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Economic Stimulus at the Expense of Routine-Task Jobs

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ABSTRACT

Do investment tax incentives improve job prospects for workers? We explore states' adoption of a major federal tax incentive that accelerates the depreciation of equipment investments for eligible firms but not for ineligible ones. Analyzing massive establishment-level data sets on occupational employment and computer investment, we find that when states expand investment incentives, eligible firms immediately increase their equipment and skilled employees; whereas they reduce routine-task employees after a delay of up to two years. These opposing effects constitute an overall insignificant effect on the firms' total employment and shed light on the nuances of job creation through investment incentives.

"Our bill aimed to help small businesses invest, grow, and create jobs by providing needed tax relief and certainty. … In light of the positive effects these provisions [permanent extension of Section 179 expensing]

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would have on small businesses, on jobs, and on our economy, I urge my colleagues to support the tax relief package."
– Senator Susan Collins, co-sponsor of Small Business Tax Certainty and Growth Act of 2015 (Congressional Record, December 17, 2015)

INVESTMENT TAX INCENTIVES, SUCH AS accelerated depreciation of capital, are important policy tools for governments to stimulate the economy. Although these incentives are defined over capital investment, their stated goal is usually to generate growth and boost job prospects.[1] Although the response of capital investment to these incentives is generally considered to be positive (most recently, see Zwick and Mahon (2017)), evidence regarding labor outcomes is limited and inconclusive. In this paper, we use confidential establishment–occupation level panel data to study the effect of investment tax incentives on the demand for different types of workers. We find no effect on firms' total employment, while we uncover positive effects for skilled workers and negative effects for routine-task workers (who are often regarded as low- to mid-skilled). These findings offer a nuanced narrative of how such stimulus plans affect job creation. In addition, our specific policy setting provides a laboratory to examine the extent to which equipment substitutes for routine-task labor and complements skilled labor in U.S. firms. Our findings therefore shed light on how firms make their input choices.

Specifically, we study the causal effect of Section 179 depreciation deduction on firm investment and employment. Section 179 of the Internal Revenue Code allows firms to expense limited amounts of qualifying investments in equipment and software instantly rather than following the standard depreciation schedule. By shifting the timing of tax deductions to day zero, this incentive increases the present value of tax benefits and significantly reduces the immediate funding needs for investment.[2] Importantly, Section 179 targets small businesses by imposing deduction limits and phaseout thresholds on firm investment, making firms with large equipment investments ineligible for this benefit.[3]

Since 2002, several federal acts have significantly increased the Section 179 deduction limits for federal taxes—from \$24,000 in 2002 to \$500,000 starting in 2010. Although some states adopt the federal deduction limits and allow deductions to also increase for state taxes, others deviate. Such deviations not only deprive firms of the deductions for state taxes but also complicate firms'

[1] For example, the title of the federal tax law passed in 2003 is the "Jobs and Growth Tax Relief Reconciliation Act" and that of the 2010 law is the "Small Business Jobs Act." Both laws had provisions that offered significant investment tax incentives. The "Tax Cuts and Jobs Act" of 2017 expanded the scale and scope of these investment tax incentives.

[2] In 2014, over \$120 billion of investments were expensed through Section 179, which account for approximately 10% of total investment in equipment and software in the United States.

[3] The deduction limit is the maximum deduction that a firm may claim in a year. If the firm's investment in a given year exceeds the phaseout threshold, the Section 179 deduction is reduced by the amount exceeding the threshold. Our definition of eligible firms closely follows these features of the law.

bookkeeping processes, deterring some eligible firms from claiming federal Section 179 deductions (Kitchen and Knittel (2016)). Using both the variation in states' treatment of Section 179 and the variation in firms' eligibility for Section 179, we examine the effect of changes in Section 179 limits on firms' investment and employment outcomes.

We develop our testable hypotheses using a simple framework in which firms choose optimal allocations of four factors of production to maximize firm value: equipment capital, skilled labor, routine labor, and nonroutine unskilled labor. Similar to Autor, Levy, and Murnane (2003), we assume that equipment and routine-task labor provide routine production inputs and are relative substitutes, while skilled labor and equipment are relative complements. Nonroutine unskilled labor neither substitutes for nor complements equipment. Faster tax expensing reduces the effective after-tax cost of investment, increases firm investment, and boosts the hiring of skilled labor, but leads to substitution of routine labor with capital. There is no direct effect on nonroutine unskilled labor.

We first examine firms' investment response to Section 179 by using a privately sourced database—the Computer Intelligence Technology Database (CiTDB), which provides annual data on the number of computers in over 500,000 establishments before 2010 and in 3.2 million establishments thereafter. We classify establishments with expected investment in equipment exceeding the federal Section 179 phaseout threshold as ineligible firms and the rest as eligible firms (see Section II.B). We find that a \$250,000 increase in the state Section 179 deduction limit (the increase that many states adopted in 2010) leads to a 1.7% additional increase in computers for eligible establishments relative to their matched counterparts not subject to the state limit increase.[4] We estimate a user cost elasticity of -1.6 for eligible firms, which is identical to the estimate in Zwick and Mahon (2017) and is consistent with their finding that small firms respond more strongly to tax incentives than large firms.

After confirming the effect of tax incentives on eligible firms' investment, we study employment outcomes using confidential establishment–occupation level microdata from the Bureau of Labor Statistics (BLS). This database provides employment data for over 800 detailed occupations in 1.2 million establishments in the United States over three-year cycles but does not include information on investment. We find little effect of Section 179 on firms' total employment. We further show that focusing solely on total employment is misguided, however, as the effects vary across segments of the labor market. Specifically, eligible firms significantly reduce the number of workers who perform procedural and rule-based tasks, that is routine tasks, in response to an

[4] We provide additional evidence on the response of qualified investments to tax incentives by examining a different data set that focuses solely on small businesses from the National Federation of Independent Business (NFIB). The NFIB data offer rich information about purchasing and leasing decisions related to various types of capital, such as equipment, buildings, land, and vehicles. We find that only equipment purchases respond positively to states' increases in Section 179 limits, whereas unqualified investments do not.

increase in state Section 179 limits, while they increase the number of skilled workers who perform nonroutine tasks: a $250,000 increase in the state limit leads to a 6% decrease in the routine-task employment of eligible firms in the three-year window after the policy change. The same policy shock leads to a 3.5% increase in skilled employment. We do not find any effect on nonroutine unskilled workers.[5] These different outcomes across the three distinct labor groups together drive the insignificant effect of Section 179 incentives on firms' total employment.

These findings are consistent with two major hypotheses from the labor economics literature on routine-biased technological change and skill-biased technological change. In a seminal paper, Autor, Levy, and Murnane (2003) hypothesize that computers tend to substitute for routine-task jobs, complement abstract-task jobs, and have no direct relation to manual-task jobs. In our setting, skilled workers perform mainly abstract tasks while nonroutine unskilled workers perform mainly manual tasks. We use our reduced-form estimates for investment from the CiTDB data set and for employment from the Occupational Employment Statistics (OES) data set to calculate substitution elasticities between computers and labor. Our estimates suggest strong substitutability between computers and routine-task labor (3.3) and strong complementarity between computers and skilled labor (0.1), confirming the main predictions of these hypotheses.

In addition to the direction of the effects on different types of labor, which are direct implications of our conceptual framework, our empirical tests also uncover a novel finding on the timing of these effects: eligible firms increase skilled workers shortly after states increase the tax incentive; but reduce routine-task workers more slowly over the following three years. It is intuitive that firms have an immediate need for compatible skilled workers when they purchase new equipment (Krusell et al. (2000)). This finding is also consistent with the idea that skilled workers are required to install new capital, as argued by Beaudry, Green, and Sand (2016). The delayed reduction of routine workers potentially indicates a time-to-build feature (Kydland and Prescott (1982)), whereby firms reduce routine workers only when new equipment is ready for full-scale production. This delay may also indicate firing frictions that lead to a transition period. The implication of this finding could be substantial: while investment tax incentives may boost skilled jobs immediately, the disruptive effect of these incentives on routine jobs may show up only a few years later.

Access to external financing is instrumental to firms' ability to pursue investments eligible for Section 179 deductions and substitute routine task jobs. A growing body of work argues that smaller firms are financially constrained and have higher discount rates for future cash flows.[6] The presence of

[5] Examples of routine-task jobs include bank tellers, assembly line workers, travel agents, and tax preparers. Examples of nonroutine-task skilled jobs include managers, physicians, and civil engineers. Examples of nonroutine unskilled jobs include drivers, animal trainers, and janitors.

[6] Hadlock and Pierce (2010) find that firm size is a particularly useful predictor of a firm's financial constraint. Farre-Mensa and Ljungqvist (2015) show that, unlike public firms, small private firms are financially constrained.

financial constraints make the tax deduction from Section 179 more valuable. Firms still need additional funds to finance eligible investments, however, and severe limits to external financing can prevent constrained firms that would benefit the most from deductions from taking them up. Consistent with this view, we find that both the immediate computer investment response and the subsequent routine-task employment response to Section 179 are more pronounced in counties with greater access to small business lending, as proxied by the prevalence of small banks in the county (Berger, Bouwman, and Kim (2017), Chen, Hanson, and Stein (2017)).

Our empirical design relies on the identifying assumption that changes in states' Section 179 limits are not correlated with other state-level shocks that differentially affect eligible and ineligible firms' investment and employment outcomes. We find support for this assumption in several ways. First, we graphically show that differences in investment and employment growth between eligible and ineligible firms are uncorrelated with state adoption of Section 179 prior to the initial increase in federal Section 179 limits in 2003. Second, we calculate all estimates including fixed effects of a full interaction of eight employment bins, over 300 NAICS four-digit industry classifications, and 12 years to control for industry-specific shocks that can have different effects on firms of different size. Third, we show that our results are not sensitive to controlling for state characteristics or state-year fixed effects. Fourth, we show that significant effects do not obtain in a placebo test where we replace the eligibility identifier, which is determined by firms' expected equipment investment and the given year's Section 179 policy, with standard small business identifiers, such as firms with employment below 50 or 100. Hence, any other state-level shocks that target all small businesses (instead of small businesses eligible for the Section 179 deductions) are unlikely to be driving our results. In sum, while the assumption underlying our research design is fundamentally untestable, our empirical strategies and robustness checks significantly mitigate concerns that our findings are driven by a spurious relation.

Our paper contributes to the growing literature that explores the effects of investment tax incentives. Most of the literature to date studies the effects on capital investment.[7] The few recent studies that look at labor outcomes include Gaggl and Wright (2017), Ohrn (2019), and Garrett, Ohrn, and Suárez Serrato (2020).[8] Ohrn (2019) finds no effect of states' adoption of Section 179 on the total employment of the manufacturing sector using public Census data. This aggregate-level result is consistent with our finding on firms' total employment using micro data. Garrett, Ohrn, and Suárez Serrato (2020) measure county-level exposure to the federal bonus depreciation incentive using industry-level

[7] Summers (1981), Summers (1987), Cummins et al. (1994), Goolsbee (1998), and Chirinko, Fazzari, and Meyer (1999), among others, are some of the earlier contributions to the area. Post-2000 U.S. investment tax incentives are studied in House and Shapiro (2008), Edgerton (2010), Zwick and Mahon (2017), and Ohrn (2019).

[8] Although not directly testing employment, Zwick and Mahon (2017) find a positive effect of the federal bonus depreciation incentive (another major investment incentive that targets all firms) on total payroll.

differences in bonus depreciation generosity and find a positive employment effect of the incentive. In Section II.D, we discuss the differences between the two settings and, in particular, how they relate to the labor-technology substitution channel. Using household-level data in the United Kingdom, Gaggl and Wright (2017) find that routine cognitive workers and nonroutine cognitive workers experience different patterns in earnings and working hours if their firms receive investment incentives. The authors point out that due to high labor firing costs in the United Kingdom, it is unclear how such incentives affect firms' employment. As we observe the dynamics of firm employment in each occupation, we provide more direct evidence on the response of firm employment to investment incentives.

Our findings also contribute to the literature on routine-biased technological change (RBTC) and skill-biased technological change (SBTC). The central theme of the RBTC literature is that technologies can directly replace routine-task jobs (e.g., Autor, Levy, and Murnane (2003), Autor, Katz, and Kearney (2006), Brynjolfsson and McAfee (2011), Autor and Dorn (2013), Goos, Manning, and Salomons (2014), Acemoglu and Restrepo (2019), Hershbein and Kahn (2018), Jaimovich and Siu (2019)). Acemoglu and Autor (2011) extensively review this literature and show that such skill-replacing technologies are essential for explaining documented changes in the distribution of employment, such as job polarization, over the past four decades. The central theme of the SBTC literature is that technologies can directly complement skilled labor (e.g., Griliches (1969), Goldin and Katz (1998), Berman, Bound, and Machin (1998), Krusell et al. (2000), Card and DiNardo (2002)). Our exploration of the investment tax policy shocks shows that when firms face cheaper access to technological equipment, they increase skilled employment and reduce routine-task employment, which is consistent with the central themes of both strands of literature. Our findings also suggest that these policy shocks have potentially accelerated the disappearance of routine-task jobs and the increase in job polarization over the past two decades.

We note that one must exercise caution when interpreting our results beyond their current context. First, we refrain from generalizing our findings on offsetting the employment effects observed in the treated firms to the entire economy. In particular, our setting does not capture possible spillover effects from eligible firms to their out-of-state suppliers. We therefore interpret our findings on total employment as pertaining to eligible firms but not to the economy as a whole. Second, while our findings show heterogeneous effects on firms' routine-task and skilled jobs, we refrain from drawing conclusions for individual or social welfare because we do not observe the subsequent outcomes for routine-task and skilled workers (e.g., job relocation). Third, our investment and employment results are based on separate data sets. Although both data sets deliver similar results for total employment, we cannot condition the routine-task and skilled employment results on firm investment behavior.

This paper is organized as follows. Section I presents a conceptual framework to guide our empirical tests. Section II describes the policy background that forms the basis of our identification strategy. Section III describes the data used in our empirical analysis and introduces our key measures. Section IV presents our empirical results related to tax policy, investment, and labor outcomes. Section V calculates price and substitution elasticities. Finally, Section VI concludes.

I. Conceptual Framework

In this section, we present our conceptual framework and hypotheses. A simple two-period model that explicitly derives the effect of investment tax incentives on a firm's investment and labor decisions is provided in the Internet Appendix.[9]

Firms use four factors of production. Three of these factors are labor inputs: routine labor, skilled labor, and nonroutine unskilled labor (L_R, L_S, and L_{NU}, respectively). The last factor is equipment capital (K). Routine labor (e.g., assembly line workers) and capital (e.g., robotic arms) perform routine tasks, whereas skilled labor (e.g., managers) performs abstract tasks that complement routine tasks in the production process. Autor, Levy, and Murnane (2003) emphasize that nonroutine unskilled labor (e.g., janitors) perform manual tasks that have limited opportunity to complement or substitute for capital. Following their lead, we assume that nonroutine unskilled labor does not interact with capital. Firms produce output with these inputs using the technology[10]

$$Y = L_S^{\alpha}\left(L_R^{\mu} + K^{\mu}\right)^{\frac{\beta}{\mu}} + mL_{NU}^{\alpha+\beta}, \tag{1}$$

where $\mu, \beta, \alpha \in (0, 1)$, and $\alpha + \beta < 1$.[11]

The routine task inputs are aggregated using a constant elasticity of substitution (CES) aggregator given by $(L_R^{\mu} + K^{\mu})^{\frac{1}{\mu}}$. The elasticity of substitution between routine labor and capital is given by $\frac{1}{1-\mu}$ and, by assumption, is greater than 1. The elasticity of substitution between skilled labor and aggregated routine task inputs is one. In other words, routine labor and capital are "relative substitutes," while aggregated routine inputs and skilled labor are "relative complements." Firms are competitive and take as given the prices of all inputs, namely, wages w_R, w_S, and w_{NU} and the purchase price of capital P.

The tax code allows firms to deduct the cost of new investment from taxable income over time. Accelerated depreciation of investment shifts tax deductions from later periods to earlier periods, effectively lowering the cost of investment (purchase price of capital P) due to the time value of money. We propose the following hypotheses.

[9] The Internet Appendix is available in the online version of the article on *The Journal of Finance* website.

[10] Autor and Dorn (2013) specify the technology for goods production as $L_S^{\alpha}(L_R^{\mu} + K^{\mu})^{\frac{\beta}{\mu}}$ and for service production as $mL_{NU}^{\alpha+\beta}$. We assume that these two production functions are additive in the aggregate.

[11] The last inequality captures decreasing returns to scale, meaning that a proportional increase in productive inputs leads output to increase by a smaller proportion.

Hypothesis 1: Faster depreciation tax policy (i.e., lower P) leads to higher investment in K.

Hypothesis 2: Faster depreciation tax policy leads to lower routine employment L_R.

Hypothesis 3: Faster depreciation tax policy leads to higher skilled employment L_S.

Hypothesis 4: Depreciation tax policy does not affect nonroutine unskilled employment L_{NU}.

II. Policy Background and Identification Strategy

A. Accelerated Depreciation as a Form of Investment Tax Incentive

"Accelerated depreciation" refers to the methods through which a company, for tax purposes, depreciates a fixed asset in such a way that the amount of depreciation taken each year is higher during the earlier years of that asset's ownership. In the United States, businesses typically may deduct the cost of newly installed assets from their taxable income according to the Modified Accelerated Cost Recovery System (MACRS). MACRS specifies the life and depreciation method for each type of property and equipment. For instance, based on MACRS, computers are depreciated by 20% in the year of purchase and by 32%, 19.2%, 11.5%, 11.5%, and 5.8% in the following five years.

Two sections of the Internal Revenue Code, Sections 179 and 168, allow certain businesses to further accelerate the deduction of certain types of investments. Section 179 allows a limited amount of qualified property to be depreciated 100% in the year of purchase, whereas Section 168, also known as "bonus deprecation," shifts a certain bonus proportion of depreciation into the year of purchase without capping the amount of investment. In both cases, accelerated depreciation shifts the timing of deductions from later years to earlier years without changing the total amount to be deducted.

Accelerated depreciation incentivizes investment through three possible channels. The first channel operates through the time value of money: faster depreciation increases the present value of tax benefits. This effect can be especially pronounced if the firm's cost of capital is high. The second channel operates through financial frictions: accelerated depreciation reduces the immediate funding needs for investment. If firms face financing constraints, freeing up funds can boost investment. The last channel operates through the tax-minimization motive. Xu and Zwick (2021) argue that firms with taxable income tilt their capital purchases toward fiscal year-ends to minimize taxes. To the extent that firms are motivated to minimize taxes through depreciation deductions, accelerated depreciation makes investments more effective in achieving this goal.

B. Eligibility for Section 179

Section 179 allows firms to expense a limited amount of qualified equipment investments instantly instead of following the baseline MACRS depreciation schedule. With few exceptions, investments qualified for Section 179 are limited to depreciable tangible assets such as machinery and equipment with a tax life of 3, 5, 7, 10, 15, or 20 years. Most structure investments, such as buildings, do not qualify. The use of Section 179 expensing is subject to three limitations. The first is the "deduction limit,' which is the maximum investment amount allowed for instant expensing within a year. The second limitation is the "phaseout threshold." If in a given year the firm places in service more equipment than the phaseout threshold, the tax benefits of Section 179 will be reduced dollar-for-dollar by the amount that exceeds the limit. Finally, the income limitation bars the firm from claiming a Section 179 deduction greater than its taxable income. Although firms in all lines of business and firms of all sizes have the option to elect Section 179 expensing, the deduction limit and the phaseout threshold make it more appealing to smaller firms. In 2014, 54% of equipment and software investments by sole proprietors—which are dominated by small firms—were claimed for Section 179 expensing. This percentage declines for other tax entities that include fewer small firms: 27% for S-corporations, 4% for partnerships, and 2% for C-corporations.[12]

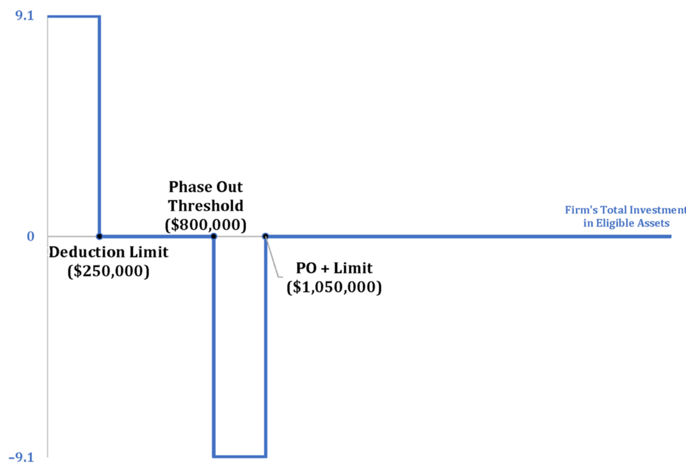
Panel A of Figure 1 illustrates the marginal Section 179 state tax benefits (i.e., the reduction in the present value of state taxes) for every additional $1,000 computer investment as a function of firms' total computer investment. For firms with computer investment below the deduction limit, an extra $1,000 investment in computers leads to a $9.1 state tax benefit (assuming that the firm is a pass-through entity with a discount rate of 10% and that the state income tax rate is 6.08%).[13] For firms with computer investment above the deduction limit but below the phaseout threshold, an extra dollar of investment does not affect the marginal tax benefit, because this extra dollar cannot be expensed immediately and is subject to the MACRS schedule. Finally, due to the dollar-for-dollar reduction in Section 179 deductions, when firms' computer investment exceeds the phaseout threshold, an extra dollar of investment reduces the Section 179 tax benefit until it reaches zero, that is, until firms' investment reaches the sum of the phaseout threshold and the deduction limit.

Panel B of Figure 1 illustrates how the above-mentioned marginal tax benefits are affected by increases in Section 179 deduction limits and phaseout

[12] These numbers are calculated from Tables 4–7 of Kitchen and Knittel (2016). Section 179 deductions reduce the corporate income taxes of C-corporations and individual income taxes of the owners of pass-throughs (sole-proprietorships, partnerships, and S-corporations).

[13] Section 179 is part of the federal tax code. Some states adopt the federal tax law for Section 179 expensing, while others deviate. We use this heterogeneity in our tests. As we focus on state taxes, we calculate the Section 179 benefits for state taxes. The median state individual income tax rate in our sample is 6.08%. The $9.10 marginal tax benefit of Section 179 is the difference between the present value of the depreciation tax shield under Section 179 ($1,000 × 6.08%) and its value under the baseline MACRS schedule ($1,000 × 6.08% × $(0.2 + \frac{0.32}{1+10\%} + \frac{0.192}{(1+10\%)^2} + \frac{0.115}{(1+10\%)^3} + \frac{0.115}{(1+10\%)^4} + \frac{0.058}{(1+10\%)^5})$).

Panel A. Marginal Section 179 tax benefits as a function of firm investment



Panel B. Effect of changes in Section 179 on the marginal tax benefits

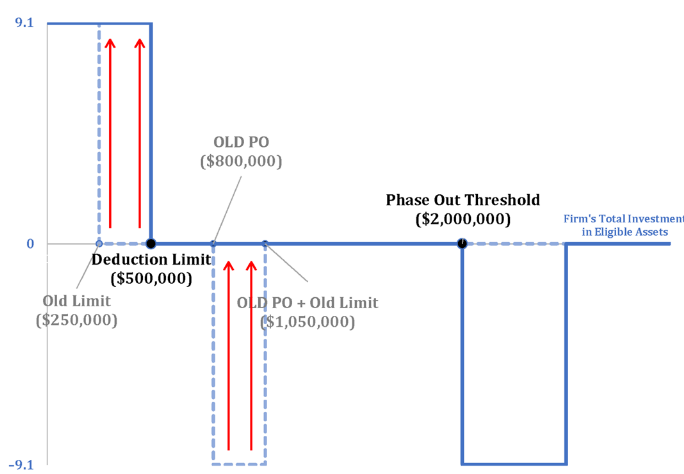


Figure 1. Changes in Section 179 and firms' tax benefits. Panel A illustrates the Section 179 tax benefit of an additional $1,000 investment in qualified assets, conditional on the firm's total investment in qualified assets. The x-axis represents the firm's total investment in qualified assets. The y-axis represents the reduction in the present value of state taxes for a pass-through entity due to an additional $1,000 qualified investment, such as computer investment, for a firm with a 10% discount rate operating in a state with the median income tax rate ($\tau_{state} = 6.08\%$). In the absence of Section 179, the asset is depreciated over five years following MACRS. Panel B illustrates the changes in the above-mentioned tax benefits when the deduction limit and the phaseout threshold of Section 179 increase from $250,000 to $500,000 and from $800,000 to $2,000,000, respectively. (Color figure can be viewed at wileyonlinelibrary.com)

thresholds from \$250,000 and \$800,000 to \$500,000 and \$2,000,000, respectively.[14] Given these changes, firms with equipment investments between the old deduction limit (\$250,000) and the new deduction limit (\$500,000) experienced an increase in Section 179 tax benefits for marginal investments, because an extra dollar of investment could now be expensed immediately under the new Section 179 policy. Moreover, firms with equipment investments between the old phaseout threshold (\$800,000) and the sum of the old phaseout threshold and the old deduction limit (\$1,050,000) also experienced an increase in tax benefits for marginal investments, because an extra dollar of investment did not lead to a dollar-for-dollar reduction in the Section 179 deduction as it would have under the old Section 179 policy. In contrast, firms with equipment investments exceeding the sum of the old phaseout threshold and the old deduction limit (\$1,050,000) were either not affected or incentivized to reduce investments due to the policy change.[15] Therefore, increases in Section 179 limit lead to nonnegative investment tax incentives for firms that invest less than the sum of the old phaseout threshold and the old deduction limit (old PO + old limit); these investment tax incentives do not hold or even run in the opposite direction for firms that invest more than this cutoff threshold.

Given these features of Section 179, we classify a firm as "eligible" for Section 179 benefits if the firm's expected investment in equipment is below the sum of the federal phaseout threshold and the deduction limit in the previous year.[16] A firm with expected equipment investment above this cutoff threshold is classified as "ineligible."

C. State Adoption of Federal Section 179

The Section 179 depreciation deduction was initially introduced by the Small Business Tax Revision Act of 1958 to reduce the tax burden on small business owners and to stimulate small business investment. This incentive was not significant before 2003. In 2002, the deduction limit was \$24,000 and the phaseout threshold was \$200,000.[17] Since 2003, several Acts have significantly increased the Section 179 deduction limit and the phaseout threshold for federal taxes, reaching \$500,000 and \$2,000,000, respectively, in 2010. Table I provides a timeline for the relevant legislation and changes to federal Section 179 limits.

[14] This example mirrors the increases in federal Section 179 limits in 2010.

[15] As illustrated in Panel B of Figure 1, firms with equipment investments more than the new phaseout threshold, but less than the sum of the new phaseout threshold and the new deduction limit (between 2 and 2.5 million dollars), experience reductions in Section 179 deductions after the limit increases.

[16] If the new phaseout threshold is lower than the sum of the old phaseout threshold and the old deduction limit, there will be no tax benefit beyond the new phaseout threshold. In this case, we set the cutoff threshold to the new phaseout threshold.

[17] See Guenther (2015) for a detailed discussion of Section 179 expensing and its legislative history.

Table I

Timeline of Federal Section 179 Program

This table shows the timeline for the Section 179 federal deduction limits and phase-out thresholds. The deduction limit is the maximum deduction that a firm may claim in a year. If a firm's investment in qualifying equipment and software in a given year exceeds the phaseout threshold, the Section 179 deduction is reduced, dollar for dollar, by the amount exceeding the threshold.

Date Introduced	Date Enacted	Applied Period	Deduction Limit	Phase-out Threshold	Act
Baseline		≤2002	\$24,000	\$200,000	
February 27, 2003	May 28, 2003	2003 to 2005	\$100,000	\$400,000	Jobs and Growth Tax Relief Reconciliation Act
June 4, 2004	October 22, 2004	2006 to 2007	\$100,000	\$400,000	American Jobs Creation Act of 2004
January 17, 2007	May 25, 2007	2007	\$125,000	\$500,000	Small Business and Work Opportunity Tax Act of 2007
January 28, 2008	February 13, 2008	2008	\$250,000	\$800,000	Economic Stimulus Act of 2008
January 26, 2009	February 17, 2009	2009	\$250,000	\$800,000	American Recovery and Reinvestment Tax Act of 2009
June 12, 2009	March 18, 2010	2010	\$250,000	\$800,000	Hiring Incentives to Restore Employment Act
May 13, 2010	September 27, 2010	2010 to 2011	\$500,000	\$2,000,000	Small Business Jobs and Credit Act of 2010
July 24, 2012	January 2, 2013	2012 to 2013	\$500,000	\$2,000,000	American Taxpayer Relief Act of 2012
December 1, 2014	December 19, 2014	2014	\$500,000	\$2,000,000	Tax Increase Prevention Act of 2014

Before 2003, nearly all states adopted federal Section 179 limits for state taxes. Following the Jobs and Growth Tax Relief Reconciliation Act of 2003 (JGTRRA), the federal Section 179 deduction limit was increased from \$24,000 to \$100,000 for federal taxes. Twenty-eight states continued to adopt the federal deduction limit by increasing their Section 179 limit for state taxes, but other states deviated. Panel A of Figure 2 provides a map of states' adoption status with respect to the new federal Section 179 limit in 2003. Panel B provides the same map as it would appear in 2014. Comparing Panels A and B, we observe that states' adoption decisions are sticky, which implies that states' adoption decisions in 2003 largely shaped the cross-sectional variation in states' Section 179 limits for state taxes for many years thereafter.[18] Due to states' sticky adoption decisions, the time-series variation in states' Section 179 limits is strongly influenced by the staggered increases in federal Section 179 limits that affect the adopting states but do not directly affect the nonadopting states.

We next examine whether changes in states' Section 179 deduction limits are correlated with changes in other state policies and business conditions that may affect firms' investment and employment decisions. To do so, we run cross-sectional regressions of changes in state Section 179 deduction limits on lagged changes in the political affiliation of states' governors, changes in measures of states' fiscal health, and economic indicators. Table II shows that increases in states' Section 179 limits are accompanied by the adoption of a tax bonus depreciation incentive, which targets all businesses. Changes in state Section 179 limits are not systematically related to any other political, fiscal, or economic factors, consistent with the findings in other studies (e.g., Ohrn (2019)). As these factors can still potentially affect investment and employment outcomes, we add these time-varying state-level controls to our regression specifications.

States' adoption of the federal Section 179 incentives not only increases investment incentives by offering additional state tax benefits but also helps incentivize businesses to take up the federal Section 179 deduction. Kitchen and Knittel (2016) study Statistics of Income (SOI) tax data and show that over the 2002 to 2014 period, 32.3% of firms that had eligible investments and allowable income for federal Section 179 deductions did not claim the deductions.[19] These firms did not claim Section 179 deductions for several reasons. One important factor was whether the firm's state has adopted the federal incentive. Indeed, if states do not adopt the federal law, firms must maintain two separate sets of books for depreciation treatment for tax purposes when filing federal and state taxes.[20] Consistent with this "complexity" argument, Kitchen and Knittel (2016) find that state adoption is an important determinant of whether firms take up the bonus depreciation incentive, which targets

[18] During the 2003 to 2014 period, 12 states made changes to their adoption status, while 38 states maintained their previous status.

[19] This number is calculated based on Tables 4–7 of Kitchen and Knittel (2016).

[20] Zwick (2021) argues that corporate tax complexity significantly deters firms from claiming corporate tax refunds. Out of 1.2 million corporate tax returns analyzed, only 37% of eligible firms claimed their refund.

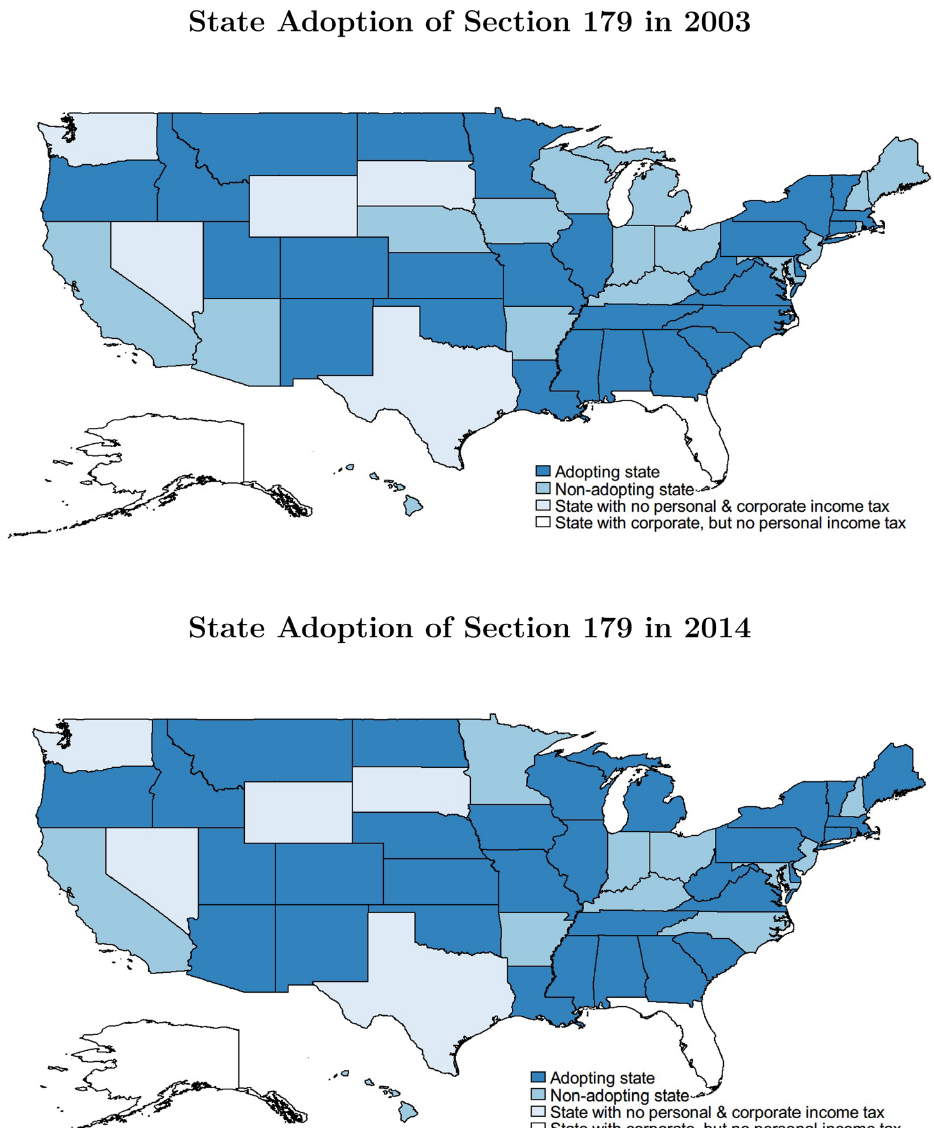


Figure 2. State adoption of the federal Section 179 deduction limits in 2003 and 2014. This figure illustrates states' adoption of the federal Section 179 deduction limits as of 2003 and 2014. States that collect corporate income tax but not individual income tax are colored white. States that do not collect either individual or corporate income tax are shaded very light blue. Dark blue states adopt the federal Section 179 deduction limits, whereas light blue states do not adopt the federal limits. (Color figure can be viewed at wileyonlinelibrary.com)

Table II

Changes in State Section 179 Deduction Limits

This table relates lagged changes in states' economic and political characteristics to changes in state Section 179 deduction limits. *Hiring Credits* is the number of state job creation hiring credit programs. *Bonus Adoption* is a dummy variable that equals 1 if the state adopts the federal bonus depreciation tax incentive. *Budget Surplus* is the state's budget surplus in millions of dollars (a negative number indicates a budget deficit). *Democratic Dummy* is a dummy variable that equals 1 if the state is governed by a Democratic governor. *State Indiv.Tax Rate* and *State Corp.Tax Rate* are the state's individual and corporate income tax rates, respectively. *GSP* is the real gross state product. All regressions include year fixed effects. States with zero individual and corporate income tax rates are included in the set of states that do not change state 179 deduction limits. States that collect corporate income tax but not individual income tax are excluded. Standard errors are clustered at the state level and reported in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively. The sample period is 2003 to 2014.

	Changes in State Section 179 Limit ($thousands)				
Lagged Changes in…	(1)	(2)	(3)	(4)	(5)
State Hiring Credits	−2.61				−1.62
	(5.40)				(5.35)
State Bonus Adoption	13.10***				11.86***
	(3.43)				(3.37)
State Budget Surplus		1.67			1.51
		(1.26)			(1.27)
State GSP		1.02			1.07
		(0.90)			(0.89)
State Unemployment			2.70		2.33
			(7.90)		(7.62)
State Indiv. Tax Rate				−7.88	−7.35
				(7.95)	(7.80)
State Corp. Tax Rate				6.46	6.14
				(4.96)	(4.71)
State Democratic Dummy				1.84	2.64
				(3.65)	(3.47)
Observations	576	576	576	576	576
Adjusted R^2	0.28	0.28	0.28	0.28	0.28

both large and small businesses. The complexity of keeping different books for federal and state taxes can only be more burdensome for smaller businesses, which Section 179 targets.[21] In summary, when federal Section 179 limits increase, eligible firms in adopting states not only have better fiscal outcomes due to the state tax deduction, but also face less complexity when taking up the federal incentive than eligible firms in nonadopting states.

[21] According to Kitchen and Knittel (2016), 77% of eligible C-corporations with taxable income claimed Section 179 deductions over 2002 to 2014, whereas 64% of partnerships and 65% of sole proprietorships, which are typically smaller than C-corporations, claimed Section 179 deductions in the same period.

D. Identification Strategy Based on State Adoption and Firm Eligibility

Having discussed firms' eligibility for Section 179 and states' Section 179 deduction limits, we present our identification strategy. Our thought experiment is that when the federal government increases Section 179 deduction limits, adopting states increase their Section 179 deduction limits for state taxes but nonadopting states do not. In this case, eligible firms located in nonadopting states receive investment incentives only from the federal government. In contrast, eligible firms located in adopting states receive incentives from both federal and state governments. Ineligible firms receive no incentives regardless of which state they are located in.

It is important to note that unobservable factors may drive both a state's policy on Section 179 and the investment and employment decisions of firms that operate in that state. This in turn can lead to a correlation between state Section 179 policies and firm outcomes. Assuming that such unobservable state-level factors affect both eligible firms and ineligible firms operating in the state, the effect of these factors can be controlled largely by examining the investment and employment outcomes of ineligible firms across states. Therefore, our identification strategy relies on the interaction between changes in state Section 179 limits and firms' eligibility, as shown in equation (6) of Section IV.B. The assumption underlying this strategy is that states' increase in Section 179 limits is not correlated with other state-level shocks that differentially affect eligible and ineligible firms' investment and employment outcomes. In Section IV, we present evidence in support of this assumption.

Our identification strategy, based on state adoption and firm eligibility, is distinct from the strategy employed by Zwick and Mahon (2017) and Garrett, Ohrn, and Suárez Serrato (2020), who study the bonus depreciation incentive. Zwick and Mahon (2017) identify industries' exposure to bonus depreciation based on the depreciation period of assets in each industry. The idea is that industries investing in capital with a longer depreciation period will receive larger tax benefits from the incentive's accelerated depreciation.[22] Under this identification strategy, the policy variable sorts industries not only by the tax benefits, but also by the depreciation period of capital used in each industry. Given that the elasticity of substitution between different types of capital and labor can vary significantly (Krusell et al. (2000)), this identification strategy may not differentiate between labor effects driven by heterogeneous tax benefits, or those driven by heterogeneous elasticity of substitution between capital and labor across industries. For example, the technology we propose in our conceptual framework, where capital substitutes for routine-task labor and complements skilled labor, applies more readily to industries that invest in information systems equipment (Acemoglu and Autor (2011)). This type of capital has a relatively short depreciation period of five years.[23] In Zwick and

[22] Garrett, Ohrn, and Suárez Serrato (2020) utilize similar variation by measuring each county's exposure to bonus depreciation through the county's local industries' depreciation schedules.

[23] See https://www.irs.gov/forms-pubs/about-publication-946.

Mahon (2017) and Garrett, Ohrn, and Suárez Serrato (2020), firms from industries that invest heavily in information systems equipment would be classified as having relatively less incentive to respond to bonus depreciation than firms in other industries. Accordingly, we adopt an orthogonal identification strategy that relies on the heterogeneity in state policies and firm eligibility and run all comparisons within industry. By controlling for industries, our identification strategy uncovers the marginal effect of incentives on investment and labor outcomes.

III. Data and Measurement

In this section, we describe the data used in this paper and the measurement of key variables.

A. Investment Outcome Measures

Our primary measure of firms' investment outcomes is derived from the CiTDB, which is based on telephone surveys of establishments (usually annually) and includes roughly 500,000 establishments before 2010 and 3.2 million thereafter. Although the database includes many variables related to information technology (IT) investment, the only variable that has been consistently surveyed over our sample period is the number of computers (see the Internet Appendix for more details about the database). We measure an establishment's computer investment rate as the difference between the establishment's number of computers in year t and $t+1$ divided by the average of the two.[24] The database also provides other establishment-level information, such as the name, address, industry of the business, and number of employees. We supplement our computer investment measure by also exploring changes in establishments' IT intensity, which is measured as the number of computers per employee.[25]

To gain more insight into firms' investment responses to Section 179 incentives beyond computers, we use the Small Business Economic Trends survey compiled by the NFIB. This monthly survey of roughly 900 NFIB member businesses asks whether the firms invested in the past six months and, if so, what type of investment they pursued (e.g., equipment, vehicles, building or land

[24] CiTDB surveys firms throughout the year and reports the data by survey year. Therefore, on average, computer holdings are reported as of the middle of the year. Changes to Section 179 limits tend to occur toward the year-end. In addition, Xu and Zwick (2021) document that firm investment tends to spike toward the year-end due to tax-minimization motives. Hence, it is safe to assume that any computer investment following year t's Section 179 policy shock is better reflected in year $t+1$ computer holdings in CiTDB. We therefore measure the investment rate in year t as the change in computer counts from year t to year $t+1$. CiTDB data are not updated in 2011 and thus we do not have the investment rate measure for 2010 and 2011.

[25] A common use of the CiTDB data is to measure establishments' IT intensity. See, for instance, Brynjolfsson and Hitt (2003), Tambe, Hitt, and Brynjolfsson (2012), Bloom, Sadun, and Van Reenen (2012), among others.

purchases, building improvements, and whether new property is purchased or leased). This level of detail is useful, as Section 179 applies primarily to purchases of equipment, while most other purchases and leases do not qualify for Section 179. Furthermore, unlike our other data sets, NFIB reports the business entity type (C-corporation, S-corporation, sole proprietorship, or partnership), which allows us to specifically target state taxation of different business forms in our empirical tests.

B. Employment Outcome Measures

We construct employment measures from the establishment–occupation level microdata provided by the OES program of the BLS. This data set covers surveys that track employment and wage rates for over 800 detailed occupations in approximately 1.2 million establishments over the course of three years. This sample of establishments covers, on average, 62% of the nonfarm employment in the United States. Within a three-year period, 200,000 establishments are surveyed in each half year, and each establishment is surveyed in three-year cycles. The Internet Appendix provides more details about these data.

We classify employees along two dimensions based on their occupations. The first dimension measures the routineness of the tasks performed in each occupation. The second dimension measures how much skill is required for each occupation, characterized by a requirement for higher education or an expertise level attained through related work experience.

We measure an establishment's routine-task and nonroutine-task employment following the methodology described in Zhang (2019), which is an improved version of a commonly used procedure in the labor economics literature (see Autor, Levy, and Murnane (2003); Autor and Dorn (2013)). The procedure starts by identifying occupations that can be classified as routine-task labor. Specifically, we use the Revised Fourth [1991] Edition of the U.S. Department of Labor's Dictionary of Occupational Titles (DOT) to capture each occupation's required skill level in performing "abstract," "routine," and "nonroutine manual" tasks (scaled from 1 to 10).[26] Following Autor and Dorn (2013), we define the routine-task intensity (RTI) score for each OES occupation as

$$RTI_k = \ln\left(T_k^{\text{Routine}}\right) - \ln\left(T_k^{\text{Abstract}}\right) - \ln\left(T_k^{\text{Manual}}\right),$$

where T_k^{Routine}, T_k^{Abstract}, and T_k^{Manual} represent the required skill level for performing routine, abstract, and nonroutine manual tasks in occupation k, respectively.

We define routine-task labor as follows. In each year, as suggested by the OES program, we select all workers in the OES sample in the current year

[26] Abstract skill level is measured as a combination of mathematical skills and skills in direction, control, and planning. Routine skill is measured as a combination of finger dexterity skills and skills in setting limits, tolerances, or standards. Nonroutine manual skill is measured from eye-hand-foot coordination skills. See Zhang (2019) for more details.

as well as in the previous two years to represent the current year's total labor force.[27] We then sort all workers in the current year's labor force based on their occupations' RTI scores. We define workers as "routine-task labor" if their RTI scores fall within the top quintile of the distribution for that year. By classifying routine-task labor each year, this measure of routine-task labor accounts for technological evolution, which is not captured previously in the labor economics literature. In particular, this measure also accounts for the fact that certain occupations that are not substitutable by machines in earlier years become substitutable as their RTI rankings increase over time.[28]

We further classify employees as skilled versus unskilled. Skill can be attained through higher education or related work experience. We thus define "skilled labor" as employees with occupations that require at least two years of related work experience or a college degree using data from O*Net.[29] Based on this definition, roughly 40% of employees in the OES are classified as skilled labor, and the remaining 60% are classified as unskilled labor.[30]

Figure 3 provides a breakdown of OES employees, classified based on routineness and skill. The size of the nonroutine skilled workforce and that of the nonroutine unskilled workforce are almost the same: both categories account for roughly 40% of employment. Almost all of the remaining employees are classified as routine unskilled labor, which accounts for 18% of the workforce. Employees who are classified as routine and skilled account for only 1% of the workforce, consistent with prior labor economics literature, which finds that routine jobs are not high-skilled jobs. Furthermore, the types of tasks performed by these routine skilled employees and routine unskilled employees are similar to each other while different from the other groups, prompting us to combine these two groups into one "routine" group.

A comparison of the characteristics of nonroutine skilled and nonroutine unskilled labor reveals stark differences in the types of tasks performed by these employees as well as differences in their average compensation. Nonroutine skilled employees, such as managers and engineers, perform mostly abstract tasks and earn an average wage of $31 an hour. In contrast, nonroutine unskilled employees, such as drivers and janitors, perform primarily manual tasks and earn an average wage of $15 an hour. These differences are economically meaningful and highlight the heterogeneity within the nonroutine workforce. As nonroutine skilled employees account for 98% of the skilled

[27] The OES program produces occupational estimates for public use based on surveys from year $t-2$ to t. See https://www.bls.gov/oes/tables.htm.

[28] Although we think that the time-varying measurement of routineness is desirable, the results are robust to using a fixed classification of occupations. The Internet Appendix provides the results using a fixed classification of routine-task labor based on the 2003 RTI rankings.

[29] Most skilled occupations (e.g., managers and mechanical engineers) require both higher education and work experience. In a few cases, skilled occupations rely on a college degree but no related work experience, such as chemists and computer engineers, or require work experience but no college degree, such as electricians and commercial pilots.

[30] Skill can be measured in alternative ways. The correlation between our measure and a measure based solely on education (requiring college completion or better) is 0.82, and a measure based only on related work experience is 0.77.

Figure 3. Characteristics of occupations by routineness and skill. This figure shows the characteristics of occupations categorized by routineness and skill. Definitions of routine and non-routine occupations are provided in Section III. Occupations classified as skilled require at least two years of related work experience or a college degree based on O*Net. All statistics are calculated each year over the period 2003 to 2014 and are averaged based on the OES data. Task intensities are calculated by the authors based on Dictionary of Occupational Titles data and standardized to have a mean of zero and a standard deviation of one. (Color figure can be viewed at wileyonlinelibrary.com)

workforce, for brevity we refer to them as "skilled" labor. These comparisons result in our three distinct labor categories: routine, skilled, and nonroutine unskilled labor.

Following this categorization, we measure establishment j's routine employment ($L_{R,j,t}$), skilled employment ($L_{S,j,t}$), nonroutine unskilled employment ($L_{NU,j,t}$), and total employment ($L_{Tot,j,t}$) in year t as

$$L_{Tot,j,t} = \sum_k emp_{j,k,t}, \tag{2}$$

$$L_{R,j,t} = \sum_k \mathbb{1}\left[RTI_k > RTI_t^{P80}\right] \times emp_{j,k,t}, \tag{3}$$

$$L_{S,j,t} = \sum_k \mathbb{1}\left[RTI_k \leq RTI_t^{P80}\right] \times \mathbb{1}\left[Skilled_k\right] \times emp_{j,k,t}, \tag{4}$$

$$L_{NU,j,t} = L_{Tot,j,t} - L_{R,j,t} - L_{S,j,t}, \tag{5}$$

where $emp_{j,k,t}$ is the number of employees of occupation k in establishment j at year t, $\mathbb{1}$ is the index function, RTI_k is the RTI score of occupation k, RTI_t^{P80} is the 80th percentile of RTI scores for the labor force in year t, and $Skilled_k$ is an indicator that takes the value of 1 if the occupation is categorized as a skilled occupation. As the OES program surveys each establishment once every three years, we construct the three-year employment growth for each type of labor in establishment j as the difference between employment in year t and year $t+3$, divided by the average of the two (similar to our measure of computer investment).[31]

C. Eligibility Measure for Firms

Firms benefit from Section 179 limit increases if their investment in qualifying assets (primarily equipment) is below the cutoff threshold defined in Section II.B. We construct an ex ante measure of firm eligibility by predicting a firm's investment and comparing it to the federal Section 179 cutoff threshold. We estimate expected equipment investment based on each firm's (beginning-of-period) employment and the ratio of equipment investment to employment for the industry the firm belongs to.[32] If the expected equipment investment of a firm is below the cutoff threshold, the firm is classified "eligible," otherwise, it is "ineligible" to benefit from Section 179 limit increases.

Challenges arise in determining the eligibility of multi-establishment firms. First, firms that operate in multiple states follow state legislation to apportion their taxable income across states. These apportionment rules are complex and are based largely on the firms' employment, assets, and revenues in each state.[33] Such complexity makes it difficult for us to gauge a multi-state firm's exposure to a given state's taxation. Second, both the CiTDB and the OES data are based on establishment-level surveys and do not allow us to identify all establishments within a multi-establishment firm.

[31] When computing the growth rates from t to $t+3$, we fix the classification of routine-task labor, skilled labor, and nonroutine unskilled labor as of year t.

[32] We use Bureau of Economic Analysis (BEA) data to calculate the average equipment investment-employment ratio for each industry (at the three-digit NAICS level). Employment is defined as the full-time equivalent employees by industry, and investment is defined as investment in private equipment by industry. We smooth the ratio by taking the average over the last three years. To test robustness of our results to measurement errors in this procedure, we perturb establishments' equipment investment by increasing or decreasing it by 5% and 10%. We continue to find very similar results (see the Internet Appendix).

[33] Firms that have physical presence (assets or employees) in multiple states must use state rules to apportion their profits to determine how much of their income will be taxed by each state. Profits are typically apportioned across states based on the ratios of the company's property, payroll, and sales in each state. Each state chooses its weights for the three ratios to determine the final proportion of the firm's income to be taxed by the state. Over time, many states increased the weight for the sales ratio and reduced the weights of the two other ratios. If a firm sells to a state in which the firm does not have a physical presence, such income may not be taxed by any state. To counter this phenomenon, many states have increasingly adopted "throwback" or "throwout" rules to identify and tax profits earned in other states but not taxed by those states. See Giroud and Rauh (2019) for more details about taxation of multi-state firms.

Due to these limitations, we conduct our analyses at the establishment level rather than the firm level. Under this strategy, establishments that are classified as ineligible should belong to an ineligible firm, given that the firm's expected equipment investment, aggregated over all of its establishments, can only be larger. We are therefore confident that ineligible establishments are truly ineligible for Section 179. However, this strategy introduces noise to the eligible group as small establishments from large ineligible firms are also categorized as eligible for Section 179. Misclassifying these small establishments could potentially bias our estimates toward being insignificant. In Section IV.F, we restrict eligible establishments to single-unit businesses, which removes the noisy multi-establishment firms from the set of eligible firms, and we find very similar results to our baseline.

D. Policy Variable: Changes in State Section 179 Deduction Limits

We hand-collected state Section 179 deduction limits and phaseout thresholds from CCH State Tax Handbooks, and we supplemented these handbooks with state tax authorities' websites when needed.

Our main policy variable is the year-over-year change in state Section 179 deduction limits. As we discuss in Section II.C, these changes derive primarily from changes in federal Section 179 limits in adopting states but not in nonadopting states. A less common source of variation in state deduction limits results from states opting in or out of the adoption of the federal limits. Although we see no reason to believe that these state-level changes are less influential for firm investment and employment than those induced by changes in federal limits, excluding state–years in which a state switched its status does not affect our results (see the Internet Appendix).

Section 179 deductions benefit firms by reducing their corporate income tax bills for C-corporations or the individual income tax bills of the owners of pass-through entities. Our main investment and employment data sets (except for the NFIB data set) do not provide information on the business entity type, so we do not know exactly which tax rates (corporate or individual) investors will be subject to. Accordingly, we restrict our sample to states that collect both corporate and individual income taxes or that collect neither of them. If a state collects neither corporate nor individual income tax, we assume that the state's Section 179 limit changes are zero.[34] We exclude states that collect corporate income tax but not individual income tax (i.e., Alaska and Florida) from our tests.

E. Other State-Level Controls

We hand-collected state Section 168 bonus depreciation adoption from CCH State Tax Handbooks, and we supplemented these handbooks with state tax authorities' websites when needed. We also use various state-level controls in

[34] These states are Nevada, South Dakota, Texas, Washington, and Wyoming.

our empirical tests, including states' number of hiring credit programs, unemployment rate, real GDP growth, budget surplus, gubernatorial party affiliation, corporate income tax rates, and individual income tax rate. The Internet Appendix provides more details on the data sources and construction of these state-level measures.

IV. Empirical Evidence

In this section, we test the hypotheses developed in Section I. We predict that increasing tax incentives for investment will boost equipment investment and skilled employment but reduce routine employment. We predict no effect on nonroutine unskilled labor. We first discuss the summary statistics for our CiTDB and OES samples. We then study the effect of changes in state Section 179 limits on investment. After confirming the effect of state policy changes on investment, we study how these changes affect labor (total, routine, skilled, and nonroutine unskilled) outcomes and discuss results of our robustness analysis.[35]

A. Summary Statistics

Table III reports the CiTDB and OES sample statistics at the establishment-year level between 2003 and 2014. We require establishments in the CiTDB and OES data sets to have consecutive observations so that we can compute current and past investment rates and employment growth. We also require establishments to have at least three employees.[36] For the CiTDB sample, we further exclude industries in which computer investment accounts for less than 5% of total investment in equipment and software. This sample selection ensures that computer investment is a relevant investment category for the establishments in our sample. The OES sample includes 331,948 establishment-year observations. The average (median) OES establishment has 102 (25) employees, 21 (3) routine employees, 39 (7) skilled employees, and 42 (9) nonroutine unskilled employees. The CiTDB sample includes slightly larger establishments, with the average (median) establishment having 158 (50) employees and 174 (43) computers. The sample consists of 354,566 establishment-year observations.

[35] Although our two micro data sets, CiTDB and OES, allow us to study firms' computer investment and various types of employment responses to investment tax incentives separately, we cannot study these responses within the same firm simultaneously because these data sets do not share a common identifier.

[36] In the Internet Appendix, we examine an alternative sample selection criteria by excluding establishments with fewer than five employees. We find similar results, and hence our findings are not driven by the smallest establishments.

Table III

Summary Statistics for Establishments

This table reports summary statistics for employment and computers at the establishment level. Employment data are from the Occupational Employment Statistics database at the Bureau of Labor Statistics. The variables L_{tot}, L_R, L_S, and L_{NU} are the establishments' total employment and employment of routine-task labor, skilled labor, and nonroutine unskilled labor, respectively. Computer data come from the Computer Intelligence Technology Database (CiTDB). *Computer* is the total number of computers in the establishment. The variable *IT Int.* is the IT intensity of the establishment as captured by the number of computers per employee. We require that establishments have consecutive observations that allow computation of current and past employment or the investment ratio. We further require that establishments have at least three employees. In the CiTDB sample, we exclude establishments from industries in which computer investment accounts for less than 5% of total investment in equipment. States that collect corporate income tax but not individual income tax are excluded. The sample period is 2003 to 2014.

Variable	Mean	Std. Dev.	Min	P25	P50	P75	Max
CiTDB Sample (Obs = 354,566, Eligible = 196,457, Ineligible = 158,109)							
L_{tot}	158.46	795.81	3	22	50	125	300,100
Computer	173.98	1,760.72	0	17	43	130	601,990
IT Int.	1.26	2.34	0	0.56	0.98	1.46	505.33
OES Sample (Obs = 331,948, Eligible = 203,758, Ineligible = 128,190)							
L_{tot}	101.70	393.36	3	11	25	74	40,142
L_R	20.48	79.26	0	1	3	12	11,003
L_S	39.28	249.82	0	2	7	21	38,029
L_{NU}	41.95	146.37	0	3	9	30	12,119

B. Investment Outcomes

Our first hypothesis is that an increase in state Section 179 limits leads to additional investment (as measured by computer purchases) by eligible firms. We do not anticipate such an effect for the firms that are ineligible for the deduction.[37]

B.1. Regression Analysis for Investment

We test our hypothesis by running the regression

$$\begin{aligned} Inv_{j,s,t} = {} & b_0 + b_1 \Delta Limit_{s,t} + b_2 Eligible_{j,t} + b_3 \Delta Limit_{s,t} \times Eligible_{j,t} \\ & + b_4 \Delta X_{s,t} + b_5 Inv_{j,s,t-1} + Dummy_{EmpBin \times Ind \times Year} + \epsilon_{j,s,t}, \end{aligned} \tag{6}$$

where $Inv_{j,s,t}$ is the investment rate measured from the computer growth of firm j from year t to $t+1$, $\Delta Limit_{s,t}$ is the change in state Section 179 limit from year $t-1$ to t in millions of dollars, and $Eligible_{j,t}$ is a dummy that equals

[37] Given that our conceptual framework ignores any indirect general equilibrium effects, we can only claim that we anticipate no direct effects of changes in Section 179 limits on ineligible establishments.

1 if firm j is eligible for federal Section 179 deduction (for the definition of eligibility, see Section II.B).

This regression specification implements the thought experiment we describe in Section II.D. Several points are worth noting. First, the main policy variation comes from variation across states. To ensure that we are comparing establishments in the treated states (i.e., states that increase Section 179 limits), with similar establishments in control states (i.e., states that do not increase limits), we include fixed effects for a full interaction of establishment-level employment bins, NAICS four-digit industry codes, and year, $Dummy_{EmpBin\times Ind\times Year}$. Employment bins are based on beginning-of-period employment levels and are defined as (1, 4), (5, 9), (10, 14), (15, 24), (25, 49), (50, 99), (100, 199), and 200 or more.[38]

Second, we control for unobservable state-level confounding factors that (i) drive investments of both eligible and ineligible firms in the state and (ii) are correlated with the state's increase in Section 179 limits. The coefficient b_1 captures the effect of these potential confounding factors, while b_3 estimates the effect of increases in state Section 179 limits on eligible firms after controlling for the effect of the confounding factors. The focus on the interaction between the state Section 179 policy shock and firm eligibility as the treatment variable forms the core of our identification strategy.

Third, we control for changes in other observable time-varying state-level variables that may affect firm investment, $\Delta X_{s,t}$, including changes in state hiring credits, adoption of bonus depreciation, budget surplus, gross state product, unemployment rate, individual income tax rate, corporate tax rate, and the political affiliation of the governor.[39]

Finally, we control for firms' lagged investment to address two concerns. First, establishment-level investment is known to be lumpy.[40] Second, the federal Section 179 limits were increased in 2003, 2007, 2008, and again in 2010 (see Table I). The response to shocks that occurred in later years may be affected by the response to earlier shocks.[41] By including these controls, if firm investment responds to state Section 179 changes, we expect to find a positive estimate for b_3. We cluster standard errors at the state level.

[38] Note that the variation in $Eligible_{j,t}$ is mostly absorbed by the $Dummy_{EmpBin\times Ind\times Year}$ fixed effects as eligibility is constructed from industry-level equipment investment ratios and firm employment.

[39] In Section IV.F, we also control for the interaction between these variables and firm eligibility. We find similar results.

[40] Goolsbee and Gross (1997) and Doms and Dunne (1998), among others, empirically investigated investment dynamics using plant- or firm-level data disaggregated to capital types. Their common finding is that firms do not invest in all types of capital every period, and thus investment patterns are lumpier at the disaggregate level than at the aggregate level.

[41] As we discuss later, we show parallel pre-trends in firm investment before the initial shock in 2003 (see Figure 4). The repeated increases in federal Section 179 limits also prevent us from having a consistent set of eligible versus ineligible firms throughout our sample period and conducting a long postperiod difference-in-differences analysis as in Ohrn (2019) and Garrett, Ohrn, and Suárez Serrato (2020). In Section IV.F, we show that our results are robust to controlling for establishment fixed effects.

Table IV

Response of Computer Investments to Changes in State Section 179 Deduction Limits

This table estimates the effect of changes in state Section 179 deduction limits on establishments' investment in computers and on establishments' changes in IT intensity using the regressions in equation (6). *Computer Investment* is the growth rate of the number of computers in each establishment in year t calculated from the Computer Intelligence Technology Database (CiTDB). The variable Δ *IT Intensity* is the change in the logarithm of the number of computers per worker. The variable $\Delta Limit_{s,t}$ is the change in the maximum Section 179 deduction that a firm may claim in a year from state taxes from $t-1$ *to* t, presented in millions of dollars. The variable $Eligible_{j,t}$ is a dummy that equals 1 if the firm is eligible for the federal Section 179 in year t (see Section II.B). *Lagged Dep.Var*. is the establishment's computer investment or change in IT intensity in year $t-1$. For brevity, we do not report the following variables that are also included in the regression: the standalone eligibility dummies and contemporaneous changes in state political, economic, and other policy characteristics. All regressions include fixed effects that include a full interaction of eight employment bins, NAICS four-digit industry codes, and year. Employment bins are defined as (1, 4), (5, 9), (10, 14), (15, 24), (25, 49), (50, 99), (100, 199), and 200 or more. Standard errors are clustered at the state level and reported in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively. The sample period is 2003 to 2014.

	Computer Investments (1)	Δ IT Intensity (2)
$\Delta Limit_{s,t} \times Eligible_{j,t}$	6.70**	13.72***
	(2.88)	(4.09)
$\Delta Limit179_t$	0.50	−5.80
	(3.52)	(3.63)
Lagged Dep.Var.	−0.13***	−0.17***
	(0.00)	(0.00)
Observations	353,912	342,420
Adjusted R^2	0.21	0.21

Column (1) of Table IV reports a significantly positive coefficient for the interaction term, suggesting a positive treatment effect of increasing state Section 179 limits on eligible firms' computer investments. For eligible firms from a state that increases its Section 179 deduction limit by $250,000 , the point estimates imply a 1.7% ($6.7 \times \$250,000 \div \$1,000,000$) higher computer investment rate annually compared to similar firms from states that do not increase their Section 179 deduction limits. We also find that the coefficient on past investment is negative and highly significant, providing strong evidence for lumpy computer investment at the establishment level. Other state control variables have no effect on investment and are omitted from the table.

In column (2) of Table IV, we replace the dependent variable with changes in IT intensity, which is the (log) change in an establishment's computers per employee from year t to $t+1$. This regression can help us identify whether eligible firms respond to employment shocks by expanding all of their inputs without changing their composition or by tilting production inputs towards computers. We find that eligible firms significantly increase their IT intensity in response to state Section 179 expansion, suggesting that these firms tilt their production inputs from labor toward computers.[42]

B.2. Graphic Evidence for Investment

We next inspect the pre-trend of our treatment by graphically depicting the estimated investment effects of the first major federal Section 179 limit increase in 2003. As we discuss earlier, the repeated increases in federal Section 179 limits imply that the years before 2003 represent the only clear pretreatment years for our study. We thus run the following regression over the period 2000 to 2005:

$$\begin{aligned}\mathrm{Inv}_{j,s,t} = b_0 &+ \sum\nolimits_{k=2000}^{2005} b_{1,k}\left[Adoption_{s,2003} \times 1[\mathrm{Year}_k]\right] \\ &+ \sum\nolimits_{k=2000}^{2005} b_{2,k}\left[Eligible_{j,2003} \times 1[\mathrm{Year}_k]\right] \\ &+ \sum\nolimits_{k=2000}^{2005} b_{3,k}\left[Eligible_{j,2003} \times Adoption_{s,2003} \times 1[\mathrm{Year}_k]\right] \\ &+ b_4 \Delta X_{s,t} + Dummy_{EmpBin \times Ind \times Year} + \epsilon_{j,s,t}, \end{aligned} \tag{7}$$

where $Adoption_{s,2003}$ is a dummy that equals 1 for states that adopt the federal Section 179 limit increase in 2003, $Eligible_{j,2003}$ is a dummy that equals 1 for firms eligible for federal Section 179 in 2003, and $1[\mathrm{Year}_k]$ is a dummy that equals 1 if $t = k$. If eligible firms respond to state adoption of the 2003 federal limit increase, we expect $b_{3,k}$ to be significant for $k = 2003$ but not for years before 2003.

Figure 4 plots estimated $b_{3,k}$ for k between 2000 and 2005. We observe that the point estimates are essentially zero from 2000 and 2002, which supports the parallel pre-trend assumption prior to our 2003 treatment. In 2003, we observe a significantly positive effect, consistent with our results presented in Table IV. Specifically, the estimated $b_{3,k}$ for $k = 2003$ suggests that states' adoption of the federal Section 179 limit increase leads to a 2.1% higher computer investment rate for eligible establishments in 2003. This estimated treatment effect forms the basis for our price and substitution elasticity estimates discussed in Section V. For the years after 2003, the estimated effects are positive but statistically indistinguishable from zero, suggesting no significant delayed responses of computer investment to Section 179.

B.3. Additional Evidence by Investment Type

We corroborate our findings on computer investments by providing additional evidence from a different data set. Although CiTDB allows us to measure computer investments, which qualify for Section 179 deductions, ideally

[42] The Internet Appendix shows that increases in state Section 179 limits do not have a significant effect on establishments' total employment growth in the CiTDB sample. We thus conclude that the increase in IT intensity is due to the response of computer investments.

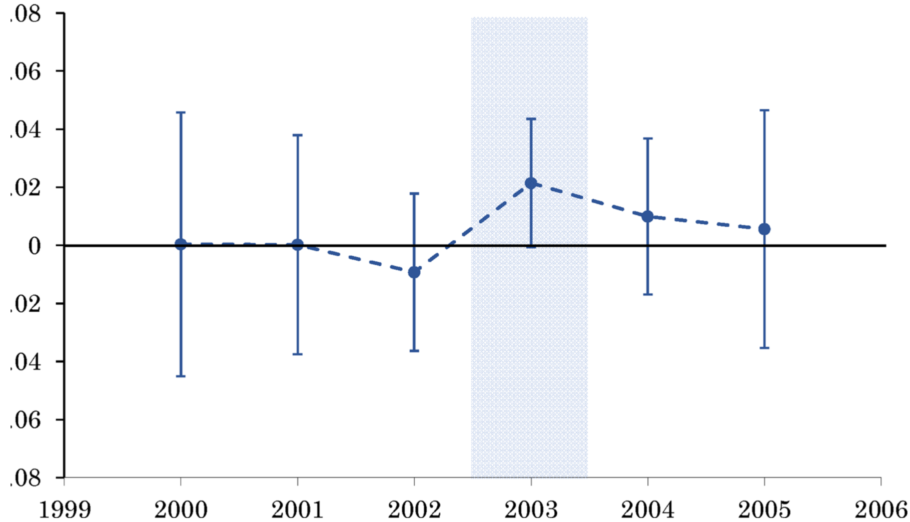


Figure 4. Estimated investment effects of increased Section 179 deduction limits in 2003. This figure shows the estimated effects of the increase in federal Section 179 deduction limits in 2003 on computer investment from 2000 to 2005. The regression specification is given in equation (7). We regress an establishment's annual computer investment rate on a dummy variable that equals 1 for states that adopt the federal limit increase in 2003, a dummy variable that equals 1 for firms that are predicted to be eligible for Section 179 in 2003, and their interaction, where all of the independent variables are further interacted with year dummies. The figure presents the point estimates of $b_{3,k}$ in the $\sum_{k=2000}^{2005} b_{3,k}[Eligible_{j,2003} \times Adoption_{s,2003} \times 1[\text{Year}_k]]$ term in equation (7), along with the 95% confidence interval. We control for contemporaneous changes in states' political, economic, and other policy characteristics. All regressions include fixed effects that include a full interaction of eight employment bins, NAICS four-digit industry codes, and year. Employment bins are defined as (1, 4), (5, 9), (10, 14), (15, 24), (25, 49), (50, 99), (100, 199), and 200 or more, based on the establishment's employment in 2003. Standard errors are clustered at the state level. The shaded area indicates the treatment year. (Color figure can be viewed at wileyonlinelibrary.com)

we would like to run tests using data on different investment categories that would allow us to distinguish the effects on qualified versus unqualified investments. In addition, to qualify for Section 179 deductions, capital must be purchased rather than leased. We explore these alternative investments using the NFIB Small Business Economic Trends database.[43] Table A.I of the Appendix shows that state Section 179 limit increases lead small businesses to increase qualified investments (e.g., equipment purchases), while there is no effect on any other investment category (notably, investments that do not qualify for Section 179, such as buildings and land or leases of any investment

[43] This database covers only NFIB member businesses, which are small firms. It is therefore safe to assume that all firms in the sample are eligible for Section 179 deductions. Firms are only surveyed once, so we cannot control for past investments in these tests. However, the data include the business entity type (C-corporation versus pass-through), which allows our tests to control for the business type and to include states that collect corporate income tax but not individual income tax.

type). These results further strengthen our conclusion based on CiTDB computer investment regressions that eligible firms increase qualified investments in response to state Section 179 limit increases.

C. Labor Outcomes

Having confirmed the effect of investment tax incentives on the investments of eligible firms, we next turn to labor outcomes. In this section, we investigate the effects on total employment, as well as the employment of routine-task, skilled, and nonroutine unskilled labor. Our conceptual framework implies a negative effect on routine labor, a positive effect on skilled labor, and no effect on nonroutine unskilled labor. These predictions are formally derived in the Internet Appendix. We do not make a prediction for total employment.

C.1. Regression Analysis for Employment

As the OES surveys each establishment once every three years, changes in employment are constructed as the growth rate from year t to year $t+3$. With each observation measuring multiple-year employment changes, policy changes over several years could affect these outcomes. We thus run the following regression by including changes in state Section 179 limits in years t, $t+1$, and $t+2$:

$$\Delta L_{j,s,t\rightarrow t+3} = b_0 + \sum_{n=0}^{2}(b_{1,n}\Delta Limit_{s,t+n} + b_{2,n}Eligible_{j,t+n} + b_{3,n}\Delta Limit_{s,t+n} \times Eligible_{j,t+n}) + \sum_{n=0}^{2} b_{4,n}\Delta X_{s,t+n} + b_5\Delta L_{j,s,t-3\rightarrow t} + Dummy_{EmpBin\times Ind\times Year} + \epsilon_{j,s,t}, \tag{8}$$

where $\Delta L_{j,s,t\rightarrow t+3}$ is the growth rate of the number of employees (total, routine, skilled, nonroutine unskilled) from year t to $t+3$, and $\Delta L_{j,s,t-3\rightarrow t}$ controls for the lagged dependent variable. The key difference between this regression specification and the investment regression in equation (6) is that here we have three treatments, represented by the three interaction terms of $\Delta Limit_{s,t+n} \times Eligible_{j,t+n}$ for $n = (0, 1, 2)$. If eligible establishments' employment responds positively to shocks, we expect some or all of $b_{3,0}$, $b_{3,1}$, and $b_{3,2}$ to be positive.

Beyond the direction of the effects, we are also interested in learning when Section 179 affects firms' employment. In particular, $b_{3,0}$, $b_{3,1}$, and $b_{3,2}$ capture the effect of policy shocks that occurred in year t, $t+1$, and $t+2$, respectively, on an eligible firm's employment change from year t to $t+3$. Note that a policy shock that occurred in $t+2$ has less time to affect employment outcomes compared to a shock that occurred in t. If a firm's employment response to the shock is not immediate but occurs with a delay, we would expect $b_{3,2}$ to be insignificant but $b_{3,0}$ to be significant. In contrast, if a firm's employment response to the shock is immediate, we would expect $b_{3,0}$, $b_{3,1}$, and $b_{3,2}$ to be all significant. If an immediate response is subject to some reversal, we would expect $b_{3,2}$ and $b_{3,1}$ to be more significant than $b_{3,0}$ because they represent shocks that occurred more recently.

Column (1) of Table V reports the effect of limit changes on total employment. All interaction terms are indistinguishable from zero, implying that state limit changes have no effect on firms' total employment. This finding is consistent with the total employment results based on CiTDB data that we report in Section IV.B, and tabulate in the Internet Appendix.

The results are drastically different, however, when we look at detailed components of the labor market. Column (2) of Table V reports the results for routine-task labor. We find that the estimated $b_{3,2}$, $b_{3,1}$, and $b_{3,0}$ move from close to zero to increasingly more negative, with only $b_{3,0}$ being statistically significant. Hence, eligible firms respond to state incentives in investment by reducing routine-task labor. Moreover, the negative effect arises with a delay rather than immediately following the investment shock, as the longer term response is large and significant, whereas the shorter term responses are not. Following our example in the investment regressions, a \$250,000 increase in a state's Section 179 deduction limit in year t corresponds to a 6% $(24.38 \times \$250,000 \div \$1,000,000)$ reduction in routine-task employment in the three years from t to $t+3$.

Column (3) of Table V reports the results for skilled labor. In sharp contrast to our routine-task labor results, here we find positive and significant estimates for $b_{3,1}$ and $b_{3,2}$, and a smaller and insignificant estimate for $b_{3,0}$. Hence, eligible firms respond to state incentives by quickly hiring more skilled labor. A \$250,000 increase in a state's Section 179 deduction limit in year $t+1$ or $t+2$ leads to an approximately 3.4% increase in skilled labor from t to $t+3$.

Column (4) of Table V reports the results for nonroutine unskilled labor. Consistent with our prediction from the conceptual framework, we find no effect of investment shocks on nonroutine unskilled labor at any horizon.

We further test whether the estimated coefficients for the three categories of labor in Table V are statistically different from each other. We confirm that the estimated $b_{3,0}$ for routine-task labor is significantly different from that for skilled labor and nonroutine unskilled labor. Hence, the delayed negative effect of Section 179 only applies to routine-task labor, consistent with our conceptual framework in which equipment capital only substitutes for routine-task labor. We also observe that the estimated $b_{3,1}$ for skilled labor is significantly different from that for routine-task labor and nonroutine unskilled labor. The estimates for $b_{3,2}$ are not significantly different from each other. Hence, while firms respond to Section 179 by immediately hiring skilled labor, such response is not always distinct from firms' treatment of other labor groups.

The regression coefficient on past employment growth is negative and significant for all labor types, implying lumpy hiring behavior. Furthermore, the coefficients for the three labor subgroups are three times larger than the coefficient on total employment, confirming that aggregating different labor groups into total employment masks the lumpiness in individual groups, a point we discuss in Section IV.B.

Table V

Response of Employment to Changes in State Section 179 Deduction Limits

This table estimates the effects of changes in state Section 179 deduction limits on establishments' total employment, and employment of routine-task employees, skilled employees, and nonroutine unskilled employees, respectively, using the regressions in equation (8). The dependent variable is the three-year growth rate of the employment metric in each establishment from year t to $t+3$. The variable $\Delta Limit_{s,t}$ is the change in the maximum Section 179 deduction that a firm may claim in a year from state taxes from $t-1$ to t, presented in millions of dollars. The variable $Eligible_{j,t}$ is a dummy variable that equals 1 if the firm is eligible for the federal Section 179 in year t (see Section II.B). *Lagged Dep.Var*. is the three-year growth rate of the employment metric for the corresponding type of employees in each establishment from year $t-3$ to t. For brevity, we do not report the following variables that are also included in the regression: the standalone eligibility dummies for each year from t to $t+2$, and contemporaneous changes in state political, economic, and other policy characteristics for each year from t to $t+2$. All regressions include fixed effects that include a full interaction of eight employment bins, NAICS four-digit industry codes, and year. Employment bins are defined as (1, 4), (5, 9), (10, 14), (15, 24), (25, 49), (50, 99), (100, 199), and 200 or more. Standard errors are clustered at the state level and reported in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively. The sample period is 2003 to 2014. The bottom table reports the probability of obtaining the chi-square statistic given that the corresponding null hypothesis is true, *Prob > Chi squared*.

	$\Delta\text{Emp}^{Tot}_{t,t+3}$ (1)	$\Delta\text{Emp}^{R}_{t,t+3}$ (2)	$\Delta\text{Emp}^{S}_{t,t+3}$ (3)	$\Delta\text{Emp}^{NU}_{t,t+3}$ (4)
$\Delta Limit_{s,t} \times Eligible_{j,t}$	−5.67	−24.38***	8.54	−4.81
	(3.62)	(8.30)	(6.57)	(5.68)
$\Delta Limit_{s,t+1} \times Eligible_{j,t+1}$	−5.32	−9.43	13.45*	−8.35
	(4.41)	(10.48)	(6.75)	(6.90)
$\Delta Limit_{s,t+2} \times Eligible_{j,t+2}$	4.00	0.56	13.84*	3.35
	(3.85)	(10.45)	(7.28)	(7.99)
$\Delta Limit_{s,t}$	5.63*	5.83	−1.34	13.07**
	(3.11)	(9.49)	(7.07)	(5.36)
$\Delta Limit_{s,t+1}$	5.90	0.03	−6.30	14.60**
	(4.72)	(9.23)	(6.50)	(6.12)
$\Delta Limit_{s,t+2}$	−2.91	−4.10	−6.94	−1.20
	(3.03)	(8.20)	(6.85)	(8.09)
Lagged Dep.Var.	−0.15***	−0.45***	−0.40***	−0.40***
	(0.01)	(0.00)	(0.01)	(0.01)
Observations	329,943	269,784	302,873	304,617
Adjusted R^2	0.11	0.23	0.20	0.20

Comparing regression coefficients across three labor groups (Prob > Chi squared)

H0:	$\beta^R = \beta^S$	$\beta^R = \beta^{NU}$	$\beta^S = \beta^{NU}$	$\beta^R = \beta^S = \beta^{NU}$
$\Delta Limit_{s,t} \times Eligible_{j,t}$	0.0006	0.0526	0.0766	0.0022
$\Delta Limit_{s,t+1} \times Eligible_{j,t+1}$	0.0283	0.9180	0.0066	0.0109
$\Delta \text{Limit}_{s,t+2} \times \text{Eligible}_{j,t+2}$	0.2428	0.8320	0.3627	0.4358

Table VI
Response of Wage Bills to Changes in State Section 179 Deduction Limits

This table reports the effects of changes in state Section 179 deduction limits on establishments' wage bills for all employees, routine-task employees, skilled employees, and nonroutine unskilled employees, using the regressions specified in equation (8). See Table V for variable definitions and more details on the regression specification. Standard errors are clustered at the state level and reported in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively. The sample period is 2003 to 2014. The bottom table reports the probability of obtaining the chi-square statistic given that the corresponding null hypothesis is true, *Prob > Chi squared*.

	$\Delta\text{WageBill}^{Tot}_{t,t+3}$ (1)	$\Delta\text{WageBill}^{R}_{t,t+3}$ (2)	$\Delta\text{WageBill}^{S}_{t,t+3}$ (3)	$\Delta\text{WageBill}^{NU}_{t,t+3}$ (4)
$\Delta Limit_{s,t} \times Eligible_{j,t}$	−7.83**	−25.47***	4.17	−6.18
	(3.68)	(8.38)	(6.37)	(6.02)
$\Delta Limit_{s,t+1} \times Eligible_{j,t+1}$	−1.96	−5.95	9.89	−6.71
	(4.98)	(10.89)	(6.62)	(7.55)
$\Delta Limit_{s,t+2} \times Eligible_{j,t+2}$	9.40**	3.05	18.55***	7.91
	(3.68)	(9.73)	(6.35)	(7.93)
$\Delta Limit_{s,t}$	9.20**	5.66	3.52	15.24**
	(3.67)	(9.57)	(6.67)	(5.76)
$\Delta Limit_{s,t+1}$	2.98	−5.40	−2.44	13.27*
	(4.89)	(9.45)	(6.56)	(6.62)
$\Delta Limit_{s,t+2}$	−7.66**	−6.59	−11.31*	−5.54
	(2.94)	(7.79)	(5.98)	(8.23)
Lagged Dep.Var.	−0.18***	−0.45***	−0.40***	−0.40***
	(0.00)	(0.00)	(0.01)	(0.01)
Observations	329,943	269,784	302,873	304,617
Adjusted R^2	0.11	0.23	0.19	0.20

Comparing regression coefficients across three labor groups (Prob > Chi squared)

H0:	$\beta^R = \beta^S$	$\beta^R = \beta^{NU}$	$\beta^S = \beta^{NU}$	$\beta^R = \beta^S = \beta^{NU}$
$\Delta Limit_{s,t} \times Eligible_{j,t}$	0.0013	0.0509	0.1752	0.0054
$\Delta Limit_{s,t+1} \times Eligible_{j,t+1}$	0.1105	0.9430	0.0305	0.0584
$\Delta Limit_{s,t+2} \times Eligible_{j,t+2}$	0.1478	0.6892	0.3164	0.3054

Above we document that the two labor groups that are significantly affected by the investment tax incentives—routine-task and skilled labor—earn significantly different wages (Figure 3). How does a firm's payroll (rather than the number of employees) respond to the investment policy shocks? To address this question, we replicate our main labor results using wage bills (wage rate × employment) instead of employment counts. Table VI reports these results, which are generally similar to our employment results reported in Table V. In particular, we find a significant and delayed negative response for the routine-task employees' wage bill, an immediate positive response for the wage bill of skilled labor, and no effect on the wage bill of nonroutine unskilled labor. The effect on the total wage bill is more nuanced. Consistent with the quick rise in skilled labor, firms' total wage bill initially rises. However, the later decline in routine-task labor leads to a longer term decline in the total wage bill in response to increases in Section 179 deduction limits.

C.2. Graphic Evidence for Employment

Similar to our investment analysis, we inspect the pre-trend of our treatment by graphically illustrating the estimated employment effects of the increase in federal Section 179 limits in 2003. To estimate the employment effects of the 2003 limit increase, we run the following regression from $t = 1999$ to 2004:

$$\begin{aligned}\Delta L_{j,s,t\to t+3} = b_0 &+ \sum\nolimits_{k=1999}^{2004} b_{1,k}\big[Adoption_{s,2003} \times 1[\text{Year}_k]\big] \\ &+ \sum\nolimits_{k=1999}^{2004} b_{2,k}\big[Eligible_{j,2003} \times 1[\text{Year}_k]\big] \\ &+ \sum\nolimits_{k=1999}^{2004} b_{3,k}\big[Eligible_{j,2003} \times Adoption_{s,2003} \times 1[\text{Year}_k]\big] \\ &+ b_4 \Delta X_{s,t\to t+3} + Dummy_{EmpBin\times Ind\times Year} + \epsilon_{j,s,t}, \end{aligned} \tag{9}$$

where $Adoption_{s,2003}$ is a dummy that equals 1 for states that adopt the federal limit increase in 2003, $Eligible_{j,2003}$ is a dummy that equals 1 for firms eligible for Section 179 deductions in 2003, and $1[\text{Year}_k]$ is a dummy that equals 1 if $t = k$. Note that the dependent variable is the three-year growth rate from year t to $t + 3$. The 1999 to 2002 and 2000 to 2003 periods represent the pretreatment period; the 2001 to 2004, 2002 to 2005, and 2003 to 2006 periods encompass the year of the limit increase (2003) and represent the treatment period; and the 2004 to 2007 period represents the posttreatment period. Furthermore, the limit increase in 2003 has a shorter time to affect employment growth over the 2001 to 2004 and 2002 to 2005 periods compared to the 2003 to 2006 period. Therefore, significant estimates of $b_{3,k}$ for $k = 2001$ and 2002 would indicate an immediate response, while significant estimates of $b_{3,k}$ for $k = 2003$ would indicate a delayed response.

Figure 5 plots estimates of $b_{3,k}$ for $k = 1999$ to 2004 for the routine-task, skilled, nonroutine unskilled, and total employment groups. Panel A shows that $b_{3,k}$ is indistinguishable from zero for the 1999 to 2002 and 2000 to 2003 periods, which supports the parallel pre-trend assumption for routine-task labor prior to the 2003 limit increase. The estimated $b_{3,k}$ is insignificant for the 2001 to 2004 and 2002 to 2005 periods but negative and statistically significant for the 2003 to 2006 period. Therefore, we observe a delayed response of firms' routine-task labor to a Section 179 limit increase, similar to our earlier findings in Table V. The average $b_{3,k}$ for k =2001, 2002, and 2003 suggests that states' adoption of the 2003 federal Section 179 limit increase leads to 1.8% lower routine-task employment for eligible establishments.

Panel B plots the estimates for skilled employment. The figure shows that estimates of $b_{3,k}$ are insignificant in the pretreatment period but significantly positive over the 2002 to 2005 period, indicating an immediate response of

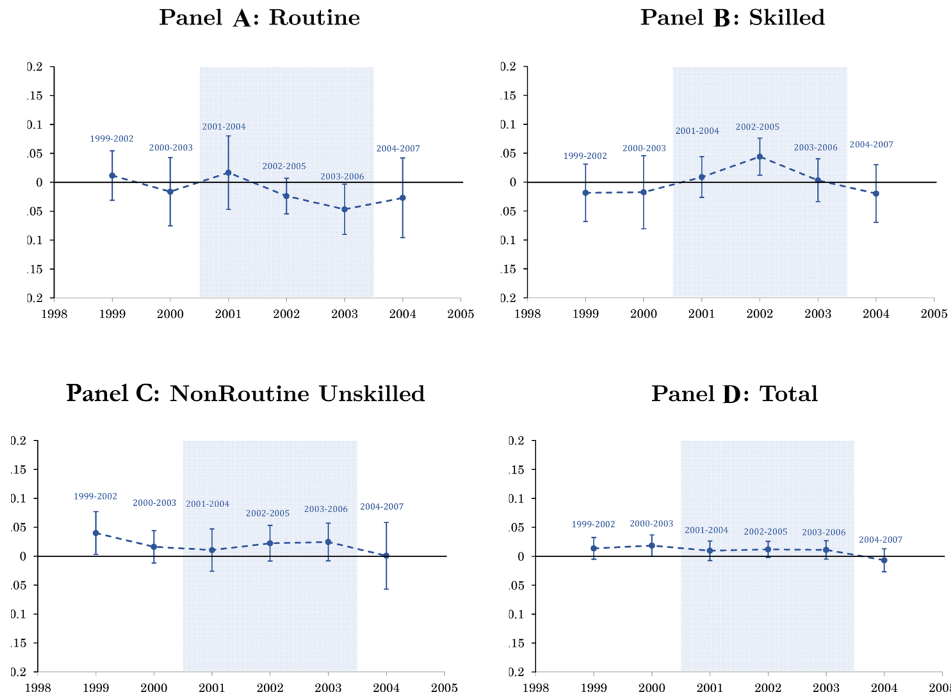


Figure 5. Estimated employment effects of increased Section 179 deduction limits in 2003. This figure shows the estimated effects of the increase in federal Section 179 deduction limits in 2003 on routine employment, skilled employment, nonroutine unskilled employment, and total employment. The regression specification is given in equation (9). We regress an establishment's three-year employment growth on a dummy variable that equals 1 for states that adopt the federal limit increase in 2003, a dummy variable that equals 1 for firms that are predicted to be eligible for Section 179 deductions in 2003, and their interaction, where all of the independent variables are further interacted with year dummies. Panels A to D present the point estimates of $b_{3,k}$ in the $\sum_{k=1999}^{2004} b_{3,k}[Eligible_{j,2003} \times Adoption_{s,2003} \times 1[\text{Year}_k]]$ term in equation (9), along with the 95% confidence interval for routine, skilled, nonroutine unskilled, and total employment, respectively. We control for contemporaneous changes in states' political, economic, and other policy characteristics. All regressions include fixed effects that include a full interaction of eight employment bins, NAICS four-digit industry codes, and year. Employment bins are defined as (1, 4), (5, 9), (10, 14), (15, 24), (25, 49), (50, 99), (100, 199), and 200 or more, based on the establishment's employment in 2003. Standard errors are clustered at the state level. The shaded areas indicate treated observations, that is the three-year period over which employment growth is measured includes the treatment year. (Color figure can be viewed at wileyonlinelibrary.com)

firms' skilled labor to Section 179. The average $b_{3,k}$ for $k = 2001$, 2002, and 2003 suggests that states' adoption of the 2003 federal Section 179 limit increase leads to 1.9% higher skilled employment for eligible establishments. Panels C and D plot the estimates for nonroutine unskilled and total employment, respectively. All coefficients in the treatment period are indistinguishable from zero, consistent with our earlier findings based on staggered Section 179 increases.

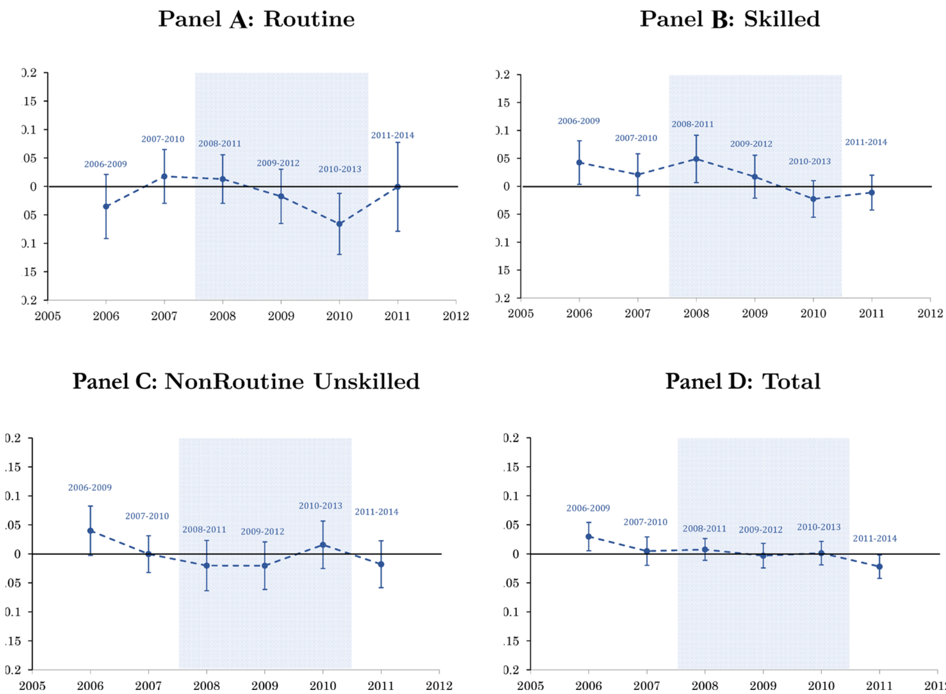


Figure 6. Estimated employment effects of increased Section 179 deduction limits in 2010. This figure shows the estimated effects of the increase in federal Section 179 deduction limits in 2010 on routine employment, skilled employment, nonroutine unskilled employment, and total employment. The regression specification is similar to equation (9). We regress an establishment's three-year employment growth on a dummy variable that equals 1 for states that adopt the federal limit increase in 2010, a dummy variable that equals 1 for firms that are predicted to be eligible for Section 179 deductions in 2010, and their interaction, where all of the independent variables are further interacted with year dummies. Panels A to D present the point estimates of $b_{3,k}$ in the $\sum_{k=2006}^{2011} b_{3,k}[Eligible_{j,2010} \times Adoption_{s,2010} \times 1[\text{Year}_k]]$ term in equation (9), along with the 95% confidence interval for routine, skilled, nonroutine unskilled, and total employment, respectively. We control for contemporaneous changes in states' political, economic, and other policy characteristics. All regressions include fixed effects that include a full interaction of eight employment bins, NAICS four-digit industry codes, and year. Employment bins are defined as (1, 4), (5, 9), (10, 14), (15, 24), (25, 49), (50, 99), (100, 199), and 200 or more, based on the establishment's employment in 2010. Standard errors are clustered at the state level. The shaded areas indicate treated observations, that is the three-year period over which employment growth is measured includes the treatment year. (Color figure can be viewed at wileyonlinelibrary.com)

Figure 6 plots the estimates from the last major increase in the federal Section 179 limits in 2010, which doubled the deduction limit from $250,000 to $500,000. We run the regression specification in equation (9) from 2006 to 2011 and replace the independent variables with their 2010 counterparts. The results are similar to those in Figure 5, including a delayed negative response from routine-task employment, an immediate positive response from skilled employment, and no significant responses from nonroutine unskilled employment or total employment during the treatment period.

A notable difference in Figure 6 is that we do not observe parallel trends for skilled employment growth and total employment growth prior to the 2010 shock. This is intuitive as the federal government increased Section 179 limits several times prior to the 2010 shock—in 2003, 2007, and 2008. The lagged employment growth thus reflects important heterogeneous trends due to past shocks. This observation highlights the importance of controlling for lagged employment growth in our baseline specification.

C.3. Assessing the Economic Magnitudes

How does the labor market accommodate the changing labor demand of eligible firms induced by Section 179? To gain insight into this question, we conduct a back-of-the-envelope calculation on the magnitude of Section 179's effects on different types of labor, and compare these magnitudes to overall mobility across different types of jobs. We use the 2003 federal limit increase to conduct this calculation. Our sample shows that eligible firms account for 20.3% of all skilled and 25.1% of all routine-task employment in adopting states in 2002. We combine these magnitudes with the estimated 1.9% increase in skilled labor and 1.8% decline in routine-task labor in eligible firms in adopting states due to the 2003 federal limit increase. Our treatment effects on eligible firms translate into approximately 0.39% (= 1.9% $\times$ 20.3%) incremental demand for skilled labor and 0.45% (= 1.8% $\times$ 25.1%) incremental supply of routine-task labor in adopting states.

We next analyze the mobility of workers across different job categories in adopting states as a benchmark for evaluating the magnitude of the Section 179 effects. Our establishment-level data do not track workers and hence do not provide information on a worker's prior or future employment. In the Internet Appendix, we use Annual Social and Economic Supplement to the Current Population Survey (CPS ASEC) data to examine individual workers' annual transition propensity among five employment categories over the period 2003 to 2006. These employment categories are working in skilled occupations, working in routine-task occupations, working in nonroutine unskilled occupations, unemployed but in the labor force, and out of the labor force, where the skilled, routine-task, and unskilled nonroutine jobs are classified based on our definitions.[44] We observe that 21.7% of employees who work in skilled occupations were not in skilled occupations in the prior year, with 12.7% moving from nonroutine unskilled occupations, 4.9% from routine occupations, and 4% from outside the labor force. Hence, the market for skilled labor in adopting states seems to be highly dynamic, and can potentially meet the 0.39% incremental demand for skilled labor from eligible firms due to states' adoption of the Section 179 incentive. As eligible firms compete with ineligible firms in local labor markets, we expect eligible firms to offer higher wage rates to quickly recruit

[44] Using the CPS personal permanent identifier, "cpsidp," we observe each individual's prior labor force participation status and occupation. We crosswalk skilled occupations in our occupation code (SOC) to the Census occupation code using the concordance provided on the Census website.

Table VII

Response of Wage Rates to Changes in State Section 179 Deduction Limits

This table reports the effects of changes in state Section 179 deduction limits on establishments' wage rates for all employees, routine-task employees, skilled employees, and nonroutine unskilled employees, using the regressions specified in equation (8). See Table V for variable definitions and more details on the regression specification. Standard errors are clustered at the state level and reported in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively. The sample period is 2003 to 2014. The bottom table reports the probability of obtaining the chi-squared statistic given that the corresponding null hypothesis is true, *Prob > Chi squared*.

	$\Delta \text{WageRate}^{Tot}_{t,t+3}$ (1)	$\Delta \text{WageRate}^{R}_{t,t+3}$ (2)	$\Delta \text{WageRate}^{S}_{t,t+3}$ (3)	$\Delta \text{WageRate}^{NU}_{t,t+3}$ (4)
$\Delta Limit_{s,t} \times Eligible_{j,t}$	-0.04^{*}	-0.02	-0.05^{*}	-0.02
	(0.02)	(0.03)	(0.03)	(0.02)
$\Delta Limit_{s,t+1} \times Eligible_{j,t+1}$	0.03	0.04	-0.05	0.01
	(0.02)	(0.03)	(0.04)	(0.02)
$\Delta Limit_{s,t+2} \times Eligible_{j,t+2}$	0.06^{***}	0.03	0.09^{***}	0.03
	(0.02)	(0.03)	(0.03)	(0.02)
$\Delta Limit_{s,t}$	0.05^{**}	0.02	0.06^{*}	0.04
	(0.02)	(0.03)	(0.03)	(0.03)
$\Delta Limit_{s,t+1}$	-0.01	-0.04^{*}	0.08^{**}	-0.00
	(0.02)	(0.02)	(0.03)	(0.02)
$\Delta Limit_{s,t+2}$	-0.04	-0.02	-0.05^{*}	-0.03
	(0.02)	(0.03)	(0.03)	(0.02)
Lagged Dep.Var.	-0.29^{***}	-0.35^{***}	-0.28^{***}	-0.31^{***}
	(0.00)	(0.00)	(0.00)	(0.00)
Observations	329,943	198,250	270,494	262,398
Adjusted R^2	0.11	0.16	0.12	0.15

Comparing regression coefficients across three labor groups (Prob > Chi squared)

H0:	$\beta^R = \beta^S$	$\beta^R = \beta^{NU}$	$\beta^S = \beta^{NU}$	$\beta^R = \beta^S = \beta^{NU}$
$\Delta Limit_{s,t} \times Eligible_{j,t}$	0.3633	0.9946	0.3293	0.5849
$\Delta Limit_{s,t+1} \times Eligible_{j,t+1}$	0.0266	0.2977	0.1012	0.0820
$\Delta Limit_{s,t+2} \times Eligible_{j,t+2}$	0.0511	0.9576	0.1088	0.0881

skilled employees. Consistent with this view, in Table VII we find that Section 179 has an immediate positive effect on the wage rates of skilled labor in eligible firms. This effect on wage rates explains the immediate positive effect on the employment of skilled labor in eligible firms.

Our analysis of the CPS data also reveals that more than half of the current routine-task workers transition out of these types of jobs in the next year, with 24.9% entering nonroutine unskilled occupations, 14.9% entering skilled occupations, 11.7% exiting the labor force, and 0.1% becoming unemployed. The high propensity of transitioning from routine-task jobs to nonroutine unskilled jobs is consistent with Autor and Dorn (2013), who show that the decline of routine-task labor accelerated the rise of service industries that provide many nonroutine unskilled jobs in local economies. These dynamics provide a potential resolution to the 0.45% decline in routine-task employment at eligible firms due to states' adoption the of Section 179 incentive.

D. Sensitivity to State Tax Rates

Section 179 deductions benefit firms by reducing their corporate income tax bills (for C-corporations) or the individual income tax bills of their owners (for pass-through entities). Marginal tax rates vary across states and over time, and the benefit of Section 179 deductions increases with the tax rates. In Tables VIII and IX, we explore the effect of changes in states' Section 179 limits conditional on states' corporate and individual tax rates, respectively. In this analysis, we interact our treatment variable, $\Delta Limit_{s,t}$, with the marginal state tax rates. Both sets of results corroborate our baseline results: we observe a strong positive effect on eligible firms' computer investments, an immediate positive effect on the employment and wage bills of skilled labor, and a strong delayed negative effect on the employment and wage bills of routine-task labor. These results reinforce the view that our baseline effects are due to taxes and validate the mechanism that we propose and test in Sections B and C.

E. Sensitivity to Availability of External Financing

Section 179 deductions provide firms with a financial incentive to invest, but deductions alone are not sufficient to cover the investment cost. Firms still need additional internal or external funds to finance eligible investments. Small firms targeted by Section 179 are known to be more financially constrained than large businesses. Hence, easier access to external financing may intensify the response to the incentive. In this section, we explore whether geographic variation in access to small business lending is related to the propensity of firms to take up these investment incentives.

A large literature on financial constraints and small businesses, summarized by Berger, Bouwman, and Kim (2017), documents that small businesses targeted by Section 179 rely heavily on relationship lending based on soft, qualitative information (rather than hard information such as financial ratios from audited statements). Small banks are typically viewed as superior at using soft information because such information is easier to communicate within a small organization with fewer layers of management. Berger, Bouwman, and Kim (2017) show that the prevalence of small banks in an area increases the availability of external financing to small firms. Similarly, Chen, Hanson, and Stein (2017) show that in counties where the largest banks had a high market share, the aggregate flow of small business credit fell from 2008 to 2014. Following their lead, we calculate $SmallBankShare_{c,t}$, the deposit share of small banks (defined as banks with total assets below \$50 billion dollars) in each county based on information from quarterly bank Call Reports. The $SmallBankShare_{c,t}$ variable captures local firms' access to small banks, and more generally, the availability of external financing to small firms.

In Table X, we examine the effect of changes in state Section 179 limits on firms' investment and employment conditional on the $SmallBankShare_{c,t}$

Table VIII

Responses of Investment and Employment to the Interaction of State Deduction Limit Changes and State Corporate Income Tax Rates

This table reports the effects of changes in state Section 179 deduction limits, interacted with marginal state corporate income tax rates ($\Delta Limit^c_{s,t} = \Delta Limit_{s,t} \times \tau^c_{s,t}$), on computer investments and employment metrics based on the regression specifications in equations (6) and (8). See Tables IV and V for variable definitions and more details on the regression specifications. Standard errors are clustered at the state level and reported in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively. The sample period is 2003 to 2014.

Panel A: Investment Regressions

	Computer Investments	Δ IT Intensity
	(1)	(2)
$\Delta Limit^c_{s,t} \times Eligible_{j,t}$	0.79**	1.80***
	(0.38)	(0.49)
$\Delta Limit^c_{s,t}$	−0.04	−1.06**
	(0.41)	(0.41)
Lagged Dep.Var.	−0.13***	−0.17***
	(0.00)	(0.00)
Observations	353,912	342,420
Adjusted R^2	0.21	0.21

	Panel B: Employment Regressions				Panel C: Wage Bill Regressions			
	Tot	R	S	NU	Tot	R	S	NU
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\Delta Limit^c_{s,t} \times Eligible_{j,t}$	−0.71*	−3.07**	1.54*	−0.86	−0.83**	−3.19**	0.95	−0.88
	(0.37)	(1.30)	(0.82)	(0.75)	(0.40)	(1.28)	(0.79)	(0.74)
$\Delta Limit^c_{t+1} \times Eligible_{j,t+1}$	−0.70	−1.60	2.17**	−1.18	−0.21	−1.21	1.69*	−1.07
	(0.50)	(1.36)	(0.94)	(0.77)	(0.57)	(1.42)	(0.87)	(0.85)
$\Delta Limit^c_{t+2} \times Eligible_{j,t+2}$	0.62	−0.01	2.10**	0.08	1.20***	0.20	2.48***	0.54
	(0.48)	(1.59)	(0.89)	(0.88)	(0.44)	(1.49)	(0.82)	(0.80)
$\Delta Limit^c_{s,t}$	0.70*	1.15	−0.42	1.64**	1.07***	1.15	0.27	1.88**
	(0.35)	(1.20)	(0.88)	(0.65)	(0.39)	(1.19)	(0.80)	(0.70)
$\Delta Limit^c_{t+1}$	0.93*	0.73	−0.62	1.87**	0.59	0.15	−0.01	1.79**
	(0.53)	(1.25)	(0.75)	(0.74)	(0.54)	(1.34)	(0.73)	(0.81)
$\Delta Limit^c_{t+2}$	−0.25	−0.21	−0.86	0.31	−0.67	−0.36	−1.08	−0.08
	(0.44)	(1.23)	(0.82)	(0.81)	(0.43)	(1.24)	(0.77)	(0.76)
Lagged Dep.Var.	−0.15***	−0.45***	−0.40***	−0.40***	−0.18***	−0.45***	−0.40***	−0.40***
	(0.01)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)
Observations	329,943	269,784	302,873	304,617	329,943	269,784	302,873	304,617
Adjusted R^2	0.11	0.23	0.20	0.20	0.11	0.23	0.19	0.20

of a firm's county. We do so by adding interaction terms between all key independent variables and $SmallBankShare_{c,t}$ as well as the standalone $SmallBankShare_{c,t}$ to our baseline regression specifications in equations (6) and (8). In the investment regressions, we find that the coefficient on the triple interaction term is positive and highly significant, implying that firms respond

Table IX

Responses of Investment and Employment to the Interaction of State Deduction Limit Changes and State Individual Income Tax Rates

This table reports the effects of changes in state Section 179 deduction limits, interacted with marginal state individual income tax rates ($\Delta Limit^{i}_{s,t} = \Delta Limit_{s,t} \times \tau^{i}_{s,t}$), on computer investments and employment metrics based on the regression specifications in equations (6) and (8). See Tables IV and V for variable definitions and more details on the regression specifications. Standard errors are clustered at the state level and reported in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively. The sample period is 2003 to 2014.

Panel A: Investment Regressions

	Computer Investments (1)	Δ IT Intensity (2)
$\Delta Limit^{i}_{s,t} \times Eligible_{j,t}$	1.04**	1.97***
	(0.49)	(0.62)
$\Delta Limit^{i}_{s,t}$	0.12	−0.86*
	(0.66)	(0.46)
Lagged Dep.Var.	−0.13***	−0.17***
	(0.00)	(0.00)
Observations	353,912	342,420
Adjusted R^2	0.21	0.21

	Panel B: Employment Regressions				Panel C: Wage Bill Regressions			
	Tot (1)	R (2)	S (3)	NU (4)	Tot (5)	R (6)	S (7)	NU (8)
$\Delta Limit^{i}_{s,t} \times Eligible_{j,t}$	−0.72	−3.16**	1.42	−0.91	−0.93*	−3.10**	0.82	−1.06
	(0.50)	(1.24)	(1.06)	(0.87)	(0.56)	(1.23)	(1.01)	(0.94)
$\Delta Limit^{i}_{t+1} \times Eligible_{j,t+1}$	−1.10*	−1.52	1.93*	−2.14**	−0.81	−0.93	1.13	−2.09*
	(0.63)	(1.55)	(0.96)	(0.97)	(0.69)	(1.56)	(0.93)	(1.05)
$\Delta Limit^{i}_{t+2} \times Eligible_{j,t+2}$	0.74	−0.23	2.40**	0.43	1.40**	0.12	2.96***	1.02
	(0.54)	(1.66)	(1.15)	(1.14)	(0.52)	(1.54)	(1.08)	(1.09)
$\Delta Limit^{i}_{s,t}$	0.77*	0.81	0.23	1.89**	1.20**	0.64	0.87	2.24**
	(0.43)	(1.52)	(1.15)	(0.78)	(0.50)	(1.48)	(1.07)	(0.85)
$\Delta Limit^{i}_{t+1}$	1.15*	0.58	−0.85	2.87***	0.84	−0.28	−0.11	2.84***
	(0.65)	(1.39)	(0.91)	(0.91)	(0.65)	(1.40)	(0.86)	(0.97)
$\Delta Limit^{i}_{t+2}$	−0.67	−0.53	−1.67	0.03	−1.31***	−0.94	−2.25**	−0.63
	(0.48)	(1.27)	(1.19)	(1.14)	(0.46)	(1.21)	(1.10)	(1.12)
Lagged Dep.Var.	−0.15***	−0.45***	−0.40***	−0.40***	−0.18***	−0.45***	−0.40***	−0.40***
	(0.01)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)
Observations	329,943	269,784	302,873	304,617	329,943	269,784	302,873	304,617
Adjusted R^2	0.11	0.23	0.20	0.20	0.11	0.23	0.19	0.20

to Section 179 more strongly in counties with easier access to external financing. On the employment side, we also find a stronger result for the delayed routine employment response to Section 179 in counties with a greater small bank presence. However, small bank share does not seem to be related to the response of skilled labor. Overall, our results are consistent with the view that

Table X

Responses of Investment and Employment to Changes in State Section 179 Deduction Limits Conditional on County Small Bank Share

This table estimates the effects of changes in state Section 179 deduction limits on computer investments and employment metrics conditional on the deposit share of small banks in the county by running the regressions in equations (6) and (8) with additional interaction terms with small bank share. The dependent variables are annual investment measures and three-year growth rate of the employment metrics in each establishment. $SmallBankShare_{c,t}$ is the deposit share of small banks in the establishments' county, where small banks are defined as banks with gross total assets below $50 billion (in 2005 dollars) following Chen, Hanson, and Stein (2017). See Tables IV and V for definitions of other variables and more details on the regression specifications. Standard errors are clustered at the state level and reported in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively. The sample period is 2003 to 2014.

Panel A: Investment Regressions

	Computer Investments (1)	Δ IT Intensity (2)
$\Delta Limit_{s,t} \times Eligible_{j,t} \times SmallBankShare_{c,t}$	0.27**	0.38**
	(0.13)	(0.16)
$\Delta Limit_{s,t} \times Eligible_{j,t}$	−8.55	−7.65
	(8.46)	(9.97)
Lagged Dep.Var.	−0.13***	−0.17***
	(0.00)	(0.00)
Observations	353,602	342,123
Adjusted R^2	0.21	0.21

(*Continued*)

Table X—*Continued*

	Panel B: Employment Regressions				Panel C: Wage Bill Regressions			
	Tot (1)	R (2)	S (3)	NU (4)	Tot (5)	R (6)	S (7)	NU (8)
$\Delta Limit_{s,t} \times Eligible_{j,t} \times SmallBankShare_{c,t}$	−0.17	−0.56**	−0.26	−0.29	−0.15	−0.66**	−0.23	−0.26
	(0.14)	(0.25)	(0.21)	(0.32)	(0.13)	(0.25)	(0.21)	(0.31)
$\Delta Limit_{s,t+1} \times Eligible_{j,t+1} \times SmallBankShare_{c,t+1}$	−0.06	0.00	−0.17	0.18	−0.01	0.05	−0.12	0.24
	(0.11)	(0.27)	(0.17)	(0.25)	(0.11)	(0.27)	(0.16)	(0.24)
$\Delta Limit_{s,t+2} \times Eligible_{j,t+2} \times SmallBankShare_{c,t+2}$	0.06	0.15	0.03	0.40*	0.09	0.11	0.15	0.35*
	(0.10)	(0.26)	(0.20)	(0.20)	(0.11)	(0.26)	(0.20)	(0.18)
$\Delta Limit_{s,t} \times Eligible_{j,t}$	5.05	8.16	24.59	13.76	1.39	12.85	18.25	10.12
	(10.23)	(15.81)	(15.65)	(22.20)	(10.08)	(15.81)	(15.24)	(22.41)
$\Delta Limit_{s,t+1} \times Eligible_{j,t+1}$	−1.61	−9.15	23.45**	−18.10	−1.72	−9.21	16.65	−20.36
	(8.56)	(18.81)	(10.49)	(17.20)	(8.61)	(19.52)	(11.17)	(17.32)
$\Delta Limit_{s,t+2} \times Eligible_{j,t+2}$	1.49	−7.27	12.48	−18.24	4.64	−2.31	10.62	−10.55
	(7.73)	(16.89)	(14.79)	(11.53)	(8.37)	(17.01)	(14.00)	(10.96)
Lagged Dep.Var.	−0.15***	−0.45***	−0.40***	−0.40***	−0.18***	−0.45***	−0.40***	−0.40***
	(0.01)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)
Observations	329,092	269,258	302,094	303,828	329,092	269,258	302,094	303,828
Adjusted R^2	0.11	0.23	0.20	0.20	0.11	0.23	0.19	0.20

access to external financing through small banks increases the take up of Section 179 and strengthens the economic response to the incentive.

F. Additional Robustness

We next check the robustness of our main results, presented in Tables IV and V, to various regression specifications, control variables, and variable definitions. We present the key regression coefficients from these tests in Table XI, and we report the full regression results as well as additional checks in the Internet Appendix.

First, we add establishment fixed effects to the regressions. Our baseline regressions already control for several observable establishment characteristics, including the establishment's lagged investment and employment, and fixed effects of a full interaction of establishments' employment bin, NAICS four-digit industry code, and year to ensure that we are comparing similar establishments. In the second row of Table XI, we add establishment fixed effects to control for any remaining unobserved time-invariant differences across establishments. We confirm that specifications with establishment fixed effects yield very similar results to our baseline (in terms of both point estimates and statistical significance). However, including establishment fixed effects significantly reduces our sample size. The reduction is especially severe for the employment data, where we lose more than 40% of the sample with establishment fixed effects.[45]

Second, we check whether the results are robust to adding additional state controls. The third row of Table XI presents key coefficients with state-year fixed effects that account for unobservable time-varying state-level factors (such as heterogeneous state-level impacts of the Great Recession). In the fourth and fifth rows, we control for the interaction between firm eligibility and changes in states' adoption of the federal bonus depreciation incentive and in states' Gross State Product (GSP), respectively. These two specifications account for the heterogeneous impacts of state adoption of another major investment tax incentive and changes in states' economic conditions, respectively, on the investment and employment outcomes of eligible firms. We again observe that our baseline findings are robust to these controls.

Third, we require that eligible firms be single-establishment businesses. This robustness check addresses two potential concerns about the sample of firms that we classify as eligible. As we discuss in Section III, in our baseline analysis we define eligibility at the establishment level, effectively assuming that each establishment is a separate entity for tax purposes. Therefore, small establishments from large ineligible firms are classified as eligible for Section 179, adding noise to our baseline analysis. In addition, eligible firms that operate in multiple states need to apportion their taxable income across states and thus are subject to policy shocks from multiple states. In the sixth row of

[45] BLS surveys establishments over three-year cycles. Adding establishment fixed effects to our baseline specification requires each establishment to be surveyed at least four times.

Table XI

Robustness of Main Results

This table reports four key coefficients from the investment and employment regressions with alternative regression specifications, control variables, and subsamples. The first column presents the investment response (b_3 in equation (6)), the second column measures the delayed routine-task employment response ($b_{3,0}$ in equation (8)), and the last two columns measure the immediate skilled employment response ($b_{3,1}$ and $b_{3,2}$ in equation (8)) to changes in state Section 179 limits. The first row summarizes the baseline results from Tables IV and V, while each of the other rows presents results from a different robustness exercise. See Tables IV and V for variable definitions and more details on the specifications for the baseline regressions. Standard errors are clustered at the state level and reported in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively. The sample period is 2003 to 2014. The Internet Appendix provides complete results for each regression.

	CiTDB	OES Employment Regressions		
	Computer Investments $\Delta\text{Limit}_{s,t} \times \text{Eligible}_{j,t}$	Routine-Task $\Delta\text{Limit}_{s,t} \times \text{Eligible}_{j,t}$	Skilled $\Delta\text{Limit}_{s,t+1} \times \text{Eligible}_{j,t+1}$	Skilled $\Delta\text{Limit}_{s,t+2} \times \text{Eligible}_{j,t+2}$
Baseline Results	6.70**	−24.38***	13.45*	13.84*
	(2.88)	(8.30)	(6.75)	(7.28)
Adding Establishment Fixed Effects	8.48**	−34.19**	19.00*	16.34*
	(3.45)	(14.16)	(10.04)	(9.62)
Adding State×Year Fixed Effects	7.05**	−22.65***	12.58*	13.81**
	(2.82)	(8.44)	(6.56)	(6.81)
Adding State ΔBonus Adoption×Eligible	7.12**	−25.81***	15.22**	14.60**
	(3.05)	(8.68)	(6.62)	(6.83)
Adding State ΔGSP ×Eligible	6.83**	−24.39***	13.62*	13.51*
	(2.86)	(8.33)	(6.82)	(7.08)
Excluding Multi-Estab. Eligible Firms	7.63**	−19.57**	17.87**	13.38*
	(3.48)	(8.90)	(7.67)	(7.41)
IT-Intensive Industries	10.08**	−28.43**	19.64**	9.39
	(4.45)	(11.02)	(8.58)	(7.69)
Non-IT-Intensive Industries	0.73	−17.92	4.22	22.14**
	(6.87)	(13.16)	(12.74)	(9.76)
Service-Providing Industries	7.94**	−26.87***	13.42	15.45*
	(3.45)	(9.56)	(8.33)	(8.10)
Goods-Producing Industries	−4.61	−15.04	15.58	7.91
	(13.24)	(17.41)	(11.36)	(12.66)

Table XI, we restrict eligible firms to single-establishment firms.[46] This restriction alleviates the aforementioned concerns. However, it is overly conservative as it excludes many small multi-establishment firms that are eligible—the procedure reduces the number of eligible firms by 25% in the OES sample and by 42% in the CiTDB sample. Nonetheless, our main results continue to hold.

Fourth, we split our full sample based on relevant industry characteristics and examine the subsamples separately. In rows 7 and 8 of Table XI, we separate industries into IT-intensive industries and non-IT-intensive industries.[47] We expect investments that can replace routine-task jobs to predominantly occur in IT-intensive industries. Consistent with this view, we observe a strong positive computer investment response and strong negative delayed employment response to Section 179 in IT-intensive industries, but no significant effect in non-IT-intensive industries. However, both groups of industries present some immediate responses of skilled labor to Section 179, suggesting that skill-biased technological change applies to broad sections of the economy. In rows 9 and 10, we present results for goods-producing and service-providing industries separately.[48] We find that both investment response and labor responses are more pronounced in service-providing industries than goods-producing industries. These findings are consistent with the fact that firms in goods-producing industries are likely to be much larger than firms in service-providing industries, making them less likely to be eligible for Section 179.

G. Placebo Test

Beyond the various robustness checks, we perform a placebo test by leveraging the unique way that Section 179 targets small businesses. Note that Section 179 provides investment incentives only to firms with qualified equipment investments below a certain (and time-varying) threshold. This feature distinguishes Section 179 from other policies that target small businesses directly based on the number of employees. For instance, the Small Business Administration and many regulatory agencies define a small business based on whether it has fewer than 100 employees.[49] In a placebo test, we replace

[46] In both of our data sets, we can create an identifier of whether the establishment is a single-unit business. The single-unit identifier for the OES data is obtained by matching to the QCEW universe, which records this identifier from firms' unemployment insurance filings. The single-unit identifier for the CiTDB data is obtained by examining whether establishments' employment is the same as the firm's total employment.

[47] IT-intensive industries are NAICS four-digit industries with above-median IT intensity, where IT intensity is the employment-weighted average of the industry's occupations' IT intensity produced by Gallipoli and Makridis (2018).

[48] Classifications for service-providing versus goods-producing industries are obtained from the BLS website at https://www.bls.gov/iag/tgs/iag_index_naics.htm. Based on this classification, our sample includes roughly five times as many observations from the service-producing industries as from the goods-producing industries.

[49] See the "Table of Size Standard" from the Small Business Administration at https://www.sba.gov/document/support--table-size-standards.

Table XII

Placebo Test: Replacing Section 179 Eligibility with a Small Firm Dummy

This table reports the results of a placebo test where firm eligibility for Section 179 is replaced by a small firm dummy based on firm employment. Panel A runs the investment regressions based on equation (6), where the dependent variable is annual computer investment. Panel B runs the employment regressions based on equation (8), where the dependent variables are three-year growth rate of the employment metrics in each establishment. $Small_{j,t}$ is a dummy variable that equals 1 if the firm employs fewer than 50 or 100 employees in year t. See Tables IV and V for definitions of other variables and details on regression specifications. Standard errors are clustered at the state level and reported in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively. The sample period is 2003 to 2014.

Panel A: Computer Investment

	Small = Emp fewer than 50 (1)	Small = Emp fewer than 100 (2)
$\Delta Limit_{s,t} \times Small_{j,t}$	4.39	−0.76
	(3.18)	(3.90)
$\Delta Limit_{s,t}$	3.49	5.65
	(2.97)	(3.42)
Observations	353,912	353,912
Adjusted R^2	0.21	0.21

Panel B: Employment

	Small = Emp fewer than 50				Small = Emp fewer than 100			
	Tot (1)	R (2)	S (3)	NU (4)	Tot (5)	R (6)	S (7)	NU (8)
$\Delta Limit_{s,t} \times Small_{j,t}$	2.31	−2.18	6.33	1.34	0.73	−9.51	2.25	0.29
	(2.76)	(6.76)	(5.30)	(4.60)	(2.86)	(6.55)	(7.45)	(4.90)
$\Delta Limit_{s,t+1} \times Small_{j,t+1}$	−0.91	−8.80	17.34**	−5.74	−0.08	−11.10	19.76**	−1.30
	(2.51)	(7.35)	(6.54)	(5.08)	(3.11)	(9.08)	(8.06)	(6.73)
$\Delta Limit_{s,t+2} \times Small_{j,t+2}$	−0.72	−7.50	6.16	−1.16	1.93	−0.11	15.10**	6.47
	(2.78)	(7.33)	(6.16)	(4.78)	(3.19)	(8.28)	(6.44)	(5.12)
$\Delta Limit_{s,t}$	−1.10	−13.75**	1.85	7.99**	−0.06	−7.91	4.29	8.56
	(2.11)	(6.41)	(5.34)	(3.83)	(2.37)	(6.39)	(7.51)	(5.16)
$\Delta Limit_{s,t+1}$	1.88	−2.31	−6.23	11.18***	0.92	0.22	−10.48	8.01
	(2.37)	(7.57)	(5.81)	(4.16)	(3.32)	(7.52)	(7.87)	(6.23)
$\Delta Limit_{s,t+2}$	1.66	1.67	1.44	3.06	−0.96	−3.40	−6.88	−3.51
	(2.03)	(5.03)	(4.07)	(4.42)	(2.60)	(6.04)	(5.16)	(5.05)
Observations	329,943	269,784	302,873	304,617	329,943	269,784	302,873	304,617
Adjusted R^2	0.14	0.23	0.21	0.21	0.12	0.23	0.21	0.21

our firm eligibility dummy, $Eligible_{j,t}$, with a dummy variable that equals 1 if the establishment's employment is below 100 (or 50), $Small_{j,t}$, in Table XII.

There are two main differences between $Eligible_{j,t}$ and $Small_{j,t}$. First, $Eligible_{j,t}$ is defined based on the establishment's expected equipment investment, which is the product of the industry-level equipment investment to employment ratio and the establishment's employment, while $Small_{j,t}$ is defined based only on the establishment's employment. Second, $Eligible_{j,t}$ is computed by comparing the establishment's expected equipment investment to a time-varying threshold of federal Section 179, while $Small_{j,t}$ is computed by comparing the establishment's employment to a fixed cutoff (i.e., 100 or 50).

This placebo test sheds light on the validity of our identifying assumption that changes in states' Section 179 limits are not correlated with other state-level shocks that differentially affect eligible and ineligible firms' investment and employment outcomes. If states that adopt Section 179 also happen to implement other incentives or policies that specifically target small businesses, our findings could potentially be driven by these confounding state-level factors. That would result in even stronger results when $Eligible_{j,t}$ is replaced with $Small_{j,t}$, a more direct measure of firm size. However, if such confounding factors are not driving the results, we expect the results to be weaker when $Eligible_{j,t}$ is replaced with $Small_{j,t}$. The results presented in Table XII suggest that other state-level incentives or policies that target small businesses are unlikely to be driving our finding on labor-technology substitution: both the response of computer investment and the delayed routine-task employment get small and indistinguishable from zero when we replace $Eligible_{j,t}$ with $Small_{j,t}$.

V. Investment and Labor Elasticities

Our results so far demonstrate that Section 179 tax incentives lead to higher investment rates, higher skilled labor growth, and lower routine-task labor growth at treated firms. In this section, we calculate the elasticity of investment with respect to the after-tax price of investment (after-tax user cost) and compare it with earlier estimates in the literature. Furthermore, we provide estimates for the elasticities of routine-task and skilled labor to investment prices, as well as the elasticity of substitution between equipment (computers) and the two types of labor.

A. Price Elasticities

We use the estimated treatment effects of the initial increase in federal Section 179 limits in 2003 (see equation (7)) to calculate the elasticities.[50] Following the limit increase, the after-tax price of investment for eligible firms in adopting states becomes $1 - \tau_{federal} - \tau_{state}$, whereas the price of investment for eligible firms in nonadopting states is $1 - \tau_{federal} - \tau_{state} \times z$, where z is the discounted value of current and future tax deductions for one dollar of investment. Applying a 7% discount rate to computer investment, which is subject to

[50] Our main regression specification, given in equation (6), does not lend itself to an elasticity calculation as there is no direct translation from the quantitative increases in state deduction limits to changes in investment prices.

five-year MACRS depreciation, we obtain z equal to 0.89.[51] Assuming a federal tax rate of 35% and a state tax rate of 7%, state adoption of the federal Section 179 in 2003 decreases the after-tax price of investment for eligible firms by 1.3%. The estimated effect on eligible firms' computer investment is 2.1% (see Section IV.B and Figure 4). Dividing 2.1% by −1.3% yields a large user cost elasticity of −1.6.[52]

The user cost elasticity delivered by our empirical model is within the range of recent estimates by Zwick and Mahon (2017) and is significantly larger than the earlier estimates between −0.5 and −1 (see the review by Hassett and Hubbard (2002)). Zwick and Mahon (2017) provide several possible explanations for the difference between their finding and earlier studies, including the prevalence of small and medium-size businesses in their sample compared to the data sets used in earlier studies. They show that the elasticity for the top-decile firms is in the range of earlier studies (with a point estimate of −0.5) and rises almost monotonically as firm size declines, reaching a point estimate of −3.3 for firms in the bottom decile. For an average firm, the authors estimate a user cost elasticity of −1.6, which happens to be identical to our result. Like Zwick and Mahon (2017), our larger elasticity estimate can be attributed to the nature of firms eligible for Section 179, which are much smaller than the firms included in earlier studies.

To precisely compare our estimated user cost elasticity with those in Zwick and Mahon (2017), one would ideally match firm sizes in their study with our eligible firms. This turns out to be infeasible, as Zwick and Mahon (2017) categorize firm size by sales while eligibility for Section 179 is determined by equipment investment. Assuming that investment and sales are perfectly correlated, one may compare our elasticity estimate with those for the bottom five deciles of firms in Zwick and Mahon (2017), which average to −2.4.[53] In this sense, our estimated user cost elasticity of −1.6 is slightly lower than their estimates.

Our empirical model also allows us to produce estimates for the elasticity of employment to investment prices. In Section IV.C, we estimate that states' adoption of the federal Section 179 limit increase in 2003 reduces eligible firms'

[51] We apply 7% discount rate to be consistent with Zwick and Mahon (2017) and the rest of the investment literature.

[52] This estimated user cost elasticity is likely to be an upper bound, because the calculation assumes that all eligible firms take up the federal Section 179 deduction regardless of their states' adoption status. As we discuss in Section II.C, state adoption of the federal Section 179 can increase the likelihood of eligible firms taking up the federal Section 179 deduction. Kitchen and Knittel (2016) estimate 6.6% greater likelihood based on state adoption of federal bonus depreciation incentive. Assuming that 6.6% of eligible firms do not take up the federal Section 179 deduction unless the state also adopts the incentive, the average after-tax investment price in nonadopting states is $6.6\% \times (1 - \tau_{federal} \times z - \tau_{state} \times z) + 93.4\% \times (1 - \tau_{federal} - \tau_{state} \times z)$. This adjustment leads to a lower estimated user cost elasticity of −1.3.

[53] Table 2 of Zwick and Mahon (2017) reports that the median equipment investment is \$367,000 in their sample, whereas we categorize firms with equipment investment below \$224,000 as eligible for the federal Section 179 incentive in 2003 (see Section II.B). Therefore, firms in the bottom five deciles are approximately comparable with eligible firms in 2003.

routine-task employment by -1.8% and increases eligible firms' skilled employment by 1.9%. Dividing these treatment effects by the 1.3% decline in after-tax investment price due to the state treatment in 2003 yields elasticity estimates of 1.4 for routine-task employment and -1.5 for skilled employment.

B. Substitution Elasticities

We also conduct back-of-the-envelope calculations of the elasticity of substitution between capital and labor using estimates from the 2003 experiment. The elasticity of substitution between inputs K (computers) and L (routine or skilled labor) is

$$E = -\frac{d\ln(K/L)}{d\ln(P_K/P_L)} \sim -\frac{\frac{\Delta K}{K} - \frac{\Delta L}{L}}{\frac{\Delta P_K}{P_K} - \frac{\Delta P_L}{P_L}}. \tag{10}$$

As explained in Section V.A, state adoption of the federal Section 179 limit increase in 2003 led to a 1.3% decline in the after-tax computer price, a 2.1% increase in computers, a 1.8% decline in routine-task labor, and a 1.9% increase in skilled labor in eligible firms. A similar analysis shows that the same experiment led to small but statistically insignificant changes in the wages of routine-task labor (-0.12%) and skilled labor (0.51%) in eligible firms.[54] Plugging these numbers into equation (10), we obtain an estimated elasticity of substitution between computers and routine-task labor of 3.3 and an elasticity of substitution between computers and skilled labor of 0.1.

Our estimates indicate strong substitutability between computers and routine-task labor, and strong complementarity between computers and skilled labor. These results are consistent with earlier estimates in the literature. For example, Krusell et al. (2000) use aggregate data and estimate an elasticity of substitution between equipment and unskilled labor (the sum of routine and nonroutine unskilled labor in our setting) of 1.7, and an elasticity between equipment and skilled labor of 0.7. The particularly strong substitutability and complementarity in our findings highlight the special interaction between computers and the labor market (Autor, Levy, and Murnane (2003)).

It is important to point out several caveats in interpreting these results due to data limitations. The first is that our estimated investment response to Section 179 is at the one-year horizon while our labor responses are measured at three-year horizons. The second is that while computers and skilled employment adjust immediately to the shocks, routine-task employment adjusts with a delay of up to two years. In the absence of a model with adjustment frictions, we simply assume that both capital and labor adjust smoothly within three-year horizons. Finally, our employment and computer investment data come from two separate sources, and the two data sets are not linked. One should therefore exercise caution in interpreting the estimated elasticities.

[54] Using the full sample, Table VII shows a positive and significant response of wages for skilled labor in eligible firms.

VI. Conclusion

This paper explores the implications of investment tax incentives for small firms' investment and labor outcomes using detailed establishment-level data. Standard models with homogeneous capital and labor inputs that complement each other would imply that both inputs respond positively to investment tax incentives. Earlier literature finds a positive effect of tax incentives on investment, which we also confirm by studying establishment-level computer investments. Yet few studies that touch upon the employment side do not find conclusive effects of investment tax incentives on employment.

We introduce heterogeneous labor inputs to study labor outcomes. In particular, we adopt a hypothesis from the labor economics literature (Autor, Levy, and Murnane (2003)) that equipment capital complements skilled labor and substitutes for routine-task labor. Consistent with this hypothesis, we find that firms eligible for a major investment tax incentive in the United States increase their skilled labor (quickly) and reduce their routine-task labor (after some delay) when responding to the incentive. Nonroutine unskilled labor, which neither substitutes for nor complements capital, is not affected by the investment tax incentive. Put together, the investment tax incentive has little effect on the total employment of eligible firms.

It is important to note that our findings on computer investment and routine-task and skilled employment are based on two separate establishment-level data sets, the CiTDB and the OES databases. Because these two data sets do not share a common identifier, we cannot simultaneously study investment and various employment responses of a given firm. However, using the CiTDB data, which provide each establishment's computer investment and total employment, we show that the investment tax incentive increased computer investment, had little effect on total employment (consistent with our findings using the OES data), and increased the number of computers per employee (IT intensity) within eligible firms. Hence, we conclude that investment tax incentives increased firms' reliance on capital relative to labor, consistent with the predictions of our conceptual framework.

We call for caution, however, in interpreting our quantitative estimates. Our identification relies on differences in tax policy across states with limited direct monetary effect on firms' bottom lines. Although we find strong responses to these differences in tax policy, one should exercise caution when extrapolating these effects to larger changes in the policy.

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Appendix

Table A.I

Response of Small Business Investment to Changes in State Section 179 Deduction Limits

This table reports the effect of changes in state Section 179 deduction limits on small businesses' purchasing and leasing of different types of capital using data from the National Federation of Independent Business (NFIB). The dependent variable is a dummy variable that equals 100 if a small business purchased or leased a specific type of capital in the last six months. The key independent variable, $\Delta Limit$, is the change in the maximum Section 179 deduction that a firm may claim in a year from state taxes between year $t-1$ to t, presented in millions of dollars. For C-corporation (pass-through) businesses, changes in state Section 179 limits and changes in state adoption of bonus depreciation are set to zero if the states do not levy corporate (individual) income taxes; all states are included. The NFIB surveys small businesses monthly. We exclude surveys from the first quarter of each year to allow the survey results to reflect firms' responses to the current year's changes in state Section 179 limits. Changes in state political, economic, and other policy characteristics from year $t-1$ to t are included to control for confounding effects. All regressions include fixed effects that include a full interaction of employment size bins, industry sector, a pass-through dummy, and year. Businesses with fewer than three employees are excluded from the sample. Employment size bins are given in the NFIB data as (3, 5), (6, 9), (10, 14), (15, 19), (20, 39), and (40, $+\infty$). Standard errors are clustered at the state level and reported in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively. The ample period is 2003 to 2014.

	Equipment		Furniture		Building Imp.		Land		Vehicle	
	Purch	Lease	Purch	Lease	Purch	Lease	Purch	Lease	Purch	Lease
$\Delta Limit_{s,t}$	9.36***	−1.15	−0.10	−0.15	4.30	−0.37	2.07	0.33	5.05	−0.40
	(3.19)	(1.07)	(2.08)	(0.30)	(2.83)	(0.37)	(1.52)	(0.59)	(3.17)	(0.96)
Observations	90,529	90,529	90,529	90,529	90,529	90,529	90,529	90,529	90,529	90,529
Adjusted R^2	0.07	0.01	0.04	0.01	0.03	0.00	0.03	0.01	0.10	0.03

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Supporting Information

Additional Supporting Information may be found in the online version of this article at the publisher's website:

Appendix S1: Internet Appendix.
Replication Code.