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# Corporate Tax Cuts, Firm Growth, and Workers' Earnings[†]

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*We study the effects of the largest corporate income tax cut in US 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 preexisting 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 percent of private income gains flow to the top 10 percent of the income distribution.* (*JEL* D22, D31, G31, H25, H32, J23, L25)

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 US 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 1980s, policymakers and researchers today continue to debate the costs and benefits of declining corporate tax burdens. Advocates for tax cuts argue

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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 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 firm known as C corporations (C corps) from 35 percent to 21 percent (a 40 percent reduction). At the same time, TCJA cut the implied top marginal tax rate for a distinct legal entity type of firm known as S corporations (S corps) from 39.6 percent to 37 percent (about a 7 percent reduction) and also introduced a new tax deduction that, for some of these firms, further reduced the top marginal rate to 29.6 percent (for a cumulative 25 percent 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 the 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 (SE $=$ 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 forgone 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 percent of the within-firm distribution but do increase for C corp workers in the top 10 percent, 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 the sample 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 percent of gains flow to firm owners, 8 percent flow to executives, 32 percent flow to high-paid workers, and 0 percent 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 percent of the gains from tax cuts accrue to the top 10 percent of earners, and 13 percent of gains flow to the bottom 90 percent. 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, Hassett, and Hubbard 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, Ohrn, and Suárez Serrato 2020; Giroud and Rauh 2019; Fuest, Peichl, and Siegloch 2018; Suárez Serrato and Zidar 2016; Heiser, Love, and Mortenson 2025), industry-level variation in exposure to federal tax deductions or credits (Curtis et al. 2021; Ohrn 2023, 2018; Dobridge, Landefeld,

and Mortenson forthcoming; 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, Malgouyres, Py, and Urvoy 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 US 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 studied in the literature—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 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, Peichl, and Siegloch 2018; Suárez Serrato and Zidar 2016). Methodologically, the large shock facilitates an unusually transparent event-study design. Conceptually, the magnitude of the tax cut is relevant on theoretical grounds under the conventional view that economic distortions are nonlinear (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 magnitude 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, Malgouyres, Py, and Urvoy 2022; Dobridge, Landefeld, and Mortenson forthcoming; Fuest, Peichl, and Siegloch 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 is more limited, in contrast to studies that do not observe firms' profits (e.g., Fuest, Peichl, and Siegloch 2018; Suárez Serrato and Zidar 2016). We further show how incidence varies across US regions and contributes to geographic disparities.

Third, we contribute to the literature on TCJA and its effects on the US economy. Researchers have studied impacts on macroeconomic performance (Gale and Haldeman 2021; Barro and Furman 2018), international and intertemporal profit shifting (Clausing 2020; Dowd, Giosa, and Willingham 2020), pass-through businesses (Goodman, Lim, Sacerdote, and Whitten 2025), executive compensation (De Simone, McClure, and Stomberg 2022), and capital structures (Carrizosa, Gaertner, and Lynch 2023). This 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, Smith, Zidar, and Zwick (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, Malgouyres, Py, and Urvoy 2022; Bachas and Soto 2021; Risch 2024; Hendren and Sprung-Keyser 2020; Fuest, Peichl, and Siegloch 2018; Suárez Serrato and Zidar 2016). With respect to efficiency, the 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, the 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 suggest that policymakers face an equity-efficiency trade-off 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.

## I. The Tax Cuts and Jobs Act

### A. *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.

### B. *C versus 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 percent of total US employment and 74 percent of total payrolls, with government, nonprofits, and noncorporate 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. 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 8 income brackets and a top rate of 35 percent. After TCJA, these brackets collapsed to a uniform 21 percent rate. Supplemental 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. Supplemental Appendix A.2 details the collapse of the progressive tax brackets following TCJA. Supplemental Appendix A.3 puts the US corporate tax cuts in a global perspective. Supplemental 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 US 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 percent. 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 percent to 37 percent. Second, it introduced a 20 percent 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 percent to 29.6 percent. 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 Supplemental 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 Supplemental Appendix A.6 that endogenous switching is not a concern for the analysis.

### C. *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 in Section II.

## II. 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 Supplemental Appendix B.

### A. *Tax Data*

*Corporate Returns*.—We study firms in the corporate Statistics of Income (SOI) files produced by the US 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, Kennedy, Landefeld, and Mortenson 2025) as well as the largest firms in the top 0.001 percent 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 percent (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 the sample period (excluding only 3 percent of firms that collectively account for less than 0.5 percent of aggregate

[1] Evaluating outcomes prior to 2020 circumvents pandemic-related issues affecting the internal and external validity of the research design in later years; see Supplemental 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, Goldberg, Kennedy, and Khandelwal 2020; Chodorow-Reich, Smith, Zidar, and Zwick 2025).

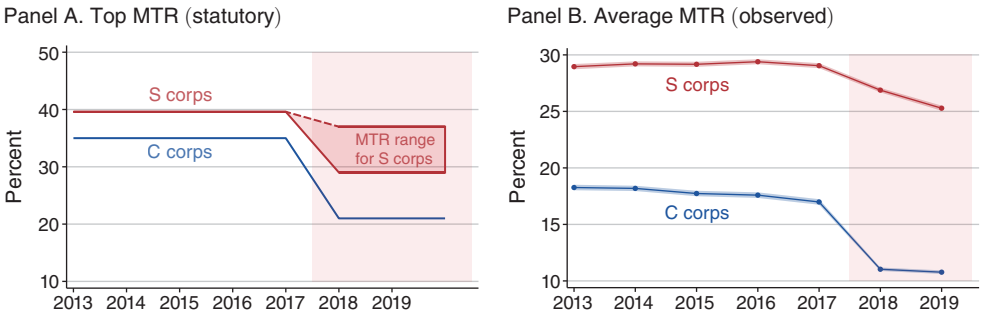


FIGURE 1. MARGINAL INCOME TAX RATES FOR C AND S CORPS

*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 II and Supplemental Appendix B for additional data details.

corporate sales or profits; see Supplemental Appendix A.6 for details). Firms in our sampling range represent approximately the eightieth to the 99.999th percentile of the aggregate firm sales distribution and collectively accounted for more than half of corporate employment in 2016.

*Individual Data*.—We merge the sample of corporate tax returns with the universe of worker-level filings of IRS Form W-2 (Joint Committee on Taxation 2022). We also collect information about the owners of S corps in the sample from the universe of filings of IRS Forms 1040 and 1099-K1.

### B. *Variable Definitions and Measurement*

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 Supplemental Appendix B.

*Marginal Tax Rates*.—For C corps, we observe taxable income and infer marginal tax rates using the federal corporate income tax schedule reproduced in Supplemental 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 Supplemental 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 2 owners who receive an equal share of that firm's business income, facing observed marginal tax rates on their individual income of 25 percent and 35 percent, respectively, then we compute the firm's implied marginal tax rate as $(0.5 * 0.25) + (0.5 * 0.35) = 30\%$. Supplemental 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 the 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. Supplemental Appendix B provides additional details about these computations.

*Sales*, *Costs*, *and Profits*.—Sales are gross receipts. Pretax 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 nonnegative 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 (Joint Committee on Taxation 2022); see Supplemental 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 the 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 percent 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 IIID 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.

### C. *Descriptive Statistics*

Panels A and B of Figure 2 show the unweighted distributions of log firm employment and log firm sales in the 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 percent. The size, industry, and joint distributions of these variables after reweighting are shown in Supplemental Appendix B.4, along with raw data trends for key outcomes.

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.

## III. 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

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

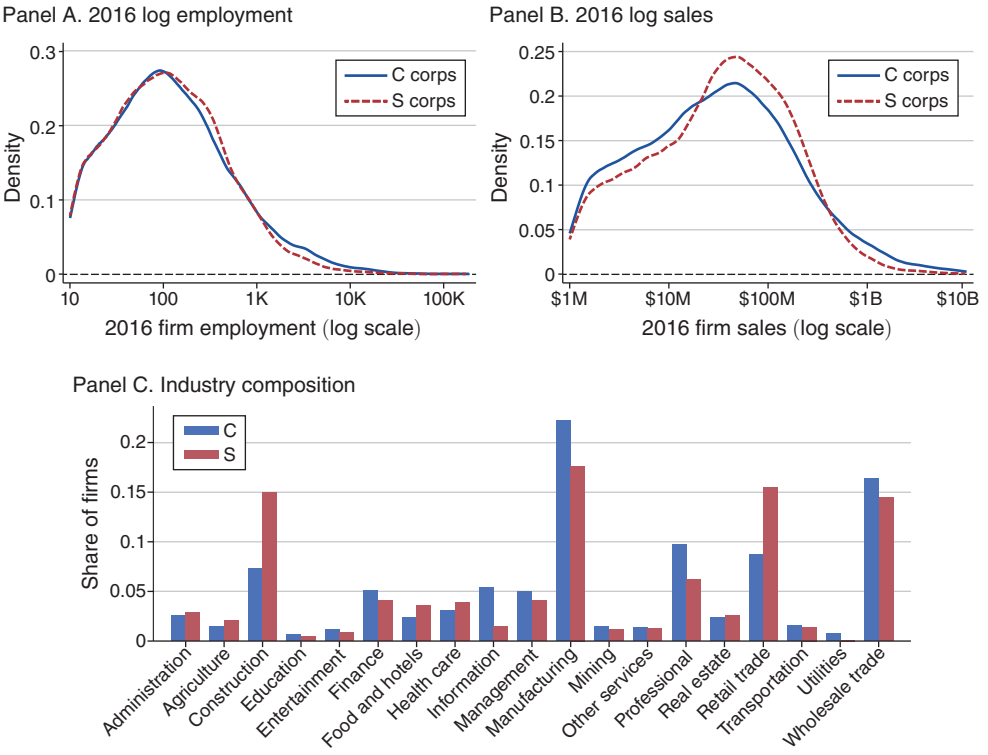


FIGURE 2. FIRM SIZE DISTRIBUTIONS AND INDUSTRY COMPOSITION

*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 Bureau (2017). The reweighted distributions are shown in Supplemental Appendix B.4.

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, $\gamma_f$ is a firm fixed effect, and $\alpha_{is(f),t}$ is an industry-size-year fixed effect. The coefficients $\beta_t$ 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.

The identifying assumptions permitting a causal interpretation of the $\beta_t$ 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

Table 1—Summary Statistics

| | All firms | | C corporations | | | S corporations | | |
|---|---|---|---|---|---|---|---|---|
| | Mean (1) | SD (2) | Mean (3) | SD (4) | p50 (5) | Mean (6) | SD (7) | p50 (8) |
| *Taxes* | | | | | | | | |
| Marginal tax rate | 0.229 | 0.166 | 0.176 | 0.170 | 0.150 | 0.294 | 0.136 | 0.364 |
| Federal tax (million) | 0.917 | 1.620 | 0.846 | 1.750 | 0.023 | 1.003 | 1.442 | 0.267 |
| Federal tax per worker | 6,884 | 13,457 | 4,640 | 8,695 | 285 | 9,612 | 17,198 | 2,598 |
| *Sales and profits* (*millions*) | | | | | | | | |
| Sales | 136.6 | 471.2 | 160.3 | 572.2 | 30.6 | 107.8 | 304.1 | 36.6 |
| Costs | 140.6 | 509.6 | 170.3 | 631.3 | 34.6 | 104.5 | 297.5 | 34.8 |
| Operating profit | 47.6 | 213.0 | 60.9 | 265.7 | 10.5 | 31.4 | 119.2 | 10.6 |
| After-tax op. profit | 46.7 | 212.6 | 60.0 | 265.2 | 10.0 | 30.4 | 118.9 | 9.8 |
| Taxable income | 2.8 | 8.4 | 1.7 | 9.9 | 0.3 | 4.2 | 5.8 | 1.7 |
| After-tax net income | 1.9 | 7.2 | 0.9 | 8.6 | 0.2 | 3.2 | 4.8 | 1.2 |
| *Shareholder payouts* (*millions*) | | | | | | | | |
| Dividends | 2.9 | 19.6 | 1.2 | 18.3 | 0.0 | 5.0 | 21.0 | 1.0 |
| Share buybacks | 0.1 | 5.1 | 0.1 | 6.2 | 0.0 | 0.1 | 3.0 | 0.0 |
| Total payouts | 3.2 | 21.2 | 1.4 | 20.1 | 0.0 | 5.4 | 22.3 | 1.0 |
| *Capital and investment* (*capital in millions*) | | | | | | | | |
| Capital | 27.1 | 295.8 | 39.0 | 390.3 | 3.0 | 12.8 | 91.4 | 1.8 |
| Net investment | 1.6 | 90.7 | 2.5 | 121.4 | −0.0 | 0.5 | 11.5 | −0.0 |
| Net investment/lagged capital | 0.07 | 0.34 | 0.08 | 0.36 | −0.02 | 0.06 | 0.32 | −0.02 |
| *Payroll and employment* (*payroll in millions*) | | | | | | | | |
| Payroll (1120) | 25.2 | 119.2 | 31.1 | 144.0 | 6.5 | 18.0 | 78.7 | 5.9 |
| Payroll (W-2) | 23.4 | 105.0 | 28.7 | 128.1 | 6.3 | 16.9 | 66.5 | 5.7 |
| Employment | 472 | 2,817 | 544 | 3,174 | 106 | 385 | 2,306 | 106 |
| *Worker earnings and characteristics* (*earnings in thousands*) | | | | | | | | |
| Mean annual earnings | 63.2 | 59.6 | 66.8 | 58.9 | 55.1 | 58.7 | 60.2 | 50.0 |
| Median annual earnings | 46.6 | 29.4 | 50.1 | 32.4 | 42.9 | 42.3 | 24.8 | 38.8 |
| Ninety-fifth centile earnings | 170.7 | 282.1 | 178.0 | 270.5 | 131.6 | 161.9 | 295.4 | 118.6 |
| Mean top 5 earnings | 375 | 967 | 393 | 1,028 | 218 | 352 | 887 | 197 |
| Female share | 0.35 | 0.23 | 0.36 | 0.22 | 0.32 | 0.35 | 0.24 | 0.28 |
| Worker age | 42.1 | 5.1 | 42.5 | 5.2 | 42.9 | 41.7 | 5.1 | 41.9 |
| *Firm characteristics* | | | | | | | | |
| Firm age | 28.0 | 21.3 | 26.6 | 22.1 | 22.3 | 29.8 | 20.2 | 26.8 |
| 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 II and Supplemental Appendix B.

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 to 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 Supplemental 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. Last, in Section IIID, 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

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

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

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

where $\ln\left(1 - \tau_{ft}\right)$ 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 preexisting 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, Hansen, and Rossi 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 pretax profits all increase substantially in response to the tax cuts. Third, we present evidence on 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.

### A. *First Stage*: *Marginal Tax Rates and Taxes Paid*

Figure 3 plots the $\beta_t$ coefficients and 95 percent 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-differences coefficient and standard error from equation (2).

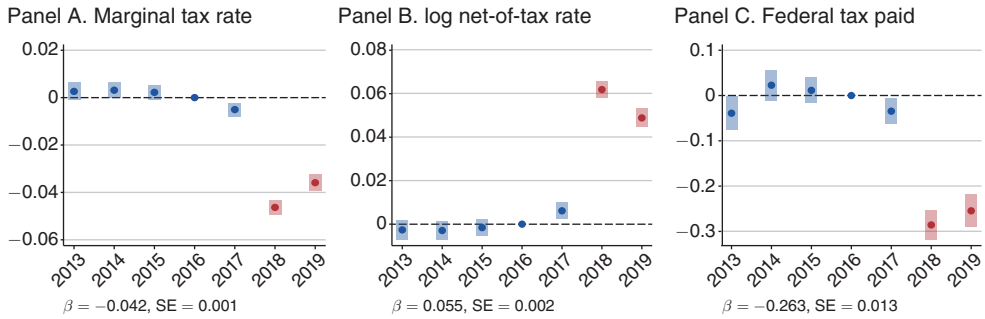


FIGURE 3. EVENT STUDIES: FIRST STAGE

*Notes*: The unit of analysis is a firm-year. The panels plot the $\beta_t$ 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 percent 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, $\tau_f^{MTR}$, measured as a rate in levels (0–1). The outcome in panel B is the log net-of-tax rate, $\ln\left(1 - \tau_f^{MTR}\right)$. 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 II and Supplemental Appendix B for data and measurement details.

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 (SE = 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 Supplemental Appendix C.3.

Panel B of Figure 3 shows an analogous version of panel A where the outcome is transformed as the log net-of-tax rate, $\ln\left(1 - \tau_{ft}\right)$. On average, C corps saw their net-of-tax rate increase by approximately 5.5 percent (SE = 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 (SE = 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.

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

Table 2—Marginal Tax Rates and Taxes Paid

| | $\tau_{ft}^{MTR}$ (1) | $\ln(1-\tau_{ft}^{MTR})$ (2) | Tax paid (3) | Tax per worker (4) |
|---|---|---|---|---|
| $C_f \times \text{Post}_t$ | −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 fixed effects | Yes | Yes | Yes | Yes |
| Industry-size-year fixed effects | Yes | Yes | Yes | Yes |
| Observations | 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, $\tau_f^{MTR}$, and the outcome in column 2 is the log net-of-tax rate, $\ln(1-\tau_{ft}^{MTR})$. 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 II 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.

versus S corps. These results also indicate that the relevance assumption underlying the instrumental variables strategy is satisfied. Supplemental 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.

### B. *Evidence of Real Distortions*: *Capital*, *Labor*, *Output*, *and Pretax 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 $\beta_t$ coefficients from equation (1) to assess relative trends in log capital and net investment 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 (SE = 0.8). Panel B shows a 2.6 percent (SE = 0.4) increase in their net investment rate.

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

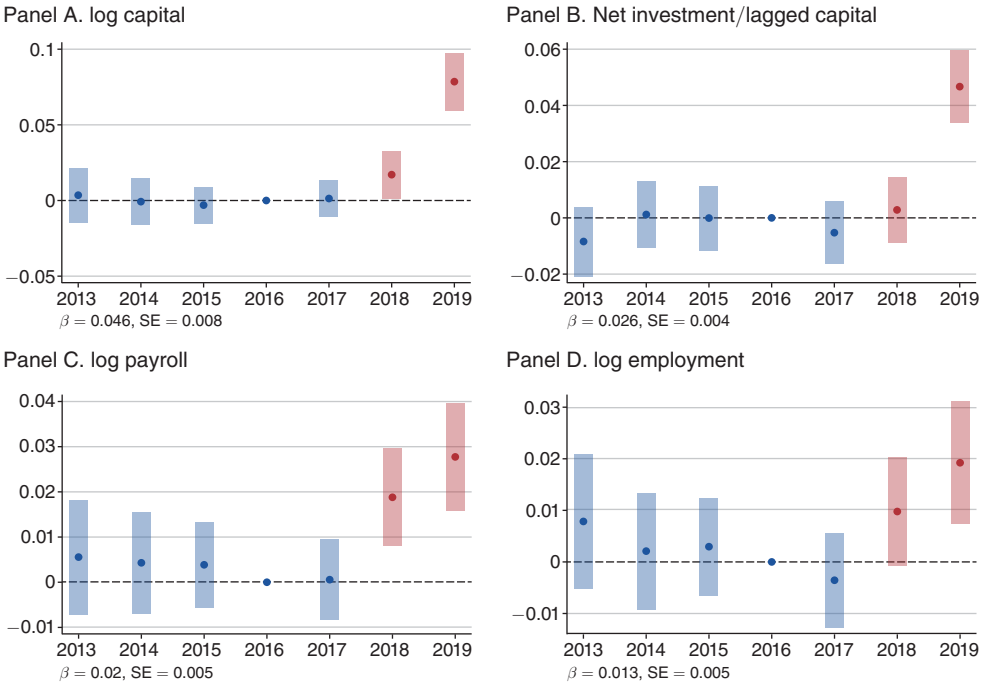


FIGURE 4. EVENT STUDIES: CAPITAL INVESTMENT AND LABOR DEMAND

*Notes*: The unit of analysis is a firm-year. The panels plot the $\beta_t$ 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 percent confidence intervals. The corresponding DiD coefficient and standard error from equation (2) are shown in the bottom left of each panel. See Section II and Supplemental Appendix B for data and measurement details.

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 (SE = 0.14) and an investment rate semielasticity of 0.45 (SE = 0.07).

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 IIIE, 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 percent (SE = 0.5) on average. Employment, shown in panel D, also trended upward, by 1.3 percent (SE = 0.5).

These difference-in-differences 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 (SE = 0.09) and an employment elasticity of 0.23 (SE = 0.08). The increases in payrolls and employment imply that the tax cuts caused firms to increase their labor demand. In Section IIIC we assess the distributional effects of increased labor demand on different types of workers, and in Section IIIF we estimate general equilibrium effects on employment and workers' earnings.

*Sales*, *Costs*, *and Pretax 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 pretax 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 percent (SE = 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 percent, SE = 0.4) is smaller than for sales. Given sharply increasing sales and modestly increasing costs, panel C shows that the average pretax operating profits of C corps also increased relative to S corps, by 2.3 percent (SE = 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 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 (SE = 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]

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 US 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, Slemrod, and Giertz (2012)

[2] Supplemental 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. Supplemental Appendix C.7 reports robustness checks showing that the main estimates are robust to excluding 2017 and 2018 from the sample. Supplemental 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.

[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.

TABLE 3—CAPITAL, LABOR, OUTPUT, AND PRETAX PROFITS

| | $\ln(K_{ft})$ (1) | $\ln(K_{ft})$ (2) | $I_{ft}/K_{f,t-1}$ (3) | $I_{ft}/K_{f,t-1}$ (4) | | |
|---|---|---|---|---|---|---|
| *Panel A. Capital and investment* | | | | | | |
| $C_f \times Post_t$ | 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 fixed effects | Yes | Yes | Yes | Yes | | |
| Industry-size-year fixed effects | Yes | Yes | Yes | Yes | | |
| First-stage $F$ | | 1,308 | | 1,245 | | |
| Observations | 231,360 | 231,360 | 204,411 | 204,411 | | |
| $N$ firms | 49,235 | 49,235 | 42,711 | 42,711 | | |
| | $\ln(\overline{w}_{ft}L_{ft})$ (1) | $\ln(\overline{w}_{ft}L_{ft})$ (2) | $\ln(L_{ft})$ (3) | $\ln(L_{ft})$ (4) | | |
| *Panel B. Payroll and employment* | | | | | | |
| $C_f \times Post_t$ | 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 fixed effects | Yes | Yes | Yes | Yes | | |
| Industry-size-year fixed effects | Yes | Yes | Yes | Yes | | |
| First-stage $F$ | | 1,308 | | 1,308 | | |
| Observations | 231,360 | 231,360 | 231,360 | 231,360 | | |
| $N$ firms | 49,235 | 49,235 | 49,235 | 49,235 | | |
| | ln(Sales) (1) | ln(Costs) (2) | ln(O.Pft) (3) | ln(Tax Inc) (4) | **1**(Tax Inc>0) (5) | ln(Tax Inc) (6) |
| *Panel C. Sales, costs, and pretax profits* | | | | | | |
| $C_f \times Post_t$ | 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 fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry-size-year fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| First-stage $F$ | | | | | | 3,093 |
| Observations | 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, pretax operating profits, and the intensive and extensive margins of taxable income. Column 6 reports the elasticity of taxable income. Nonpositive 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 II and Supplemental Appendix B for data and measurement details.

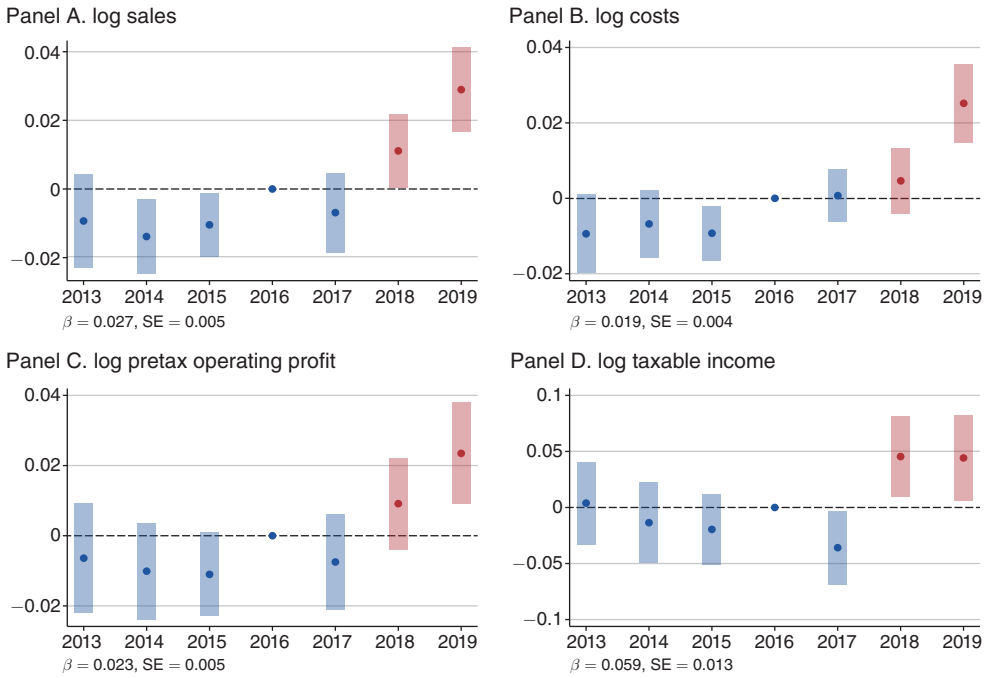


FIGURE 5. EVENT STUDIES: FIRM SALES, COSTS, AND PRETAX PROFITS

*Notes*: The unit of analysis is firm-year. The panels plot the $\beta_t$ 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 percent 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 Supplemental Appendix C.3. See Section II and Supplemental Appendix B for data and measurement details.

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 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.

## C. *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

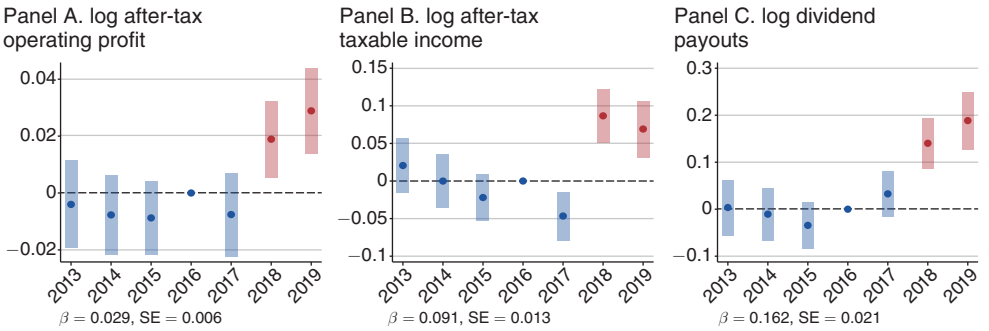


FIGURE 6. EVENT STUDIES: AFTER-TAX PROFITS AND DIVIDEND PAYOUTS

*Notes*: The unit of analysis is a firm-year. The panels plot the $\beta_t$ 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 percent 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 Supplemental Appendix C.3. See Section II and Supplemental Appendix B for data and measurement details.

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, 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 percent (SE = 0.6) and 9.1 percent (SE = 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 percent (SE = 2.1).

Table 4 presents the difference-in-differences 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 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 (SE = 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.

[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 percent of aggregate payouts. Supplemental Appendix C.4 reports that C corps also increased buybacks.

TABLE 4—AFTER-TAX PROFITS AND DIVIDEND PAYOUTS

| | ln(Dividends) (1) | **1**(Dividends>0) (2) | ln((1 − $\tau_{ft}$)O.Pft) (3) | ln((1 − $\tau_{ft}$)$\pi$) (4) | **1**((1 − $\tau_{ft}$)$\pi$ >0) (5) | ln((1 − $\tau_{ft}$)$\pi$) (6) |
|---|---|---|---|---|---|---|
| $C_f \times Post_t$ | 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 |
| Observations | 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). Nonpositive 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 II and Supplemental Appendix B. For analyses of negative profits and extensive margin responses, see Supplemental 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 percent (SE = 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 percent, SE = 0.2).[5] By contrast, panel C shows that the earnings of C corp workers at the ninety-fifth centile increased relative to their counterparts in S corps (1.2 percent, SE = 0.3), and panel D indicates that earnings of executives (as proxied by the top 5 highest-paid workers in the firm) increased even more sharply, by 2.6 percent (SE = 0.4).

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 ninety-fifth percentile of log worker earnings as the outcome, as in panel C.[6]

[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 the industry-size-year fixed effects. We evaluate the general equilibrium effects of the tax cuts on workers' earnings in Section IIIF.

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

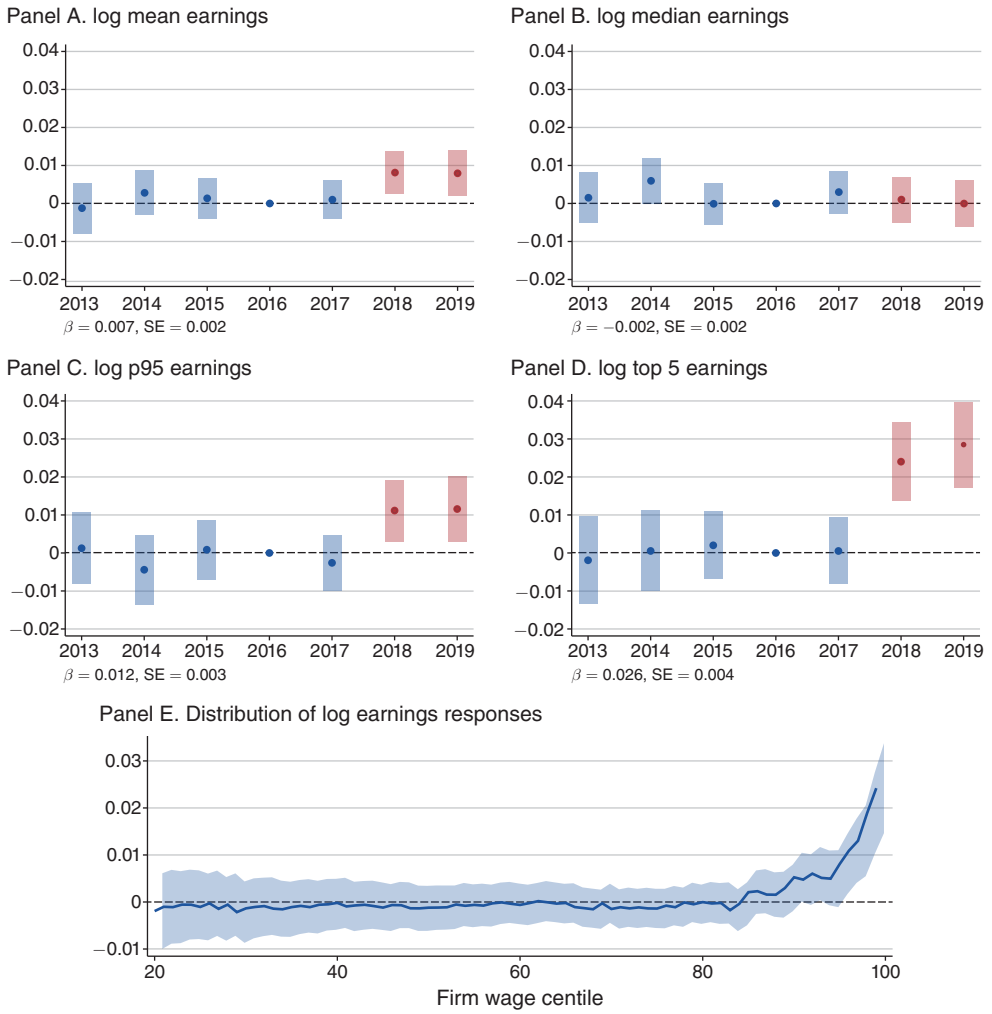


FIGURE 7. EVENT STUDIES: WORKERS' EARNINGS

*Notes*: The unit of analysis is a firm-year. Panels A–D plot the $\beta_t$ 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 percent 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 $\beta$ 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 ninetieth percentile within the firm. We exclude the bottom 20 percent 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 percent confidence intervals. See Section II and Supplemental Appendix B for data and measurement details.

Panel E of Figure 7 plots the $C \times Post$ coefficients from these regressions along with their corresponding 95 percent confidence intervals. The figure shows that the relative earnings of workers in C and S corps below the ninetieth 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 percent of the earnings distribution. On average, workers

at the ninetieth percentile of the within-firm distribution see their relative earnings change by 0.7 percent (SE = 0.3), and these impacts grow quantitatively larger further up the distribution. At the ninety-fifth percentile, we estimate a relative earnings increase of 1.2 percent (SE = 0.3) for C corp workers, and this magnitude climbs to 2.3 percent (SE = 0.5) at the ninety-ninth 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 ninety-fifth percentile, we estimate an earnings elasticity of 0.22 (SE = 0.06), and for executives, we estimate a larger earnings elasticity of 0.47 (SE = 0.08). The mean baseline earnings of these workers and executives are high: The average worker in the sample at the ninety-fifth percentile of the within-firm distribution earns $170,740 per year, and the average worker in the top 5 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 ninety-fifth percentile increased by approximately $2,100, while relative gains for workers below the ninetieth percentile are statistically indistinguishable from 0.

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-à-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 percent declines to 2.0 percent when controlling for sales 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.

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"

Table 5—Workers' Earnings

| | $\ln(\overline{w})$ (1) | $\ln(w^{50})$ (2) | $\ln(w^{95})$ (3) | $\ln(w^{exec})$ (4) |
|---|---|---|---|---|
| *Panel A. All workers* | | | | |
| $C_f \times Post_t$ | 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 |
| $\varepsilon$ | 0.130 | −0.027 | 0.224 | 0.466 |
| SE | 0.040 | 0.039 | 0.057 | 0.079 |
| Firm fixed effects | Yes | Yes | Yes | Yes |
| Industry-size-year fixed effects | Yes | Yes | Yes | Yes |
| Observations | 231,360 | 231,360 | 231,360 | 231,360 |
| *N* firms | 49,235 | 49,235 | 49,235 | 49,235 |

| | $\ln(w^{exec})$ (1) | $\ln(w^{exec})$ (2) | $\ln(w^{exec})$ (3) | $\ln(w^{exec})$ (4) |
|---|---|---|---|---|
| *Panel B. Executive pay* | | | | |
| $C_f \times Post_t$ | 0.026 | 0.020 | 0.022 | 0.026 |
| | (0.004) | (0.004) | (0.004) | (0.004) |
| Firm fixed effects | Yes | Yes | Yes | Yes |
| Industry-size-year fixed effects | Yes | Yes | Yes | Yes |
| Controls $X_{ft}$ | None | Sales | Profit | Rel. sales |
| Observations | 231,360 | 231,360 | 231,360 | 231,360 |
| *N* firms | 49,235 | 49,235 | 49,235 | 49,235 |

| | Incumbents | | New hires | |
|---|---|---|---|---|
| | $\ln(w^{50})$ (1) | $\ln(w^{95})$ (2) | $\ln(w^{50})$ (3) | $\ln(w^{95})$ (4) |
| *Panel C. Incumbents and new hires* | | | | |
| $C_f \times Post_t$ | 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 fixed effects | Yes | Yes | Yes | Yes |
| Industry-size-year fixed effects | Yes | Yes | Yes | Yes |
| Observations | 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, ninety-fifth 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 nonpositive. Panel C estimates equation (2) separately for incumbents in columns 1 and 2 (defined as "stayers," who are employed at the same firm over the entire sample period) and new hires in columns 3 and 4. The outcomes are log median earnings and log ninety-fifth 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.

who are employed at the same firm in every year of the sample (e.g., as in Kline, Petkova, Williams, and Zidar 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 posttreatment 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, Malgouyres, Py, and Urvoy 2022; Dobridge, Landefeld, and Mortenson forthcoming; Kline, Petkova, Williams, and Zidar 2019) and with a growing literature emphasizing that labor market frictions may generate firm-specific pay premia or wage markdowns (Card 2022). The 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, Peichl, and Siegloch (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 IV.

### D. *Robustness*, *Heterogeneity*, *and Additional Results*

*Alternate Specifications*.—Supplemental Appendix C.6 reports results from alternate modeling specifications, where we include controls for state-by-year fixed effects, six-digit NAICS industry-by-year fixed effects, firm age-by-year fixed effects, and preexisting 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*.—Supplemental 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 US-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. The results are robust across the alternate samples.

[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, Peichl, and Siegloch (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, Peichl, and Siegloch (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 Supplemental Appendix A.7. While we cannot rule out modest interactions, it is unlikely that other provisions of TCJA are main drivers of the results for four reasons. First, the difference-in-differences 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 Supplemental Appendix A.7). Third, Supplemental 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 IIIE.

*Accounting for Temporary Policies and Expectations*.—Supplemental 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, Supplemental Appendix C.8 shows that the net-of-tax elasticities remain close to the benchmark results across a broad range of assumptions respecting forward-looking behavior and adjustment costs. In practice, tax changes in 2026 were too distant in the future and too uncertain to meaningfully affect firm 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.[10] We report additional heterogeneity tests in Supplemental Appendix C.9 and explore the user cost mechanism in greater detail below.

*Evaluating Results at a Longer Time Horizon*.—Supplemental Appendix C.10 examines how the COVID-19 pandemic and related policy responses affect the

[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.

[10] Liquidity constraints can generate differential payroll responses across firm sizes without differential investment responses if, for example, capital purchases can be collateralized while payroll must be financed using internal funds. The stronger payroll elasticity among capital-intensive firms is consistent with greater capital-labor complementarity in their production technology.

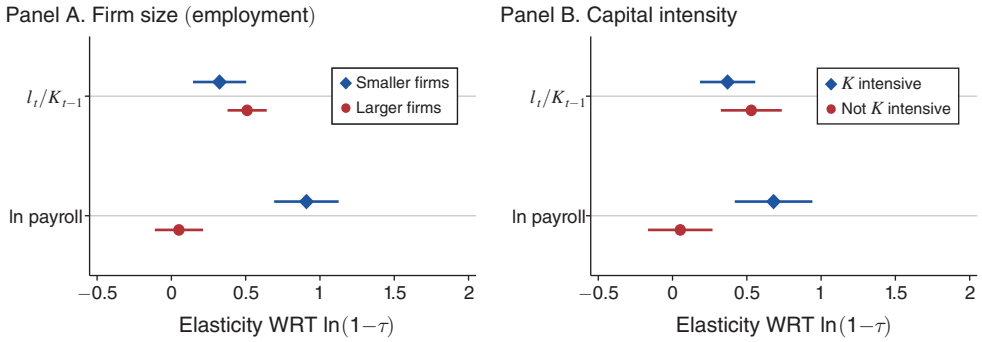


FIGURE 8. FIRM HETEROGENEITY

*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 percent confidence intervals. Supplemental Appendix C.9 reports heterogeneity tests for additional outcomes.

internal and external validity of the research design beginning in 2020. Federal pandemic policy differentially affected C and S corps, confounding causal intepretations. Capital, pretax profits, and after-tax profits of C corps remain elevated relative to S corps five years after TCJA, while payroll and wage differences are attenuated; this likely reflects the impact of the federal Paycheck Protection Program, which disproportionately benefited S corps.

## E. *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 $\pi$:

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

where $\tau^c$ 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 nonwage 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 nondeductible 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 $\phi$. Either decreasing the tax rate

$\tau$ or increasing the expensing parameter $\theta$ lowers the user cost $\phi$. 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 US tax system as follows:

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

where $r^d$ and $r^e$ are the cost of debt and equity, respectively; $\alpha$ and $\left(1 - \alpha\right)$ are the share of debt versus 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 $k$; $\lambda$ is the effective share of capital investments eligible for bonus; $\tau^c$ is the marginal income tax rate (adjusted for operating loss deductions and, in the case of S corps, the qualified business income deduction); $\tau^d$ is the marginal tax shield from debt; $\rho$ is the inflation rate; and $\delta$ 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. Additional measurement details are provided in Supplemental 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 $\alpha_{ft}$ is the ratio of debt to market value, with market values estimated as in Smith, Zidar, and Zwick (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 $\lambda$ accounts for incomplete take-up of bonus (Kitchen and Knittel 2016). The annual inflation rate $\rho$ is set to 1.5 percent, the average over the sample period (BLS 2024). The depreciation rate $\delta_{isj}$ is depreciation scaled by lagged capital. $\tau^c_{ft}$ is the top statutory income tax rate on business income adjusted for applicable deductions (see Supplemental Appendix B for details). The marginal tax shield from debt $\tau^d_{ft}$ is equal to $\tau^c_{ft}$ before TCJA and afterward is equal to $\tau^c_{f,t} \cdot \min\left(c_{f,t}, 0.3 \cdot \max\left(0, Y_{f,t}\right)\right)/c_{f,t}$, 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 $\phi$ and the after-tax return $r$, where $r$ is the weighted average return on debt and equities, scaled by the user cost:

$$(8) \quad \tau^{EMTR}_{ft} = \frac{\phi_{ft} - r_{ft}}{\phi_{ft}}.$$

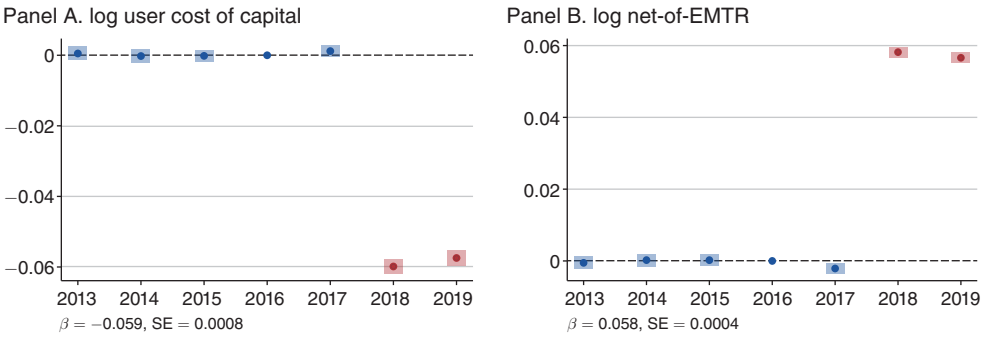


FIGURE 9. USER COST OF CAPITAL AND EFFECTIVE MARGINAL TAX RATES

*Notes*: The unit of analysis is a firm-year. The panels plot the $\beta_t$ 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 percent confidence intervals. The corresponding DiD coefficient and standard error are shown in the bottom left of each panel. See Section IIIE and Supplemental Appendix B for measurement details.

TABLE 6—ELASTICITIES TO TAX RATES AND USER COSTS

| | $I_t/K_{t-1}$ (1) | ln(Payroll) (2) | $\ln(w^{50})$ (3) | $\ln(w^{95})$ (4) | $\ln(w^{exec})$ (5) | ln(TI) (6) | ln(After-Tax $\pi$) (7) |
|---|---|---|---|---|---|---|---|
| *Panel A. Elasticities with respect to the log net-of-tax rate (benchmark)* | | | | | | | |
| $\ln(1-\tau_{f,t}^{MTR})$ | 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* | | | | | | | |
| $\ln(1-\tau_{f,t}^{EMTR})$ | 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* | | | | | | | |
| $\ln(\Phi_{f,t})$ | −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 fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry-size-year fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 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. Nonpositive observations are excluded from the regressions when the outcome is logged. The first-stage $F$-statistics (not shown) are all greater than 1,000. See Section IIIE for measurement details.

Figure 9 plots the coefficients from equation (2), where the outcomes are the log user cost and log $(1-\tau_{ft}^{EMTR})$. User costs declined by −5.9 percent (SE = 0.1) for C corps relative to S corps after TCJA, and the net-of-tax EMTR increased by 5.8 percent (SE = 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 this setting the benchmark elasticities approximate those accounting for comprehensive features of the reform.[11]

### F. *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 $\varepsilon^m$ using variations of the following specification:

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

instrumented with

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

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 Supplemental Appendix C.12.

[11] Note the empirically observed marginal tax rates in panel A differ 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 Supplemental Appendix C.11 for additional results on the role of expensing; a calibration suggests around 70 percent of the reduction in the user cost tax term for C corps is attributable to the change in the tax rate.

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 ninety-fifth 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 fixed effects | Yes | Yes | Yes | Yes |
| State-year fixed effects | Yes | Yes | No | No |
| Ind-year fixed effects | Yes | Yes | No | No |
| Year fixed effects | No | No | Yes | Yes |
| Controls | No | Yes | No | Yes |
| First-Stage $F$ | 26 | 27 | 24 | 29 |
| Observations | 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 ninety-fifth 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.

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 include demographic controls for mean worker wage and share of female workers. 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 0; averaging the coefficients in columns 1–4 yields an elasticity of −0.0002. Panel B reports results for ninety-fifth centile earnings, where similarly averaging yields an elasticity of 0.0475. The confidence intervals in panels A and B rule out elasticities larger than 0.21 and 0.26, respectively.

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 who were previously employed at a different C corp (or S corp workers who were previously employed at a different S corp). Column 2 indicates the share of newly hired C corp workers who were previously hired at an S corp, and column 3 reports the converse. Column 4 reports the share of new hires who were previously employed outside the corporate sector (for example,

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-differences 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 fixed effects | Yes | Yes | Yes | Yes | Yes |
| Industry-size-year fixed effects | Yes | Yes | Yes | Yes | Yes |
| $R^2$ | 0.45 | 0.68 | 0.72 | 0.38 | 0.48 |
| *N* firms | 49,235 | 49,235 | 49,235 | 49,235 | 49,235 |
| Observations | 231,360 | 231,360 | 231,360 | 231,360 | 231,360 |

*Notes*: Panel A reports shares of newly hired workers in C and S corps who 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 who 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 who were previously hired at an S corp, and column 3 reports the converse. Column 4 reports the share of new hires who were previously employed outside the corporate sector (e.g., in the pass-through, nonprofit, 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.

in the noncorporate pass-through, nonprofit, 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 noncorporate 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 noncorporate 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 primarily reallocated from other sectors of the economy into the corporate sector.[12] This evidence also implies that incidence effects on workers were driven primarily by changes in the

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

intensive margin (earnings) rather than the extensive margin (employment) and motivates our modeling approach in Section IV.

## IV. MVPF and Incidence

We use the reduced-form elasticities estimated in Section III 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.

### A. *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 $= \left(1 - \frac{\tau^*}{1-\tau^*}\varepsilon^{\pi}\right)^{-1}$, where $\varepsilon^{\pi}$ is the corporate elasticity of taxable income and $\tau^*$ is the tax rate inclusive of payout taxes.[13] Averaging the pre- and post-TCJA tax rates of 35 percent and 21 percent, applying an effective payout tax rate of 2.5 percent, and using our empirical estimate of $\varepsilon^{\pi} = 0.70$ while accounting for its statistical uncertainty (SE $= 0.16$) yields an MVPF of 1.47 with a 95 percent confidence interval ranging from 1.21 to 1.91.[14] Among firms in the sample, this estimate implies that \$1 in forgone 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, Smith, Zidar, and Zwick 2025). MVPF estimates may also differ for very large C corps not included in the estimation sample. For comparison, Hendren and Sprung-Keyser (2020) estimate an MVPF of 1.16 and 1.83 for the US top individual income tax rate reforms in 2013 and 1993, respectively.

### B. *Incidence*

To assess the distributional impacts of the corporate tax changes, we adapt the frameworks developed in Suárez Serrato and Zidar (2016) and Fuest, Peichl, and Siegloch (2018) to estimate the share of each productive factor in the total tax burden. The 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 the analysis to assess incidence not only on factors of production but also over the income distribution, taking account

[13] To estimate an individual's willingness to pay for a corporate tax cut, the relevant tax rate $\tau^*$ 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 percent, and Burman, Clausing, and Austin (2017) report that 30 percent of corporate income is subject to payout taxes. Thus, we use an effective payout tax rate of $0.0825 \times 0.30 = 2.5\%$.

[14] The asymmetric confidence interval reflects the nonlinear transformation from the ETI to the MVPF.

of the empirical fact that many low-income workers own capital and most capital owners also work.[15] Fourth, using workers' observable locations, we evaluate the geographic incidence of corporate income tax cuts across US 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, Smith, Zidar, and Zwick 2025), implying that tax-induced increases in after-tax profits do not reflect higher returns to capital 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.[16]

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 percent of the national wage distribution, and high-paid workers as those in the top 10 percent. Workers and executives optimize consumption $C^L = w^j\left(1 - \tau^{p(j)}\right)L^j$, 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.[17] The indirect utility function for workers is given by $U^j\left(w^j\right) = w^jL^j$, and the change in utility from a change in wages is given by

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

where $\varepsilon^{L_j}$ is the elasticity of earnings for workers of type $j$ with respect to the net-of-tax corporate rate and $w^jL^j$ is their initial labor earnings (or equivalently for the incidence computations below, their intial labor share in pretax corporate value added, $\frac{w^jL^j}{\sum_j w^jL^j + \pi}$). By the envelope theorem, the change in firm owners' profits is given by $dy^K = -d\tau B - dwL\left(1 - \tau^c\right)$, 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

[15] In Fuest, Peichl, and Siegloch (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.

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

[17] 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.

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 pretax 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, Yagan, Zidar, and Zwick 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, Heathcote, and Perri (2025), respectively. We ascribe capital ownership of S corps to workers using data from Cooper et al. (2016). We define executives as the top 5 highest-paid workers at the firm, high-paid workers as those in the ninetieth to ninety-ninth percentiles of the national distribution, and low-paid workers as the bottom 90 percent. Letting $\omega^j$ 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

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

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

| | C corps (1) | S corps (2) | All corps |
|---|---|---|---|
| *Panel A. Corporate income MTRs* | | | |
| Mean 2016 $\tau^c$ | 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 percent workers | 0.44 | 0.25 | 0.37 |
| Bottom 90 percent workers | 0.32 | 0.28 | 0.31 |
| *Panel C. Distribution of firm ownership* | | | |
| Top 1 percent | 0.41 | 0.67 | 0.45 |
| 91–99 percent | 0.38 | 0.20 | 0.35 |
| Bottom 90 percent | 0.21 | 0.13 | 0.20 |
| Foreign share | 0.34 | 0.00 | 0.29 |
| *Panel D. Net-of-tax elasticities* | | | |
| Pretax 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 percent earnings | 0.26 | 0.26 | 0.26 |
| Bottom 90 percent earnings | 0.00 | 0.00 | 0.00 |
| *Panel E. Personal income MTRs* | | | |
| Executives | 0.396 | 0.396 | 0.396 |
| Top 10 percent workers | 0.280 | 0.280 | 0.280 |
| Bottom 90 percent 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 million and $10 billion 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 pretax 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 Board of Governors of the Federal Reserve System (2018) and Cooper et al. (2016), respectively. The foreign ownership share for C corps in 2016 is from Atkeson, Heathcote, and Perri (2025). The parameters in panel D are estimated in the empirical analysis. The top 10 percent earnings elasticity is the average of the ninetieth, ninety-fifth, and ninety-ninth centile elasticities, and the bottom 90 percent earnings elasticity is set to 0. Panel E reports the calibrated personal income tax rates on labor income for each worker type. The parameters in panels D and E do not vary by firm type.

## C. *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 Federal Reserve (2018); Cooper et al. (2016); and Atkeson, Heathcote, and Perri (2025), respectively. 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

Table 10—Incidence Estimates

| | C corps | S corps | All corps | US pop. |
|---|---|---|---|---|
| | Percent (SE) (1) | Percent (SE) (2) | Percent (SE) (3) | Percent (4) |
| *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 percent | 33 (3) | 63 (2) | 40 (2) | 1 |
| 91–99 percent | 55 (3) | 27 (2) | 46 (2) | 9 |
| Bottom 90 percent | 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 US Census, provided here [https://www2.census.gov/geo/pdfs/maps-data/maps/reference/us_regdiv.pdf], 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 US population shares. Standard errors are constructed from 500 bootstrap simulations of the jointly estimated parameters.

marginal income tax bracket schedule prior to TCJA. The parameters in panels D and E do not vary by firm type.

### D. *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. The benchmark estimates in column 1 isolate the effects of corporate tax cuts on C corps. Column 1 reports that 60 percent of gains from the corporate tax cuts flow to firm owners; 8 percent flow to executives, 32 percent flow to high-paid workers, and 0 percent of the gains flow to low-paid workers. Modest differences in factor intensities generate modest differences in factor incidence between C and S corps, but these differences are not statistically significant.

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 percent of the gains from corporate tax cuts accrue to the top 1 percent of the earnings distribution, 55 percent accrue to the ninetieth to

ninety-ninth percentiles, and 13 percent accrue to the bottom 90 percent. 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 versus S corps (column 1 versus column 2) reflect that, for firms in the sample, ownership of S corporations is more highly concentrated at the top of the income distribution relative to C corps. Supplemental 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 US residents, and incidence across census regions, estimated from equation (15). A significant share of C corporate equity, approximately 38 percent in the 2016 baseline year, is held by foreigners. As a result, we estimate that 20 percent 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 percent) is about 1.5 as large as for residents of the South (20 percent).

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 Supplemental 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 percent, 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 Supplemental 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, Kline, Vergara, and Yagan 2021).

## V. Conclusion

This paper studies the short-run impacts of the largest corporate income tax cut in US 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 percent of the within-firm earnings distribution, and rise sharply for executives, but do not change for workers in the bottom 90 percent. 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 the sample is 1.47. We estimate that 87 percent

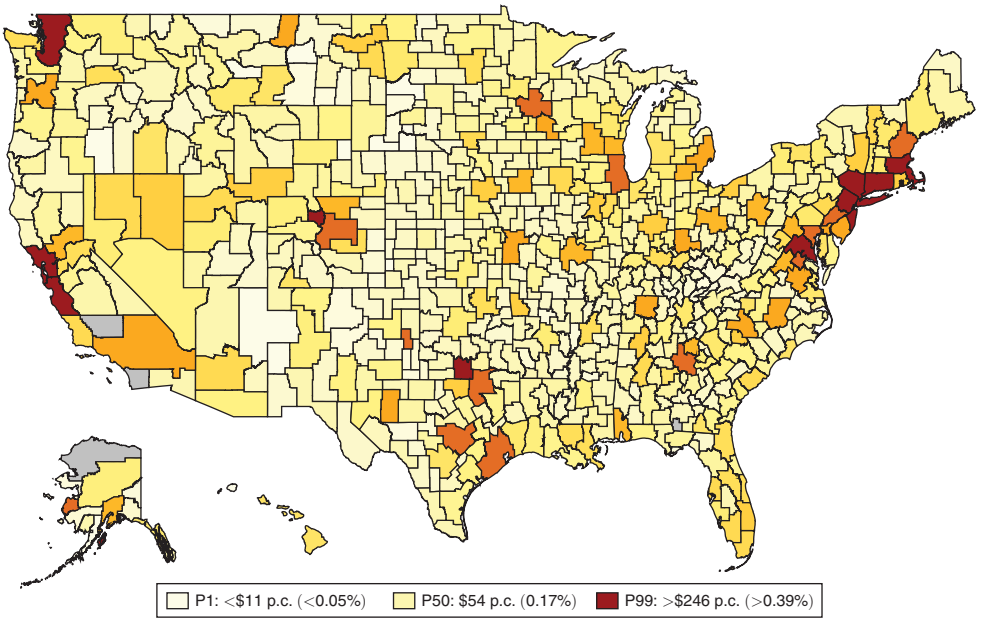


FIGURE 10. GEOGRAPHIC INCIDENCE

*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.

of private income gains flow to the top 10 percent of the earnings distribution, and 13 percent flow to the bottom 90 percent.

One implication from this study is that policymakers appear to face a trade-off 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 analogs 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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