How Does Capital Investment Affect Workers? Evidence from Bonus Depreciation and Matched Employer-Employee Data *

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Abstract

We use matched employer-employee data to study how tax policies that lower the cost of capital impact workers. The policy we study, bonus depreciation, was implemented by the United States government in 2001, coinciding with significant employment declines in manufacturing. Using difference-in-differences event study methodologies, we find that workers at affected firms in 2001 experience significant, persistent positive earnings gains through 2011. These workers are also significantly less likely to leave the labor force over the same time period. While earnings effects partly reflect gains for workers that remain at their 2001 firm, we find suggestive evidence that changes to worker mobility patterns are important for explaining our positive earnings estimates. These effects are strongest for workers likely to fare poorly in the face of sectoral transformation, particularly those in production occupations and relatively older workers. Take together, our results suggest that tax policies that stimulate capital investment can be particularly beneficial to workers facing external labor market risk.

Keywords: capital-labor substitution, bonus depreciation, corporate taxation
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Introduction

Governments around the world spend hundreds of billions of dollars on incentives for capital investment, with a growing body of literature suggests that these incentives have substantial effects on capital accumulation (House and Shapiro, 2008; Zwick and Mahon, 2017; Maffini et al., 2019). A particular concern among policymakers and the general public is that new technologies – possibly adopted in response to public subsidies – may lead to dismal outcomes for workers if firms replace their workforce with machinery. While recent work suggests that these incentives can stimulate labor demand (Curtis et al., 2023; Aghion et al., 2022; Hirvonen et al., 2022), it is much less clear how they affect worker earnings and employment over time. Such long-run outcomes may be particularly important for evaluating the efficacy of fiscal policy that incentivizes capital investment (Auerbach, 2003), particularly given the persistence of negative economic shocks on local labor markets (Yagan, 2019).

This project addresses this void by studying the effects of one of the largest tax incentives for capital investment in U.S. history on worker earnings and employment status. The federal tax policy we study, bonus depreciation ("bonus"), decreases the present value cost of capital investment, and does so more for firms in industries that investment in longer-lived assets as defined by the tax code. We combine industry-level bonus depreciation treatment intensity with confidential, matched employer-employee microdata from the U.S. Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) program. We then implement an event-study difference-in-difference analysis that compares earnings and employment outcomes of manufacturing workers that, at the time of policy implementation in 2001, were employed at firms that benefited most from bonus to those employed at firms that benefited less. Because, the period we study coincides with a dramatic decline in US manufacturing activity, we pay particular attention to job loss as a mediator for changes in worker earnings while including rich controls for the potentially confounding forces driving manufacturing sectoral transformation.

Our core analysis focuses on workers with a high degree of attachment to their firm in 2001, both in terms of job tenure (at least 3 years at the same firm) and earnings (at least $15,000 in each year of the same period). This focus ensures that our estimates do not reflect exposure to the policy from transitory job spells during a time of significant labor market upheaval in manufacturing. We find that workers at treated firms prior to policy implementation experienced a statistically significant increase in annual labor earnings of 3.7% over the 2002-2011 period. Affected workers were also 2.3% less likely to have exited the labor force and 1.9% less likely to have migrated to a different U.S. state by 2011. This result suggests that the potential welfare benefits of investment incentives go beyond earnings and employment by also reducing the likelihood that workers incur other labor market adjustments costs.

These estimates are robust to the inclusion of rich controls for the salient forces driving sectoral transformation, as defined in (Charles et al., 2019), which drove significant declines in

employment rates and hours worked among US manufacturing workers during our sample period. Heterogeneity analysis along these dimensions show that the effects of bonus on worker earnings are driven by larger effects among workers in industries with greater exposure to the Chinese import competition measure of Pierce and Schott (2016). Conversely, effects on worker earnings are smaller among workers in earnings with greater initial capital intensity or greater exposure to industrial robots (Acemoglu and Restrepo, 2020).

Given the apparent role of these sectoral trends, an important question that follows from these results is whether positive earnings effects reflect subsidized firms passing through the benefits associated with greater capital investment to workers (a rent-sharing channel) or changes to workers labor market attachment, whether through job loss or moves to different firms (a job mobility channel). We study these mechanisms by focusing on a balanced sample of workers that maintain positive annual earnings over the 2001–2011 period.

In contrast to our estimates on labor force exit, we find that bonus did not have a statistically significant effect on these workers' probability of separating from their 2001 firm over the study period. However, this effect masks important heterogeneity across different types of job separations: exposed workers were significantly more likely to experience a job-to-job transition in 2002, though these positive effects are more noisy through 2005. By 2005, exposed workers were significantly less likely to experience a job loss, as defined by a job separation that involved at least one quarter of zero earnings.[1] Thus, while bonus did not change these workers' overall likelihood of a job transition through the early years of policy exposure, it did lead to higher quality moves.

We explore the implications of these results for worker earnings by estimating heterogeneous effects across workers depending on whether they were separated from their original firm by 2005. Because job mobility is endogenous to our treatment, we rely on several different comparisons across treatment and firm separation status to tease out the relationship between mobility and our earnings effects. We first compare treated and control workers that do not move jobs before 2005. This "stayers" comparison shows that treated workers that retain their 2001 jobs experience very similar earnings growth to "stayer" control workers. Using industry-level estimates on value-added per worker, these estimates imply a pass-through elasticity of 0.57 for workers at exposed firms that retain their jobs through 2005. This estimate is relatively large compared to existing pass-through estimates, implying that rent-sharing is an important margin through which bonus improves worker earnings.

At the same time, treated workers that move firms had significantly lower average earnings than the average control worker (a "movers vs. all" comparison) over the 2001-2005 period, peaking at a 7.9% relative earnings decline in 2003. This result is consistent with the idea that

[1]The LEHD does not report whether job changes reflect lay-offs or quits. As a result, we define separations as any year where a worker no longer reports positive earnings at their original firm.

job loss in manufacturing during our sample period was particularly harmful to workers, even among workers whose firms benefited from investment subsidies (Pierce et al., 2024). Still, we estimate precise zero earnings differences between these groups after 2006.

A "movers vs. movers" comparison yields a large positive earnings effect for treated workers, particularly over the 2002-2005 period, that exceeds that of "stayers" by a peak of 5.8 percentage points in 2003. Unsurprisingly, treated "stayers" had larger earnings over our sample period compared to the average control worker (a "stayers vs. all" comparison). Taken together, these results provide evidence that, while bonus did lead to earnings increases for incumbent workers that remained at their original firm, job mobility played an important role in explaining our estimates, both by reducing the likelihood of job loss and by tempering the earnings losses associated with job separation.

In the final part of our analysis, we document important heterogeneity across worker types that speaks to the role of bonus for ameliorating the costs of sectoral reallocation for workers. We find that positive earnings effects are strongest for workers in production occupations and workers over the age of 35 in 2001.[2] To the extent that these groups were either more likely to experience the obsolescence of existing skills or a disadvantage in acquiring new skills,[3] these results provide further suggestive evidence that bonus benefited workers by preserving existing jobs for the workers most threatened by forces of sectoral transformation. The positive effects of bonus exposure on 2011 labor force participation was also strongest for production workers and workers without a college education. In other words, bonus benefited these workers both by preserving labor market attachment and generating better labor market outcomes for these workers.

Our study contributes to a growing literature on the effects of tax policy on worker outcomes. In a previous paper, Curtis et al. (2023) find that the 2001 bonus depreciation policy lead to significant increases in employment and capital investment among exposed plants and industries. In the context of a structural model of factor demand, these estimates imply that capital and production employment were complements, though close to 90% of the positive employment effect was driven by an overall decline in the cost of production (i.e. a scale effect). The policy did not, however, increase average earnings at plants or TFP. Similarly, Garrett et al. (2020) find that local labor markets with greater exposure saw increases in employment but no effect on average wages. In closely related work, Moon and Duan (2024) study the effects of a 2007 Canadian accelerated depreciation policy on long-run worker outcomes, finding that policy exposure lowered earnings for incumbent workers at affected firms. In contrast, we show that

[2]Because information on worker occupation and (unimputed) educational attainment is not readily available in the LEHD, we supplement our analysis with the 2000 Decennial Census Long-Form Questionnaire, which reports this information for a 20% cross section of U.S. workers.

[3]Retraining of workers displaced by technological change is an important focus of so-called "active" labor market policies designed to dull the effects of job loss on workers. Card et al. (2018) report that these policies have been shown to be relatively less effective for helping older workers readjust to new positions.

highly attached manufacturing workers in 2001 experienced an increase in earnings as a result of the U.S. wave of bonus depreciation.

In contextualizing these results, we also speak to an extensive literature on the causes and consequences of sectoral transformation in U.S. manufacturing. Charles et al. (2019) document that the decline in US manufacturing activity over the 2000-2017 period was driven by four major factors: increased skill intensity, defined by greater relative demand for workers in non-production activities; greater capital intensity; increased usage of industrial robots (Acemoglu and Restrepo, 2020); and, perhaps most significantly, the dramatic increase in global import competition.[4] We include a rich set of controls in all specifications to ensure that our estimates are not confounded by these forces. However, the sector-wide risk of job loss faced by manufacturing workers, particularly post-2001, suggests an important potential role for job mobility to mediate the effects of bonus depreciation on worker outcomes during our sample period. Our results suggest that bonus depreciation played an important role in buffering exposed workers from the worst impacts of sectoral transformation by reducing their probability of leaving the workforce, and by reducing the costs of job loss for those that separated from their 2001 firm.

Lastly, our study makes novel contributions to wide-ranging literature on labor market policies that address the consequences of economic shocks to workers and firms. The significant earnings declines associated with job loss have been well-established in the economics literature since Jacobson et al. (1993). A particular point of interest are recent debates of competing approaches to aiding workers during the Covid-19 pandemic-induced recession. Giupponi et al. (2022) frame this debate as a contrast between insuring jobs, i.e. by providing subsidies to firms to prevent layoffs, or insuring workers, i.e. by increasing the generosity of unemployment insurance. In ongoing work, we plan to benchmark the fiscal costs of bonus against existing labor market policies to provide a like-for-like comparison suitable for policy evaluation.

1 Background

The Job Creation and Worker Assistance Act of 2002 created the first modern version of the incentive for businesses investment in physical capital that is commonly referred to as "bonus depreciation." Bonus is a form of accelerated depreciation that initially allowed firms to deduct 30% of their investment costs from their taxable income immediately and then follow normal depreciation schedules – usually the Modified Accelerated Cost Recovery System (MACRS) – for the remaining 70% of of the expenditure.

Bonus depreciation is a politically popular subsidy because it only changes the timing of tax deductions for businesses instead of changing the size of the deduction or by applying direct

[4] Autor et al. (2014b) and Pierce et al. (2024) find that import competition led to significant, persistent declines in earnings for exposed manufacturing workers.

subsidies, so the cost of the policy appears very small over long time periods that don't account for the time value of money such as the Congressional Budget Office's 10-year forecasting window. However, bonus has real costs as a tax expenditure and real value as a subsidy because of the relative change in timing. Bonus is economically equivalent to giving a firm that purchases a qualified asset an interest free loan equal to the bonus portion, 30% originally, multiplied by the tax rate and the value of the asset. The business then pays back that loan by not taking the normal deductions they would have taken under MACRS in following years.

The original round of 30% bonus depreciation initiated in 2002 was imposed retroactively to September 11, 2001 and was intended to be temporary. The policy was increased to 50% in mid 2003 and was phased out on January 1, 2005. In response to the 2008 financial crisis, bonus was implemented again at 50% and has continued with temporary extensions until the Tax Cuts and Jobs Act of 2017, which increased the policy to 100% bonus depreciation, or full expensing. Panel A of Figure 1 shows the time variation in the bonus depreciation policy at the federal level. Calculations from Zwick and Mahon (2017) using IRS data taken with the average level of bonus depreciation of 37% from 2001 to 2011 implies an average decrease in the cost of capital of 2.13% during this time period.

A canonical example of the bonus policy in action is a firm that purchases a computer for \$1,000 that they would the depreciate across 5 years with a discount rate of 7% and a tax rate of 35%.[5] With straight-line depreciation, the firm will deduct \$200 each year from their taxable income, that will lower their tax liability by $\$200 \times 0.35 = \$70$. Under 30% Bonus depreciation, the firm would instead use a bonus depreciation deduction in the first year of \$440 ($\$300 + (\$700 \times 0.2)$) and would only be able to deduct \$140 in years 2 through 5. In both cases the firm deducts the full value of the asset across 5 years to lower their total tax liability by \$350 ignoring the time value of money. With a discount rate of 7%, without bonus the depreciation deductions are only worth \$307.10 in net present value while the same deductions for the same asset purchase are worth \$319.97 in NPV terms, 4.2% more than the baseline, with an allowance for 30% bonus depreciation.[6]

Most physical assets are depreciated according to a modified schedule that already partially front-loads some depreciation deductions instead of straight-line depreciation.[7] Panels B and C of Figure 1 show the changes in depreciation schedules for a Tractor unit, a 3-year asset, and a barge, a 10-year asset, from Table A-1 from IRS Publication 946 (2019) when they gain access to 50% bonus. Both assets benefit from Bonus, but the longer lived asset benefits much more in the

[5]For the sake of this example, we ignore the half-year convention and assume deduction happens proportionally in all 5 years including the first year.

[6]In an environment with declining corporate tax rates, the savings in NPV terms for Bonus depreciation are even larger in *ex post* terms because the time periods in which firms are able to deduct relatively less of their investment have lower tax rates.

[7]The "Alternative Depreciation System" (ADS) is the primary set of rules by which assets depreciate in a straight line fashion, but most of those assets don't qualify for Bonus.

ability to shift depreciation deductions into the purchase period. Bonus depreciation provides a larger subsidy for capital purchases where the NPV of depreciation deductions are smaller under normal depreciation rules while it has no effect on investments that are fully depreciated in a single year.

Zwick and Mahon (2017) note that there is scope for other frictions to accentuate the effects of bonus depreciation as an investment incentive. In particular, they show that firms who may be more cash constrained or who lack access to perfect capital markets can take advantage of the liquidity provided by the bonus deductions in year 1. In their estimates, the smallest decile of firms in their data have an investment semi-elasticity that is twice as large as the elasticity of the largest decile of firms firms. The cash flow characteristic of Bonus depreciation way is an important technical way in which it helps firms.[8]

1.1 Bonus Depreciation and 21st Century U.S. Manufacturing

The 2002 implementation of bonus depreciation came at a time of significant transformation in U.S. manufacturing. While bonus affects investment throughout the economy, bonus had a significant impact on exposed plants and industries in manufacturing. Figure 2 demonstrates the aggregate trends in employment and capital stock implied by the estimated plant-level effects of bonus from Curtis et al. (2023). The forces of sectoral transformation outlined in Charles et al. (2019) contributed both to a significant drop in aggregate employment and a relative shift toward greater capital intensity, as implied by the relatively static supply of capital in the economy over the same period. In the face of these forces, bonus generated a relatively smaller decline in total employment for treated industries and an increase in capital stock (relative to a slight decline in less exposed industries).

To provide further context on the effects of bonus on employment in light of these patterns, Figure 3, also adapted from Curtis et al. (2023), decomposes the estimated 10% increase in NAICS 4-digit level employment associated with bonus in terms of different forms of job flows. By 2011, about equal shares of the employment effect was explained by a reduction in job destruction, either in the form of layoffs or plant closings, and due to new job creation. In the five years following policy implementation, however, reduced job destruction explained most of the overall employment effect. This provides suggestive evidence that job mobility in this window was a potentially meaningful channel for bonus to affect labor market outcomes for incumbent workers in more exposed industries.

The overall decline in employment over this time period had a significant impact on workers.

[8]Some firms do not claim bonus depreciation deductions when they are allowed Kitchen and Knittel (2016). Many of these firms are those with net operating losses so they don't have taxable income from which to deduct in the current year or those that otherwise may have unobservable information that their future tax rates will be higher than their current tax rates.

Autor et al. (2014a) examine a range of outcomes for workers as a function of import competition over the 1990 to 2007 period. They find that exposed workers experienced lower earnings growth and worse labor market attachment, with effects concentrated among low-wage workers. Pierce et al. (2024) similarly find substantial earnings declines that were mostly driven by transitions into other sectors or to worse-paying manufacturing jobs. These facts motivate our focus on bonus as potentially mitigating the negative effects associated with job destruction during our sample period.

2 Sources of Worker-Level Data

A complete accounting of the effects of tax-induced capital investment on workers requires longitudinal data on workers before and after policy implementation. Panel data is especially important in this context because it allows us to study the mechanisms by which investment influences worker outcomes across firm transitions and over time.

A primary advantage of this paper over previous studies on the labor market effects of capital investment is access to the U.S. Census Bureau's LEHD files. This administrative dataset provides quarterly earnings and employment data, as well as basic demographic information such as age and sex, for the near-universe of the U.S. workforce in states that report earnings information to the LEHD.[9] The matched nature of the LEHD allows us to recover detailed information about the firms to which workers are attached. Critically, we also observe the 4-digit NAICS industry classification code to which the firm belongs, which allows us to classify firms by investment duration following Zwick and Mahon (2017). For more detailed discussions on the construction of the LEHD files, see Abowd et al. (2008) and McKinney and Vilhuber (2008).

We also supplement the LEHD with personal-level data from the 2000 Decennial Census. The long-form questionnaire, which was administered to approximately 1 in 5 households and asked additional questions, with occupation of the primary job and educational attainment being of primary interest for our study. While the Decennial Census data does not allow us to track worker occupation across jobs, the timing of the long-form responses, together with the tenure restrictions we impose on our analysis sample, allows to identify worker occupation at the time of policy with reasonable confidence.

2.1 LEHD Sample Construction

Our research design relies on categorizing U.S. workers by the duration status of their firm in 2001 when bonus depreciation was first enacted. As a result, we limit our sample to workers for whom the LEHD reports positive earnings in 2001 and that appear in one of the 21 states

[9] LEHD earnings coverage averages 96 % of U.S. private sector workers. Omitted categories include agricultural workers and some public sector jobs, including all federal employees.

in the LEHD that report complete yearly earnings records over the entire 1997 to 2011 period. When a worker receives earnings from more than one firm in any year throughout the sample, we aggregate their earnings across sources to obtain a single yearly earnings figure and assign them to the firm at which they received their highest earnings. We then use the 4-digit NAICS industry code of this firm in 2001 to classify workers by investment duration.

We also restrict our sample to ensure that the pool of workers we consider are appropriate for studying the long-run consequences of capital investment on worker outcomes. We only consider workers between the ages of 25 and 54 years old in 2001, and we exclude those that never make more than $15,000 in 2001 dollars in any year throughout the sample.

One important limitation of the state-level LEHD files is that they do not allow us to distinguish between workers that are unemployed or exit the labor force from those that move to a state that does not report earnings in the LEHD. We are able to address this limitation via the LEHD Earnings Indicators file, which reports whether a worker reported positive earnings in any LEHD state, regardless of our access to the full earnings data for that state. For workers that leave our full sample but report positive earnings in another state, we impute earnings with a linear regression model that predicts earnings changes associated with moves across states within our sample period with worker demographics and prior industry of work as controls.

Given these restrictions, our analysis is based on two separate samples that help us isolate margins of labor market adjustment. The first ("full") sample contains yearly observations for all 428,000 workers who meet the above criteria, regardless of whether they ever report nonpositive earnings in the 2001-2011 period. Worker earnings are set to 0 for these observations, yielding a balanced panel of 6,845,000 observations over the 1996 to 2011 period. The second sample only contains workers who report positive earnings for every sample year, yielding 4,956,000 observations of about 310,000 workers (the "highly attached. The first sample is used to primarily to examine worker attachment to the labor market, while the second sample allows us to estimate earnings effects for sample of workers that maintain a relatively strong attachment to the labor market. Given these restrictions, our analysis is based on two separate samples that help us isolate margins of labor market adjustment. The first ("full") sample contains yearly observations for all 428,000 workers who meet the above criteria, regardless of whether they ever report nonpositive earnings in the 2001-2011 period. Worker earnings are set to 0 for these observations, yielding a balanced panel of 6,845,000 observations over the 1996 to 2011 period. The second sample only contains workers who report positive earnings for every sample year, yielding 4,956,000 observations of about 310,000 workers (the "highly attached" sample). The first sample is used to primarily to examine worker attachment to the labor market, while the second sample allows us to estimate earnings effects for sample of workers that maintain a relatively strong attachment to the labor market.

3 Research Design and Results

Our core event study regression design estimates the effect of bonus on worker-level outcomes using the following model:

$$\log Y_{ijt} = \alpha + \sum_{y=1996}^{2011} \beta_y \Big[\mathbb{I}[d = Long] * \mathbb{I}[y = t] \Big] + \gamma_{\mathbf{i}} + \mu_{\mathbf{jt}} + \mathbf{X_{it}} + \epsilon_{cdt}, \tag{1}$$

where i indexes workers, j indexes worker industry in 2001, and t indexes years. The fixed effect term $\mu_{\mathbf{jt}}$ includes industry-level controls for salient forces of sectoral transformation. This includes (1) measures of trade exposure from Pierce and Schott (2016); (2) measures of industry-level capital intensity, defined as capital stock per worker; (3) measures of skill intensity, defined as nonproduction employment over total employment; and (4) measures of robot adoption from Acemoglu and Restrepo (2020). We operationalize these controls by fixing values in 2001, taking deciles of the resulting distribution in our sample, and interacting decile indicators for items (1)–(3) with year fixed effects, and the continuous robot exposure measure with year fixed effects. These fine-grained, flexible controls allow us to compare workers with very similar exposure to otherwise confounding forces in U.S. manufacturing.

In many specifications, we also include controls for worker demographics in $\mathbf{X_{it}}$, namely years of tenure at their 2001 firm, and years of education, both of which are interacted with year fixed effects. In our heterogeneity analysis, we also control for other demographics when applicable (such as age, gender, and race).

Our treatment definition, $\mathbb{I}[d = Long]$ is a binary indicator for whether a worker is in a relatively more exposed (long duration) industry. We follow Curtis et al. (2023) and define long duration industries as those in the bottom 33% of the distribution of NPV of depreciation deductions across industries, as determined by the IRS.

3.1 The Effects of Bonus Depreciation on Worker Earnings

Figures 4 and 5 present estimates of the effects of bonus depreciation on worker earnings in our two estimating samples. For our "full" sample that includes zero earnings observations when workers leave the labor force, we estimate our event study model on earnings levels using a Poisson Pseudo-Maximum Likelihood Estimator. This allows us to interpret coefficients as percent changes when the outcome variable takes on zeros.

In both samples, we estimate positive and significant effects on worker earnings for the years 2003 to 2011, with the most precise estimates in the years 2004 to 2007. Pre-trend terms are all estimated to be significantly indistinguishable from zero, supporting the common trends assumption underlying our event study design. Tables 1 shows that the accompanying difference-in-differences terms are significant and positive across additional sets of controls, including those

that omit demographic controls, those that control for the initial earnings level of workers, controls for 2001 firm size, and controls for labor market size.

3.2 The Effects of Bonus on Worker Employment and Mobility

We now turn to the extensive margin effects of bonus for workers. As figure 3 shows, Bonus significantly reduced job destruction among treated firms. Figure 6 shows that bonus depreciation also leads to a significant reduction in the probability that workers who were employed at exposed firms in 2001 will report zero earnings in the years 2002 to 2011. By 2011, we find that exposed workers were 2.3 percentage points less likely to have exited the labor force. Beyond effects on total earnings or labor force participation, we also find that bonus reduces the probability that workers undertake other costly labor market adjustments in the form of cross-state moves. Figure 7 shows that bonus reduced the probability that exposed workers migrated to a different U.S. state after 2003, topping out at a 1.9 percentage point effect by 2011.

3.2.1 Worker Earnings and Job Mobility

Beyond these extensive margin adjustments, bonus could also affect worker earnings via changes to job mobility patterns. That is, workers that maintain a relatively stable labor market attachment may still be impacted, on average, by changes to job mobility, either by preserving existing jobs or facilitating better matches in other jobs.

We explore these possibilities in our "highly attached" worker sample by first estimating the effects of bonus on workers' cumulative probability of experience different kinds of job separations. Figure 8 summarizes these results in event study form, where the outcome is the cumulative probability of the specified move type in a given year relative to 2001.[10] Despite our full sample revealing significant and persistent effects on workers' labor market attachment, Panel A shows that we find no significant effect on workers' probability of moving jobs. In 2002, we estimate a positive but noisy effect on the probability of a move (at the 15% level), but statistical significance disappears by 2005. We next split job moves into job-to-job moves, defined as firm transitions that do not involve a quarter of zero earnings, or job separations (those that do include at least one quarter of zero earnings). Panel C shows that bonus significantly reduced the probability of job loss by 2005 and through 2011 (except for 2008). While this result aligns with our prior extensive margin effects, Panel B shows some evidence that bonus did increase job-to-job moves (effects are significant at the 10% level in 2002 and again in 2010–2011). We discuss these results further in section 3.5.

We explore how these mobility patterns affect worker earnings by re-estimating our earnings regression while interacting our event study coefficients with indicators for whether workers

[10] Due to US Census restrictions, these results estimate "qualitative" estimates of the sign and significance level of event study coefficients.

separated from their 2001 firm by 2005. Because this is an endogenous outcome, we rely on several different comparisons to unpack the relationship between job mobility and our main earnings effects. In this sense, we regard these results as descriptive facts about our main causal effects rather than causal statements about earnings effects for different subsets of workers.

Our first model fully interacts the event study coefficients in Equation 1 with an indicator for whether workers change firms at any time over the 2001 to 2005 period. By also including job mobility-by-year fixed effects, this model yields "stayer vs. stayer" and "mover vs. mover" comparisons to recover the effects of bonus on worker earnings. Figure 9 presents these comparisons. The blue line indicates that treated workers that remain at their firm experience very similar earnings growth to the full sample comparison in 4. In 2005, the "stayers" comparison yields a 4.1% earnings effect vs. a 4.7% effect in the analogous full-sample specification. This result suggests that the effect of bonus on worker earnings is not driven fully by changes to worker mobility patterns. Instead, these relative earnings increases for incumbent workers are consistent with exposed firms passing through increased rents derived from greater investment to their workers (Kline et al., 2019).

Exposed workers who move also fare better than less exposed workers that move, as shown in the orange line. While these results themselves are silent on whether job mobility reflects layoffs or voluntary separations, this result aligns with the differential mobility patterns we find in Figure 8. Note that null effects for both of these comparisons would still be consistent with the positive earnings effects estimated in Figure 4. For example, this could be the case if (1) all else equal, movers have lower earnings in the post period and (2) bonus reduced the probability of moves. The fact that we find positive (though noisy) effects of bonus on move probability thus provides strong evidence that both rent-sharing and improved job match quality contribute to our headline earnings result.

We supplement these comparisons by also estimating the differential effects of bonus while omitting job mobility indicator-by-year fixed effects. This model yields a "stayers vs. all" and "movers vs. all" comparison, as shown in Figure 10. These comparisons are valuable because they keep the control group from our baseline estimates constant and thus provide a more straightforward decomposition of our main earnings effect. Formally, our estimated earnings effects from Table 1 can be expressed as:

$$
\begin{aligned}
\beta_{bonus} \equiv & \mathbb{E}[Earn|Bonus=1] - \mathbb{E}[Earn|Bonus=0] \\
= & \Pr(JobMove_{01=05}=1|Bonus=1) \times \mathbb{E}[Earn|Bonus=1, JobMove_{01=05}=1] \\
& + \Pr(JobMove_{01=05}=0|Bonus=1) \times \mathbb{E}[Earn|Bonus=1, JobMove_{01=05}=0] \\
& - \mathbb{E}[Earn|Bonus=0].
\end{aligned}
$$

Adding and subtracting $\Pr(JobMove_{01=05} = 1|Bonus = 1) \times \mathbb{E}[Earn|Bonus = 0]$ from this expression and rearranging then yields the result that the overall earnings effect of bonus is a weighted average of these comparisons:

$$\begin{aligned}\beta_{bonus} =& \Pr(JobMove_{01=05} = 1|Bonus = 1) \times \beta_{bonus}^{\text{move vs. all}} \\ &+ \Pr(JobMove_{01=05} = 0|Bonus = 1) \times \beta_{bonus}^{\text{stay vs. all}}.\end{aligned}$$

Using the decomposition and our estimated effects, we can establish that $\Pr(JobMove_{01=05} = 1|Bonus = 1) = 0.35$ in our sample. This decomposition thus shows that the earnings effects of bonus can either manifest by altering the relative earnings effect associated with job moves relative to staying ($\beta_{bonus}^{\text{move vs. all}}$ vs. $\beta_{bonus}^{\text{stay vs. all}}$) or the probability of moves.

The blue line in Figure 10 shows, unsurprisingly, that exposed "stayers" earned significantly more than the average control worker. More interestingly, exposed "movers" still fared significantly worse than the average control worker over the 2001 to 2006 period, but this disadvantage entirely disappeared by 2006. This result provides some suggestive evidence that bonus lead to better long-term job matches for this subset of treated workers despite the initial decline in wages around the initial job change.[11] As established before, the fact that we find that bonus has no significant effect on the probability of job moves suggests that these effects primarily reflect either rent-sharing to "stayers" or improvements in the quality of job moves that workers experience. The latter mechanism is also supported by our results showing an increase in "good" moves and a decrease in "bad" moves for exposed workers.

3.3 Policy Incidence and Earnings Pass-through

To better understand the importance of the rent-sharing channel, we now turn to a quantifying our estimated earnings effects relative to the benefits of bonus depreciation to firms. Traditionally, studies of the effects of corporate tax policy on workers typically rely on wage elasticities with respect to changes in the net-of-tax rate for firms (e.g. Kennedy et al., 2024), which serves as a common benchmark for comparing different estimates. However, because the benefits of bonus come primary through lowering the user cost of capital, which may influence worker earnings through several potential channels (e.g. increases in worker productivity), it is less clear which benchmark should be used to quantify our effects.

We instead turn to a broad literature in labor economics that seeks to estimate the degree to which firms pass through rents to worker earnings. To quantify how bonus depreciation increases firm rents, we rely on public, industry-level manufacturing data and estimate the effects of policy exposure on value added, defined as total revenue net of materials costs. Figure 11 presents these

[11] In ongoing work, we will use the quantitative estimates associated with Figure 8 to formally decompose the earnings effects into mobility, job quality, and rent-sharing channels.

estimates for both total log value added and log value added per worker.[12]

Following Kline et al. (2019), we consider a simple model in which the wages of incumbent workers, w^I, are a function of market-level wages w^M and firm-level quasi-rents per worker, S, which firms pass-through to incumbent workers according to the pass-through parameter π. While various models of imperfect labor market competition can produce $\pi < 1$, this simple model generates less than perfect pass-through by assuming that firms face training costs associated with new hires, which drives a wedge between market (i.e. new hire) wages and realized wages for incumbent workers.

Formally, this yields the following expression:

$$w^I = (1-\theta)w^M + \underbrace{\theta\left(1-\frac{1}{\epsilon}\right)}_{=\text{pass-through, } \pi} S,$$

where pass-through is a function of product demand elasticity ϵ and $\theta = \frac{\eta}{1+\eta}$, where η is the firm-specific labor supply elasticity. As this represents a retention elasticity with respect to wages, this expression shows that firms will engage in more rent-sharing with workers if they are relatively more likely to leave the firm given a wage decrease.

To calculate a pass-through elasticity, we focus on the 2005 event-study coefficient in our "stayers" comparison in Figure 9. This time period corresponds to the end of the initial wave of bonus depreciation, as depicted in Figure 1, and restricting attention to stayers allows us to consider direct, firm-level earnings effects rather than those partially reflecting job moves to other firms.

We pair this earnings effect, $\beta^w_{stayers} = 0.041$, with the corresponding 2005 effect on log value added per worker from Panel B, Figure 11, $\beta^s = 0.072$, though this effect is estimated with noise ($p = 0.11$). These estimates yield a pass-through elasticity, defined as the ratio $\varepsilon_\pi = \frac{\beta^w_{stayers}}{\beta^s}$, of 0.57. This estimate is relatively large given existing estimates. Kline et al. (2019) report a value-added elasticity of 0.47 from patent-induced rents, though it is remarkably close to the estimate of 0.58 in Van Reenen (1996). This large pass-through estimate could be explained by the context of our study period: if US manufacturing firms were otherwise highly constrained in their ability to raise wages due to external import competition, bonus could "unlock" wage increases that firms would otherwise be unable to achieve. Relatedly, Zwick and Mahon (2017) find that the investment effects of bonus are particularly large for firms facing liquidity or credit constraints.

[12] In ongoing work, we directly estimate pass-through in worker-level earnings regressions that instrument firm-level log value-added per worker with our policy variation.

3.4 Heterogeneity Analysis

A substantial concern among policymakers is that capital investment may have disparate impacts across workers, particularly those that may be likely to be displaced by automation technologies (Acemoglu and Restrepo, 2020). As shown in Curtis et al. (2023), the 2001 wave of bonus stimulate the largest employment growth among production workers while also increasing the share of workers in historically disadvantaged groups, namely those without a college education, Black, Hispanic, and younger workers. However, these effects could mask displacement of incumbent workers these industry and plant-level responses were primarily driven by new hiring. In this section, we explore how the effects of bonus differed across both sectoral forces and worker types for our sample of workers that maintain a labor market attachment through 2011.

3.4.1 Sectoral Heterogeneity

We first explore heterogeneity across the four forces of sectoral transformation described in Charles et al. (2019), which also serve as control variables in all of our regressions as described in section 3. We conduct heterogeneity analysis by interacting our difference-in-differences coefficients with indicators for whether each of the sectoral measures is above the sample median. Table 2 presents these results. We find significant scope for heterogeneity according to both import exposure and robot exposure. Workers in industries with below-median import exposure only experience a noisy ($p < 0.15$) positive earnings effect, while those with above-median exposure experience significantly larger positive earnings.

Somewhat surprisingly, we find the opposite pattern among workers in industries with relatively higher robot exposure. We find similar patterns for more capital intensive industries, though the more muted effects for highly-exposed workers is noisier in this case. One potential explanation for these patterns is that bonus did more to preserve the earnings of workers facing the "external" negative shock of import competition. On the other hand, technological change as embodied in industrial robot adoption and increased capital intensity represent more structural changes, and the earnings-generating effects of bonus may have been tempered if increased capital investment led to greater displacement of incumbent workers.

3.4.2 Worker Heterogeneity

We next estimate heterogeneous earnings effects across several demographic categories, presented in Figures 12 and 13 and Table 3. The first key result is that our estimating earnings effects are significantly larger for workers above the age of 35. Figures 12 shows that the relative earnings gain for older workers begins in 2004 and grows through 2011.[13] This is consistent with older

[13]Event study coefficients are significantly different from one another at the 10% level in 2004, the 5% level in 2006, and the 1% level in 2005 and 2007-2011.

workers having greater difficulty finding good future job matches, and thus benefiting more from bonus reducing the probability of job loss.

Similarly, workers without a college education receive relatively more positive earnings increases from bonus, though this difference is not significant. Production workers also receive relatively larger, though small and somewhat noisy, earnings boosts of 0.8% relative to non-production workers. This piece of evidence also accords with the idea that the types of workers for whom secular forces reduce labor demand, in this case due to automation technology penetration during our time period, could benefit more from holding on to existing jobs.[14] We find that black workers experienced larger earnings gains from bonus exposure, though these effects dissipate after 2008.

We also find interesting heterogeneity patterns for worker non-employment and state migration. Table 4 shows that the negative effects of bonus on the probability of exiting the labor force are largest for production workers, those without a college degree, men, and white workers. Similar patterns emerge for the reductions in the probability that workers migrate to a new state, except we do not estimate a significant difference for black workers, as shown in table 5. Continued work on this project will delve further into these patterns to understand how they relate to labor supply and demand for these workers. However, the fact that heterogeneity patterns for extensive margin adjustments do not perfectly map to those estimated for earnings highlights the point that bonus may have distinct impacts on workers through both channels.

3.5 Local Labor Market Exposure to Bonus

In the final part of our empirical analysis, we provide preliminary evidence that, in addition to the effects associated with firm-level exposure to bonus, workers also benefited from working in labor markets with greater exposure to bonus depreciation. We do so by generating commuting zone-level measures of the share of 2001 employment with greater exposure to Bonus. To isolate different potential channels through which bonus could influence worker earnings, we generate two different exposure measures. Our primary measure of interest uses observed worker flows across 4-digit NAICS industries to generate measures of the effective labor market facing workers in a given industry, following Arnold (2021). We then calculate the weighted share of employment in industries with greater bonus exposure, with these flow-based measures as weights. This measure thus captures how subsidies to potential employers may lead to greater earnings for workers, either via better outcomes in the event of job loss, or by improving workers' outside options and thus bargaining power with other employers. The second measure is the share of exposed employment in 3-digit NAICS industries, which captures more general local economic stimulus resulting from increased labor demand in a location.

[14] In ongoing work, we will test whether the attenuated earnings effects among industries with greater robot exposure holds up for these workers.

We operationalize this local labor market treatment by defining workers in the top 20% of LLM bonus exposure as "treated." Using this definition, Figure 14 shows that our flow-weighted exposure measure predicts significant earnings growth for workers in 2002 and again over the 2008-2011 period. One possible interpretation of these results is that the immediate effect in 2002 reflects the positive earnings effect on job-to-job moves in Panel B of Figure 8. This could be the case if LLM and firm-level treatment were highly correlated: that is, some of the positive effects of bonus in Figure 4 reflect workers at bonus firms reallocating to other treated firms. We also find in Panel D of Figure 8 that firm-level bonus exposure initially increased the likelihood of workers leaving the manufacturing sector, though by 2004, there was no significant difference in workers' likelihood of no longer working in manufacturing. In light of the our LLM effects, this could reflect bonus "re-timing" an inevitable exit from manufacturing in a way that lead to better job matches and higher earnings as a result.

While we regard these patterns as suggestive, they are consistent with the possibility that bonus increased worker earnings by better inducing reallocation across labor markets in the face of sectoral transformation, and that local labor market exposure to bonus helped facilitate these effects.

4 Conclusion

This paper provides evidence that tax policy that stimulates capital investment can significantly affect manufacturing worker earnings and employment outcomes. In situating our results, we pay particular attention to the time period we study, when manufacturing underwent dramatic transformation, including significant declines in aggregate employment. The positive earnings and labor force attachment effects we estimate can thus be understood as evidence that bonus provided a buffer against these forces for workers. This interpretation is supported by suggestive evidence on how our estimated earnings effects differ according to whether workers left their original job in the years following policy implementation.

In ongoing work, we will further explore the heterogeneity patterns we find by both worker demographics and forces of manufacturing sectoral transformation over our sample period. In particular, the fact that we find very different effects across different forces of transformation, specifically import competition and robot adoption, suggests that investment stimulus policies enacted in the face of external or "internal" shocks may have very different effects on workers depending on the nature of these shocks.

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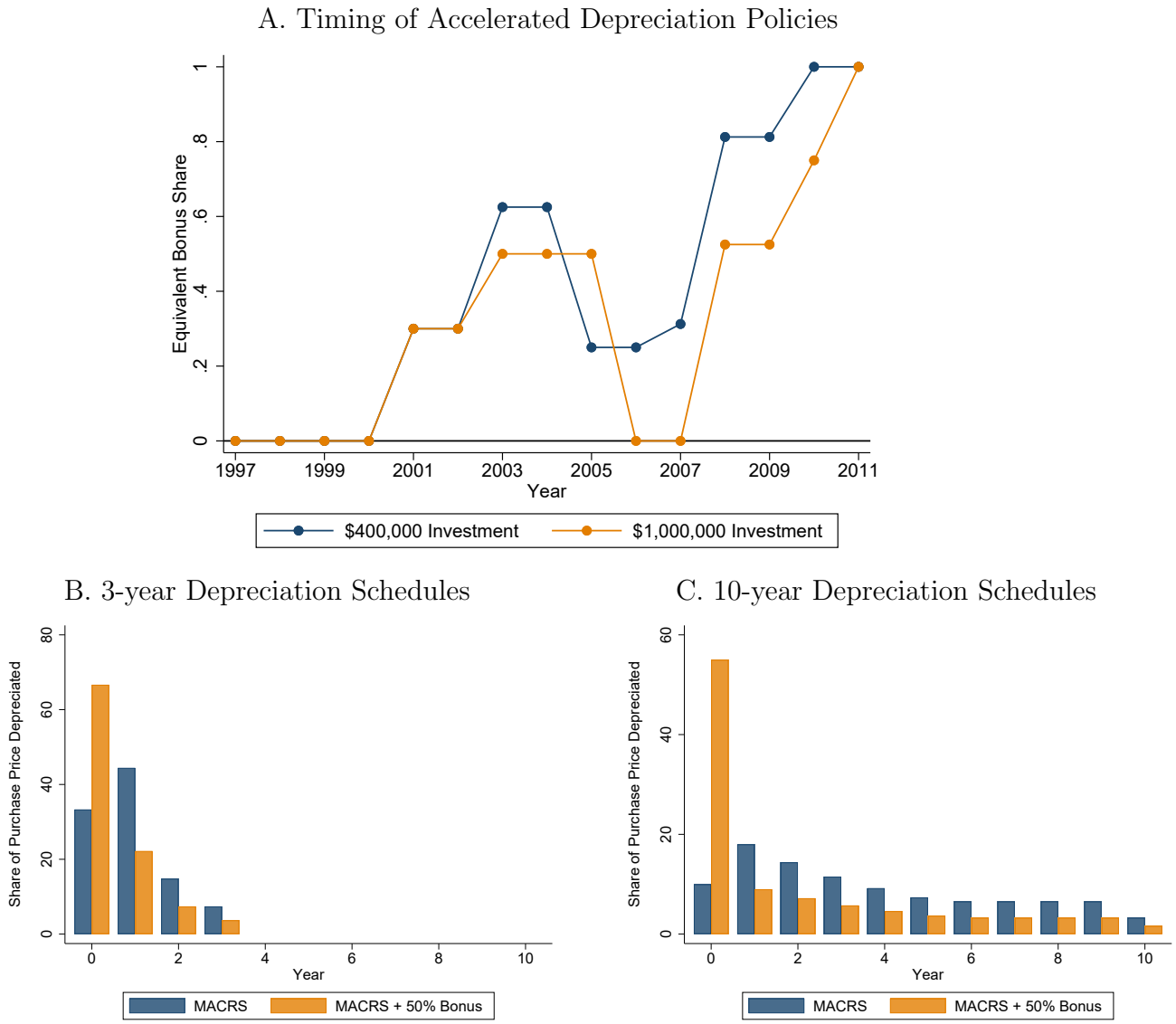
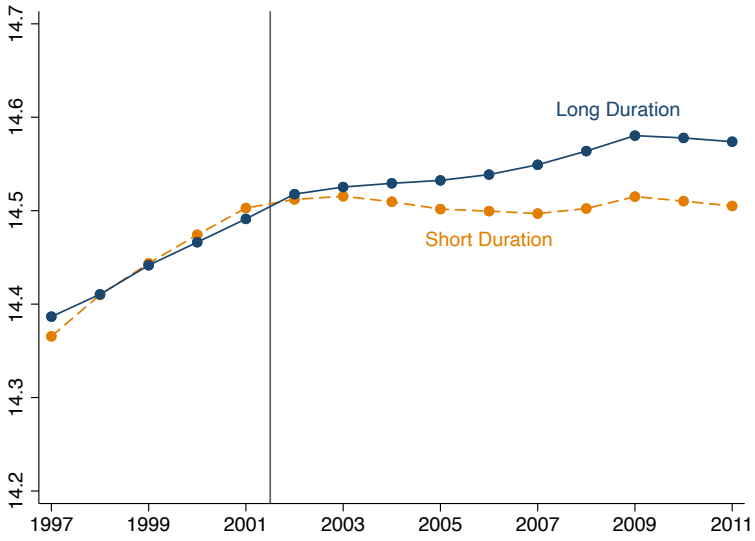
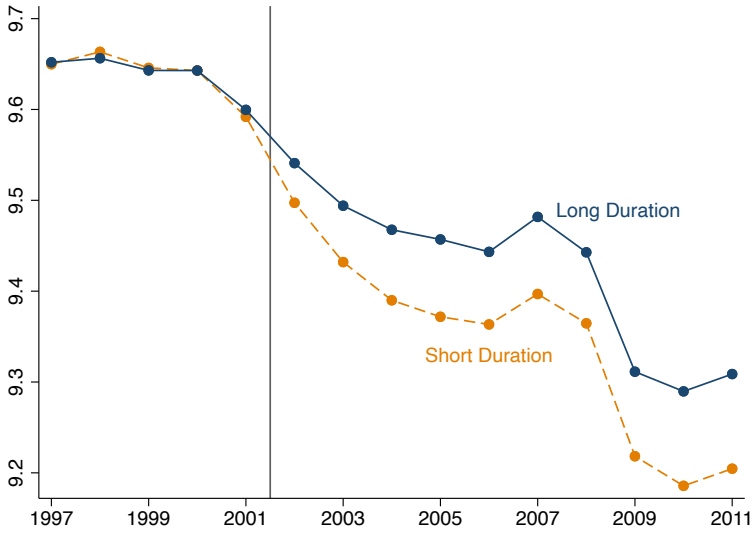


Figure 1: Bonus Depreciation Policy Description

Notes: Author's calculations using data from Internal Revenue Service Publication 946 (2019). Panel A shows the level of Bonus depreciation deductions allowed for qualified equipment at the end of each year from 2002 to 2012. Panels B and C show the relative depreciation schedules under MACRS with and without Bonus depreciation for two machines used in the transportation sector: tractor units and barges. Tractor units are used in ground transportation to pull tractor trailers. Barges are boats used for transportation across water. The later is subsidized much more under bonus depreciation than the former.



Panel A: Log Capital Stock



Panel B: Log Employment

Figure 2: Effects of Bonus Depreciation Relative to Aggregate Trends

Note: Figure 2 presents the effect of bonus depreciation on aggregate trends over log employment and log capital stock over the 1997–2011 implied by the reduced-form estimates of Curtis et al. (2023).
Source: Curtis et al. (2023), calculations based on NBER-CES Manufacturing Industry Database, ASM, CM, and Zwick and Mahon (2017) data.

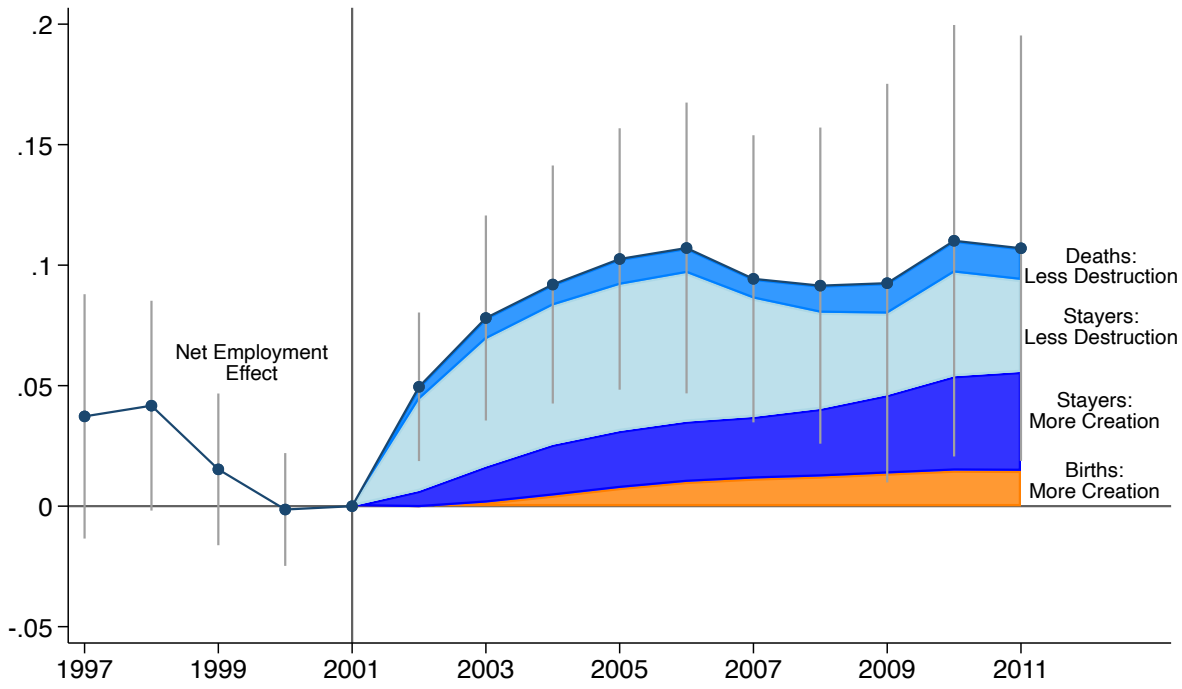


Figure 3: Effects of Bonus Depreciation Job Creation and Destruction

Note: Figure 3 describes the effect of bonus depreciation on job creation and destruction using Business Dynamic Statistics data from the US Census in the form of event study estimates and standard errors of the effect of bonus depreciation on net employment flows. The shaded regions illustrate the respective contributions of different employment flow components, which by definition aggregate to the net effect. We define the net employment change as jobs created minus jobs destroyed. Regressions include 4-digit NAICS and year fixed effects. 95% confidence intervals are based on standard errors clustered at the 4-digit NAICS level.

Source: Authors' calculations based on BDS and Zwick and Mahon (2017) data.

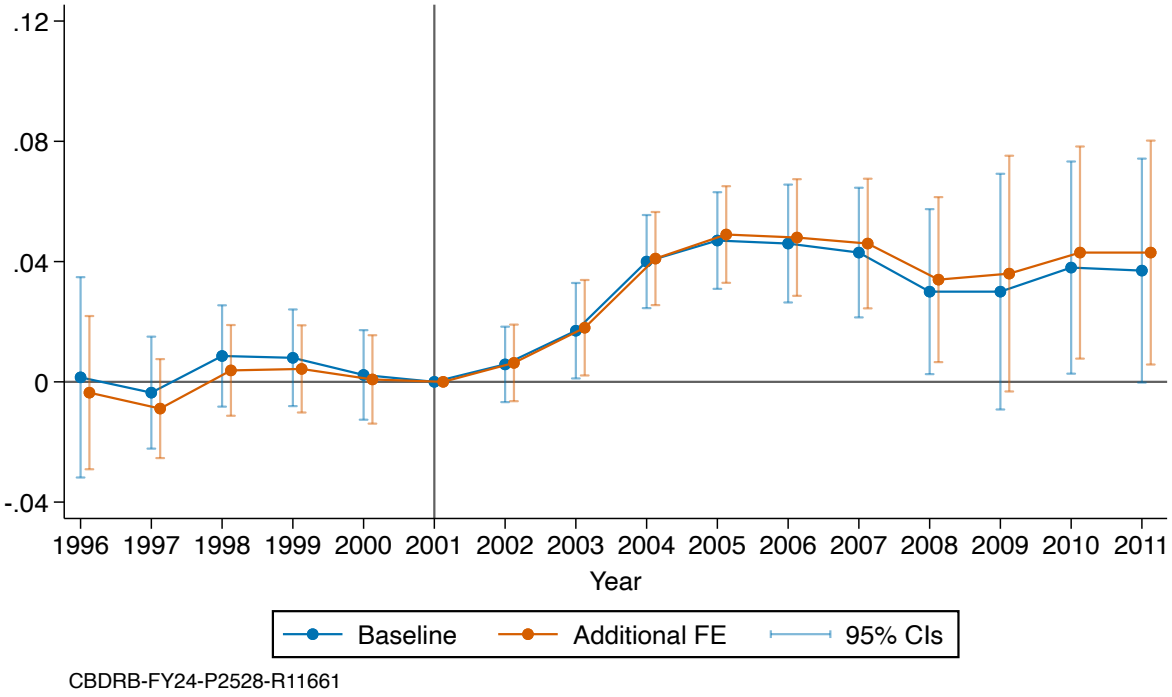


Figure 4: Effects of Bonus Depreciation on Worker Earnings

Note: Figure 4 describes the effect of bonus depreciation on log earnings of workers at exposed firms in 2001 with 95% confidence intervals. Standard errors clustered at the 4-digit NAICS level.
Source: Authors' calculations based on the LEHD, Decennial Census, and Zwick and Mahon (2017) data.

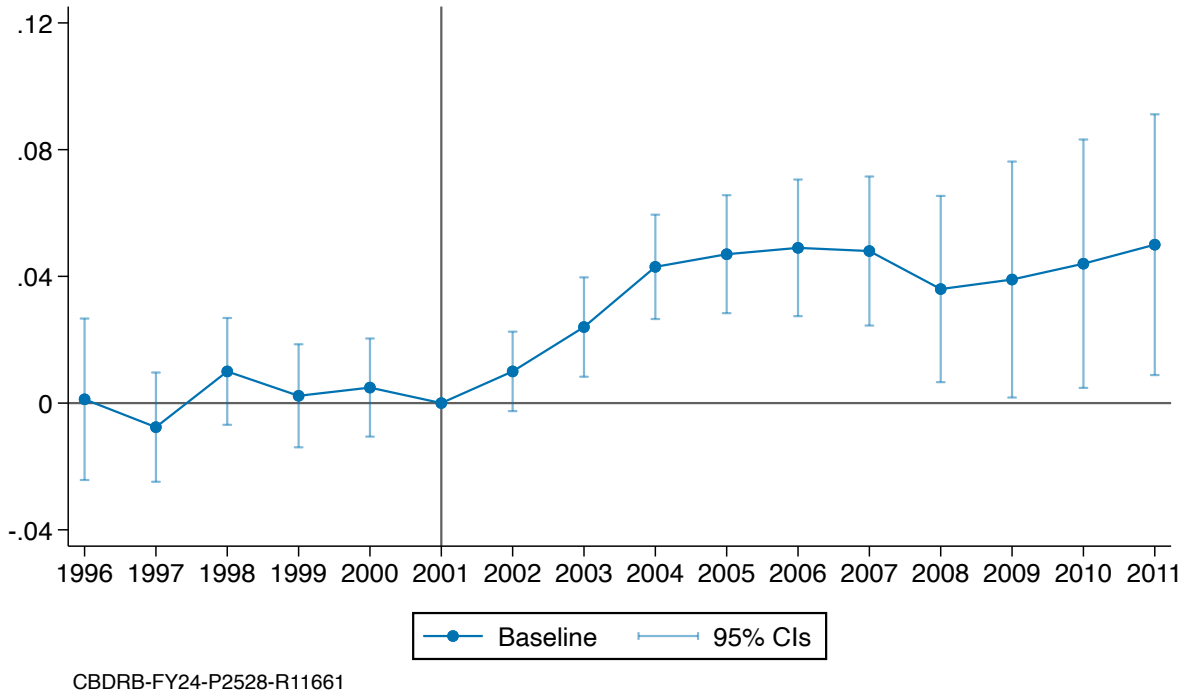


Figure 5: Effects of Bonus Depreciation on Worker Earnings

Note: Figure 5 describes the effect of bonus depreciation on earnings of workers at exposed firms in 2001. Estimates are derived from a Poisson Pseudo-Maximum Likelihood (PPML) model, such that coefficient estimates Error bars represent 95% confidence intervals from standard errors clustered at the 4-digit NAICS level.

Source: Authors' calculations based on the LEHD, Decennial Census, and Zwick and Mahon (2017) data.

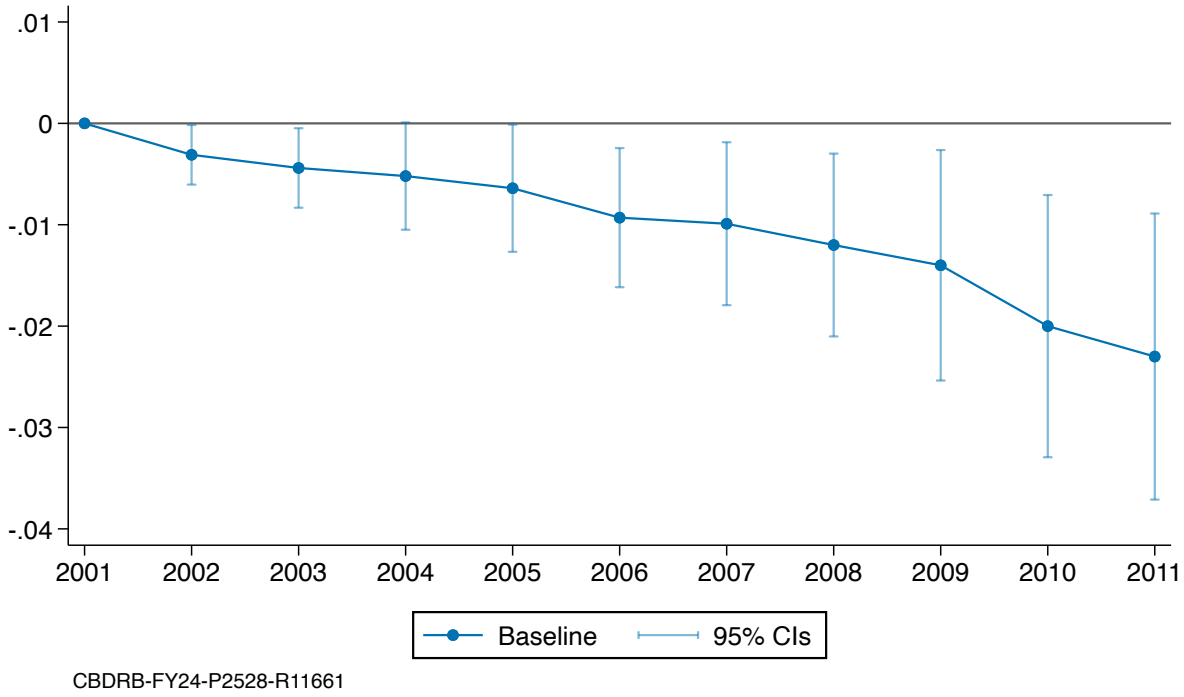


Figure 6: Effects of Bonus Depreciation on Employment Probability

Note: Figure 6 describes the effect of bonus depreciation on workers' probability of reporting zero earnings in a given year. Error bars represent 95% confidence intervals from standard errors clustered at the 4-digit NAICS level.

Source: Authors' calculations based on the LEHD, Decennial Census, and Zwick and Mahon (2017) data.

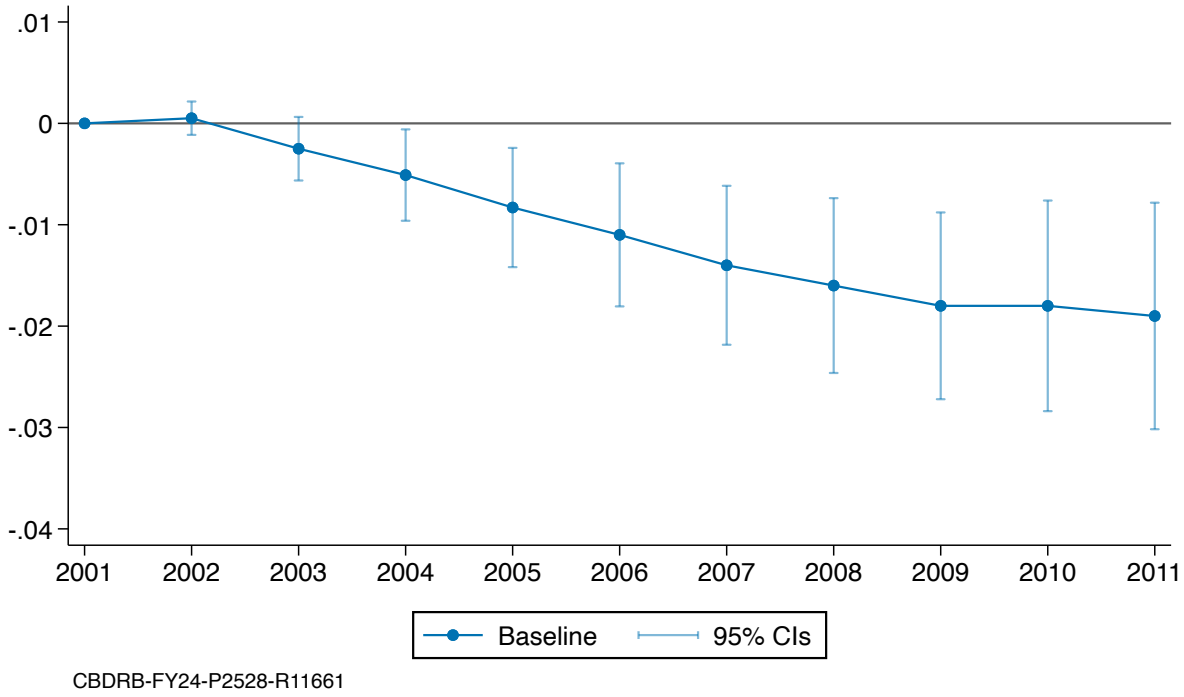
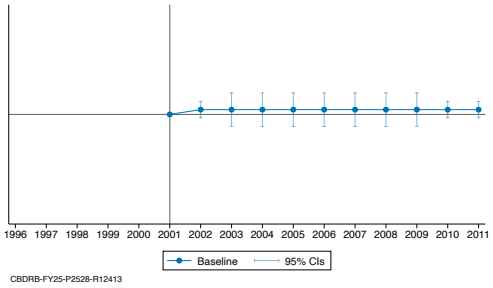


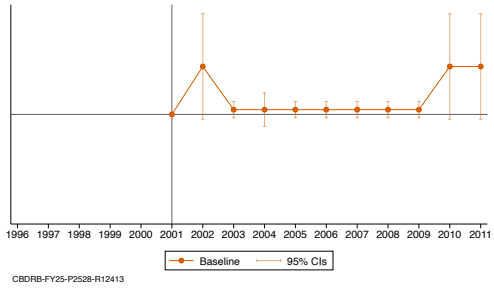
Figure 7: Effects of Bonus Depreciation on Probability of State Migration

Note: Figure 7 describes the effect of bonus depreciation on workers' probability of reporting earnings in a different state than that in 2001. Error bars represent 95% confidence intervals from standard errors clustered at the 4-digit NAICS level.

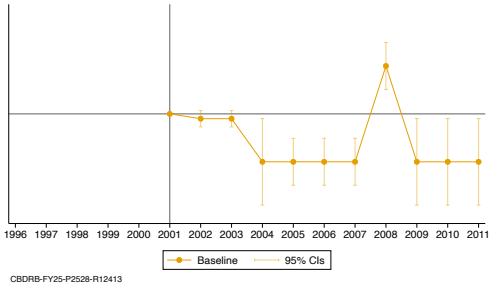
Source: Authors' calculations based on the LEHD, Decennial Census, and Zwick and Mahon (2017) data.



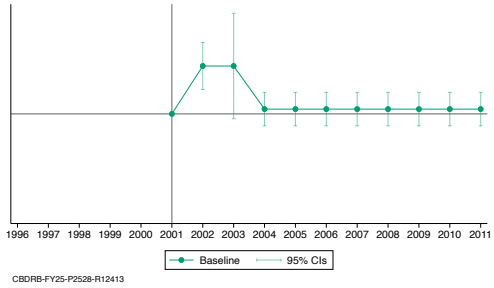
(A) Probability of Leaving Job



(B) Probability of Changing Job



(C) Probability of Losing Job



(D) Probability of Leaving Manufacturing

Figure 8: Effect of Bonus on the Probability of Worker Mobility

Note: Figure 8 describes the effect of bonus depreciation on the cumulative probability of a job move after 2001. Event study coefficient magnitudes that are statistically significant at the 10% level are normalized to 1, and statistically insignificant coefficients are normalized to 0.1. Error bars are scaled to represent 95% confidence intervals for a one unit change corresponding to significance at the 1%, 5%, or 10% levels. Standard errors clustered at the 4-digit NAICS level.

Source: Authors' calculations based on the LEHD, Decennial Census, and Zwick and Mahon (2017) data.

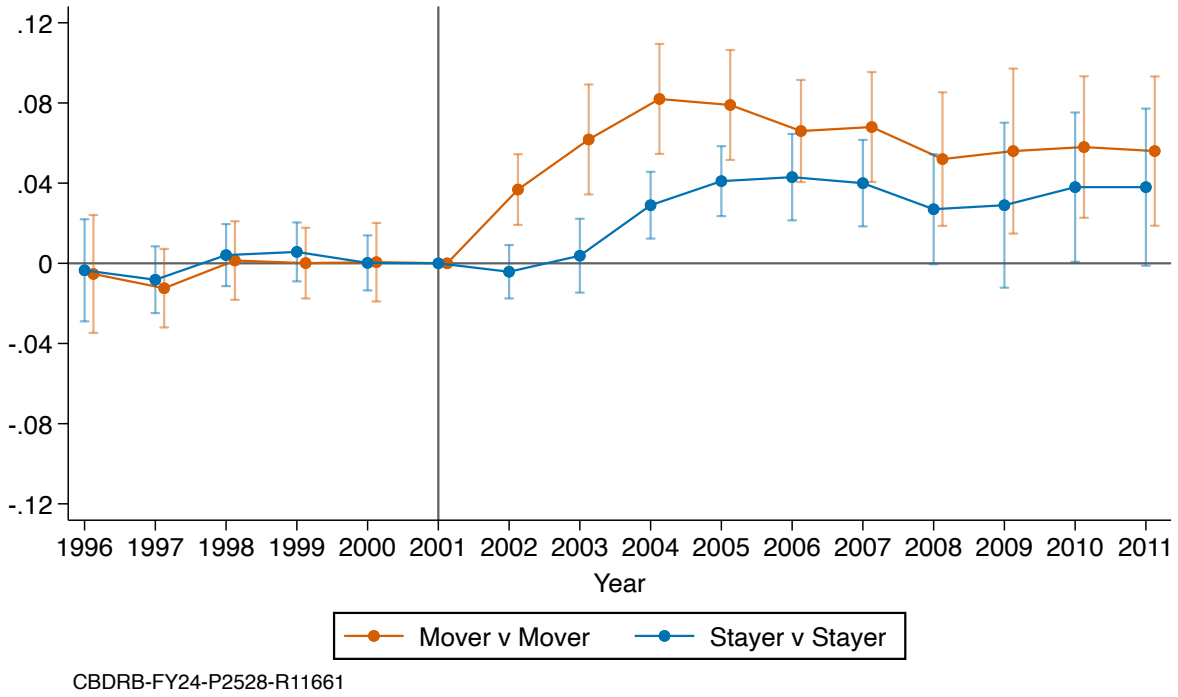


Figure 9: Effects of Bonus Depreciation on Log Earnings: Stayers and Movers Comparisons

Note: Figure 9 describes the effect of bonus depreciation on log earnings for worker depending on whether they separated from their 2001 firm by 2005. The blue line estimates the effect of bonus on worker earnings by comparing workers who do not change jobs by 2005 ("stayers v. stayers"). The orange line shows estimates derived from comparing workers who did experience a job separation over the same time period ("movers v. movers"). Error bars represent 95% confidence intervals from standard errors clustered at the 4-digit NAICS level.

Source: Authors' calculations based on the LEHD, Decennial Census, and Zwick and Mahon (2017) data.

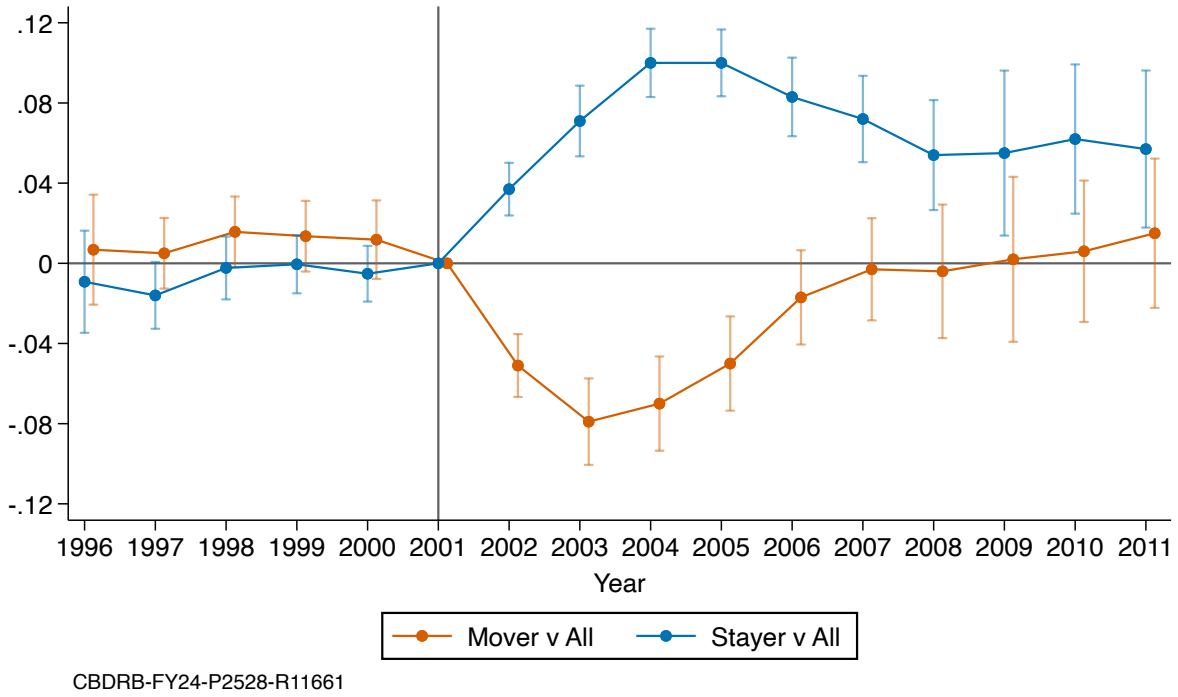


Figure 10: Effects of Bonus Depreciation on Log Earnings: Alternative Job Mobility Comparisons

Note: Figure 10 describes the effect of bonus depreciation on log earnings for worker depending on whether they separated from their 2001 firm by 2005. The blue line estimates the effect of bonus on worker earnings by comparing treated workers who do not change jobs by 2005 to all untreated workers ("stayers v. all"). The orange line shows estimates derived from comparing treated workers who did experience a job separation over the same time period to all control workers ("movers v. all"). Error bars represent 95% confidence intervals from standard errors clustered at the 4-digit NAICS level.

Source: Authors' calculations based on the LEHD, Decennial Census, and Zwick and Mahon (2017) data.

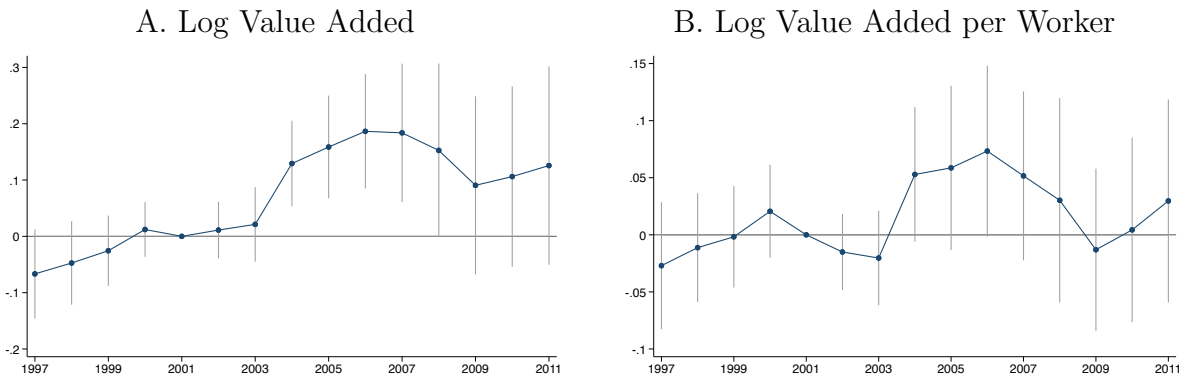


Figure 11: Effects of Bonus Depreciation on Industry-Level Value-Added

Note: Figure 11 describes the effect of bonus depreciation on value added in treated 6-digit NAICS industries. Panel A shows effects on total log value added, while Panel B shows effects on log value added per worker, which we use to benchmark our earnings effects against pass-through estimates in the literature. Each regression is weighted by the 2001 employment level of each 6-digit NAICS industry and includes industry and flexible controls for the salient drivers of sectoral transformation, as defined above. Error bars represent 95% confidence intervals from standard errors clustered at the 4-digit NAICS level.

Source: Authors' calculations based on the NBER-CES manufacturing dataset and Zwick and Mahon (2017) data.

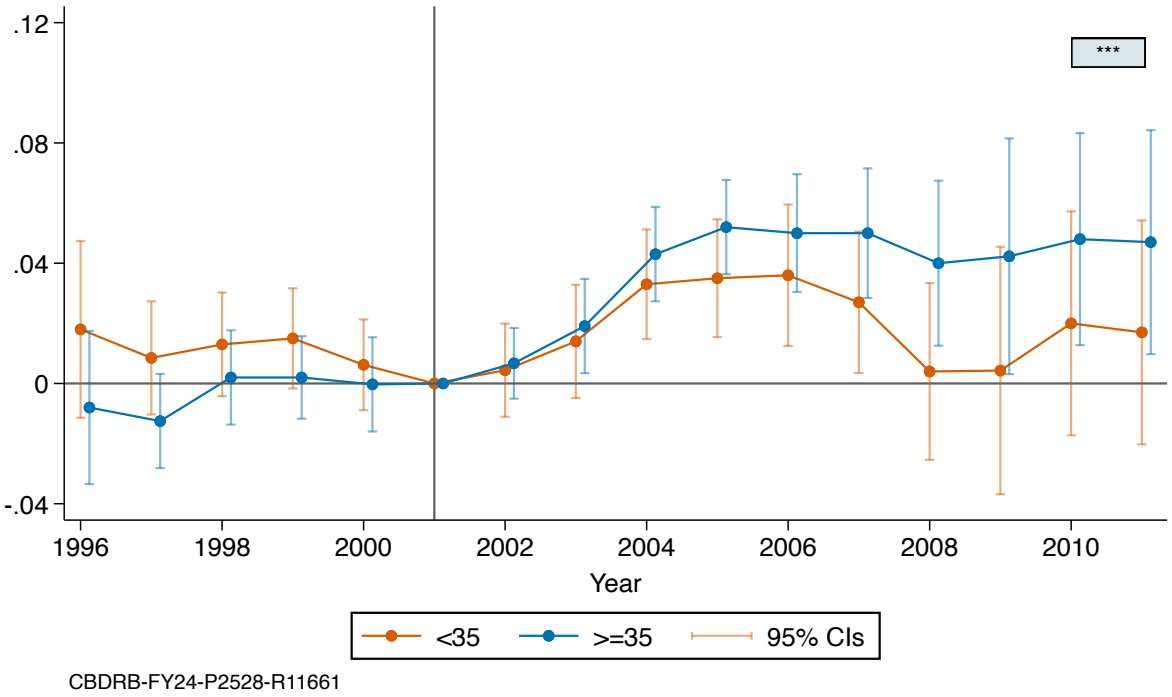


Figure 12: Effects of Bonus Depreciation on Log Earnings: Heterogeneity by Worker Age

Note: Figure 12 describes the effect of bonus depreciation on log earnings for relatively older or younger workers. The blue line estimates the effect of bonus on worker earnings for workers over the age of 35 in 2001. The orange line estimates the effect of bonus on worker earnings for workers below the age of 35 in 2001. Error bars represent 95% confidence intervals from standard errors clustered at the 4-digit NAICS level.

Source: Authors' calculations based on the LEHD, Decennial Census, and Zwick and Mahon (2017) data.

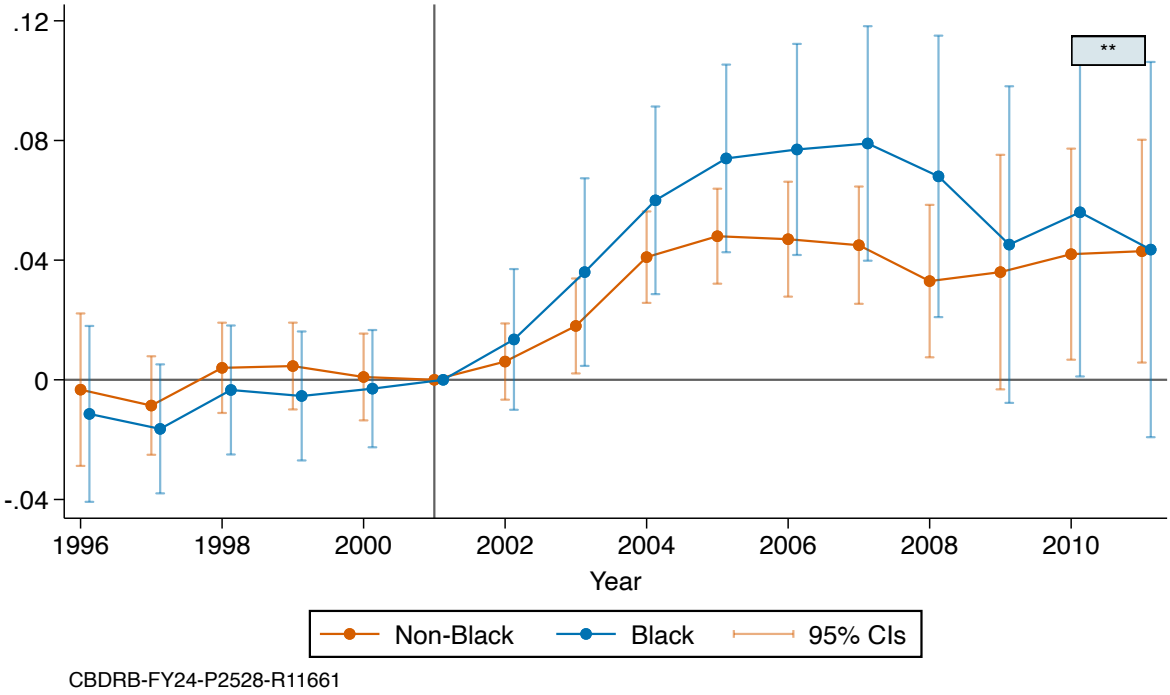


Figure 13: Effects of Bonus Depreciation on Log Earnings: Heterogeneity by Worker Race

Note: Figure 13 describes the effect of bonus depreciation on log earnings for black and white workers, respectively. Error bars represent 95% confidence intervals from standard errors clustered at the 4-digit NAICS level.

Source: Authors' calculations based on the LEHD, Decennial Census, and Zwick and Mahon (2017) data.

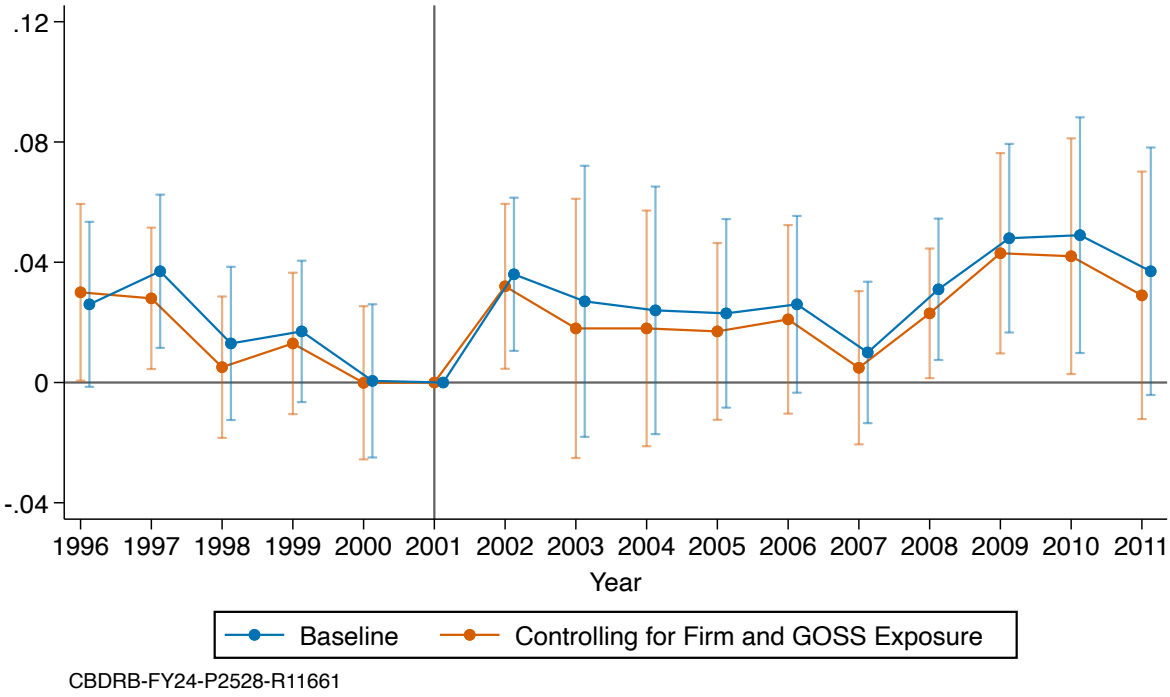


Figure 14: Effects of Bonus Depreciation on Log Earnings: Local Labor Market Exposure

Note: Figure 14 describes the effect of local labor market level exposure to bonus depreciation on log earnings. The treatment variable is a binary variable for whether a worker lies in the top 20% of 2001 local labor market employment exposure to bonus. We define labor markets using the flow-weighting measure of Arnold (2021) Each regression includes the firm-level bonus treatment indicators to isolate the effect of local treatment exposure. The orange line additionally includes the commuting zone-level bonus exposure measure of Garrett et al. (2020) interacted with year effects as controls. Error bars represent 95% confidence intervals from standard errors clustered at the 4-digit NAICS level.

Source: Authors' calculations based on the LEHD, Decennial Census, and Zwick and Mahon (2017) data.

Table 1: Effects of Bonus Depreciation on Log Earnings

Balanced Panel	(1)	(2)	(3)	(4)	(5)
Bonus	0.03	0.037	0.037	0.037	0.038
	(0.011)	(0.011)	(0.011)	(0.01)	(0.01)
	[0.006]	[0.001]	[0.001]	[0.000]	[0.000]
Observations	4,956,000	4,956,000	4,956,000	4,956,000	4,956,000
Balanced Panel with Zeros					
Bonus	0.025	0.036	0.039	0.038	0.038
	(0.011)	(0.011)	(0.012)	(0.011)	(0.011)
	[0.023]	[0.001]	[0.001]	[0.001]	[0.001]
Observations	6,845,000	6,845,000	6,845,000	6,845,000	6,845,000
Worker FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Sector Shock FE	✓	✓	✓	✓	✓
Tenure × Year FE		✓			
Schooling × Year FE		✓			
Earnings Decile × Year FE			✓		
Firm Size Decile × Year FE				✓	
CZ Population Decile × Year FE					✓

Note: Table 1 presents difference-in-differences estimates of the effects of bonus on log annual worker earnings. Panel A does so on a balanced sample of workers that never report 0 earnings in a year over 1996 to 2011. Panel B presents difference-in-differences estimates of the effects of bonus on annual worker earnings in a sample that imputes zeros for years in which a worker does not report positive earnings. Estimates are generated from a Poisson pseudo-maximum likelihood procedure such that coefficient estimates can be estimated as percent effects in the presence of zeros. Alternative controls specified in table are operationalized as interactions with year fixed effects. Standard errors are presented in parenthesis with p-values in brackets.
Source: Authors' calculations based on the LEHD, Decennial Census, and Zwick and Mahon (2017) data.

Table 2: Effects of Bonus on Log Earnings: Sectoral Heterogeneity

	(1) China Shock	(2) Skill Intensity	(3) Capital Intensity	(4) Robot Exposure
Bonus × Post	+†	+*	+***	+***
Bonus × Post × Shock	+***	+	−*	−***

†$p < 0.15$, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table 2 presents heterogeneous difference-in-differences estimates of the effects of bonus on annual worker earnings across the forces of sectoral transformation from Charles et al. (2019) specified in column titles. Each control is operationalized as above or below the sample median for (1) exposure to the import competition measure of Pierce and Schott (2016), (2) 2001 6-digit NAICS industry skill intensity, (3) 2001 6-digit NAICS industry capital intensity, and (4) the robot exposure measure of Acemoglu and Restrepo (2020). For each column, the first row denotes the for below median exposure while the second row denotes the differential effect of associated with above median exposure relative to the baseline in the first row. Stars represent significance at the 15% level (†), 10% level (*), 5% level (**), or 1% level (***).

Source: Authors' calculations based on the LEHD, Decennial Census, and Zwick and Mahon (2017) data.

Table 3: Effects of Bonus on Log Earnings: Worker Heterogeneity

Balanced Panel	(1) Female	(2) Production
Bonus	.037	.034
	(.011)	(.011)
	[0.001]	[0.002]
Bonus × Interaction	.00023	.0081
	(.006)	(.0045)
	[0.969]	[0.072]
Observations	4,956,000	4,956,000
Worker FE	✓	✓
Year FE	✓	✓
Sector Shock FE	✓	✓
Tenure × Year FE	✓	✓
Schooling × Year FE	✓	✓

Note: Table 3 presents heterogeneous difference-in-differences estimates of the effects of bonus on annual worker earnings across the demographic characteristics specified in column titles. The first row denotes the effect on noncollege educated, nonproduction, and male workers, respectively, while the second row denotes the differential effect of demographic groups relative to the baseline in the first row. Standard errors are presented in parenthesis with p-values in brackets.
Source: Authors' calculations based on the LEHD, Decennial Census, and Zwick and Mahon (2017) data.

Table 4: Effects of Bonus on Non-Employment: Worker Heterogeneity

	(1) Older	(2) College	(3) Female	(4) Production	(5) Black
Bonus	-.027	-.027	-.018	-.019	-.023
	(.0071)	(.0077)	(.0071)	(.0072)	(.0071)
	[0.000]	[0.000]	[0.011]	[0.008]	[0.001]
Bonus × Interaction	.0067	.011	-.015	-.01	.024
	(.0059)	(.0046)	(.005)	(.0035)	(.012)
	[0.256]	[0.017]	[0.003]	[0.004]	[0.046]
Observations	6,845,000	6,845,000	6,845,000	6,845,000	6,845,000
Worker FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Sector Shock FE	✓	✓	✓	✓	✓
Tenure × Year FE	✓	✓	✓	✓	✓
Schooling × Year FE	✓	✓	✓	✓	✓

Note: Table 4 presents heterogeneous long differences estimates of the effects of bonus on probability of reporting zero earnings across the demographic characteristics specified in column titles. The first row denotes the effect on noncollege educated, nonproduction, and male workers, respectively, while the second row denotes the differential effect of demographic groups relative to the baseline in the first row. Standard errors are presented in parenthesis with p-values in brackets.
Source: Authors' calculations based on the LEHD, Decennial Census, and Zwick and Mahon (2017) data.

Table 5: Effects of Bonus on State Migration: Worker Heterogeneity

	(1) Older	(2) College	(3) Female	(4) Production	(5) Black
Bonus	-.015	-.024	-.016	-.016	-.018
	(.0071)	(.006)	(.0057)	(.0056)	(.0057)
	[0.035]	[0.000]	[0.005]	[0.004]	[0.002]
Bonus × Interaction	-.0043	.012	-.0086	-.0078	-.015
	(.0042)	(.0034)	(.0039)	(.0031)	(.0092)
	[0.306]	[0.000]	[0.027]	[0.012]	[0.103]
Observations	6,845,000	6,845,000	6,845,000	6,845,000	6,845,000
Worker FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Sector Shock FE	✓	✓	✓	✓	✓
Tenure × Year FE	✓	✓	✓	✓	✓
Schooling × Year FE	✓	✓	✓	✓	✓

Note: Table 5 presents heterogeneous long differences estimates of the effects of bonus on probability of moving to a new state across the demographic characteristics specified in column titles. The first row denotes the effect on noncollege educated, nonproduction, and male workers, respectively, while the second row denotes the differential effect of demographic groups relative to the baseline in the first row. Standard errors are presented in parenthesis with p-values in brackets.
Source: Authors' calculations based on the LEHD, Decennial Census, and Zwick and Mahon (2017) data.