Estimating the Effects of Investment Incentives on Worker Earnings*

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Abstract

This paper studies the effects of tax incentives for capital investment on worker earnings and employment outcomes. Using matched employer–employee data from the U.S. Census Bureau and quasi-experimental variation in firms' exposure to bonus depreciation, I estimate difference-in-differences models that compare workers based on their initial firm-level exposure to the policy. My results indicate that bonus depreciation generated significant and persistent gains in incumbent worker earnings while also reducing unemployment spells and exit from employment. Earnings effects were strongest in industries exposed to import competition and attenuated in capital-intensive and robot-adopting industries. Additional evidence suggests significant passthrough of policy-induced productivity gains to the earnings of workers who remained at their firm. My findings highlight that investment tax incentives can yield broad benefits to workers, particularly amid structural transformation in manufacturing.

Keywords: bonus depreciation, wage determination, corporate taxation
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Introduction

This paper investigates the effects of tax incentives for investment on worker earnings and employment outcomes. Policymakers and economists have long questioned how investments in new equipment influence labor market outcomes. Proponents typically assert that, by stimulating investment, incentives encourage firm growth that benefits workers through more job opportunities and higher wages. Critical views toward these policies instead focus on how incentives can accelerate structural shifts toward labor-replacing technology, displace workers, and exacerbate rising wage inequality (Acemoglu and Restrepo, 2022).

Despite recent work that allays the worst fears about investment incentives destroying jobs, evidence on the effects of new investment on worker earnings or wages has proven much more elusive (Curtis et al., 2022; Hirvonen et al., 2025; Aghion et al., 2023b). This project addresses this void by systematically documenting the effects of one of the largest tax incentives for capital investment in U.S. history on worker-level outcomes. The federal tax policy I study, bonus depreciation (or "bonus"), was first implemented in 2001 and stimulates physical equipment investments by allowing firms to immediately deduct a proportion of depreciation deductions that are usually taken over several years. I combine quasi-experimental variation in policy generosity from IRS depreciation schedules with confidential, matched employer-employee microdata from the U.S. Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) program. In a difference-in-differences design that compares workers based on their firm-level exposure to the policy, I show that bonus stimulated significant and persistent positive earnings growth for workers at more exposed firms.

Matched employer-employee data helps address several key empirical challenges that have made it difficult to draw out clear evidence on the effects of investment incentives on worker outcomes. First, new investments are likely to involve substantial reallocation of workers and other economic resources as firms adapt their production processes to new machinery. This means that tracking workers as they transition to new firms, industries, or locations is likely important for characterizing the full effects of investment incentives. Second, firm or industry-level payroll data masks worker heterogeneity, which may confound aggregate estimates of the effects of investment incentives on worker earnings. This feature is especially likely to matter when incentives stimulate labor demand, making worker-level data essential for accounting for unobserved heterogeneity. Third, as both a practical and substantive challenge, investment incentives are often implemented as fiscal policy, either to address economic downturns or to facilitate or mitigate structural transformation. This feature necessitates a careful consideration of the ways that investment incentives interact with the economic and policy environment in which they are implemented, particularly as it relates to other worker-level shocks.

I conduct difference-in-differences analyses spanning the 1997 to 2007 period and compares earnings and employment outcomes of workers initially employed at firms investing in long duration assets to workers at firms that deduct equipment over a shorter time period. Industries that typically deduct new equipment purchases over a longer time period receive the largest benefit from bonus depreciation because the net present value of re-timing more distant deductions is higher (Zwick and Mahon, 2017). I then utilize the matched structure of the LEHD to characterize the effects of the policy inclusive of worker transitions across firms and out of the labor force.

I first find that bonus had a significant effect on worker earnings following policy implementation. Workers in long duration industries in 2001 had very similar earnings trends to those in other industries prior to the policy before diverging in 2002 and culminating in a 4.5 percent increase by 2005, when bonus expired. This effect is robust to my sampling criteria, which focuses on workers with relatively high labor force attachment, and a number of alternative specifications, including those that control for narrow location and occupation-specific trends.

I also find that bonus had significant positive effects on workers' attachment to the labor market. Workers in long duration industries were significantly more likely to be employed in the calendar years following policy implementation. By 2007, this 1.0 percentage point effect represents up to a 10.1 percent increase in workers' probability of reporting positive earnings. Among workers who retain positive earnings in all calendar years, bonus also reduces by 25 percent the probability that workers face an unemployment spell that includes at least a quarter of missing earnings. In light of significant job destruction in U.S. manufacturing during this time period, these results show that bonus played an important role in preserving jobs for workers at treated firms.

My 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 my sample period. Heterogeneity analysis by sectoral characteristics reveals important interactions with bonus depreciation. I find significantly larger positive earnings effects among workers in industries facing higher import competition, pointing to an especially valuable role for bonus depreciation as a bulwark against demand shocks that lead to significant labor displacement in U.S. manufacturing (Acemoglu et al., 2016; Pierce and Schott, 2016). On the other hand, I find much more muted earnings effects in industries with greater initial capital intensity and adoption of industrial robots. These results suggest that the benefits of investment incentives to workers may be undone in industries where labor-replacing technology is more widespread.

Policy concerns around investment incentives have often focused on the potential for disparate impacts across worker groups, particularly among those likely to be displaced by technological change (Acemoglu et al., 2020; Acemoglu and Autor, 2011). I find relatively little scope for differential impact of bonus by worker age, education level, occupation, gender, or race. Instead, workers in production occupations and over the age of 35 at the time of the policy benefited relatively more from bonus in the form of higher earnings, while production workers and workers without a college education further benefited from greater labor force attachment. The fact that I find larger scope for heterogeneity by industry rather than worker-level characteristics suggests that the broader stimulus effects of investment incentives can drive disparate policy impact more than granular worker characteristics.

My results stand out from a number of recent studies that fail to find significant earnings or wage effects from policies that stimulate investment. I conduct firm and local labor market level analyses to explore why the effects I identify at the worker-level have eluded prior approaches. I find modest positive earnings growth among long-duration firms on average, but these effects disappear when weighting regressions by initial employment levels, suggesting that compositional differences in effects across firms largely mask any earnings growth at the industry or plant level. Using publicly-available state-industry level data, I find that an increase in new hires, who earn disproportionately less than incumbent workers, can explain large declines in average earnings among long duration industries following policy implementation. Worker-level data is thus essential for detecting earnings effects in my setting, particularly because coincident labor demand growth changes the composition of the workforce in a way that confounds aggregate estimates.

At the local labor market level, I show that simple policy exposure measures based solely on geographic location are likely to produce attenuated earnings effects due to differences in policy exposure among workers in the same location (Garrett et al., 2020; Aghion et al., 2023a). I instead leverage job flows across industries to construct a market-level policy exposure measure that captures how likely a given worker is to transition to firms in long duration industries. I find significant effects of market-level exposure on worker earnings and mobility patterns. This result highlights the importance of appropriately measuring labor market structure when studying the market-level effects of tax policies on workers (Nimczik, 2023).

In the final part of my analysis, I consider how the effects of bonus on worker labor force attachment mediate the effects of bonus on worker earnings. Despite reducing the probability that workers experience costly job separations, I fail to detect a significant effect on the overall probability that exposed workers separate from their 2001 firm following the policy. However, among workers that separate from their firm by 2005, I estimate a much larger 7.9 percent positive earnings effect for those in long duration industries in 2001. This effect size is difficult to reconcile with the effects I find on costly job separations, suggesting significant unobserved heterogeneity in the types of job moves induced by bonus in a way that leads to higher earnings. Local policy exposure plays an important role in explaining the positive earnings effects among job movers, as workers initially at treated firms are subsequently more likely to move to other treated firms.

A broad class of models of imperfect competition in labor markets predict that incentive-induced investments generate wage passthrough. I find significant positive earnings effects among workers that remained at their firm through 2001, which provides suggestive evidence that firms may increase worker earnings by passing on the productivity benefits of new investment. I find that bonus lead to a significant increase in log value-added per worker among treated industries. These dynamic effects closely track relative earnings growth for workers that remain at their firm, providing further evidence that these effects reflect productivity passthrough. Using my value-added estimates as a proxy for firm quasi-rents, I calculate empirical earnings passthrough elasticities of $0.45 - 0.57$ after correcting for correlations between firm and market-level exposure of bonus. This benchmark suggests significant passthrough from productivity to worker earnings.

This study contributes to a growing literature on the effects of tax policies that incentivize investment in new machinery on worker outcomes. In a previous paper, Curtis et al. (2022) find that the 2001 bonus depreciation policy lead to significant employment and capital growth among exposed plants and industries. In the context of a structural model of factor demand, these estimates imply that around 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 or total factor productivity at plants. Similarly, Garrett et al. (2020) find that counties with greater exposure to bonus 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, I show that manufacturing workers in 2001 experienced an increase in earnings and a reduction in adverse job separations as a result of the U.S. wave of bonus depreciation.

These results 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.[1] I include a rich set of controls in all specifications to ensure that my 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 my sample period. My 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.

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

The paper is organized as follows. Section 1 presents details about the implementation of bonus depreciation, discusses salient features of U.S. manufacturing during the sample period, and discusses the matched employer-employee data essential for the study. Section 2 presents my main results on the effects of bonus depreciation on worker earnings and employment outcomes, as well heterogeneity along worker and industry characteristics. Section 3 explores my worker-level evidence in the context of prior studies that found null effects at the firm, industry, or local level. Section 4 relates my earnings effects to worker mobility patterns and estimates productivity passthrough. Section 5 concludes.

1 Institutional Background and Data

This section describes how bonus depreciation stimulates new investment. I then describe how I utilize administrative matched employer-employee data to construct my analysis samples. I also discuss the salient trends in U.S. manufacturing over my sample period that are important for contextualizing my results and the potential impacts of bonus on worker outcomes.

1.1 Bonus Depreciation as an Investment Incentive

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" or "bonus." Bonus is a form of accelerated depreciation that allows firms to deduct a percentage 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 remainder of the expenditure.

To illustrate how bonus exploits existing tax rules for depreciation to stimulate investment, consider a manufacturing firm that wishes to expand their production and purchases $10 million in new equipment to do so. Unlike wages and many other business expenses, the firm cannot immediately deduct this $10 million purchase from its taxable income. Instead, they must follow a depreciation schedule specified by the IRS for equipment purchases in their industry. Panel A of Figure 1 shows an example of an asset class with a 3-year recovery period as determined by MACRS. In normal circumstances, the firm would be able to deduct approximately $3.3 million of the value of the new equipment in the year in which it was purchased, an additional $4.4 million in the following year, and the remainder over the next years.

Bonus allows this firm to deduct a pre-determined percentage of the year 1-3 depreciation immediately. Panel A of Figure 1 shows how a bonus rate of 50% benefits this firm by allowing them to deduct an additional $3.3 million of the value of their new equipment in the year of purchase. In practice, bonus does not alter the total dollar value of these deductions, and their overall tax obligation over the life of the asset would be unchanged, all else equal. However, in present value terms this incentive has a material benefit to the firm. Though these benefits may be small if the firm faces low interest rates and can easily borrow to fund capital expenditures, bonus has been shown to be especially valuable for firms as a source of cash flow (Edgerton, 2010; Zwick and Mahon, 2017), a potentially important feature in U.S. manufacturing at a time in which many firms were contracting.

The fact that MACRS specifies different depreciation schedules across industries creates meaningful variation in the generosity of the policy across industries. Panel B of Figure 1 provides an example for bonus if the manufacturing firm instead normally deducts the $10 million purchase over ten years. A bonus rate of 50% instead increases their purchase year tax benefit in the form of deductions from $1 million to $5.5 million.

Formally, let z_0 denote the net present value of the depreciation deductions associated with $1 of investment, as determined by differences in depreciation schedules in the absence of bonus. For a bonus rate b, the NPV of the deductions associated with this investment are given by $z = b + (1 - b)z_0$. This expression formalizes the idea that industries with initially lower z_0 values – that is, those that invest in long duration assets – incur a greater benefit from bonus.

I follow Zwick and Mahon (2017), who calculate z_0 values across industries, and exploit these differences in the net present value of deduction benefits across industries to investigate the effects of bonus across workers and firms. Specifically, I compare outcomes for workers at firms with relatively lower z_0 values to workers at firms under higher z_0. Brazell et al. (1989) provide a history of the MACRS system and highlight how this system evolved relatively independently from the actual use of machinery in modern manufacturing. It is also valuable to note that the asset classes specified by the IRS correspond to use rather than asset type. Thus, firms are very unlikely to be able to incur a tax advantage from shifting the types of equipment they purchase in response to the policy. Finally, Chodorow-Reich et al. (2024) demonstrate formally that industry z_0 values have a relatively low correlation with economic depreciation of assets across industries.

1.1.1 Bonus Depreciation in the United States

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. However, many purchases remained eligible through 2005. Panel C 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 42.3% from 2001 to 2005 imply an average decrease in the cost of capital of 2.4% during this time period.

In most of my analysis, I limit attention to this initial policy period, since workers are likely to sort across firms and industries with different policy exposure after 2001. In response to the 2008 financial crisis, bonus was implemented again at 50% and continued with temporary extensions until the Tax Cuts and Jobs Act of 2017, which increased the policy to 100% bonus depreciation, or full expensing, for several years before phasing out. Because the primary focus of this study is the effects of the policy on workers based on their exposure in 2001, I also discuss effects through 2011 for these workers, who may also be affected by the 2008 wave of bonus, in Section 2.5.

Bonus depreciation is a politically popular subsidy because it only changes the timing of tax deductions for firms. However, bonus has real costs as a tax expenditure because it implicitly defers tax revenue to later years, the value of which can further be eroded by inflation. The 2017 TCJA temporary expansion of full expensing came at a ten-year budget cost of $86 billion. The 2025 One Big Beautiful Bill Act, on the other hand, made 100% bonus depreciation permanent at a budget cost of $369 billion over ten years (Joint Committee on Taxation, 2025). Bonus and other accelerated depreciation policies have proven popular throughout the world since 2001, and many countries expanded incentives following the COVID-19 pandemic (Guceri and Albinowski, 2021; Maffini et al., 2019; Fan and Liu, Forthcoming). These patterns reinforce the need to rigorously evaluate the effects bonus on workers, particularly in the context of structural economic transformation.

1.2 Investment Incentives and 21st Century U.S. Manufacturing

The 2001 implementation of bonus depreciation came at a time of significant transformation in U.S. manufacturing. Charles et al. (2019) outline several forces of sectoral transformation that 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 during the early 2000s. Panel A of Figure 2 demonstrates these aggregate trends in manufacturing employment and capital stock over the 1997–2007 period. Of particular relevance to my study are trends in job creation and destruction rates over this period. Panel B of Figure 2 demonstrates that, while job creation rates remained relatively stable over the 1997–2007 period, the 2000–2004 period featured a significant increase in the job destruction rate.

While bonus stimulated investment throughout the economy, it had large effects on exposed plants and industries in manufacturing (Curtis et al., 2022). The fact that manufacturing featured high levels of job destruction following the policy points to the preservation of existing jobs as a meaningful channel through which bonus could affect labor market outcomes for workers in more exposed industries. Appendix Figure A.1 demonstrates that, while job destruction rates also increased in long duration industries, this trend was less severe than in industries primarily investing in short-duration assets.

The overall decline in employment over this time period had a significant impact on workers. Autor et al. (2014) 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.

A separate literature has emphasized the significant impact of industrial robot adoption on manufacturing workers. Acemoglu and Restrepo (2020) find negative effects of industrial robots on employment and wages across U.S. commuting zones. In a follow up paper, Acemoglu and Restrepo (2022) argue that the adoption of automation technologies was a significant driver of rising wage inequality over the 1980 to 2016. Acemoglu et al. (2020) argue that bonus depreciation and other features of the U.S. tax code systematically drive adoption of automation technologies in a way that is biased against labor.

1.3 Worker-Level Data Sources and Sample Construction

A complete accounting of the effects of investment incentives on workers requires longitudinal data on workers before and after policy implementation. Panel data is especially important in this context because it allows me to study individual workers' exposure to the policy, based on their firm attachment at policy implementation, and subsequently examine their employment and earnings outcomes over time and across firm transitions.

A primary advantage of this paper over many previous studies on the labor market effects of investment incentives 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 date of birth, race, and and sex for the near-universe of the U.S. workforce in states that report earnings information to the LEHD.[2] This project uses data from twenty-one states and the District of Columbia.[3]

The matched nature of the LEHD allows me to recover detailed information about the firms to which workers are attached. Crucially, I observe the modal 4-digit NAICS industry classification code for employment within the firm, which allows me to classify firms by investment duration following Zwick and Mahon (2017) for each worker based on their 2001 firm. Throughout the analysis, I follow prior research and classify workers as exposed to the policy if their firm falls below a discrete cutoff in the NPV distribution, $z_0 < 0.875$. This corresponds roughly to the bottom tercile of manufacturing industries and a break in the z_0 distribution, as shown in Panel D of Figure 1). As calculating z_0 requires a discount rate, a discrete treatment is useful because it is less sensitive to mismeasurement driven by potential differences in discount rates across

[2]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.

[3]The states used in this analysis are: Arizona, Arkansas, California, Colorado, Delaware, Idaho, Illinois, Indiana, Iowa, Kansas, Maine, Maryland, Montana, New York, Oklahoma, Oregon, South Carolina, Tennessee, Texas, Washington, West Virginia, and the District of Columbia. For more detailed discussions on the construction of the LEHD files, see Abowd et al. (2008) and McKinney and Vilhuber (2008).

industries. I show that my earnings results are robust to defining treatment using the continuous z_0 measure in Section 2.

I also supplement the LEHD with person-level data from the 2000 Decennial Census. The long-form questionnaire, which was administered to individuals from 1 in 5 households and recorded additional worker information, most notably occupation of primary job and educational attainment. The timing of the Census is thus highly useful for my study because it allows me to measure worker occupation and education level in the year prior to policy implementation.

1.3.1 LEHD Sample Selection

I consider a sample of workers who were employed by manufacturing firms in the 2001 calendar year. I impose several sampling restrictions to ensure my analysis focuses on workers with high labor force attachment and for whom I can verify similar pre-policy outcomes across treatment groups. Specifically, I restrict attention to workers between the ages of 25 and 54 years old, who had at least two years of tenure at their 2001 firm, and make more than $15,000 in 2001 dollars each year over the 1997-2001 period.[4] When a worker receives earnings from more than one firm in any year throughout the sample, I aggregate their earnings across jobs to obtain a single yearly earnings figure and assign them to the firm at which they received their highest earnings. I then use the 4-digit NAICS industry code of the primary firm in 2001 to classify workers according to the investment duration schedule faced by their employer.[5]

My primary analysis sample selects on workers who report positive earnings in every sample year, which allows me to cleanly identify effects on earnings in the absence of lengthy spells of non-employment. Over the 1997-2007 period, this sample comprises 3,304,000 observations from about 310,000 workers. To investigate effects on workers' annual employment probability, I also consider a sample that includes workers who report calendar years with zero earnings in the post-treatment period, for which I impute zero earnings in missing years, yielding a balanced panel of 4,563,000 observations over the 1997 to 2007 period. Appendix D presents the full details of my sampling procedure.

Table 1 presents summary statistics of 2001 worker characteristics across treatment and control groups, as defined above. Treated workers in my main sample of workers earn less on average than control workers. This pattern holds in another sample that relaxes the age, tenure, and minimum earnings restrictions. I supplement this information with public-use data from the Decennial Census, which I use to assess a broader range of worker characteristics. Treatment and control groups are similar across basic demographic characteristics, though treated workers are less educated and more concentrated in production occupations, as defined by Acemoglu

[4] For a recent example using similar sampling restrictions, see Kovak et al. (2024).

[5] Because the LEHD does not provide establishment-worker linkages, I rely on the modal firm-level 4-digit NAICS code to determine worker treatment status and exposure to industry-level controls.

and Autor (2011). These patterns are consistent with the relative earnings patterns in Panel A, which also appears in the Decennial data. I further validate my sample with aggregated employment data from the NBER-CES Manufacturing Database on total payroll, total employment, and employment in production occupations. These data point to a higher and more equal share of production workers across treatment groups, although treated industries still maintain a higher share of production workers, and lower average annual earnings across categories. These differences in education level and task content of jobs across treatment groups underscore the value of pairing the LEHD with Decennial Census microdata, and I include flexible time controls that interact production occupation status with educational level in all of my primary earnings regressions.

2 The Effects of Bonus Depreciation on Workers

In this section, I present my main results on the employment and earnings outcomes of workers exposed to bonus depreciation. I also explore how earnings effects are mediated by job mobility following policy implementation, and how these worker-level estimates relate to firm-level earnings and employment outcomes.

2.1 Research Design

My core research design is an event study model that estimates the effects of bonus on worker-level outcomes based on firm-level exposure to the policy in 2001. I estimate variants of the following model:

$$Y_{ijt} = \alpha + \sum_{y=1997}^{2007} \beta_y \Big[\mathbb{I}[d = Long] * \mathbb{I}[y = t] \Big] + \gamma_{\mathbf{i}} + \mu_{\mathbf{jt}} + \mathbf{X_{it}} + \epsilon_{cdt}, \tag{1}$$

where i indexes workers, j indexes worker industry in 2001, and t indexes years. The vector $\mu_{\mathbf{jt}}$ captures a range of fixed effects I consider to ensure that my estimates are not confounded by differences in trends across worker demographics or differential exposure to outside shocks. Because of imbalance in occupational and educational composition of workers across treatment and control groups, my preferred specifications include production occupation-by-education level indicators interacted with year fixed effects.

As discussed in Section 1.2, accounting for differential exposure to secular trends in manufacturing is especially important in my setting for credibly identifying the effects of bonus on worker outcomes. I include 4-digit NAICS industry-level controls for the salient forces of sectoral transformation discussed in Charles et al. (2019).[6] These include (1) a measure of industry exposure

[6] I determine worker industry-codes based on the modal employment within firms, and merge on controls at

to import competition from China of Autor et al. (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).[7] I operationalize these controls by fixing values in 2001, taking deciles of the resulting distribution in my sample, and interacting decile indicators for items (1)–(3), as well as the continuous robot exposure measure, with year fixed effects. These fine-grained, flexible controls allow me to compare workers with very similar exposure to these potentially confounding trends in U.S. manufacturing during the sample period.

2.2 Worker Employment and Job Displacement

I begin by considering the effects of bonus on the employment patterns of workers. As shown in Curtis et al. (2022), the 2001 implementation of bonus depreciation lead to significant increases in employment among exposed plants and industries. However, these aggregate trends could mask reallocation of existing workers in exposed industries as firms adjust their production processes to accommodate new investments. Matched employer-employee data thus allows me to test directly how bonus impacted the employment trajectories of these workers.

I first estimate a linear probability model of the form of Equation 1 with an indicator for whether workers report positive earnings as the outcome variable. Panel A of Figure 3 shows that bonus depreciation lead to a significant reduction in the probability that workers who were employed at exposed firms in 2001 report zero earnings over the years 2002 to 2007.[8] By 2007, I find that exposed workers were 1.0 percentage point ($p = 0.016$) more likely to report positive earnings.[9] This estimate represents a meaningful increase in worker labor force attachment. Using Panel Survey of Income Dynamics (PSID) responses on labor-force attachment, my LEHD estimates imply an increase in employment of 7.6% to 10.1%.[10]

I also explore whether bonus affects the firm separations for workers that maintain positive earnings over the sample period using similar linear probability models. Panel B of Figure 3

the 4-digit NAICS level since this likely induces less measurement error than modal 6-digit industries. I confirm that my results are also robust to utilizing 6-digit NAICS codes to merge on these controls using the LEHD U2W files, which impute worker establishments.

[7] I also demonstrate robustness to measuring exposure to trade using the "NTR Gap" measure of Pierce and Schott (2016), which measures the gap between tariff rates following the US granting China Permanent Normal Trade Relations status in 1999 and expected import tariff rate hikes in the absence of normalization at the industry level.

[8] By construction, all workers in my estimating sample report earnings in pre-trend years.

[9] Appendix Figure A.2 shows that bonus reduces the probability that workers undertake other costly labor market adjustments in the form of cross-state moves. Specifically, bonus reduced the probability that exposed workers migrated to a different U.S. state after 2003, topping out at a 1.4 percentage point effect by 2007.

[10] The PSID reports that 13.2% of age 25–54 manufacturing workers in 2001 were subsequently out of work at the time of their 2007 interview, while 9.9% had also left the labor force. Because my LEHD-based non-employment captures a combination of labor force non-participation and calendar year-long unemployment spells, this implies a reduction in non-employment of 7.6% to 10.1%.

shows estimates on the cumulative probability of a worker having separated from their 2001 firm or experiencing job loss, defined as a separation that involved at least one quarter of zero earnings.[11] In this sample, 26.1% of workers experience at least one job separation by 2005, while only 5.9% experienced such a job loss. I thus interpret effects on my job loss measure as reflecting particularly costly layoffs involving lengthy unemployment spells, rather than a comprehensive measure of involuntary job separations.

On average, bonus had a slightly negative but noisy and statistically insignificant effect on the probability that workers separated from their 2001 firm. However, I find that bonus significantly decreased the probability that exposed workers experience costly job loss starting in the year following policy implementation. Bonus reduced the likelihood that exposed workers experienced at least one costly job loss by 1.5 percentage points ($p < 0.001$) by 2005, when the first round of bonus expired. Relative to the baseline probability of such job separations, this represents a large effect size of 25.3%. Together, these patterns suggest that the labor demand stimulus of bonus played an important role in preserving the jobs of workers in exposed industries.

2.3 Worker Earnings

I now turn to describing the effects of bonus on the earnings of exposed workers. Matched employer-employee data is again essential for identifying the effects of bonus on worker earnings, since aggregated data may be confounded by sorting along observed and unobserved worker characteristics. Matched data also allows me measure the effects of bonus on worker earnings inclusive of job transitions such as those documented in the previous section.

Figure 4 plots β_t coefficient estimates and 95% confidence intervals based on Equation 1 with the natural log of earnings as the outcome variable and worker fixed effects, production worker-by-education level-by-year fixed effects, and the full set of flexible controls for salient manufacturing trends described in 2.1. The average earnings of workers trended very similarly across treatment categories prior to the implementation of bonus depreciation in 2001 but began to increase for workers in long duration industries within the following year. I estimate positive and significant effects on worker earnings for the years 2003 to 2007, with the most precise estimates in the years 2004 to 2007, This effect reaches its peak magnitude of 4.5% in 2005.[12]

[11] Job switches are identified when a worker no longer appears at a given SEIN. Because firm identifiers may change for various reasons, such as changes in corporate form or industry classification, I use the LEHD Successor-Predecessor to identify SEIN switches that likely do not constitute a worker actually moving to a new firm. This measurement is important given evidence from Bloom et al. (2024) that many U.S. manufacturing firms reconstituted themselves in service sectors during this period.

[12] Appendix Figure A.3 shows similar earnings magnitudes and dynamics when defining treatment using the continuous z_0 measure, which I scale to reflect a change equivalent to the difference in the means of z_0 in my treatment and control groups.

2.3.1 Robustness

I consider a number of additional specifications to validate my finding that bonus significantly increased the earnings of exposed workers, all of which reinforce this conclusion. First, Panel A of Figure A.4 shows that my main findings are not sensitive to the sampling criteria used to construct the primary estimation sample. Specifically, I find similar dynamic earnings effects in a sample that includes workers that report zero earnings in any post-2001 year, and a sample that does not impose any age, tenure, or minimum earnings restriction to construct the sample.[13] Appendix Table 2 shows that the difference-in-differences terms corresponding to Figure 4 are significant, positive, and of similar magnitude for samples with and without zero earnings observations for alternative sets of controls, including those that omit education controls, those that control for the initial earnings level of workers, and those that control for 2001 firm size.

My estimates of the earnings effects of bonus are also robust to a number of alternative specifications that rule out specific endogeneity concerns. Table 2 shows long difference estimates for 2001–2005 and 2001–2007 effects on earnings, respectively, for my baseline specification in Column (1) and several alternative models. Column (2) shows similar effects when I include commuting zone-by-year fixed effects. Column (3) shows that my effects are virtually unchanged in the presence of 2-digit SOC occupation-by-year fixed effects. This specification helps rule out the possibility that granular trends in the task content of manufacturing jobs across treated and control firms bias my estimates, which made possible by the linking the LEHD to the 2000 Decennial Census. Columns (4) and (5) show that my estimates are robust to potential misspecification of exposure to import competition shocks. Specifically, I find similar estimates while controlling for the import competition measure of Pierce and Schott (2016) and when using imputed worker establishment industry codes to control for import competition exposure at the 6-digit NAICS level.

Lastly, I find significant positive earnings effects over the 2002–2005 period when only including person fixed effects and control for education level trends, but these estimates are roughly half the size as those that include sector trend controls. Controlling for import competition, robotization, pre-period capital intensity, or pre-period skill intensity individually all produce positive post-2001 effects. Appendix Figure A.4 presents full event study plots for all alternative specifications.

2.4 Long-Run Effects of Bonus Depreciation

This section briefly considers the effects of 2001 exposure to bonus on worker outcomes through 2011. As noted in Section 1.1, bonus was reinstated at a 50 percent rate in response to the

[13]For the "with zeros" sample, I estimate the event study model on earnings levels using a Poisson Pseudo-Maximum Likelihood Estimator and report exponentiated coefficients to preserve the interpretation of coefficients as percent changes when the outcome variable takes on zeros (Silva and Tenreyro, 2006)

2008 financial crisis. Insofar as worker exposure to bonus is persistent, the results in this section combine potential long-run effects of the initial bonus round with additional effects from the expansion of bonus.

Table A.4 presents estimates of the 10-year effects of working in a long duration firm in 2001 across worker outcomes. Columns (1) shows that exposed workers were 2.3 percentage points more likely to report positive earnings in 2011, compared to a 2007 effect of 1.0. I similarly find a larger reduction in workers' probability of moving across state lines by 2011 compared to the 2007 effect. These results point to long-lasting benefits from bonus increasing labor force attachment in the years following 2001, particularly in the face of the 2008 recession.

Columns (3) – (6) present the long run effects of exposure to the 2001 implementation of bonus on worker earnings across several specifications. In my base specification, treated workers still have 3.7 percent higher earnings in 2011, although this is slightly smaller than the 2007 effect reported in Table 2. This result suggests that, while the effects of bonus were persistent for workers in my primary sample, the reinstatement of bonus in 2008 did not generate additional benefits to workers who were initially exposed to the policy. Column (6) shows no significant effect on earnings when I do not impose age, tenure, or minimum earnings restrictions to my sample.

2.5 Heterogeneous Effects of Bonus Depreciation

A substantial concern among policymakers is that investment incentives may stimulate capital investments that have disparate impacts across workers, particularly those likely to be displaced by automation technologies (Acemoglu and Restrepo, 2020). As shown in Curtis et al. (2022), the 2001 wave of bonus stimulated 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. In this section, I explore how the effects of bonus differed across by worker type and exposure to salient trends in the manufacturing sector.

2.5.1 Worker Demographic Heterogeneity

Table 3 presents the heterogeneous effects of bonus depreciation on worker earnings and employment probabilities over the 2001–2007 period and for the worker characteristics denoted by column headers. Panel A reveals modest differences in the earnings effects of bonus across worker groups. I find no differential earnings effects for workers by education level or for women and men. I find relatively larger earnings effects for workers aged 35 or over in 2001, in production occupations, and for black workers. Column (1) shows that relatively older workers experienced 2.3 percent ($p = 0.037$) higher earnings from exposure to bonus, relative to workers under the age of 35 in 2001. Column (2) shows slightly larger earnings effects for workers in production occupations, though this effect is small and relatively imprecise (0.8 percent, $p = 0.072$). As these patterns pertain to policy discussions around investment incentives and labor, larger effects for older workers and those in production occupations shows that bonus had stronger earnings impacts for workers who are both more vulnerable to displacement, i.e. due to automation technology, and relatively less adaptable in the face of technological change. This serves as further suggestive evidence that preventing job loss is one channel through which bonus stimulated worker earnings.[14]

I additionally consider the heterogeneous effects of bonus on the probability that workers report positive earnings in the years following policy implementation. Panel B of Table 3 shows relatively little significant heterogeneity across worker characteristics for the 2001–2007 effects of bonus exposure. However, Table A.5 reveals that production workers benefit from bonus significantly more than other workers by 2011. The relative effect of 1.0 percentage point is large compared to a 1.9 percentage point baseline effect for workers in other occupations. This result again suggests that the benefits of bonus were especially strong for workers facing external labor market risks.

The relative lack of significant heterogeneity I find, particularly across education levels or occupations, stands in contrast to other recent studies on the effects of investment incentives across workers. Moon and Duan (2024) find that a similar accelerated depreciation policy in Canada lead to the displacement of low-skill workers, as proxied by AKM worker effects. Similarly, studies on the effects of industrial robots on workers have typically found negative wage effects concentrated in production occupations (Humlum, 2019; Acemoglu and Restrepo, 2020). My results instead reinforce the importance of considering how structural trends may interact with investment incentives, as well as the value of high-quality occupation and education measures at the worker level.

2.5.2 Heterogeneity Across the Manufacturing Sector

I next explore heterogeneity across the four forces of sectoral transformation described in Charles et al. (2019) that serve as control variables in Equation 1. I conduct heterogeneity analysis by interacting event study coefficients with indicators for whether each of the sectoral measures is above the sample median. Figure 5 presents these results. In each panel, dark shading on the above-median exposure coefficient estimates denotes that an estimate is significantly different from its below-median analog at the 5 percent level.

Panel A shows that I find significant scope for heterogeneous effects on worker earnings based

[14]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. (2018b) report that these policies have been shown to be relatively less effective for helping older workers readjust to new positions. Jaimovich and Siu (2020) shows that the disappearance of jobs in routine occupations is concentrated during recessions.

on the import competition measure of Acemoglu et al. (2016). For workers in industries with below-median exposure to import competition from China, bonus induces statistically significant but more muted effects on earnings relative to my main specification. In contrast, workers initially working in industries with above-median import competition benefit more from bonus, particularly from 2005 to 2007.

I also find significant heterogeneity on worker earnings based on industry robot penetration and capital intensity.[15] Panel B of Figure 5 shows significantly larger earnings effects for workers in industries that experienced below-median robot penetration over the 1993-2007 period, based on the measure of Acemoglu and Restrepo (2020). While workers in industries with high robot adoption still experienced earnings gains from exposure to bonus, these effects are of smaller magnitude than in my main earnings specifications and are insignificant and close to zero in 2006 and 2007. Panel D shows that I also find lower earnings effects of bonus in industries with greater capital intensity in 2001, although the relative earnings differences are of relatively small magnitude. In the context of broad sectoral trends toward automation and greater capital intensity, these results suggest that, while the benefits of bonus to workers were broadly shared over this time period, earnings effects may have been muted by structural transformation. Relative to my estimates on the heterogeneous effects of bonus across worker characteristics, these estimates also suggest that, to the extent that bonus interacted with labor-replacing technology, these effects were more likely to occur at the industry level, rather than due to granular interactions between incentive-induced investments and particular worker subgroups.

3 Interpreting Worker-Level Earnings Effects

The earnings effects I document in the previous section diverge sharply from prior evidence on the effects of investment incentives at the plant, firm, industry and local labor market level. In this section, I consider how my worker-level estimates relate to alternative approaches. I first utilize firm and industry level data to serve as a bridge between my worker-level estimates and prior work. I then construct market-level exposure measures that account for the likely channels through which policy exposure may affect worker earnings at the local level.

3.1 Firm-Level Effects

Several recent studies have documented significant effects of investment incentives on both investment and employment while also finding null or negative effects on average earnings (Curtis et al., 2022; Aghion et al., 2023b; Hirvonen et al., 2025). To connect my worker-level evidence to these results, I consider firm-level estimates of the effects of bonus on mean worker earnings

[15] I find no differential earnings effects based on industry skill intensity in 2001, as shown in Panel C.

based on the LEHD data used to construct my worker-level sample.

Panel A of Figure 6 presents estimates from a firm-level equivalent of Equation 1 on a balanced panel of firms over the 1999–2007 period. As in my worker-level specifications, I flexibly control for industry-level time trends by exposure to import competition, industrial robot adoption, and capital and skill intensity. In contrast to prior work, I estimate a positive effect on average worker earnings at the firm level, reaching a peak magnitude of 2.6 percent ($p < 0.001$) in 2005. However, weighting these regressions by 2001 employment levels yields null effects, suggesting compositional differences in wage effects across firms matter for industry-level earnings effects.[16]

How do these estimates align with findings that exposed workers experienced significant, sustained increases in average earnings? Panel B of Figure A.5 provides a direct linkage between the two sets of results by showing the firm-level effects on the log mean earnings of workers that were present at the firm in 2001. These effects thus combine any within-worker earnings growth at the firm with any effect of bonus on the composition of workers that remain at their firm. On average, bonus induced significantly larger mean earnings for this cohort at the firm-level. When weighting by 2001 employment, these effects are still positive, but smaller and less persistent.

The fact that this cohort of workers experiences positive firm-level earnings growth suggests that changes in the composition of the workforce likely drive the null or negative effects found in other studies. I use industry-state level data on firm hiring from the U.S. Census Quarterly Workforce Indicators (QWI), which is constructed from LEHD files, to provide evidence that new hires contribute to this discrepancy. Panels A and B of Figure A.6 confirm the results in Curtis et al. (2022) that bonus induced a positive industry-state level effect on log employment but a negative effect on log mean earnings.[17] Panel C additionally shows that, in exposed state-industries, bonus induced an immediate spike in hiring rates of around 0.012, relative to a 2001 sample mean of 0.26. Panel D shows that new hires in exposed industries were paid relatively less than new hires in control industries.

On average, new hire earnings are significantly lower than mean earnings for all workers among manufacturing industries. In 2001, the average ratio of new full quarter hire earnings to full quarter earnings for all workers was 0.74 in my QWI sample. Thus, all else equal, an influx of new hires can drive down firm-level earnings even if incumbent workers experience real earnings growth. To formalize this intuition, I conduct an exercise similar to that in Curtis et al. (2022), who show that observed changes in the demographic composition of workforces can explain the negative effect of bonus on plant and industry-state level mean earnings. Specifically,

[16] Panel B of Figure 6 shows that bonus induced very similar growth throughout the within-firm earnings distribution. This result broadly aligns with my findings in Section 2.5.1 that the earnings effects of bonus were similar across worker demographic subgroups. It also stands in contrast to recent results on the distributional effects of corporate income tax cuts, e.g. Kennedy et al. (2024).

[17] All QWI-based regressions are weighted by 2001 state-industry employment and include controls for salient drivers of manufacturing transformation. Panel A of Figure A.5 confirms a similar effect on log employment at the firm level.

I regress log earnings on the ratio of total hires to total employment at the annual level using pre-2002 data. I then use this regression to generate an out-of-sample prediction of log earnings based on changes in the hire rate in the post-2001 period. Panel A of Figure A.7 presents the results of this exercise, which shows that the 2002–2003 spike in hiring among treated industries can explain a 1.1 percent decline in mean earnings in 2002 and a 0.89 percent decline in 2003.[18] Given my result that treated new hires in these years earned relatively less than new hires in the control group, this likely represents an underestimate of the effects of new hires on mean earnings.

The results in this section demonstrate that worker-level data is particularly essential for detecting wage and earnings effects in the presence of labor demand shocks, which may induce compositional shifts in the workforce that mask earnings growth among incumbent workers.

3.2 Local Labor Market Effects

Existing work on the local labor market incidence of tax policy has implicitly assumed either geographic locations or location-industries represent discrete labor markets (e.g. Suárez Serrato and Zidar, 2016; Fuest et al., 2018; Garrett et al., 2020). In practice, however, the incidence of local shocks on labor market outcomes, particularly worker earnings, could vary markedly depending on the structure of local labor markets (Nimczik, 2023). In Section 2, I estimate very similar effects of bonus on worker earnings when including commuting zone-by-year fixed effects. The fact that labor market competition does not lead to earnings equalization across workers in the same geographic location provides strong initial evidence that any market-level effects occur at a more granular level within locations. The extent to which local policy exposure influences the earnings of a given worker thus depends on whether other exposed firms are likely to hire that worker, which may increase earnings directly via job transitions or by placing competitive pressure on other firms to raise wages.

In this section, I construct a measure of local policy exposure that incorporates information on job-to-job transitions in the LEHD to identify the effective labor markets facing workers in a given industry. This allows me to identify the local effects of bonus depreciation via workers' likely job network without making *a priori* assumptions about policy exposure based on location or industry.

[18] Because the QWI does not track the composition of new or recent hires over time, predicted effects in years after 2002 likely do not capture how the presence of low-tenure workers attenuates mean earnings. Panel B of Figure A.7 shows that changes in worker demographics explain the persistent negative earnings effect from Panel B of Figure A.6, though this model only predicts modestly more negative effects in 2002 and 2003 relative to the model that only relies on changes in the hire rate.

3.2.1 Labor Market-Level Policy Exposure

I first consider a simple commuting zone-level measure which defines policy exposure as the share of total employment in 2001 in industries investing in long duration assets:

$$E_{c,2001} = \frac{\sum_k \mathbb{1}[Long_k] \cdot L_{c,k,2001}}{\sum_j L_{c,k,2001}},$$

where $\mathbb{1}[Long_k]$ is an indicator denoting that the NPV of depreciation deductions in industry k is below my treatment threshold ($z_0^k < 0.875$) and $L_{c,k,2001}$ denotes total employment in commuting zone c and industry k in 2001. This measure mirrors that of Garrett et al. (2020), who find that county-level bonus exposure increased industry-location employment but had no effect on mean earnings.

Estimates of the effects of this bonus exposure measure on worker earnings are likely to be attenuated relative to the policy's true effect if workers differ in terms of their effective policy exposure via their respective labor markets. As an alternative, I adapt a simple framework from Arnold (2023) that models the value of jobs across industries for workers using observed job-to-job transitions. Appendix B presents the full details of this approach. I suppose that workers receive job offers across industries in their local labor market at a rate proportional to local market size. Workers then accept offers based on the utility associated with jobs in other industries relative to that of their current job. This framework yields the following expression for a term $V(k|m)$ that captures the common utility value of jobs in an industry k for a worker in industry m:

$$V(k|m) = \frac{P(k|m)}{P(m|m)} \cdot \frac{L_m}{L_k},$$

where $P(k|m)$ is the probability that a worker in industry m transitions to industry k, $P(m|m)$ is the probability that worker moves to a job in their own industry, and L_m and L_k denote the total employment in each industry in the local labor market. This expression captures the idea that if workers move from m to k disproportionately often, jobs in industry k are likely to be more valuable to workers in industry m, all else equal. I construct an empirical analog to this measure with job transition probabilities from within-commuting zone job moves and the average relative market size within commuting zones at the annual level for the 1996–2011 period. This yields an empirical measure of $V(k|m)$ denoted by $\nu_{m\to k}$.

With these flow-based measures in hand, I construct the following flow-weighted bonus exposure measure at the commuting zone-4-digit NAICS industry level:

$$\tilde{E}_{c,m,2001} = \frac{\sum_j \mathbb{1}[Long_k] \cdot \nu_{m\to k} \cdot L_{c,j,2001}}{\sum_j \nu_{m\to k} \cdot L_{c,j,2001}}. \tag{2}$$

This measure preserves the interpretation of the commuting-zone level measure $E_{c,2001}$ as the share of effective job opportunities in treated industries. However, this measure scales policy exposure from other local industries by the probability that a given worker will move to those industries.

3.2.2 Characterizing Market-Level Policy Exposure

Since post-2001 job moves may bias the continuous $\tilde{E}_{c,m,2001}$ measure, I consider a binary indicator that splits my sample into workers that are above and below the sample median of market-level exposure, $\mathbb{1}[\tilde{E}_{c,m,2001} < \tilde{E}_{p(50)}]$. Table 4 presents summary statistics across these "high" and "low" local exposure categories. Several important insights emerge from these statistics. First, workers in both local treatment categories have on average very similar policy exposure measured by the share of commuting zone employment in treated industries. By contrast, workers in the high exposure group have significantly higher policy exposure by the flow-weighted measure (0.467 compared to 0.015 in the low exposure group). Mechanically, this discrepancy is also reflected in workers' firm-level exposure. Only 0.6 percent of workers in the low treatment category also have firm-level exposure. Firm and market policy exposure are thus highly correlated in my worker-level sample. Because this correlation suggests that market-level exposure could be an important omitted variable in my analysis of firm-level policy exposure, I now turn to re-estimating Equation 1 and include the full interaction of year dummies with the binary flow-weighted exposure measure $\mathbb{1}[\tilde{E}_{c,m,2001} < \tilde{E}_{p(50)}]$.

3.2.3 Local Policy Exposure and Worker Earnings

Figure 7 presents event study estimates from the market exposure model with log earnings as the outcome.[19] Panel A shows that workers with above median local exposure to bonus have earnings that trend similarly prior to 2001 before increasing starting in 2002. In 2004 and 2005, I estimate that these workers have 1.4 and 1.5 percent higher earnings, respectively, than those with below median local exposure. Each of these estimates is statistically significant at the 1 percent level. Effect sizes fall in 2006 and 2007 after the policy's termination, consistent with elevated labor demand in years in which investment was subsidized by bonus. I also estimate this model while interacting the simple continuous commuting zone level exposure measure with year indicators, also shown in Panel A. In contrast to the flow-weighted exposure measure, these estimates are slightly negative and statistically insignificant.

As shown in Table 4, very few workers with firm-level treatment lie in the below-median

[19] In addition to the industry-level controls used throughout my empirical analysis, I also adapt Equation 2 to construct flow-weighted exposure measures to the Acemoglu et al. (2016) import competition measure. Specifically, I substitute an indicator for above-median import competition exposure for the $\mathbb{1}[z_0^j < 0.875]$ term and interact the resulting indicators with year dummies.

local exposure category. Thus, identification of the flow-weighted treatment coefficients comes primarily from comparing workers without firm-level treatment. However, the inclusion of this measure can still affect my estimates of firm-level exposure by restricting identification primarily to workers within the high local exposure category. Panel B of 7 presents estimates of firm-level bonus exposure in the presence of these controls. This figure shows that controlling for market-level exposure produces smaller estimates of firm-level exposure, suggesting that correlation with market-level exposure produces a small upward bias on the effects of firm-level exposure. Relative to prior studies that sought to detect wage or earnings effects at the market level, this result again emphasizes the value of linking individual workers to firm-level policy exposure.

4 Mechanisms: Job Mobility and Earnings Passthrough

In the final part of my analysis, I explore the mechanisms underlying my finding that bonus produced large and persistent positive earnings effects for treated workers. Section 2 demonstrated that bonus reduced the probability of job loss, increased the probability of employment, and increased average earnings for workers exposed to the policy at the firm-level. An extensive literature has documented large and persistent earnings losses associated with job displacement (e.g. Jacobson et al., 1993; Davis and von Wachter, 2011). A natural question that thus follows from these patterns is the extent to which the effects of bonus on job displacement mediate the policy's effects on worker earnings.

Beyond preventing costly job losses, there are two additional reasons why the role of job mobility matters for interpreting positive earnings effects. First, while bonus did not significantly affect workers' probability of separating from their firm overall, Panel B of Figure 3 shows relatively large confidence intervals on these estimates, which could mask differences in the types of moves experienced by workers following the policy. Second, the whether or not workers who remain at their firm experience an increase in earnings speaks directly to the question of whether new investments increase productivity in a way that passes through to worker earnings.

To investigate these separate channels, I consider the heterogeneous effects of bonus on worker earnings based on whether workers separated from their original firm in the years after policy implementation. I then present a simple theoretical framework to motivate how bonus depreciation can stimulate productivity in a way that generates pass through to worker earnings. Finally, I estimate the effects of bonus on industry-level productivity measures to provide a quantitative benchmark for the earnings effects I find among workers who remain at their 2001 firms.

4.1 Job Mobility and Worker Earnings

I explore how job mobility patterns affect worker earnings by re-estimating my earnings model while interacting event study coefficients with indicators for whether workers separated from their 2001 firm by 2005. This specification yields "stayers vs. stayers" and "mover vs. movers" comparisons of the effects of bonus. Although post-2001 moves are an endogenous outcome, these specifications provide a transparent descriptive comparison of worker earnings outcomes by move status, which allows me to assess the plausibility of potential selection mechanisms.

4.1.1 Earnings Effects Among Job Movers

Figure 8 presents these comparisons alongside the total earnings effect reproduced from Figure 4. Among workers who separated from their 2001 firm, exposure to bonus induced an immediate positive earnings effect following policy implementation. These effects are large and persistent, reaching a peak effect of 7.9 percent in 2005, where I can reject the null that the earnings effect on treated movers is statistically indistinguishable from that of stayers at the 1% level. Although a larger earnings effect among job movers could be explained by a reduction in costly job loss, as demonstrated in Section 2.2, the magnitude of these earnings discrepancies suggests a much larger scope for bonus to preserve worker earnings beyond my narrow job loss measure. Assuming job displacement leads to a 39 percent decrease in earnings, my estimated 1.5 percentage point reduction in the probability of a costly job loss would only induce an increase in earnings among movers of 0.6 percent.[20] While my job loss measure may undercount the number of layoffs experienced by workers in my sample, my measured effect on mover earnings is difficult to reconcile with my finding that around 26 percent of workers separated from their firms by 2005, particularly in the absence of a statistically measurable effect on job displacements. For instance, if the 7.9 percent effect on worker earnings is entirely driven by reducing layoffs, a negative 39 percent mean effect from job displacement would imply a $\frac{7.9}{39} = 20.3$ percent reduction in job displacement among treated workers.[21] This would imply an effect on job separations of 5.3 percentage points, which my estimates can reject with a p-value of 0.002.

To further unpack how bonus induces different mobility patterns, I revisit the market-level policy exposure measures constructed in Section 3.2. Table 5 presents 2005 and 2007 coefficient estimates for both local and firm exposure on four different cumulative measures of job mobility. The first two columns use the same outcomes reported in Panel B of Figure 3. In the presence of my local exposure measure, firm-level exposure still leads to a null effect on job moves and a reduction in the probability of workers experiencing a costly job loss. Interestingly, local policy exposure reduces the probability of both all job moves and costly job losses.

I additionally estimate the effects of policy exposure on workers' probability of moving to long-duration industries, in keeping with the idea that local exposure to the policy may benefit

[20] This benchmark corresponds to the first-year post displacement earnings loss for workers laid off in a recession reported by Davis and von Wachter (2011).

[21] Formally, if earnings for workers who did not experience a job loss were equal across treatment groups, and all workers who are laid off experience the same decline in earnings, this movers vs. movers estimate would be equivalent to $\beta = [\Pr(JobLoss|Bonus = 1) - \Pr(JobLoss|Bonus = 0)] \times \mathrm{E}[\ln Earn|JobLoss]$.

workers by providing additional job opportunities. In my sample, 6.3 percent of workers transition to a treated firm by 2005, and unsurprisingly, workers with above median policy exposure are more likely to make such transitions. Workers with firm-level exposure, however, are significantly more likely to make such transitions, with a 2005 effect of 7.2 percentage points. Relative to the sample average, these results suggest that the majority of workers moving into treated industries by 2005 were originally working in treated industries.

Given the apparent high correlation between firm and local policy exposure, these estimates on job moves point to local policy exposure as one mechanism through which bonus induced better job moves for workers at treated firms. However, workers with firm-level policy exposure are the primary beneficiaries of job moves to other treated firms. The benefits associated with local policy exposure in the form of higher earnings are likely to result from a reduction in job displacement, for example, via local general equilibrium effects, rather than promoting high quality job moves for workers without firm-level policy exposure in 2001. Still, the fact that workers with firm-level policy exposure in 2001 also benefit disproportionately from mobility to other treated firms provides further support for the idea that the benefits of bonus for job movers go beyond the more restrictive job loss measure I can construct using LEHD data.[22]

4.1.2 Earnings Effects Among Job Stayers

Among workers who remain at their 2001 firm through 2005, I estimate significant positive earnings effects that are more delayed but of similar magnitude to my estimates across all workers. By 2005, treated stayers receive 4.1 percent higher earnings. Despite a null effect of bonus on worker moves, this stayers comparison could still suffer from selection bias if bonus induces changes in the composition of workers who remain at the firm. Specifically, these effects could reflect selection rather than real earnings gains if workers differ in their expected earnings trajectories in the absence of bonus. Selection bias would be present if bonus disproportionately caused incumbent workers with low expected earnings trajectories to separate from their firms, relative to the control group. The fact that I still find very stable pre-trends provides evidence against this possibility. I also estimate precise null effects on worker earnings in 2002 and 2003, which implies that any such compositional shifts induced by bonus would have to select on workers with this particular relative earnings trajectory. These results thus provide motivating evidence for the estimation of passthrough elasticities driven by productivity growth at exposed firms.

[22] Appendix C provides an alternative movers and stayers comparison that serves as a strict decomposition of my main earnings effect. These "movers vs. all" and "stayers vs. all" comparisons, shown in Figure A.8, reveal that workers in long duration industries that remain at their firm earn significantly more than the average worker in short duration industries. Treated movers still experience sizable declines in earnings, consistent with the idea that job separation during my sample period was particularly costly for manufacturing workers. Interestingly, treated movers fully recover earnings relative to all control workers by 2007. This result again points to market-level bonus exposure as a force that allows workers to weather job displacement.

4.2 Earnings Passthrough and Productivity Effects

To formalize how capital investment at the firm level can induce earnings growth for workers at firms with greater exposure to bonus, I consider a stylized wage bargaining framework in which firms face costs to recruit and hire new workers. I use this framework to motivate the estimation of wage passthrough parameters when paired with data on value-added among manufacturing industries. Passthrough provides a useful benchmark for understanding the magnitudes of the effects I find on worker earnings while also clarifying how market-level shocks can also induce wage growth.

A broad class of models of imperfect competition can generate passthrough to worker earnings from an increase in capital investment. All else equal, new investment increases the marginal revenue product of labor, $MRPL$. In competitive labor markets, firms will concurrently increase labor demand such that $MRPL$ stays constant and equal to the competitive wage rate. However, if firms face frictions when increasing labor demand, such hiring or training costs, workers may capture some of the benefits of increased productivity in the form of higher labor earnings.

I consider a single-period setup in which a single firm combines labor and capital in production and faces competitive factor prices. After firms make their initial labor and capital choices, bonus depreciation is implemented as a shock that reduces the rental rate r, Bonus $\equiv \frac{dr}{d\ln\phi} < 0$. Firms may freely adjust their capital in response to the shock, but I introduce frictions by assuming firms face hiring costs $c(H)$ to expand their existing stock of labor. For simplicity, I assume that these hiring costs are (at least initially) infinitely inelastic such that firms may not adjust labor demand in response to bonus.

Because greater capital investment increases the marginal revenue product of labor, bonus generates a wedge between $MRPL$ and the competitive outside wage at which worker earnings are initially set. Workers may leave the firm and receive a competitive outside option $\bar{w}(\Phi)$, which may be a function of the market-level effects of bonus depreciation Φ.

I assume wage determination follows the multilateral bargaining solution of Stole and Zwiebel (1996) in which workers possess hold-up power over their individual marginal product. Workers also possess hold-up power because the bargaining position of others workers will improve if they leave the firm.[23] Under Cobb-Douglas production, this framework yields the following expression for worker earnings following the implementation of bonus, where β is the worker Nash bargaining

[23] In Appendix E, I present a full model based on the Stole and Zwiebel (1996) bargaining solution. Labor and capital are jointly determined in anticipation of the wage bargaining process, which preserves the typical Stole-Zwiebel over-hiring channel. Examples of other models that can generate passthrough via increases in firm surplus include union bargaining models in which employees can hold up firm output and either bargain over employment and wages, as in efficient bargaining models (Brown and Ashenfelter, 1986) and right-to-manage models where firms determine employment and bargain over wages with a union (Van Reenen, 1996). Passthrough can also occur in wage posting models where firms face upward-sloping labor supply curves, as in Manning (2011), Card et al. (2018a) and Kline et al. (2019). Garin and Silvério (2024) provides an overview of passthrough from product demand shocks in the context of alternative models.

weight:

$$w(\bar{L}, K) = \bar{w}(\Phi) + \beta \times (\tilde{\gamma} \times MRPL(\bar{L}, K^*(\phi)) - \bar{w}(\Phi)), \tag{3}$$

where $\tilde{\gamma}$ is a scalar that captures how worker hold-up power reflects the $MRPL$ of all workers within the firm. This expression shows that worker wages are equal to the outside option, plus a share of firm quasi-rents: a rescaled measure of worker marginal revenue product of labor minus the competitive outside wage.

This expression is useful for illustrating the distinct channels through which bonus may influence worker earnings. First, if the competitive outside option is unchanged by bonus, then the earnings effect is simply the quasi-rent share in Equation 3. However, if bonus also increases market wages such that $\bar{w}(\Phi') > \bar{w}(\Phi)$, then this competitive pressure will increase wages further through the bargaining process. This yields the following expression for the total wage effect for workers at firms in long duration industries:

$$\frac{dw}{d\ln\phi} = \underbrace{\beta \times (\tilde{\gamma} \times MRPL(\bar{L}, K^*(\phi)) - \bar{w}(\Phi'))}_{\text{Firm Passthrough}} + \underbrace{[\bar{w}(\Phi') - \bar{w}(\Phi)]}_{\text{Market Exposure}}. \tag{4}$$

In the next section, I estimate passthrough elasticities with respect to proxies for these firm-level quasi-rents using bonus as a policy instrument. I then discuss these estimates in the context of the market-level exposure estimates presented in Section 3.2 as a measure of the relative bias induced by market-level competition.

4.3 Passthrough Estimation

Estimating elasticities of passthrough with respect to bonus depreciation requires pairing worker-level estimates on earnings with a measure of quasi-rents per worker. Specifically, I aim to estimate a regression of the form:

$$\ln w_{ijt} = \alpha + \varepsilon^{w,R} \ln R_{i,t} + \gamma \Phi_{j,t} + \nu_{ijt}, \tag{5}$$

where $\ln w_{ijt}$ are earnings for worker i in labor market j, $\ln R_{i,t}$ is a measure of log rents per worker at worker i's firm, and $\Phi_{j,t}$ is a measure of local labor market-level shocks that may confound estimates of my parameter of interest, the firm-level passthrough elasticity $\varepsilon^{w,R}$.

I approach estimation of $\varepsilon^{w,R}$ by using the NBER-CES Manufacturing Database to estimate the effects of bonus on log value-added per worker at the industry level. These estimates mirror the worker-level event study Equation 1. As with my worker-level regressions, I include controls for the four salient drivers of manufacturing transformation in Charles et al. (2019). Figure 9 shows that log value-added per worker trended similarly among long and short duration industries before 2001, and remained similar in 2002 and 2003. However, I estimate positive and marginally significant effects in the years 2004 through 2006. Specifically, log value-added per worker increased by 5.2 percent ($p = 0.082$) in 2004, 5.9 percent ($p = 0.114$) in 2005, and 7.3 percent ($p = 0.058$) in 2006.

To calculate a pass-through elasticity, I pair these estimates with earnings effects from the "stayers" comparison in Figure 8, since this group corresponds to workers who remained at treated firms and thus may plausibly benefit from increases in value added per worker. Figure 9 reproduces these estimates for comparison. As discussed in Section 4, the positive earnings effects I find on job stayers could be driven by bonus inducing selection on who remains at the firm. Reassuringly, the dynamics of these earnings effects precisely track the effects of bonus on log value-added per worker. That positive effects on value-added per worker only appear two years after policy implementation is consistent with productivity gains only appearing several years after new equipment investments begin to take place.

With access to firm-level data, pass-through elasticities for a given year can be recovered by estimating Equation 5 while instrumenting $\ln R_{it}$ with firm exposure to bonus. This yields a two-stage least squared estimate equivalent to the effect of bonus on log worker earnings divided by the policy's effect on log value added per worker. In 2004, my estimates imply a passthrough elasticity of $\varepsilon^{w,R} \equiv \frac{0.029}{0.0529} = 0.57$. For 2005, my estimates imply a larger elasticity of 0.70. As noted in the previous section, the primary threat to identification of passthrough elasticities in my setting is that market-level shocks will confound estimates of firm-level quasi-rent passthrough. the relative size of my earnings effects, with and without controls for market exposure, provides one such bound on the bias in these passthrough elasticities. Assuming the true earnings effect for stayers is 82 percent of the estimated effect in Figure 9 implies passthrough elasticities of 0.45 and 0.57 for 2004 and 2005, respectively. In any case, I regard these estimates as upper bounds on the true passthrough elasticities, which provides evidence that the earnings effects I document are of plausible magnitude in light of the benefits of bonus in the form of increased value-added per worker in exposed industries.[24]

5 Conclusion

This paper provides evidence that tax incentives for investment can significantly increase worker earnings. The policy I study, bonus depreciation, improved worker outcomes through firm-level exposure and by stimulating market-level labor demand.

My results point to important interactions between the implementation of bonus depreciation

[24] My passthrough estimates are relatively high compared to the limited number of existing estimates in the literature. Kline et al. (2019) report a value-added elasticity of 0.47 from patent-induced rents at small, innovative firms. My estimated passthrough elasticity for 2004 is similar to an implied elasticity of 0.58 from Van Reenen (1996).

in 2001 and structural trends in U.S. manufacturing. In the face of significant declines in aggregate employment, bonus was especially valuable as a buffer against job destruction. Workers in industries threatened by import competition saw particularly large benefits in the form of higher earnings, although effects were more muted in industries where labor-replacing technology was more prevalent. These patterns suggest future work should carefully consider how structural and transitory trends may influence the ultimate effects of investment incentives on workers.

In light of prior studies that fail to find effects of investment incentives on worker earnings, I present evidence at the firm, industry and local level that helps reconcile my worker-level results with these patterns. I also find null or negative earnings effects at the firm and industry level while also showing that increased hiring due to the labor demand-stimulating effects of bonus can drive down aggregate worker earnings estimates. This result emphasizes the particular value of worker-level data for detecting wage and earnings effects in the presence of labor demand shocks.

Lastly, prior studies on investment incentives have also struggled to document clear effects on productivity. I instead find positive and significant effects on value-added per worker at the industry level. I utilize these effects to estimate earnings passthrough elasticities that serve as a useful benchmark for quantifying the benefits of bonus to workers. This result provides novel evidence on how investments can stimulate productivity gains that pass through to worker earnings and shows that the benefits of bonus depreciation in 2001 went beyond preserving jobs during manufacturing decline.

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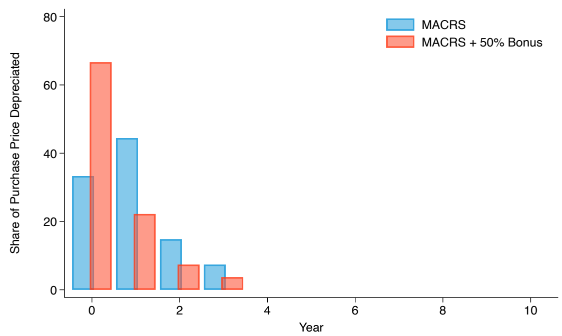
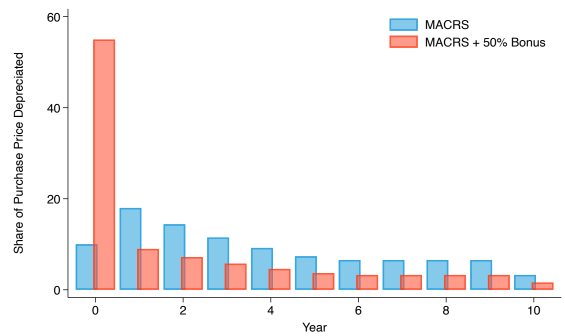
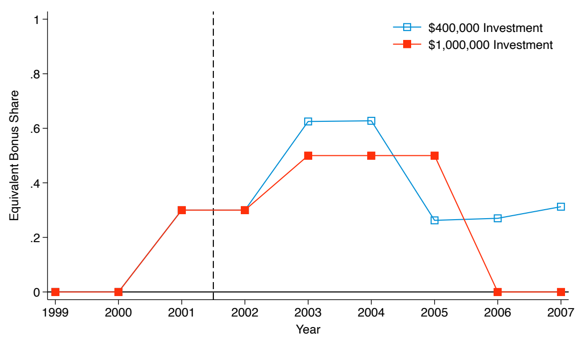
Figure 1: **Bonus Depreciation Policy Description**



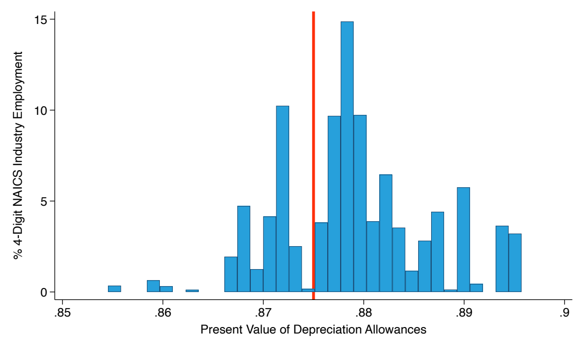
A. 3-year Depreciation Schedules



B. 10-year Depreciation Schedules



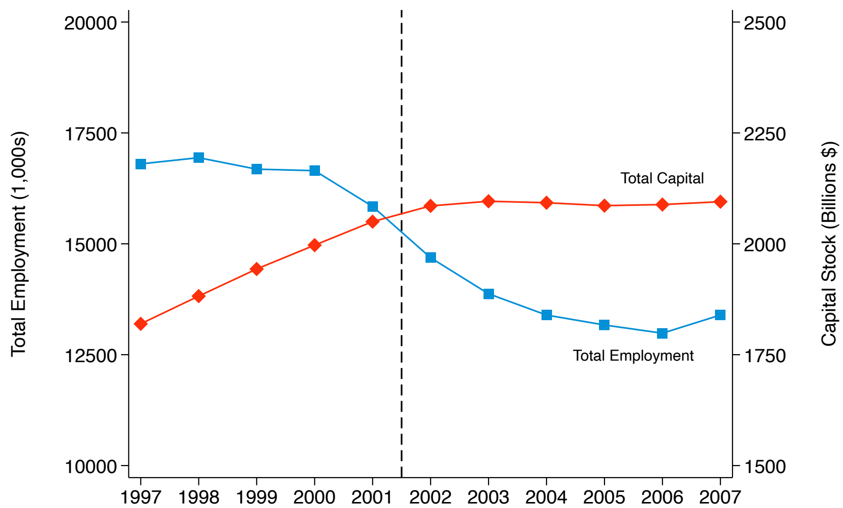
C. Timing of Accelerated Depreciation Policies



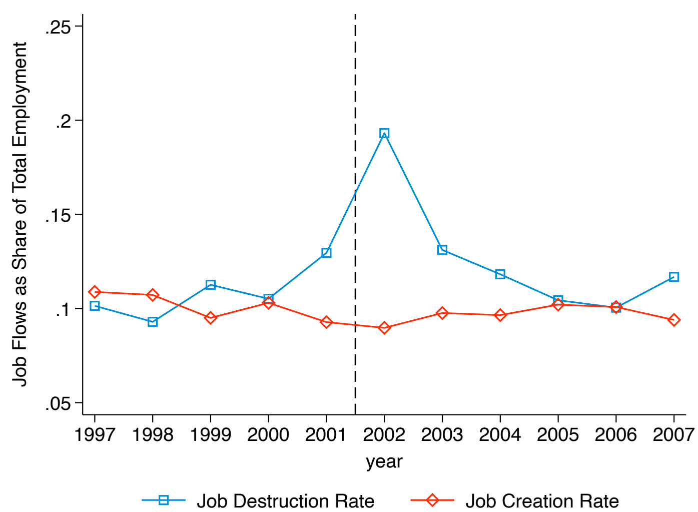
D. Distribution of NPV by Industry

Note: Author's calculations using data from Internal Revenue Service Publication 946 (2019). Panels A and B 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 C shows the level of Bonus depreciation deductions allowed for qualified equipment at the end of each year from 1999 to 2007. Panel D shows the distribution of NPV of depreciation deductions across manufacturing industries.

Figure 2: **Aggregate Trends in U.S. Manufacturing: 1997–2007**



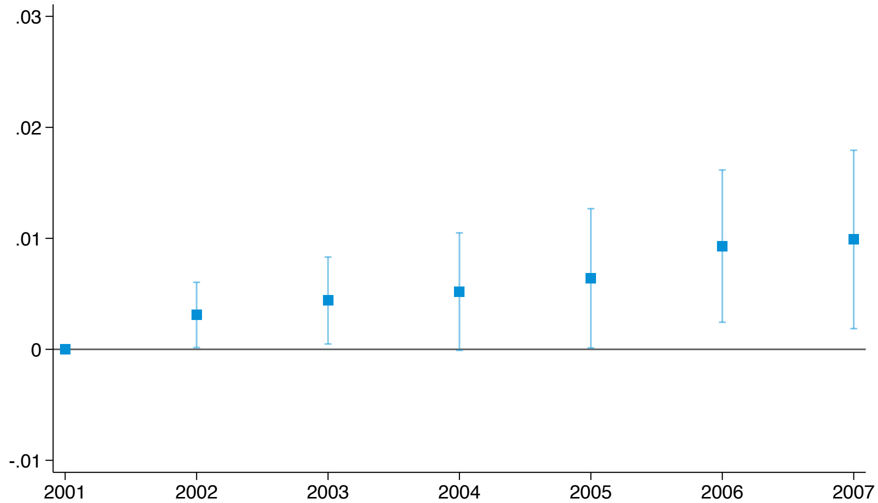
A: Aggregate Employment and Capital Stock



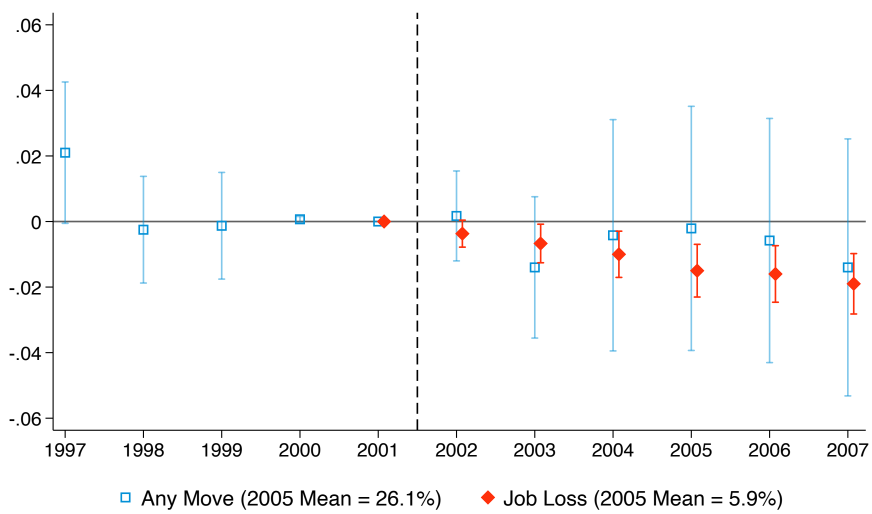
B: Job Creation and Destruction Rates

Note: Figure 2 key trends in the U.S. manufacturing sector over the sample period. Panel A plots time-series of total employment and total value of capital stock, respectively. Capital stock is in 2012 dollars using investment deflators provided by the Federal Reserve Bank. Panel B shows job creation and job destruction rates over the sample period. Job creation is defined by the number of jobs created, either via new plant openings or net increases in plant-level jobs year-over-year, divided by total employment. Job destruction is defined by the number of jobs destroyed, either via plant closures or net decreases in plant-level jobs year-over-year, divided by total employment.
Source: NBER-CES Manufacturing Industry Database and Business Dynamics Statistics (BDS).

Figure 3: **Effects of Bonus Depreciation on Employment Outcomes**



A: Employment Probability



B: Cumulative Probability of Job Separations After 2001

Note: Figure 3 describes the effect of bonus depreciation on workers' employment and job loss outcomes. Panel A presents estimates of workers' probability of reporting positive earnings in the years following 2001. Panel B presents estimates of workers' cumulative probability of experiencing job loss, defined as a separation from a firm that involves at least one quarter of zero earnings, from a "highly-attached" sample of workers who always report positive earnings in the years 2001–2007. Both sets of estimates are derived from linear probability models akin to Equation 1 with person fixed effects, schooling level-by-year fixed effects, and controls for salient driver of manufacturing transformation described in Section 2. 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.

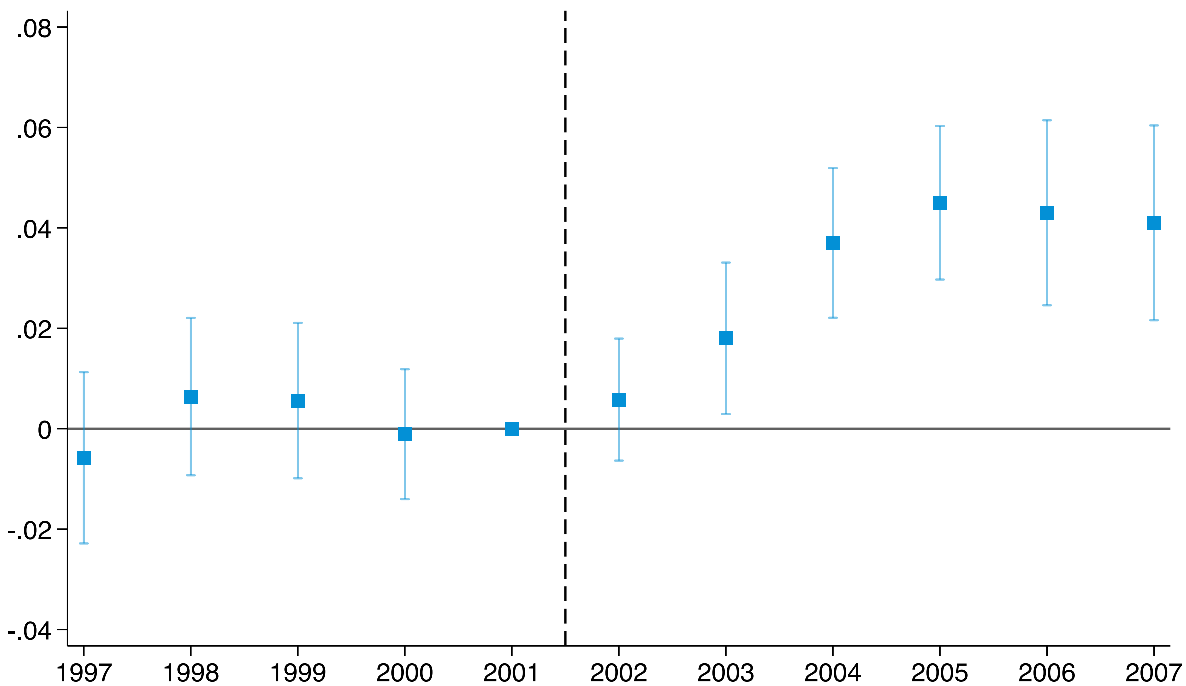
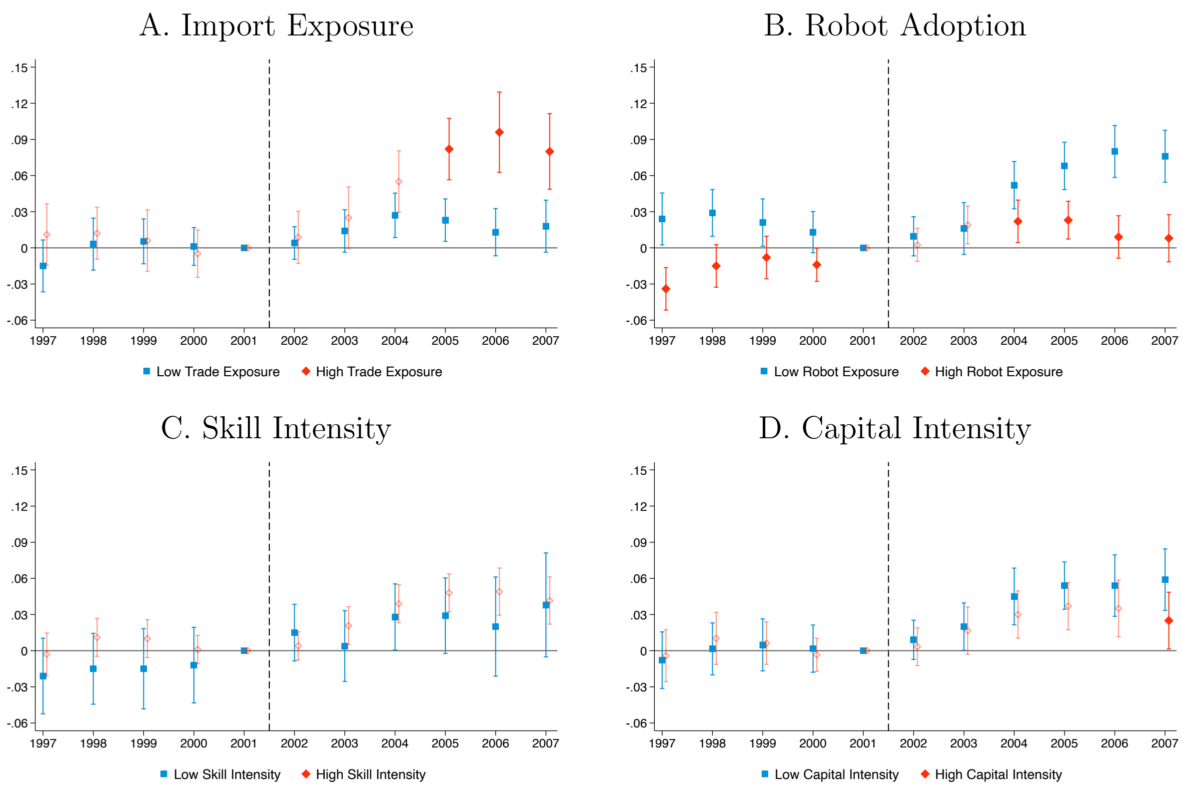


Figure 4: Effects of Bonus Depreciation on Worker Earnings

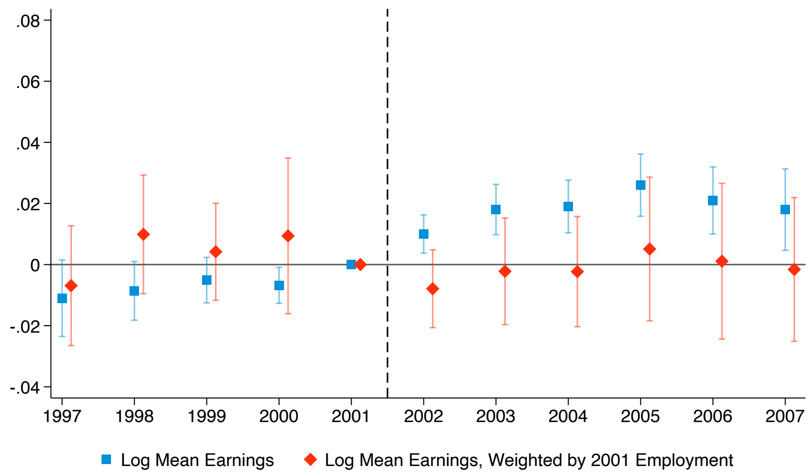
Note: Figure 4 describes the effect of bonus depreciation on the earnings of workers at exposed firms in 2001 with 95% confidence intervals from standard errors clustered at the 4-digit NAICS level. Coefficients follow from estimation of Equation 1 with log total earnings as the outcome variable and include person fixed effects, schooling level-by-year fixed effects, and the controls for salient driver of manufacturing transformation described in Section 2. *Source:* Authors' calculations based on the LEHD, Decennial Census, and Zwick and Mahon (2017) data.

Figure 5: **Effects of Bonus Depreciation on Log Earnings: Industry Heterogeneity**

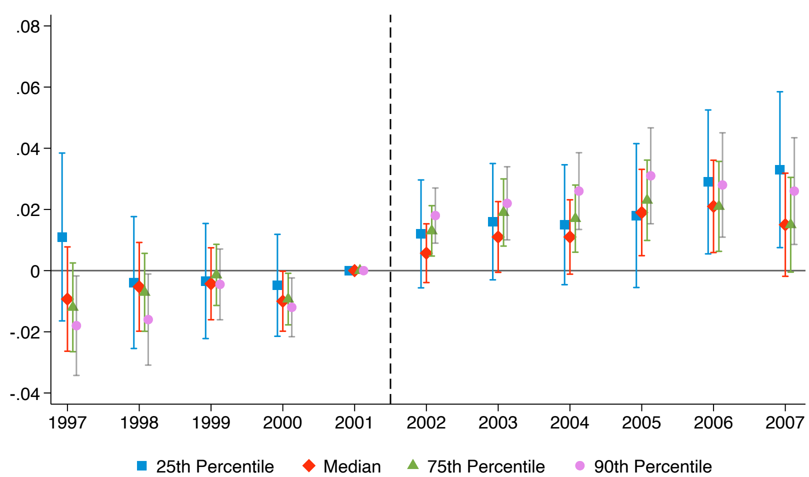


Note: Figure 5 describes the effect of bonus depreciation on log earnings across four dimensions of heterogeneity. Dark shading on red lines denotes whether coefficients are significantly different from the corresponding blue line coefficient at the 5% level. Error bars represent 95% confidence intervals 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 Firm-Level Outcomes**



A. Log Mean Earnings

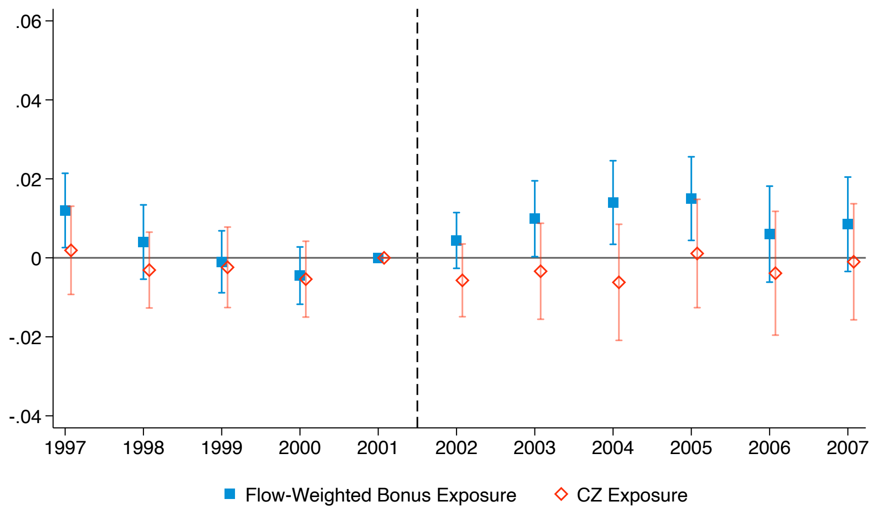


B. Log Earnings Percentiles

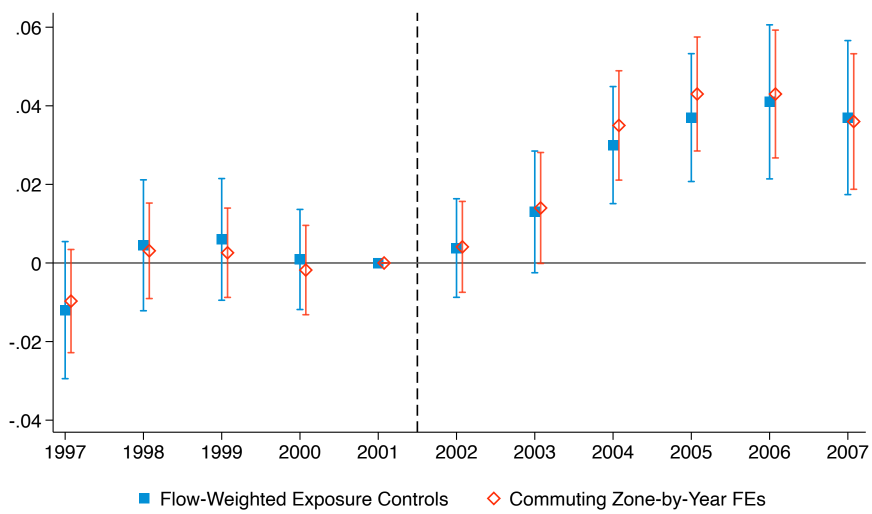
Note: Figure 6 describes the effect of bonus depreciation on employment and earnings outcomes at the state-firm level using LEHD data. Each regression includes firm and flexible controls for the salient drivers of sectoral transformation, as defined above. Panel A shows effects on the log mean earnings of workers that were at the firm in 2001. Panel B shows effects on percentiles of the within-firm earnings distribution. Error bars represent 95% confidence intervals from standard errors clustered at the 4-digit NAICS level. *Source:* Authors' calculations based on the LEHD and Zwick and Mahon (2017) data.

Figure 7: **Effects of Local and Firm Exposure to Bonus Depreciation on Log Earnings**

A. Local Exposure

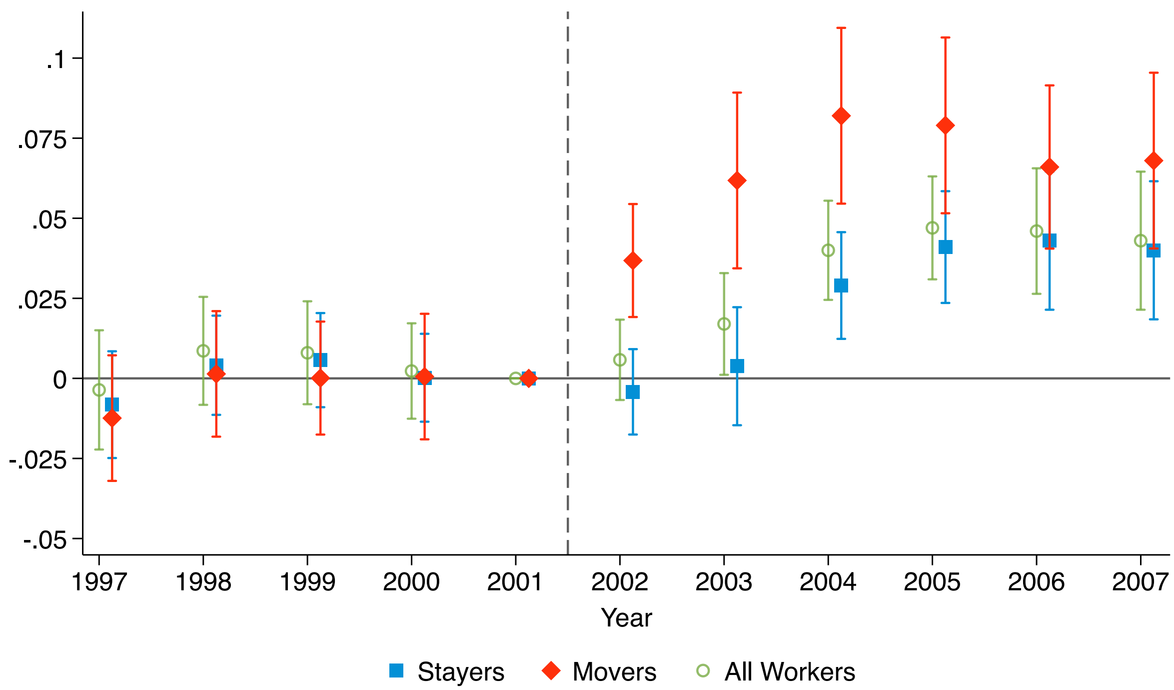


B. Firm Exposure



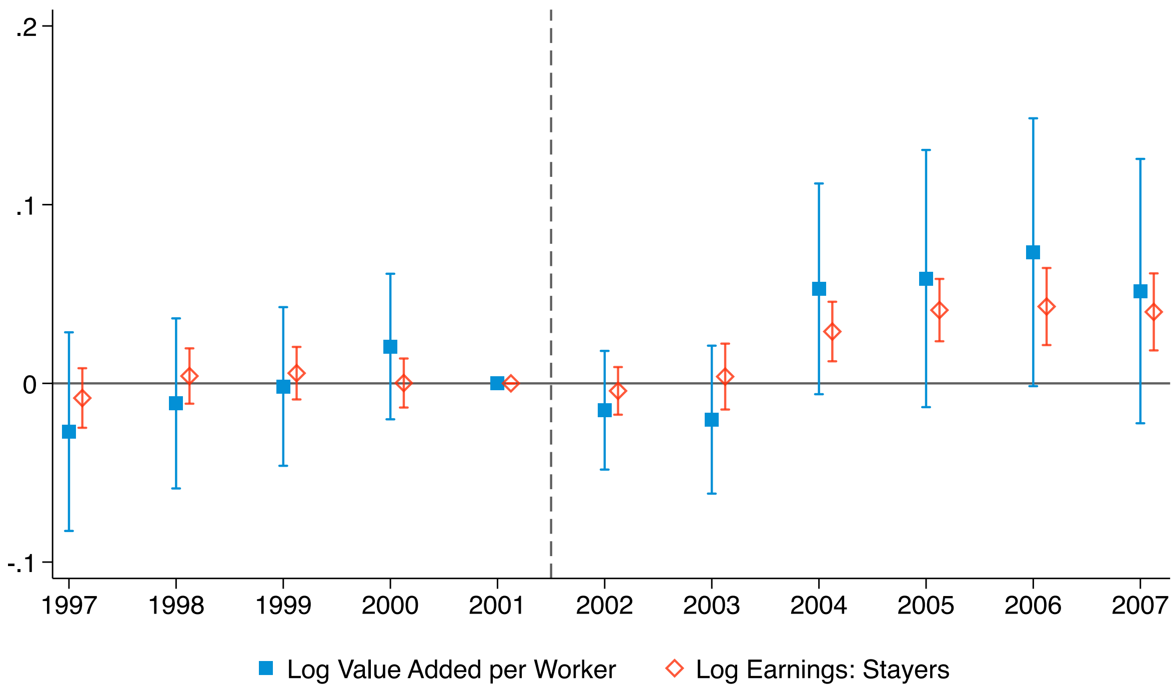
Note: Figure 7 describes the effects of local and firm-level exposure to bonus depreciation on worker earnings. Panel A shows the effects of local exposure using the flow-weighted exposure measure described in Section 3.2 and a simple measure based on the share 2001 commuting zone-level employment in treated industries. Panel B shows the effect of firm-level policy exposure while also controlling for the flow-weighted local exposure measure. 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, NBER-CES manufacturing dataset, and Zwick and Mahon (2017) data.

Figure 8: **Effects of Bonus Depreciation on Log Earnings: Stayers vs. Movers**



Note: Figure 8 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 red line shows estimates derived from comparing workers who did experience a job separation over the same time period ("movers v. movers"). The green lean is reproduced from Figure 4 for comparison. 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 9: **Effects of Bonus Depreciation on Industry-Level Value-Added**



Note: Figure 9 describes the effect of bonus depreciation on log value added per worker in treated 6-digit NAICS industries. I use these estimates to benchmark the "stayers" earnings effects against pass-through estimates in the literature. Industry value-added regressions are 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. The "Log Earnings: Stayers" estimates are reproduced from Figure 8. 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, NBER-CES manufacturing dataset and Zwick and Mahon (2017) data.

Table 1: **Manufacturing Worker Characteristics, 2001**

	Treat	Control
Panel A: LEHD Mean Earnings		
Primary Sample (330,400 workers)	$47,200	$57,400
Relaxed Restrictions (477,300 workers)	$43,300	$52,500
Panel B: Decennial Census Worker Characteristics		
Male	0.77	0.68
Black	0.09	0.07
Hispanic	0.10	0.07
Under 35	0.32	0.32
Some or More College	0.34	0.52
Production Occupation	0.67	0.48
Mean Income	$42,241	$49,843
Panel C: NBER-CES Employment Statistics		
Production Occupation	0.77	0.68
Mean Earnings	$35,728	$38,421

Note: Table 1 presents summary statistics for worker characteristics across treated and control industries and for different data sources. Panel A reports sumamry statistics for my LEHD sample. Panel B presents worker demographics from the 2000 Decennial Census. Panel C presents additional information from the NBER-CES Manufacturing Database.
Source: Authors' calculations based on the LEHD, Public-Use Decennial Census, and Zwick and Mahon (2017) data.

Table 2: **Effects of Bonus on Log Earnings**

	(1)	(2)	(3)	(4)	(5)	(6)
Bonus × Year_{2005}	0.045	0.043	0.046	0.057	0.046	0.04
	(0.0078)	(0.0074)	(0.0078)	(0.0092)	(0.0076)	(0.0082)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Bonus × Year_{2007}	0.041	0.036	0.041	0.036	0.025	0.025
	(0.0099)	(0.0088)	(0.0099)	(0.011)	(0.0085)	(0.01)
	[0.000]	[0.000]	[0.000]	[0.001]	[0.003]	[0.012]
Worker FE	✓	✓	✓	✓	✓	✓
Production×Schooling×Year FE	✓	✓	✓	✓	✓	✓
Sector Shock FE	✓	✓	✓	✓	✓	✓
Commuting Zone×Year FE		✓				
Occupation×Year FE			✓			
Pierce-Schott Trade ×Year FE				✓		
6-Digit NAICS Sector Shocks					✓	
Includes Low-Attachment Workers						✓
Worker Count	309,800	309,800	309,800	309,800	309,800	477,300

Note: Table 2 presents long differences estimates of the effects of bonus on annual worker earnings for the years 2005 and 2007. 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 3: **Effects of Bonus on Log Earnings: Worker Heterogeneity**

	(1) Age> 35	(2) College	(3) Production	(4) Female	(5) Black
Panel A: Effects on Log Earnings					
Bonus	0.027	0.047	0.034	0.037	0.045
	(0.012)	(0.011)	(0.011)	(0.011)	(0.01)
	[0.024]	[0.000]	[0.002]	[0.001]	[0.000]
Bonus × Interaction	0.023	-0.0019	0.0081	0.00023	0.034
	(0.011)	(0.011)	(0.0045)	(0.006)	(0.02)
	[0.037]	[0.863]	[0.072]	[0.969]	[0.089]
Worker Count	309,800	309,800	309,800	309,800	309,800
Panel B: Effects on Employment					
Bonus	0.015	0.013	0.0092	0.0071	0.01
	(0.0042)	(0.0044)	(0.0041)	(0.004)	(0.004)
	[0.000]	[0.003]	[0.025]	[0.076]	[0.012]
Bonus × Interaction	-0.0064	-0.0065	0.0022	0.0072	-0.016
	(0.0037)	(0.0032)	(0.0024)	(0.0035)	(0.0082)
	[0.084]	[0.042]	[0.359]	[0.040]	[0.051]
Worker Count	427,800	427,800	427,800	427,800	427,800
Worker FE	✓	✓	✓	✓	✓
Sector Shock FE	✓	✓	✓	✓	✓
Schooling × Year FE	✓	✓	✓	✓	✓

Note: Table 3 presents the heterogeneous effects of bonus depreciation on annual worker earnings and employment probability by worker demographic characteristics. In each panel, coefficients correspond to the 2007 long difference effect, except for columns (3) and (4) in panel A, which show difference-in-differences effects for the full post-period. Column (1) presents the effect for workers under the age of 35 in 2001 in the first row and the relative effect for those over the age of 35 the second row. Column (2) shows the effect for workers without a college education in 2000 and the relative effect for those with a college degree. Column (3) shows the effect for workers in non-production occupations in 2000 and the relative effect for those in production occupations, as defined by Acemoglu and Autor (2011). Column (4) and (5) show effects for men and non-black workers, respectively, and the relative effects for female and black workers. 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: **Local vs. Firm Level Exposure to Bonus Depreciation: Worker-Level Sample**

	$\tilde{E}_{c,k,2001} < \tilde{E}_{p(50)}$	$\tilde{E}_{c,k,2001} > \tilde{E}_{p(50)}$
Commuting Zone Exposure	0.242	0.292
Flow-Weighted Local Exposure	0.015	0.467
Share of Workers at Treated Firms	0.006	0.559

Note: Table 4 presents summary statistics describing alternative measures of policy exposure. Columns split my sample of 309,800 workers into those with above and below median local policy exposure based on the flow-weighted measure described in Section 3.2. "Flow-Weighted Local Exposure" shows the average value of this measure in each group, and demonstrates that workers in the below median group have very low local exposure to the policy. "Commuting Zone Exposure" corresponds to the share of total commuting zone exposure that is in treated industries, which is relatively similar across both groups. The share of workers with firm-level policy exposure but below-median local exposure is only 0.6 percent of my estimating sample, with the remaining workers with firm-level exposure falling into the above-median exposure group.
Source: Authors' calculations based on the LEHD, Decennial Census, and Zwick and Mahon (2017) data.

Table 5: **Local and Firm-Level Effects of Bonus on Job Mobility**

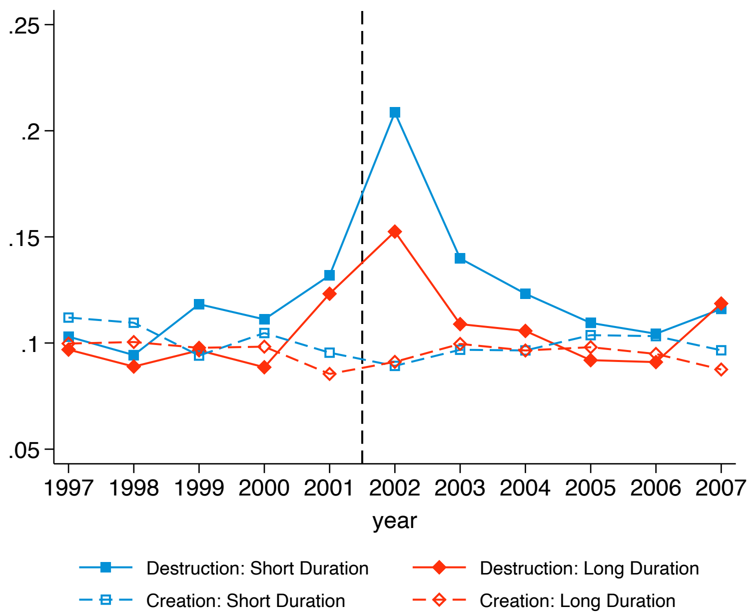
	(1) Any Move	(2) Job Loss	(3) Other Moves	(4) Long Move
Panel A: Local Labor Market Effects				
Bonus $\times$ Year_{2005}	-0.016	-0.0096	-0.012	0.0071
	(0.0091)	(0.0028)	(0.0077)	(0.0036)
	[0.079]	[0.001]	[0.119]	[0.049]
Bonus $\times$ Year_{2007}	-0.024	-0.011	-0.021	0.0082
	(0.01)	(0.0033)	(0.0089)	(0.004)
	[0.016]	[0.001]	[0.018]	[0.040]
Panel B: Firm-Level Effects				
Bonus $\times$ Year_{2005}	0.01	-0.011	0.015	0.072
	(0.018)	(0.0039)	(0.018)	(0.016)
	[0.579]	[0.005]	[0.405]	[0.000]
Bonus $\times$ Year_{2007}	0.0025	-0.014	0.0086	0.081
	(0.019)	(0.0046)	(0.019)	(0.016)
	[0.895]	[0.002]	[0.651]	[0.000]
2005 Means	0.261	0.0592	0.226	0.0632
Worker Count	309,800	309,800	309,800	309,800

Note: Table 5 presents long differences estimates of the effects of bonus on worker mobility outcomes for the years 2005 and 2007 and for both local and firm-level policy exposure. 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.

Online Appendix

A Additional Exhibits

Figure A.1: **Bonus Depreciation and Aggregate Job Creation and Destruction Rates**



Note: Figure A.1 presents aggregate job creation and destruction rates separately based on industry exposure to bonus depreciation. "Long Duration" corresponds to employment in industries with $z_0 <$ 0.875, as treatment is defined throughout the paper.
Source: e Authors' calculations based on the U.S. Census Business Dynamics Statistics Database and Zwick and Mahon (2017) data.

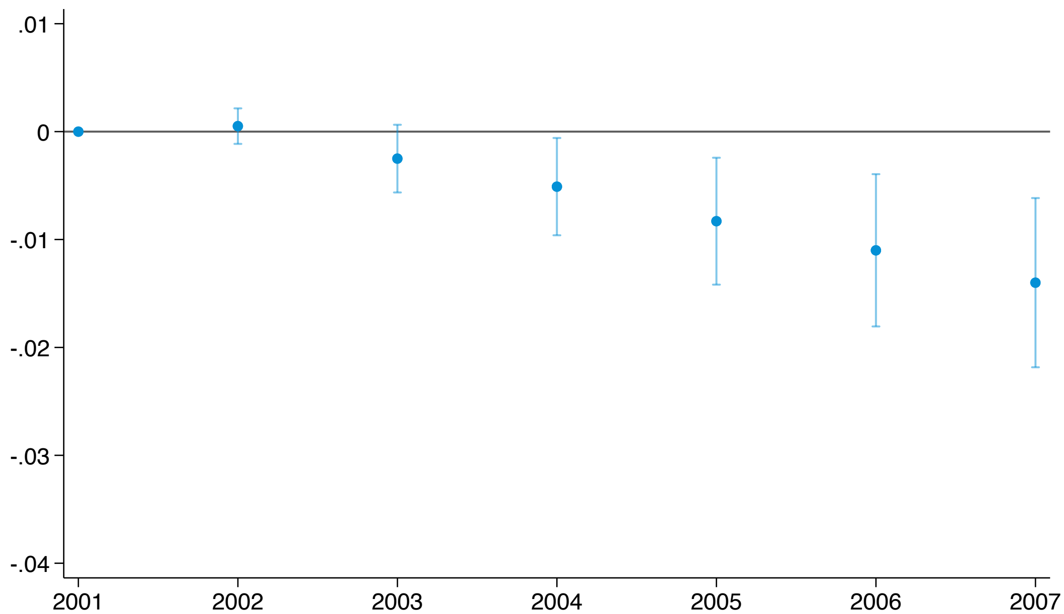
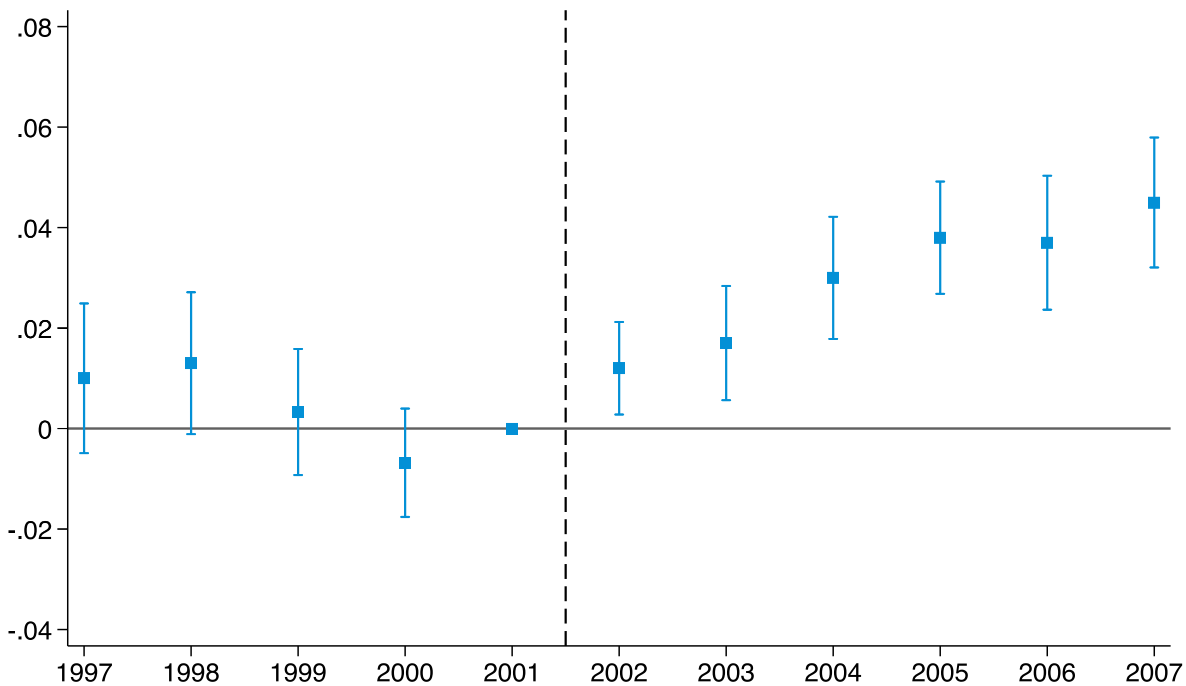


Figure A.2: Effects of Bonus Depreciation on Probability of State Migration

Note: Figure A.2 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.

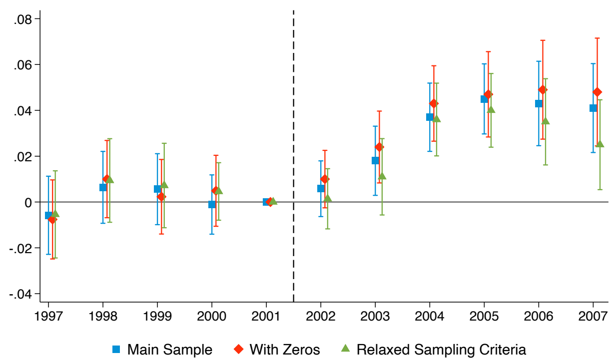
Source: Authors' calculations based on the LEHD, Decennial Census, and Zwick and Mahon (2017) data.

Figure A.3: **Effects of Bonus Depreciation on Log Earnings: Continuous Treatment**

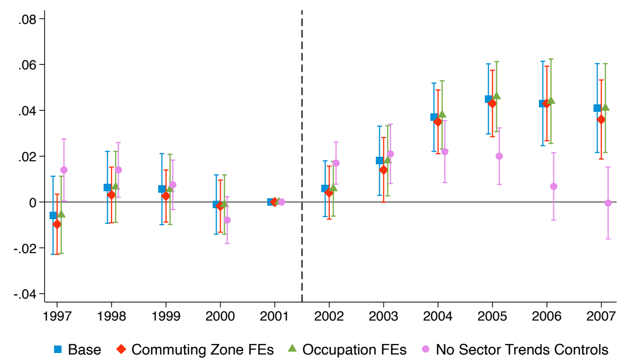


Note: Figure A.3 describes the effect of bonus depreciation on log earnings of workers using the continuous z_0 measure of Zwick and Mahon (2017) as the treatment variable. Error bars represent 95% confidence intervals from standard errors clustered at the 4-digit NAICS level.
Source: Authors' calculations based on the LEHD, Decennial Census, and Zwick and Mahon (2017) data.

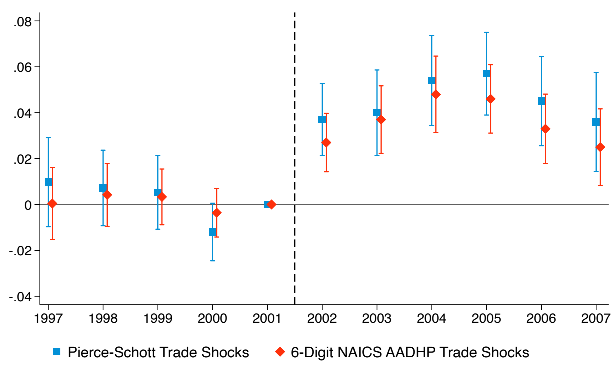
Figure A.4: **Effects of Bonus Depreciation on Log Earnings: Robustness**



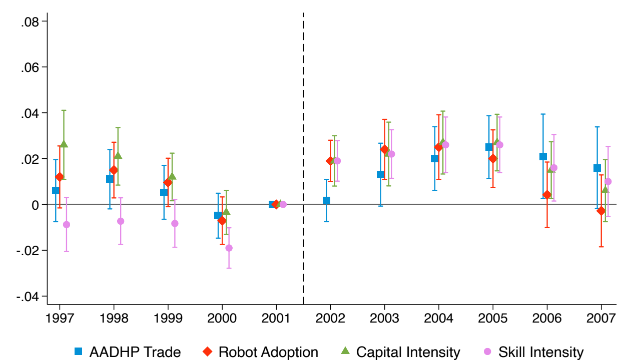
A. Alternative Samples



B. Location and Occupation Controls



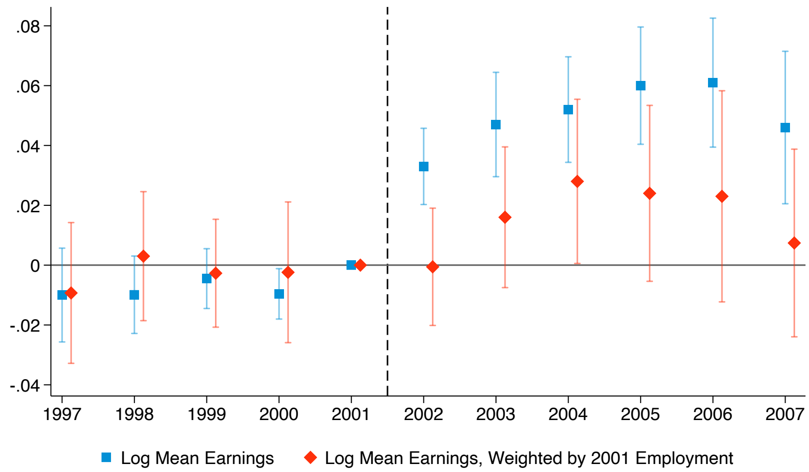
C. Alternative Trade Controls



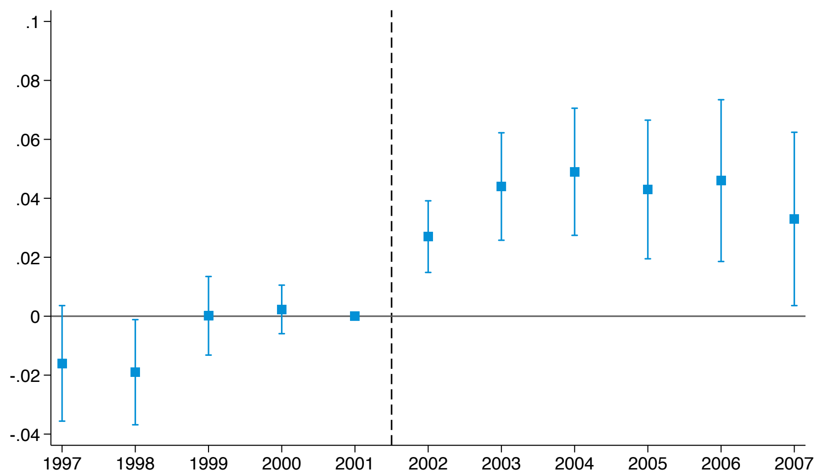
D. Individual Sector Trend Controls

Note: Figure A.4 describes the effect of bonus depreciation on the log earnings of workers for different alternative specifications. 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 A.5: **Effects of Bonus Depreciation on Firm-Level Outcomes**



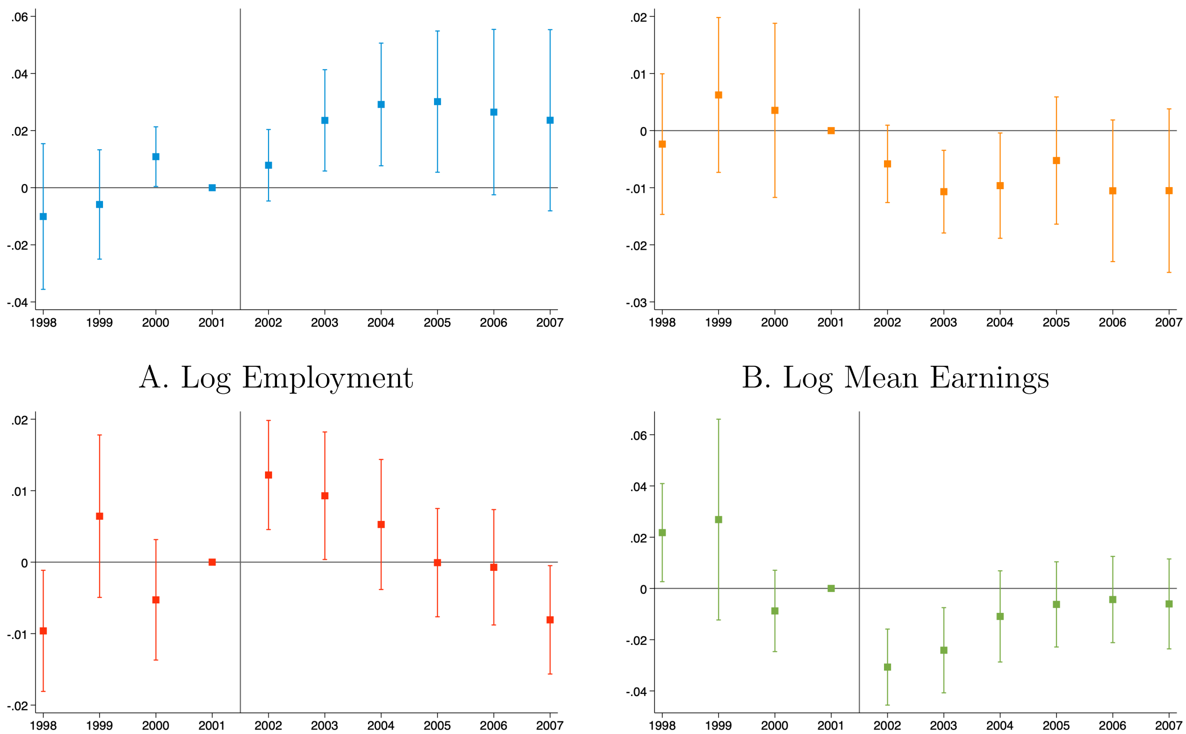
A. Log Earnings: 2001 Worker Cohorts



B. Log Employment

Note: Figure 6 describes the effect of bonus depreciation on employment and earnings outcomes at the state-firm level using LEHD data. Each regression includes firm and flexible controls for the salient drivers of sectoral transformation, as defined above. Panel A shows effects on the log mean earnings of workers that were at the firm in 2001. Panel B shows effects on log total employment. Error bars represent 95% confidence intervals from standard errors clustered at the 4-digit NAICS level. *Source:* Authors' calculations based on the LEHD and Zwick and Mahon (2017) data.

Figure A.6: **Effects of Bonus Depreciation on State-Industry Level Outcomes**



A. Log Employment

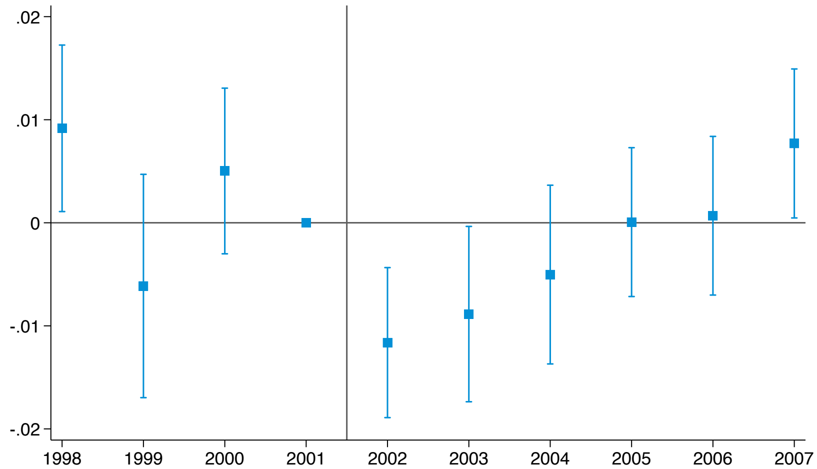
B. Log Mean Earnings

C. Hire Rate

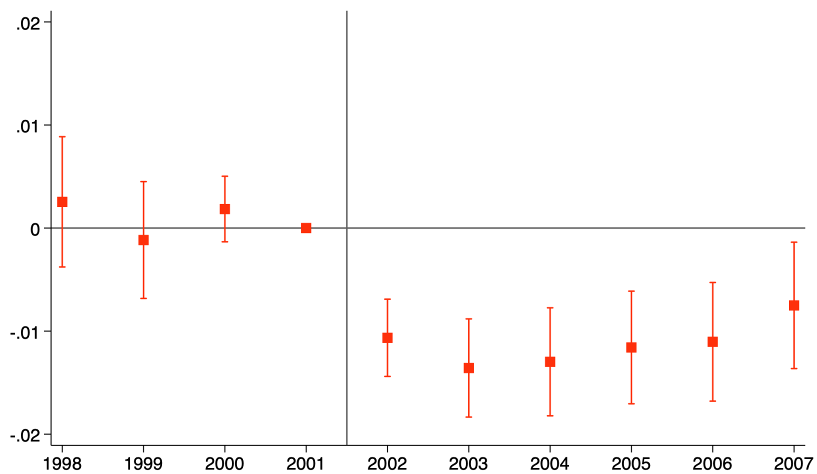
D. Log Mean Earnings: New Hires

Note: Figure A.6 describes the effect of bonus depreciation on employment and earnings outcomes at the 4-digit NAICS industry-by-State level. Each regression is weighted by the 2001 employment level of each 4-digit NAICS industry-by-state unit and includes industry-state 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 U.S. Census Quarterly Workforce Indicators (QWI) database and Zwick and Mahon (2017) data.

Figure A.7: **Predicted Effects of Bonus Depreciation on State-Industry Log Mean Earnings**



A. Predicted Earnings: Hire Rate



B. Predicted Earnings: Hire Rate + Worker Demographics

Note: Figure A.7 describes the effect of bonus depreciation on predicted log mean earnings at the 4-digit NAICS industry-by-state level. Panel A shows predicted log mean earnings based only on the hire rate, defined as total annual stable hires over average quarterly employment. Panel B repeats this exercise while also including in the prediction model the share of workers below the age of 25, the share of workers without a college education, the share of nonwhite workers, and the share of women in the workforce. Each regression is weighted by the 2001 employment level of each 4-digit NAICS industry-by-state unit and includes industry-state 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 U.S. Census Quarterly Workforce Indicators (QWI) database and Zwick and Mahon (2017) data.

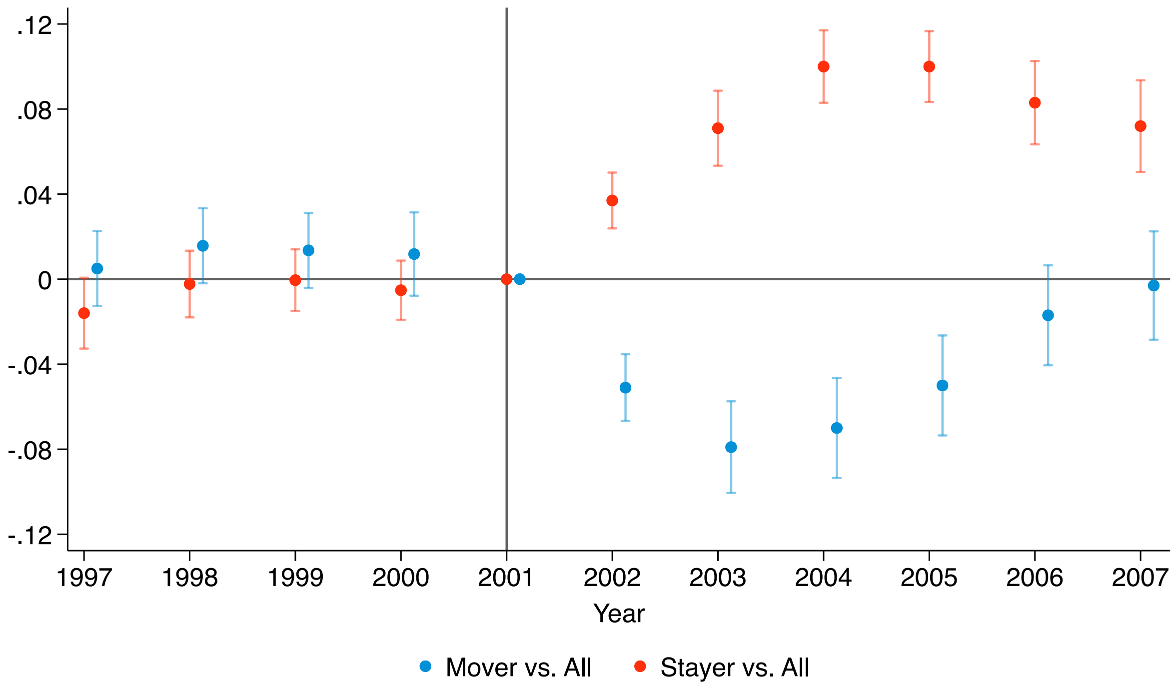
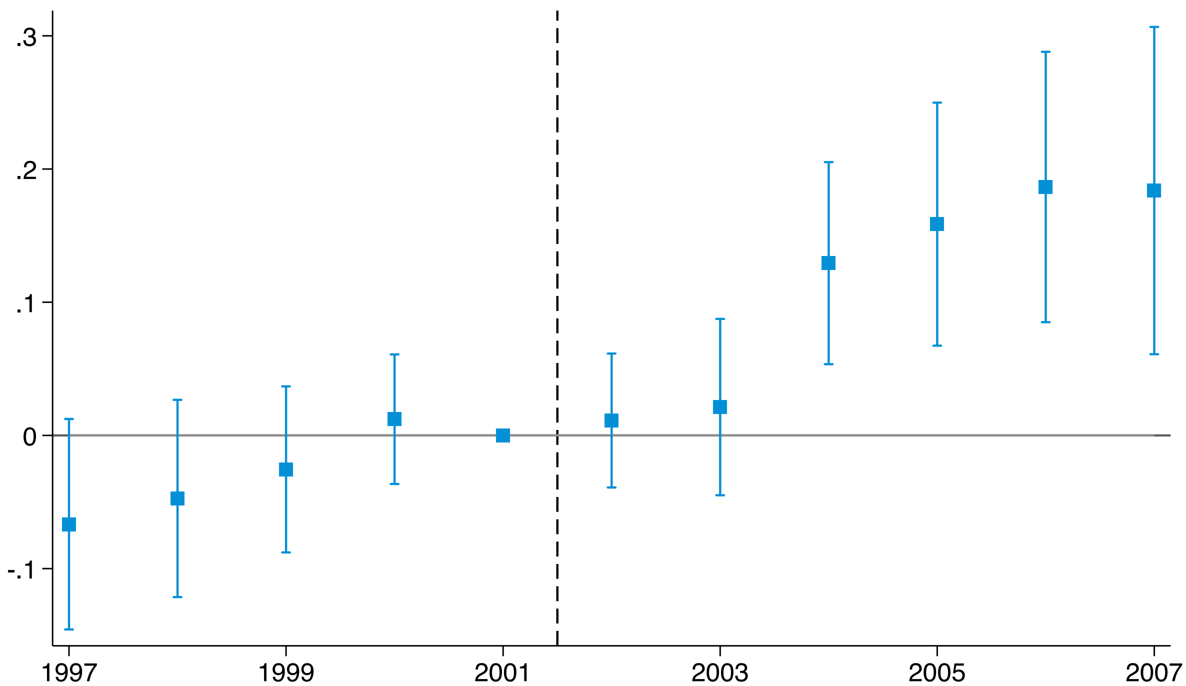


Figure A.8: Effects of Bonus Depreciation on Log Earnings: Alternative Job Mobility Comparisons

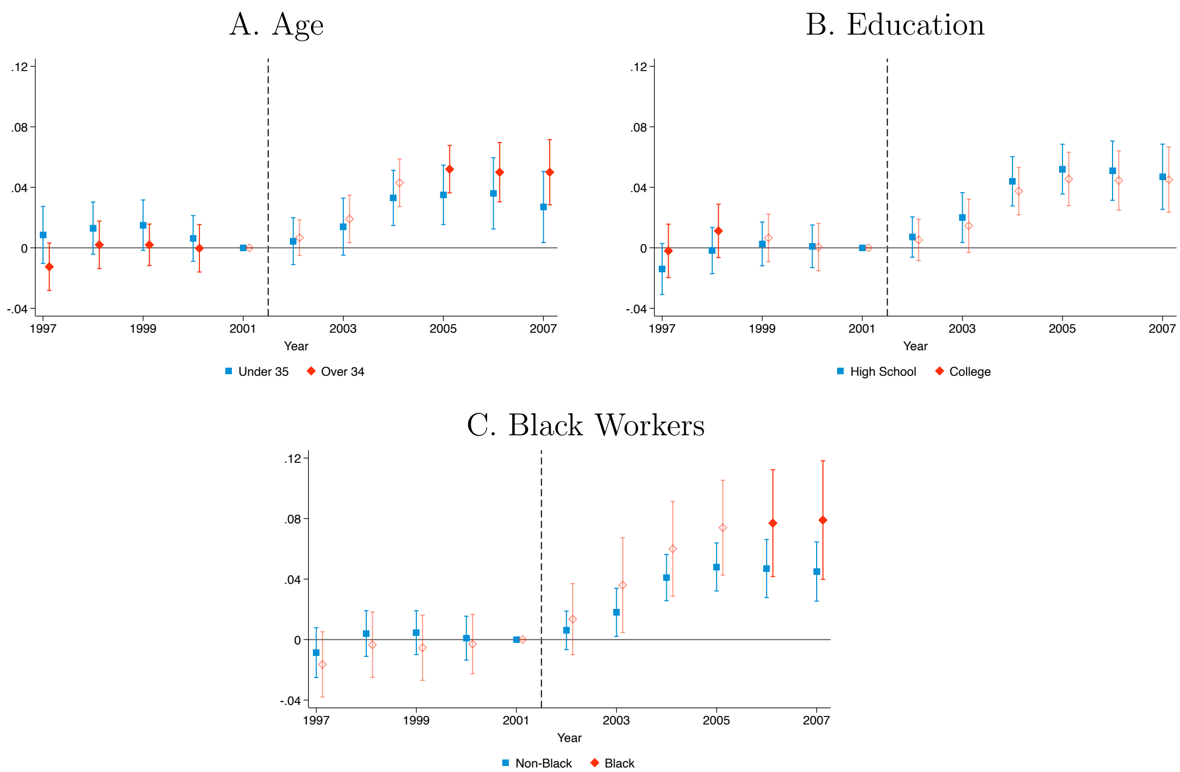
Note: Figure A.8 describes the effect of bonus depreciation on log earnings for worker depending on whether they separated from their 2001 firm by 2005. The blue line estimates the effect of bonus on worker earnings by comparing treated workers who do not change jobs by 2005 to all untreated workers ("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 A.9: **Effects of Bonus Depreciation on Industry-Level Log Value-Added**



Note: Figure A.9 describes the effect of bonus depreciation on log value added in treated 6-digit NAICS industries. 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 A.10: **Effects of Bonus Depreciation on Log Earnings: Heterogeneity by Worker Characteristics**



Note: Figure A.10 describes the heterogeneous effect of bonus depreciation on log earnings of workers by age, education level, and race, 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.

Table A.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]
Worker Count	309,800	309,800	309,800	309,800	309,800
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]
Worker Count	427,800	427,800	427,800	427,800	427,800
Worker 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 A.2 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. 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 A.2: **Effects of Bonus Depreciation on Log Earnings**

	(1) All Workers	(2) Highly Attached	(3) Stayers	(4) Movers	(5) Local Controls
Bonus × Year_{1997}	-0.0076	-0.0058	-0.0082	-0.0124	-0.014
	(0.0088)	(0.0087)	(0.0085)	(0.0067)	(0.0081)
	[0.3878]	[0.5050]	[0.3347]	[0.0642]	[0.0839]
Bonus × Year_{1998}	0.01	0.0064	0.0041	0.0014	0.0028
	(0.0086)	(0.008)	(0.0079)	(0.0076)	(0.0074)
	[0.2449]	[0.4237]	[0.6038]	[0.8538]	[0.7051]
Bonus × Year_{1999}	0.0023	0.0056	0.0057	0.0001	0.0046
	(0.0083)	(0.0079)	(0.0075)	(0.0063)	(0.0068)
	[0.7817]	[0.4784]	[0.4473]	[0.9873]	[0.4987]
Bonus × Year_{2000}	0.0049	-0.0011	0.0002	0.0006	-0.00053
	(0.0079)	(0.0066)	(0.007)	(0.0065)	(0.0062)
	[0.5351]	[0.8676]	[0.9772]	[0.9301]	[0.9319]
Bonus × Year_{2002}	0.01	0.0058	-0.0042	0.0368	0.004
	(0.0064)	(0.0062)	(0.0068)	(0.008)	(0.0064)
	[0.1182]	[0.3495]	[0.5368]	[0.0000]	[0.5320]
Bonus × Year_{2003}	0.024	0.018	0.0038	0.0618	0.014
	(0.008)	(0.0077)	(0.0094)	(0.014)	(0.0077)
	[0.0027]	[0.0194]	[0.6860]	[0.0000]	[0.0690]
Bonus × Year_{2004}	0.043	0.037	0.029	0.082	0.031
	(0.0084)	(0.0076)	(0.0085)	(0.013)	(0.0072)
	[0.0000]	[0.0000]	[0.0006]	[0.0000]	[0.0000]
Bonus × Year_{2005}	0.047	0.045	0.041	0.079	0.037
	(0.0095)	(0.0078)	(0.0089)	(0.012)	(0.008)
	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]
Bonus × Year_{2006}	0.049	0.043	0.043	0.066	0.041
	(0.011)	(0.0094)	(0.011)	(0.011)	(0.0099)
	[0.0000]	[0.0000]	[0.0001]	[0.0000]	[0.0000]
Bonus × Year_{2007}	0.048	0.041	0.04	0.068	0.037
	(0.012)	(0.0099)	(0.011)	(0.01)	(0.01)
	[0.0001]	[0.0000]	[0.0003]	[0.0000]	[0.0002]
Worker Count	427,800	309,800	309,800	309,800	309,800
Worker FE	✓	✓	✓	✓	✓
Production×Schooling×Year FE	✓	✓	✓	✓	✓
Sector Shock FE	✓	✓	✓	✓	✓

Note: Table A.2 presents event study point estimates for select specifications. Columns (1 and (2) correspond to the "with zeros" and "main saample" specifications in Panel A of Figure A.4, respectively. Columns (3) and (4) represent the "stayers" and "movers" specifications from Figure 8, respectively. Column (5) presents the effect of firm-level exposure in the presence of flow-weighted local policy exposure controls, as shown in Panel B of Figure 7. 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 A.3: **Effects of Bonus on State Migration: Worker Heterogeneity**

	(1) Older	(2) College	(3) Female	(4) Production	(5) Black
Bonus	-0.013	-0.017	-0.012	-0.013	-0.013
	(.0052)	(.0042)	(.0039)	(.0039)	(.0039)
	[0.012]	[0.000]	[0.002]	[0.001]	[0.001]
Bonus × Interaction	-0.0015	0.0081	-0.0058	-0.0043	-0.014
	(.0035)	(.0025)	(.0029)	(.0023)	(.0074)
	[0.668]	[0.001]	[0.046]	[0.062]	[0.059]
Worker Count	427,800	427,800	427,800	427,800	427,800
Worker FE	✓	✓	✓	✓	✓
Sector Shock FE	✓	✓	✓	✓	✓
Tenure × Year FE	✓	✓	✓	✓	✓
Schooling × Year FE	✓	✓	✓	✓	✓

Note: Table A.3 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.

Table A.4: **Effects of Bonus Depreciation on Worker Outcomes: 2011 Estimates**

	(1)	(2)	(3)	(4)	(5)	(6)
	Extensive Margins		Log Earnings			
BonusxYear_{2011}	-0.023	-0.019	0.037	0.035	0.03	0.01
	(0.0072)	(0.0057)	(0.018)	(0.016)	(0.016)	(0.016)
	[0.001]	[0.001]	[0.040]	[0.029]	[0.061]	[0.532]
Outcome	Pr(Emp)	State Move				
Worker FE	✓	✓	✓	✓	✓	✓
Prod. $\times$ Educ. $\times$ Year FE	✓	✓	✓	✓	✓	✓
CZ $\times$ Year FE				✓		
6-Digit NAICS Sector Shocks					✓	
Includes Low-Attachment Workers						✓
Worker Count	427,800	427,800	309,800	309,800	309,800	477,300

Note: Table A.4 presents long differences estimates of the effects of bonus on annual worker earnings for the year 2011. 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 A.5: **Heterogeneous Effects of Bonus by Worker Characteristics: 2011 Estimates**

	(1) Age	(2) College	(3) Production	(4) Female	(5) Black
Panel A: Effects on Log Earnings					
Bonus	0.017	0.044			0.043
	(0.019)	(0.019)			(0.019)
	[0.371]	[0.021]			[0.024]
Bonus × Interaction	0.03	-0.0027			0.00053
	(0.019)	(0.02)			(0.032)
	[0.114]	[0.893]			[0.987]
Worker Count	309,800	309,800			309,800
Panel B: Effects on Employment					
Bonus	0.027	0.027	0.019	0.018	0.023
	(0.0071)	(0.0077)	(0.0072)	(0.0071)	(0.0071)
	[0.000]	[0.000]	[0.008]	[0.011]	[0.001]
Bonus × Interaction	-0.0067	-0.011	0.01	0.015	-0.024
	(0.0059)	(0.0046)	(0.0035)	(0.005)	(0.012)
	[0.256]	[0.017]	[0.004]	[0.003]	[0.046]
Worker Count	427,800	427,800	427,800	427,800	427,800
Worker FE	✓	✓	✓	✓	✓
Sector Shock FE	✓	✓	✓	✓	✓
Schooling × Year FE	✓	✓	✓	✓	✓

Note: Table A.5 presents estimates of the heterogeneous effects of bonus on worker outcomes across the worker characteristics in the column titles for the year 2011. 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 A.6: **Heterogeneous Effects of Bonus by Sector Characteristics: 2011 Estimates**

	(1) Trade	(2) Skill	(3) Capital	(4) Robot
Bonus	0.018	0.020	0.059	0.097
	(0.021)	(0.029)	(0.020)	(0.019)
	[0.3914]	[0.4904]	[0.0032]	[0.0000]
Bonus × Interaction	0.052	0.021	-0.04	-0.120
	(0.032)	(0.028)	(0.027)	(0.021)
	[0.1042]	[0.4533]	[0.1385]	[0.0000]
Worker Count	309,800	309,800	309,800	309,800
Worker FE	✓	✓	✓	✓
Sector Shock FE	✓	✓	✓	✓
Prod. × Educ. × Year FE	✓	✓	✓	✓

Note: Table A.6 presents estimates of the heterogeneous effects of bonus on worker earnings across sector characteristics in the column titles for the year 2011. 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.

B Flow-Weighted Local Labor Market Exposure

I adapt from Arnold (2023) a simple framework that models the value of jobs across industries for a worker in a given industry using observed job-to-job transitions. Whereas Equation 3 assumes that all workers face the competitive outside wage, suppose instead that for a worker in industry m, this wage is a function of utility values $V(k|m)$ and employment levels L_k for each industry k in the same commuting zone. The term $V(k|m)$ captures the value of jobs in industry k for the worker in industry m. To the extent that employment levels contain information on the likelihood of a given worker receiving job offers in a given industry, the market wage can be expressed as function of the sum value of employment, $\bar{w}(\bar{V}_m)$, where $\bar{V}_m = \sum_k V(k|m)L_k$. These $V(k|m)$ terms thus capture of the effects

As shown in Arnold (2023), worker flows across industries can be used to construct empirical measures of $V(k|m)$. Assuming the utility of a job in a industry k for a worker in industry m is given by $U(k,|m) = \ln V(k|m) + \zeta$, where ζ is a type I extreme value distributed idiosyncratic shock, suppose that workers move jobs if they receive a job offer from an industry where this utility exceeds that of their current industry. With the simplifying assumption that the arrival rate of offers from an industry is proportional to industry employment, $V(k|m)$ can be expressed as a function of the relative market sizes of k and m and the probability that a worker in market m moves to either industry k or another firm in industry m:

$$V(k|m) = \frac{P(k|m)}{P(m|m)} \cdot \frac{L_m}{L_k}.$$

Intuitively, this expression states that, for a worker in industry m, jobs in industry k will be relatively more valuable if industry m workers tend to move to k disproportionately offer relative to the relative market size of the two industries. I construct an empirical analog to this measure with job transition probabilities from within-commuting zone job moves and the average relative market size within commuting zones at the annual level for the 1996–2011 period. This yields an empirical measure of $V(k|m)$ denoted by $\nu_{m\to k}$.

C Additional Results

I supplement the mover and stayer comparisons from Section 4 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 Appendix Figure A.8. 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 Section 2 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:

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

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 A.8 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. As established before, the fact that I find that bonus has no significant effect on the probability of job moves provides evidence that these effects primarily reflect either productivity passthrough to "stayers" or improvements in the quality of job moves that workers experience. The latter mechanism is also supported by my results showing a decline in job loss risk for exposed workers.

D LEHD Sample Construction

The research samples used in this paper all comprise workers that were surveyed as a part of the 2000 Decennial Census long-firm questionnaire, for whom the LEHD reports positive earnings over the 1997–2001 period, who appear in one of the 21 states for which this project has access after 1996, and who are attached to a firm in the manufacturing sector in 2001. When a worker receives earnings from more than one firm in any year throughout the sample, I 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. I then use the 4-digit NAICS industry code of the primary firm in 2001 to classify workers according to the investment duration schedule faced by their employer.[25]

I make several additional restrictions to my samples to ensure that the pool of workers I study are of prime working age in 2001, that maintained relatively stable labor force attachment prior to policy implementation, and for whom their 2001 firm identifier does not reflect a transitory job spell. Specifically, I restrict attention to workers that were between the ages of 25 and 54 years old in 2001 and who had at least two years of tenure at their 2001 firm. The latter restriction is useful because it likely reduces measurement error in my occupation variable, which is recorded as of 2000, though I cannot rule out within-firm occupational transitions from 2001 to 2002. I also exclude those that do not make more than $15,000 in 2001 dollars each year over the 1997-2001 period. In section 2, I show that my main earnings results are unchanged when I relax this set of additional restrictions.

Given this initial sampling procedure, my analysis is based on two separate samples designed to isolate different margins of labor market adjustment. The first ("with zeros") sample contains yearly observations for all 428,000 workers who meet the above criteria, regardless of whether they ever report nonpositive earnings in a calendar year over the 2001-2011 period.[26] Worker earnings are set to 0 for these observations, yielding a balanced panel of 4,563,000 observations over the 1997 to 2007 period. The second ("highly attached" workers) sample only contains workers who report positive earnings for every sample year, yielding 3,304,000 observations of about 310,000 workers. The first sample is used to primarily to examine worker attachment to the labor market, while the second sample is the primary focus of my analysis of the effects of bonus on worker earnings.

Table 1 presents summary statistics of 2001 worker characteristics across treatment and control groups, as defined above. Treated workers in my main sample of workers who never report a full year of zero earnings earn less on average than in the control. This pattern holds in the worker sample that relaxes the age, tenure, and minimum earnings restrictions, with these workers earning slightly less in 2001. I supplement this information with public-use data from the Decennial Census, which I use to assess a broader range of worker characteristics. Treatment and control groups are similar across basic demographic characteristics, though treated workers are less educated and more concentrated in production occupations, as defined by Acemoglu and Autor (2011). These patterns are consistent with the relative earnings patterns in Panel A, which also appears in the Decennial data. I further validate my sample with aggregated

[25]Because the LEHD does not provide establishment-worker linkages, I determine firm-level industry as the modal 4-digit NAICS code by total employment.

[26]I use the LEHD Earnings Indicators file to identify whether a missing worker appears in an LEHD state for which this project does not have access. For workers that leave the full sample but report positive earnings in another state, I impute earnings with a linear regression model that predicts earnings changes associated with moves across states with worker demographics, prior earnings level, and prior industry of work as controls.

employment data from the NBER-CES Manufacturing Database on total payroll, total employment, and employment in production occupations. These data point to a higher and more equal share of production workers across treatment groups, although treated industries still maintain a higher share of production workers, and lower average annual earnings across categories. These differences in education level and task content of jobs across treatment groups underscore the value of pairing the LEHD with Decennial Census microdata, and I include flexible time controls that interact production occupation status with educational level in all of my primary earnings regressions.

E Model Derivations

This section presents an expanded wage bargaining model which yields passthrough expressions similar to the basic model in Section 4.2. The model exposition is based on a version presented in Garin and Silvério (2024), who consider passthrough from TFP shocks to worker earnings where firms employ a single-input production function.

E.1 Model Setup

Firms combine labor with capital in a constant returns to scale Cobb-Douglas production function, $F(K, L) = AL^{1-\alpha}K^{\alpha}$, which is used to produce output they sell in a competitive product market at an exogenous price P. I consider a single-period model. At the start of the period, firms are endowed with an existing stock of incumbent workers L_0, face an competitive rental rate of capital R, and may hire additional workers H by incurring a cost $c(H)$, which reflects expenses incurred to recruit or train these workers. At the beginning of the period, a share δ of the existing workforce, which is assumed to be sufficiently large that firms always choose to hire more workers.

I use the Stole and Zwiebel (1996) multilateral bargaining solution to model wage determination such that individual workers Nash bargain with their employer over the equilibrium wage received by all workers, but this wage may be renegotiated if other workers leave. The cost to the firm if a worker leaves during negotiations comes from lost marginal revenue product, less wages not paid, and higher wages that must be paid to remaining workers as the $MRPL$ increases and is redistributed. Workers may leave and receive a competitive outside option $\bar{w}(\Phi)$ which can be a function of labor demand from other firms.

Formally, the cost to the firm of an individual worker leaving during negotiations is the derivative of firm profits with respect to labor: $J_L(L, K) = Y_L(L, K) - w(L, K) - w_L(L, K) \times L$. Setting worker's Nash bargaining weight to be β, the equilibrium wage solves the following differential equation in which the weighted surplus received by each party is equalized for all values of L:

$$\beta \times J_L(L, K^*) = (1 - \beta) \times [w(L, K^*) - \bar{w}(\Phi)].$$

Note that, based on the sequence of investment, hiring, and production, capital is fixed prior to negotiation, while labor may change if bargaining breaks down with individual workers.

Plugging in the above expression for $J_L(L, K^*)$, dividing by βL and applying the integrating factor $L^{\frac{1}{\beta}}$ leads to the following expression for a symmetric wage accepted by and paid to all workers in equilibrium:

$$w(L, K^*) = (1 - \beta)\bar{w}(\Phi) + \beta \times \frac{\int_0^L n^{\frac{1-\beta}{\beta}} Y_L(n, K^*; \phi) dn}{\int_0^L n^{\frac{1-\beta}{\beta}} dn}. \tag{E.1}$$

With Cobb-Douglas production, this yields the following analytical expression:

$$w(L, K^*) = (1 - \beta)\bar{w}(\Phi) + \beta \times \gamma \times Y_L(L, K^*; r) \tag{E.2}$$

with $\gamma \equiv \frac{\beta}{1-\beta+\beta(\alpha-1)}$. As in Garin and Silvério (2024), the wage can be rearranged to clarify the relationship between wages, the outside option, and firm rents:

$$w(L, K^*) = \bar{w}(\Phi) + \beta \times (\tilde{\gamma} \times MRPL(L, K) - \bar{w}), \tag{E.3}$$

where $MRPL(L, K) = Y_L(L, K)$ and $\tilde{\gamma} = \frac{\gamma}{\beta}$. This expression provides an intuitive expression for how policies like bonus depreciation may impact worker wages. Specifically, firm-level policy exposure can increase wages by increasing $MRPL$, while market-level policy exposure can induce wage growth via the outside option.

E.2 Deriving Passthrough Expressions

The above expression demonstrates that a shock to the cost of capital can influence wages if it affects the marginal revenue product of labor. Based on the setup above, firms choose capital and hiring to maximize the following profit function:

$$\Pi(L, K; r) = P \cdot F(L, K) - w(L, K)L - rK - c(H), \tag{E.4}$$

where $L = H + \delta L_0$, and δ is the share of workers that retire prior to production. P represents the exogenous price at which firms sell their product variety, such that revenue is given by $Y(L, K) \equiv P \cdot F(L, K)$. This setup yields the following first-order conditions:

$$L : Y_L(L, K) = c'(H) + w(L, K) + w_L(L, K) \times L, \tag{E.5}$$

$$K : Y_K(L, K) = w_K(L, K) \times L + r. \tag{E.6}$$

Firms thus choose a hiring level H^* and investment K to satisfy the two first-order conditions as well as the wage expression Equation 3. To derive an expression for how a shock to the cost of capital $\phi \equiv \frac{\partial \ln r}{\partial \text{Bonus}} < 0$ affects wages, I first consider the first-order condition for labor at the optima:

$$MRPL(L^*(r), K^*(r)) = c'(H^*(r)) + w(L^*(r), K^*(r)) + w_L(L^*(r), K^*(r)) \times L^*(r). \tag{E.7}$$

A shock to r induces a change in capital via its FOC, which in turn also affects L. Differentiating E.7,

$$\frac{dMRPL}{dr} = c''(H^*)\frac{dL^*}{dr} + 2w_L(L^*, K^*)\frac{dL^*}{dr} + w_{LL}(L^*(r), K^*(r)) \times L^*\frac{dL^*}{dr} \tag{E.8}$$

$$+ w_K(L^*(r), K^*(r))\frac{dK^*}{dr} + w_{LK}(L^*(r), K^*(r)) \times L^*\frac{dK^*}{dr}. \tag{E.9}$$

Using the fact that $MRPL(L^*(r), K^*(r)) = Y_L(L^*(r), K^*(r))$ and simplifying this expression yields the following elasticity of wages with respect to the change in the cost of capital:

$$\varepsilon^{MRPL,r} = \frac{1}{1+\chi}\alpha \cdot \varepsilon^{K,r}, \chi \equiv \frac{-Y_{LL}(L^*, K^*)}{c''(H^*)} \times (1 - \gamma(1-\alpha)).$$

Note that $\varepsilon^{K,r} < 0$. This expression shows that wage passthrough occurs so long as hiring costs are not constant, with the term $\frac{1}{1+\chi}$ capturing how greater convexity in hiring costs, relative to convexity in the production function, generates more passthrough. This term is then scaled by $\alpha \cdot \varepsilon^{K,r}$, which captures both how much a given reduction in r increases capital and how much greater capital increases productivity.