Economic Stimulus at the Expense of Routine-Task Jobs[*]

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Abstract

Do investment tax incentives improve job prospects for all workers? Using two massive establishment-level datasets on occupational employment and computer investment, we study the causal effect of a major tax incentive for equipment investment on labor outcomes. Section 179 of Internal Revenue Code allows firms to deduct *limited* amount of qualifying equipment investments instantly rather than following the standard depreciation schedule, hence lowering the effective price of equipment investment for *eligible* businesses but not for ineligible ones. By exploring the variation in states' Section 179 deduction limits for state taxes, we find that (1) eligible firms purchase more computers and hire more nonroutine-task labor shortly after states increase their deduction limits; (2) however, they start reducing their routine-task employment, and most significant reductions occur one year after the limit increases; (3) due to these opposite effects on two distinct labor groups, the effect on total employment is insignificant; (4) none of the above effects are detected among ineligible firms. Our results highlight the importance of heterogeneous worker skills for policy outcomes.

Keywords: Investment Tax Incentives, Labor-Technology Substitution, Section 179

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1. Introduction

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

Since the early 2000's, policymakers in the Unites States made significant changes to the Internal Revenue Code promoting investment with the intent of providing pro-growth, job-creating incentives. While the effect of these policies on investment is usually considered to be positive, despite the policymakers' emphasis on job creation, there is limited and inconclusive evidence on the labor market outcomes. This paper studies the causal effect of a major tax incentives for investment on labor outcomes. We find that the effect of investment incentives on jobs is nuanced, and heterogeneity in worker skills plays a major role on job outcomes.

We study the causal effect of Section 179 allowances on firm investment and employment. Section 179 of the Internal Revenue Code allows firms to expense a limited amount 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, due to time value of money, and more importantly, significantly reduces the immediate funding needs for investment. While Section 179 provides generous tax deductions to eligible firms, due to its deduction limits and phaseout thresholds, it mainly targets small businesses.[1] These firms account for a substantial fraction of economic activity: In 2014, firms with up to 250 employees make up more than 99% of all firms, and account for 43% of total employment in the United States.[2] There is a growing body of work that argues that smaller firms are more financially constrained, making the

[1] 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, Section 179 deduction is reduced by the amount exceeding the threshold. Our definition of eligible firms follows this feature closely and is detailed in Section 5.

[2] U.S. Census Bureau, Business Dynamics Statistics, https://www.census.gov/ces/dataproducts/bds/data_firm.html.

tax deduction from Section 179 potentially more appealing.[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. While some states conform to the federal deduction limits and allow deductions to increase for *state taxes* as well, others deviate. Using this variation in states' treatment of Section 179, we explore the effect of changes in Section 179 limits on firms' investment and employment outcomes.

We find that firms purchase more computers shortly after states increase Section 179 deduction limits. The effect on firms' employment is heterogeneous and depends on worker skills: Firms eligible for Section 179 reduce the number of workers who perform procedural and rule-based tasks, i.e., routine tasks. On the other hand, they increase the number of workers who perform nonroutine tasks.[4] The overall effect is insignificant due to the opposite outcomes for the two distinct labor groups. Moreover, the responses of routine and nonroutine employment also differ in timing: Firms start hiring additional nonroutine-task employees shortly after state Section 179 limits are raised, while the reduction in routine-task employment intensifies one year after the policy change. Deduction limits have no effect on the investment and employment outcomes of ineligible firms.

We interpret our findings on the effect of investment incentives on firms' employment based on the complementarity and substitutability of production factors. Autor, Levy, and Murnane (2003) and a large body of subsequent literature argue that capital, specifically computer capital, substitutes for workers who perform routine tasks, and complements workers in performing nonroutine tasks. Numerous empirical studies ascribe to this channel to explain the significant decline of routine-task labor and the rise of nonroutine-task labor in the past decades.[5] The Section 179 tax treatment, which lowers the effective price of equipment such as computers, provides an ideal setting for us to analyze the complementarity/substitutability between nonroutine/routine-task labor and computers. Consistent with this view, an increase in Section 179 deduction limit should lead to an increase in nonroutine

[3] Hadlock and Pierce (2010) determine firm size to be a particularly useful predictor of financial constraint levels. Recently Farre-Mensa and Ljungqvist (2015), using a proprietary dataset of private U.S. firms, argue that size is not a good proxy of financial constraints for public firms, which are vastly larger than private firms. However, they find that small private firms are financially constrained, which is consistent with the arguments made by Saunders and Steffen (2011) and Longstaff and Strebulaev (2014) before.

[4] Examples of routine-task jobs include bank tellers, assembly line workers, travel agents, and tax preparers. Examples of nonroutine-task jobs include managers, physicians, civil engineers, and janitors.

[5] See Acemoglu and Autor (2011) for a detailed review of this literature.

labor and a decrease in routine-task labor. Our results confirm, and provide direct evidence for this channel. To our knowledge, our paper is the first to examine and confirm the heterogeneous effects of computer investment on routine- and nonroutine-task labor using data from U.S.-based firms.

We formalize our testable hypothesis using a simple 2-period model with taxes where firms optimally choose three factors of production, capital, routine, and nonroutine labor, to maximize firm value. We assume that capital and routine-task labor provide routine inputs to the production and are relative substitutes, while routine inputs and nonroutine-task labor are relative complements. A faster tax expensing increases firms' after-tax profits in the first period and reduces them in the second period. We also assume that firms are subject to costly external financing, similar to Kaplan and Zingales (1997), which may be especially important for the small businesses Section 179 targets (Hadlock and Pierce (2010), Farre-Mensa and Ljungqvist (2015)). We show that faster expensing boosts up firms' investment. Furthermore, faster expensing leads to increasing nonroutine-task labor and decreasing routine-task labor. Two channels are at work in creating these results. First, firms discount future cash flows, so future deductions are less valuable than current period deductions, and the effect gets stronger as the discount rate gets higher. The second channel is due to financial constraints: Same period deduction reduces the firms' demand for costly external funding and amplifies the effect of the tax incentive.[6]

Our empirical tests are made possible by utilizing two establishment-level panel datasets. The first dataset, Computer Intelligence Technology Database (CiTDB) is a proprietary database that provides annual data on number of computers used in roughly 500,000 establishments before 2010, and in 3.2 million establishments afterward. Using CiTDB, we find that a $250,000 increase in state Section 179 limit, which is the case in 2010 for many states, leads to roughly 2% additional increase in computers in eligible establishments compared to matching establishments that are not subject to a state limit change.

The second dataset is the confidential microdata from the Occupational Employment Statistics (OES) program of the Bureau of Labor Statistics (BLS). The OES program provides employment data for over 800 detailed occupations in 1.2 million establishments in the U.S.

[6]In addition to the two channels considered in our model, discounting and financial constraints, another channel that potentially affects the firms' response to investment incentives is the presence of investment adjustment costs. Convex adjustment costs lead to gradual adjustment whereas non-convex costs lead to infrequent adjustment and lumpy investments. If Section 179 policy induces a firm across its adjustment threshold, firm investment may increase sharply.

over three-year cycles. We follow the labor economics literature to classify occupations based on their routineness and construct employment counts for routine-task and nonroutine-task labor at the establishment level. Using the OES microdata, we find that changes in state Section 179 deduction limits do not have a significant effect on total employment of eligible firms. However, we get a different picture when we look at the outcomes for routine and nonroutine-task labor separately. Consistent with our model's predictions, a $250,000 increase in state limit leads to roughly 1% additional increase in nonroutine-task employment in the following three years, yet a 6% decrease in routine-task employment in the three-year window starting from one year after the policy change, compared to matching establishments that are not subject to a policy change.

To examine whether establishments that reduce routine-task employment are the same establishments that increase computer investments and nonroutine-task employees following an earlier increase in state Section 179 limits, we run regressions of changes in routine-task employment on the interaction of past changes in state Section 179 limits and the establishments' past computer investment or nonroutine-task employment growth. Our results confirm the conjecture that following an increase in state Section 179 limits, firms first invest in additional computers and hire nonroutine-task labor, and then reduce routine-task labor. These findings are consistent with the literature on learning costs of technology adoption (see Greenwood (1999)).

A key concern for our identification strategy is that states ultimately choose their Section 179 policy. Consequently, factors that drive the states' policy choices may also drive the investment and employment decisions of establishments that operate there, leading to a spurious correlation between state Section 179 policy and firm outcomes. Our empirical design overcomes the concerns about states' endogenous policy choices in several ways. First, we run first-difference regressions of changes in establishments' computers and employment on changes in state Section 179 deduction limits. Hence, any unobservable yet persistent state characteristics that drive both state decisions on Section 179 and firm outcomes are controlled.[7] Second, the fact that Section 179 is only beneficial when firms' investment in equipment is below a certain cap provides a natural separation of *eligible* firms and *ineligible* firms. We thus separately analyze eligible firms, firms with expected investment in equipment below the cap, and ineligible firms. If any other unobserved time-varying state policies or

[7] See Heider and Ljungqvist (2015) for a similar empirical design.

characteristics are driving our results, their effects are likely to show up in both groups. However, we only observe the effects among eligible firms. Third, we control for changes in various state economic, political, and policy environment, and find our results to be robust to adding these observable time-varying controls. Fourth, to control for the heterogeneity in states' composition of industry and establishment size, we run all regressions with fixed effects that include a full interaction of 8 employment bins, NAICS 4-digit industry codes and year.

Our paper contributes to the growing literature that explores the effects of investment tax incentives. Most of the literature so far focused on the effect of tax incentives on investment, which is closely related to the broader question of price elasticity of investment. Summers (1981), Summers (1987), Cummins, Hassett, Hubbard, Hall, and Caballero (1994), Goolsbee (1998), Chirinko, Fazzari, and Meyer (1999) (among others) are some of the earlier contributors to the area. While these studies find that investment responds to incentives/changes in user cost, they disagree on the size of the effect. Post-2000 U.S. investment tax incentives are studied in House and Shapiro (2008), Edgerton (2010), Ohrn (2016), and Zwick and Mahon (2017). Zwick and Mahon (2017) study the bonus depreciation episodes of 2001-2004 and 2008-2010 using administrative tax records and find a large investment response to bonus depreciation, especially from small firms and firms that are likely to be financially constrained. Ohrn (2016) and Gaggl and Wright (2016) are the only two papers that explicitly study the effect of tax incentives on labor.[8] Ohrn (2016) studies the investment and total employment response to both bonus depreciation and Section 179 programs, and finds that employment does not respond to investment incentives, while investment does, and that the two programs diminish the effect of each other. Gaggl and Wright (2016) study a small firm tax incentive episode from the U.K. for computer and communications equipment and find that the primary effect of these types of capital is to complement nonroutine work, which provides further support for our modeling assumptions.

Our findings also contribute to the literature on job polarization, which refers to the increasing concentration of employment at the tails of the occupational skill distribution with the highest and lowest wages. This hollowing out of the middle has been linked to the disappearance of routine-task jobs.[9] Jaimovich and Siu (2014) argue that job polarization

[8]While not directly testing employment, Zwick and Mahon (2017) test the effect of bonus eligible investments on total payroll and find a positive effect.

[9]Job polarization is documented by Acemoglu (1999), Autor, Katz, and Kearney (2006), and many others.

is connected to jobless recoveries, which are periods following recessions in which rebounds in aggregate output are accompanied by much slower recoveries in aggregate employment. Major Section 179 limit increases overlapped with these recovery periods. By increasing the employment of nonroutine-task labor and reducing the employment of routine-task labor of eligible firms, Section 179 might have contributed to the job polarization and jobless recovery episodes following recent recessions.

While our evidence is consistent with increased job polarization, we refrain from generalizing our findings on offsetting employment effects to the entire economy. In particular, our identification strategy does not capture the possible spillover effects from the eligible firms to their out-of-state suppliers, such as equipment manufacturers, which might be employing additional workers due to the increased demand for their product. Therefore, we interpret our findings on total employment as the outcome for eligible firms, and not as an equilibrium result for the whole economy. Furthermore, due to the nature of our establishment-level, rather than individual-level data, one should be cautious when extending our employment results from the effects on jobs to potential effects on individual routine/nonroutine-task workers. While our findings show opposite effects of firms' demand for routine and nonroutine-task job positions, because we do not observe the subsequent outcomes for routine and nonroutine-task workers (e.g., job relocation), we refrain from drawing conclusions for individual or social welfare.

The paper is organized as follows. Section 2 presents a model to guide our empirical tests. Section 3 describes the policy background. Section 4 describes the data used in our empirical analysis and introduces our key measures. Section 5 presents our empirical results relating tax policy, investment, and labor outcomes. Section 6 concludes.

2. A Simple Model

We develop a simple two-period model where we derive the effect of investment tax incentives on firm's investment and labor decisions.

Firms use three factors of production. Two of these are labor inputs, routine and nonroutine labor (L_R and L_N), and the last factor is capital (K). Routine labor and capital perform routine tasks, whereas nonroutine labor performs nonroutine tasks in the production

See Autor and Acemoglu (2011) for a detailed review.

process. Firms produce output with these inputs using the following technology:[10]

$$Y = L_N^{\alpha} \left(\lambda L_R^{\mu} + (1-\lambda) K^{\mu}\right)^{\frac{\beta}{\mu}},$$

where $\mu, \beta, \alpha \in (0,1)$ and $\alpha + \beta < 1$. The last inequality captures decreasing returns to scale, meaning that a proportional increase in productive inputs causes output to increase by a smaller proportion. The routine task inputs are aggregated using a constant elasticity of substitution (CES) aggregator, given by $\left(\lambda L_R^{\mu} + (1-\lambda) K^{\mu}\right)^{\frac{1}{\mu}}$.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 nonroutine labor and aggregated routine task inputs is 1. Routine labor and capital are relative substitutes, whereas nonroutine labor and capital are relative complements. Firms are competitive and take as given the prices of all inputs (wages, w_N and w_R, and prices of capital, P).

Capital is a long term asset and depreciates at the rate δ. The tax code allows the firm to deduct the cost of new investment from taxable income over time, however, depreciation tax schedule is decoupled from the economic depreciation rate. The firm is allowed to deduct η fraction of the new investment in the same period that investment is made, and the rest (1-η fraction) is depreciated (expensed) in the next period. Variations in η will be the primary vehicle of tax policy in the paper.

There are two periods. Firms start the first period with an existing capital stock K_1, hire labor, produce, make investments for the next period, and pay taxes. Firms finance new investment I_1 either with internal funds (after-tax profits) or external funds. Similar to Kaplan and Zingales (1997), we assume that external financing is costly, and model it in reduced form as a linear cost. The resulting first period cash flow of the firms is given by:

$$\begin{aligned} D_1 &= (1-\tau)\left(Y_1 - w_{N,1}L_{N,1} - w_{R,1}L_{R,1}\right) - (1-\tau\eta) I_1 P_1 \\ &\quad - c\left[(1-\tau\eta) I_1 P_1 - (1-\tau)\left(Y_1 - w_{N,1}L_{N,1} - w_{R,1}L_{R,1}\right)\right] \end{aligned}$$

where c is the linear cost of raising external financing and τ is the marginal tax rate of the

[10] Autor and Dorn (2013) use a similar specification for the goods sector aggregating the same three inputs.

investors.[11] Next period's capital K_2 is determined by the capital accumulation rule:

$$K_2 = (1-\delta) K_1 + I_1.$$

In the second period firms produce, take the remaining depreciation deduction, and liquidate capital. Since the capital is completely depreciated for tax purposes, the sale of capital results in capital gains and is taxed at the rate τ.[12] The second period cash flow is given by:

$$D_2 = (1-\tau)(Y_2 - w_{N,2}L_{N,2} - w_{R,2}L_{R,2}) + \tau(1-\eta) I_1 P_1 + (1-\tau)(1-\delta) K_2 P_2.$$

Firms make labor and investment decisions $(L_{N,1}, L_{R,1}, I_1, L_{N,2}, L_{R,2})$ to maximize the firm value V, which is the sum of period 1 cash flows and the present value of the period 2 cash flows:

$$\max_{L_{N,1},L_{R,1},I_1,L_{N,2},L_{R,2}} V = D_1 + \frac{D_2}{r}$$

where r is the rate firms use to discount future cash flows. The first order conditions with respect to L_N, L_R, and I give the optimality conditions:

$$\begin{aligned} w_N &= \alpha L_N^{\alpha-1} \left(\lambda L_R^{\mu} + (1-\lambda) K^{\mu}\right)^{\frac{\beta}{\mu}} && (1) \\ w_R &= \beta\lambda L_N^{\alpha} L_R^{\mu-1} \left(\lambda L_R^{\mu} + (1-\lambda) K^{\mu}\right)^{\frac{\beta}{\mu}-1} && (2) \\ (1-\tau\eta)(1+c) P_1 r &= (1-\tau)\beta(1-\lambda) L_N^{\alpha} K^{\mu-1} \left(\lambda L_R^{\mu} + (1-\lambda) K^{\mu}\right)^{\frac{\beta}{\mu}-1} \\ &\quad +\tau(1-\eta)P_1 + (1-\tau)(1-\delta) P_2. && (3) \end{aligned}$$

Equations 1 and 2 show that the nonroutine and routine wage rate are the marginal product of nonroutine and routine labor, respectively. Equation 3 equates the marginal cost of investing in period 1, $(1-\tau\eta)(1+c) P_1$, to the marginal benefit. All benefits, which are the after-tax marginal product of capital, tax benefit of remaining depreciation deduction, and after-tax proceeds from the sale of depreciated capital, happen in period 2 and are discounted at the rate r.

[11] Kaplan and Zingales (1997) assumes that external financing cost is a convex function of funds raised. Hennessy and Whited (2007)) model them to be fixed and linear quadratic, however, their estimate for the quadratic term is small and statistically insignificant. For simplicity we assume that they are linear.

[12] While we allow the sale of capital at the end of period 2, none of the results depend on the depreciation rate δ. Assuming complete economic depreciation ($\delta = 1$) yields the same results.

We are interested in understanding the implications of depreciation tax policy, captured by η in this simple economy, on firms' investment and labor decisions. Higher η implies that a larger fraction of investment cost is deducted from taxable income in the period that the investment is made and proxies the accelerated depreciation provisions in the tax code. Specifically, we are interested in solving for $\frac{dI_1}{d\eta}, \frac{L_{N,2}}{d\eta}, \frac{L_{R,2}}{d\eta}$ to understand how η affects investment and labor choices of the firms.[13]

Proposition 1 *Given $\frac{\beta}{\mu} < (1-\alpha)$, faster depreciation tax policy (higher η) leads to higher investment I and nonroutine employment L_N, and lower routine employment L_R.*

In order to derive the expressions for $\frac{dI_1}{d\eta}, \frac{L_{N,2}}{d\eta}, \frac{L_{R,2}}{d\eta}$ we work with the first order conditions given in equations 1-3. In the following analysis, we suppress the second period index for notational simplicity unless otherwise indicated.

We proceed in three steps. We first examine the relation between K and L_R and L_N and L_R. Then we calculate $\frac{dL_R}{d\eta}$, which measures the sensitivity of firm's routine employment to η. Lastly, we obtain the sign of $\frac{dK}{d\eta}$ and $\frac{dL_N}{d\eta}$.

Step 1: From equation 1 and 2, we have:

$$K = \left[\psi_1 L_R^{\frac{-\mu(1-\alpha)(1-\mu)}{\mu-\alpha\mu-\beta}} - \left(\frac{\lambda}{1-\lambda}\right) L_R^{\mu}\right]^{\frac{1}{\mu}}, \tag{4}$$

and

$$L_N = \psi_2 L_R^{\frac{-(1-\mu)\beta}{\mu-\alpha-\mu\beta}}, \tag{5}$$

where

$$\psi_1 = \frac{1}{1-\lambda}\left[\left(\frac{\beta\lambda}{w_R}\right)^{1-\alpha}\left(\frac{\alpha}{w_N}\right)^{\alpha}\right]^{\frac{\mu}{\mu-\alpha\mu-\beta}}$$

and

$$\psi_2 = \left[\left(\frac{\beta\lambda}{w_R}\right)^{\beta}\left(\frac{\alpha}{w_N}\right)^{\mu-\beta}\right]^{\frac{\mu}{\mu-\alpha\mu-\beta}}.$$

Taking the derivative of K and L_N with respect to L_R,

$$\frac{dK}{dL_R} = -K^{1-\mu}\left[\psi_1 \frac{(1-\alpha)(1-\mu)}{\mu-\alpha\mu-\beta} L_R^{\frac{-\mu(1-\alpha)(1-\mu)}{\mu-\alpha\mu-\beta}-1} + \left(\frac{\lambda}{1-\lambda}\right) L_R^{\mu-1}\right]$$

[13] Note that $L_{N,1}$ and $L_{R,1}$ will be determined only based on K_1, which is given.

and

$$\frac{dL_N}{dL_R} = -\frac{(1-\mu)\beta}{\mu-\alpha-\mu\beta}\psi_2 L_R^{\frac{-(1-\mu)\beta}{\mu-\alpha-\mu\beta}-1}.$$

Given that $\psi_1 > 0$, $\psi_2 > 0$, and $\frac{\beta}{\mu} < 1-\alpha$, we have,

$$\begin{aligned} \frac{dK}{dL_R} &< 0 \\ \frac{dL_N}{dL_R} &< 0 \end{aligned}$$

Step 2: Plugging K from 4 and L_N from 5 in the first order condition given in equation 3:

$$\begin{aligned} (1-\tau\eta)(1+c)P_1 r &= (1-\tau)w_R\left(\frac{1-\lambda}{\lambda}\right)\left[\psi_1 L_R^{\frac{-\mu(1-\alpha-\beta)}{-\alpha\mu-\beta+\mu}} - \left(\frac{\lambda}{1-\lambda}\right)\right]^{\frac{\mu-1}{\mu}} \\ &\quad +\tau(1-\eta)P_1 + (1-\tau)(1-\delta)P_2. \end{aligned} \tag{6}$$

Implicitly differentiating equation 6 with respect to η yields:

$$\tau P_1(1-r(1+c)) = (1-\tau)w_R\left(\frac{1-\lambda}{\lambda}\right)\left[\begin{array}{c} \frac{\mu-1}{\mu}\left(\psi_1 L_R^{\frac{-\mu(1-\alpha-\beta)}{-\alpha\mu-\beta+\mu}} - \left(\frac{\lambda}{1-\lambda}\right)\right)^{-\frac{1}{\mu}} \\ \times\frac{-\mu(1-\alpha-\beta)}{-\alpha\mu-\beta+\mu}\psi_1 L_R^{\frac{-\mu(1-\alpha-\beta)}{-\alpha\mu-\beta+\mu}-1} \end{array}\right]\frac{dL_R}{d\eta} \tag{7}$$

To understand the sign of $\frac{dL_R}{d\eta}$, we have to understand the signs of the multiplicative components of equation 7. Since $r > 1$ (the discount rate) and $c \geq 0$ (external financing cost), the left hand side is negative. On the right hand side, $(1-\tau)w_R\left(\frac{1-\lambda}{\lambda}\right) > 0$, $\left(\psi_1 L_R^{\frac{-\mu(1-\alpha-\beta)}{-\alpha\mu-\beta+\mu}} - \left(\frac{\lambda}{1-\lambda}\right)\right)^{-\frac{1}{\mu}} = \frac{L_R}{K} > 0$, $\psi_1 > 0$, $L_R^{\frac{-\mu(1-\alpha-\beta)}{-\alpha\mu-\beta+\mu}-1} > 0$, $-\alpha\mu-\beta+\mu > 0$. Two terms are negative: $\frac{\mu-1}{\mu} < 0$ and $-\mu(1-\alpha-\beta) < 0$. Equation will be satisfied if and only if $\frac{dL_R}{d\eta} < 0$.

Step 3: From $\frac{dL_R}{d\eta}$, $\frac{dK}{dL_R}$, and $\frac{dL_N}{dL_R}$, we have:

$$\frac{dK}{d\eta} = \frac{dK}{dL_R}\frac{dL_R}{d\eta} > 0, \tag{8}$$

and

$$\frac{dL_N}{d\eta} = \frac{dL_N}{dL_R}\frac{dL_R}{d\eta} > 0. \tag{9}$$

Therefore, increasing tax incentive for investment will boost investment I and nonroutine employment L_N, but dampen routine employment L_R. It is useful to analyze the left hand side of equation 7 to understand the two channels that contribute to the effect. Firms discount future cash flows with the rate r, so future deductions are less valuable than current period deductions, and the effect gets stronger as the discount rate gets higher. The second channel is due to financial constraints, captured by c: Same period deduction provides a cheaper source of funding for the investment compared to external financing. In the absence these channels ($r = 1$ and $c = 0$), incentives do not have any effect. Having at least one of the channels lead to derived effects, and having both channels leads to strongest effects.

While the model always generates positive response for capital and nonroutine labor to investment tax incentives, our result for $\frac{dL_R}{d\eta}$ (sensitivity of routine labor to incentive) depend critically on an assumption on the parameter values, $\frac{\beta}{\mu} < (1 - \alpha)$. This expression implies a relationship between returns to scale and the elasticity of substitution between capital and routine labor. When the returns to scale ($\alpha + \beta$) is high, the condition is satistied with a higher μ (higher elasticity of substitution). Therefore, investment tax incentives lead to lower routine task labor if the returns to scale is relatively low, or the elasticity of substitution is sufficiently high. The interpretation of this condition is related to income versus substitution effects: Lower price of capital leads to both substitution from routine labor to capital (substitution effect), and increasing the scale of operations by increasing its inputs (income effect). The substitution effect dominates when the economy has sufficiently low returns to scale, or when capital and routine labor are strong substitutes, leading to lower routine labor. Otherwise, while the firm still tilts its routine inputs from routine labor to capital due to the substitution effect, lower price of capital may induce the firms to increase the scale of its operations by investing and adding both types of labor, leading to higher routine labor.

3. Policy Background

3.1. Section 179 Expensing

Section 179 of the Internal Revenue Code allows firms to expense limited amount of qualified investments instantly instead of depreciating the asset according to the baseline depreciation schedule known as the Modified Accelerated Cost Recovery System (MACRS). With few exceptions, qualified investments 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 do not qualify. The use of the Section 179 expensing is subject to three limitations. There is a maximum expensing allowance, which is the maximum deduction that can be taken in a year. There is also a phaseout threshold. If in a given year the firm places in service more property than the phaseout threshold, 179 deduction is reduced, dollar for dollar, by the amount exceeding the limit. Finally, the income limitation bars the firm from claiming a Section 179 deduction greater than its taxable income. While firms in all lines of business and sizes have the option to elect 179 expensing, the deduction and phaseout limits make it more appealing to smaller firms.

Section 179 expensing began as a first year depreciation allowance with the Small Business Tax Revision Act of 1958 to reduce the tax burden on small business owners and stimulate small business investment. The original deduction amount was limited to $2,000 ($4,000 in the case of a married couple filing a joint return). Over the years, successive tax laws made changes to its coverage and limits. Small Business Job Protection Act of 1996 placed a timetable for scheduled increases to its limits from 1997 to 2003, where maximum allowance and phaseout limit were scheduled to increase to $25,000 and $200,000 respectively in 2003.[14]

Since 2003, several acts have significantly increased the 179 deduction and phaseout limits for federal taxes. The federal deduction limit is increased to $100,000 in 2003, $125,000 in 2007, $250,000 in 2008, and $500,000 in 2010. The phaseout limits are also increased from $200,000 to $2,000,000 over the same period. Table 1 provides a timeline for the relevant legislations and changes to federal Section 179 limits.

[TABLE 1 ABOUT HERE]

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

3.2. State Taxation of Business Income and Treatment of Section 179

Since early 2000's, roughly half of the U.S. business income is generated by the traditional corporate sector subject to corporate income tax (C-corporations). The other half is generated by "pass-through" businesses, like S-corporations, partnerships, and sole proprietorships, where business income is passed to the owners who pay individual income taxes. (Cooper, McClelland, Pearce, Prisinzano, Sullivan, Yagan, Zidar, and Zwick (2016)) Firms with all legal forms can use Section 179 expensing. While C-corporations account for half of the total business income, only 4% of the firms that claimed 179 deductions in 2014 were C-filers (Kitchen and Knittel (2016)). During our sample period corporations are not subject to income-based state taxes in Nevada, South Dakota, Washington, and Wyoming; and individuals are not subject to income-based state taxes in Alaska, Florida, Nevada, South Dakota, Texas, Washington, and Wyoming.[15] Since firms that use 179 expensing are overwhelmingly tax pass-through entities, we focus mainly on individuals' (rather than C-corporations') Section 179 deductions for state taxes. Over the 2002-2014 period, the median marginal tax rate is 7.5% for corporate, and 6.2% for individual taxes among the states that collect income-based state taxes from individuals.

Prior to the Jobs and Growth Tax Relief Reconciliation Act of 2003 (JGTRRA), which increased the federal Section 179 limits from $25,000 to $100,000, nearly all states with income-based state taxes allowed full expensing of qualified investments up to the federal limit for state tax purposes. Following the passage of JGTRRA and the successive acts that led to higher federal limits, half of the states required various adjustments to federal deduction limits. While 22 states fully adopted the federal 179 limits throughout 2003-2014, 10 states required adjustments in every year, and 12 states required adjustments in some years of the sample period. Panel A of Figure 1 provides a map of states' federal 179 limit adoption status in 2003. Panel B provides the map for 2014. The maps show that, overall, there are nearly twice as many adopting states as non-adopting states. However, the two groups are much closer in terms of their share of the total economic activity: $4.9

[15]Four states imposed taxes on business receipts rather than income during part or all of our sample period. Washington imposed Business and Occupation Tax based on the gross receipts, not income, of the business and does not allow 179 deduction. Texas switched from Franchise Tax to Margin Tax (modified gross receipts) in 2008, but allowed full 179 deduction from gross receipts. Michigan had a Single Business Tax before 2008, introduced the Michigan Business Tax in 2008 (modified gross receipts), and switched to corporate income tax in 2012. Ohio started phasing out Franchise Tax in 2006 and completely phased in Commercial Activities Tax on gross receipts in 2010.

trillion GDP for non-adopting states versus $7 trillion for adopting states on an annual basis. Furthermore, non-adopting states are geographically spread out. There is a negative but insignificant relationship between the Section 179 limit of a state and the average limit of its neighboring states. In our tests we exploit this variation in the state 179 limits, which also varies considerably over time.

In Table 2, we examine whether states' changes in Section 179 limits are correlated with changes in states' other policies and business conditions that may potentially affect firms' investment and employment decisions. This exercise helps us to understand the confounding factors, if any, to our main analyses so that we can better analyze and control for them. We run cross-sectional regressions of changes in state Section 179 limits on changes in governor's political affiliation, changes in measures of states' fiscal health (state's S&P credit rating, state budget surplus), economic indicators (change in unemployment rate, gross state product growth), changes in state corporate and individual tax rates, and changes in related state policies (whether the state adopts Section 168 bonus depreciation, number of job creation hiring credit programs offered by the state), and the fraction of routine-task jobs within the state.[16] Table 2 shows that states' increase in their Section 179 limits is accompanied by the adoption of tax bonus depreciation incentive, which targets mainly large businesses. State changes in Section 179 limits are not systematically related to any other political, fiscal, or economic conditions. Since these factors can still potentially affect investment and employment outcomes, we will add these time-varying controls to our regression specifications.

[TABLE 2 ABOUT HERE]

[FIGURE 1 ABOUT HERE]

3.3. Alternative Tax Incentive: Bonus Depreciation

There is another federal investment tax incentive, bonus depreciation, which was available between 2001-2004 and later starting 2008. Bonus depreciation does not have any limits on

[16] We classify states with zero personal income tax rate as states that don't change state 179 deduction limits since the limits are practically always zero for individuals in these states.

investment size, however, it allows up to 50% additional depreciation, rather than 100% expensing allowed in Section 179.[17] Similar to Section 179, while some states conform to federal bonus depreciation for state taxes, others require adjustments. We primarily focus on Section 179 rather than bonus depreciation for several reasons. The first reason is the speed of deduction: For eligible firms Section 179 provides a more generous incentive, allowing firms to expense the entire investment in the first year. Second, as we confirm with our model, financial constraints are likely to be an important channel through which accelerated depreciation operates, and the firms that are eligible for Section 179 are most likely be financially constrained. Section 179 deduction is claimed by roughly 6 million firms in 2014, which is more than twice as many as the number of firms claiming bonus deduction (Kitchen and Knittel (2016)). Our establishment-level datasets provide a good coverage of these firms that are likely to be eligible for Section 179. Finally, Section 179 federal limits have been raised from $24,000 to $500,000 in several increments during our sample period, which provides a nice variation and helps with identification in our tests. While we primarily focus on Section 179, as we discuss in Section 3.2, we find that state adoption of bonus depreciation and state Section 179 deduction limit increases are correlated. Therefore, we control for state bonus depreciation conformity in all our tests.

4. Data and Measurement

In this section we describe the data used in the paper and discuss the measurement of critical variables.

Investment data Our primary investment measure is derived from the Computer Intelligence Technology Database (CiTDB), which is a proprietary database that provides detailed information on information technology spending at the establishment level. This database is compiled from telephone surveys of the establishments, usually annually, and includes roughly 500,000 establishments before 2010, and 3.2 million establishments afterwards. The database is used frequently to measure the impact of IT investments in management and informations systems literatures (Brynjolfsson and Hitt (2003); Tambe, Hitt, and Brynjolfsson (2012); Bloom, Garicano, Sadun, and Van Reenen (2014); and many others).While the database includes many different variables related to IT investments, the only variable that

[17]Bonus depreciation was 100% between 9/8/2010 and 12/31/2011.

has been consistently surveyed over our sample period is the number of personal computers. We measure investment rate as the percent change in the number of PCs that are put in service in an establishment.[18] The database also provides other establishment level information, such as the name, address, industry of the business and the number of employees.

Employment data We construct measures related to employment from the microdata at the establishment-occupation level provided by the OES program of the Bureau of Labor Statistics (BLS). This dataset covers surveys that track employment by occupations in approximately 200,000 establishments every six months over three-year cycles. These data represent, on average, 62% of the non-farm employment in the U.S. The data use the OES taxonomy occupational classification with 828 detailed occupation definitions before 1999, and the Standard Occupational Classification (SOC) with 896 detailed occupation definitions thereafter. Besides occupational information, the microdata also cover establishments' location and industry affiliation, as well as their parent company's employer identification number (EIN), legal name, and trade name.

We measure an establishment's routine-task and nonroutine-task employment following the methodology described in Zhang (2016), which is based on a commonly used procedure in the labor economics literature and is closest to 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 obtain skill information of occupations classified at a very detailed level. For each DOT occupation, we select the occupation's required skill level in performing five categories of tasks: abstract analytic, abstract interactive, routine cognitive, routine manual and nonroutine manual tasks. We rescale these skill levels to be between 1 and 10. We then take the average of the routine cognitive and routine manual skill levels as the skill level required by the occupation in performing routine tasks. Similarly, we obtain the skill level required by each occupation in performing abstract tasks. We then aggregate the DOT occupations to the OES occupation level. The task skill measures for the OES occupations are the average of the skill measures for the corresponding DOT occupations following a weighting approach proposed by Autor, Levy, and Murnane (2003). Following Autor and

[18] We measure investment and employment growth rates by dividing the level change in variable by the average of the level of the variable before and after the change.

Dorn (2013), we define the routine-task intensity (RTI) score for each OES occupation as

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

where $T_k^{Routine}$, $T_k^{Abstract}$, and T_k^{Manual} are the routine, abstract, and nonroutine manual task skill levels required by occupation k, respectively.

Routine-task labor is defined as follows: In each year, as suggested by the OES program, we select all workers in the OES sample in the current year as well as in the previous two years to represent the current year's total labor force. We then sort all workers in current year's labor force based on their occupations' RTI scores. We define workers as routine-task labor if their RTI scores fall in 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. In particular, it accounts for the fact that certain occupations that are not substitutable by machines in previous years become substitutable because their RTI rankings increase over time.

We construct routine-task employment, $L_{R,j,t}$, nonroutine-task employment, $L_{N,j,t}$, total employment, $L_{tot,j,t}$, and $RShare_{j,t}$, the share of routine-task labor, for each establishment j in year t as:

$$\begin{aligned} L_{tot,j,t} &= \sum_k emp_{j,k,t} \\ L_{R,j,t} &= \sum_k \mathbb{1}\left[RTI_k > RTI_t^{P80}\right] \times emp_{j,k,t} \\ L_{N,j,t} &= L_{tot,j,t} - L_{R,j,t} \\ Rshare_{j,t} &= \frac{L_{R,j,t}}{L_{tot,j,t}} \end{aligned}$$

where $\mathbb{1}$ 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 at year t, and $emp_{j,k,t}$ is the number of employees of occupation k in establishment j at year t.

Section 179 limits We hand-collected state Section 179 deduction limits and phaseout tresholds between 2002 and 2014 from CCH State Tax Handbooks, and supplemented the handbooks with state tax authorities' websites when needed.

State-level data We use various state level controls in our empirical tests. Similar to Section 179 limits, data on states' Section 168 bonus depreciation conformity is hand-collected from CCH State Tax Handbooks. The number of state job creation hiring credit programs is based on the data collected by and provided in Appendix Table 1 of Neumark and Grijalva (2013). The data ends in 2012. We extend the last year's credit counts to 2013 and 2014 in our tests. State unemployment rate is provided by the Bureau of Labor Statistics. State (real) GDP growth is downloaded from the Bureau of Economic Analysis website. State budget balance is compiled from the State Government Finances, U.S. Census Bureau. Budget surplus is measured as the difference between the "general revenue" and "general expenditure." The results of the gubernatorial elections is collected from the Congressional Quarterly Voting and Elections Collection. State corporate income tax rates are taken from the Tax Foundation.[19] State personal income tax rates are obtained from the NBER database of marginal state income tax rates.[20]

5. Empirical Evidence

In this section we test the predictions of the simple model developed in section 2. The model predicts that increasing tax incentive for investment will boost up computer investment and nonroutine employment, but reduce routine employment. We first compare the summary statistics from our datasets for the states that increase state Section 179 limits to the states that do not. Then we study the effect of state Section 179 limits on computer investments and labor outcomes independently. Finally, we merge the two datasets and study the effect of 179 limits on employment outcomes, conditional on the establishment's investment.

5.1. Summary Statistics

How do firms in states that raise state Section 179 deduction and phaseout limits compare to those from the states that do not? To answer that question, Table 3 reports the OES and CiTBD sample statistics at the establishment-year level between 2003 and 2014. Our samples cover all establishments in OES and CiTDB datasets that have consecutive observations that

[19] https://taxfoundation.org/state-corporate-income-tax-rates

[20] Two states, New Hampshire and Tennessee, only tax interest and dividend income components of personal income. These rates are taken from the Tax Foundation.

allow computation of current and past employment or investment growth.[21] OES sample includes 179 thousand such establishment-year observations in states that increased Section 179 limits in that year, 255 thousand observations in state that did not. The average (median) establishment is essentially indistinguishable: 199 (50) total employees in states with limit increases, compared to 204 (50) employees in other states. Establishments in both groups of states also employ similar number of routine- and nonroutine-task employees.

While the CiTDB dataset is compiled from a somewhat different cross section of firms, the median establishment size is similar to those from the OES sample. Average (median) establishment has 126 (30) computers in states with limit increases and 147 (31) computers in the remaining states. The sample covers roughly 298 thousand and 434 thousand establishment-year observations in states with increased limits and other states, respectively.

[TABLE 3 ABOUT HERE]

5.2. Eligibility for Section 179 Deduction

While the legislation for Section 179 does dot exclude any firms explicitly from taking deductions, the presence of deduction limits and phaseout thresholds make Section 179 tax benefits practically irrelevant for large firms. Firms can claim deductions for investments up to the deduction limit, and if the firm's investment in a year exceeds the phaseout threshold, then 179 deduction is reduced, dollar by dollar, by the amount exceeding the threshold. To classify whether the firms are effectively eligible for Section 179 deduction, we predict the amount of equipment investment an establishment will make based on its employment and the equipment investment to employment ratio for the industry it belongs to.[22] We classify establishments with predicted investment (establishment's employment multiplied by the industry's equipment investment to employment ratio) below the federal 179 phaseout

[21] In addition, for the CiTDB sample, we exclude industries in which computer investment accounts for less than five percent of the total investment in equipment and software to strengthen our use of computer investment as a reasonably proxy for establishments total investment in equipment and software.

[22] We use BEA data to calculate the average equipment investment/employment ratio for each industry (at 3 digit NAICS level). Employment is full-time equivalent employees by industry and investment is investment in private equipment by industry. We smooth the ratio by taking the average of the last three years.

threshold as eligible establishments and the rest as ineligible establishment.[23]

5.3. Investment and Employment Outcomes

Our first hypothesis is that an increase in state Section 179 limits will lead to additional computer purchases by eligible firms. We do not anticipate such an effect for the firms that are ineligible for the deduction.[24] This prediction is theoretically derived in equation 8. We test this hypothesis and report the results in Table 4. Specifically, we run the following regressions:

$$\begin{aligned} \Delta PC_{j,s,t\to t+1} &= b_0 + b_1 \Delta Limit_{s,t-1\to t} + b_2 \Delta X_{s,t-1\to t} + b_3 \Delta PC_{j,s,t-3\to t} \\ &\quad + Dummy_{EmpBin\times Ind\times Year} + \epsilon_{j,s,t+1}, \end{aligned} \tag{10}$$

where $\Delta PC_{j,s,t\to t+1}$ is the investment rate measured from PC growth, $\Delta Limit_{s,t-1\to t}$ is the change in state Section 179 limit (in millions of dollars). Changes to Section 179 limits tend to occur towards the end of the year. Hence, in year $t+1$, the information available to firm j is typically the most recent change in Section 179 deduction limit from year $t-1$ to t. $\Delta X_{s,t-1\to t}$ includes changes in state characteristics and tax policies to control for changes in other time-varying investment opportunities in the state. $\Delta PC_{j,s,t-3\to t}$ measures the PC growth over the past three years. $Dummy_{EmpBin\times Ind\times Year}$ is a set of dummies that include a full interaction of establishment-level employment bins, industry, and year. Due to the fixed effects, coefficient estimates reflect the variation within the same establishment size bin, industry, and year, across states with different deduction limits. We expect to find a positive estimate for b_1, implying that investment rate increases in response to increasing deduction limits, relative to investment rate in the same industry and size group but lower deduction limit changes. We present results for several different specifications for the entire sample, subsamples for eligible and ineligible establishments, and different controls. We

[23] For multi-establishment firms, some of the establishments we classify as ”eligible” may actually be ineligible if total investment of the firm exceeds the threshold. Our use of establishment-level employment counts instead of firm-level employment is mainly due to data limitation on firm employment. By using establishment-level employment, we potentially contaminate our sample of eligible establishments by including small establishments from large firms, making it more difficult for us to find any results among eligible firms. In untabulated results, we repeat the analyses by aggregating OES establishments based on employer identification number (EIN) to create firm employment. We find that the results are materially the same, partly because most small businesses are single unit establishments.

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

cluster standard errors at the state level.

Consistent with our hypothesis, we find that the coefficient for deduction limit change is positive and significant for firms that are classified as eligible, while it is not significant for ineligible firms. For eligible firms from a state with $250,000 Section 179 state deduction limit increase, the estimates imply roughly 2% higher computer investments annually compared to matching firms that are not subject to state limit increases. Compared to the unconditional investment rate of 7%, the effect is economically significant. We find no effect of state control variables on investment among all firms and eligible firms.

Next, we investigate the timing of establishments' investment responses to states' changes in Section 179 limits. In Table 5, we run the same first-difference regressions, but also include two lagged changes in state Section 179 limits. The results show that all the effects are coming from the most recent limit changes, which we will refer to as contemporaneous changes, and not the lagged changes, indicating that firms respond to changes in state Section 179 limit quickly.

[TABLE 4 ABOUT HERE]

[TABLE 5 ABOUT HERE]

After confirming the effect of investment tax incentives on the investment rate of eligible firms, we turn to the labor outcomes. Our model have separate and opposite predictions for the outcomes for routine-task and nonroutine task labor, as derived in equations 7 and 9. We expect a negative effect on routine-task labor, and a positive effect on nonroutine-task labor.

Since 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 can potentially affect these outcomes. In Table 6, we run the following first-difference regression by including changes in state Section 179 limits in the year before t (possible delayed response), and in each of the three years during t to $t+3$ (contemporaneous responses).

$$\Delta L_{j,s,t\to t+3} = b_0 + b_1 \Delta Limit_{s,t-2\to t-1} + b_2 \Delta Limit_{s,t-1\to t} + b_3 \Delta Limit_{s,t\to t+1} \quad (11)$$

$$+b_4 \Delta Limit_{s,t+1\to t+2} + b_5 \Delta L_{j,s,t-3\to t} + Dummy_{EmpBin\times Ind\times Year} + \epsilon_{j,s,t+3},$$

where $\Delta L_{j,s,t\to t+3}$ is the percent change in the number of employees (routine, nonroutine, and total) or the level change in the share of routine-task labor from year t to $t+3$ and $\Delta L_{j,s,t\to t+3}$ controls for the past trend of the dependent variable. If establishments' respond with delay, we expect to see b_1 to be significant. If the response is contemporaneous, we expect to see some or all of b_2, b_3, and b_4 to be significant.

In Table 6, we see an interesting finding that firms respond to state incentives in equipment investment by hiring nonroutine-task labor quickly, with both b_2 and b_3 being significant. However, firms' response for routine-task labor differs in both direction and timing — firms significantly reduce routine-task labor starting from one year after the states increase their Section 179 limits. Putting the responses of routine and nonroutine employment together, we see that firms' share of routine-task labor goes down both instantly and also with a delay. The change in firm's total employment is not significant due to offsetting effects.

[TABLE 6 ABOUT HERE]

To further understand the mechanism, in what follows, we will inspect the contemporaneous and delayed responses from each of the four labor measures to changes in state Section 179 limits. We will also conduct the analyses among eligible and ineligible establishments, and with and without controls for changes in potential confounding factors at the state level. Specifically, we will run the following first-difference regressions.

$$\text{Delayed Response: } \Delta L_{j,s,t\to t+3} = b_0 + b_1 \Delta Limit_{s,t-2\to t-1} + b_2 \Delta X_{s,t-2\to t-1} + Dummy_{EmpBin\times Ind\times Year} + \epsilon_{j,s,t+3}, \quad (12)$$

$$\text{Contemporaneous Response: } \Delta L_{j,s,t\to t+3} = b_0 + b_1 \Delta Limit_{s,t-1\to t} + b_2 \Delta X_{s,t-1\to t} + Dummy_{EmpBin\times Ind\times Year} + \epsilon_{j,s,t+3}, \quad (13)$$

Changes in deduction limits and other state controls are measured from year $t-1$ to t when inspecting the contemporaneous responses, and from year $t-2$ to $t-1$ when inspecting delayed responses.

Table 7 confirms that most decline in routine-task employment happens with one year delay, while Table 8 shows that nonroutine-task employment responds more quickly to policy changes. Both results are significant only among eligible establishments. Table 9 confirms that establishments' share of routine-task labor demonstrates both contemporaneous and delayed response to state Section 179 limit changes, and is again only significant among eligible establishments. The delayed response is slightly larger in magnitude, reflecting the strong delayed response in reducing routine-task employment. Table 10 shows that establishments' total employment does not respond to policy shocks at either horizon, for either group of establishments.

[TABLE 7 ABOUT HERE]

[TABLE 8 ABOUT HERE]

[TABLE 9 ABOUT HERE]

[TABLE 10 ABOUT HERE]

Table 11 replicates our main labor results using wage bills (wage times employment) instead of employment counts. These results are very similar to our baseline results reported in Tables 7-10: We find contemporaneous positive response of nonroutine employment, and mostly delayed negative response of routine employment to state limit increases.

[TABLE 11 ABOUT HERE]

Since we run our regressions with first differences, our specification removes unobserved firm-specific fixed effects in the corresponding levels equation. We believe it is unlikely that establishments might have some unobservable characteristics that also correlate with the state Section 179 deduction limit changes they face. However, to rule out the possibility that such an unobservable characteristic might be driving the results, in untabulated results we re-examine our main specifications by adding establishment fixed effects. Regressions with establishment fixed effects lead to a reduction in the sample size, and the drop in sample size is especially severe in the OES sample due to their sampling methodology. OES very rarely surveys the same establishment more than three times. Since our specifications already require two consequtive observations to calculate first differences, and an additional observation to include the past trend, specifications with establishment fixed effects require at least four consecutive observations. Despite a severe drop in sample size, especially for the smaller eligible firms, results with establishment fixed effects are qualitatively similar to our benchmark results. We find strong results for PC growth and routine-task labor regressions, but the nonroutine task labor results get weaker, possibly due to the severe drop in sample size.

Are the establishments that make additional investment in computers and increase nonroutine-task employees the same establishments that later reduce routine-task employees in larger numbers? Or, is it possible that, some establishments made additional computer investments in response to Section 179 limit increases, while other establishments later reduced their routine-task employees? While the answers to these questions do not affect our paper's main finding that routine and nonroutine-task employees face different job prospects in the aftermath of the investment incentive, knowing the answer would help us understand the channel that caused such heterogeneous effects on the two groups of workers. To investigate whether the subsequent reduction of routine-task employment is driven by the substitution effect of the computer investment, we merge our two datasets at the establishment-year level. While merging the datasets provides us with a richer dataset that allows additional tests, it also leads to a substantial loss in the number of observations. With this caveat, we proceed with our conditional tests. In columns (1) and (2) of Table 12, we we run the following regressions with an interaction term:

$$\begin{aligned} \Delta Emp^{Routine}_{j,s,t\to t+3} &= b_0 + b_1 \Delta Limit_{s,t-2\to 1} \times \Delta PC_{j,s,t-1\to t} + b_2 \Delta Limit_{s,t-2\to t-1} \\ &\quad + b_3 \Delta PC_{j,s,t-1\to t} + b_4 \Delta X_{s,t-1\to t} + Dummy_{EmpBin\times Ind\times Year} + \epsilon_{j,s,t+3}, \end{aligned} \tag{14}$$

where the left hand side of the regression is change in routine-task labor. We expect to find a negative estimate for the interaction term, b_1, implying that when states increase Section 179 limits, firms that react by investing in additional PCs will subsequently reduce more of their routine task employment than matching firms that did not make this investment. Table 12 confirms that b_3 is negative for the routine task employment of eligible firms. One drawback is that the establishment-year matching between CiTDB and OES sample yields an extremely small sample.

In columns (3) and (4) we inspect whether establishments that responded to increases in state Section 179 limits by hiring more nonroutine-task labor subsequently reduce more routine-task employees. The answer is yes.[25]

Can the delayed decline in routine-task labor be a result of negative autocorrelation in employment, possibly due to measurement errors in employment data or lumpy hiring decisions? Specifically, establishments that previously hired (any type of) workers following the previous changes in state Section 179 limits may potentially be less likely to hire this year. To test this hypothesis, which would act as a placebo test, in columns (5) and (6) we examine whether the results in columns (3) and (4) are driven by previous hiring of routine-task employees. When we condition on past routine hiring, instead of nonroutine hiring, we don't find any effect for the interaction term. These results provide further support for our earlier findings that firms that respond to tax incentives with increased investment and nonroutine hiring tend to consequently reduce routine-task employment.

[TABLE 12 ABOUT HERE]

6. Conclusion

This paper explores the implications of investment tax incentives for small firms on investment and labor outcomes using two establishment-level datasets. Standard models with homogeneous investment and labor inputs imply that both inputs should respond positively to tax incentives. Earlier literature confirms the positive effect on investment, but there is little and inconclusive evidence on the employment side. We depart from the earlier work in recognizing that the labor input is not homogeneous: Substitutability between capital and routine-task labor, and complementarity between routine inputs and nonroutine-task labor lead to starkly different implications for the two types of workers. We find that routine-task labor is reduced, and nonroutine-task labor is increased by eligible firms in response to tax incentives. In aggregate, there is no effect on the total employment of eligible firms.

We draw our conclusions from two micro datasets that are not well explored in the

[25] To make sure we have enough observations to perform this analysis, we do not control for past trend in this regression, which increases the number of observations significantly. Adding past trend makes the estimates conditional on past computer investment insignificant, but do not change the results conditional on past hiring of nonroutine-task labor.

economics literature, and allow us to inspect some key aspects of the mechanism. They both provide an extensive coverage of the small firms eligible for the incentive. The OES program at the BLS contains very detailed occupation-establishment level data that allows investigating the heterogeneity in the labor pool of establishments.

While our work shows that tax policy has asymmetric effects on the employment of routine- and nonroutine-task labor by the eligible firms, which offset each other, we refrain from extrapolating the effect to the total employment in the economy. General equilibrium effects of the tax policy, and its welfare implications are beyond the scope of this paper and are left for future research.

Furthermore, our identification relies on relatively small differences in tax policy across states with limited monetary effect on the firms' bottom lines. We find strong response to these differences in tax policy, however, one should exercise caution when extrapolating the effects we find to larger changes in policy.

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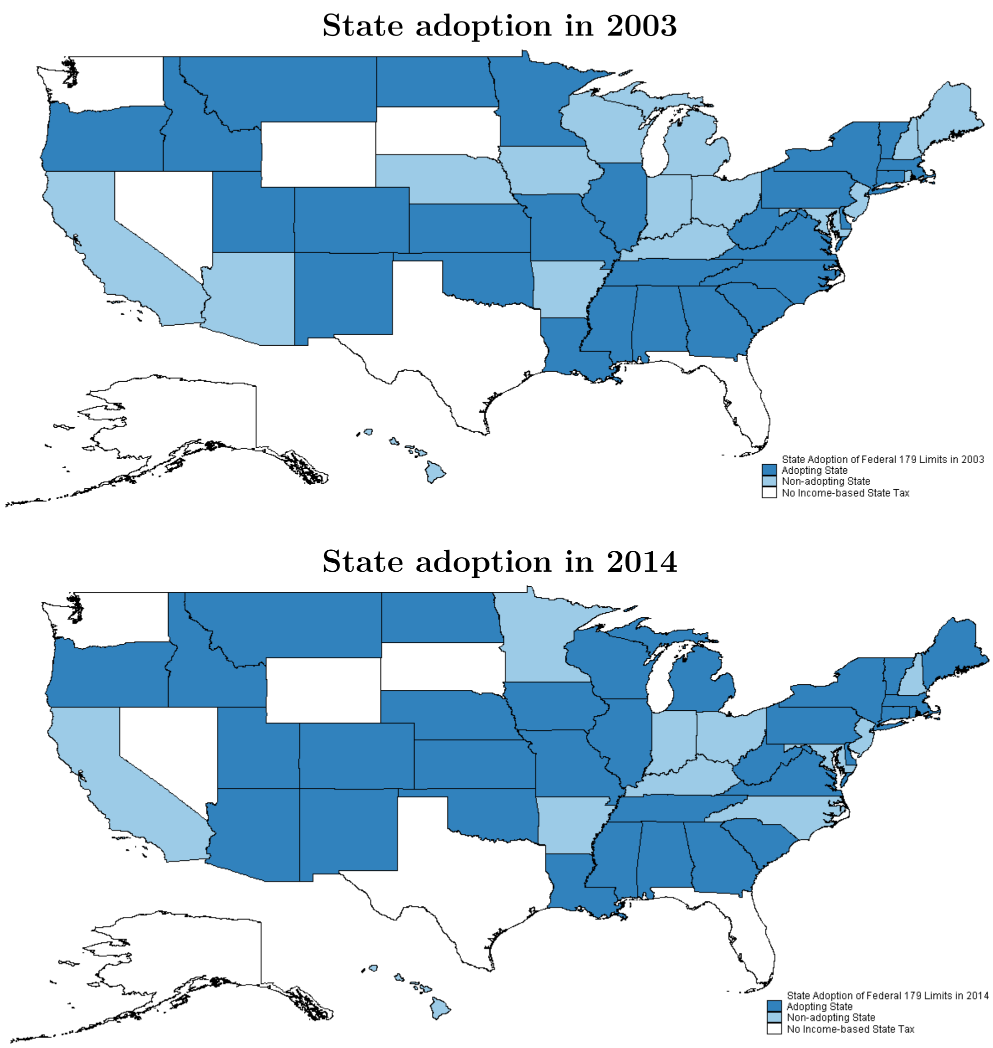

Figure 1. **State adoption of federal Section 179 deduction limits in 2003 and 2014.** This figure provides snapshots of states' adoption of federal Section 179 deduction limits in 2003 and 2014, respectively. States with zero personal income tax rate are colored white. Dark blue states adopt federal Section 179 deduction limits. Light blue states do not adopt federal limits.

Table 1
Timeline of Federal Section 179 Program

This table shows the timeline for the Section 179 federal deduction limits and the phase-out thresholds. Deduction limit is the maximum deduction that a firm may claim in a year. If the firm's investment in qualifying equipment and software in a given year exceeds the phaseout threshold, 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	
2/27/2003	5/28/2003	2003-2005	$100,000	$400,000	Jobs and Growth Tax Relief Reconciliation Act
6/4/2004	10/22/2004	2006-2007	$100,000	$400,000	American Jobs Creation Act of 2004
1/17/2007	5/25/2007	2007	$125,000	$500,000	Small Business and Work Opportunity Tax Act of 2007
1/28/2008	2/13/2008	2008	$250,000	$800,000	Economic Stimulus Act of 2008
1/26/2009	2/17/2009	2009	$250,000	$800,000	American Recovery and Reinvestment Tax Act of 2009
6/12/2009	3/18/2010	2010	$250,000	$800,000	Hiring Incentives to Restore Employment Act
5/13/2010	9/27/2010	2010-2011	$500,000	$2,000,000	Small Business Jobs and Credit Act of 2010
7/24/2012	1/2/2013	2012-2013	$500,000	$2,000,000	American Taxpayer Relief Act of 2012
12/1/2014	12/19/2014	2014	$500,000	$2,000,000	Tax Increase Prevention Act of 2014

Table 2
Changes in State Section 179 Deduction Limits

This table relates 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. *Tax Bonus Conformity* is a dummy variable that equals to 1 if the state adopts the federal bonus depreciation tax incentive. *Budget Surplus* is the state's budget surplus in $ millions (negative means budget deficit). *Democratic Dummy* is a dummy variable that equals to 1 if the state is governed by a Democratic governor. *Personal Inc. Tax Rate* and *Corporate Inc. Tax Rate* are the state's personal and corporate income tax rates, respectively. *Share of Routine-Task Labor (RShare)* is the state's share of routine-task labor. *GSP Growth* is the growth rate of real gross state product. For variable definitions, see Section 4. All regressions have year fixed effects. States with zero personal income tax rate are included in the set of states that don't change state 179 deduction limits. Standard errors are clustered at the state level and reported in parentheses. * , ** , and *** represent significance at the 10%, 5%, and 1% level, respectively. Sample period is 2003-2014.

	Changes in State Section 179 Limit ($thousands)				
	(1)	(2)	(3)	(4)	(5)
Δ State Hiring Credits	1.08				0.33
	(3.85)				(3.83)
Δ State Bonus Adoption	42.72**				42.79**
	(19.73)				(20.07)
Δ State Budget Surplus		−1.72			−1.89
		(1.06)			(1.14)
Δ State GSP		−0.52			−0.96
		(0.99)			(0.97)
Δ State Unemployment			−0.73		−2.04
			(5.51)		(5.37)
Δ State RShare			−8.06*		−6.52
			(4.20)		(4.29)
Δ State Income Tax				2.37	1.51
				(4.78)	(4.31)
Δ State Corp. Tax				0.68	0.72
				(3.68)	(3.89)
Δ State Democratic Dummy				−1.41	−1.18
				(11.03)	(11.02)
Observations	516	516	516	516	516
Adjusted R^2	0.33	0.31	0.31	0.31	0.33

Table 3
Summary Statistics for Establishments

This reports the summary statistics for employment and computers at the establishment level. Employment data is from the Occupational Employment Statistics database at the Bureau of Labor Statistics. L_{tot}, L_R, L_N, $RShare$ are the establishments' total employment, employment of routine-task labor, employment of nonroutine-task labor, and share of routine-task labor, respectively. Computer data is from the Computer Intelligence Technology Database (CiTDB), previously known as Harte-Hanks database. PC is the total number of computers in the establishment. We require establishments to have consecutive observations that allow computation of current and past employment or investment growth for calculating the past growth trend and future growth rates. $\Delta State\ Limit179 > 0$ are the state-year observations when the state increases its Section 179 deduction limit. States with zero personal income tax rate are included in the set of states that don't change state 179 deduction limits. The sample period is 2003-2014, during which there are 218, 292, and 6 state-year observations that experienced increased, unchanged, and decreased Section 179 limits, respectively.

	ΔState Limit179 > 0			ΔState Limit179 ≤ 0		
	Mean	S.D.	Median	Mean	S.D.	Median
OES Sample						
L_{tot}	198.51	792.73	50	204.42	773.48	50
L_R	33.07	115.03	5	33.44	119.77	5
L_N	165.43	719.97	38	170.98	692.16	38
$RShare(\%)$	19.68	24.11	10	19.75	24.10	10
Obs.	178,569	–	–	255,306	–	–
CiTDB Sample						
L_{tot}	163.55	760.58	55	170.85	662.66	50
PC	125.91	671.87	30	146.67	1759.62	31
Obs.	298,298	–	–	434,389	–	–

Table 4
Response of PC Investment to Changes in State Section 179 Deduction Limits

This table reports the effect of changes in state Section 179 deduction limits on establishments' investment in computers, using first-difference regressions. The dependent variable is the growth rate of the number of computers in each establishment from year t to $t+1$. The key independent variable, Δ*State Limit179*, is the change in the maximum deduction that a firm may claim in a year from the state taxes from year $t-1$ to t, presented in millions. Changes in state political, economic, and other policy characteristics from year $t-1$ to t are added to control for confounding effects. *Past Trend* is the establishment's PC growth from year $t-1$ to year t. *Eligible* and *Ineligible* establishments are establishments with estimated investment in equipment below and above the Section 179 phaseout threshold in year $t-1$, respectively. Establishments with zero or one employees or from industries in which computer investment accounts for less than five percent of total equipment and software investment are excluded from the sample. States with zero personal income tax rate are included in the set of states that don't change state 179 deduction limits. All regressions have fixed effects that include a full interaction of 8 employment bins, NAICS 4-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 above 200. Standard errors are clustered at the state level and reported in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% level, respectively. The sample covers establishments computer investment in 2003-2014 .

	All Establishments		Eligible Establishments		Ineligible Establishments	
	(1)	(2)	(3)	(4)	(5)	(6)
Δ State Limit179	5.63**	6.23**	7.98***	8.79***	0.70	0.72
	(2.25)	(2.63)	(2.53)	(2.88)	(3.56)	(3.98)
Δ State Hiring Credits		−0.11		−0.18		0.16
		(0.28)		(0.30)		(0.34)
Δ State Bonus Adoption		−0.06		−0.18		0.22
		(0.29)		(0.38)		(0.58)
Δ State RShare		10.81		9.00		15.14
		(28.38)		(27.01)		(51.70)
Δ State Democratic Dummy		−0.33		0.21		−1.34**
		(0.48)		(0.58)		(0.61)
Δ State Credit Score		0.09		0.09		0.06
		(0.10)		(0.11)		(0.22)
Δ State Unemployment		0.15		0.10		0.33
		(0.14)		(0.14)		(0.24)
Δ State Budget Surplus		−0.02		−0.06**		0.09
		(0.03)		(0.03)		(0.06)
Δ State Income Tax		−0.09		−0.09		0.01
		(0.12)		(0.11)		(0.24)
Δ State Corp. Tax		−0.04		−0.08		0.09
		(0.05)		(0.05)		(0.16)
Δ State GSP		0.02		0.03		−0.01
		(0.06)		(0.07)		(0.08)
Past Trend	−0.17***	−0.17***	−0.18***	−0.18***	−0.15***	−0.15***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)
Observations	384303	384303	282348	282348	101862	101862
Adjusted R^2	0.19	0.19	0.21	0.21	0.14	0.14

Table 5
Contemporaneous and Delayed Response of PC Investment to Changes in State Section 179 Deduction Limits

This table reports the effect of changes in state Section 179 deduction limits on establishments' investment in computers, using first-difference regressions. The dependent variable is the growth rate of the number of computers in each establishment from year t to $t+1$. The key independent variable, Δ*State Limit179*, is the change in the maximum Section 179 deduction from state taxes that a firm may claim in a year, presented in million dollars. In addition to the baseline deduction limit change measured from year $t-1$ to t, we include two lagged changes in state Section 179 limits. Changes in state political, economic, and policy characteristics from year $t-1$ to t are added to control for confounding effects. *Past Trend* is the establishment's PC growth from year $t-1$ to year t. *Eligible* and *Ineligible* establishments are firms with estimated investment in equipment below and above the federal Section 179 phaseout threshold in year $t-1$, respectively. Establishments with zero or one employees or from industries in which computer investment accounts for less than five percent of total equipment and software investment are excluded from the sample. States with zero personal income tax rate are included in the set of states that don't change state 179 deduction limits. All regressions have fixed effects that include a full interaction of 8 employment bins, NAICS 4-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 above 200. Standard errors are clustered at the state level and reported in parentheses. * , ** , and *** represent significance at the 10%, 5%, and 1% level, respectively.

	All Establishments		Eligible Establishments		Ineligible Establishments	
	(1)	(2)	(3)	(4)	(5)	(6)
ΔState Limit179 [$t=-2$]	1.09	0.18	1.43	0.14	−3.47	−2.42
	(2.74)	(3.02)	(2.45)	(2.73)	(6.66)	(7.77)
ΔState Limit179 [$t=-1$]	−2.24	−1.70	−3.52	−2.86	2.67	3.17
	(2.69)	(2.92)	(2.51)	(2.76)	(4.25)	(4.43)
ΔState Limit179 [$t=0$]	5.92**	6.63**	8.25***	9.18***	0.95	1.19
	(2.24)	(2.65)	(2.59)	(2.96)	(3.59)	(4.14)
State Controls	N	Y	N	Y	N	Y
Observations	370859	370859	271927	271927	98835	98835
Adjusted R^2	0.19	0.19	0.21	0.21	0.14	0.14

Table 6
Contemporaneous and Delayed Response of Labor Outcomes to Changes in State Section 179 Deduction Limits

This table reports the effect of changes in state Section 179 deduction limits on establishments' employment of routine-task labor, nonroutine-task labor, share of routine-task labor and total employment, using first-difference regressions. The dependent variables are the three-year growth rates of the number of employees in each establishment from year t to $t+3$ (change in routine share is calculated as level changes in routine employment share). The key independent variable, Δ*State Limit179*, is the change in the annual state Section 179 deduction limit, presented in million dollars. We include Δ*State Limit179* in year $t-1$ (i.e., measured from year $t-2$ to $t-1$, to capture possible delayed response), and in each of the three following years (measured from $t-1$ to t, t to $t+1$, $t+1$ to $t+2$, to capture contemporaneous responses). *Past Trend* is the establishment's appropriate employment growth (or routine share change) from year $t-3$ to year t. Establishments with five or fewer employees are excluded from the sample. States with zero personal income tax rate are included in the set of states that don't change state 179 deduction limits. All regressions have fixed effects that include a full interaction of 8 employment bins, NAICS 4-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 above 200. Standard errors are clustered at the state level and reported in parentheses. * , ** , and *** represent significance at the 10%, 5%, and 1% level, respectively.

	Changes from t to $t+3$			
	Routine Emp (1)	NonRoutine Emp (2)	Routine Share (3)	Total Emp (4)
ΔState Limit179 [$t=-1$]	−18.85***	3.54	−2.39*	−1.20
	(6.36)	(2.76)	(1.20)	(2.09)
ΔState Limit179 [$t=0$]	−10.94	3.26*	−1.95*	−0.11
	(6.85)	(1.63)	(1.03)	(1.56)
ΔState Limit179 [$t=1$]	−0.73	4.37*	−0.90	1.84
	(7.17)	(2.56)	(1.07)	(2.55)
ΔState Limit179 [$t=2$]	−2.49	0.04	−0.21	0.72
	(4.17)	(3.04)	(0.67)	(2.11)
Past Trend	−0.45***	−0.29***	−0.44***	−0.14***
	(0.00)	(0.01)	(0.00)	(0.01)
Observations	317121	358701	359099	359099
Adjusted R^2	0.22	0.11	0.21	0.07

Table 7
Contemporaneous and Delayed Response of Routine-Task Employment to Changes in State Section 179 Deduction Limits

This table reports the contemporaneous and delayed effects of changes in state Section 179 deduction limits on establishments' employment of routine-task labor, using first-difference regressions. The dependent variable is the three-year growth rate of the number of routine-task employees in each establishment from year t to $t+3$. The key independent variable, Δ*State Limit179*, is the change in the maximum deduction that a firm may claim in a year from the state taxes from year $t-1$ to t, presented in millions. Changes in state political and economic characteristics from year $t-1$ to t are added to control for confounding effects (state controls). *Eligible* and *Ineligible* establishments are firms with estimated investment in equipment below and above the Section 179 phaseout threshold, respectively. *Past Trend* is the establishment's routine-task employment growth from year $t-3$ to year t. Δ*State Limit179*, state controls, and establishment eligibility criterion are lagged by one year in Panel B, where we measure the delayed response of routine employment to deduction limit changes. Establishments with five or fewer employees are excluded from the sample. States with zero personal income tax rate are included in the set of states that don't change state 179 deduction limits. All regressions have fixed effects that include a full interaction of 8 employment bins, NAICS 4-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 above 200. Standard errors are clustered at the state level and reported in parentheses. * , ** , and *** represent significance at the 10%, 5%, and 1% level, respectively.

Panel A: Contemporaneous Response to Changes in State Section 179 Limits						
	All Establishments		Eligible Establishments		Ineligible Establishments	
	(1)	(2)	(3)	(4)	(5)	(6)
ΔState Limit179	−10.22	−10.54	−12.48*	−11.84*	−2.91	−5.82
	(6.89)	(6.88)	(6.95)	(6.98)	(8.47)	(8.63)
State Controls	N	Y	N	Y	N	Y
Observations	348299	348299	225840	225840	121954	121954
Adjusted R^2	0.22	0.22	0.24	0.24	0.20	0.20

Panel B: Delayed Response to Changes in State Section 179 Limits						
	All Establishments		Eligible Establishments [t=-1]		Ineligible Establishments [t=-1]	
	(1)	(2)	(3)	(4)	(5)	(6)
ΔState Limit179 [$t=-1$]	−19.14**	−20.34***	−23.08***	−24.77***	−9.98	−10.34
	(7.42)	(7.15)	(8.31)	(8.50)	(8.64)	(7.47)
State Controls	N	Y	N	Y	N	Y
Observations	348299	348299	211790	211790	135295	135295
Adjusted R^2	0.22	0.22	0.24	0.24	0.20	0.20

Table 8
Contemporaneous and Delayed Response of Nonroutine-task Employment to Changes in State Section 179 Deduction Limits

This table reports the contemporaneous and delayed effects of changes in state Section 179 deduction limits on establishments' employment of nonroutine-task labor, using first-difference regressions. The dependent variable is the three-year growth rate of the number of nonroutine-task employees in each establishment from year t to $t+3$. The key independent variable, Δ*State Limit179*, is the change in the maximum deduction that a firm may claim in a year from the state taxes from year $t-1$ to t, presented in millions. Changes in state political and economic characteristics from year $t-1$ to t are added to control for confounding effects (state controls). *Past Trend* is the establishment's nonroutine-task employment growth from year $t-3$ to year t. *Eligible* and *Ineligible* establishments are firms with estimated investment in equipment below and above the Section 179 phaseout threshold, respectively. Δ*State Limit179*, state controls, and establishment eligibility criterion are lagged by one year in Panel B, where we measure the delayed response of routine employment to deduction limit changes. Establishments with five or fewer employees are excluded from the sample. States with zero personal income tax rate are included in the set of states that don't change state 179 deduction limits. All regressions have fixed effects that include a full interaction of 8 employment bins, NAICS 4-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 above 200. Standard errors are clustered at the state level and reported in parentheses. * , ** , and *** represent significance at the 10%, 5%, and 1% level, respectively.

Panel A: Contemporaneous Response to Changes in State Section 179 Limits

	All Establishments		Eligible Establishments		Ineligible Establishments	
	(1)	(2)	(3)	(4)	(5)	(6)
Δ State Limit179	2.99**	3.63**	3.91**	4.45**	0.63	1.30
	(1.42)	(1.69)	(1.79)	(2.07)	(3.15)	(3.31)
State Controls	N	Y	N	Y	N	Y
Observations	393686	393686	266342	266342	126864	126864
Adjusted R^2	0.12	0.12	0.12	0.12	0.11	0.11

Panel B: Delayed Response to Changes in State Section 179 Limits

	All Establishments		Eligible Establishments [t=-1]		Ineligible Establishments [t=-1]	
	(1)	(2)	(3)	(4)	(5)	(6)
ΔState Limit179 [t=-1]	4.04	5.49**	4.06	5.68*	4.77	5.87*
	(2.57)	(2.40)	(3.23)	(3.14)	(3.73)	(3.45)
State Controls	N	Y	N	Y	N	Y
Observations	393686	393686	251229	251229	141188	141188
Adjusted R^2	0.12	0.12	0.12	0.12	0.11	0.11

Table 9
Contemporaneous and Delayed Response of Routine-task Employment Share to Changes in State Section 179 Deduction Limits

This table reports the contemporaneous and delayed effects of changes in state Section 179 deduction limits on establishments' routine-task employment share, using first-difference regressions. The dependent variable is the three-year (level) change in the share of routine-task labor in each establishment from year t to $t+3$. The key independent variable, Δ*State Limit179*, is the change in the maximum deduction that a firm may claim in a year from the state taxes from year $t-1$ to t, presented in millions. Changes in state political and economic characteristics from year $t-1$ to t are added to control for confounding effects (state controls). *Eligible* and *Ineligible* establishments are firms with estimated investment in equipment below and above the Section 179 phaseout threshold, respectively. *Past Trend* is the change in establishment's routine-task employment share from year $t-3$ to year t. Δ*State Limit179*, state controls, and establishment eligibility criterion are lagged by one year in Panel B, where we measure the delayed response of routine employment share to deduction limit changes. Establishments with five or fewer employees are excluded from the sample. States with zero personal income tax rate are included in the set of states that don't change state 179 deduction limits. All regressions have fixed effects that include a full interaction of 8 employment bins, NAICS 4-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 above 200. Standard errors are clustered at the state level and reported in parentheses. * , ** , and *** represent significance at the 10%, 5%, and 1% level, respectively.

Panel A: Contemporaneous Response to Changes in State Section 179 Limits						
	All Establishments		Eligible Establishments		Ineligible Establishments	
	(1)	(2)	(3)	(4)	(5)	(6)
Δ State Limit179	−1.86*	−2.00**	−2.34**	−2.37**	−0.41	−0.95
	(0.96)	(0.93)	(1.09)	(1.03)	(1.06)	(1.13)
State Controls	N	Y	N	Y	N	Y
Observations	394111	394111	266762	266762	126869	126869
Adjusted R^2	0.21	0.21	0.21	0.21	0.22	0.22

Panel B: Delayed Response to Changes in State Section 179 Limits						
	All Establishments		Eligible Establishments [t=-1]		Ineligible Establishments [t=-1]	
	(1)	(2)	(3)	(4)	(5)	(6)
ΔState Limit179 [t=-1]	−2.37*	−2.82**	−2.69*	−3.24**	−1.73	−2.01
	(1.23)	(1.16)	(1.43)	(1.42)	(1.49)	(1.38)
State Controls	N	Y	N	Y	N	Y
Observations	394111	394111	251644	251644	141197	141197
Adjusted R^2	0.21	0.21	0.21	0.21	0.22	0.22

Table 10
Contemporaneous and Delayed Response of Total Employment to Changes in State Section 179 Deduction Limits

This table reports the contemporaneous and delayed effects of changes in state Section 179 deduction limits on establishments' total employment, using first-difference regressions. The dependent variable is the three-year growth rate of the number of employees in each establishment from year t to $t+3$. The key independent variable, Δ*State Limit179*, is the change in the maximum deduction that a firm may claim in a year from the state taxes from year $t-1$ to t, presented in millions. Changes in state political and economic characteristics from year $t-1$ to t are added to control for confounding effects (state controls). *Past Trend* is the establishment's total employment growth from year $t-3$ to year t. *Eligible* and *Ineligible* establishments are firms with estimated investment in equipment below and above the Section 179 phaseout threshold, respectively. Δ*State Limit179*, state controls, and establishment eligibility criterion are lagged by one year in Panel B, where we measure the delayed response of total employment to deduction limit changes. Establishments with five or fewer employees are excluded from the sample. States with zero personal income tax rate are included in the set of states that don't change state 179 deduction limits. All regressions have fixed effects that include a full interaction of 8 employment bins, NAICS 4-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 above 200. Standard errors are clustered at the state level and reported in parentheses. * , ** , and *** represent significance at the 10%, 5%, and 1% level, respectively.

Panel A: Contemporaneous Response to State Section 179 Limits

	All Establishments		Eligible Establishments		Ineligible Establishments	
	(1)	(2)	(3)	(4)	(5)	(6)
Δ State Limit179	−0.24	0.00	0.17	0.48	−1.40	−1.70
	(1.63)	(1.74)	(1.79)	(1.89)	(2.53)	(2.72)
State Controls	N	Y	N	Y	N	Y
Observations	394111	394111	266762	266762	126869	126869
Adjusted R^2	0.08	0.08	0.07	0.07	0.08	0.08

Panel B: Delayed Response to Changes in State Section 179 Limits

	All Establishments		Eligible Establishments [t=-1]		Ineligible Establishments [t=-1]	
	(1)	(2)	(3)	(4)	(5)	(6)
ΔState Limit179 [t=-1]	−0.80	−0.45	−0.76	−0.49	−1.18	−0.74
	(2.10)	(1.90)	(2.14)	(2.06)	(3.23)	(2.78)
State Controls	N	Y	N	Y	N	Y
Observations	394111	394111	251644	251644	141197	141197
Adjusted R^2	0.08	0.08	0.08	0.08	0.08	0.08

Table 11
Contemporaneous and Delayed Responses of Wage Bills to Changes in State Section 179 Deduction Limits

This table reports the contemporaneous and delayed responses of wage bills for routine, nonroutine, and total employees, and routine-task employees' wage bill share to changes in state Section 179 deduction limits (i.e., replaces the employment-based dependent variables in Tables 7-10 with wage-bill based measures). Contemporaneous and delayed response regressions are run separately and only coefficients and standard errors for the policy variable, i.e., the current or the lagged change in state Section 179 limits, are reported. See Tables 7-10 for additional details. * , ** , and *** represent significance at the 10%, 5%, and 1% level, respectively.

	All Establishments		Eligible Establishments		Ineligible Establishments	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Responses of Wage Bills for Routine-Task Labor						
ΔLimit179	−10.94	−11.28	−13.07*	−12.43	−4.06	−6.87
	(7.69)	(7.51)	(7.78)	(7.67)	(9.11)	(9.15)
ΔLimit179 [t-1]	−20.42**	−21.29***	−24.73***	−26.33***	−10.40	−9.94
	(8.42)	(7.82)	(9.23)	(9.20)	(9.47)	(7.71)
Panel B: Responses of Wage Bills for Nonroutine-Task Labor						
ΔLimit179	4.19*	4.83**	5.09*	5.67**	1.61	2.57
	(2.20)	(2.37)	(2.56)	(2.79)	(3.78)	(3.95)
ΔLimit179 [t-1]	5.04	6.84**	4.71	6.89*	6.76	7.88*
	(3.51)	(3.02)	(3.87)	(3.51)	(4.66)	(4.06)
Panel C: Responses of Routine-Task Wage Bill Share						
ΔLimit179	−1.70*	−1.86**	−2.14**	−2.25**	−0.35	−0.78
	(0.86)	(0.83)	(0.99)	(0.94)	(0.94)	(1.00)
ΔLimit179 [t-1]	−2.25**	−2.70***	−2.45**	−3.08**	−1.82	−1.96*
	(1.04)	(0.99)	(1.21)	(1.20)	(1.24)	(1.17)
Panel D: Responses of Total Wage Bills						
ΔLimit179	1.47	1.75	1.80	2.06	0.39	0.73
	(2.56)	(2.50)	(2.90)	(2.83)	(3.34)	(3.50)
ΔLimit179 [t-1]	0.71	1.50	0.22	1.11	2.26	2.96
	(3.50)	(3.08)	(3.48)	(3.29)	(4.51)	(3.64)
State Controls	N	Y	N	Y	N	Y

Table 12
Delayed Response of Routine-Task Employment to Changes in State Section 179 Deduction Limits Conditional on Contemporaneous Responses

This table reports the delayed effect of changes in state Section 179 deduction limit on the establishment's routine-task employment, conditional on contemporaneous responses of the establishment's PC growth, nonroutine employment, and routine employment, using first-difference regressions. The results show that when states increase Section 179 deduction limits, establishments that respond by purchasing more computers and hiring nonroutine-task labor subsequently reduce their routine-task employment more. The dependent variable is the three-year growth in the number of routine-task employees from year t to $t+3$. The key independent variables are Δ*State Limit179 [t-1]*, the changes in the maximum deduction that a firm may claim in a year from the state taxes from year $t-2$ to $t-1$, presented in millions; *Lagged Response* which can be the changes in computers from $t-1$ to t, or changes in nonroutine-task employment from $t-3$ to t, or changes in routine-task employment from $t-3$ to t (Placebo Test). Changes in state political and economic characteristics from year $t-2$ to $t-1$ are added to control for confounding effects. *Eligible* establishments are establishments with estimated investment in equipment below the federal Section 179 phaseout threshold in year $t-1$. Establishments with five or fewer employees are excluded from the sample. States with zero personal income tax rate are included in the set of states that don't change state 179 deduction limits. All regressions include fixed effects that include a full interaction of 8 employment bins, NAICS 4-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 above 200. Standard errors are clustered at the state level and reported in parentheses. * , ** , and *** represent significance at the 10%, 5%, and 1% level, respectively.

	Cond. on $\Delta \text{PC}_{t-1,t}$		Cond. on $\Delta \text{Emp}^{NR}_{t-3,t}$		Cond. on $\Delta \text{Emp}^{R}_{t-3,t}$	
	All (1)	Eligible (2)	All (3)	Eligible (4)	All (5)	Eligible (6)
ΔState Limit179 [t=-1]	2.79	−30.63	−23.90***	−27.88***	−20.39***	−24.90***
	(16.89)	(22.54)	(6.65)	(7.80)	(7.12)	(8.42)
Conditioning Variable	−0.01	−0.01	0.31***	0.33***	−0.45***	−0.47***
	(0.01)	(0.02)	(0.01)	(0.01)	(0.00)	(0.00)
ΔState Limit179 [t=-1] × Cond. Var.	0.10	−0.25*	−0.18**	−0.19**	0.01	0.02
	(0.38)	(0.14)	(0.07)	(0.08)	(0.04)	(0.05)
State Controls	Y	Y	Y	Y	Y	Y
Observations	41246	16027	360728	222507	348299	211790
Adjusted R^2	0.04	0.03	0.04	0.03	0.22	0.24