of significant provisions see Auerbach (2018) and Joint Committee on Taxation (2018) — but rather to illuminate the key institutional details and policy variation that we leverage in the empirical analysis.

2.2 C vs. S Corporations

At the heart of TCJA was an overhaul of the income tax schedules facing two legally distinct types of businesses, known as C corporations and S corporations. Combined, C and S corps account for approximately 70% of total U.S. employment and 74% of total payrolls, with government, non-profits, and non-corporate private businesses comprising the remainder (Census Bureau 2019). Our analysis focuses exclusively on the corporate sector, as other entity types face different tax and regulatory regimes, and are beyond the scope of this paper. Below we describe salient differences between C and S corps.

C Corporations. C corps pay income taxes directly to the federal government and are subject to both income taxes (paid on corporate profits) and dividend taxes (paid by shareholders on profits distributed as dividends). C corps can be either publicly traded or privately owned. Before TCJA, C corps faced a progressive tax schedule with eight income brackets and a top rate of 35%. After TCJA, these brackets collapsed to a uniform 21% rate. Appendix A.1 documents the evolution of top marginal income tax rates for C corps in the United States since 1909, illustrating the historic nature of this large and rare tax cut. Appendix A.2 details the collapse of the progressive tax brackets following TCJA. Appendix A.3 puts the U.S. corporate tax cuts in a global perspective. Appendix A.4 benchmarks the magnitude of the TCJA corporate tax cut against other recent studies in the literature.

S Corporations. S corps do not pay entity-level taxes to the federal government. Rather, income is passed through to individual shareholders, who pay taxes on profits as ordinary income and can deduct losses. S corps may have up to 100 shareholders, whom generally must be U.S. citizens and not businesses or institutional investors. S corps are thus not permitted to sell shares on public exchanges. Unlike C corps, S corps are not subject to corporate income taxes, nor are their distributed profits subject to dividend taxes. Prior to 2018, owners of S corps faced a top marginal income tax rate of 39.6%. Beginning in 2018, TCJA provided two forms of tax relief to S corp owners. First, it reduced the top personal income tax rate from 39.6% to 37%. Second, it introduced a 20% tax deduction on qualified business income that further reduced the effective marginal tax

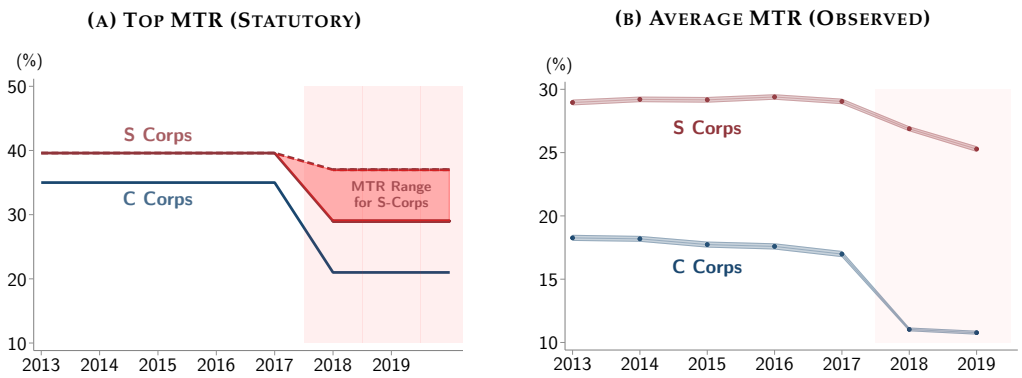
rate on S corp income for most high-income taxpayers from 37% to 29.6%. This Qualified Business Income ("QBI") deduction is claimable by many, but not all, owners of S corps. Since the QBI limitations are complex and not crucial for the empirical analysis, we abstract from details here and provide more information in Appendix A.5.

Entity Type Choice and Switching. The decision of firm owners to choose one corporate form over the other may reflect a variety of considerations, including shareholder preferences for concentrated versus diffuse ownership, access to capital, and tax planning. Switching entity types is costly, rare, and subject to regulatory restrictions. Thus, a firm's entity type prior to TCJA is strongly related to the tax rate change it faced after TCJA. We show evidence in Appendix A.6 that endogenous switching is not a concern for our analysis.

2.3 Policy Variation in Marginal Income Tax Rates

Figure 1 shows the evolution of top marginal income tax rates for C and S corps in the years before and after TCJA, illustrating the key policy variation we exploit in the analysis. Panel A shows the sharp reduction in top statutory rates for C corps, as well as the change in the implied range of top statutory rates for S corps depending on whether or not they are eligible for the QBI deduction. Panel B shows the change in observed marginal tax rates from our analysis sample of firms, which we describe in detail below in Section 3.

FIGURE 1: MARGINAL INCOME TAX RATES FOR C AND S CORPS



Notes: Panel A shows top statutory marginal tax rates (MTRs) for C and S corps before and after TCJA. Panel B shows the average MTRs observed in the sample. Top statutory and observed MTRs differ because some firms are in loss positions and pay no tax. For C corps in the pre-period, and always for S corps, they may also differ due to the progressive structure of the income tax brackets. See Section 3 and Appendix B for additional data details.

3 Data

We use a panel of employer-employee-matched annual federal tax records from tax years 2013 to 2019. We begin the sample period in 2013, allowing us to compare trends in the outcomes of C and S corps several years before TCJA, and end the sample in 2019, prior to the onset of

the COVID-19 pandemic.[1] Below we describe the data sources and sample construction, provide variable definitions, and present descriptive statistics. We provide additional details about the data and cleaning procedures in Appendix B.

3.1 Tax Returns

Corporate Returns. We study firms in the corporate Statistics of Income (SOI) files produced by the U.S. Internal Revenue Service (IRS 2022). The corporate SOI files include stratified random samples of corporate tax returns from both C corps (from IRS Form 1120) and S corps (from IRS Form 1120-S). IRS produces and cleans these random samples to estimate aggregate statistics and to provide government agencies with essential data for development of legislation and policy analysis. Among other outcomes, the corporate tax returns allow us to observe firms' capital investment, sales, costs, profits, and taxes paid, as well as their year of incorporation and industry.

We impose the following sample restrictions on the SOI panel, yielding an analysis sample of 49,235 unique firms and 231,360 distinct firm-year observations. We require that firms have at least 10 employees, between $1 million and $10 billion in annual sales, positive capital, and positive costs. The sample thus excludes small firms (some of which faced tax increases due to TCJA, as documented in Dobridge et al. 2025) as well as the largest firms in the top 0.001% of the firm sales distribution (all of which are C corporations). We further drop publicly traded firms (which are exclusively C corporations); firms with a foreign sales share greater than 5% (to rule out potentially confounding effects from changes in the tax treatment of foreign income); and firms that switch entity type from C to S or vice versa over the course of our sample period (excluding only 3% of firms that collectively account for less than 0.5% of aggregate corporate sales or profits; see Appendix A.6 for details). Firms in our sampling range represent approximately the 80th to the 99.999th percentile of the aggregate firm sales distribution and collectively accounted for more than half of corporate employment in 2016.

Individual Returns. We merge the sample of corporate tax returns with the universe of worker-level filings of IRS Form W-2 (JCT 2022). We also collect information about the owners of S corps in our sample from the universe of filings of IRS Forms 1040 and 1099-K1.

3.2 Variable Definitions, Measurement, and Weighting

We take care to measure economic variables consistently over time, such that the outcomes are not affected by changes in the tax base or reporting requirements on tax forms. Additional details on variable definitions are provided in Appendix B.

Marginal Tax Rates. For C corps, we observe taxable income and infer marginal tax rates using

[1] Evaluating outcomes prior to 2020 circumvents pandemic-related issues affecting the internal and external validity of the research design in later years; see Appendix C.10. Our findings are consistent with prior studies showing that fiscal policies may have economically significant effects even in the short-run (e.g., Auerbach and Gorodnichenko 2012; Fajgelbaum et al. 2020; Chodorow-Reich et al. 2025).

the federal corporate income tax schedule reproduced in Appendix Table 1 Panel A. For S corps, we observe each owner's taxable income from their personal tax returns, and infer their tax rate using the federal personal income tax schedules reproduced in Appendix Table 1 Panel B. We then compute the marginal tax rate (MTR) for the firm as a weighted average of the observed marginal personal income tax rates faced by the firms' owners, where the weights are given by the share of ordinary business income distributed to each owner of that firm. For example, if an S corp has two owners who receive an equal share of that firm's business income, facing observed marginal tax rates on their individual income of 25% and 35%, respectively, then we compute the firm's implied marginal tax rate as $(.5 * .25) + (.5 * .35) = 30\%$. Appendix Figure 6 shows the sample distribution of corporate MTRs for both C and S corps before and after TCJA.

Taxes Paid. For C corps, we observe federal tax payments on Form 1120. For S corps, we estimate tax payments using information from the individual owners' tax returns. To do so, we first compute each owner's average tax rate from Form 1040 as total federal tax divided by taxable income. We also record each owner's total net ordinary business income from Form 1040 Schedule E, and estimate total business taxes paid on this income by multiplying it by the owner's average tax rate. We bottom code total business taxes at zero, ensuring in our calculations that owners do not pay tax on business losses. For each owner, we allocate her total business tax payments to each firm that she owns in proportion to the share of ordinary business income received from that business. Finally, we sum the tax payments of each firm's owners to record an estimate of firm-level tax payments. Appendix B provides additional details about these computations.

Sales, Costs, and Profits. Sales are gross receipts. Pre-tax operating profits are sales minus cost of goods sold, which includes both material and labor inputs. Costs are costs of goods sold plus total deductions minus domestic production activities deductions (DPAD). Taxable income, defined consistently over time and across entity types, is net income minus special deductions plus DPAD. After-tax profits are taxable income minus taxes paid.

Dividends, Share Buybacks, and Total Payouts. Dividends are total cash and property payments to shareholders. Share buybacks are equal to non-negative changes in treasury stock, and total payouts are the sum of dividends and share buybacks.

Capital and Net Investment. Capital assets are equal to the book value of tangible investment minus capital asset retirements and accumulated book depreciation. Net investment is the change in the dollar value of capital assets.

Labor Market Outcomes. Employment is measured by matching firms with individual-level W-2s. We use crosswalks to improve linkages between parent companies and their subsidiaries (JCT 2022); see Appendix B for details. Workers' annual earnings are equal to Medicare wages from the W-2. Payrolls are the sum of W-2 earnings. We measure executive compensation as the earnings of

the top five highest paid workers in the firm.

Additional Firm Characteristics. We group firms into time-invariant size quintiles based on their employment in the first year they enter our sample. The bins in the first year correspond to: 10-30 employees; 31-65 employees; 66-132 employees; 133-323 employees; and 324+ employees. We also classify firms into time-variant industries using the NAICS-3 codes they report on Forms 1120 and 1120-S. In the resulting data we observe 88 distinct industries and 416 distinct industry-size bins. Firm age is inferred from the firm's year of incorporation, reported on Form 1120. We measure capital intensity at the industry level as the ratio of capital to sales. Firms are classified as capital intensive if the mean of this ratio in the pre-period is greater than the sample median, and others are classified as non-capital intensive.

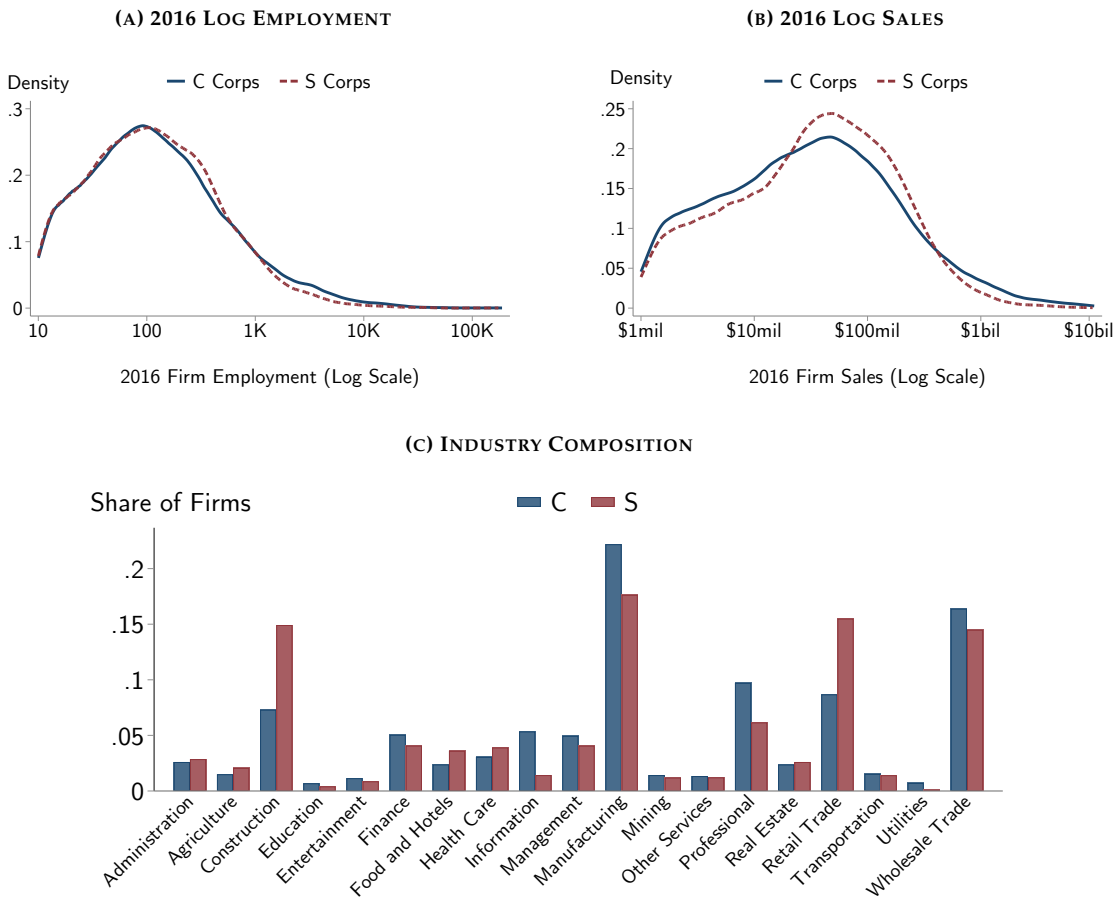
Winsorizing. We winsorize the top and bottom 5% of taxes paid, net investment, and taxable income separately for C and S corps in each year to improve statistical precision and to ensure that the results are not driven by outliers or measurement error. Section 4.4 shows that the results are robust to alternate winsorizing thresholds.

Weighting. The main sample is unbalanced, and we follow Yagan (2015) and Zwick and Mahon (2017) in addressing potential concerns about sample attrition by also reporting results from a balanced panel and by reweighting the sample to match the pre-TCJA joint industry-firm-size distribution. These weights are constructed by estimating a probit regression, in the pre-period, of a C corp indicator on the industry-by-size bin indicators and quintile indicators for capital, sales, and costs. We obtain predicted values from this regression for all firm-years and construct firm-specific inverse propensity scores using the average predicted values in 2015 and 2016, or otherwise the first year the firm enters the sample. In practice, the unweighted and weighted estimates are economically and statistically indistinguishable.

3.3 Descriptive Statistics

Panels A and B of Figure 2 show the unweighted distributions of log firm employment and log firm sales in our sample, and illustrate broad overlap in the size distributions of C and S corps. Panel C shows the NAICS-2 industry composition of the sample, again revealing broad overlap. Most industries exhibit similar shares of C and S corps. Some sectors, such as management and professional services, have a higher proportion of C than S corps, while the reverse is true for others, such as construction and retail trade. Because the analyses will use industry-size-year fixed effects to compare C and S corps in the same industry and employment size bin, the observed sectoral overlap in the sample is sufficient for the empirical design. In robustness checks, we show the results are insensitive to the exclusion of industries in which the firm share of C corps or S corps exceeds 80%. The size, industry, and joint distributions of these variables after reweighting are shown in Appendix B.4, along with raw data trends for key outcomes.

FIGURE 2: FIRM SIZE DISTRIBUTIONS AND INDUSTRY COMPOSITION



Notes: Panels A and B show the unweighted distributions of 2016 log firm employment and log sales, respectively, for C and S corps in the analysis sample. Panel C shows the unweighted NAICS-2 industry composition of firms in the sample. NAICS codes are from Census (2017). The reweighted distributions are shown in Appendix B.4.

Table 1 presents unweighted descriptive statistics for the analysis sample from 2016. The mean firm in the sample pays $0.917 million in federal taxes, has annual sales of $136.6 million, and makes net capital investments of $1.6 million per year. Mean firm employment is 472 workers, and the average worker earns approximately $63,200 per year.

4 Empirical Analysis

We implement a research design comparing trends in outcomes of C and S corps in the same industry-size bin before and after TCJA. The event study specification is given by:

$$y_{ft} = \sum_{t \neq 2016} \beta_t C_f * \mathbf{1}(year = t) + \gamma_f + \alpha_{is(f),t} + \epsilon_{ft} \tag{1}$$

where y_{ft} is an outcome for firm f in year t; C_f is a binary variable equal to 1 if firm f is a C

corp or 0 if it is an S corp; γ_f is a firm fixed effect; and $\alpha_{is(f),t}$ is an industry-size-year fixed effect. The coefficients β_t capture the average differential change in outcomes between C and S corps in the same industry-size bin in year t relative to 2016. We set 2016 as the reference year, allowing us to compare C and S corp trends for several years prior to TCJA and also to observe potential anticipatory tax-shifting behaviors beginning in 2017. Standard errors are clustered by firm.

TABLE 1: SUMMARY STATISTICS

	All Firms		C Corporations				S Corporations			
	(1) Mean	(2) SD	(3) Mean	(4) SD	(5) p50	(6) p90	(7) Mean	(8) SD	(9) p50	(10) p90
Taxes										
Marginal Tax Rate	0.229	0.166	0.176	0.170	0.150	0.350	0.294	0.136	0.364	0.396
Federal Tax (mil)	0.917	1.620	0.846	1.750	0.023	2.877	1.003	1.442	0.267	3.961
Federal Tax Per Worker	6,884	13,457	4,640	8,695	285	16,042	9,612	17,198	2,598	27,240
Sales and Profits										
Sales (mil)	136.6	471.2	160.3	572.2	30.6	297.1	107.8	304.1	36.6	238.1
Costs (mil)	140.6	509.6	170.3	631.3	34.6	307.2	104.5	297.5	34.8	231.7
Operating Profit (mil)	47.6	213.0	60.9	265.7	10.5	107.6	31.4	119.2	10.6	63.4
After-Tax Op. Profit (mil)	46.7	212.6	60.0	265.2	10.0	105.3	30.4	118.9	9.8	61.3
Taxable Income (mil)	2.8	8.4	1.7	9.9	0.3	11.4	4.2	5.8	1.7	14.5
After-Tax Net Income (mil)	1.9	7.2	0.9	8.6	0.2	8.3	3.2	4.8	1.2	11.5
Shareholder Payouts										
Dividends (mil)	2.9	19.6	1.2	18.3	0.0	0.6	5.0	21.0	1.0	11.0
Share Buybacks (mil)	0.1	5.1	0.1	6.2	0.0	0.0	0.1	3.0	0.0	0.0
Total Payouts (mil)	3.2	21.2	1.4	20.1	0.0	1.0	5.4	22.3	1.0	11.9
Capital and Investment										
Capital (mil)	27.1	295.8	39.0	390.3	3.0	51.8	12.8	91.4	1.8	26.1
Net Investment (mil)	1.6	90.7	2.5	121.4	-0.0	3.6	0.5	11.5	-0.0	2.0
Net Investment / Lagged Capital	0.07	0.34	0.08	0.36	-0.02	0.54	0.06	0.32	-0.02	0.48
Payroll and Employment										
Payroll (1120; mil)	25.2	119.2	31.1	144.0	6.5	56.3	18.0	78.7	5.9	37.3
Payroll (W2; mil)	23.4	105.0	28.7	128.1	6.3	52.3	16.9	66.5	5.7	34.8
Employment	472	2,817	544	3,174	106	884	385	2,306	106	701
Worker Earnings and Characteristics										
Mean Annual Earnings (thous)	63.2	59.6	66.8	58.9	55.1	111.1	58.7	60.2	50.0	91.9
Median Annual Earnings (thous)	46.6	29.4	50.1	32.4	42.9	85.0	42.3	24.8	38.8	67.2
95th Centile Earnings (thous)	170.7	282.1	178.0	270.5	131.6	296.8	161.9	295.4	118.6	260.2
Mean Top 5 Earnings (thous)	375	967	393	1,028	218	708	352	887	197	669
Female Share	0.35	0.23	0.36	0.22	0.32	0.71	0.35	0.24	0.28	0.72
Worker Age	42.1	5.1	42.5	5.2	42.9	48.6	41.7	5.1	41.9	47.7
Firm Characteristics										
Firm Age	28.0	21.3	26.6	22.1	22.3	53.9	29.8	20.2	26.8	56.5
Manufacturing	0.20		0.22				0.18			
Capital Intensive	0.58		0.63				0.53			
N Firms	32,649		17,915				14,734			

Notes: The table shows unweighted summary statistics from 2016 for firms in the analysis sample. Medians and centile statistics are fuzzed to protect taxpayer privacy. For data sources and variable definitions see Section 3 and Appendix B.

The identifying assumptions permitting a causal interpretation of the β_t coefficients are (i) that the outcomes of C and S corps would have trended similarly in the absence of TCJA's changes to firms' marginal income tax rates, and (ii) that C and S corps respond similarly to changes in tax rates — that is, they share common elasticities with respect to net-of-tax rates. Assumption (ii) is necessary because both C and S corps receive tax cuts, but of different magnitudes. Both assumptions likely hold in this setting for several reasons.

First, legislative passage of TCJA was widely unexpected prior to the 2016 federal elections, leaving limited scope for firms to anticipate the reform and endogenously adjust their behavior prior to the policy changes. Second, the industry-size-year fixed effects imply that all comparisons are among C and S corps that compete in similar product markets and are subject to the same industry-by-size-specific supply and demand shocks. Third, Yagan (2015) documents that C and S corp trends in real outcomes were statistically indistinguishable for all years in his sample period from 1996-2008, indicating that C and S corps have historically responded similarly to economic shocks and trends. Fourth, in our own sample period, the event studies exhibit parallel trends in the key outcomes of C and S corps in the years prior to the policy reform. Fifth, in Appendix C.1, we replicate and extend research by Zwick and Mahon (2017) to document that the elasticity of investment with respect to bonus depreciation tax shocks is economically and statistically identical for C and S corps, supporting the common elasticity assumption. Lastly, in Section 4.4, we carefully consider additional identification threats, and assess sensitivities to non-MTR features of TCJA, anticipation effects, tax-shifting behaviors, unrelated economic shocks occurring at the same time as TCJA, or other firm characteristics.

To estimate the elasticities that will serve as inputs in the analysis, we pool outcomes in the post-period and use two-stage least squares. The reduced form, first stage, and structural equations are given, respectively, by:

$$y_{ft} = \beta C_f * Post_t + \gamma_f + \alpha_{is(f),t} + \epsilon_{ft} \tag{2}$$

$$\ln(1 - \tau_{ft}) = \delta C_f * Post_t + \gamma_f + \alpha_{is(f),t} + \epsilon_{ft} \tag{3}$$

$$y_{ft} = \varepsilon \ln(1 - \tau_{ft}) + \gamma_f + \alpha_{is(f),t} + \epsilon_{ft} \tag{4}$$

where $\ln(1 - \tau_{ft})$ is the log marginal net-of-tax rate for firm f in year t, $Post_t$ is an indicator equal to 1 for years after 2017, and the fixed effects are the same as in equation 1. Intuitively, we instrument for the change in firms' net-of-tax rate using their pre-existing entity type status as C or S corps. The identifying assumptions underlying this empirical strategy are exogeneity, relevance, monotonicity, and exclusion. We do not claim strict exogeneity in this setting — that is, we do not claim random assignment of C or S status — but rather rely on the weaker claim of parallel trends in the outcome absent the changes in the tax rate (Conley et al. 2012). We examine the relevance and monotonicity conditions below, and return to a discussion of the exclusion restriction when we evaluate mechanisms.

We organize the initial empirical analysis in three parts. First, we document that the research design delivers a strong first stage, with tax rates and tax payments of C corps falling sharply relative to S corps following the tax reform. Second, we provide evidence that corporate taxes induce real distortions in firms' behavior, such that investment, labor demand, sales, and pre-tax profits all increase substantially in response to the tax cuts. Third, we present evidence on the the distributional effects of the tax cuts, in particular on changes in income accruing to firms' shareholders and to workers.

After documenting the core findings, we conduct heterogeneity tests and an investigation of mechanisms, where the focus is naturally related to the task of disentangling the impacts of TCJA's marginal tax rate cuts from other concurrent policy changes. We perform empirical analyses of general equilibrium effects in the labor market, and in the final section of the paper we synthesize the evidence to estimate the MVPF and the incidence of the corporate tax cuts over different population subgroups.

4.1 First Stage: Marginal Tax Rates and Taxes Paid

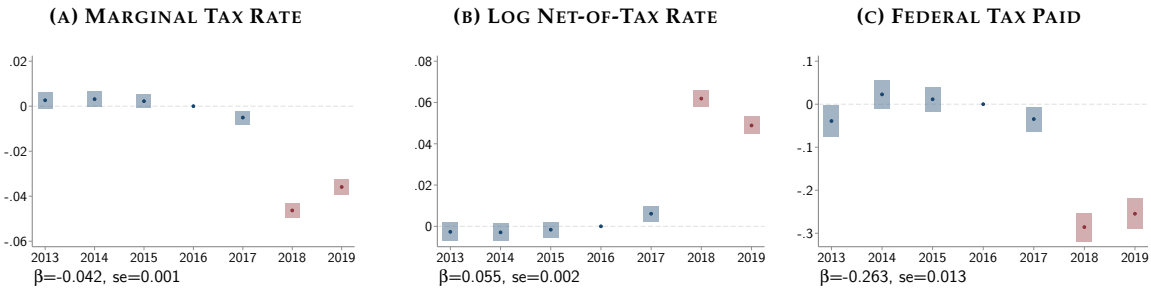
Figure 3 plots the β_t coefficients and 95% confidence intervals from estimating equation 1, using the firms' marginal tax rates and taxes paid as outcomes. Below each panel we report the difference-in-difference coefficient and standard error from equation 2.

Panel A of Figure 3 shows that the observed marginal tax rates of C and S corps trended similarly prior to TCJA, but diverged sharply thereafter. On average, the marginal tax rate of C corps fell by approximately 4.2 percentage points (s.e.=0.1) compared to S corps in the sample. The panel also shows that firms' tax rates started to decline modestly in 2017, even though most of TCJA's provisions did not take effect until 2018. This pattern provides initial evidence that firms engaged in intertemporal shifting behaviors to minimize tax liability, such as reporting costs in 2017 rather than 2018 so that those business expenses could be deducted at a higher rate. We discuss shifting behaviors in greater detail in Section 4.4.

Panel B of Figure 3 shows an analogous version of Panel A where the outcome is transformed as the log net-of-tax rate, $\ln(1 - \tau_{ft})$. On average, C corps saw their net-of-tax rate increase by approximately 5.5% (s.e.=0.2) relative to S corps following TCJA. Below, we use this result to scale other reduced form effects, allowing us to estimate elasticities of key outcomes with respect to changes in the net-of-tax rate.

Panel C shows that the differences in tax rate changes also translated into differences in taxes paid. Estimated using Poisson regressions to account for firms paying zero tax, the panel shows that C corps paid approximately 26.3 percent (s.e.=1.3) less in federal tax in 2019 relative to 2016 compared to S corps. Using the C corp mean from Table 1, this implies an average tax cut of approximately $1,200 per worker.

FIGURE 3: EVENT STUDIES: FIRST STAGE



Notes: The unit of analysis is a firm-year. The panels plot the β_t coefficients estimated from equation 1. These coefficients capture average differences in outcomes between C and S corps over time after controlling for firm and industry-size-year fixed effects and reweighting the sample to match the joint industry-size distribution of firms in the pre-period. Standard errors are clustered by firm and error bands show 95% confidence intervals. The corresponding DiD coefficient and standard error from equation 2 are shown in the bottom left of each panel. The outcome in Panel A is the firm's marginal tax rate, τ_f^{MTR}, measured as a rate in levels (0-1). The outcome in Panel B is the log net-of-tax rate, $\ln(1-\tau_f^{MTR})$. The outcome in Panel C is federal taxes paid. Panel C is estimated using PPML to include observations where the outcome is zero. See Section 3 and Appendix B for data and measurement details.

Table 2 reports the $C \times Post$ estimates produced from equation 2. Similar to the event studies, these coefficients capture the average differences between C and S corps in the pre- and post-periods for each outcome after controlling for firm and industry-size-year fixed effects. Overall, the results in Figure 3 and Table 2 provide evidence of a strong first stage, demonstrating an economically and statistically powerful differential effect of TCJA on the tax rates and tax payments of C versus S corps. These results also indicate that the relevance assumption underlying the instrumental variables strategy is satisfied. Appendix C.2 further shows that the distribution of first stage coefficients is uniformly positive when estimated within different industry and size bins, consistent with the monotonicity assumption.

TABLE 2: MARGINAL TAX RATES AND TAXES PAID

	(1) τ_{ft}^{MTR}	(2) $\ln(1-\tau_{ft}^{MTR})$	(3) Tax Paid	(4) Tax Per Worker
$C_f \times Post_t$	-0.042***	0.055***	-0.263***	-0.248***
	(0.001)	(0.002)	(0.013)	(0.014)
2016 Outcome Mean	0.23	0.77	916,718	6,884
Firm FE	Yes	Yes	Yes	Yes
Industry-Size-Year FE	Yes	Yes	Yes	Yes
N	231,360	231,360	231,360	231,360
N Firms	49,235	49,235	49,235	49,235

Notes: The unit of analysis is a firm-year. The table shows the $C \times Post$ coefficients from equation 2. These coefficients estimate average differential changes in outcomes between C and S corps before and after TCJA, controlling for firm and industry-size-year fixed effects. The outcome in column 1 is the firm's marginal tax rate, τ_f^{MTR}, and the outcome in column 2 is the log net-of-tax rate, $\ln(1-\tau_f^{MTR})$. The outcome in column 3 is federal tax payments, and the outcome in column 4 is tax per worker. The coefficients in columns 3 and 4 are estimated using PPML regressions to include observations where the outcome is zero. Marginal tax rates for S corps are defined as the weighted average of the shareholders' individual marginal tax rates, where the weights are given by the ownership shares. See Section 3 for details on the measurement of tax payments for S corps. The 2016 outcomes means are reported before applying logs or other variable transformations. The data are reweighted to match the industry-size distribution of firms in the pre-period. Standard errors are clustered by firm.

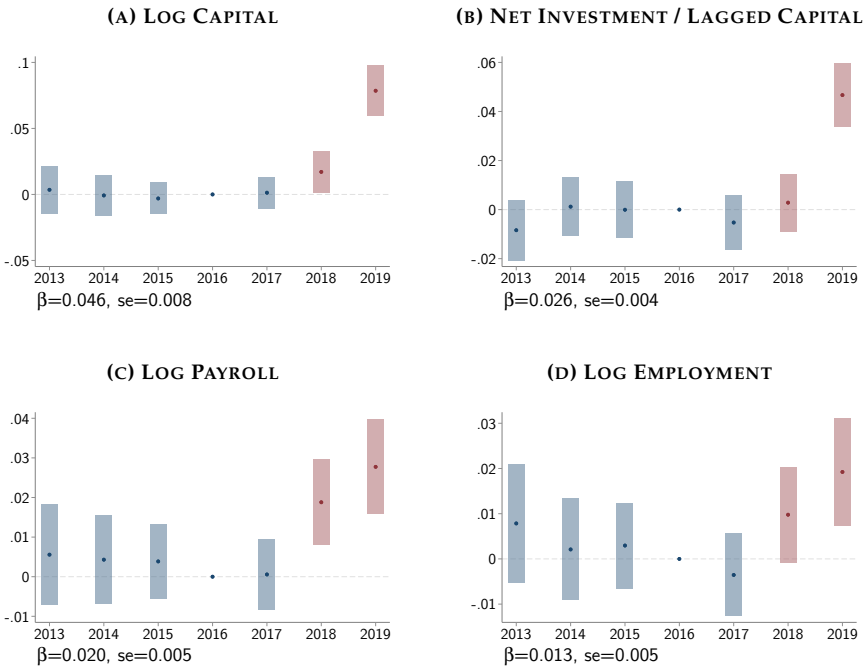
4.2 Evidence of Real Distortions: Capital, Labor, Output, and Pre-Tax Profits

The growth effects of tax cuts hinge on the responsiveness of firms' and workers' real economic behaviors. Changes in investment, hiring, and production decisions are thus central channels through which tax cuts may generate efficiency gains (Auerbach 1983). Below we provide visual and quantitative evidence on the magnitudes of these responses and interpret their implications.

Capital Investment

Because capital expenditures are not fully tax deductible by firms, standard theories predict that corporate taxes distort investment (Hall and Jorgenson 1967). To empirically evaluate the effects of TCJA's corporate tax cuts, Panels A and B of Figure 4 plot the β_t coefficients from equation 1 to assess relative trends in log capital among C and S corps over time. Trends in these outcomes were statistically similar before TCJA, again lending support to the parallel trends assumption underlying the research design. After TCJA, Panel A shows that C corps increased their capital stock relative to S corps by 4.6 percent (s.e.=0.8). Panel B shows a 2.6 percent (s.e.=0.4) increase in their net investment rate.

FIGURE 4: EVENT STUDIES: CAPITAL INVESTMENT AND LABOR DEMAND



Notes: The unit of analysis is a firm-year. The panels plot the β_t coefficients estimated from equation 1. These coefficients capture average differences in outcomes between C and S corps over time after controlling for firm and industry-size-year fixed effects and reweighting the sample to match the joint industry-size distribution of firms in the pre-period. Standard errors are clustered by firm, and error bands show 95% confidence intervals. The corresponding DiD coefficient and standard error from equation 2 are shown in the bottom left of each panel. See Section 3 and Appendix B for data and measurement details.

Columns 1 and 3 of Table 3 Panel A present the $C \times Post$ coefficients associated with the event studies in Figure 4. Columns 2 and 4 report the elasticity of capital and investment with respect

to the net-of-tax rate using equation 4. Using 2SLS to scale the reduced form estimates in columns 1 and 3 by the net-of-tax-rate yields a capital elasticity of 0.82 (s.e.= 0.14) and an investment rate semi-elasticity of 0.45 (s.e.= 0.07).

TABLE 3: CAPITAL, LABOR, OUTPUT, AND PRE-TAX PROFITS

(A) CAPITAL AND INVESTMENT

	(1) $\ln(K_{ft})$	(2) $\ln(K_{ft})$	(3) $I_{ft}/K_{f,t-1}$	(4) $I_{ft}/K_{f,t-1}$
$C_f \times Post_t$	0.046***		0.026***	
	(0.008)		(0.004)	
$\ln(1-\tau_{f,t})$		0.824***		0.449***
		(0.138)		(0.070)
2016 Outcome Mean	27.1	27.1	0.07	0.07
Firm FE	Yes	Yes	Yes	Yes
Industry-Size-Year FE	Yes	Yes	Yes	Yes
First-Stage F		1,308		1,245
N	231,360	231,360	204,411	204,411
N Firms	49,235	49,235	42,711	42,711

(B) PAYROLL AND EMPLOYMENT

	(1) $\ln(\bar{w}_{ft}L_{ft})$	(2) $\ln(\bar{w}_{ft}L_{ft})$	(3) $\ln(L_{ft})$	(4) $\ln(L_{ft})$
$C_f \times Post_t$	0.020***		0.013***	
	(0.005)		(0.005)	
$\ln(1-\tau_{f,t})$		0.369***		0.233***
		(0.086)		(0.084)
2016 Outcome Mean	23.4	23.4	472	472
Firm FE	Yes	Yes	Yes	Yes
Industry-Size-Year FE	Yes	Yes	Yes	Yes
First-Stage F		1,308		1,308
N	231,360	231,360	231,360	231,360
N Firms	49,235	49,235	49,235	49,235

(C) SALES, COSTS, AND PRE-TAX PROFITS

	(1) ln(Sales)	(2) ln(Costs)	(3) ln(O.Pft)	(4) ln(Tax Inc)	(5) **1**(Tax Inc>0)	(6) ln(Tax Inc)
$C_f \times Post_t$	0.027***	0.019***	0.023***	0.059***	0.001	
	(0.005)	(0.004)	(0.005)	(0.013)	(0.004)	
$\ln(1-\tau_{f,t})$						0.697***
						(0.158)
2016 Outcome Mean	136.6	140.6	47.6	2.8	0.75	2.8
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Size-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
First-Stage F						3,093
N	231,360	231,360	229,274	167,773	231,360	167,773
N Firms	49,235	49,235	48,842	39,032	49,235	39,032

Notes: The unit of analysis is a firm-year. The table reports the $C \times Post$ coefficients from equation 2. These coefficients estimate average differential changes in outcomes between C and S corps before and after TCJA, controlling for firm and industry-size-year fixed effects. The table also reports corresponding net-of-tax elasticities from equation 4. In Panel A, the outcome in columns 1 and 2 is log capital, and the outcome in columns 3 and 4 is net investment scaled by lagged capital. The sample size declines slightly in columns 3 and 4 because lagged capital is unobservable for a small fraction of firms. In Panel B, the outcome in columns 1 and 2 is log payroll, and the outcome in columns 3 and 4 is log employment. In Panel C, the outcomes in columns 1-5 are (log) sales, costs, pre-tax operating profits, and the intensive and extensive margins of taxable income. Column 6 reports the elasticity of taxable income. Non-positive observations are excluded from the regressions when the outcome is logged. The 2016 outcome means are reported before taking logs (if applicable), and dollar-denominated outcomes are scaled in nominal millions. The data are reweighted to match the joint industry-size distribution of firms in the pre-period. Standard errors are clustered by firm. See Section 3 and Appendix B for data and measurement details.

The magnitudes of these effects are both economically and statistically significant, implying that the mean C corp in the sample increased its capital stock relative to S corps by approximately $1.8 million. In Section 4.5, we further report investment elasticities with respect to comprehensive measures of user costs.

Payrolls and Employment

Panels C and D of Figure 4 show the results from estimating equation 1 to assess relative trends in payroll and employment growth. The plots show that the labor market outcomes of C and S corps followed similar trends prior to TCJA. After TCJA, Panel C shows that total payrolls of C corps increased relative to S corps by 2.0% (s.e.=0.5) on average. Employment, shown in Panel D, also trended upward, by 1.3% (s.e.=0.5).

These difference-in-difference estimates and the accompanying labor demand elasticities are reported in Columns 1-4 of Table 3 Panel B. After scaling the reduced form effects by the corresponding net-of-tax change, the results yield a payroll elasticity of 0.37 (s.e.= 0.09) and an employment elasticity of 0.23 (s.e.= 0.08). The increases in payrolls and employment imply that the tax cuts caused firms to increase their labor demand. In Section 4.3 we assess the distributional effects of increased labor demand on different types of workers, and in Section 4.6 we estimate general equilibrium effects on employment and workers' earnings.

Sales, Costs, and Pre-Tax Profits

The increases in factor demand documented in Figure 4 imply that firms increased their output in response to the tax cuts, and furthermore, that it was profitable for them to do so. Consistent with this view, Figure 5 shows upward trends in the sales, costs, and pre-tax profits of C relative to S corps after TCJA. Panel A shows that C corps increased their sales relative to S corps by 2.7 % (s.e.=0.5). The effect is precisely estimated and economically significant: using values from Table 1, the coefficient implies that the mean C corp increased its annual sales by approximately $4.4 million relative to comparable S corps.

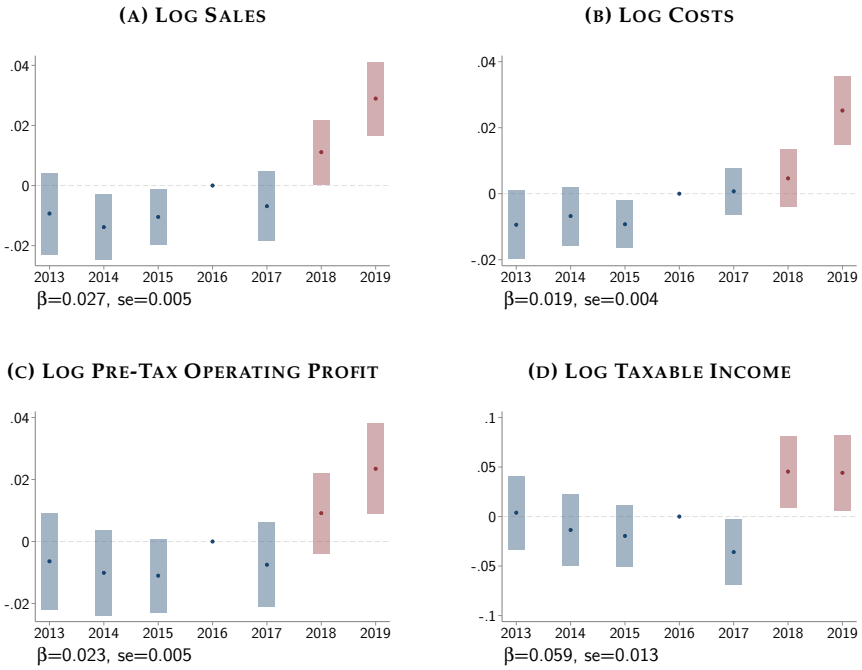
Reflecting higher capital and labor inputs, C corps also faced higher costs. The cost increase is shown in Panel B, although the magnitude of the increase (1.9 %, s.e.=0.4) is smaller than for sales. Given sharply increasing sales and modestly increasing costs, Panel C shows that the average pre-tax operating profits of C corps also increased relative to S corps, by 2.3% (s.e.=0.5). Panel D similarly reveals a clear and sharp increase in taxable income of C corps relative to S corps.[2]

Columns 1 to 4 of Table 3 Panel C show the $C \times Post$ coefficients associated with the event studies in Figure 5. Column 5 reports results from the same specification where the outcome is an indicator for whether taxable income is positive, and shows that we do not observe effects on the

[2]Appendix C.3 presents evidence that the dip in taxable income in 2017 and subsequent immediate increase in 2018 partly reflects intertemporal shifting by C corps in 2017 and 2018 to minimize their tax burdens. Section 4.4 reports robustness checks showing that the main estimates are robust to excluding 2017 and 2018 from the sample. Appendix C.4 studies both the intensive and extensive margin responses of taxable income and other outcomes. Accounting for extensive margin responses does not significantly affect the estimates.

extensive margin. Column 6 reports the elasticity of taxable income with respect to the net-of-tax rate using equation 4. Using 2SLS to scale the reduced form estimate in column 4 by the net-of-tax rate yields a taxable income elasticity of 0.70 (s.e.= 0.16). As shown by Feldstein (1999), under plausible assumptions the elasticity of taxable income (ETI) can be used to estimate the welfare impacts and efficiency costs of tax changes. In general, a larger ETI implies greater deadweight loss, since it implies a larger distortion of economic activity due to the tax.[3]

FIGURE 5: EVENT STUDIES: FIRM SALES, COSTS, AND PRE-TAX PROFITS



Notes: The unit of analysis is firm-year. The panels plot the β_t coefficients estimated from equation 1. These coefficients capture average differences in outcomes between C and S corps over time after controlling for firm and industry-size-year fixed effects and reweighting the sample to match the joint industry-size distribution of firms in the pre-period. Standard errors are clustered by firm, and error bands show 95% confidence intervals. The corresponding DiD coefficient and standard error from equation 2 are shown in the bottom left of each panel. The decline in taxable income in 2017 in Panel D reflects intertemporal income shifting by firms; see Appendix C.3. See Section 3 and Appendix B for data and measurement details.

Our estimate of the corporate ETI, 0.70, is smaller relative to most estimates identified from policy variation in small open economies. For example, Suárez Serrato and Zidar (2016) report elasticities of establishment growth with respect to U.S. state corporate tax cuts ranging from approximately 3.0-4.0; and Bachas and Soto (2021) find taxable income elasticities of 3.0-5.0 for small firms in Costa Rica. By contrast, our estimate of the corporate ETI is on the higher end of most existing estimates of the ETI for personal incomes, which Saez et al. (2012) find in a literature review ranges from approximately 0.14 to 0.40, with a central estimate of 0.25.

In the context of prior research, we interpret our corporate ETI estimate as consistent with the

[3]Because the ETI reflects features of the tax regime such as the tax base and the enforcement environment, it should be interpreted as a policy-specific reduced form elasticity rather than a structural parameter (Kleven 2021). However, it offers a policy-relevant summary of the responsiveness of the corporate tax base to marginal tax rate changes under the prevailing regime.

standard view in public finance that tax distortions vary with factor mobility. Firms and workers are less mobile at the federal level than at the state and local level, mitigating distortions from the federal corporate tax relative to the state and local corporate tax. However, capital is generally more mobile than labor (Kotlikoff and Summers 1987), suggesting that federal taxes on labor income, the primary source of personal income tax revenue, may be less distortive than the federal corporate tax.

Overall, the results in this section provide clear evidence that firms expanded in response to tax cuts, consistent with the conventional view that tax cuts reduce distortions in the economy and increase growth.

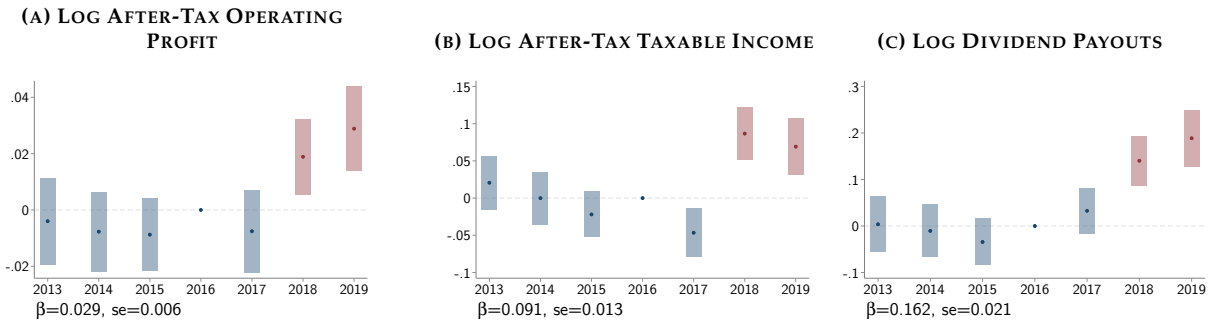
4.3 Evidence of Distributional Effects: After-Tax Profits, Shareholder Payouts, and Workers' Earnings

After-Tax Profits and Dividend Payouts

Tax cuts not only induce changes in factor quantities but may also affect prices and the after-tax income accruing to different factors. These changes, in turn, may generate uneven benefits for individuals across the economy.

To investigate gains for firm shareholders, Figure 6 presents the event studies for after-tax operating profits, and net after-tax profit (i.e., taxable income less taxes paid), and dividend payouts to shareholders. Panels A and B of Figure 6 show that the after-tax operating profits and after-tax taxable income of C corps increased relative to S corps following TCJA by 2.9 % (s.e.=0.6) and 9.1 % (s.e.=1.3), respectively. Firms also returned some of these profits to their shareholders via dividends.[4] Panel C of Figure 6 reports that log payouts of C corps relative to S corps increased by 16.2% (s.e.=2.1).

FIGURE 6: EVENT STUDIES: AFTER-TAX PROFITS AND DIVIDEND PAYOUTS



Notes: The unit of analysis is a firm-year. The panels plot the β_t coefficients estimated from equation 1. These coefficients capture average differences in outcomes between C and S corps over time after controlling for firm and industry-size-year fixed effects and reweighting the sample to match the joint industry-size distribution of firms in the pre-period. Standard errors are clustered by firm and error bands show 95% confidence intervals. The corresponding DiD coefficient and standard error from equation 2 are shown in the bottom left of each panel. The decline in after-tax taxable income in 2017 in Panel B reflects intertemporal income shifting by firms; see Appendix C.3. See Section 3 and Appendix B for data and measurement details.

[4]Total payouts to shareholders are the sum of dividend payouts and share buybacks. In our sample of private firms, buybacks account for less than 1% of aggregate payouts. Appendix C.4 reports that C corps also increased buybacks.

Table 4 presents the difference-in-difference estimates corresponding to these event studies, along with estimates of changes in the extensive margin. We do not observe significant responses on the extensive margin for payouts or after-tax profits. The elasticity of after-tax profits with respect to the net-of-tax rate, estimated in column 6 from equation 4, is 1.06 (s.e.= 0.16). The estimate is economically and statistically significant, and underscores that tax cuts are lucrative to shareholders. Later, we use this parameter as an input to estimate the incidence of TCJA's tax cuts on capital. Collectively, the results from Figure 6 and Table 4 foreshadow that corporate firm owners bear a portion of the short-run incidence of the corporate tax.

TABLE 4: AFTER-TAX PROFITS AND DIVIDEND PAYOUTS

	(1) ln(Dividends)	(2) **1**(Dividends>0)	(3) ln((1 − τ_{ft})O.Pft)	(4) ln((1 − τ_{ft})π)	(5) **1**((1 − τ_{ft})π >0)	(6) ln((1 − $\tau_{ft}\pi$))
$C_f \times Post_t$	0.162***	0.013***	0.029***	0.091***	-0.003	
	(0.021)	(0.003)	(0.006)	(0.013)	(0.004)	
$\ln(1-\tau_{f,t})$						1.064***
						(0.156)
2016 Outcome Mean	6.3	0.46	47.4	4.1	0.74	4.1
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Size-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
First-Stage F						3,098
N	101,544	231,360	228,137	166,197	231,360	166,197
N Firms	22,921	49,235	48,708	38,833	49,235	38,833

Notes: The unit of analysis is firm-year. Columns 1-3 show the $C \times Post$ coefficients from equation 2. These coefficients estimate average differential changes in outcomes between C and S corps before and after TCJA, controlling for firm and industry-size-year fixed effects. The outcome in column 1 is log dividend payouts (the intensive margin) and in column 2 is an indicator for any dividend payouts (the extensive margin). The outcome in column 3 is log after-tax operating profit, and the outcomes in columns 4-5 are the intensive and extensive margins of after-tax taxable income, respectively. Column 6 reports the after-tax profit elasticity estimated from equation 4. Non-positive observations are excluded from the regressions when the outcome is logged. Where applicable, the 2016 outcome means are reported before taking logs in millions of nominal dollars. The data are reweighted to match the joint industry-size distribution of firms in the pre-period. Standard errors are clustered by firm. For additional information on data sources and variable definitions see Section 3 and Appendix B. For analyses of negative profits and extensive margin responses see Appendix C.4.

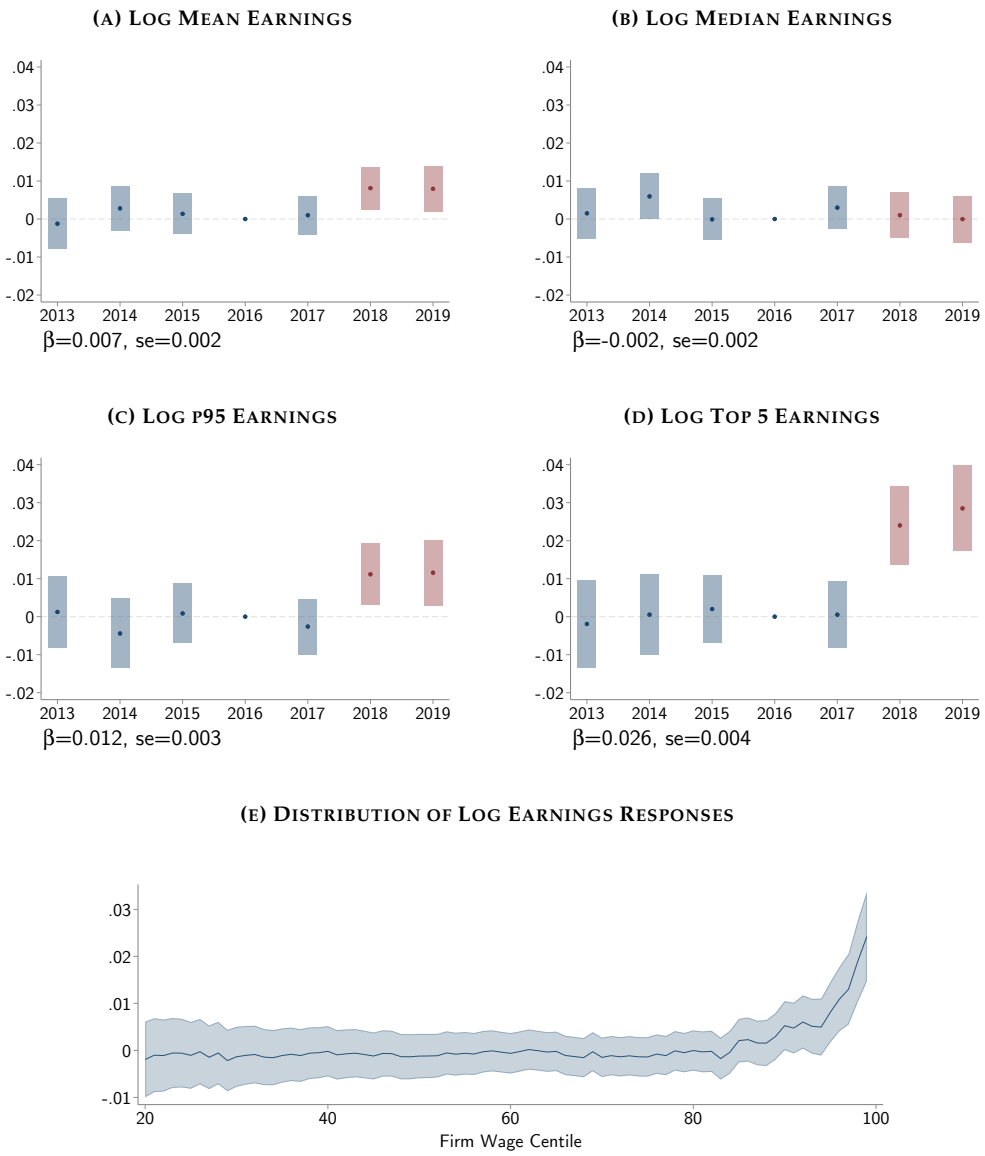
Workers' Earnings

We use the matched employer-employee data to study the effects of the tax cuts on workers' earnings. Panel A of Figure 7 shows that mean log employee earnings at C and S corps trended similarly before TCJA, and then increased modestly for the average C corp worker, by 0.7% (s.e.=0.2), following the tax cut.

However, this average effect masks heterogeneity across workers. Panel B shows that the earnings of the median worker at the firm evolved similarly for both C and S corps over the entire sample period, and implies that corporate tax cuts did not have a differential effect on earnings for the typical worker (-0.2%, s.e.=0.2).[5] By contrast, Panel C shows that the earnings of C corp workers at the 95th centile increased relative to their counterparts in S corps (1.2%, s.e.=0.3), and Panel D indicates that earnings of executives (as proxied by the top five highest paid workers in the firm) increased even more sharply, by 2.6% (s.e.=0.4).

[5]Models of perfect labor market competition predict that wages increase equally for workers of both C and S corps. It is possible that these general equilibrium effects are absorbed in our industry-size-year fixed effects. We evaluate the general equilibrium effects of the tax cuts on workers' earnings in Section 4.6.

FIGURE 7: EVENT STUDIES: WORKERS' EARNINGS



Notes: The unit of analysis is a firm-year. Panels A-D plot the β_t coefficients estimated from equation 1. These coefficients capture average differences in outcomes between C and S corps over time after controlling for firm and industry-size-year fixed effects and reweighting the sample to match the joint industry-size distribution of firms in the pre-period. Standard errors are clustered by firm and error bands show 95% confidence intervals. The corresponding DiD coefficient and standard error from equation 2 are shown in the bottom left of each panel. Panel E plots the β coefficients obtained from equation 2, where the outcomes are centiles of the distribution of workers' log earnings within the firm. For example, centile 50 corresponds to the log annual earnings of the median worker within the firm, and centile 90 captures the log annual earnings of the worker at the 90th percentile within the firm. We exclude the bottom 20% of the distribution, which is imprecisely measured and estimated due to the presence of part-time workers. Standard errors are clustered by firm. Error bands show 95% confidence intervals. See Section 3 and Appendix B for data details.

To more comprehensively evaluate the effects of TCJA on the distribution of workers' earnings, we estimate specifications of equation 2 where the outcome $y_{ft}(q)$ is log annual earnings of workers in firm f and year t at within-firm quantile q. For example, $y_{ft}(q = 50)$ uses log median earnings as the outcome, as shown in Panel B of Figure 7, and $y_{ft}(q = 95)$ uses the 95th percentile of log

worker earnings as the outcome, as in Panel C.[6]

Panel E of Figure 7 plots the $C \times Post$ coefficients from these regressions along with their corresponding 95% confidence intervals. The figure shows that the relative earnings of workers in C and S corps below the 90th percentile are statistically indistinguishable following TCJA; we cannot reject that the coefficients are statistically different from zero. However, the figure reveals a different pattern for workers in the top 10% of the earnings distribution. On average, workers at the 90th percentile of the within-firm distribution see their relative earnings change by 0.7% (s.e.=0.3), and these impacts grow quantitatively larger further up the distribution. At the 95th percentile, we estimate a relative earnings increase of 1.2% (s.e.=0.3) for C corp workers, and this magnitude climbs to 2.3% (s.e.=0.5) at the 99th percentile.

Panel A of Table 5 reports the $C \times Post$ coefficients obtained from estimating equation 2, as well as the dependent variable means in the baseline year and implied elasticities with respect to the net-of-tax rate from equation 4. For workers at the 95th percentile, we estimate an earnings elasticity of 0.22 (s.e.=0.06), and for executives we estimate a larger earnings elasticity of 0.47 (s.e.=0.08). The mean baseline earnings of these workers and executives are high: the average worker in the sample at the 95th percentile of the within-firm distribution earns $170,740 per year, and the average worker in the top five earns $374,595 per year. Applying the baseline sample levels, the average firm net-of-tax-rate change in the sample, and the estimated net-of-tax elasticity, the results imply that average executive earnings increased by approximately $9,700. Similar computations yield that average earnings for workers at the 95th percentile increased by approximately $2,100, while relative gains for workers below the 90th percentile are statistically indistinguishable from zero.

What drives the sharp increase in executive pay? The joint increase in sales, profits, investment, and payrolls of C corps relative to S corps implies there is scope for managerial decision making and effort to drive firm growth. Moreover, firms may incentivize managerial effort by explicitly compensating executives on the basis of firm performance metrics (e.g., Jensen and Murphy 1990). On the other hand, to the extent that executives have significant bargaining power vis-a-vis shareholders, they may be in a position to extract a portion of after-tax profits even in the absence of increases in managerial productivity.

Panel B of Table 5 presents a series of empirical tests developed by Ohrn (2023) to evaluate the relevance of these mechanisms, which are not necessarily mutually exclusive. The outcome in all columns is log executive pay. The first column shows the benchmark specification given by equation 2. The remaining columns respectively add controls for three measures of firm performance: sales growth, profit growth, and sales growth relative to other firms in the same industry. To the extent that executive pay is correlated with these measures, we may expect the $C \times Post$ coefficient to shrink as we add the controls. The results in columns 2-4 show that the $C \times Post$ coefficient on executive pay shrinks only modestly after adding controls for the firm performance metrics. The benchmark estimate of 2.6% declines to 2.0% when controlling for sales

[6] Appendix C.5 shows that workers in the top 10% of the within-firm distribution are extremely likely to also be in the top 10% of the national distribution.

growth, and shrinks by a smaller amount when controlling for profit growth or sales growth relative to other firms in the same industry. The results thus imply that increases in executive pay were only modestly correlated with measures of firm performance after the tax cuts.

TABLE 5: WORKERS' EARNINGS

(A) ALL WORKERS

	(1) $\ln(\bar{w})$	(2) $\ln(w^{50})$	(3) $\ln(w^{95})$	(4) $\ln(w^{exec})$
$C_f \times Post_t$	0.007***	-0.002	0.012***	0.026***
	(0.002)	(0.002)	(0.003)	(0.004)
2016 Outcome Mean	63,158	46,581	170,740	374,595
ε	0.130	-0.027	0.224	0.466
s.e.	0.040	0.039	0.057	0.079
Firm FE	Yes	Yes	Yes	Yes
Industry-Size-Year FE	Yes	Yes	Yes	Yes
N	231,360	231,360	231,360	231,360
N Firms	49,235	49,235	49,235	49,235

(B) EXECUTIVE PAY

	(1) $\ln(w^{exec})$	(2) $\ln(w^{exec})$	(3) $\ln(w^{exec})$	(4) $\ln(w^{exec})$
$C_f \times Post_t$	0.026***	0.020***	0.022***	0.026***
	(0.004)	(0.004)	(0.004)	(0.004)
Firm FE	Yes	Yes	Yes	Yes
Industry-Size-Year FE	Yes	Yes	Yes	Yes
Controls X_{ft}	None	Sales	Profit	Rel. Sales
N	231,360	231,360	231,360	231,360
N Firms	49,235	49,235	49,235	49,235

(C) INCUMBENTS AND NEW HIRES

	Incumbents		New Hires	
	(1) $\ln(w^{50})$	(2) $\ln(w^{95})$	(3) $\ln(w^{50})$	(4) $\ln(w^{95})$
$C_f \times Post_t$	0.000	0.020***	-0.004	0.002
	(0.002)	(0.003)	(0.005)	(0.006)
2016 Outcome Mean	67,274	234,160	28,293	86,736
Firm FE	Yes	Yes	Yes	Yes
Industry-Size-Year FE	Yes	Yes	Yes	Yes
N	227,190	227,190	226,371	226,371
N Firms	48,171	48,171	48,702	48,702

Notes: Unit of analysis is firm-year. Panel A reports the $C \times Post$ coefficients from equation 2, where the (log) outcomes are mean earnings, median earnings, 95th centile earnings, and executive pay (defined as the top 5 highly paid workers in the firm). These coefficients estimate average differential changes in outcomes between C and S corps before and after TCJA, controlling for firm and industry-size-year fixed effects. The corresponding net-of-tax elasticities are shown in the table notes, as estimated from equation 4. Panel B estimates variations of equation 2 where the outcome is log executive pay, and adds time-varying controls for several measures of firm performance. In column 3 the specification includes an indicator for observations where profits are non-positive. Panel C estimates equation 2 separately for incumbents in columns 1 and 2 (defined as "stayers" who are employed at the same firm over our entire sample period) and new hires in columns 3 and 4. The outcomes are log median earnings and log 95th centile earnings. The 2016 outcome means are reported before taking logs in nominal dollars. The data are reweighted to match the joint industry-size distribution of firms in the pre-period. Standard errors are clustered by firm.

Panel C reports changes in log earnings separately for incumbent workers and new hires. If earnings increases are driven by changes in productivity, we may expect the price of the marginal unit of labor (i.e., new hires) to increase, while if increases are driven by rent-sharing, we may

expect gains to be concentrated among incumbent workers. We use a restrictive definition of incumbents, corresponding to "stayers" who are employed at the same firm in every year of our sample (e.g., as in Kline et al. 2019). The results show that earnings increases are driven entirely by gains for high-income incumbents in the firm, consistent with a rent-sharing mechanism.[7] However, we caution that these tests are suggestive rather than dispositive: the econometric problems with conditioning on post-treatment outcomes are well-known (e.g., Imbens 2020), the relevant marginal unit of labor may be hours or effort (which we do not observe) rather than new workers, and increasing managerial productivity may not be fully reflected in firm performance metrics over the two-year sample horizon.

Collectively, these results are consistent with studies documenting evidence of rent-sharing with high-income workers and incumbents in response to tax or productivity shocks (Ohrn 2023; Carbonnier et al. 2022; Dobridge et al. 2021; Kline et al. 2019), and with a growing literature emphasizing that labor market frictions may generate firm-specific pay premia or wage markdowns (Card 2022). Our results are also consistent with Risch (2024), who finds that the incidence of tax increases on S corporations falls mainly on shareholders and high-income workers in the firm. By contrast, our results differ from Fuest et al. (2018), who study municipal corporate tax changes in Germany and find that the incidence of tax increases falls substantially on low-skilled and marginally attached workers.[8]

Overall, the labor market findings suggest that the short-run effects of corporate tax cuts are regressive, increasing earnings only for workers at the top of the within-firm earnings distribution. We return to the task of assessing the incidence of the corporate tax cuts in Section 5.

4.4 Robustness, Heterogeneity, and Additional Results

Alternate Specifications. Appendix C.6 reports results from alternate modeling specifications, where we include controls for: state-by-year fixed effects; 6-digit NAICS industry-by-year fixed effects; firm age-by-year fixed effects; and pre-existing trends. We also report specifications with only firm and year fixed effects; unweighted regressions; robustness to various winsorizing thresholds; and firm size-weighted regressions. The elasticity estimates across specifications and outcomes are stable and broadly within the confidence interval of the benchmark specification.

Alternate Samples. Appendix C.7 reports robustness to using alternate estimation samples that: exclude industry-size cells with unbalanced shares of C or S corps; exclude firms most likely affected by the 2018-2019 U.S.-China trade war; use a balanced panel; exclude tax years 2017 and 2018 from the estimation; exclude single-owner S corps; include public firms; or include multinational firms.

[7] The results for incumbents in Panel C also imply that the estimated effects on the within-firm earnings distribution in Panel A are not driven by changes in the composition of workers.

[8] A possible explanation for this difference, discussed in Fuest et al. (2018), is that the labor incidence of corporate tax hikes and tax cuts may be asymmetric. Another possible reconciliation is that the wage effects in Fuest et al. (2018) are driven by workers in small firms, whom they argue may be relatively immobile in Germany and thus bear a larger share of the incidence.

Other Provisions of TCJA. We summarize the major concurrent provisions of TCJA affecting corporations in Appendix A.7. While we cannot rule out modest interactions, it is unlikely that other provisions of TCJA are main drivers of our results for four reasons. First, the difference-in-difference design implicitly controls for concurrent policies to the extent that C and S corps were similarly affected, since TCJA's other policy changes legally applied to both C and S corps. Second, the legislative budget scoring report by the Congressional Joint Committee on Taxation (2017) projected that the rate cuts would reduce tax liabilities more than any other TCJA provision (see Appendix A.7). Third, Appendix C.7 reports additional tests to control for exposure to concurrent policies, and shows that these controls do not significantly influence the results. Fourth, a theory-driven approach to modeling firms' user costs suggests the tax rate change was the dominant driver; we provide details on this approach in Section 4.5.

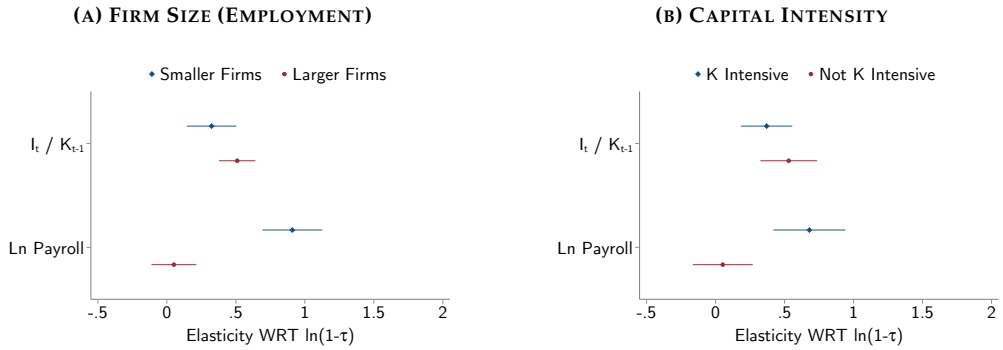
Accounting for Temporary Policies and Expectations. Appendix C.8 explores sensitivity of the results to accounting for forward-looking behavior by firms in a dynamic model with temporary policies and adjustment costs. Some provisions of TCJA affecting S corps, including the top individual rate cut and the qualified business income deduction, were ostensibly scheduled to expire in 2026, whereas the provisions affecting C corps were ostensibly permanent.[9] Adopting the approach of Auerbach and Hassett (1992) to model dynamic tax policies, Appendix C.8 shows that the net-of-tax elasticities remain close the benchmark results across a broad range of assumptions about firms' forward-looking behavior and adjustment costs. In practice, tax changes in 2026 were too distant in the future and too uncertain to meaningfully affect firms' behavior in 2018 and 2019.

Heterogeneity. Figure 8 reports tests for heterogeneity in firms' capital and labor demand responses with respect to firm size and capital intensity. We do not observe clear differences in investment of smaller relative to larger firms, but there is strong evidence that smaller firms were more likely to expand payrolls. Although salary and wage expenses are fully tax deductible, the payroll response for small firms is consistent with the view that hiring, training, and retaining workers may impose additional costs on firms that are not fully deductible in practice. Capital intensive firms also show a stronger payroll elasticity. Taken together, the results are suggestive of liquidity effects for small firms, and consistent with price effects driven by reductions in the user cost of capital for all firms. We report additional heterogeneity results in Appendix C.9, and explore the user cost mechanism in greater detail below.

Evaluating Results at a Longer Time Horizon. Appendix C.10 examines how the Covid-19 pandemic and related policy responses affect the internal and external validity of the research design beginning in 2020. Federal pandemic policy differentially affected C and S corps, confounding causal intepretations. Capital, pre-tax profits, and after-tax profits of C corps remain elevated relative to S corps five years after TCJA. Payroll and wage differences are attenuated, likely reflecting the impact of the federal Paycheck Protection Program, which disproportionately benefitted S corps.

[9] Ostensibly temporary tax policies are often repeatedly extended by Congress, and policies are rarely truly permanent in a democracy. Congress and the President passed legislation extending key provisions of TCJA in 2025.

FIGURE 8: FIRM HETEROGENEITY



Notes: The unit of analysis is a firm-year. The panels report heterogeneity in the investment and payroll elasticities. Firm size is measured as initial employment. Capital intensity is the ratio of capital to sales at the industry level. The sample is split at the median for both variables. All specifications control for firm and industry-size-year fixed effects and reweight the sample to match the joint industry-size distribution of firms in the pre-period. Error bars show 95% confidence intervals. Appendix C.9 reports heterogeneity tests for additional outcomes.

4.5 User Cost Elasticities

In this section we quantify firms' responses with respect to holistic measures of user costs. Consider first a stylized model where firm optimize after-tax profits π:

$$\pi = F(K, L)(1 - \tau^c) - wL(1 - \tau^c) - rK(1 - \theta\tau^c) \tag{5}$$

where τ^c is the corporate income tax rate (or the implied marginal business income tax rate for S corps) and $\theta \in [0, 1]$ is an expensing rate parameter capturing the share of non-wage production costs that are tax deductible. These costs may include fully deductible capital purchases (such as durable equipment eligible for bonus depreciation), partially deductible capital purchases (such as structures), and non-deductible costs (such as managerial effort and hiring or training costs, to the extent that they are not reflected in cost-deductible labor compensation). $F(K, L)$ may vary across firms due to heterogeneous productivities, such that some firms produce higher output than others with a fixed set of inputs. The first-order condition with respect to capital yields:

$$\underbrace{\frac{\partial F}{\partial K}}_{\text{MRPK}} = \underbrace{\frac{(1 - \theta\tau^c)}{1 - \tau^c} r}_{\text{cost of capital}} \equiv \phi \tag{6}$$

where the left side of the equation is the marginal revenue product of capital, and the right side is the user cost of capital, denoted by ϕ. Either decreasing the tax rate τ or increasing the expensing parameter θ lowers the user cost ϕ. The corporate tax distorts firms' decisions because capital costs are only imperfectly deductible from the tax base. Moving beyond the stylized model, the Hall and Jorgenson (1967) user cost formula can be extended to incorporate additional features of the U.S. tax system as follows:

$$\phi = \left(r^d \left(1 - \tau^d\right) \alpha + r^e (1 - \alpha) - \rho + \delta \right) \cdot \left(\frac{\sum_k s_k (1 - \lambda z_k \tau^c)}{1 - \tau^c} \right) \tag{7}$$

where r^d and r^e are the cost of debt and equity, respectively; α and $(1 - \alpha)$ are the share of debt vs. equity financing, respectively; s_k is the capital share with depreciation schedule k; z_k is

the present value of deductions from bonus depreciation allowances for capital of class k; λ is the effective share of capital investments eligible for bonus; τ^c is the marginal income tax rate (adjusted for operating loss deductions and, in the case of S corps, the qualified business income deduction); τ^d is the marginal tax shield from debt; ρ is the inflation rate; and δ is the depreciation rate. We estimate or calibrate each of these parameters as follows, using our data sample where possible and otherwise relying on estimates from the literature. The subscript f denotes firms, i denotes industries, is denotes industry-size pairs, j denotes entity type (C or S), k denotes asset classes, and t denotes years. Parameters without subscripts are applied uniformly to all firms in all years, and additional measurement details are provided in Appendix B.2.

The interest rate on debt r^d_{ft} is net interest payments divided by debt. The risk-adjusted cost of equity r^e is calibrated to 0.089 (Damodaran 2025). The debt share α_{ft} is the ratio of debt to market value, with market values estimated as in Smith et al. (2023). Baseline capital shares s_{isjkt} and expensing rates z_{isjkt} are estimated in the pre-period following Zwick and Mahon (2017). After TCJA, the introduction of full expensing implies that z_{isjkt} is uniformly set to 1 for bonus-eligible capital. The scaling factor λ accounts for incomplete take-up of bonus (Kitchen and Knittel 2016). The annual inflation rate ρ is set to 1.5%, the average over our sample period (BLS 2024). The depreciation rate δ_{isj} is depreciation scaled by lagged capital. τ^c_{ft} is the top statutory income tax rate on business income adjusted for applicable deductions (see Appendix B for details). The marginal tax shield from debt τ^d_{ft} is equal to τ^c_{ft} before TCJA and afterwards is equal to $\tau^c_{f,t} \cdot \min(c_{f,t}, 0.3 \cdot \max(0, Y_{f,t}))/c_{f,t}$, where c denotes interest costs and Y denotes EBITDA. Firms below the post-TCJA interest limitation threshold deduct interest costs at the marginal income tax rate and firms above the threshold deduct costs at a lower effective rate equal to the product of the marginal tax rate and the share of interest costs that are deductible.

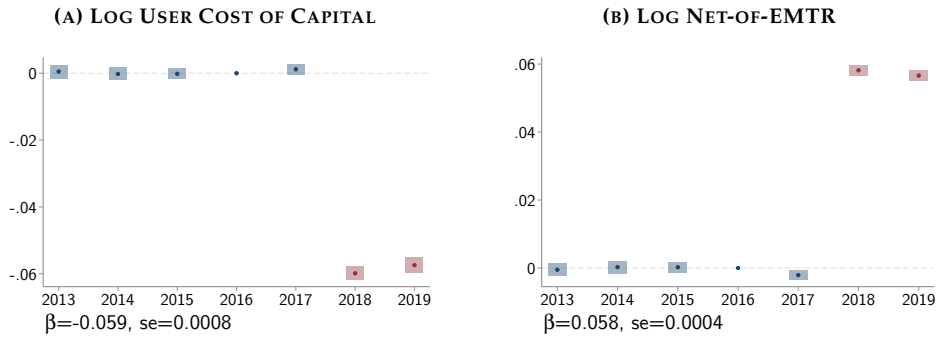
We leverage the user cost measures to recover corresponding effective marginal tax rates (EMTRs). The EMTR is the wedge between the after-tax user cost ϕ and the after-tax return r, where r is the weighted average return on debt and equities, scaled by the user cost:

$$\tau^{EMTR}_{ft} = \frac{\phi_{ft} - r_{ft}}{\phi_{ft}} \tag{8}$$

Figure 9 plots the coefficients from equation 2 where the outcomes are the log user cost and log $(1 - \tau^{EMTR}_{ft})$. User costs declined by -5.9% (s.e.=0.1) for C corps relative to S corps after TCJA, and the net-of-tax EMTR increased by 5.8% (s.e.=0.0). In Table 6, we use the EMTR and user cost measures to estimate elasticities with respect to observed marginal tax rates (the benchmark in Panel A), effective marginal tax rates (Panel B), and holistic user costs (Panel C). The elasticities in Panels B and C can be interpreted as taking into account the full package of tax treatments due to TCJA. The results in Panels A and B are broadly similar, implying that in our setting the benchmark elasticities approximate those accounting for comprehensive features of the reform.[10]

[10]Note the empirically observed marginal tax rate in Panel A differs from the statutory parameterization of tax rates in Panels B and C. See Chodorow-Reich (2025) for a theoretical and empirical survey harmonizing user cost elasticities in the literature. See Appendix C.11 for additional results on the role of expensing; a calibration suggests around 70% of the reduction in the user cost tax term for C corps is attributable to the change in the tax rate.

FIGURE 9: USER COST OF CAPITAL AND EFFECTIVE MARGINAL TAX RATES



Notes: The unit of analysis is a firm-year. The panels plot the β_t coefficients estimated from equation 1. These coefficients capture average differences in outcomes between C and S corps over time after controlling for firm and industry-size-year fixed effects and reweighting the sample to match the joint industry-size distribution of firms in the pre-period. Standard errors are clustered by firm and error bands show 95% confidence intervals. The corresponding DiD coefficient and standard error are shown in the bottom left of each panel. See Section 4.5 and Appendix B for measurement details.

TABLE 6: ELASTICITIES TO TAX RATES AND USER COSTS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A: Elasticities With Respect to the Log Net-of-Tax Rate (Benchmark)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	I_t/K_{t-1}	ln(Payroll)	ln(p50 w)	ln(p95 w)	ln(Top 5 w)	ln(TI)	ln(After-Tax π)
$\ln(1-\tau_{f,t}^{MTR})$	0.449***	0.369***	-0.027	0.224***	0.466***	0.697***	1.064***
	(0.070)	(0.086)	(0.039)	(0.057)	(0.079)	(0.158)	(0.156)
	Panel B: Elasticities With Respect to the Log Net-of-EMTR						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	I_t/K_{t-1}	ln(Payroll)	ln(p50 w)	ln(p95 w)	ln(Top 5 w)	ln(TI)	ln(After-Tax π)
$\ln(1-\tau_{f,t}^{EMTR})$	0.439***	0.353***	-0.026	0.215***	0.445***	1.052***	1.614***
	(0.068)	(0.082)	(0.038)	(0.053)	(0.073)	(0.234)	(0.232)
	Panel C: Elasticities With Respect to Log User Cost of Capital						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	I_t/K_{t-1}	ln(Payroll)	ln(p50 w)	ln(p95 w)	ln(Top 5 w)	ln(TI)	ln(After-Tax π)
$\ln(\Phi_{f,t})$	-0.424***	-0.346***	0.026	-0.211***	-0.437***	-1.065***	-1.634***
	(0.066)	(0.080)	(0.037)	(0.052)	(0.072)	(0.238)	(0.236)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Size-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	204,411	231,360	231,360	231,360	231,360	167,773	166,197

Notes: The table reports elasticities for key outcomes estimated from equation 4 using alternate measures of the endogenous tax shock variable. Panel A reports the benchmark elasticities with respect to the net-of-tax rate. Panel B reports elasticities with respect to the effective marginal tax rate. Panel C reports elasticities with respect to the user cost of capital. All specifications include firm and industry-size fixed effects, and are reweighted to match the joint industry-size distribution of firms in the pre-period. Non-positive observations are excluded from the regressions when the outcome is logged. The first stage F statistics (not shown) are all greater than 1000. See Section 4.5 for measurement details.

4.6 General Equilibrium Effects in the Labor Market

The firm-level empirical analyses leverage the fact that C and S corps on average received differently sized tax cuts to identify causal effects. However, in the firm-level

difference-in-differences design, it is possible that general equilibrium effects, operating through market-level adjustments impacting both C and S corps, are absorbed by the industry-size-year fixed effects. In this case, firm-level micro elasticities may diverge from market-level macro elasticities. For example, in the case of workers' earnings, if corporate tax cuts cause firms to increase labor demand, and if workers are perfectly substitutable across firms and labor markets are perfectly competitive, then the earnings of both C and S corp workers would increase by an equal amount. In this case, the market-level earnings elasticity would be positive even if the firm-level earnings elasticity is zero.

To empirically investigate these general equilibrium responses, we implement a market-level analysis. We construct measures of labor market outcomes using two market definitions: state-by-industry pairs, and states. Within each market m, we estimate the sales-weighted market share of C corps in the pre-period, C_m, as well as the sales-weighted marginal tax rate in each year, $\tau_{m,t}$. We then instrument the log net-of-tax rate with the C corp exposure share to estimate effects on log wages, $\ln(w_{m,t})$. Formally, we estimate the market-level elasticity ε^m using variations of the following specification:

$$\ln w_{m,t} = \varepsilon^m \ln(1 - \tau_{m,t}^{MTR}) + \alpha_m + \gamma_t + X_{m,t}^{'}\lambda + \epsilon_{m,t} \tag{9}$$

instrumented with:

$$\ln(1 - \tau_{m,t}^{MTR}) = \zeta(C_m \times Post_t) + \delta_m + \kappa_t + X_{m,t}^{'}\rho + \nu_{m,t} \tag{10}$$

where m indexes markets and t indexes years. All specifications include market and time fixed effects, and some specifications include additional demographic controls $X_{m,t}$. The identifying strategy underlying this research design, exploiting variation in policies or policy exposure across markets, is well-established in the empirical public finance literature (e.g., Auerbach and Gorodnichenko 2013; Nakamura and Steinsson 2014; Suárez Serrato and Zidar 2016; Giroud and Rauh 2019; Zidar 2019). We report placebo tests to document parallel pre-trends in Appendix C.12.

Where markets are defined as state-industry pairs, the specifications include both state-year and industry-year fixed effects. These fixed effects control for potentially confounding supply and demand shocks across different states and industries. However, defining markets as states allows for the possibility that workers may reallocate across industries. Thus both market definitions have advantages and limitations. The results from both analyses are reported in Table 7.

Columns 1 and 2 of Table 7 report results at the state-industry level, and columns 3 and 4 report results at the state level. Columns 2 and 4 also include demographic controls for mean worker wage and share of workers who are female. The instruments in both market definitions are statistically powerful, as reflected in the first stage F statistics. Panel A reports results on median earnings. Even with a powerful instrument, the net-of-tax elasticity estimates are precisely centered around zero; averaging the coefficients in columns 1-4 yields an elasticity of -0.0002. Panel B reports results for 95th centile earnings, where similarly averaging yields an elasticity of 0.0475. The confidence intervals in Panels A and B panels rule out elasticities larger than 0.21 and 0.26, respectively.

TABLE 7: MARKET-LEVEL ELASTICITIES

	(1)	(2)	(3)	(4)
	Panel A: Log Median Earnings			
$\ln(1-\tau_{mt})$	-0.067	0.048	0.036	-0.018
	(0.103)	(0.089)	(0.089)	(0.087)
	Panel B: Log 95th Centile Earnings			
$\ln(1-\tau_{mt})$	0.035	0.078	0.073	0.004
	(0.094)	(0.096)	(0.075)	(0.064)
Market	State-Ind	State-Ind	State	State
Market FE	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	No	No
Ind-Year FE	Yes	Yes	No	No
Year FE	No	No	Yes	Yes
Controls	No	Yes	No	Yes
First-Stage F	26	27	24	29
N	6,156	6,156	357	357

Notes: The unit of analysis is a state-industry-year in columns 1 and 2, and a state-year in columns 3 and 4. The outcome is log median earnings in Panel A and the log 95th centile earnings in Panel B. The panels estimate equation 9, instrumenting the sales-weighted log net-of-tax rate with the pre-TCJA sales-weighted share of C corps in each market. Columns 1 and 2 include market fixed effects, state-year fixed effects, and industry-year fixed effects, with standard errors multiway clustered by state and industry. Columns 3 and 4 include state and year fixed effects and are clustered by state. Columns 2 and 4 include time-varying demographic controls for average worker age and share of female workers in each market.

TABLE 8: LABOR REALLOCATION

	(1)	(2)	(3)	(4)	(5)
	Panel A: Raw Data, Transition Rates				
S Corps	Same-Same	S-C	C-S	Noncorp-Corp	NoW2-Corp
Pre-TCJA	0.281	0.000	0.284	0.233	0.185
Post-TCJA	0.291	0.000	0.274	0.243	0.175
C Corps					
Pre-TCJA	0.353	0.225	0.000	0.226	0.173
Post-TCJA	0.291	0.000	0.274	0.243	0.175
	Panel B: Difference-in-Difference Estimates				
	Same-Same	S-C	C-S	Noncorp-Corp	NoW2-Corp
$C_f \times Post_t$	-0.019***	0.009***	-0.006***	0.001	0.002
	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
Firm FE	Yes	Yes	Yes	Yes	Yes
Industry-Size-Year FE	Yes	Yes	Yes	Yes	Yes
R2	0.45	0.68	0.72	0.38	0.48
N Firms	49,235	49,235	49,235	49,235	49,235
N	231,360	231,360	231,360	231,360	231,360

Notes: Panel A reports shares of newly hired workers in C and S corps that were previously employed (or not) in other sectors, before and after enactment of TCJA. Column 1 indicates the share of newly hired workers at C corps that were previously employed at different C corps (or S corp workers previously employed at different S corps). Column 2 indicates the share of newly hired C corp workers that were previously hired at an S corp, and Column 3 reports the converse. Column 4 reports the share of new hires that were previously employed outside the corporate sector (e.g., in the pass-through, non-profit, or public sectors). Column 5 reports the share of new hires who did not have a W-2 in the prior year (e.g., individuals who were self-employed or out of the labor force). The shares sum to one across rows. Panel B reports results from estimating equation 2 using these shares as outcomes. The shares in Panel B column 3 are multiplied by -1 so that a negative coefficient is interpreted as a decline in worker flows from C to S corps.

Beyond wages, we use the W-2 data to assess reallocation in employment. The W-2 data allow us to observe workers' employment histories, and thus to directly observe the movement of labor across firms and sectors after TCJA. To study these patterns, Table 8 Panel A reports the share of newly hired workers in C and S corps that were previously employed in various sectors, before and after TCJA. Column 1 shows the share of newly hired workers at a C corp that were previously employed at a different C corp (or S corp workers that were previously employed a different S corp). Column 2 indicates the share of newly hired C corp workers that were previously hired at an S corp, and Column 3 reports the converse. Column 4 reports the share of new hires that were previously employed outside the corporate sector (for example, in the non-corporate pass-through, non-profit, or public sectors). Column 5 reports the share of new hires who did not have a W-2 in the previous year (e.g., individuals who were self-employed or out of the labor force). The shares sum to one across rows.

Panel B reports results from estimating equation 2 using these shares as outcomes. For ease of interpretation, we multiply the outcome in Panel B column 3 by -1 so that a negative coefficient on $C \times Post$ implies a decline in worker flows from C to S corps. The negative and statistically significant coefficient in column 1 of Panel B implies that, after TCJA, newly hired C corps workers were less likely to come from other C corps. The negative and significant coefficient in column 3 implies that C corps workers were less likely to move to S corps. The positive and significant coefficient in column 2 implies that workers were more likely to move to C corps from S corps or from the non-corporate sector, respectively. The small and statistically insignificant coefficients in columns 4 and 5 imply that newly hired C corp workers were not more or less likely to have previously been employed in the non-corporate sector, self-employed, or out of the labor force before versus after TCJA. This evidence suggests that aggregate employment gains from the tax changes were likely to have been modest, because workers were primarily reallocated from other sectors of the economy into the corporate sector.[11] This evidence also implies that incidence effects on workers were driven primarily by changes in the intensive margin (earnings) rather than the extensive margin (employment), and motivates our modeling approach in Section 5 below.

5 MVPF and Incidence

In this section we use the reduced form elasticities estimated in Section 4 to evaluate the marginal value of public funds (MVPF) and the short-run incidence of the corporate tax cuts. We focus only on the effects of the corporate rate cut, abstracting from issues relating to changes in personal income taxes, deficit financing, public goods provision, consumer prices, and dynamics.

5.1 Marginal Value of Public Funds

A widely used benchmark for evaluating government policies is the MVPF framework of Hendren and Sprung-Keyser (2020). In the case of a corporate tax change, the MVPF $= (1 - \frac{\tau^*}{1-\tau^*}\varepsilon^\pi)^{-1}$, where ε^π is the corporate elasticity of taxable income and τ^* is the tax rate inclusive of payout

[11] This finding is consistent with contemporaneous macroeconomic indicators, which showed that the U.S. unemployment rate in December 2017 was at a 17-year low and thus the labor market was likely near full-employment.

taxes.[12] Averaging the pre- and post-TCJA tax rates of 35% and 21%, applying an effective payout tax rate of 2.5%, and using our empirical estimate of $\varepsilon^{\pi} = 0.70$ while accounting for its statistical uncertainty (s.e. = 0.16), yields an MVPF of 1.47 with a 95% confidence interval ranging from 1.21 to 1.91.[13] Among firms in our sample, this estimate implies that $1 in foregone corporate tax revenue generates approximately $1.47 in aggregate private income. While the empirical results imply an MVPF clearly above 1, this method delivers an upper bound if general equilibrium effects dampen aggregate responses (e.g., as in Chodorow-Reich et al. 2025). MVPF estimates may also differ for very large C corps not included in our estimation sample. For comparison, Hendren and Sprung-Keyser (2020) estimate an MVPF of 1.16 and 1.83 for the U.S. top individual income tax rate reforms in 2013 and 1993, respectively.

5.2 Incidence

To assess the distributional impacts of the corporate tax changes, we adapt the frameworks developed in Suárez Serrato and Zidar (2016) and Fuest et al. (2018) to estimate the share of each productive factor in the total tax burden. Our analysis builds on these studies in several respects. First, the detailed microdata used in this study allow us to directly observe returns to shareholders, and thus to empirically estimate how these returns are affected by changes in the corporate tax rate. Second, we estimate the effects of corporate tax changes on the full distribution of workers' earnings. Third, we extend our analysis to assess incidence not only on factors of production, but also over the income distribution, taking account of the empirical fact that many low-income workers own capital and most capital owners also work.[14] Fourth, using workers' observable locations, we evaluate the geographic incidence of corporate income tax cuts across U.S. commuting zones. Overall, the analysis provides a more complete picture of the incidence of corporate taxes while using weaker assumptions than those required in prior studies.

In evaluating incidence, we impose the standard assumptions of a representative consumer and equal redistribution of tax revenues to all citizens. The former assumption rules out distributional impacts through changes in consumer prices, which are unobservable in our data. The latter assumption, while strong, allows us to avoid making even stronger alternative assumptions about the future path of fiscal policy. TCJA's tax cuts were deficit financed, and the future trajectory of tax policy is always uncertain. Lastly, we also follow the standard assumption in the literature that global capital supply is perfectly elastic (Barro and Furman 2018; Chodorow-Reich et al. 2025), implying that tax-induced increases in after-tax profits do not reflect higher returns to capital

[12] To estimate an individual's willingness-to-pay for a corporate tax cut, the relevant tax rate τ^* is inclusive of both corporate and payout taxes: $\tau^* = 1 - (1 - \tau^c)(1 - \tau^p)$. Cooper et al. 2016 report an average payout tax rate of 8.25%, and Burman et al. 2017 report that 30% of corporate income is subject to payout taxes. Thus we use an effective payout tax rate of $0.0825 \times 0.30 = 2.5\%$.

[13] The asymmetric confidence interval reflects the non-linear transformation from the ETI to the MVPF.

[14] In Fuest et al. (2018), the effects of corporate tax changes on returns to firm owners are unobservable, and changes in rental rates are assumed to be negligible. This assumption is consistent with their small open economy setting, German municipalities. In Suárez Serrato and Zidar (2016), returns to firm owners are unobserved but inferred via structural estimation. These studies empirically evaluate impacts of corporate tax changes on median and mean wages, respectively, but do not assess impacts over the earnings distribution.

broadly but rather accrue via increases in firms' scale, dynamic adjustment cost frictions, and/or via firm-specific economic rents, such as those arising from proprietary technology, fixed factors, market power, regulatory advantages, or information frictions.[15]

Guided by the empirical results, we classify workers into three groups: low-paid workers, high-paid workers, and executives. Low-paid workers are defined as those in the bottom 90% of the national wage distribution, and high-paid workers as those in top 10%. Workers and executives optimize consumption $C^L = w^j(1-\tau^{p(j)})L^j$, where w^j is the wage for workers of type j , $\tau^{p(j)}$ is the personal income tax rate, and L^j is labor supply.[16] The indirect utility function for workers is given by $U^j(w^j) = w^j L^j$, and the change in utility from a change in wages is given by:

$$dU^j(w^j) = L^j dw^j(1-\tau^{p(j)}) = w^j L^j(1-\tau^{p(j)})\varepsilon^{L_j}\frac{d(1-\tau^c)}{(1-\tau^c)} \tag{11}$$

where ε^{L_j} is the elasticity of earnings for workers of type j with respect to the net-of-tax corporate rate and $w^j L^j$ is their initial labor earnings (or equivalently for the incidence computations below, their intial labor share in pre-tax corporate value added, $\frac{w^j L^j}{\sum_j w^j L^j + \pi}$). By the envelope theorem, the change in firm owners' profits is given by $dy^K = -d\tau B - dwL(1-\tau^c)$, where B corresponds to the corporate tax base (i.e., taxable income) and the second term captures offsetting effects of the tax cuts on profits due to higher labor costs. Empirically, these gains to firm owners are embedded in the elasticity of after-tax profits to the net-of-tax rate, which we have estimated directly in the analysis. The change in welfare for firm owners is then given by:

$$dy^K = y^K \varepsilon^{\pi(1-\tau^c)}\frac{d(1-\tau^c)}{(1-\tau^c)} \tag{12}$$

where $\varepsilon^{\pi(1-\tau^c)}$ is the elasticity of after-tax profits with respect to the net-of-tax corporate rate, and y^K is initial profits (or equivalently, the share of corporate profits in pre-tax corporate value added, $\frac{\pi}{\sum_j w^j L^j + \pi}$). The incidence share of worker type j is given by:

$$I^{L_j} = \frac{dU^j}{\sum_j dU^j + dy^K} \tag{13}$$

Similarly, the share of firm owners in the total tax burden, I^K, is given by replacing the numerator in equation 13 with dy^K. We further expand the analysis of factor incidence along two dimensions. First, we evaluate incidence with respect to the population distribution of income. Doing so allows us to account for the fact that many workers are also capital owners (Auerbach 2006) and that many capital owners also earn labor income (Smith et al. 2019). We assume that everyone works. We ascribe capital ownership of C corps to workers and foreigners using data from the Federal Reserve Board's Distributional Financial Accounts (2018) and Atkeson et al. (2025), respectively. We ascribe capital ownership of S corps to workers using data from Cooper et al. (2016). We define executives as the top five highest paid workers at the firm, high-paid workers as those in the 90-99th percentiles of the national distribution, and low-paid workers as

[15] Appendix D.1 shows that relaxing this assumption implies greater gains for higher-income households.

[16] We abstract from disutility of labor because empirically we do not find that the tax cuts affect aggregate labor supply (see Table 8) and because we do not observe hours of work in the data.

the bottom 90%. Letting ω^j represent the capital share of workers of type j, incidence shares across the income distribution for all workers, inclusive of both labor and capital income, are given by:

$$I^{L_j} = \frac{dU^j + \omega^j dy^K}{dy^K + \sum_j dU^j} \tag{14}$$

Second, we combine the distributional estimates from equation 14 with individuals' observed locations to estimate the geographic incidence of corporate income tax cuts across regions and commuting zones. Letting $\nu^{j(r)}$ represent the share of workers of type j living in region r, and N^r represent the region's population, we compute:

$$\frac{dY^r}{N^r} = \frac{\sum_j \nu^{j(r)}(dU^j + \omega^j dy^K)}{N^r} \tag{15}$$

which estimates the effect of the tax change on per capita income in region r. To the extent that firm ownership and high-paid and low-paid workers are unequally spatially distributed, the benefits of corporate tax cuts will also be unequally geographically distributed.

TABLE 9: MOMENTS AND PARAMETERS

	(1) C Corps	(2) S Corps	All Corps
Panel A: Corporate Income MTRs			
Mean 2016 τ^c	0.22	0.27	0.23
Mean $\Delta\tau^c$	-0.09	-0.05	-0.08
Mean $\Delta\tau^c/\tau^c_{t-1}$	-0.41	-0.19	-0.35
Mean $\Delta \ln(1-\tau^c)$	0.13	0.07	0.11
Panel B: Share in Value Added			
Firm Owners	0.16	0.33	0.22
Executives	0.08	0.14	0.10
Top 10% Workers	0.44	0.25	0.37
Bottom 90% Workers	0.32	0.28	0.31
Panel C: Distribution of Firm Ownership			
Top 1%	0.41	0.67	0.45
91-99%	0.38	0.20	0.35
Bottom 90%	0.21	0.13	0.20
Foreign Share	0.34	0.00	0.29
Panel D: Net-of-Tax Elasticities			
Pre-Tax Profit	0.70	0.70	0.70
After-Tax Profit	1.06	1.06	1.06
Executive Pay	0.47	0.47	0.47
Top 10% Earnings	0.26	0.26	0.26
Bottom 90% Earnings	0.00	0.00	0.00
Panel E: Personal Income MTRs			
Executives	0.396	0.396	0.396
Top 10% Workers	0.280	0.280	0.280
Bottom 90% Workers	0.150	0.150	0.150

Notes: The table shows the 2016 sample moments and elasticity parameters used to quantify the incidence of the tax cuts. Panel A reports the empirically observed sales-weighted average marginal tax rates and changes (from 2016 to 2019) for C corps (column 1) and S corps (column 2) and all firms (column 3), among SOI firms with between $1 mil and $10 bil in sales and at least 10 employees. Panel B reports the share of each factor in net value added, where value added is defined as the sum of workers' earnings and pre-tax profits. Panel C reports the distribution of firm ownership. Ownership shares for C and S corps in 2016 with respect to income are from the Federal Reserve Board (2018) and Cooper et al. (2016), respectively. The foreign ownership share for C corps in 2016 is from Atkeson et al. (2025). The parameters in Panel D are estimated in the empirical analysis. The top 10% earnings elasticity is the average of the 90th, 95th, and 99th centile elasticities, and the bottom 90% earnings elasticity is set to zero. Panel E reports the calibrated personal income tax rates on labor income for each worker types. The parameters in Panels D and E do not vary by firm type.

5.3 Moments and Parameters

Table 9 summarizes the key inputs that we use to quantify the incidence of TCJA's business tax cuts. Panel A reports the empirically observed average marginal tax rates and changes faced by C corps (column 1) and S corps (column 2) in the sample. Panel B reports the share of each factor in net value added. Panel C reports the distribution of C and S corp ownership over the income distribution and ownership by foreigners using data from the Federal Reserve (2018), Cooper et al. (2016), and Atkeson et al. (2025), respecitvely. Panel D reviews the key net-of-tax elasticities estimated in the empirical analysis. Panel E reports the personal income tax rate on labor income for the three worker types based on the marginal income tax bracket schedule prior to TCJA. The parameters in Panels D and E do not vary by firm type.

5.4 Incidence Results

Panel A of Table 10 reports the incidence estimates for firm owners, executives, and high- and low-paid workers for C corporations, S corporations, and all corporations combined. Our benchmark estimates in column 1 isolate the effects of corporate tax cuts on C corps. Column 1 reports that 60% of gains from the corporate tax cuts flow to firm owners; 8% flow to executives, 32% flow to high-paid workers, and 0% of the gains flow to low-paid workers. Modest differences in factor intensities genereate modest differences in factor incidence between C and S corps, but these differences are not statistically significant.

TABLE 10: INCIDENCE ESTIMATES

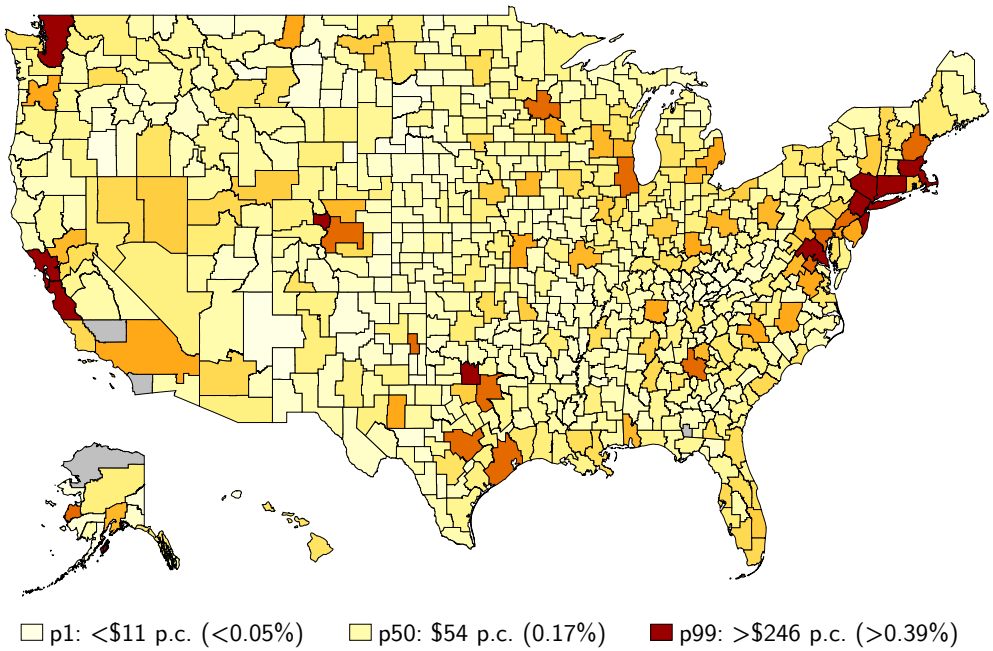
	(1) **C Corps**	(2) **S Corps**	(3) **All Corps**	(4) **U.S. Pop.**
	% (se)	% (se)	% (se)	%
Panel A: By Factor				
Capital	60 (7)	80 (4)	69 (6)	n.a.
Executives	8 (1)	9 (2)	9 (1)	<1
High-Paid Workers	32 (5)	11 (2)	22 (4)	10
Low-Paid Workers	0 (4)	0 (2)	0 (3)	90
Panel B: By Income				
Top 1%	33 (3)	63 (2)	40 (2)	1
91-99%	55 (3)	27 (2)	46 (2)	9
Bottom 90%	13 (4)	10 (4)	14 (3)	90
Panel C: By Geography				
Foreign	20 (2.4)	0 (0.0)	9 (0.8)	n.a.
Domestic	80 (2.4)	100 (0.0)	91 (0.8)	100
Of which:				
Northeast	29 (0.4)	35 (0.3)	33 (0.3)	19
Midwest	23 (0.3)	22 (0)	22 (0.2)	21
South	20 (0.4)	19 (0.1)	19 (0.2)	36
West	28 (0.3)	23 (0.1)	26 (0.1)	24

Notes: Panel A reports the share of the incidence of the corporate tax cuts on firm owners, executives, and high- and low-paid workers, based on equations 11, 12, and 13, by firm type. Panel B reports distributional incidence, based on equation 14. Panel C reports geographic incidence, based on equation 15, and leveraging the observed residential locations of individuals in the tax data. States are classified into regions using the definitions from the U.S. Census, provided here, with the modification of classifying Delaware, DC, and Maryland as belonging to the Northeast rather than to the South. Totals may not sum to 100 due to rounding. Columns 1-3 show results for C, S, and all corporations, respectively. Column 4 reports corresponding U.S. population shares. Standard errors are constructed from 500 bootstrap simulations of the jointly estimated parameters.

Panel B reports the estimates of incidence over the income distribution. Accounting for both wage earnings and shareholder income, column 1 reports that approximately 33% of the gains from corporate tax cuts accrue to the top 1% of the earnings distribution, 55% accrue to the 90-99th percentiles, and 13% accrue to the bottom 90%. These results highlight the importance of considering the joint impacts of changes in both capital and labor income when assessing the distributional effects of corporate tax changes. Moreover, the substantial differences in distributional incidence of tax cuts affecting C vs. S corps (column 1 vs. column 2) reflect that, for firms in our sample, ownership of S corporations is more highly concentrated at the top of the income distribution relative to C corps. Appendix D.3 shows that a modestly larger share of incidence falls on capital and foreigners when extrapolating to a sample including large C corps.

Panel C of Table 10 reports geographic incidence for foreigners and U.S. residents, and incidence across Census regions, estimated from equation 15. A significant share of C corporate equity, approximately 38% in the 2016 baseline year, is held by foreigners. As a result, we estimate that 20% of the overall gains from the corporate tax cuts flow to foreigners. By contrast, S corporations generally cannot be owned by foreigners. Within the United States, corporate tax cuts disproportionately benefit residents of the Northeastern and Western regions, reflecting that corporate shareholders and highly-paid workers are heavily concentrated in those areas. The table shows that the share of income gains for residents of the Northeast (29%) is about 1.5 as large as for residents of the South (20%).

FIGURE 10: GEOGRAPHIC INCIDENCE



Notes: The unit of analysis is a commuting zone. The map illustrates geographic variation in changes in per capita income due to the corporate tax cuts, estimated from equation 15. Income gains are proportional to color intensity, with darker colors representing larger gains.

Figure 10 maps variation in incidence across commuting zones (CZs). Beyond the regional patterns discussed in Table 10, the map highlights substantial within-region variation, with larger and higher-income CZs seeing larger gains. This pattern is underscored in Appendix Figure 21, which plots the change in income against the 2016 average earnings of corporate-sector employees. Relative to the median CZ income gain of approximately 0.19%, gains are approximately 2-3 times larger in New York City and San Francisco. We do not find strong evidence of heterogeneous earnings effect across industries with varying capital intensities (see Appendix C.9), implying the geographic patterns are driven by residential sorting patterns by income. The results imply that corporate income tax cuts not only increase income dispersion across workers, but also contribute to widening dispersion across urban and rural areas (Gaubert et al. 2021).

6 Conclusion

This paper studies the short-run impacts of the largest corporate income tax cut in U.S. history on American firms and workers. Tax cuts cause firms to increase their capital investment, labor demand, sales, and profits. Labor earnings increase for workers in the top 10% of the within-firm earnings distribution, and rise sharply for executives, but do not change for workers in the bottom 90%. We interpret the evidence as consistent with both price effect and income effect mechanisms.

We empirically estimate key elasticities of firm- and worker-level outcomes with respect to changes in corporate tax rates, and use these estimates to assess the MVPF and short-run incidence of TCJA's corporate tax cuts. The implied marginal value of public funds (MVPF) among firms in our sample is 1.47. We estimate that 87% of private income gains flow to the top 10% of the earnings distribution, and 13% flow to the bottom 90%.

One implication from this study is that policymakers appear to face a tradeoff when setting business tax policy: corporate tax cuts cause businesses to expand and thus increase economic growth, but also generate an increase in income dispersion across households and across regions. The net social costs and benefits of the corporate tax as an instrument of public finance will depend on the relative merits of alternative tax instruments, and on the welfare weights that voters and elected officials place on households with varying incomes and geographic backgrounds. We view such a holistic assessment of the broader tax system as beyond the scope of this paper but as a valuable subject for further study.

We conclude with important caveats. The largest C corps have no analogues in the pass-through sector, and thus alternative research designs should be used to understand their responses to the tax cuts. These results also do not capture potential long-run channels through which corporate tax cuts may affect welfare. For example, higher investment may increase productivity, lower consumer prices, and broadly increase workers' real wages over time. Alternately, corporate tax cuts may accelerate capital-labor substitution due to technological change and put downward pressure on wages. Credible evidence at much longer time horizons is necessary to arbitrate between these views. Lastly, lower tax revenues may lead to reductions in government activity that either augment or diminish private production and social welfare.

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Supplemental Appendix

"Corporate Tax Cuts, Firm Growth and Workers' Earnings"

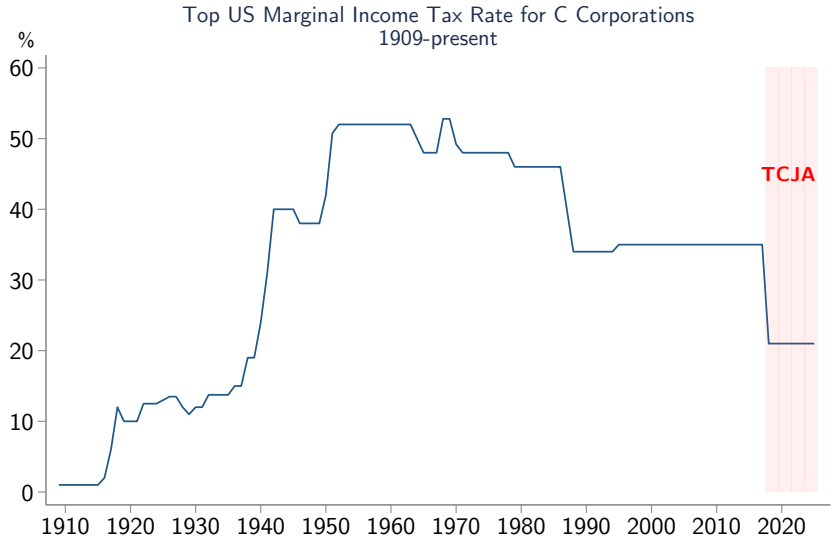
Kennedy, Dobridge, Landefeld, and Mortenson

A Appendix to Section 2: Setting and Institutional Details

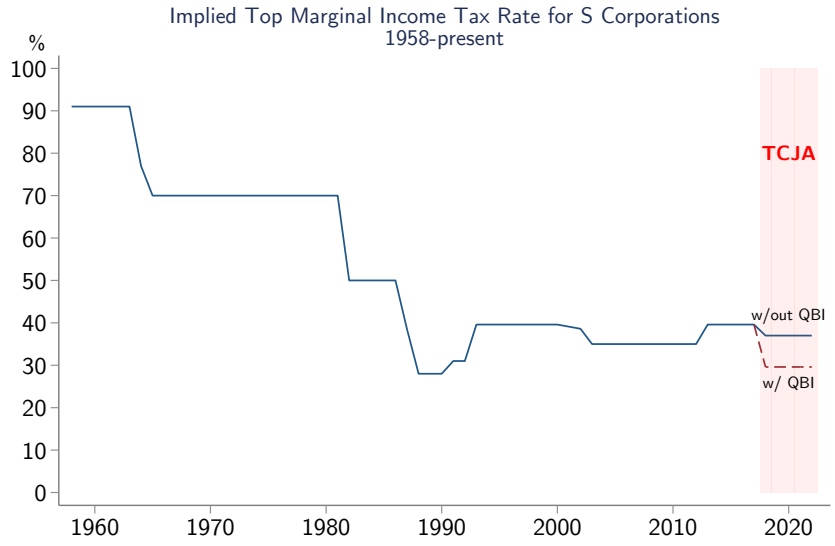
A.1 Historical Statutory Federal Top Marginal Income Tax Rates

Appendix Figure 1: Top Marginal Income Tax Rates in Historical Context

Panel A: Top Marginal Tax Rate for C corporations



Panel B: Implied Top Marginal Tax Rate for S corporations



Notes: Data are from Tax Foundation 2020a, 2020b. Panel A shows the evolution of the top statutory marginal corporate income tax rate facing C corporations since 1958. Panel B shows the implied top statutory marginal income tax rate facing S corporations, which is equal to the top federal rate facing individuals.

A.2 Marginal Income Tax Rates and Brackets Before and After TCJA

Appendix Table 1: Marginal Income Tax Brackets Before and After TCJA

Panel A: Tax Brackets for C Corporations

Income Bracket	Upper Income Threshold ($)	Pre-TCJA MTR	Post-TCJA MTR	Firm Share	Emp Share	Sales Share
0	0	0	0	0.916	0.659	0.477
1	50,000	0.15	0.21	0.061	0.026	0.012
2	75,000	0.25	0.21	0.006	0.006	0.004
3	100,000	0.34	0.21	0.003	0.004	0.002
4	335,000	0.39	0.21	0.007	0.013	0.010
5	10,000,000	0.34	0.21	0.005	0.043	0.047
6	15,000,000	0.35	0.21	0.000	0.008	0.009
7	18,000,000	0.38	0.21	0.000	0.003	0.005
8	>18,000,000	0.35	0.21	0.001	0.240	0.433

Panel B: Implied Tax Brackets for S Corporations (Married Joint Filers)

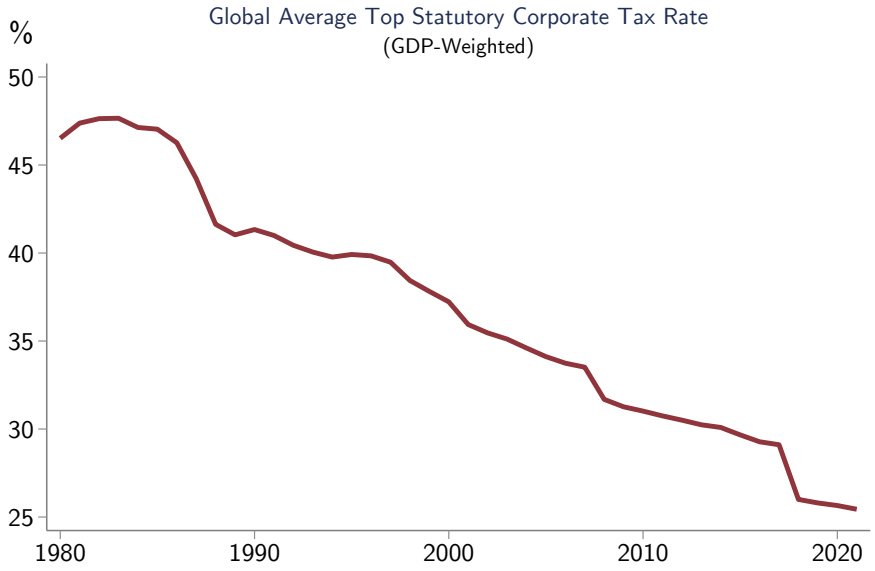
Income Bracket	Upper Income Threshold 2017 ($)	2017 MTR	Upper Income Threshold 2019 ($)	2019 MTR
1	18,650	.1	19,400	.1
2	75,900	.15	78,950	.12
3	153,100	.25	168,400	.22
4	233,350	.28	321,450	.24
5	416,700	.33	408,200	.32
6	470,700	.35	612,350	.35
7	>470,700	.396	>612,350	.37

Notes: Panel A reports the statutory marginal income tax brackets facing C corps before and after TCJA. The firm, employment, and sales shares are calculated in tax year 2016 using SOI data. Panel B illustrates an example of the implied statutory marginal income tax brackets facing S corp owners. This schedule varies depending on the taxpayer's filing status. For illustrative purposes, the schedule shown here is for married joint filers.

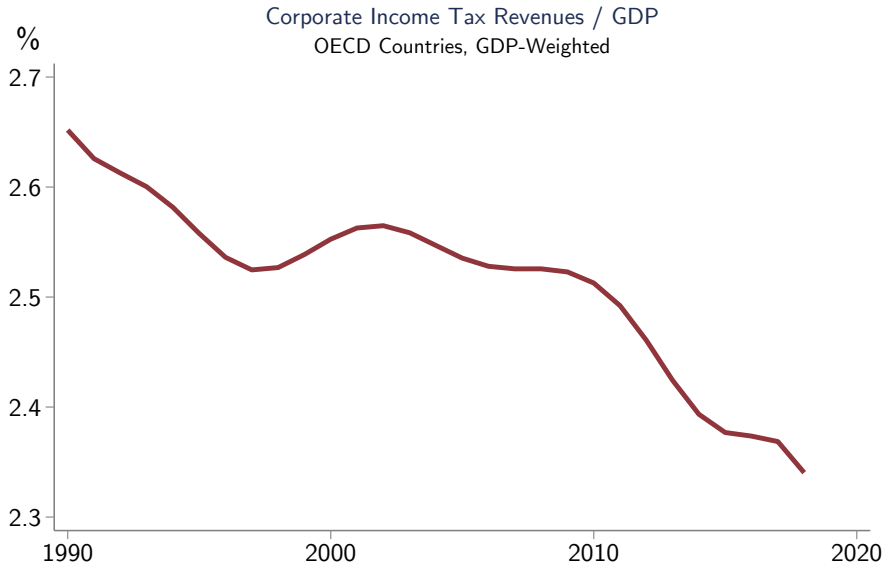
A.3 Global Perspective on Corporate Income Taxes

Appendix Figure 2: Corporate Taxes in Global Perspective

Panel A: Average Global Corporate Tax Rates



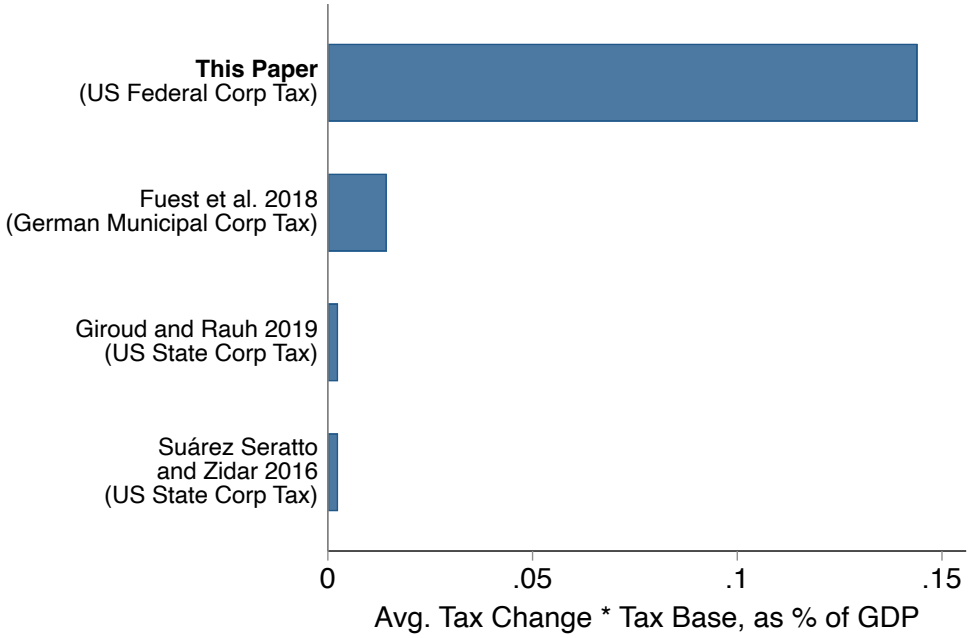
Panel B: Average Corporate Income Taxes in OECD Countries as a Share of GDP



Notes: Panel A shows the GDP-weighted global average top statutory corporate income tax rate since 1980, using data from Tax Foundation (2021) and World Bank (2025). Panel B shows the lowess-smoothed GDP-weighted ratio of corporate income tax revenues to GDP since 1990 for OECD countries, using data from OECD (2022). Data are not available for this series before 1990.

A.4 TCJA Tax Cut in the Context of Recent Literature

APPENDIX FIGURE 3: TCJA TAX CHANGE VS. OTHER RECENT STUDIES



Notes: The figure shows the average tax change studied in several recent papers, multiplied by the tax base and scaled by GDP. The average tax change in Fuest et al. (2018) is 0.9 percentage points, and the LBT tax base is approximately 1.6% of GDP (see OECD/UCLG 2019). The average tax change in Suárez Serrato and Zidar (2016) and Giroud and Rauh (2019) is 1.0 percentage point, and the state corporate tax base is approximately 0.25% of GDP (Census 2019). In this study, the 2016 federal corporate tax base is approximately 1.6% of GDP (OMB 2022), and the unconditional average tax change is 9 percentage points.

A.5 The QBI Deduction

Here we broadly summarize the Qualified Business Income (QBI) deduction enacted in TCJA. For comprehensive details, see documentation from the IRS here.

Commonly referred to as "Section 199A" after the corresponding section of the tax code, the QBI deduction is potentially available to owners of S corporations, as well as other pass-through businesses not included in our analysis sample, and is not relevant for C corporations. The deduction amount is equal to the minimum of:

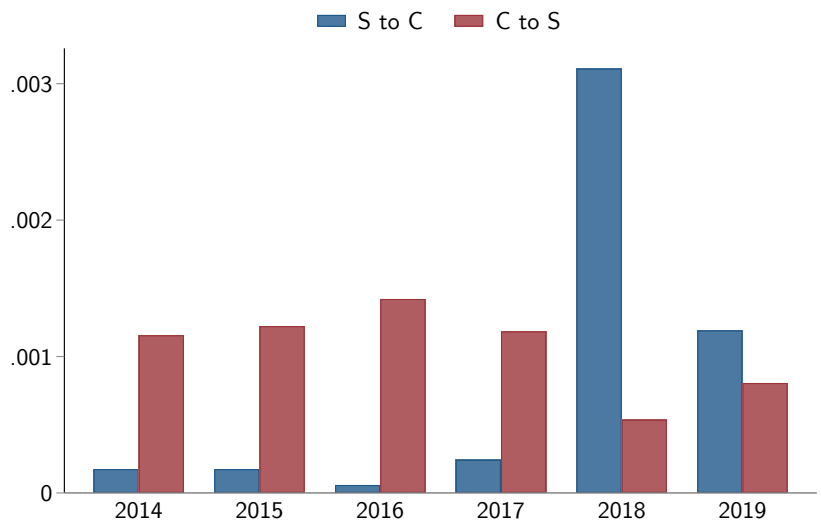
1. 20% of taxable income, where taxable income is calculated prior to assessing the QBI deduction, minus net capital gains; or,
2. The sum of both:
   (a) 20% of the aggregate qualified real estate and investment trust (REIT) dividends and publicly traded partnership (PTP) income; and
   (b) 20% of net income from qualified trades or businesses.

Moreover, the deduction is limited for taxpayers with taxable incomes exceeding certain thresholds who receive qualified business income from businesses with certain characteristics. For tax year 2019, taxpayers with taxable income below $321,400 (married filing jointly) or $160,725 (single) are eligible for the full deduction without regard to firm-level wage or capital limitations. For taxpayers with taxable income within the phase-in range above these thresholds ($100,000 for married filers and $50,000 for single filers), the deduction is partially subject to additional limitations. For taxpayers with taxable income above the phase-in range, the deduction is subject to two constraints. First, income from specified service trades or businesses (SSTBs) is ineligible for the deduction. Second, the deduction is limited to 20 percent of QBI, capped at the greater of (i) 50 percent of W-2 wages paid by the business or (ii) 25 percent of W-2 wages plus 2.5 percent of the unadjusted basis immediately after acquisition of qualified depreciable property.

The deduction amount is claimed by individual owners on Schedule K-1. The phase-out and eligibility restrictions imply that, within S corporations, some owners may be eligible for QBI and others may not. The QBI deduction effectively lowers the marginal business income tax rate for firm owners who can claim the deduction, and in turn lowers firms' cost of capital (i.e., it lowers τ^c_{ft} for S corps in equation 7). Properly accounting for the QBI affects the first stage of the research design (i.e., it affects the tax rate for S corps) but does not affect the reduced form.

A.6 Entity-Type Switching

APPENDIX FIGURE 4: CORPORATE ENTITY-TYPE SWITCHING, 2013-2019



Notes: Figure shows the profit-weighted share of firms that switch their legal entity type from C-to-S or from S-to-C over the sample period.

A.7 Other Policy Changes Enacted in TCJA

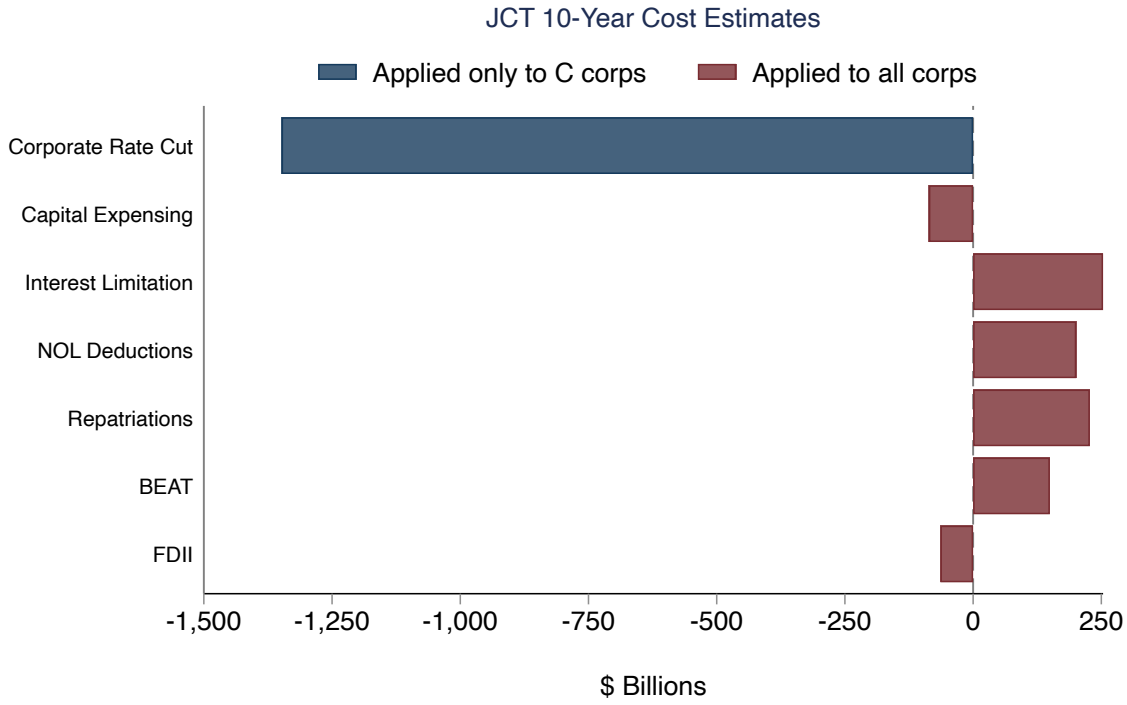
Below we briefly summarize other major provisions of TCJA affecting corporations. For more details on these reforms, see Auerbach (2018) and Joint Committee on Taxation (2018).

- **Net Operating Loss (NOL) Deductions:** TCJA limited NOL deductions to 80% of a corporation's taxable income, eliminated NOL carrybacks, and allowed indefinite NOL carryforwards.
- **Domestic Production Activities Deduction (DPAD):** TCJA repealed the DPAD, which provided a tax deduction to corporations for property manufactured, produced, extracted or grown by the taxpayer within the United States.
- **Alternative Minimum Tax (AMT):** TCJA repealed the corporate AMT, which imposed a minimum tax of 20% on corporations' relevant taxable income in excess of a $40,000 exemption threshold, excluding the firm's AMT foreign tax credit.
- **Interest Deductions**: TCJA limited the net interest payment deductions to 30% of adjusted taxable income.
- **Bonus Depreciation:** TCJA temporarily allowed corporations to immediately deduct 100% of the cost of newly purchased eligible capital investments (known as "full expensing"), an increase from 50% prior to TCJA, but scheduled to phase out beginning in 2023.
- **Taxation of Foreign Income**: TCJA introduced several changes to taxation of corporations' income earned abroad. The most significant changes include: (a) a one-time tax on previously accumulated foreign income and an elimination of tax on repatriated income; (b) a minimum tax on foreign income above a threshold return on tangible assets (known as Global Intangible Low-Taxed Income, or GILTI); (c) a minimum tax on deductible related-party transactions to U.S. subsidiaries, known as the Base Erosion and Anti-Abuse Tax, or BEAT; and (d) a lower tax rate on income earned from foreign sales, known as Foreign Derived Intangible Income, or FDII.

Figure 5 below plots the estimated budget impacts of each major provision over 10 years produced by Joint Committee on Taxation (2017). Among domestic firms, the corporate rate cut is the only major provision of the business tax code that applied exclusively to C corporations, and was the most expensive of these provisions. See Appendix C.11 for an empirical and theoretical

discussion of the relative importance of expensing and the tax rates in driving changes in user costs and marginal incentives.

APPENDIX FIGURE 5: ESTIMATED REVENUE IMPACTS OF MAJOR TCJA BUSINESS TAX PROVISIONS BY JOINT COMMITTEE ON TAXATION (2017)



Notes: The figure plots the estimated budget impacts of major TCJA business tax provisions over 10 years produced by Joint Committee on Taxation (2017). Among domestic firms, the corporate rate cut is the only provision of the business tax code that applied exclusively to C corporations, and was the most expensive provision of these reforms.

B Appendix to Section 3: Data Sources, Variable Definitions, and Measurement

B.1 Variable Definitions

Below we describe how we measure key outcomes in the study. For additional details on construction of the SOI samples, see documentation provided by the IRS here.

Taxes Paid

Taxes paid are defined for C corporations as Form 1120: Schedule L, line 31. For S corps, taxes paid are measured using the following methodology:

1. Link S corp returns to K-1s; retrieve S corp owners' 1040s
2. Use 1040s to compute each owner's average tax rate (ATR) and total taxes paid on pass-through income
   (a) Calculate ATR for a tax unit: ATR = Taxes Paid / Taxable Income
   (b) Record net ordinary business income: NET_OBI = Line 32 from 1040 (from Schedule E)
   (c) Compute taxes paid on business income: BIZ_TAX_PAID = min(max{ATR*(NET_OBI),0},total tax paid on 1040)
   (d) Save table unique by TIN-year
3. Compute total non-negative pass-through income from 1120s and 1065s by owner
   (a) Append all K1s from 1120s and 1065s with positive OBI (drop K1s with OBI<$0)
   (b) Sum up OBI by TIN-year; call the sum OBI_SUM
   (c) Save table unique by TIN-year
4. Merge table 2 and 3 by TIN-year
5. Compute the OBI share of each pass-through business in the owner's portfolio
   (a) Append all K1s from 1120s with positive OBI
   (b) Match m:1 by TIN with table 4; new table is unique by TIN-K1-year
   (c) Compute share of each K1 in total OBI, call it W = OBI / OBI_SUM
   (d) Allocate tax_paid in proportion to the shares: S_TAX = W*BIZ_TAX_PAID
   (e) Sum up S_TAX by firm-year, final table is unique by EIN-year

Sales, Costs, and Profits

Sales are defined for C and S corporations as Form 1120: line 1c and Form 1120-S: line 1c, respectively. Costs of goods sold are defined for C and S corporations as Form 1120: line 2 and Form 1120-S: line 2, respectively. Pre-tax operating profits are defined as sales minus costs of goods sold. After-tax operating profits are defined as pre-tax operating profits minus taxes paid. Harmonized Net Income is defined for C corporations as Form 1120: line 28 minus Form 1120: line 29b plus Form 1120: line 25, and is defined for S corporations as Form 1120-S Schedule K: line 18.

Shareholder Payouts

Dividends are defined for C corporations as Form 1120: Schedule M-2, line 5a plus line 5c. For S corporations, dividends are defined as Form 1120-S: Schedule K, line 17c. Share buybacks are defined as the non-negative annual dollar change in the treasury stock; treasury stock is defined for C corporations as Form 1120: Schedule L, line 27(d) and for S corporations as Form 1120-S: Schedule L, line 26(d). Total payouts are defined as dividends plus share buybacks.

Investment

Capital assets are defined for C and S corporations as buildings and other depreciable assets less accumulated depreciation, as measured on Form 1120: Schedule L, line 10b(d) and Form 1120-S: Schedule L, line 10b(d), respectively. Net investment is defined as the annual dollar change in capital assets. When constructing measures of investment scaled by lagged capital, in a small share of observations we use lagged capital from the IRS CDW database if it is missing from the SOI files, or use two-period lagged capital if the one-period lag is missing or zero. When computing exposure to accelerated bonus depreciation, eligible investment is defined as the sum of Form 4562 lines 19d(c) to 19i(c) plus the sum of lines 20a(c) to 20c(c) plus the sum of line 14, line 15, and line 16. We winsorize the top and bottom 5% of investment outcomes within entity-type-year bins.

Payroll, Employment, and Earnings

Worker earnings are measured for C and S corporation employees from Form W-2, box 5 (Medicare Wages). Payroll is the sum of W-2 earnings by firm and year. Employment is measured by matching firms with individuals' W-2s using the crosswalks described in JCT (2022). To increase precision and reduce measurement error due to part-time workers or tax reporting errors, we exclude workers without a valid taxpayer identification number, workers younger than 18 or older than 70, and workers in the bottom 5% of the national annual earnings distribution when computing employment counts and firm earnings centiles. Top-5 compensation is defined for C and S corporations as the average annual W-2 earnings of the top five highest paid workers at the firm in each year. Reported compensation captures several but not all components of pay, including: wage, salary, and bonus income; stock options and grants, when exercised; and non-qualified deferred compensation. However, this measure is a lower

bound on executive compensation, as it does not capture stock options or grants before they are exercised, and does not include incentive stock option plans.

Other Firm Characteristics

Firm age is defined as tax year minus year of incorporation, where year of incorporation for C corporations and S corporations is defined as Form 1120: box C and Form 1120-S: box E, respectively. Multinational firms are defined as those whose foreign sales share in the pre-period is greater than 5%, where foreign sales are defined as the sum of gross receipts from all Controlled Foreign Corporations (that is, foreign subsidiaries) reported for each foreign subsidiary on Form 5471 Schedule C: line 1c. Capital intensity is defined at the industry level as total capital assets divided by total sales. C and S corporations are classified as capital intensive if the mean of this ratio in the pre-period (2013 to 2016) is greater than the sample median. Industry is defined for C corporations as the first three digits of Form 1120: Schedule K, line 2a and for S corporations as the first three digits of Form 1120-S: Schedule B, line 2a. NAICS industry codes are defined using the code reported by the firm in the first year it appears in the sample.

Fuzzed Centile Statistics

When reporting centile statistics, we compute averages of all observations within a quantile range rather than report the singular centile observation in the data. For example, rather than report the true median, we report the average of all observations in the 50th quantile of the distribution. We take these measures to ensure and protect taxpayers' privacy in compliance with IRS policy.

Additional Data and Measurement Notes

In the robustness checks in Table 7, we reclassify the NAICS-6 or state of fewer than 1% of observations to prevent them from falling out the sample when using finer fixed effects than in the benchmark specification. We do so by binning observations that would otherwise be excluded from the regression into a single new industry or state bin. Excluding these observations from the sample does not change the results. When controling for pre-trends, we estimate missing values at the firm level with the average pre-trend in the same industry-size bin, or failing that, the same NAICS-3 or NAICS-2 industry. In Table 8, the NOL, 163(j), and bonus exposure variables are winsorized at the 1st/99th percentiles to improve precision and reduce the influence of outliers. In Table 7, the SOI sample is collapsed by state (or state-industry) and year; firms in Puerto Rico, Virgin Islands, and Guam are excluded due to thin coverage and to protect taxpayer privacy. More broadly, we impose minimum cell sizes of at least 20 firms and 50 employees to protect taxpayer privacy. While it is legally possible for an S corp to own a C corp, this occurs in only 0.17% of observations in our sample and dropping them does not influence the results. C corps cannot own S corps.

Sample Restrictions

The Corporate SOI files from 2013-2019 begin with approximately 642,000 firm-year observations. Dropping firms with less than $1 million or greater than $10 billion in sales reduces the sample to 397,000. Dropping firms with fewer than 10 employees reduces the sample to 312,000. Dropping firms with non-positive capital and expenses reduces the sample to 299,000. Dropping multinational firms reduces the sample to 265,000. Dropping public firms reduces the sample to 254,000. Dropping firms that switch entity types reduces the sample to 247,000. Dropping singletons from the fixed effect regressions yields the final analysis sample of 231,360 firm-year observations.

B.2 Measuring User Costs

Here we provide additional details underlying the measurement of user costs in equation 7. The Hall and Jorgenson (1967) cost of capital expression is given by the following equation, here omitting subscripts for clarity:

$$\Phi = \left(r^d \Big(1 - \tau\Big) \cdot \alpha + r^e \cdot (1 - \alpha) - \rho + \delta \right) \cdot \left(\frac{\sum_k s_k (1 - \lambda z_k \tau^c)}{1 - \tau^c} \right)$$

where r^d and r^e are the nominal returns on debt and equity, respectively; α and $(1 - \alpha)$ are the share of debt vs. equity financing, respectively; s_k is the capital share with depreciation schedule k; z is the present value of deductions from bonus depreciation allowances of class k; λ is the effective share of capital investments eligible for bonus; τ^c is the marginal income tax rate (adjusted for debt and operating loss deductions, and for the QBI in the case of S corps); ρ is the inflation rate; and δ is the depreciation rate. We estimate or calibrate each of these parameters as follows, leveraging our data sample where possible and otherwise relying on estimates from the literature. The subscript f denotes firms, i denotes industries, is denotes industry-size bins, j denotes entity type (C or S), k denotes asset classes, and t denotes years. Parameters without subscripts are applied uniformly to all firms in all years.

- r^d_{ft}, the interest rate on debt, is estimated in the data at the firm-year level net interest payments divided by debt.
- r^e, the cost of equity, is calibrated to 0.089, using U.S. estimates from Damodaran (2025).
- α_{isj}, the share of debt financing, is estimated in the data at the industry-size-entity-type level as the ratio of debt to the sum of debt and equity in the pre-period. Since the firms in our sample are private, we estimate the market value of equity as in Smith et al. (2023). Those authors use Compustat data on public firms to estimate the ratios of market value to sales, market value to assets, and market value to EBITDA, all at the industry level. They

then apply those ratios to the observed firm-level values in the tax data, and estimate market value as the average of the three measures.

- z_{isjkt}, the expensing rate for eligible capital, is estimated by applying the methodology of Zwick and Mahon (2017) at the industry-size-entity-asset-type level separately in the pre-period and post-period. It is a weighted average of the present discounted value of bonus deductions, where the weights are given by firm-specific bonus-eligible asset mixes. Structures are never eligible for bonus. After TCJA, the introduction of full expensing means that $z_{isjk,t}$ is uniformly set to 1 for all bonus-eligible capital.

- λ is a scaling factor accounting for incomplete take-up of bonus. We follow Congressional Budget Office (2024) and Kitchen and Knittel (2016) in making these adjustments. Kitchen and Knittel (2016) use IRS data to document that the dollar-weighted share of claimed expensing deductions ranges from 50-70% for C and S corporations. They also document that take-up rates are correlated with firm size. Since our sample covers mid-market firms, we use a benchmark take-up rate of 60%.

- ρ, the annual inflation rate, is set to 1.5%, the average over our sample period as computed from BLS data (BLS 2024).

- δ_{isj}, the depreciation rate, is estimated in the data at the industry-size-entity-type level as depreciation scaled by lagged capital.

- $\tau^c_{f,t}$, the marginal income tax rate, is computed as follows. For C corps, the top rate is 35% before TCJA and 21% after TCJA. The top tax rate in the pre-period for C corps that claimed the DPAD deduction before it was elimiated by TCJA was 31.85%. For S corps, the top rate is 39.6% before TCJA. We empirically observe a 3.2 percentage point decline in MTRs from 2016 to 2019 for firms with positive taxable income, after accounting for DPAD deductions in the pre-period and QBI deductions in the post-period, yielding a calibrated post-TCJA rate of 36.4%. Lastly, we adjust this tax rate, which we denote as $\tau^{'}_{f,t}$, for net operating loss (NOL) deductions as follows. First, we estimate the share of NOL deductions in taxable income by industry-size bin among C corps in the pre-period, denoted as $\omega_{is,j}$. Second, to account for the favorable tax treatment of losses for S corps relative to C corps — S corp owners may immediately deduct business losses against positive income from other sources — we scale $\omega_{is,j}$ by 1.63 for S corps, equal to the aggregate S-to-C ratio of NOLs over positive business income in the pre-period.[17] Third, we compute the applicable marginal income tax rate before TCJA as $\tau^c_{f,t} = \tau^{'}_{f,t} \cdot (1 - \omega_{isj(f)})$. Fourth, we use the same formula to compute $\tau^c_{f,t}$ in the post-period, except where $\omega_{isj(f)} = \min(\omega_{isj(f)}, 0.8)$ to account for TCJA's limitation on NOL deductions, capped at 80% of taxable income.

[17] The average ratio of NOL deductions to the sum of taxable income and NOL deductions in the pre-period in the sample is 8.8% for C corps but not empirically observable for S corps on Form 1120-S, as these deductions are claimed by the individual owners on Form 1040. We estimate this ratio as 14.3% for S corps using publicly available SOI data (IRS 2025). The ratio 0.143/0.088 = 1.63. Varying the scaling factor from 1 to 2 does not significantly affect the estimates.

- $\tau^d_{f,t}$, the marginal tax benefit from interest payments on debt, is equal to the firm's marginal income tax rate in years prior to TCJA. After TCJA, deduction amounts were capped at 30% of EBITDA. Denoting debt interest costs as c and EBITDA as Y, the effective marginal tax rate on debt is given by: $\tau^d_{f,t} = \tau^c_{f,t} \cdot \min(c_f, 0.3 \cdot \max(0, Y_{f,t}))/c_{f,t}$, where firms below the interest limitation threshold deduct interest costs at the marginal income tax rate, and firms above the threshold deduct costs at a lower effective rate equal to the product of the marginal tax rate and the share of interest costs that are deductible.
- When estimating parameters in the data, we winsorize ratios at the 5th/95th percentiles (e.g., interest/debt, NOL deductions/taxable income, etc). If values of parameters are missing at the {firm, industry-size, NAICS-3 industry, NAICS-2 sector} level, then we use the average value of the parameter in the next most aggregate bin {industry-size, NAICS-3 industry, NAICS-2 sector}, in that order.

APPENDIX TABLE 2: USER COST PARAMETERS

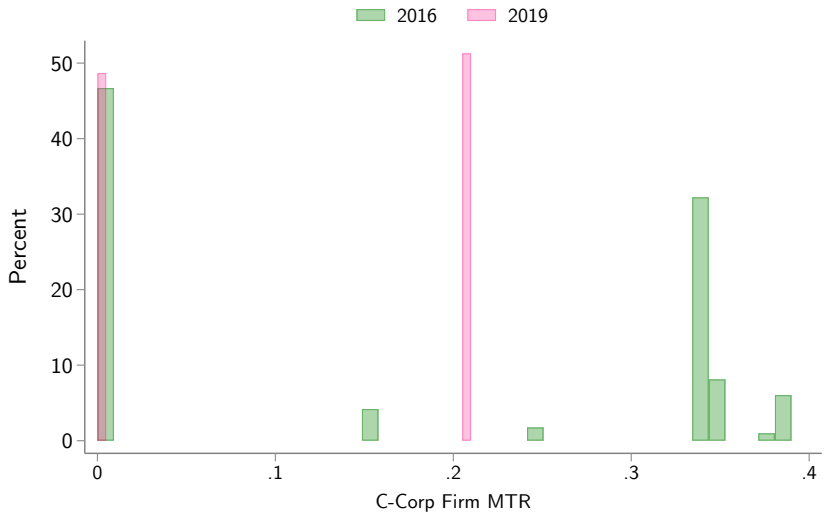
	All Firms		**C Corps**		**S Corps**	
	(1) Pre	(2) Post	(3) Pre	(4) Post	(5) Pre	(6) Post
$r^d_{f,t}$	0.031	0.031	0.030	0.028	0.033	0.035
r^e	0.089	0.089	0.089	0.089	0.089	0.089
$\alpha_{f,t}$	0.295	0.295	0.326	0.326	0.258	0.258
$z_{isj,t}$	0.733	1.000	0.740	1.000	0.726	1.000
λ_{isj}	0.600	0.600	0.600	0.600	0.600	0.600
ρ	0.015	0.015	0.015	0.015	0.015	0.015
δ_{isj}	0.048	0.048	0.050	0.050	0.046	0.046
$\tau^c_{f,t}$	0.244	0.184	0.206	0.123	0.289	0.254
$\tau^d_{f,t}$	0.244	0.234	0.206	0.182	0.289	0.294
$\phi_{f,t}$	0.132	0.119	0.130	0.114	0.134	0.125

Notes: The table reports the estimated and calibrated parameters used to measure user costs for firms in the balanced analysis sample. The table reports unconditional, unweighted means for all firms, C corps, and S corps in years pre- and post-TCJA. See above for additional measurement details.

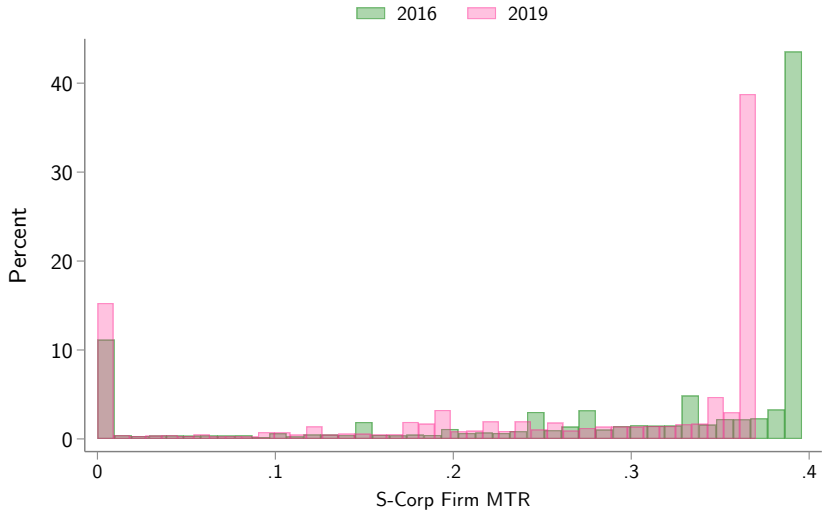
B.3 Sample Distribution of Firms' Statutory Marginal Income Tax Rates Before and After TCJA

Appendix Figure 6: Sample Distribution of C corp Statutory MTR's

Panel A: Marginal Income Tax Rates for C corporations



Panel B: Implied Marginal Income Tax Rates for S corporations



Notes: Panels A and B show the sample distribution of statutory marginal income tax rates for C corps and S corps, respectively, before and after TCJA.

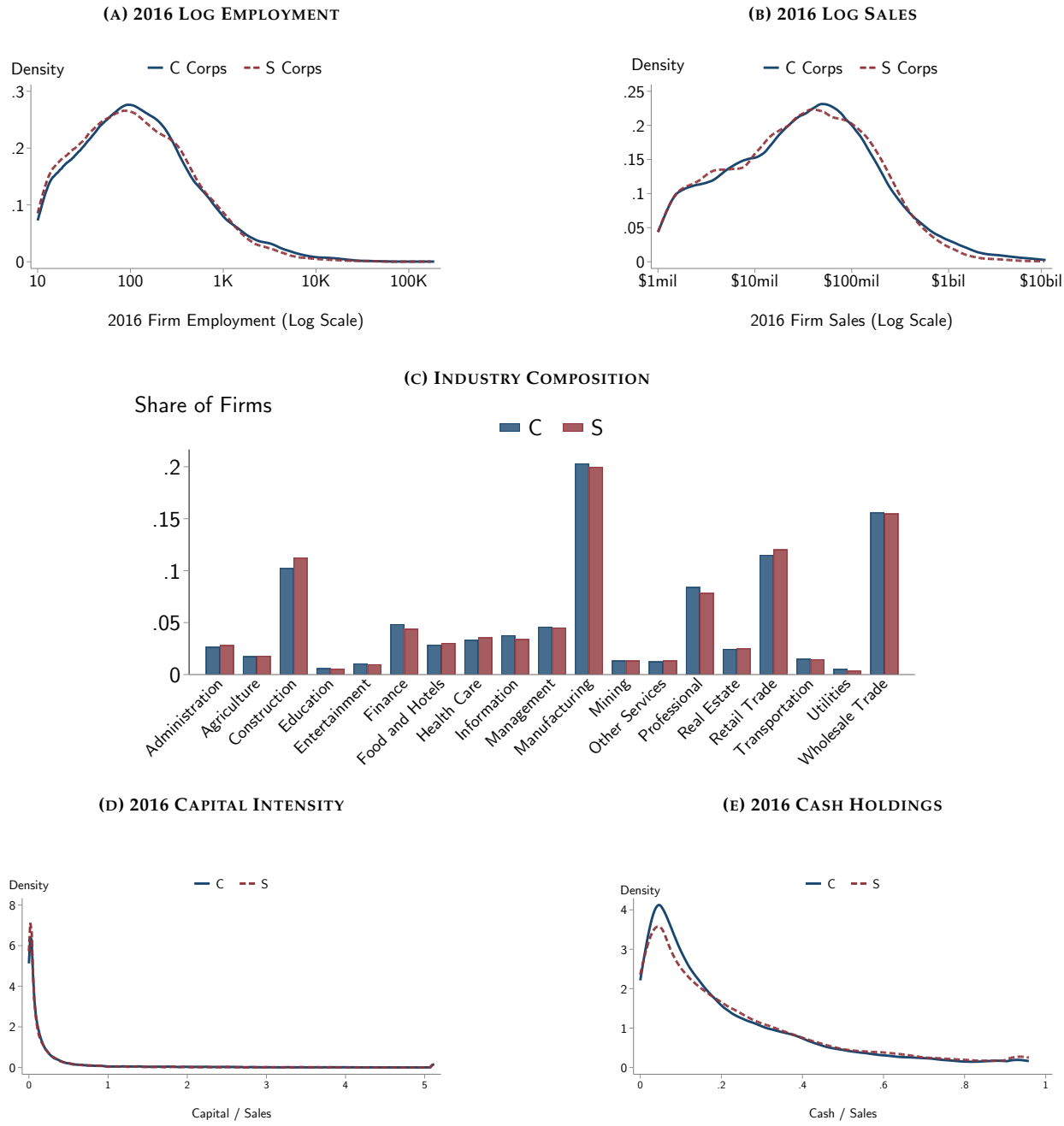
B.4 Additional Summary Statistics

APPENDIX TABLE 3: WEIGHTED SUMMARY STATISTICS

	All Firms		C Corporations				S Corporations			
	(1) Mean	(2) SD	(3) Mean	(4) SD	(5) p50	(6) p90	(7) Mean	(8) SD	(9) p50	(10) p90
Taxes										
Marginal Tax Rate	0.236	0.163	0.184	0.170	0.150	0.350	0.287	0.138	0.364	0.396
Federal Tax (mil)	0.909	1.584	0.821	1.701	0.023	2.877	0.995	1.455	0.267	3.961
Federal Tax Per Worker	7,125	13,992	4,580	8,592	285	16,042	9,603	17,381	2,598	27,240
Sales and Profits										
Sales (mil)	129.8	445.7	154.1	550.8	30.6	297.1	106.3	309.3	36.6	238.1
Costs (mil)	132.8	475.1	162.2	602.0	34.6	307.2	104.2	301.7	34.8	231.7
Operating Profit (mil)	44.3	197.2	55.2	245.4	10.5	107.6	33.7	133.7	10.6	63.4
After-Tax Op. Profit (mil)	43.4	196.8	54.3	244.9	10.0	105.3	32.7	133.4	9.8	61.3
Taxable Income (mil)	3.1	7.9	1.9	9.3	0.3	11.4	4.3	6.0	1.7	14.5
After-Tax Net Income (mil)	2.2	6.7	1.0	8.0	0.2	8.3	3.3	5.0	1.2	11.5
Shareholder Payouts										
Dividends (mil)	3.2	18.9	1.1	16.0	0.0	0.6	5.4	21.2	1.0	11.0
Share Buybacks (mil)	0.1	4.7	0.1	6.0	0.0	0.0	0.1	2.9	0.0	0.0
Total Payouts (mil)	3.5	20.4	1.3	17.8	0.0	1.0	5.8	22.5	1.0	11.9
Capital and Investment										
Capital (mil)	24.0	278.9	33.8	385.9	3.0	51.8	14.5	91.3	1.8	26.1
Net Investment (mil)	1.3	71.1	2.0	100.1	-0.0	3.6	0.5	11.5	-0.0	2.0
Net Investment / Lagged Capital	0.07	0.34	0.08	0.36	-0.02	0.54	0.06	0.31	-0.02	0.48
Payroll and Employment										
Payroll (1120; mil)	23.9	113.4	28.5	134.2	6.5	56.3	19.4	88.4	5.9	37.3
Payroll (W2; mil)	22.2	99.3	26.3	119.4	6.3	52.3	18.1	74.6	5.7	34.8
Employment	461	2,790	517	3,067	106	884	407	2,489	106	701
Worker Earnings and Characteristics										
Mean Annual Earnings (thous)	62.8	61.5	65.0	58.4	55.1	111.1	60.6	64.2	50.0	91.9
Median Annual Earnings (thous)	45.9	29.2	48.6	32.2	42.9	85.0	43.2	25.7	38.8	67.2
95th Centile Earnings (thous)	170.7	285.7	173.7	266.1	131.6	296.8	167.8	303.5	118.6	260.2
Mean Top 5 Earnings (thous)	374	961	378	919	218	708	371	1,001	197	669
Female Share	0.35	0.23	0.35	0.22	0.32	0.71	0.36	0.23	0.28	0.72
Worker Age	42.2	5.2	42.4	5.1	42.9	48.6	41.9	5.2	41.9	47.7
Firm Characteristics										
Firm Age	28.7	21.1	27.6	21.8	22.3	53.9	29.7	20.3	26.8	56.5
Manufacturing	0.20		0.20				0.20			
Capital Intensive	0.58		0.58				0.57			
N Firms	32,649		17,915				14,734			

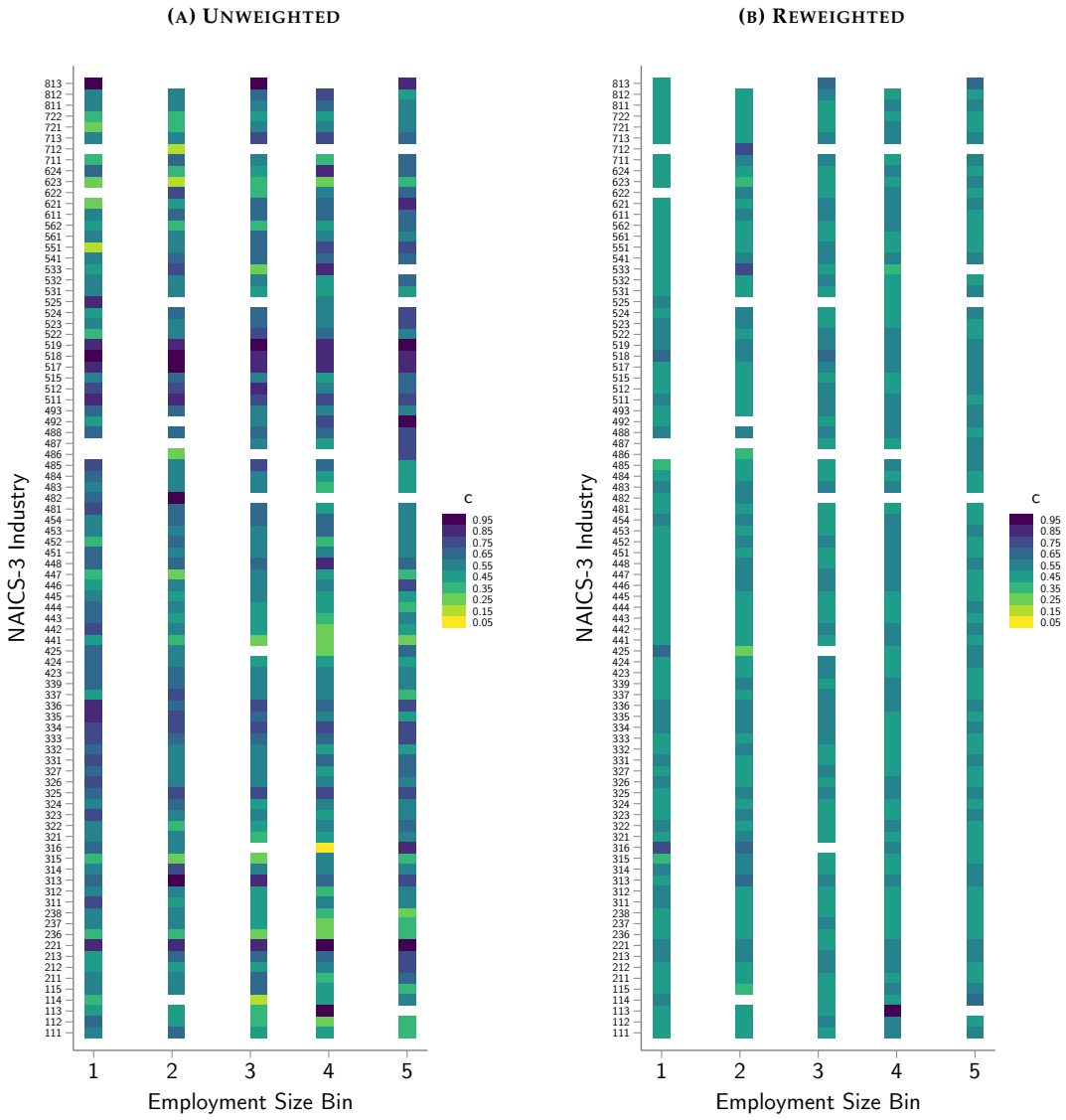
Notes: Table shows summary statistics from 2016 for firms in the analysis sample after reweighting by industry and size. Medians and centile statistics are fuzzed to protect taxpayer privacy. The reweighting procedure ensures that the joint probability distribution of industry-size cells is identical for C and S corps in the estimation sample but does not precisely equalize continuous firm characteristics such as employment or sales within each cell. For data sources and variable definitions see Section 3.

Appendix Figure 7: Weighted Firm Size Distributions and Industry Composition



Notes: The figure shows the distributions of firm characteristics for C and S corps after reweighting the sample to match the industry-size composition of the sample in the pre-period. Panels A and B show the distributions of 2016 log firm employment and log sales, respectively. Panel C shows the NAICS-2 industry composition. Panels D and E show the 2016 distribution of capital intensity (capital / sales) and cash holdings (cash / sales). In Panels D and E the top 1% of outcomes are winsorized to improve the data visualization. The unweighted industry and size distributions are shown in Figure 2.

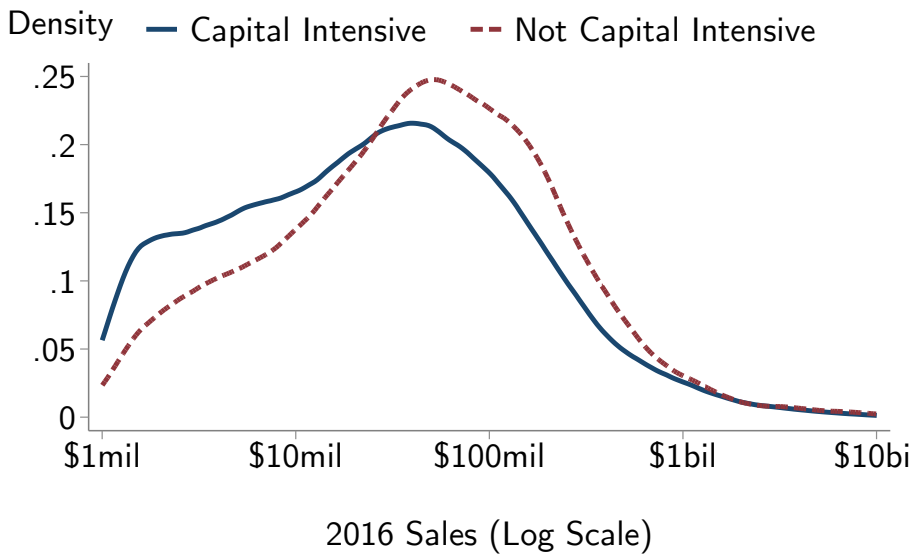
APPENDIX FIGURE 8: JOINT INDUSTRY-SIZE DISTRIBUTION



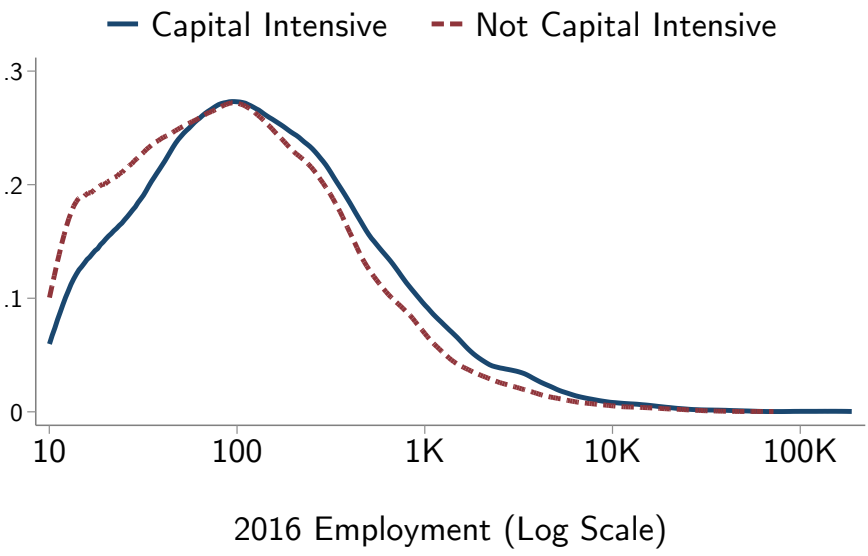
Notes: Panel A shows the unweighted shares of C corporations within each industry-size bin in the pre-period. Panel B shows the shares after reweighting by industry and size using the procedure described in Section 3. White cells indicate bins with no observations. Lighter cells indicate lower shares of C corporations and darker shares indicate higher shares.

Appendix Figure 9: Size Distributions of Firms, by Capital Intensity

Panel A: 2016 Sales

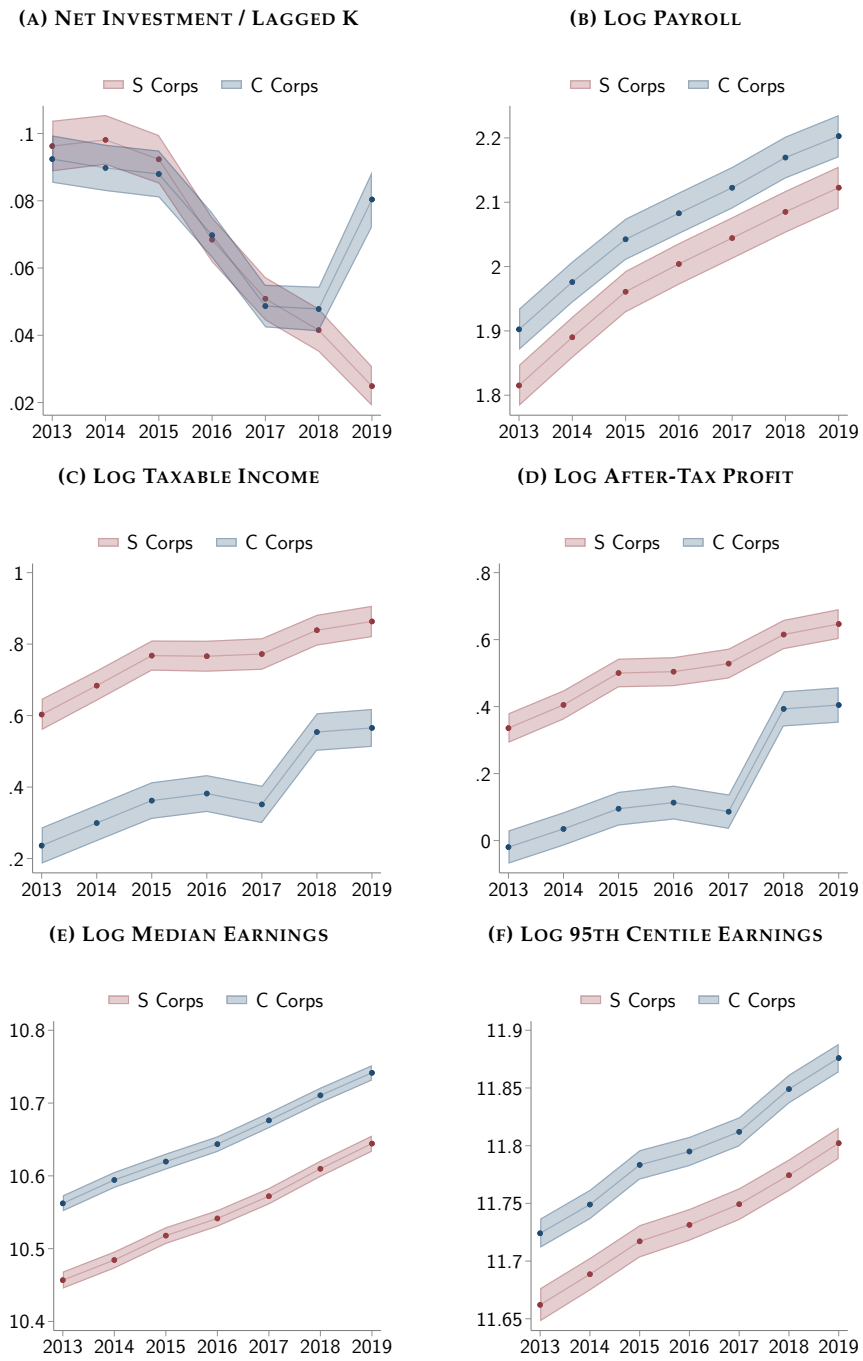


Panel B: 2016 Employment



Notes: Panels A and B show the unweighted distributions of 2016 log firm sales and employment, respectively, for capital-intensive and non-capital-intensive firms in the sample.

Appendix Figure 10: Raw Outcome Trends



Notes: The panels report raw outcome means for C and S corps in the analysis sample from 2013-2019, using the balanced panel to remove firm composition effects. Error bands show 95% confidence intervals.

C Appendix to Section 4: Empirical Results

C.1 Evidence Supporting the Common Elasticity Assumption

The research design comparing C and S corporations relies on two key identifying assumptions. The first is the standard assumption of parallel trends in the absence of the tax reform, and is supported by the broadly flat pre-trends documented in the event studies in Section 4. Since C and S corps are both treated with tax changes in our setting, but with different treatment doses, the research design also requires a second assumption: namely, that C and S corps respond to tax shocks with common elasticities.

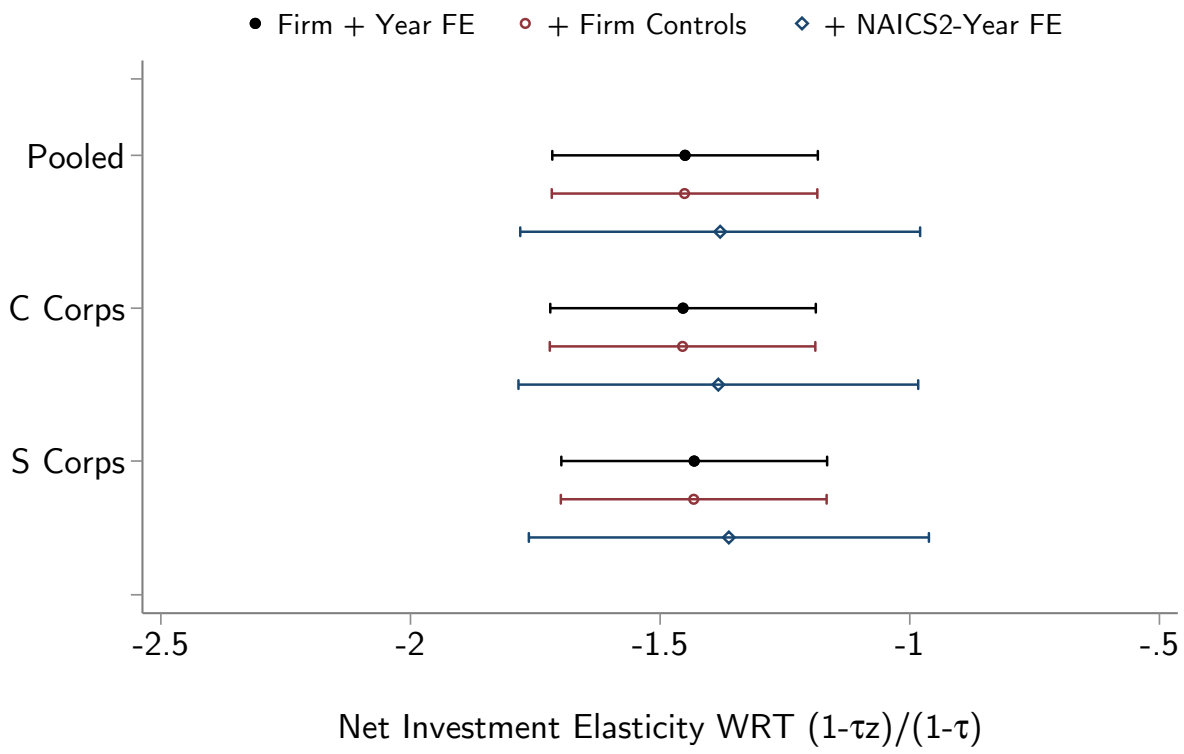
To study whether C and S corps have similar elasticities with respect to tax shocks, we require a source of quasi-experimental variation in effective tax rates *within* the C corp sector and, separately, *within* the S corp sector. The setting in Zwick and Mahon (2017) (henceforth, ZM) is ideal, as their research design exploits cross-industry differences in exposure to changes in accelerated depreciation policies (i.e., bonus depreciation) over time. The original ZM data sample includes both C and S corps, which the authors use to estimate the elasticity of investment with respect to changes in the tax term $\frac{1-\tau z}{1-\tau}$, where τ is the tax rate and z is the present value of a dollar of investment eligible for bonus depreciation. While the original analysis does not report results separately for C vs. S corps, we can apply their cross-industry research design to sub-samples of C and S corps to assess whether the elasticity varies by firm type.

Using the IRS SOI data, we first replicated ZM's pooled results for the most recent period in their sample (2005-2009), using the same sample restrictions and regression specifications used in their original analysis. ZM estimate investment elasticities using the following specification:

$$\frac{I_{ft}}{K_{f,t-1}} = \alpha_f + \beta \frac{1-\tau_{ft} z_{ft}}{1-\tau_{ft}} + \gamma X_{ft} + \varepsilon_{ft}$$

We precisely replicate the investment elasticity of 1.42 (se = 0.13) reported in the third panel of Table 3, column 4 of ZM. Our replication results are shown in the "Pooled" coefficients in Figure 11 below. Beyond the baseline specification, we further report specifications with and without the controls used in their original analysis (i.e., time-varying controls for sales, firm age, and assets). In the lower panels, we split the ZM sample by entity type to estimate elasticities separately for C and S corps. The elasticities are economically and statistically indistinguishable for C versus S corps. This evidence is consistent with the common elasticities assumption underlying the research design as described in Section 4.

APPENDIX FIGURE 11: REPLICATION AND EXTENSION OF ZWICK AND MAHON (2017) TO TEST FOR COMMON ELASTICITIES OF C AND S CORPS TO TAX SHOCKS

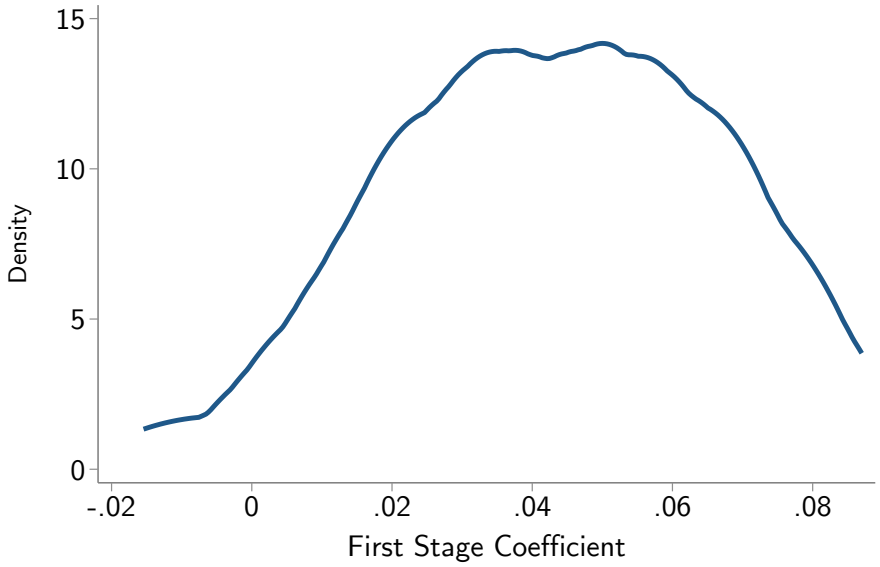


Notes: Unit of analysis is firm-year. The figure plots results from our replication of the investment elasticity from Zwick and Mahon (2017), equation (3), as reported in the third panel of their Table 3, column 4. The outcome is investment scaled by lagged capital and the independent variable is the tax term, $\frac{1-\tau z}{1-\tau}$, where τ is the tax rate and z is the present value of a dollar of investment eligible for bonus depreciation. Specifications are estimated separately for the full pooled sample, for C corporations separately, and for S corporations separately. The specifications designated by the solid black circle, open red circle, and open blue diamond respectively include firm and year fixed effects; add time-varying controls for firms' sales, age, and assets; and add NAICS-2 industry-year fixed effects. Error bars show 95% confidence intervals and standard errors are clustered by firm.

C.2 Evidence Supporting the Monotonicity Assumption

The monotonicity assumption underlying the IV estimation of net-of-tax elasticities requires that no C corp received a smaller tax cut than an S corp due its status as a C corp. Theoretically, this assumption is supported by the institutional fact that the top statutory rate cut was larger by law for C corps than for S corps. Empirically, several additional pieces of evidence support the mononiticty assumption. First, Figure 12 below plots the distribution of β_t coefficients from variations of equation 2, where the outcome is the log net-of-tax change. The coefficients are estimated separately within NAICS-2 industries and within firm size bins, yielding 28 distinct coefficients. The regressions are unweighted and include firm and year fixed effects. 27 of the 28 coefficients are positive, and the single negative coefficient is not statistically distinguishable from zero. Although it is not possible to test for violations of monotonicty at the firm level, this distribution of positive first-stage coefficients across a broad range of subsamples supports the monotonicty assumption. Second, the assumption is further supported by the sub-sample analyses presented in Table 8, which show a uniformly positive first stage in all subsamples. Third, Appendix A.6 showed that switching entity types is extremely rare.

APPENDIX FIGURE 12: DISTRIBUTION OF FIRST-STAGE COEFFICIENTS



Notes: Figure plots the distribution of β_t coefficients from variations of equation 2, where the outcome is the log net-of-tax change. The coefficients are estimated separately within NAICS2 industries and within firm size bins. The regressions are unweighted and include firm and year fixed effects.

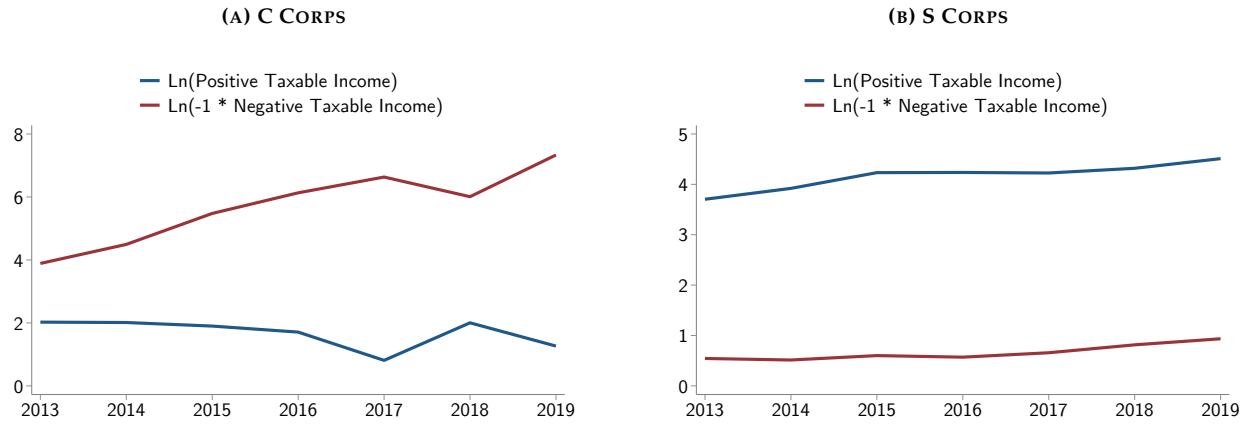
C.3 Intertemporal Profit Shifting

The event studies in Section 4 show that, in response to corporate tax cuts, C corps increased their capital and labor demand in 2018 and 2019, and also increased their sales, costs, and pre-tax profits. However, whereas investment, labor demand, sales, and costs all increase gradually, pre-tax profits increase sharply and immediately in 2018. This appendix provides evidence that the sharp increase in pre-tax profits in 2018 reflects, in part, efforts by C corporations to intertemporally shift income so as to minimize their tax burdens (see also Dowd et al. (2020) who document intertemporal shifting behaviors by C corporations after TCJA in detail).

In particular, firms faced incentives to accelerate expenses in 2017 (when those expenses could be deducted at a higher rate) and to delay income into 2018 (when that income would be taxed at a lower rate). Such incentives are consistent with the modest dip in taxable income and MTR measures observed in 2017 relative to 2016 in our event studies (reflecting accelerated reporting of expenses) and are also consistent with taxable income increasing sharply and suddenly in 2018 (reflecting delayed income reporting), even as real measures like capital and labor demand increased more gradually. The incentives to intertemporally shift income and losses were larger for C corporations, who faced a significantly larger tax change due to TCJA.

These intertemporal shifting behaviors are small but sufficiently systematic to be discernible in the raw data. Figure 13 below plots raw means of log(taxable income) and log(-1 * negative taxable income) in our analysis sample for both C and S corps. Among C corps, the two series diverge inversely in 2017 and 2018 in a mirror-image manner. The results are more muted for S corps, consistent with their weaker incentives to shift income given their smaller tax change. These results help to explain the timing patterns observed in the event studies. To assess sensitivity to this shifting, Table 8 in Section 4.4 of the paper reports elasticity estimates from specifications that exclude 2017 and 2018 from the sample.

APPENDIX FIGURE 13: RAW DATA TRENDS IN TAXABLE INCOME FOR C AND S CORPS



Notes: The unit of analysis is a firm-year. The panels show raw trends of log(taxable income) and log(-1 * taxable income) for C and S corporations, respectively. The figure is strongly suggestive of intertemporal shifting of profits among C corps in 2017 and 2018.

C.4 Extensive Margin Responses

In this section we present additional analyses to study how tax cuts may affect both intensive and extensive margin firm outcomes. To do so, we use methods developed in the literature to study outcomes with zeros (following Chen and Roth 2024) and to study negative capital income (following Kleven and Schultz 2014).

Chen and Roth (2024) propose guidance for studying outcomes with zeros as follows. When the researcher aims to estimate a single, scale-invariant percentage-of-the-mean effect and is willing to treat intensive and extensive responses symmetrically, they recommend Poisson quasi-maximum likelihood (PPML). Estimation via PPML does not exclude observations where the outcome is zero, and PPML coefficients are scale invariant and can be interpreted as percent changes with log regressors. However, when zeros reflect distinct economic decisions that may be of interest to the researcher (for example, in our setting, such as the decision to initiate dividend payments), Chen and Roth advise estimating the conditional change in the amount paid (the intensive margin) in one model and, separately, the probability of paying anything at all (the extensive margin).

Kleven and Schultz (2014) estimate taxable income elasticities for individuals in Denmark following significant tax reforms. For our purposes, their key innovation is to model effects on positive and negative capital income separately, taking into account that the two forms of income may face asymmetric tax rates.

Below we describe how we have applied the logic from these papers in our setting, and present additional analyses to illuminate additional dimensions of intensive and extensive margin responses.

Federal Taxes Paid

Federal corporate tax liability is a function of positive taxable income and zero otherwise. Empirically, taxable income is more likely to be positive than negative, but is quite smooth above and below zero, suggesting that firms have limited capacity to "choose" positive or negative profits. With respect to taxes paid, our aim is to illuminate the overall fiscal response, rather than to document intensive versus extensive margin responses. In this case, the guidance from Chen and Roth (2024) implies that the scale-invariant percentage-of-the-mean parameter is appropriate. Thus, in the event studies in Figure 3, we report PPML regressions where taxes paid and tax per worker are the outcomes.

For completeness, Appendix Table 4 below also reports intensive and extensive margin elasticities separately. Column 1 reports that the intensive margin elasticity is negative and statistically significant, as expected. The extensive margin is also negative and statistically signficiant, although the total effect is small: a 5-10% increase in the net-of-tax rate decreases the probability of paying positive tax by approximately 4-8 percentage points. A larger tax cut increases the chance that fixed tax credits, deductions, or carryforwards are sufficient to fully offset tax liability, especially for firms near the bottom of the taxable income distribution. This makes it more likely

that firms with large rate reductions pay zero tax, even if their pre-tax profits may rise. As a result, firms receiving larger tax cuts may be modestly less likely to report positive tax payments, due to nonlinearities and thresholds in the tax code.

Dividends and Share Buybacks

Dividend payouts and share buybacks, by contrast, reflect discrete choices. Firms first choose whether to pay dividends to shareholders (Lintner 1956; Brav et al. 2005), and then choose the payout amount. Because economic theory assigns different determinants to the two margins, the guidance from Chen and Roth (2024) implies that it is appropriate to model the two outcomes separately.

We report dividend and buyback estimates for both margins below in Table 5. Column 3 indicates a large intensive margin effect on dividend payouts, and columns 4 and 6 imply economically and statistically significant increases in the extensive margin probabilites that firms initiate dividend payouts or share buybacks, respectively. Column 5 reports a positive but statistically insignificant intensive margin effect on share buybacks.

APPENDIX TABLE 4: TAXES, DIVIDENDS, AND SHARE BUYBACKS

	(1) ln(Tax)	(2) **1**(Tax>0)	(3) ln(Dividends)	(4) **1**(Div>0)	(5) ln(Buybacks)	(6) **1**(Buy>0)
$\ln(1-\tau_{f,t})$	-0.456	-0.824	1.468	0.242	1.545	0.402
	(0.177)	(0.068)	(0.198)	(0.062)	(1.344)	(0.063)
2016 Outcome Mean	0.92	0.67	2.91	0.46	0.12	0.13
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Size-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N	144,453	231,360	101,544	231,360	6,250	231,360
N Firms	34,053	49,235	22,921	49,235	1,804	49,235

Notes: The unit of analysis is a firm-year. The table reports results from equation 4. Standard errors are clustered by firm. The 2016 outcome means are reported before taking logs and are scaled in nominal dollars in columns 1, 3, and 5. All specifications include firm and industry-size fixed effects, and the data are reweighted to match the joint industry-size distribution of firms in the pre-period. Specifications where the outcome is binary and the endogenous variable is logged correspond to semi-log linear probability models; in this case, the interpretation of the estimated coefficient is that a 1% increase in the log net-of-tax rate is associated with a $0.01 \times \varepsilon$ increase in the probability that the outcome = 1. Thus, even the statistically significant coefficients reflect effects that are economically small.

Profits

Taxable income and other profit measures may be positive, negative, or zero. Section 4 estimates intensive margin responses (i.e., treatment effects conditional on positive profits). This section investigates extensive margin responses and impacts on negative profits. To assess extensive margin responses, we estimate linear probability models. To estimate effects on negative capital income, we adopt the approach of Kleven and Schultz (2014), who estimate the following elasticities:

$$\varepsilon^{-} = \frac{\partial \ln(-\pi_{ft})}{\partial(1-\tau_{ft})}, \text{ for } \pi_{ft} < 0$$

Following Kleven and Shultz, we use the statutory marginal tax rate as the relevant marginal subsidy. This tax rate provides a transparent, theory-consistent benchmark, and isolates how sensitive reported losses are to the headline tax incentive per dollar of loss. Since Appendix C.3 documents that firms intertemporally shifted income surrounding TCJA's tax changes, the table also reports results both including and excluding 2017/2018 from the sample.

Panel A of Table 5 presents elasticities of taxable income with respect to the net-of-tax rate, separately examining intensive and extensive margin responses. Columns 1 through 3 use the full sample, while columns 4 through 6 exclude the 2017 and 2018 tax years to address potential distortions from intertemporal shifting around the implementation of TCJA. In each panel, column 1 estimates elasticities for firms with positive taxable income; column 2 estimates elasticities for the absolute value of losses among firms with negative income; and column 3 estimates effects on the extensive margin (i.e., the likelihood that a firm reports positive income).

In both sample periods, firms with positive taxable income increase their reported profits in response to lower tax rates, as reflected in positive and statistically significant elasticities in columns 1 and 4. Column 2 suggests an increase in reported losses, but the effect is not statistically significant in column 5 after removing sample years most likely to have been affected by intertemporal shifting. Extensive margin responses are more modest: the probability of reporting positive taxable income appears null in the full sample (column 3), and is negative and statistically significant though economically small when 2017 and 2018 are excluded (column 6). Panel B reports broadly similar results for after-tax profits.

APPENDIX TABLE 5:
TAXABLE INCOME AND AFTER-TAX PROFIT

(A) TAXABLE INCOME

	All Years			Excl. 2017/18		
	(1)	(2)	(3)	(4)	(5)	(6)
	$ln(\pi)$	$ln(-\pi)$	$\mathbf{1}(\pi > 0)$	$ln(\pi)$	$ln(-\pi)$	$\mathbf{1}(\pi > 0)$
$\ln(1-\tau_{f,t})$	0.697		0.011	0.705		-0.349
	(0.158)		(0.075)	(0.246)		(0.102)
$\ln(1+\tau^{-}_{f,t})$		1.364			0.295	
		(0.457)			(0.688)	
2016 Outcome Mean	2.85	4.64	0.75	2.85	4.64	0.75
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Size-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N	167,773	48,236	231,360	114,988	28,681	157,725
N Firms	39,032	15,635	49,235	33,018	10,618	41,774

(B) AFTER-TAX PROFIT

	All Years			Excl. 2017/18		
	(1)	(2)	(3)	(4)	(5)	(6)
	$ln(\pi)$	$ln(-\pi)$	$\mathbf{1}(\pi > 0)$	$ln(\pi)$	$ln(-\pi)$	$\mathbf{1}(\pi > 0)$
$\ln(1-\tau_{f,t})$	1.064		-0.061	1.025		-0.436
	(0.156)		(0.075)	(0.242)		(0.103)
$\ln(1+\tau^{-}_{f,t})$		1.153			0.189	
		(0.473)			(0.725)	
2016 Outcome Mean	1.93	4.51	0.74	1.93	4.51	0.74
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Size-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N	166,197	49,698	231,360	113,865	29,618	157,725
N Firms	38,833	16,056	49,235	32,814	10,935	41,774

Notes: The unit of analysis is a firm-year. The table reports results from equation 4. Standard errors are clustered by firm. The 2016 outcome means are reported before taking logs and are scaled in nominal dollars in columns 1, 3, and 5. All specifications include firm and industry-size fixed effects, and the data are reweighted to match the joint industry-size distribution of firms in the pre-period. Specifications where the outcome is binary and the endogenous variable is logged correspond to semi-log linear probability models: the interpretation of the estimated coefficient is that a 1% increase in the log net-of-tax rate is associated with a $0.01 \times \varepsilon$ increase in the probability that the outcome = 1.

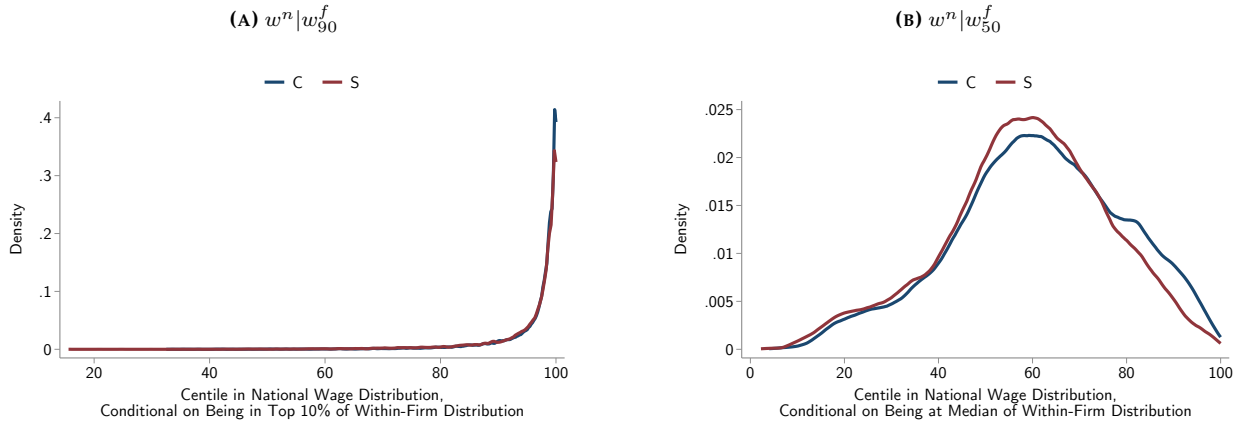
C.5 Comparing the Within-Firm and National Wage Distributions

The incidence analysis in Section 5 extrapolates treatment effects based on comparisons across the the within-firm earnings distribution to estimate distributional effects across the national earnings distribution. This appendix provides empirical evidence that this is a reasonable approximation.

First, we document that it is empirically rare that workers in the top 10% of the within-firm distribution are not also in the top 10% of the national distribution. Workers at the median of the within-firm distribution are somewhat more dispersed throughout the national wage distribution, however. These patterns are illustrated below, where w_p^n denotes rank p in the national distribution and w_p^f denotes rank p in the within-firm distribution.

Panel (a) shows that workers at the top of the within-firm are nearly always at the top of the national distribution. Panel (b) shows more dispersion, and also shows that the median earner in the corporate sector is modestly positively selected on wages relative to the median worker in the national distribution.

APPENDIX FIGURE 14: FIRM-SPECIFIC VS. NATIONAL WAGE DISTRIBUTIONS



Notes: The unit of analysis is worker-year. Panel A plots the centile of workers in the national wage distribution, conditional on being in the top 10% of the within-firm distribution, for C and S corps in the analysis sample in the pre-period. Panel B plots the centile of a worker in the national wage distribution, conditional on being at the median of the the within-firm distribution, for C and S corps in the analysis sample in the pre-period.

To disentangle the role of within-firm vs. national distributions in our results, we split the sample to estimate relative wage growth (C vs S) of workers in firms whose median workers are intially (in the pre-period) at the median of the within-firm distribution (w_{50}^f) but in the top 10% of the national wage distribution (w_{90+}^n). We also estimate relative wage growth of workers at firms initially at the 90th percentile the within-firm distribution (w_{90}^f) but not in the top 10% of the national distribution (w_{90-}^n). These tests provide bounds on the interaction effects of within-firm and national wage distributions if one is willing to assume, consistent with Figure 7, that treatment effects on earnings are likely to be weakly monotonically increasing with respect to workers' place in either distribution.

The table below reports the results. Columns 1 and 4 show the results without conditioning on initial place in the national wage distribution; columns 2 and 5 condition on initially being in the top 10% of the national distribution; and columns 3 and 6 condition on initially being below the 90th percentile of the national distribution.

APPENDIX TABLE 6: EARNINGS EFFECTS CONDITIONING ON PLACE IN THE NATIONAL EARNINGS DISTRIBUTION

	(1) w^f_{50}	(2) $w^f_{50}\|w^n_{90+}$	(3) $w^f_{50}\|w^n_{90-}$	(4) w^f_{90}	(5) $w^f_{90}\|w^n_{90+}$	(6) $w^f_{90}\|w^n_{90-}$
$C_f \times Post_t$	-0.002	-0.005	-0.002	0.007**	0.009***	0.003
	(0.002)	(0.016)	(0.002)	(0.003)	(0.003)	(0.008)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Size FE	Yes	Yes	Yes	Yes	Yes	Yes
R2	0.94	0.85	0.92	0.93	0.92	0.85
N	231,360	8,229	222,710	231,360	202,333	28,598

Notes: The unit of analysis is a firm-year. The table reports the $C \times Post$ coefficients from equation 2. The coefficients estimate average differential changes in log earnings between C and S corps before and after TCJA, controlling for firm and industry-size-year fixed effects. Column 1 reports baseline estimates for log annual earnings of workers initially at the median of the within-firm distribution in 2016. Columns 2 and 3 report estimates for log annual earnings of workers initially at the median of the within-firm distribution in 2016 and in the top 10% of the national wage distribution, and not in the top 10% of the national wage distribution, respectively. Column 4 reports baseline estimates for log annual earnings of workers initially at the 90th centile of the within-firm distribution in 2016. Columns 5 and 6 report estimates for log annual earnings of workers initially at the 90th centile of the within-firm distribution in 2016 and in the top 10% of the national wage distribution, and not in the top 10% of the national wage distribution, respectively. Standard errors are clustered by firm.

Columns 1-3 show that workers at the median of the within-firm distribution do not see increases in wages irrespective of their place in the national distribution. Columns 4-6 show that workers at the top of the within-firm distribution benefit only if they are also at the top of the national distribution. However, it is rare for workers to be "off the diagonal" of this matrix, as shown by the low observation counts in columns 2 and 6.

The results imply that extrapolating from the within-firm distribution to the national distribution yields a reasonable approximation. To the extent this analysis adds nuance to the main findings, it is that the main results may slightly understate the effects of the tax cuts on increasing wage dispersion nationally (since extrapolating the estimates at the top of the within-firm distribution to the broader population is modestly over-inclusive of a small share of top-firm workers not in the top 10% nationally.) However, given the rarity of such workers, this bias would be small.

C.6 Robustness to Alternate Specifications

Appendix Table 7 reports robustness results for the key elasticities, focusing on investment, payrolls, profits, and workers' earnings. We focus on these outcomes due to their usefulness in interpreting mechanisms and their central role in the incidence quantification in Section 5.

Row 1 reports the net-of-tax elasticities from equation 1 using the benchmark regression specification. The remaining rows assess the sensitivity of these estimates to alternate modeling choices. All specifications include firm and industry-size-year fixed effects and reweight the sample to match the pre-TCJA industry-size distribution of firms, unless noted otherwise.

Row 2 adds controls for state-by-year fixed effects, where a firm's state is defined using the address reported on Form 1120 or 1120-S. Row 3 defines industries using 6-digit, rather than 3-digit, NAICS codes. Row 4 controls for firm age with cohort-by-year fixed effects, where cohorts are defined as terciles of incorporation year. Row 5 includes controls for log mean firm employment in the pre-period, interacted with year indicators. Row 6 controls for quintiles of shareholder counts-by-year fixed effects, noting that the number of shareholders is measured imperfectly especially for C corps. Row 7 controls for pre-trends, interacting the 2013-2016 firm-specific trend in the outcome with year indicators. Row 8 controls only for firm and year fixed effects. Row 9 reports unweighted results. Row 10 winsorizes the outcomes at the 5th and 95th percentiles, and row 11 at the 1st and 99th percentiles. Row 12 weights observations by each firm's 2016 sales (and for labor market outcomes, by 2016 employment).

APPENDIX TABLE 7: ROBUSTNESS TO ALTERNATE SPECIFICATIONS

Specification	(1) $\ln(1-\tau_{ft})$	(2) ε^{I} I_t/K_{t-1}	(3) ε^{wL} Payroll	(4) ε^{π} Tax. Inc.	(5) $\varepsilon^{\pi(1-\tau)}$ After-Tax π	(6) $\varepsilon^{w_{p50}}$ p50 w	(7) $\varepsilon^{w_{p95}}$ p95 w	(8) $\varepsilon^{w_{exec}}$ Exec w
1. Benchmark	0.055	0.449	0.369	0.697	1.064	-0.027	0.224	0.466
	(0.002)	(0.070)	(0.086)	(0.158)	(0.156)	(0.039)	(0.057)	(0.079)
	231,360	204,411	231,360	167,773	166,197	231,360	231,360	231,360
2. Location Controls	0.056	0.449	0.328	0.632	0.999	-0.042	0.223	0.458
	(0.002)	(0.070)	(0.085)	(0.159)	(0.157)	(0.040)	(0.057)	(0.078)
	231,360	204,411	231,360	167,773	166,197	231,360	231,360	231,360
3. Industry = NAICS-6	0.056	0.465	0.309	0.700	1.046	0.007	0.241	0.474
	(0.002)	(0.072)	(0.086)	(0.165)	(0.163)	(0.039)	(0.057)	(0.079)
	231,360	204,055	231,360	166,789	165,193	231,360	231,360	231,360
4. Age Controls	0.056	0.461	0.262	0.695	1.064	-0.045	0.196	0.412
	(0.002)	(0.070)	(0.084)	(0.158)	(0.156)	(0.039)	(0.055)	(0.076)
	231,360	204,411	231,360	167,773	166,197	231,360	231,360	231,360
5. Finer Size Controls	0.055	0.456	0.342	0.696	1.063	-0.030	0.224	0.453
	(0.002)	(0.071)	(0.085)	(0.158)	(0.156)	(0.040)	(0.057)	(0.078)
	225,444	203,219	225,444	164,613	163,069	225,444	225,444	225,444
6. Shareholder Controls	0.058	0.388	0.322	0.568	0.957	-0.062	0.171	0.406
	(0.002)	(0.071)	(0.087)	(0.168)	(0.166)	(0.041)	(0.058)	(0.080)
	213,177	188,241	213,177	154,568	153,080	213,177	213,177	213,177
7. Pre-Trend Controls	0.055	0.447	0.374	0.707	1.080	-0.029	0.224	0.447
	(0.002)	(0.070)	(0.082)	(0.157)	(0.155)	(0.039)	(0.055)	(0.077)
	231,360	204,411	231,360	167,773	166,197	231,360	231,360	231,360
8. Firm + Year FE Only	0.056	0.459	0.359	0.696	1.069	-0.039	0.227	0.455
	(0.002)	(0.071)	(0.087)	(0.158)	(0.156)	(0.040)	(0.057)	(0.079)
	231,360	204,432	231,360	167,838	166,265	231,360	231,360	231,360
9. Unweighted	0.053	0.427	0.453	0.765	1.122	-0.023	0.224	0.511
	(0.001)	(0.073)	(0.087)	(0.157)	(0.155)	(0.041)	(0.058)	(0.080)
	231,360	204,411	231,360	167,773	166,197	231,360	231,360	231,360
10. Winsorize 5-95	0.055	0.449	0.510	0.697	1.064	-0.001	0.242	0.585
	(0.002)	(0.070)	(0.080)	(0.158)	(0.156)	(0.034)	(0.045)	(0.068)
	231,360	204,411	231,360	167,773	166,197	231,360	231,360	231,360
11. Winsorize 1-99	0.055	0.647	0.394	0.615	0.943	-0.018	0.198	0.490
	(0.002)	(0.160)	(0.085)	(0.161)	(0.158)	(0.038)	(0.052)	(0.075)
	231,360	204,411	231,360	167,773	166,138	231,360	231,360	231,360
12. Firm Size Weights	0.076	0.381	0.327	0.752	1.308	0.067	0.242	0.331
	(0.003)	(0.082)	(0.112)	(0.172)	(0.172)	(0.050)	(0.056)	(0.137)
	231,360	204,411	231,360	167,773	166,197	231,360	231,360	231,360

Notes: The table reports the first stage from equation 3 (column 1) and the net-of-tax elasticities from equation 4 (columns 2-8). The outcomes are: the net-of-tax rate, taxable income, after-tax profits, net investment, payroll, median worker earnings, 95th centile worker earnings, and executive earnings. All outcomes except net investment are logged. All specifications include firm and industry-size-year fixed effects and are reweighted to match the joint industry-size distribution of firms in the pre-period, unless otherwise noted. Row 1 shows the benchmark specification. Row 2 controls for state-by-year fixed effects, using the firm's reported address from the 1120. Row 3 defines the industries as 6-digit (rather than 3-digit) NAICS codes. Row 4 includes cohort-by-year effects, where cohorts are defined as terciles of incorporation year. Row 5 includes controls for log mean firm employment in the pre-period, interacted with year indicators. Row 6 controls for quintiles of shareholder counts-by-year fixed effects, noting that the number of shareholders is measured imperfectly especially for C corps. Row 7 controls for pre-trends, interacting 2013-2016 changes in the outcome with year indicators. Row 8 includes only firm and year fixed effects. Row 9 reports unweighted regressions. Rows 10 and 11 apply alternate winsorizing thresholds. Row 12 weights the regressions by mean firm sales, or by mean firm employment for labor market outcomes; these weights are winsorized at the 5th/95th percentiles to improve precision. Standard errors are clustered by firm.

C.7 Robustness to Alternate Samples and Controlling for Other TCJA Provisions

Appendix Table 8 assesses the sensitivity of the results to alternate samples. Row 1 shows the benchmark specification. Row 2 excludes unbalanced industry-size cells, defined as those in which C corps account for greater than 80% or less than 20% of firms in the unweighted sample. Row 3 excludes manufacturing firms, which may have been affected, for example, by changes in tax deductions for domestic production or by the 2018-2019 U.S.-China trade war. Row 4 uses the balanced panel. Row 5 excludes tax years 2017 and 2018 from the estimation, which are the years most likely to be affected by anticipation effects or intertemporal profit shifting (Dowd et al. 2020). Row 6 excludes S corps with only one owner, who have the strongest incentives to reclassify wages and profits. The results indicate that these adjustment margins do not drive the results.

The benchmark elasticities reported in the paper are estimated from data samples that exclude public and multinational firms, where multinationals are defined as firms that earn more than 5% of their revenues abroad. A reason to exclude public and multinational firms is to improve the covariate balance of C and S corps, since S corps cannot be public firms and C corps are modestly more likely to be multinational firms. However, there is also reason to include them in the analysis: if, for example, tax cuts cause C corps to expand, and expanding firms are more likely to become public firms or increase their foreign sales, then excluding them would bias our elsaticitiy estimates downward. Empirically, we find that C corps are approximately 0.006 percentage points ($\approx 9\%$) more likely to become public in 2018 and 2019 relative to C corps in the pre-period, controlling for firm and industry-size fixed effects. (Note that this analysis leverages a single difference and cannot account for time fixed effects, since S corps are not a valid control in this case.) However, we do not find any change in firms' foreign sales share after TCJA. Table 8 reports the net-of-tax elasticities from samples that include public and multinational firms. Row 7 includes public firms, and row 8 adds multinationals. The elasticities are broadly within the confidence intervals of the benchmark results.

Rows 9 to 12 report additional tests to control for exposure to concurrent TCJA policies. Row 9 excludes industries most likely to claim DPAD deductions. Row 10 controls for third-order polynomials in NOL deductions scaled by sales at the industry-size level in the pre-period and interacted with year dummies. Rows 11 and 12 do the same for net interest deductions scaled by sales and for exposure to bonus depreciation as in Zwick and Mahon (2017), both measured at the firm level in the pre-period.

APPENDIX TABLE 8: ROBUSTNESS TO ALTERNATE SAMPLES AND TO CONTROLS FOR CONCURRENT POLICIES

Specification	(1) $\ln(1-\tau_{ft})$	(2) ε^{I} I_t/K_{t-1}	(3) ε^{wL} Payroll	(4) ε^{π} Tax. Inc.	(5) $\varepsilon^{\pi(1-\tau)}$ After-Tax π	(6) $\varepsilon^{w_{p50}}$ p50 w	(7) $\varepsilon^{w_{p95}}$ p95 w	(8) $\varepsilon^{w_{exec}}$ Exec w
1. All Firms	0.055	0.449	0.369	0.697	1.064	-0.027	0.224	0.466
s.e.	(0.002)	(0.070)	(0.086)	(0.158)	(0.156)	(0.039)	(0.057)	(0.079)
N	231,360	204,411	231,360	167,773	166,197	231,360	231,360	231,360
2. Exclude Mismatch Ind-Size Bins	0.057	0.428	0.288	0.658	1.028	-0.043	0.203	0.423
s.e.	(0.002)	(0.070)	(0.084)	(0.159)	(0.157)	(0.039)	(0.056)	(0.077)
N	222,118	196,411	222,118	162,436	160,901	222,118	222,118	222,118
3. Exclude Mfg Industries	0.054	0.487	0.381	0.706	1.078	-0.021	0.270	0.557
s.e.	(0.002)	(0.084)	(0.102)	(0.184)	(0.182)	(0.047)	(0.068)	(0.092)
N	184,024	161,909	184,024	133,519	132,133	184,024	184,024	184,024
4. Balanced Panel	0.057	0.360	0.348	0.647	0.992	-0.035	0.224	0.669
s.e.	(0.002)	(0.070)	(0.150)	(0.196)	(0.195)	(0.063)	(0.081)	(0.114)
N	139,076	134,227	138,443	108,746	107,758	138,409	138,409	138,419
5. Exclude 2017-2018	0.053	0.915	0.271	0.705	1.025	-0.061	0.217	0.539
s.e.	(0.002)	(0.111)	(0.120)	(0.246)	(0.242)	(0.055)	(0.080)	(0.113)
N	157,725	140,344	157,725	114,988	113,865	157,725	157,725	157,725
6. Exclude Single-Owner S-Corps	0.060	0.398	0.421	0.776	1.109	0.011	0.244	0.523
s.e.	(0.002)	(0.071)	(0.089)	(0.163)	(0.162)	(0.039)	(0.057)	(0.083)
N	197,372	175,455	197,372	139,964	138,701	197,372	197,372	197,372
7. + Public Firms	0.055	0.439	0.546	0.749	1.127	-0.018	0.257	0.616
s.e.	(0.002)	(0.069)	(0.089)	(0.153)	(0.151)	(0.040)	(0.059)	(0.083)
N	242,473	215,051	242,473	174,970	173,381	242,473	242,473	242,473
8. + Multinational Firms	0.055	0.415	0.643	0.828	1.236	-0.018	0.225	0.637
s.e.	(0.001)	(0.065)	(0.088)	(0.139)	(0.138)	(0.038)	(0.056)	(0.083)
N	274,985	244,968	274,985	195,795	194,094	274,985	274,985	274,985
9. Exclude DPAD Industries	0.051	0.432	0.190	0.755	1.190	0.055	0.236	0.332
s.e.	(0.003)	(0.125)	(0.161)	(0.289)	(0.287)	(0.074)	(0.102)	(0.138)
N	83,050	74,752	83,050	61,230	60,445	83,050	83,050	83,050
10. NOL Exposure Controls	0.055	0.449	0.369	0.697	1.064	-0.027	0.224	0.466
s.e.	(0.002)	(0.070)	(0.086)	(0.158)	(0.156)	(0.039)	(0.057)	(0.079)
N	231,354	204,411	231,354	167,767	166,191	231,354	231,354	231,354
11. 163(j) Exposure Controls	0.055	0.465	0.368	0.746	1.089	-0.026	0.220	0.475
s.e.	(0.002)	(0.073)	(0.087)	(0.164)	(0.161)	(0.040)	(0.057)	(0.080)
N	231,354	204,411	231,354	167,767	166,191	231,354	231,354	231,354
12. Bonus Exposure Controls	0.056	0.472	0.371	0.679	1.043	-0.027	0.223	0.463
s.e.	(0.002)	(0.070)	(0.086)	(0.158)	(0.156)	(0.039)	(0.057)	(0.079)
N	231,354	204,411	231,354	167,767	166,191	231,354	231,354	231,354

Notes: The table reports the first-stage estimates and net-of-tax elasticities for key outcomes estimated from equation 3 (column 1) and equation 4 (columns 2-8) using alternate samples and including controls for concurrent policy changes. The outcomes are: the net-of-tax rate, taxable income, after-tax profits, net investment, payrolls, median worker earnings, 95th centile worker earnings, and executive earnings. All outcomes except net investment are logged. All specifications include firm and industry-size-year fixed effects and are reweighted to match the joint industry-size distribution of firms in the pre-period. Row 1 shows the benchmark specification with all firms. Row 2 excludes industry-size bins where the unweighted firm share of C corps is less than 20% or greater than 80%. Row 3 excludes manufacturing industries. Row 4 uses the balanced panel. Row 5 excludes years 2017 and 2018. Row 6 excludes single-owner S corps. Row 7 excludes the industries that account for >90% of DPAD deductions in the pre-period. Row 8 controls for NOL exposure, defined as cubic polynomials in the average observed ratio of NOL deductions to sales at the industry-size level in the pre-period interacted with year dummies. Row 9 similarly controls for 163(j) exposure, using the ratio of net intrest to sales at the firm level in the pre-period. Row 10 similarly controls for bonus exposure, where bonus exposure is defined at the firm level as in Zwick and Mahon (2017). See Appendix B for additional details.

C.8 Accounting for Expectations and Adjustment Costs

TCJA permanently lowered the corporate income tax rate from 35% to 21%, but the provisions affecting the individual tax code (and therefore affecting S corps) were formally temporary. For example, the the top personal income tax rate cut was scheduled to expire in 2026 and then rebound to 39.6%, and the QBI provisions were scheduled to similarly sunset. This Appendix evaluates how S corps' sensitivity to the temporary provisions of TCJA may affect the main results using methods developed in the literature.

Auerbach and Hassett (1992) study the effects of taxes on investment when firms are forward-looking and tax policy changes over time. The authors show that, in the presence of adjustment costs, optimization implies that firms' investment is a function of a weighted average of current and future user costs, given by:

$$\phi_t = \sum_{s \geq t} w_{s-t} \phi_s \tag{16}$$

where ϕ_s is the user cost of capital in period s, and w_{s-t} represents weights that sum to one and reflect adjustment costs. If adjustment costs are low, the weights assigned to future periods are low, and current investment is less sensitive to future user costs. Conversely, if adjustment costs are high, then the weights assigned to future periods are high, and investment is highly responsive to future user costs. Cohen, Hansen, and Hassett (2002) formulate the weights in Equation 16 using the parameter Ω:

$$w_0 = 1 - \Omega \tag{17}$$

$$w_{s-t} = w_{s-t-1} * \Omega, \forall s > t$$

such that $\sum_{s \geq t} w_{s-t} = 1$. The expression implies that, if $\Omega = 0.5$, the weights in each successive year are half as large as the prior year, and the successive year's user costs are half as important. We assess sensitivity to TCJA's temporary provisions affecting S corps by varying the Ω parameter and assuming that, after those provisions were set to expire in 2026, the marginal tax rate facing an S corp reverts back to its value in 2017.

The first row of Appendix Table 9 reports the benchmark net-of-tax elasticities using the marginal income tax rates reported in the main text; these specifications implicitly put a weight of 1 on the current period and weights of zero on all successive periods. Rows 2-4 vary the adjustment cost parameter, thus allowing firms to be forward-looking. Cohen, Hansen, and Hassett (2002) use a range of Ω from 0.3 to 0.7, arguing these correspond to plausible low and high ranges. We extend this range to also include a very high parameter value of 0.9 (i.e., a very high weight on future tax rates). For any value of Ω, the elasticities are very close to the benchmark case. The reason is that, using this conventional model, the temporary provisions of TCJA were scheduled to sunset too distantly in the future to meaningfully affect investment decisions in 2018 and 2019.

APPENDIX TABLE 9: FORWARD-LOOKING ELASTICITIES WITH ADJUSTMENT COSTS

	(1) ε^I	(2) ε^L	(3) ε^π	(4) $\varepsilon^{\pi(1-\tau)}$	(5) $\varepsilon^{w_{p50}}$	(6) $\varepsilon^{w_{p95}}$	(7) $\varepsilon^{w_{exec}}$
Benchmark	0.449	0.369	0.697	1.064	-0.027	0.224	0.466
	(0.070)	(0.086)	(0.158)	(0.156)	(0.039)	(0.057)	(0.079)
$\Omega = 0.3$	0.449	0.369	0.697	1.064	-0.027	0.224	0.466
	(0.070)	(0.086)	(0.158)	(0.156)	(0.039)	(0.057)	(0.079)
$\Omega = 0.5$	0.447	0.368	0.696	1.062	-0.027	0.223	0.464
	(0.070)	(0.086)	(0.158)	(0.156)	(0.039)	(0.056)	(0.078)
$\Omega = 0.7$	0.434	0.357	0.685	1.045	-0.026	0.217	0.450
	(0.068)	(0.083)	(0.155)	(0.153)	(0.038)	(0.055)	(0.076)
$\Omega = 0.9$	0.399	0.328	0.654	0.998	-0.024	0.199	0.413
	(0.062)	(0.076)	(0.148)	(0.146)	(0.035)	(0.050)	(0.069)

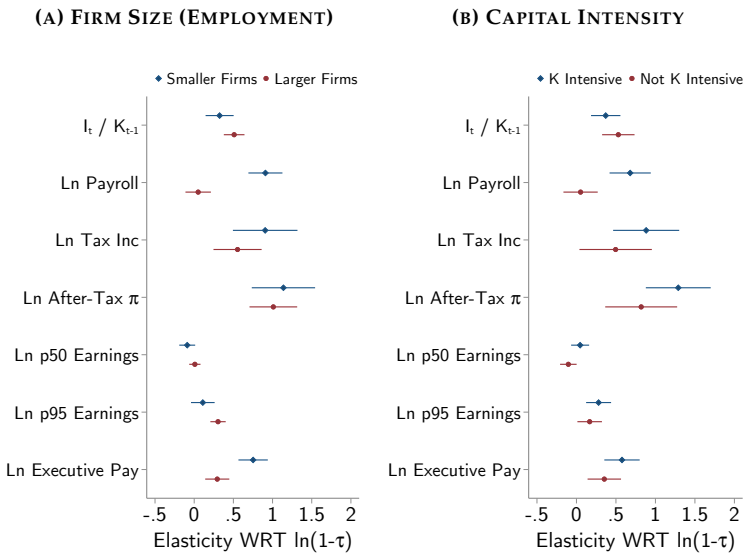
Notes: Row 1 reports benchmark net-of-tax elasticities using the marginal income tax rates reported in the main text. Rows 2-4 report elasticities with respect to the net-of-tax rate while varying the adjustment cost parameter, Ω. See above for details.

C.9 Heterogeneity

Existing research finds that the effects of tax changes may vary with firm size (if smaller firms face borrowing constraints, as in Zwick and Mahon 2017 and Saez et al. 2019), or by factor intensity (where capital-intensive firms may be most responsive, as in Acemoglu and Guerrieri 2008). We examine heterogeneity in the results across firms that vary in these characteristics. Smaller vs. larger firms are defined as those with above vs. below median employment in the first year the firm enters the sample. Capital intensity is defined at the industry level as the ratio of capital to sales in the pre-period, and the sample is split at the median.

Panels A and B of Appendix Figure 15 show that the elasticities do not appear to vary systematically with firm size or capital intensity. However, there is suggestive evidence that smaller firms were more likely to expand payrolls and increase executive pay, and that capital intensive firms exhibited a larger payroll elasticity. Although salary and wage expenses are fully tax deductible, the large payroll response suggests that hiring, training, and retaining workers may impose additional costs on firms that are not fully deductible in practice. These tests are suggestive of liquidity effects for small firms, and broadly consistent with price effects driven by reductions in the cost of capital for all firms.

APPENDIX FIGURE 15: FIRM HETEROGENEITY



Notes: The unit of analysis is a firm-year. The panels report heterogeneity in the net-of-tax elasticities for firms with varying characteristics. Smaller vs. larger firms are defined as those with above vs. below median employment in the first year the firm enters the sample. Capital intensity is the ratio of capital to sales at the industry level and the sample is also split at the median. All specifications control for firm and industry-size-year fixed effects and reweight the sample to match the joint industry-size distribution of firms in the pre-period. Error bars show 95% confidence intervals.

C.10 Evaluating Results at Longer Time Horizons

Researchers seeking to estimate the causal effects of TCJA's corporate tax cuts on firms and workers over longer time horizons must inevitably confront inference challenges posed by the COVID-19 pandemic. There are two overarching concerns. First, the internal validity of the research design comparing C and S corps may be violated by pandemic-era policies that differentially affected each firm type. Second, the external validity of estimates from this period may be low. We discuss both issues below.

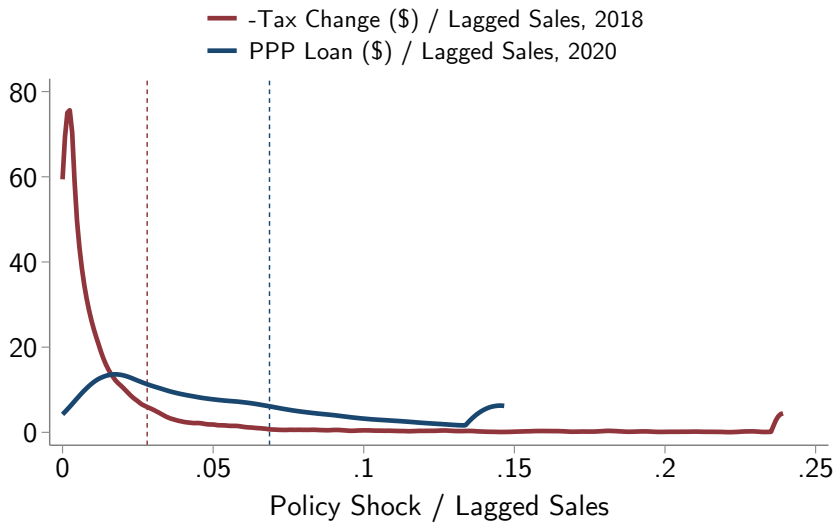
Internal Validity

A key concern is that pandemic-era policy may have differentially affected C and S corps and thus confound the research design beginning in 2020. Congress passed the Coronavirus Aid, Relief, and Economic Security (CARES) Act in March 2020 in response to the sharp recession caused by the pandemic. The Paycheck Protection Program (PPP), enacted as part of this law, offered financial support to eligible firms in the form of forgivable loans.

The purpose of PPP loans, as articulated by Congressional policymakers, was to encourage and incentivize business owners to continue employing and paying workers despite broad-based closures of businesses that were often mandated by state and local governments (U.S. Senate Committee on Small Business & Entrepreneurship 2020). Accordingly, the law required firms receiving PPP loans to spend at least 75% of their value on payroll costs, while the remaining 25% could be used to cover non-payroll expenses such as interest, rent, or utility payments. Loan amounts for eligible firms were determined primarily based on 2019 payroll expenses, as documented by firms in their loan applications.

The scope and magnitude of this stimulus was extremely large, with aggregate PPP loan volume of approximately $589 billion in 2020 and $209 billion 2021. For comparison, the $589 billion in the first year of the PPP program far eclipses the approximately $100 billion decline in corporate tax receipts in 2018, the year TCJA was first enacted (Congressional Budget Office 2019). To compare the magnitudes of the two shocks, we merge data from Splinter et al. (2025), linking PPP loan data with entity level tax returns, onto our analysis sample. Among firms that claimed PPP loans in our analysis sample, the average firm loan value in 2020 as a share of lagged revenue was approximately 2 times larger than the average firm tax cut scaled by lagged revenue in 2018. These distributions are shown in Appendix Figure 16, with the dashed vertical lines indicating the mean of each distribution.

APPENDIX FIGURE 16: TCJA VS. PPP LOANS



Notes: The unit of analysis is firms. The figure shows the distributions from our analysis sample of (a) the absolute value of negative tax changes in 2018, scaled by lagged sales, shown in red; and (b) the value of positive PPP loans in 2020 scaled by lagged sales, shown in blue. The red dashed vertical line indicates the mean value of tax cuts scaled by lagged sales, and the blue dashed vertical line indicates the mean value of PPP loans scaled by lagged sales. The average value of a PPP loan was approximately 2 times larger than the value of the average firm tax cut.

Appendix Table 10 investigates differential take-up of PPP loans for C versus S corps in our sample. Panel A reports results from linear probability models that regress PPP loan take-up (0/1) on an S corp indicator in the cross section of firms in the 2020 SOI sample. Column 1 includes no controls, column 2 adds industry controls, and column 3 adds controls for log sales and log payroll. S corps were significantly more likely to receive PPP loans than C corps, even after controlling for firm characteristics such as firm size and industry. The coefficient estimates indicate that S corps were around 17-20 percentage points (≈50%) more likely to claim a PPP loan than similar C corps. This finding of higher take-up for S corps is consistent with population-wide estimates of PPP loans documented in Splinter et al. (2025).

Why were S corps more likely than C corps to claim PPP loans? It is likely that, for several reasons, the loans were more valuable to S corp owners, and so those owners were more likely to apply for loans. In particular, since S corp shareholders often directly manage their own firms and have substantial discretion to pay themselves wages (Smith et al. 2019), the shareholders were likely able to directly capture a large fraction of the transfer while still abiding by the legal terms of the loan. Moreover, the 25% portion of the transfer dedicated to covering non-payroll expenses may directly offset costs and increase after-tax profits.

APPENDIX TABLE 10: PPP LOAN TAKEUP FOR C VS. S CORPS

	(1) 0/1	(2) 0/1	(3) 0/1	(4) 0/1
s	0.209	0.168	0.163	
	(0.004)	(0.004)	(0.004)	
S $<$ 5 owners				0.186
				(0.004)
S 5-10 owners				0.157
				(0.007)
S 11-50 owners				0.074
				(0.009)
S $>$ 50 owners				0.006
				(0.013)
C Corp Mean	0.40	0.40	0.40	0.40
Industry FE	No	Yes	Yes	Yes
Size Controls	No	No	Yes	Yes
R2	0.04	0.13	0.22	0.23
N	371,229	371,229	371,229	371,229

Notes: The unit of analysis is a firm-year. The table shows results from a linear probability model that regresses an indicator for Paycheck Protection Program (PPP) take-up on an indicator for S corp status for all firms in the 2020 SOI corporate sample. Column 1 includes no controls, column 2 includes an industry fixed effect and column 3 includes controls for log sales and log payroll. Column 4 includes indicator variables for S corporations with less than 5 owners, S corporations between 5 and 10 owners, S corporations between 11 and 50 owners, and S corporations with more than 51 owners, industry fixed effects and controls for log sales and log payroll. Standard errors are clustered by firm.

For both C and S corp owners, after conditioning on payroll (and thus loan size), the value of a PPP loan is inversely related to the number of shareholders, simply because the windfall must be split among more of them. Because S corps have fewer shareholders, they thus had stronger incentives to apply for loans.

Appendix Table 10 Column 4 provides evidence consistent with this hypothesis. The table reports results from regressions of PPP take-up (0/1) on indicators for S corps with (a) fewer than 5 owners (i.e., shareholders), (b) 5-10 owners, (c) 11-50 owners, and (d) >50 shareholders. The specification includes industry fixed effects and firm size controls, and the reference category is take-up by C corps. Consistent with the reasoning above, the results show that the S corp take-up rate relative to C corps is steadily declining in the number of shareholders.[18]

Appendix Table 11 shows results from an identical set of specifications where the outcome is the log value of the loan, conditional on take-up. The table reports an unconditional log loan gap that favored S corps by 23.8% in column 1 and by 25.3% when controlling for industry fixed effects in

[18]Individual-level information about C corp shareholders are not observable in the data and so we cannot conduct a similar exercise among C corps. We expect that a similar pattern would emerge if the data existed.

column 2. The gap shrinks to 6.6% in column 3 after controlling for firm size variables, consistent with the eligibility determinants. Column 4 does not reveal a clear monotonic relationship between loan values and the number of S corp shareholders, conditional on take-up.

APPENDIX TABLE 11: PPP LOAN AMOUNTS FOR C VS. S CORPS, CONDITIONAL ON TAKE-UP

	(1) Ln(Amount)	(2) Ln(Amount)	(3) Ln(Amount)	(4) Ln(Amount)
s	0.238	0.253	0.066	
	(0.015)	(0.015)	(0.010)	
S $<$ 5 owners				0.061
				(0.010)
S 5-10 owners				0.101
				(0.016)
S 11-50 owners				0.060
				(0.035)
S $>$ 50 owners				-0.119
				(0.144)
C Corp Mean (mil)	1.46	1.46	1.46	1.46
Industry FE	No	Yes	Yes	Yes
Size Controls	No	No	Yes	Yes
R2	0.01	0.08	0.62	0.62
N	183,081	183,081	183,081	183,081

Notes: The unit of analysis is a firm-year. The table shows results from a model that regresses the log dollar value of Paycheck Protection Program (PPP) loan values on an indicator for S corp status for all firms in the 2020 SOI corporate sample. Column 1 includes no controls, column 2 includes an industry fixed effect and column 3 includes controls for log sales and log payroll. Column 4 includes indicator variables for S corporations with less than 5 owners, S corporations between 5 and 10 owners, S corporations between 11 and 50 owners, and S corporations with 51 or more owners, industry fixed effects and controls for log sales and log payroll. Standard errors are clustered by firm.

Differential take-up of PPP loans poses a threat to the identification strategy for several reasons. As documented above, the average firm loan amount was economically large — significantly larger than our estimate of the average firm tax cut due to TCJA. Moreover, other researchers have documented that PPP loans measurably impacted firm outcomes. E.g., Autor et al. (2022), Dalton (2025), and Splinter et al. (2025) find that PPP loans increased wages and employment, and Hubbard and Strain (2020), Bartik et al. (2020), Kurmann et al. (2021), Autor et al. (2022), and Dalton (2025) find that PPP loans reduced permanent closures.

The confounding influence of PPP loans imply it is challenging to sign the direction of treatment bias from PPP loans using the C vs. S research design. If loans improved outcomes for recipient firms, take-up bias favoring S corps may increase levels of e.g. capital and employment in S corps relative to C corps, and attenuate causal estimates of the effects of TCJA's tax cuts. But if PPP loans buoyed declining firms that otherwise would have permanently closed, this would generate non-random sample attrition. In that case, declining C corps would disappear from the data while comparable declining S firms would survive, artificially inflating treatment effect estimates from the C vs. S design.

Disentangling the effects of the two overlapping policy interventions requires an alternate research design with multiple instrumental variables. We consider this a worthy ambition, but one that is beyond the scope of this paper.

A further concern is that if ownership structure affected PPP loan take-up in 2020, perhaps it also affected how S and C firms responded to TCJA's tax cuts in 2018 and 2019. This is unlikely for two reasons. First, Appendix C.1 provides direct evidence that C and S corps historically responded similarly to tax shocks, despite their different ownership structures. Second, in contrast to the pandemic-era relief programs, in the 2013 to 2019 period there were no analogous large-scale policies with elective take-up that specifically linked ownership structure to a substantial government-funded windfall or tax benefit.

External Validity

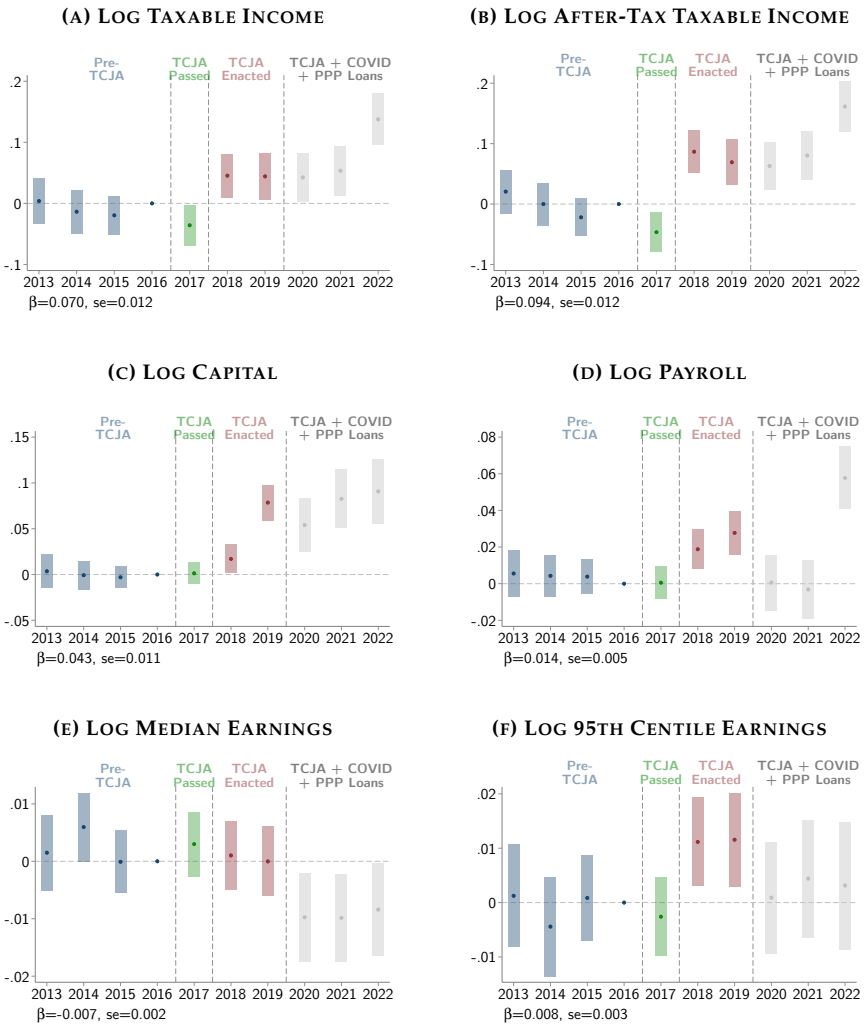
Extending the sample through COVID and the subsequent period of high inflation and macroeconomic volatility further raises concerns about external validity. Existing research finds that investment, for example, is highly sensitive to uncertainty (Bloom et al. 2007, Baker et al. 2024, Alfaro et al. 2024), and is more strongly pro-cyclical than other components of GDP (Andrle et al. 2016). Against the backdrop of the recession, inflation, and sharply elevated uncertainty during the 2020-2022 period (Altig et al. 2020), tax cuts might have muted effects on capital investment and labor markets, even if tax cuts would increase investment and labor demand in a more stable and predictable macroeconomic environment.

Evaluating Results at Longer Horizons

The considerations above imply that, for the post-pandemic period, the internal validity of the C vs. S research design is likely to be weak and external validity is likely to be low. Thus we emphasize that analyses of this period must be interpreted with care and caution. Nevertheless, below in Appendix Figure 17 we present estimates from event studies that extend the sample to 2022 (the most recent year of SOI data available) and attempt to offer interpretations of the results.

Panels A-C of Figure 17 show that pre-tax profits, after-tax profits, and depreciable capital remained elevated in C corps relative to S corps in the 2020-2022 period. These results are plausibly consistent with the view that the corporate tax cuts generated persistent and perhaps growing gains for firms. Panels D-F by contrast indicate that payroll and wage gains of C corps relative to S corps were eroded during the pandemic. This is perhaps not surprising, since PPP loans disproportionately benefited S corps and were implemented with the explicit legal purpose of increasing employment and wages of recipient firms.

APPENDIX FIGURE 17: EVENT STUDIES AT A 5-YEAR HORIZON



Notes: In response to the COVID-19 pandemic, in 2020 and 2021 the US federal government distributed more than $800 billion in Payroll Protection Program (PPP) loans, which disproportionately flowed to S corporations and threaten the internal validity of the research design starting in 2020, particularly for payroll and wage outcomes (see above for additional discussion). These estimates should thus be interpreted with care and caution. The unit of analysis is a firm-year. The panels plot the β_t coefficients from the main event studies, adding estimates for years 2020-2022. The coefficients capture average differences in outcomes between C and S corps over time after controlling for firm and industry-size-year fixed effects and reweighting the sample to match the joint industry-size distribution of firms in the pre-period. The declines in taxable income and after-tax income in 2017 in Panels A and B reflect intertemporal income shifting by firms; see Appendix C.3. Standard errors are clustered by firm and error bands show 95% confidence intervals. The corresponding DiD coefficient and standard error from equation 2 are shown in the bottom left of each panel. See Section 3 and Appendix B for data and measurement details.

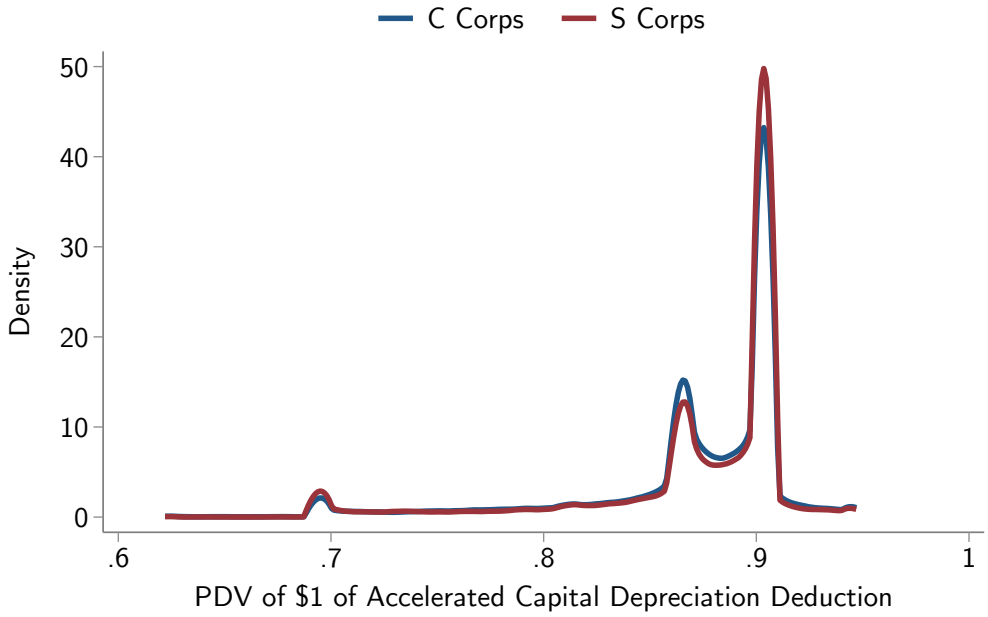
C.11 Bonus Depreciation

We assess the relevance of expensing (also called bonus depreciation) in the research design in several ways.

First, Appendix C.1 documents that C and S firms responded virtually identically to changes in accelerated depreciation policies in the past. This does not rule out potential interactions between bonus and the tax rate, but it does restrict the set of possible effects. If C and S corps responded heterogeneously to expensing changes, our research design would be unable to disentangle their effects relative to changes in tax rates. However, that does not appear to be the case empirically.

Second, we document below that exposure to expensing was highly similar among C and S corps in our data sample in the pre-TCJA period. Specifically, we use the methodology of Zwick and Mahon (2017) to compute firm-specific measures of the present discounted value of $1 of accelerated depreciation deductions in the pre-period using empirically observable data on their asset-type mix from IRS Form 4562. If C and S firms were differentially exposed to expensing changes, it would be more challenging to disentagle the effects of bonus versus tax changes. The distributions below show that this does not appear to be the case empirically.

APPENDIX FIGURE 18: EXPOSURE OF C AND S CORPS TO BONUS DEPRECIATION



Notes: The figure plots the unweighted distributions of the present discounted value (PDV) of $1 of a bonus depreciation deduction in 2016 for C and S corps in the analysis sample, using the methodology of Zwick and Mahon (2017) at the firm level. To improve the data visualization the plot excludes observations where the outcome is zero.

Third, we estimate the paper's reduced form equation 2, modified to include interactions of the pre-TCJA bonus exposure measure z with a $Post_t$ indicator and a $C_f \times Post_t$ indicator. These results are reported below. Column 1 reports results with no interaction. Column 2 includes the interaction term and reports results where we construct the pre-TCJA bonus exposure measure z_f at the firm level. Column 3 reports results where we construct the pre-TCJA bonus exposure measure z_i at the industry level. Adding $z \times Post$ in column 2 absorbs any post-TCJA change in investment that varies with expensing exposure and is common to C and S firms. The stability of the $C \times Post$ coefficient suggests the baseline C vs. S differential is not primarily driven by differential expensing exposure across entity types. The estimated $C \times z \times Post$ interaction is negative and statistically significant, implying the C vs. S differential is smaller for firms with greater expensing exposure. Using an industry-level expensing exposure measure, the corresponding interaction term in column 3 remains negative but is not statistically significant. Broadly, however, the negative point estimates for $C \times z \times Post$ are consistent with the theoretical prediction that rate sensitivity is smaller for firms with greater expensing exposure.

APPENDIX TABLE 12: INTERACTIONS OF BONUS EXPOSURE AND TAX CHANGES

	(1) $I_{ft}/K_{f,t-1}$	(2) $I_{ft}/K_{f,t-1}$	(3) $I_{ft}/K_{f,t-1}$
$C_f \times Post_t$	0.026	0.027	0.026
	(0.004)	(0.004)	(0.004)
$C_f \times Post_t \times z_f$		-0.027	
		(0.004)	
$C_f \times Post_t \times z_i$			-0.006
			(0.004)
Firm FE	Yes	Yes	Yes
Industry-Size FE	Yes	Yes	Yes
N	204,411	204,411	204,411

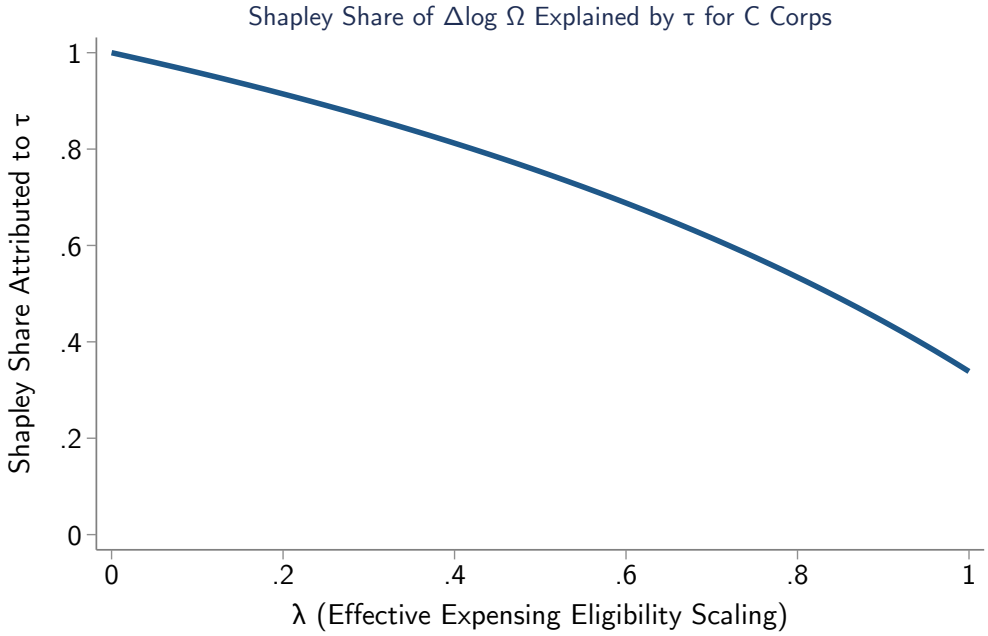
Notes: The unit of analysis is a firm-year. The outcome is net investment scaled by lagged capital. Column 1 reports the $C \times Post$ coefficient from equation 2, which estimates average differential changes in investment between C and S corps before and after TCJA controlling for firm and industry-size-year fixed effects and reweighting the sample to match the joint industry-size distribution of the sample in the pre-period. Standard errors are clustered by firm. Columns 2 and 3 also include an interaction between $C \times Post$ and a firm-level and industry-level measure of pre-TCJA bonus depreciation exposure, respectively, constructed following Zwick and Mahon (2017). In column 2 the specification includes an indicator for observations where pre-period firm-level exposure is not observable in the data.

Fourth, to quantify the share of the change in the user-cost tax term $\Omega(\tau, z; \lambda) = (1 - \lambda z \tau)/(1 - \tau)$ that is plausibly driven by the changes in the statutory tax rate versus expensing in a user cost calibration, and to show how this depends on the effective take-up parameter λ, we perform a Shapley decomposition using the mean parameters in Appendix Table 2 and vary λ from 0 to 1. Because Ω is nonlinear in τ and z, the marginal contribution of each component depends on the order in which changes are applied. The Shapley decomposition resolves this by averaging each component's marginal effect across the two possible orderings (τ then z, and z then τ). Letting $F(\tau, z; \lambda) \equiv \log \Omega(\tau, z; \lambda)$ the Shapley contribution of the tax-rate change is given by:

$$\Delta_{\tau}^{Sh} = \tfrac{1}{2}\Big(\big[F(\tau_1, z_0; \lambda) - F(\tau_0, z_0; \lambda)\big] + \big[F(\tau_1, z_1; \lambda) - F(\tau_0, z_1; \lambda)\big] \Big)$$

with an analogous expression for expensing, $\Delta_{\tau}^{Sh} + \Delta_{z}^{Sh} = F(\tau_1, z_1; \lambda) - F(\tau_0, z_0; \lambda)$. To illustrate the results from the decomposition, Figure 19 plots Δ_{τ}^{Sh} against values of $\lambda \in [0, 1]$. In the benchmark parameterization where $\lambda = 0.6$, the decomposition implies that approximately 70% of the change in $\log \Omega$ for C corps is attributable to the tax rate change and 30% to expensing. The relative importance of expensing in driving the change in user costs rises is positively correlated with λ. Kitchen and Knittel (2016) document that bonus take-up rates empirically vary between 50-70% for C and S corporations, suggesting an approximate range of Δ_{τ}^{Sh} between 62-75%.

APPENDIX FIGURE 19: DECOMPOSING THE ROLE OF τ AND z IN THE TAX TERM



Notes: The figure plots the Shapley share of the change in the user-cost tax term, $\Delta \log \Omega(\tau, z; \lambda)$, that is attributed to the statutory tax-rate change for C corporations as the effective take-up parameter λ varies from 0 to 1. We define $\Omega(\tau, z; \lambda) = (1 - \lambda z \tau)/(1 - \tau)$ and compute the Shapley contribution of τ as the average marginal effect of changing τ holding z fixed at its pre-TCJA value and at its post-TCJA value. Parameter values for τ and z are the pre- and post-TCJA means reported in Appendix Table A.2. $\lambda = 0.5$ corresponds to the baseline calibration.

C.12 Market-Level Analysis Placebo / Pre-Trend Test

The identifying assumption of the market-level research design in Section 4.6 is parallel trends in the absense of treatment (Borusyak, Hull, and Jaravel 2025). While this assumption is not directly testable, Appendix Table 13 reports placebo tests where the outcome is lagged by four periods to test for pre-trends. The placebo elasticities are economically small and statistically insignificant. The parallel trend assumption is consistent with the evidence from the pre-period.

APPENDIX TABLE 13: MARKET-LEVEL PRE-TREND TESTS

	(1)	(2)	(3)	(4)
	Panel A: Log Median Earnings			
$\ln(1-\tau_{mt})$	-0.112	0.033	-0.109	-0.089
	(0.333)	(0.234)	(0.086)	(0.081)
	Panel B: Log 95th Centile Earnings			
$\ln(1-\tau_{mt})$	0.088	0.143	-0.033	-0.037
	(0.291)	(0.281)	(0.042)	(0.040)
Market	State-Ind	State-Ind	State	State
Market FE	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	No	No
Ind-Year FE	Yes	Yes	No	No
Year Fe	No	No	Yes	Yes
Controls	No	Yes	No	Yes
First Stage F	133	136	229	239
N	6,195	6,195	204	204

Notes: The unit of analysis is an state-industry-year in columns 1 and 2, and a state-year in columns 3 and 4. The outcome is log median earnings in Panel A and the log 95th centile earnings in Panel B. The outcomes are lagged by four years as a placebo check. The panels estimate equation 9, instrumenting the sales-weighted log net-of-tax rate with the pre-TCJA sales-weighted share of C corps in each market. Columns 1 and 2 include market fixed effects, state-year fixed effects, and industry-year fixed effects, with standard errors multiway clustered by state and industry. Columns 3 and 4 include state and year fixed effects and are clustered by state. Columns 2 and 4 include time-varying controls for average worker age.

D Appendix to Section 5: Incidence

D.1 Alternate Assumptions About Capital Supply

The distributional incidence estimates reported in Section 5 provide a conservative lower bound for the short-run effects of the tax cuts on after-tax income dispersion. In the empirical analysis and the incidence quantification, lower income households do not see changes in wages but do benefit from increased corporate profits via their holdings of corporate equities (e.g., in 401K, pension, or retail investment accounts). The baseline assumption in this analysis, consistent with the benchmark modeling assumptions in Chodorow-Reich et al. (2025) and Barro and Furman (2018), is that capital supply is perfectly elastic, implying that tax-induced increases in after-tax profits do not reflect higher returns to capital broadly but rather accrue via increases in output and/or via firm-specific economic rents, such as those arising from proprietary technology, fixed factors, market power, regulatory advantages, or information frictions.

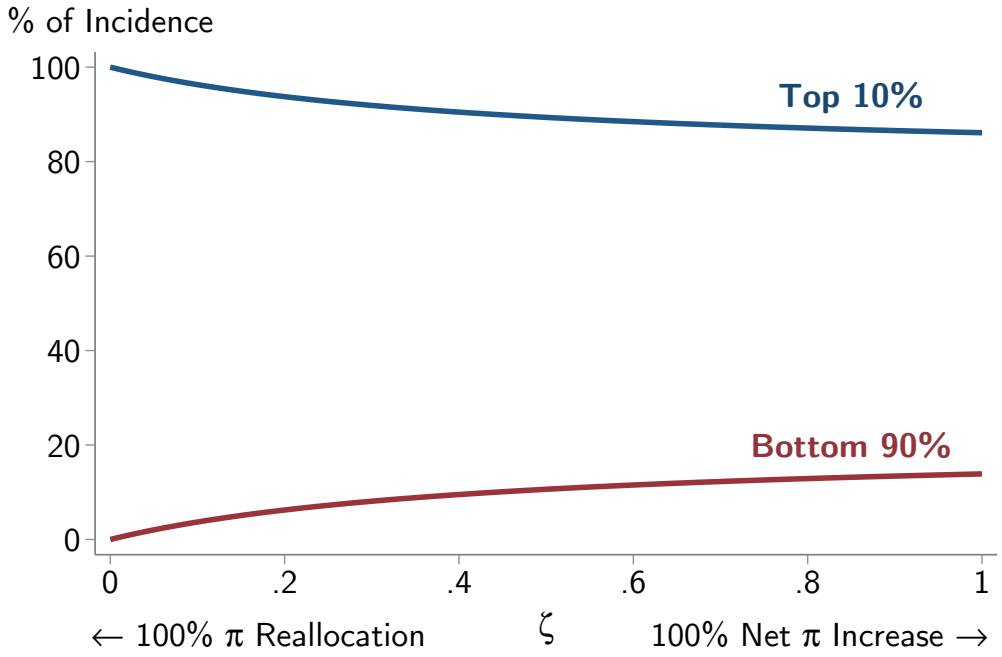
However, if capital supply is imperfectly elastic, this raises the possibility that the difference-in-difference (DiD) elasticity captures, in part, reallocation of profits among firm owners rather than net positive aggregate business income. In the extreme case where changes in after-tax profits reflect only reallocation of business income rather than aggregate net increases, the relevant market-level after-tax profit elasticity for incidence analysis is effectively zero. To assess how varying assumptions about profit reallocations affect our incidence estimates with respect to income, we use the following variation of equation 14:

$$I^{L_j} = \frac{dU^j + \omega^j \zeta dy^K}{\zeta dy^K + \sum_j dU^j}$$

where the introduction of the parameter ζ captures the share of after-tax profits from the DiD elasticity that represent net increases in capital income rather than reallocation. The case where $\zeta = 1$ corresponds to the benchmark assumption of perfectly elastic capital supply (or, not mutually exclusively, to the case where shareholders earn firm-specific economic rents, such as those arising from proprietary technology, fixed factors, market power, regulatory advantages, or information frictions). The extreme opposite case where $\zeta = 0$ corresponds to perfectly inelastic capital supply, such that all gains for shareholders of C corporations are offset by losses for shareholders of S corporation. The values in between represent intermediate cases.

Figure 20 illustrates how the distributional results in Table 10 Panel B vary with alternate assumptions about ζ. Relative to the benchmark case where $\zeta = 1$, the figure shows that alternate assumptions that generate lower gains for firm shareholders also indirectly generate lower gains for the households in the bottom 90% of the income distribution. Because lower-income households do not see wage gains from the tax cuts, they benefit only from their holdings of corporate equities. The benchmark case thus represents a conservative lower bound with respect to the paper's headline finding that approximately 87% of private income gains flow to the top 10% of the income distribution in the short run.

APPENDIX FIGURE 20: DISTRIBUTIONAL INCIDENCE UNDER ALTERNATE ASSUMPTIONS ABOUT CAPITAL SUPPLY

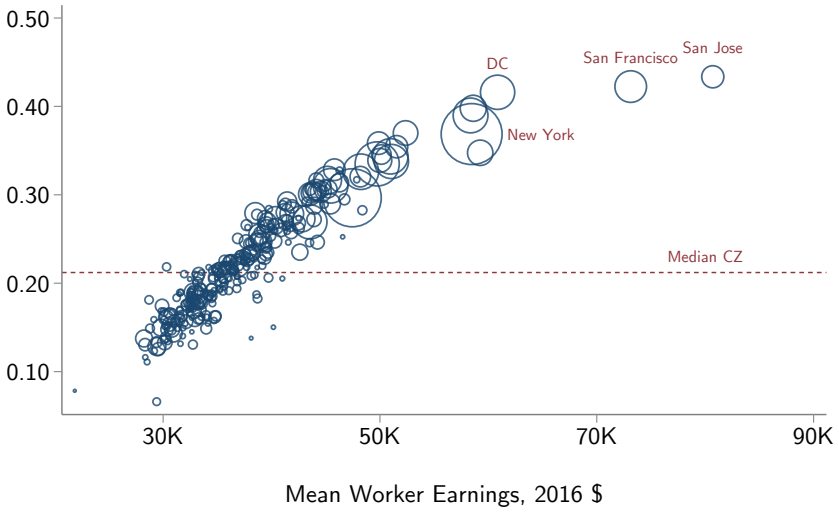


Notes: The figure illustrates how the distributional incidence results in Section 5 change with varying assumptions about the parameter ζ, which captures the share of after-tax profits that represent net increases in capital income rather than reallocation of capital income. See above for details.

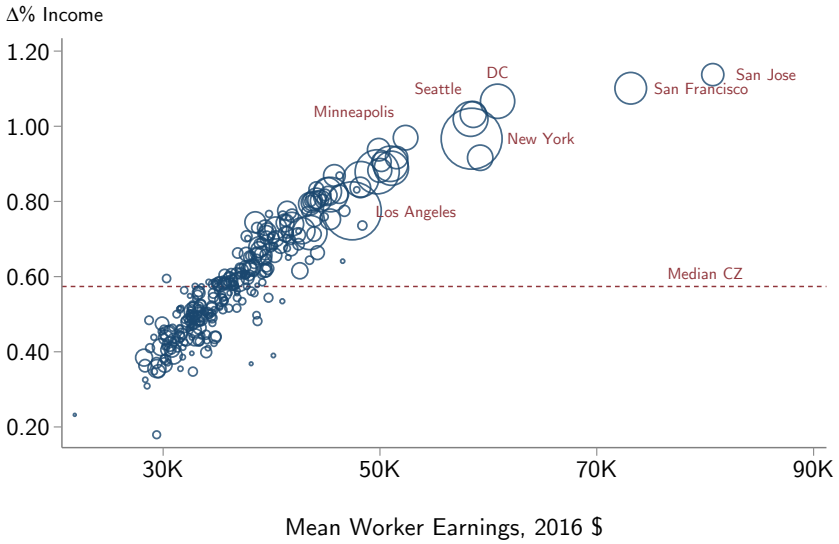
D.2 Geographic Incidence

Appendix Figure 21: Change in Per Capita Income vs. Initial Worker Earnings

(a) Main Analysis Sample



(b) Extrapolation Including Large Firms



Notes: The unit of analysis is a commuting zone. The figures plot the estimated percent change in income for each commuting zone against the 2016 average earnings of corporate-sector workers. The size of the bubbles is proportional to the 2016 population of each commuting zone. Panel A presents results for the main sample. The sample in Panel B includes large C corporations; see Appendix D.3.

D.3 Incidence Extrapolation to Large Firms

The incidence estimates reported in Table 10 are based on sample moments and elasticities estimated from the benchmark sample exlcuding the top 0.001% of firms. The very largest firms are exclusively C corporations, many of which are publicly traded and have multinational operations. Table 14 extrapolates the incidence results to these large firms. To do so, we re-estimate the moments from Table 9 Panels A and B using an SOI sample that includes these large firms. We otherwise use the same parameters as in Table 9 Panels C-E. A key difference vis-a-vis the benchmark estimates is that capital comprises a larger share of value added among the very largest firms (≈25%) relative to those in the main sample (≈16%). Relative to Table 10, extrapolating the results to large firms yields larger gains for capital relative to labor in response to corporate tax cuts, and a modestly larger share of incidence on foreigners.

APPENDIX TABLE 14: INCIDENCE INCLUDING LARGE C CORPS

	(1) **C Corps**	(2) **S Corps**	(3) **All Corps**	(4) **U.S. Pop.**
	% (se)	% (se)	% (se)	%
Panel A: By Factor				
Capital	73 (6)	80 (4)	75 (5)	n.a.
Executives	5 (1)	11 (2)	7 (1)	<1
High-Paid Workers	22 (4)	9 (2)	18 (3)	10
Low-Paid Workers	0 (3)	0 (2)	0 (3)	90
Panel B: By Income				
Top 1%	35 (2)	64 (2)	41 (2)	1
91-99th%	50 (3)	25 (1)	44 (2)	9
Bottom 90%	15 (3)	10 (3)	15 (2)	90
Panel C: By Geography				
Foreign	25 (2.0)	0 (0.0)	13 (0.9)	n.a.
Domestic	75 (2.0)	100 (0.0)	87 (0.9)	100
Of which:				
Northeast	29 (0.3)	35 (0.3)	33 (0.2)	19
Midwest	23 (0.2)	23 (0)	22 (0.2)	21
South	21 (0.3)	19 (0.1)	19 (0.1)	36
West	28 (0.2)	23 (0.1)	26 (0.1)	24

Notes: The table estimates incidence from an SOI sample that includes very large C corporations. The moments from Table 9 Panels A and B are re-estimated using the expanded sample. The parameters from Table 9 Panels C-E are unchanged.