Buyouts with Payouts: The Impacts of Private Equity on Workers and Owners*

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Abstract

We link private equity (PE) buyouts to comprehensive U.S. tax data to explore the impacts of PE on employee and firm owner outcomes. We find PE buyouts lead to employment and earnings drops among incumbent employees that reverse within a decade. Substantial and persistent negative effects are only evident for older workers, higher-wage workers, and managers, consistent with some models of how PE generates value for shareholders. A significant portion of the large negative impacts on specific groups of workers are driven by outsized effects on owner-employees, who also realize extremely large capital gains in the year of the buyout. There is little evidence of follow-on entrepreneurship among bought-out owners. Instead, we observe persistent declines in their business income. Finally, while we find that add-on acquisitions lead to larger reductions in employment for some employee groups, including managers, we cannot rule out equal effects on earnings. The results highlight that for the vast majority of employees, there are at most small effects of modern PE buyouts.

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1 Introduction

While an extensive literature has shown that private equity (PE) increases investor returns and improves managerial efficiency (Kaplan and Strömberg 2009; Bloom et al. 2015; Ewens and Farre-Mensa 2022), PE acquisitions of firms have led to significant policy concerns about impacts on workers and the broader economy.[1] In recent years, these concerns have heightened amidst increasing deal volume, assets under PE management surpassing $13 trillion, and the rise of add-on acquisitions or "roll-ups" that increase concentration in local labor markets (McKinsey 2024). Some research has begun to examine the aggregate effects of PE on labor (Davis et al. 2014), but identifying which types of workers are affected is crucial for assessing the welfare implications of PE and informing models of how PE generates value for shareholders. Progress answering these questions has been limited by a lack of comprehensive data that can capture different income sources (wages and payouts), enable classification of worker types (by occupation and ownership), and provide sufficient statistical power to detect potential heterogeneity.

We overcome this challenge by creating a new linkage between extensive U.S. PE deal data and administrative tax records. Consistent with prior work, we find short-run declines in employment and wages following PE acquisitions. However, the rich and uniquely comprehensive data allow us to explore with sufficient precision heterogeneity in outcomes in the short and long run among groups of workers differentiated by managerial status, age, and income, and consider effects on payouts and other income sources. We also identify owner-employees in the data and explore both worker outcomes excluding owners and the effects on owner employment and business activity. Finally, we can differentiate deals by whether or not they are add-on acquisitions. We focus on the effects of these roll-ups on worker outcomes given their growing prevalence, and potential to generate monopsony power that has led to increased regulatory scrutiny. Overall, we find that the employment and wage declines following PE buyouts do not extend to rank-and-file employees.

[1] For example, a bipartisan Senate report noted that "employees were left without jobs" after PE acquisitions. Titles of popular books written on the subject include Josh Kosman's "The Buyout of America: How Private Equity is Destroying Jobs and Killing the American Economy," and Brendan Ballou's "Plunder: Private Equity's Plan to Pillage America."

To explore the effects of PE acquisitions on workers and firm owners, we utilize the universe of IRS tax records from 1999 to 2023. These tax records provide extensive information on employment, wages, business income, capital gains, and worker occupations, and allow us to create linkages between firms, employees, and owners. We match the administrative tax records to proprietary data from Pitchbook, which tracks more PE deals than other sources, and utilize this novel merged dataset to study short- and long-term outcomes of workers and firm owners up to a decade following buyouts for over ten thousand PE acquisitions. We match firms acquired by PE funds to similar firms that were not acquired by PE, based on industry, census region, entity type, asset and average wage decile, and the number of employees. Worker characteristics that we do not match on are balanced, and we generally find no differential trends before buyouts in our matched sample. We then explore labor market and other outcomes for workers and firm owners following buyouts using a stacked difference-in-difference design.

Examining workers' outcomes, we find small, short-run decreases in employment following buyouts that reverse within approximately six years. We find a spike in earnings in the year of the acquisition, likely driven by stock options and phantom stock plans that are categorized as wages. This increase is more than offset by decreases in subsequent years, with earnings taking nearly a decade to return to trend. The earnings decreases are three times as large as the employment decreases, pointing to the possibility of heterogeneous effects for different types of workers.

Average employment and earnings effects of PE buyouts mask substantial heterogeneity by age, income, and managerial status. For younger workers, we cannot reject no decrease in employment or earnings, but for workers older than 45, and especially older than 55, there are persistent negative effects on earnings and employment. We also cannot reject zero effect for workers from the bottom four wage quintiles, but estimate large earnings effects for top quintile employees. Using descriptions on taxpayers' Form 1040s to classify workers into occupations, we see the largest negative employment and earnings effects on managers. While large impacts on managers in part explain the impacts on older and higher-wage workers because these characteristics are correlated, the largest negative effects are concentrated among managers that are older and have higher earnings. Notably, we cannot reject zero effects of buyouts on the full sample of workers who are not managers and can rule out negative effects on their cumulative earnings

exceeding 1.7 percent.

While older workers, higher-wage workers, and managers all experience substantial declines in employment and earnings, senior and highly compensated employees are also more likely to have equity stakes in their firms and therefore receive payouts triggered by PE acquisitions that may offset some earnings losses (Eisfeldt et al. 2023). Consistent with this, managers and top wage quintile workers realize net capital gains that on average exceed the decrease in cumulative earnings over the decade following PE acquisitions.[2]

To more systematically identify owners in our data, we use Form K-1 for passthrough entities along with other ownership data. We first check for the sensitivity of our prior findings to fully excluding owners; the results are similar with pronounced heterogeneity across worker types, though with somewhat smaller magnitudes. We then focus on owner outcomes and find that they realize net capital gains on the order of several million dollars in the buyout year and subsequently exit the labor force at high rates, with a 30 p.p. persistent reduction in employment. The negative employment effects are larger among older owners (ages 45 and over), whereas younger owners tend to remain employed and experience little change in earnings over the ten-year sample period we study. Surprisingly, we find little evidence that buyout-driven liquidity fosters serial entrepreneurship: owners experience large overall declines in business income and are no more likely to fund or see increased business income from new firms over any horizon. Unlike effects on working, this pattern is consistent across the age distribution.

We finally examine if there are heterogeneous effects by whether the buyout is an add-on acquisition or not. Roll-up strategies in which PE funds acquire multiple firms in the same market may reduce redundancies at merged businesses, but have also received increased attention due to concerns they can raise local labor and product market concentration, harming workers and consumers. Consistent with some of this, we estimate differences in short-run employment impacts between roll-ups and other acquisitions for older, higher-wage, and managerial workers, but this pattern does not extend to other workers. And despite concerns over these deals, we cannot reject equality in earnings effects across any group when excluding owners. In fact, limiting the sample to only

[2]Capital gains realizations are in part only an increase in liquidity and not wealth, albeit understated since we observe only the net amount.

add-on acquisitions, we estimate at most small earnings effects for (non-owner) workers, with the bottom of the confidence interval ruling out cumulative declines greater than 2.5 percent.

This paper joins an extensive literature on the effects of private equity buyouts, beginning with work by Kaplan and Schoar (2005). Kaplan and Strömberg (2009) and Ewens and Farre-Mensa (2022) provide reviews of this literature. More specifically, this paper contributes to the literature on the impacts of PE acquisitions of firms on workers. Davis et al. (2014) conducted a seminal study, exploring job creation and destruction at the firm level in the United States. They find that PE acquisitions lead to job losses, but their employment data is at the establishment level and does not allow either individual workers to be tracked over time or the exploration of heterogeneous effects on workers. Other papers have filled this gap by looking at effects on individual workers.[3] A number of studies have explored the effect of PE buyouts on worker wage earnings in various European countries, often coming to differing conclusions over effects on workers and the amount of heterogeneity present (Olsson and Tåg 2017; Antoni et al. 2019; Fang et al. 2025). In an important study, Herkenhoff et al. (2025) examine the effect of PE on workers using a U.S. sample of earlier PE buyouts and Census data to test theories of PE value creation, generally not rejecting that wage earnings estimates are the same between different groups of workers.

Our contributions rest on the rich and uniquely comprehensive data we bring to bear. Our novel linkage between firms acquired in PE buyouts and their owners and workers allows us to study the effects of buyouts for workers and owners separately. Our first contribution is to show that the bulk of the estimated labor market effects of PE buyouts are driven by owner separations. Estimated earnings and employment effects on rank-and-file employees are close to zero and confidence intervals can rule out relatively modest or larger effects.

Our linkage between owners and firms also allows us to examine the effects of PE buyouts on a broad set of owner outcomes, including distributions from old and new business ventures. Our second contribution is to show that, despite the large payout

[3]Several other studies have focused on PE and worker outcomes exploring workplace conditions. Agrawal and Tambe (2016) find that PE buyouts facilitate technology investments that complement worker skills. Cohn et al. (2021) find that PE acquisitions lead to fewer workplace accidents, and Gornall et al. (2024) find decreased employer satisfaction following buyouts.

event triggered by the sale of their business to PE, we find no evidence that owners, even those that are young, found or invest in new businesses. These results stand in contrast to the literature that emphasizes the importance of liquidity in fostering entrepreneurship (Holtz-Eakin et al. 1994), underscoring heterogeneity in the extent to which financial constraints bind across contexts.

Third, using information to classify deal type in our detailed PE buyout database, we are the first to estimate how workers and owners are impacted by add-on acquisitions, which have represented a growing share of PE deals and now constitute the majority.[4] Despite high levels of concern regarding these deals, we do not see large earnings decreases resulting from add-ons, and cannot reject similar effects when focusing on the transactions that lead to the largest increases in concentration.[5] While our analysis compares add-on to non-add-on PE acquisitions, another useful benchmark is to compare add-ons to merger and acquisition (M&A) deals more broadly. We do not find evidence that workers fare worse after add-ons than they do after M&As in the U.S. (Arnold 2021) or abroad (Arnold et al. 2025; He and le Maire 2025; Lagaras 2025).

Fourth, by matching the largest assembled set of buyouts to date to the universe of U.S. worker earnings histories and their demographics, including occupation strings for a substantial fraction of the population that we use to classify workers as managers, our data yields sufficient power to detect heterogeneous impacts of PE deals on different types of employees. We document differences in the impacts of PE buyouts on workers by age, income, and managerial status, even after excluding owners, that are consistent with theories for how private equity generates value for shareholders vis-à-vis employment decisions. Specifically, our results align with the view that PE breaches implicit contracts, namely laying off tenured and entrenched senior employees whose pay exceeds their marginal product (Lazear 1979; Shleifer and Summers 1988). Our findings that PE buyouts have larger negative impacts on managers are consistent with PE removing entrenched management, research showing PE-owned firms employ better management practices, and evidence that better managerial education can improve financial decision making (Carlin and Robinson 2012; Bloom et al. 2015; Giorcelli 2019;

[4]In a recent working paper, Asil et al. (2024) study the effects of roll-ups on prices charged in the anesthesia industry.

[5]Increases in profits arising from more concentrated product markets could raise worker earnings in ex post rent sharing models of wage determination, though we similarly do not find differences by whether or not the deal is in the non-tradable sector.

Giorcelli 2024). By eliminating entrenched management, external buyouts may also help resolve agency conflicts and improve governance (Jensen 1986; Kaplan and Strömberg 2009).

Finally, the breadth of our data and our ability to observe nearly all sources of income enable us to make several other contributions. Our sample horizon extends well beyond the two or three years post-buyout typically examined in prior work. We find that the dynamics can be important, with some effects persisting for at least a decade and others recovering over time but outside the window of other analyses. Estimating the impacts of PE buyouts on capital gains realizations, we find that most of the older, higher-wage, and managerial workers negatively affected in the labor market receive payouts from PE buyouts that exceed their earnings losses, even after excluding those with nontrivial ownership shares. Our last contribution is that we can consider potential fiscal externalities of PE buyouts. We find only modest increases in the receipt of government benefits, primarily in the form of unemployment compensation, indicating direct fiscal externalities of PE buyouts are not meaningful.

The remainder of this paper is organized as follows. Section 2 describes the economics of private equity, sources of efficiency gains and potential concerns with the model. Section 3 describes the administrative tax and buyout data used, and describes our empirical strategy. Section 4 presents employee-level results, while section 5 presents owner-level results and shows that the bulk of observed earnings effects are driven by owners. Section 6 analyzes roll-up deals and section 7 concludes.

2 Private Equity Acquisitions

2.1 Private Equity Buyouts

PE buyouts occur when private funds engage in leveraged buyouts (LBOs). Target firms are acquired through debt financing, and in a typical deal high amounts of leverage are placed on the target firm's balance sheet. Kaplan and Strömberg (2009) and Metrick and Yasuda (2010) provide in-depth discussions of the PE model and discussions of the academic literature. PE acquisitions lead to changes in managerial incentives relative to other forms of for-profit ownership. PE owners tend to have stronger incentives to

maximize firm value, because fund managers are compensated through call-option like functions of firm profits. General Partners (GPs) who manage PE funds are usually compensated through the right to 20% of profits from increasing portfolio company values between deal buyouts and exits. High leverage levels further concentrate firm ownership and reduce agency frictions (Kaplan 1989). PE acquisitions also typically lead to changes in senior management, lessening existing relationships between management and employees. A large literature in finance has shown that PE buyouts increase productivity, improve management practices, alleviate credit constraints and generate high returns (Kaplan 1989; Boucly et al. 2011; Bloom et al. 2015). There are several channels through which PE buyouts might affect employees or owners and we outline these in the remainder of this section.

2.2 Labor Market Effects of Private Equity Buyouts

The effects of PE buyouts on employees are more controversial in the public policy sphere, and have been studied both in the U.S. context (Davis et al. 2014; Herkenhoff et al. 2025) and internationally (Boucly et al. 2011; Bloom et al. 2015). Directionally, the effects of PE buyouts on workers' employment and earnings are ambiguous. There are several channels through which PE buyouts may affect workers, and these channels may have offsetting impacts on employment or earnings. Distinct channels may also affect managers and less senior workers differently.

There are compelling reasons to think that PE acquisitions could lead to wage increases, through several channels. First, PE acquisitions may lead to higher productivity and profitability through operational changes or the relaxation of liquidity constraints. Regardless of the reason, these gains may be shared with workers. Second, higher leverage and bankruptcy risk associated with PE deals may also lead to compensating differentials. Moreover, less efficient employees capturing rents may be terminated, redistributing gains to more productive workers (Jensen 1989).

On the other hand, PE ownership may result in weakened bargaining power for unions and the breach of implicit contracts (Shleifer and Summers 1988). This could lead to early retirement for many workers, or separations of highly paid managerial employees. Moreover, higher leverage and bankruptcy risk may lead firms to lay off

workers to cut costs. In sum, PE buyouts could affect workers through non-mutually exclusive channels in different directions, and may have significant heterogeneous impacts by managerial status, age, and income.

2.3 Private Equity Buyouts and Owners

The effects of buyouts on existing firm owners are also theoretically ambiguous. Non-mutually exclusive channels could raise or reduce owners' earnings and labor supply. Buyouts lead to large and immediate payouts for owners, providing large amounts of liquidity, and potentially increasing the wealth of owners. It is well known from many contexts that wealth effects reduce labor supply, and these effects might also apply to the substantial liquidity infusion owners receive after buyouts. On the other hand, previous owners may leave target firms following buyouts and found new firms, using their new-found liquidity to start or invest in new ventures. This serial entrepreneurship might lead to higher future earnings and capital gains for bought-out owners.[6]

The fiscal effects of buyouts depend on the aforementioned employee and owner channels. If employees salaries grow, this will lead to a positive fiscal externality, while the converse is true if wages fall or if workers are no longer employed, including through UI receipt. Moreover, if many workers retire early, this could put strain on public pension systems. Similarly, if owners reduce labor supply and earnings due to wealth and liquidity effects, this will reduce overall tax revenues. If owners exit current positions and found new firms, this will raise overall government revenues, both directly from the firm owners and from any new employment created through serial entrepreneurship.

2.4 Roll-ups

In a roll-up deal, PE funds acquire multiple small targets in the same market. PE funds argue that these roll-ups create efficiencies through economies of scale. Larger firm networks can consolidate supply chains and management, creating benefits that are passed on to consumers.

[6]While beyond the scope of our analysis, the payouts stemming from PE buyouts may also create incentives for entrepreneurs to found firms with the intent to be compensated through a later acquisition. If PE buyouts are particularly attractive for founders, the likelihood of a buyout may affect their incentives to create new firms.

While there are reasons to suspect that roll-ups may create efficiencies, critics of these deals point out that they may also harm both consumers and employees. Product market power may translate into higher prices for consumers, and labor market power may lead to a reduction in wages for employees. These concerns, combined with the increasing frequency of roll-ups over time (these deals make up two thirds of the PE deals in our sample with an increasing share over time) have led to significant policy and regulatory attention in recent years. For example, Senator Elizabeth Warren wrote in regards to the health care industry that roll-ups "have resulted in market dominance leading to higher prices, particularly when private equity has been involved." Despite significant policy concerns, empirical evidence regarding roll-ups remains scant.

3 Data, Estimation, and Summary Statistics

3.1 Data and Matching

Our primary data source is Internal Revenue Service administrative tax records including tax forms 1040, W-2, 1120, 1120-S, and 1065. These records contain detailed information on firms, owners, and employees. To supplement the tax data, we merge in proprietary PE deal data from Pitchbook, a leading market intelligence firm that provides data on private capital markets. The combination of these data sets allows us to locate firms acquired by private equity in the tax data, observe employees and owners of these firms, and to follow employees and owners across time.

First, we use business name, address, and zip code to match firms acquired in leveraged buyouts in the Pitchbook data to those from corporate tax forms 1120, 1120-S, and 1065 that employed workers (i.e. issued W-2s), identifying firms in the tax data acquired in private equity deals. We discuss our matching algorithm in Appendix A. This process yields a set of 15,767 firms acquired in PE buyouts.

Second, because we study the effects of private equity acquisitions on workers and owners using a matched differences-in-differences research design, we match firms acquired by PE to similar firms never acquired in the year before the PE buyout, and compare the outcomes of workers and owners at these firms over time.

To establish a control group for our matched differences-in-differences design, we

construct a set of plausible control firms that are never acquired by PE. We match acquired firms to control firms in the year before the PE buyout in a two step process, following a number of recent papers (Goldschmidt and Schmieder 2017; Smith et al. 2019; Arnold 2021; Arnold et al. 2025). First, for each acquired firm we find all exact matches without replacement on 2 digit NAICS industry, census region, entity type, asset and average wage decile, and a number of employee bin {<10, 10-20, 20-50, 50-100, 100-300, 300-1000, >1000}. Second, we choose a match from among these potential counterfactual firms as the firm with the closest propensity score, estimated predicting treatment with a linear probability model using quadratics in average wage, assets, and number of employees. We find matches for 13,068 out of the 15,767 firms we identify in the tax data, a match rate of 83%. After restricting to matched pairs of firms that both have more than 10 workers in the year before the buyout, we are left with 11,155 firms. Utilizing deal-specific information provided by Pitchbook, we find that 67% of these deals are add-on acquisitions.

Third, we link acquired and matched control firms to their workers and owners. We define workers as being employed at a firm if the firm issues them a W-2. We observe firm ownership for passthrough businesses (approximately 70% of acquired firms) directly on schedule K-1 attached to forms 1120-S and 1065. To measure C-corporation ownership, we rely on 1099-DIVs and form 1120 schedule G. 1099-DIVs identify all individuals receiving dividends, while schedule G provides information on large shareholders.

After identifying workers and owners, we separately construct a worker panel and an owner panel. The panels track individuals that work at or own a firm in the year before a private equity buyout from six years before the buyout year through nine years after the buyout year, regardless of whether they maintain a relationship with their original firm. The worker panel allows us to examine the impacts of PE acquisitions on workers, and we use a measure of ownership that applies across corporations and passthrough entities to exclude owner-employees from some worker analysis. However, not all owners receive wages, so they may not appear in the worker panel. The owner panel allows us to cleanly identify all owners of passthrough businesses and follow their employment, business income, capital gains, and entrepreneurial activity over time.

To construct the worker panel, we identify all workers employed at firms acquired

by private equity in the year before the PE buyout and their matched controls. We then restrict the sample to workers who earn more than $15,000 in 2017 dollars in the year prior to the buyout and also worked at the firm two years before the buyout (i.e. were employed by the firm since the beginning of the year before the deal). After these sample restrictions, our worker panel includes employees associated with 10,029 unique firms acquired in a PE buyout. We measure worker wage income as the sum of wages subject to medicare tax across all W-2s in each year, setting wage income to zero in years in which workers do not receive a W-2. We measure worker capital gains realizations on form 1040 schedule D and classify workers into occupations using strings on form 1040 available starting in 2005.[7] To explore fiscal externalities of buyouts we also measure unemployment receipt on form 1099-G, and disability and old age and survivors insurance fund (OASI) payments on form 1099-SSA.

To construct the owner panel, we identify all owners of passthrough businesses in the year before they are acquired in a PE buyout from our previous sample of firms. We restrict to owners who hold more than 1% of all equity in the year before the buyout and hold stakes for at least two consecutive years before the buyout.[8] Tracking these owners over time, we measure their wages as the sum of wages subject to medicare tax across all W-2s, business income as the sum of ordinary income on all form 1120-S or 1065 schedule K-1s, and sole proprietor income and capital gains on form 1040 schedules C and D, respectively. To analyze the worker panel in the absence of owners, we drop all owners in the owners panel, plus any owner of a C-corporation on Form 1120 schedule G with an ownership stake exceeding 1%, or anyone receiving more than 1% of total dividends paid out by the corporation in any year before the buyout, measured on form 1099-DIV.

[7] We observe detailed occupation titles on 1040s, which we group into managerial vs. non-managerial roles. In appendix A, we discuss classifying managers into managerial or non-managerial positions.

[8] For S-corporations we directly observe the share of stock owned by each owner and the business income they receive. We keep an S-corporation owner in the panel if they own more than 1% of all stock, or if they receive more than 1% of all business income distributed to owners in the year before the buyout. For partnerships, we only observe the income distributed to partners. We thus keep a partner in the panel if they receive greater than 1% of all business income distributed to the partners in the year before the buyout.

3.2 Empirical Strategy

Defining event time $e = 0$ in the year of the PE buyout, we implement a stacked differences-in-differences estimator employing the following specification

$$Y_{it} = \delta_i + \gamma_t + \sum_{e=-6,\neq -1}^{9} \beta_e \mathbb{1}(e = t) \times PE_i + \varepsilon_{it}. \quad (1)$$

Our main treatment variable of interest is PE_i, an indicator for whether a worker's firm was subject to a PE buyout in event time 0. Event time fixed effects γ_t absorb market-wide factors in the lead up to deals, such as workers wages growing as seniority increases. Individual by original payer tin fixed effects δ_i absorb time-invariant individual specific factors, such as an individual's family background or college education pre-buyout.[9] Our main coefficient of interest is β_e, the average difference between treated and control worker outcomes in event time e relative to the average difference in their outcomes in the year before the PE buyout. We use many outcome variables Y_{it} across our worker and owner samples, including an indicator for receiving positive W-2 wage income (employment), W-2 wage income, capital gains realizations, and business income for worker or owner i at event time t. We cluster standard errors at the firm level throughout the paper.

Our main identifying assumption is a standard parallel trends assumption that in the absence of a buyout, the outcomes of workers and owners at firms acquired by PE would have trended similarly to workers and owners in the matched control group. Our extensive data allow us to visually assess pre-trends for a variety of outcome variables over six years before acquisitions, and we find little evidence of differential trends. Moreover, Appendix Tables B.1 and B.2 show approximately balanced worker and owner characteristics between acquired and matched control firms in the cross-section, none of which we matched on directly.

Our empirical design does not suffer from conventional issues that arise in staggered timing setups (Goodman-Bacon 2021) because we compare newly treated workers and owners at firms acquired by private equity to never-treated workers and owners at firms never acquired by private equity. Therefore, comparisons are never made to the early-

[9] A small fraction of workers receive W-2s from multiple firms in our sample. Using only individual fixed effects leads to nearly identical results.

treated or always-treated.

3.3 Deal, Worker, and Owner Summary Statistics

Table 1 presents deal-level summary statistics for the 11,155 buyouts where we identify a matched control firm and both firms have more than 10 employees. Column 1 displays the number of deals, column 2 displays the total number of workers, and column 3 displays the number of workers who appear at the firm in the year before the buyout and the year of the buyout. The difference between the second and third columns suggests significant churn as workers move to other firms following buyouts, consistent with Davis et al. (2014) and Herkenhoff et al. (2025). These 11,155 deals affect 2,007,409 workers. Approximately two-thirds of the deals are add-ons or roll-ups, and the largest sectors both in terms of deal count and number of employees are manufacturing (2,769 deals), services (2,742 deals) and wholesale trade (1,259 deals). Appendix Figure B.1 plots deal counts by year, add-on, and industry. The number of total buyouts has increased over time, primarily driven by the rise in add-on acquisitions.

Table 2 displays summary statistics for the worker sample. After the sample restrictions discussed above, our worker panel has over 27 million worker-year observations and contains just under 2 million workers. We observe occupation codes for 47% of workers because occupation strings do not begin until 2005 and they are not present for all workers. The average worker in our sample earns $76,100 in 2017 dollars, while workers at the tenth and ninetieth percentiles earn $2,600 and $159,200 respectively. Workers on average realize $2,200 in net capital gains, earn even less sole proprietor and business income, and collect less than $1,100 of unemployment, disability, and social security income. Over 90% of employees receive $0 from all of these non-wage sources. Approximately two-thirds of our sample consists of men, and the average age is 43. Among workers for whom we observe occupation, 15% are managers. Despite two-thirds of the deals in our data being add-ons, these deals tend to target smaller businesses: slightly under half of workers are associated with add-ons.

Table 3 displays summary statistics for our owners panel. There are 654,856 owner-year observations and 47,545 owners in the panel, about 2.4% of the total number of workers. The typical owner earns significantly more than the typical worker, and the

majority of this income is not wages. Average business income is $385,400, average net capital gains are $287,600, and average wages are $164,300. While owners face significant tax incentives to classify income as business income (Smith et al. 2019), more than 50% of owners do receive wages. Close to 80% of owners are men, and the average owner age is 55.

4 Employee Level Results

4.1 General Effects of Acquisitions on Workers

We begin by exploring the impacts of PE acquisitions on all workers. Using the entire worker panel, Figure 1 plots estimates of β_e from equation (1). Panel (a) and (b) display estimates using employment and wages as outcome variables. We also plot the raw means of employment and wages for treatment and control workers by year in Appendix Figure B.2. Consistent with prior work such as Davis et al. (2014), we estimate muted employment effects.[10] There is a slight decline in employment of roughly half a percentage point in the years immediately following buyouts, but this gap shrinks over time and is completely eliminated within six years. Within a decade of the buyout, we cannot reject that employment has risen for workers employed at acquired firms relative to workers employed at matched control firms. The results suggest that aggregate employment effects are modest and not persistent over time.

Figure 1, panel (b) indicates that effects on wages are more pronounced directly around the time of buyouts. In the year of the buyout, wages jump by 6% for acquired versus matched control workers. This spike reflects severance payments as well as some bonuses and equity-like payouts associated with the buyout transaction.[11] Following

[10]Our employment effects are slightly smaller than other estimates in the literature in the United States. This is likely due to the fact that we have a more recent sample of acquisitions and employment. Appendix Figure C.1 shows employment effects by time period. Employment and earnings effects are more negative for buyouts occurring prior to the Great Financial Crisis.

[11]The fact that we can observe the spike in payments following buyouts is due to the richness of the administrative tax data. Other data sources that are based off of surveys or state unemployment insurance records may miss compensation such as one-time payments in exchange for equity, bonuses, or severance. Vesting restricted stock units, exercised non-qualified stock options, and phantom stock payouts are all included in W-2 wages. While we cannot fully decompose observed wage spikes into their components, extensive conversations with Treasury lawyers suggest phantom stock payouts – bonus compensation that is tied to stock performance but does not include any actual stock ownership – play a significant role. Furthermore, box 12 on the W-2 sometimes includes a “Code V" statement corresponding to non-qualified

the buyout, wage effects are more negative for several years. In the years immediately following the buyout, we estimate a 3% reduction in wages and reject declines smaller than 1.5%. Over time, the point estimates remain between -3 and -1% but confidence intervals widen, and we cannot reject zero relative wage declines within four years of the buyout.

Table 4, Panel A presents regression estimates of equation (1), which correspond to Figure 1. Column 1 shows there is a statistically significant decline in labor force participation of 0.61 percentage points on average over the first four years following a buyout, which dissipates over time. Column 2 indicates a $5,136.9 increase in wages in the year of the buyout, followed by a statistically significant $2,316.7 average decline in wages in each of the four years following a buyout. Column 3 shows that this decline is somewhat offset by an increase in liquidity. On average, workers realize a capital gain of $8,250.4 in the year of the buyout. We observe very small increases in sole proprietor and unemployment income in columns 4 and 5 ($57 and $116 respectively over the four years after the deal), and cannot reject zero impacts on disability and social security income in columns 6 and 7.

Panel B of Table 4 displays very similar estimates from specifications that include 2 digit NAICS industry by year fixed effects, and Appendix Table C.1 displays estimates for additional extensive margin outcomes. In aggregate, we do not observe significant extensive margin effects on other income sources, suggesting few workers are receiving them. Appendix Table C.2 presents estimates of the net present value impacts of PE acquisitions. Estimates and standard errors in the table reflect a discounted sum of the year-by-year coefficient estimates from equation (1), using a discount rate of 5, 7, or 9%. Below the estimates, we display a 95% confidence interval in terms of percent changes, scaling the net present value estimate displayed by the discounted sum of the outcome variable for the control group over the same time frame. Column 1 displays estimates for the full worker sample, and shows we rule out cumulative wage declines of more than 2.8% when using a 7% discount rate.[12]

stock options. We observe a spike in the frequency of Code V listings in box 12 in the year of buyouts for our treatment relative to control group.

[12]These aggregate results are not driven by our sample selection criteria for inclusion in the worker panel. Appendix Table C.3 displays regression estimates of equation (1) performed on an alternative worker sample constructed without any wage restrictions. The estimates are similar to our baseline estimates.

While the aggregate employment and wage effects of PE are somewhat muted and in line with previous studies, these small average effects may mask significant heterogeneity. In the remainder of this section, we explore this heterogeneity by estimating the impacts of PE acquisitions separately by workers' age, wage quintile, and occupation.

4.2 Heterogeneity by Age, Wage Quintile, and Occupation

While the aggregate effects shown in Figure 1 are well-studied, these aggregates mask significant heterogeneity. Figure 2 displays coefficient estimates from equation (1), estimated separately within decade-wide age groups and wage quintiles, defined based on workers' ages and wages in the year before PE buyouts. Tables 5 and 6 display corresponding regression estimates. Panels (a) and (b) of Figure 2 show employment and wage effects by age groups. For workers between 25 and 35, employment increases by approximately 1% over a decade and rejects zero. For workers between 35 and 45 there are zero effects. However, for workers above the age of 45 we see substantial declines in employment. The greatest effect is for workers between 55 and 65, who experience a 3% reduction in employment within five years of the buyout. This effect partially reverses but is persistent, with a 2% decline in employment even a decade later. The results are thus consistent with buyouts leading to early retirement for many workers. The wage effects in Figure 2, panel (b) mirror the employment patterns, with zero or positive effects for younger workers, but persistent wage declines of roughly 6-9% for workers between 55-65.

Panels (c) and (d) of Figure 2 show employment and wage effects by wage quintiles, defined in the year prior to the buyout. Our estimated effects by age are strongly correlated with effects by worker wage quintile, as older workers tend to earn more. For workers in the bottom 80% of the wage distribution we see no or even positive effects on employment. This pattern is mirrored for wages in years after the buyout with no declines for the bottom 80% of wage earners, and close to a 6% persistent wage decline for top quintile workers. This effect persists a decade after buyouts. Thus, in line with evidence from France, U.S. PE buyouts reduce wage inequality for workers by bringing wages down at the top of the income distribution (Fang et al. 2025).

In contrast to post-buyout wage declines, in the year of the buyout, wage spikes in-

crease by worker wage quintile. Equity and equity-like compensation that counts as wages appears more concentrated among higher-wage and older workers, suggesting potential for heterogeneous payouts associated with PE buyouts. Tables 5 and 6 display level estimates of the wage, employment, and capital gains impacts of PE buyouts by age and wage quintile. These estimates confirm that capital gains realizations are substantially larger for older and top wage quintile workers, while younger and lower wage workers realize fewer capital gains.

Much of the aforementioned effects are driven by employees in managerial positions, who tend to be both older and higher earning. It is well known that many managers are removed from their positions following buyouts, which may reflect efforts to improve management practices and increase efficiency at target firms (Kaplan 1989; Bloom et al. 2015). Figure 3 displays estimates of β_e from equation (1), estimated separately on workers that are managers, workers in non-managerial positions, and all workers for which we can observe occupation in the year before the buyout. The outcome variable is employment in panel (a) and wages in panel (b). Aggregate impacts on the sample of workers for which we can observe occupations appear similar to estimates based on all workers, suggesting our ability to observe occupation is not overtly selected. For workers not in managerial positions, who comprise the vast majority of workers for whom we observe occupation (85%), we see patterns similar to those observed for younger and lower-wage workers. We see slight declines in employment and wages following buyouts, which are marginally or not significant with later increases that eventually reach parity or surpass levels prior to the buyout. On the other hand, for managers, we estimate a sharp 3% drop in employment over the first five years after buyouts that shrinks over time, but not back to zero, and sharp and persistent wage drops of 6-9% over a decade after buyouts.

Table 7 displays regression estimates corresponding to Figure 3. Managers experience an average wage drop of $10,954.4 over each of the four years following buyouts, while the wage effects for non-managers are approximately one-tenth as large and statistically insignificant. Employment effects for non-managers cannot reject zero, while managers employment drops by 2-3%. As with higher-wage and older workers, manager wages spike by $11,294.4 and capital gains realizations increase by $31,948.1 in the year of the buyout, while non-managers experience wage spikes less than half as large and capital

gains realizations less than one-fifth as large.

There is significant overlap between older workers, higher-wage workers, and managers. Older workers are more likely to have higher wages and be managers, and the reverse is also true. Appendix Table C.4 documents overlap between these groups of workers. Restricting to workers for whom we can observe occupations, 45% of managers are in the top wage quintile, and 18% of managers are ages 55-65. To understand whether the negative employment and wage effects we estimate for these groups are driven by one characteristic, we triply split our results by age group, wage quintile, and managerial status. Figure 4 displays average post-buyout coefficient estimates for manager employment and wages by age and wage quintile in panels (a) and (b) respectively. Panels (c) and (d) display the same estimates for non-managers by age and wage quintile. While across all of these subsamples we do observe some effects for older and higher-wage non-managerial workers, the effect sizes are usually double or more for managers relative to non-managers within age and wage quintile groups.

PE may replace managers because they would like to make operational changes, in which case targeting managers directly, regardless of their age or wage, makes sense. But PE may also attempt to lay off any workers earning more than their marginal product. While wages could exceed marginal products for the oldest workers, highest wage workers, or managers, theories of implicit contracts frequently predict that older and longer-tenured workers make more than their marginal products (Lazear 1979; Harris and Holmstrom 1982). To investigate whether PE acquisitions differentially impact workers with longer tenure, in addition to our previous estimates by age, wage, and managerial status, Appendix Figure C.2 displays estimates of β_e from equation (1), separately for samples of managers and non-managers, with tenure less than four years and greater than five years. Negative employment and wage effects are largest for managers with longer tenure, consistent with PE breaking implicit contracts for workers whose wages exceed their marginal products.

Appendix Table C.2 displays net present value (NPV) estimates of the impacts of PE buyouts on wages and capital gains for top wage quintile workers, age 55-65 workers, managers, and rank and file workers who are not in any of the 3 previous categories. Referencing our NPV estimates using a discount rate of 7%, wages decline by $46,925.6 and capital gains increase by $62,910.1 for top wage quintile workers in the decade after

the buyout. Wages decline by $41,943.9 and capital gains increase by $28,385.2 for age 55-65 workers, and wages decline by $55,442.6 and capital gains increase by $62,677.3 for managers. Therefore, higher-wage and managerial employees experience capital gains realizations that exceed wage declines over the decade following buyouts on an NPV basis, while capital gains realizations are roughly 75% of the wage decline for older workers. The story is substantially different for rank and file workers, whose wages increase by $2,477.7 and capital gains realizations increase by $609.2, neither of which rejects the null of zero. For rank and file workers, we reject cumulative wage declines exceeding 1.7%.

In summary, our results suggest there are small if any impacts of PE buyouts on rank and file workers, while older, higher-wage, and managerial employees experience larger declines in employment and earnings that are somewhat offset by capital gains realizations stemming from the transaction. These capital gains suggest many older, higher-wage, and managerial employees may be owner-employees that have equity stakes in the firms they work for. Owner-employees, while still earning wage income, may have agency in the decision to sell a firm to private equity in the first place, and their outcomes are likely to be different than workers without equity stakes because they will receive large payouts from buyout transactions. In the next section, we assess the role owners play in driving the estimated impacts of buyouts on workers, and the impacts of buyouts on owners themselves.

5 Accounting for Owners

So far, we have shown that there are heterogeneous effects of PE buyouts on workers, with large employment and wage effects for older, higher-wage, and managerial workers. These groups of workers also realize substantial capital gains around the time of buyouts, suggesting they may also be owners. Previous studies have generally been unable to distinguish between owners and non-owners within firms, but in this section we take advantage of our unique data to explore whether the impacts of buyouts differ for owners and non-owners. As discussed in section 2, acquisitions may increase or decrease the future earnings of owners through several non-mutually exclusive channels.

Figure 5 displays separate estimates of the impacts of PE acquisitions on workers,

splitting the worker sample into owner-employees and non-owner workers. The estimates demonstrate that the outcomes of owner-employees diverge much more drastically from other workers than older, higher-wage, or managerial workers' outcomes do. Panel (a) shows that non-owner capital gains and wages spike by 5-7% of baseline wages in the year of the buyout. Employment and capital gains effects are close to zero and flat for the decade afterwards, while there are small negative effects on wages that often cannot reject zero. On the other hand, panel (b) shows that owner-employees experience a decline in employment of over 30% and a decline in wages approaching 40% four years after acquisitions, while both declines remain above 30% a decade after buyouts. Panel (c) shows that capital gains spike by close to 160% of baseline wages in the year of the buyouts, roughly 30 times the relative increase for non-owners, and these relative changes understate level differences because average wages are higher for owner-employees than non-owner-employees. These results are consistent with owners experiencing a very large liquidity event in the year of the acquisition; the payout from the buyout.

Table 8 presents level estimates of equation (1), splitting the worker sample into owner-employees and non-owner-employees. The left panel restricts to owners who are working at the time of the buyout, while the right panel restricts to non-owner employees at the time of the buyout. Consistent with the results in Figure 5, we see no significant employment effects for employees once owners are removed from the sample, and marginally significant negative employment effects only in the first four years following the buyout with a $1,329.0 annual decline in earnings. This is approximately half of the effect size during the first four years post-buyout that we estimate in Table 4, and longer-term we observe no statistically significant wage or employment effects for non-owner-employees. In contrast, owner-employees exhibit substantial declines in both wages and employment.

Given the drastic differences between the outcomes of owners and non-owners after PE acquisitions, we first show that even after removing owner-employees from the workers panel, our previous results still hold, albeit with smaller magnitudes. Next, to account for the fact that not all owners earn wages (and therefore do not appear in our workers sample), we turn to our owners panel to explore the impacts that buyouts have on all owners.

5.1 Aggregate Employment Effects Accounting for Owner-Employees

Figure 6 explicitly shows that the effects of PE buyouts on workers are substantially smaller once owners are taken into account, but previously documented heterogeneity remains present after removing owner-employees. Panel (a) shows estimates of β_e from equation (1) using the full worker sample, with separate series displaying estimates including owners (solid), and excluding owners (hollow). The dots show employment, while the squares show wages. Once owners are excluded, aggregate employment effects of buyouts become small and statistically insignificant, even in the years immediately after the buyout. Wages are similar and positive in the year of the buyout for the estimates including and excluding owner-employees, but diverge in years after the buyout.[13] For the sample excluding owner-employees, point estimates are smaller than the full sample, and are statistically indistinguishable from zero within three years following the buyout.

The remainder of the panels in Figure 6 repeat the estimates in panel (a), but restricting to the groups with the largest employment and wage effects: older workers in panel (b), top wage quintile workers in panel (c), and managers in panel (d). Once owner-employees are excluded, both employment and earnings effects are significantly more muted, but still present. This is most evident for managers, where once owners are excluded employment effects are barely significant, and wage effects are about half the size as they are in the sample including owners.

Table 9 displays separate estimates including and excluding owner-employees for the full worker panel, top wage quintile workers, older workers, and managers. The estimates display a consistent pattern that once owners are excluded both employment and earnings effects decline by approximately 50%, with some variation across groups. In summary, Figure 6 and Table 9 demonstrate that even for the most affected subgroups, a significant portion of observed employment and wage effects are driven by owners.

The results are robust to alternative definitions of ownership. Appendix Table C.5 presents the same estimates, but using a lower threshold for identifying owner-employees, dropping all owners of passthrough businesses with any non-zero equity stake and any

[13] Phantom stock compensation is likely to be concentrated among highly paid non-owner employees that do not own significant amounts of actual stock. Consistent with this interpretation, estimated wage spikes are slightly larger in percentage terms when excluding owner-employees.

C-corporation owner with a stake of more than 0.1% either on form 1120 Schedule G or when calculating their share of a dividend payout in any pre-buyout year. We continue to see a similar pattern that once owner-employees are excluded both employment and earnings effects decline by approximately 50%.

Appendix Table C.6 summarizes the impacts of PE buyouts on non-owner-employees on a net present value basis. Average wage declines and capital gains increases are between one-third and one-half as large after excluding owners, comparing Panel B in Appendix Tables C.2 and C.6. Unsurprisingly, the impacts of PE on rank and file workers is similar regardless of whether owner-employees are included in the estimation sample because owner-employees are more likely to be older, have higher wages, and be managers.[14] After excluding owner-employees, we rule out rank and file wage declines exceeding 1.6% over the decade following buyouts.

5.2 Effects of Buyouts on Owners Using the Owners Panel

As documented in the previous section, owner-employees play a strong role in driving the impacts of PE buyouts on workers. Aggregate employment and wage effects are close to zero on non-owner-employees, and negative impacts on older, higher-wage, and managerial workers shrink by roughly 50% but remain significant after excluding owner-employees. However, not all owners of businesses receive wage income, and therefore not all owners appear in our workers panel. To explore the impacts of PE buyouts on owners more comprehensively, we turn to our owners panel, which tracks all owners of passthrough businesses acquired by private equity and their matched control firms, restricting to owners with equity stakes exceeding 1%.

Figure 7 plots estimates of β_e from equation (1) using the owners panel, tracking the impacts of PE on owners across extensive and intensive margins. Tables 10 and 11 present analogous intensive and extensive margin regression estimates. Panel (a) of Figure 7 uses indicator variables for whether owners realize any capital gains, and have any sole proprietor income, wages, and business income. Like with owner-employees, there is a large and persistent decline in employment for owners, of 12% in the four years after the buyout and 17% in the following five years. The frequency of owners receiving

[14] Appendix Table C.4, Panel B shows that 87% of owners are in the top wage quintile, 33% of owners are 55-65, and 49% of owners are managers.

any business income immediately declines by 20% in the four years after the buyout, with that decline only shrinking slightly to 18% in the following five years. On the other hand, owners experience a 19% spike in the frequency of capital gains realizations in the year of the buyout. There is also a slight uptick of a few percentage points in the frequency of owners receiving any sole proprietor income.

Figure 7, panel (b) uses an indicator variable for owners receiving no positive wage, business, or sole proprietor income. There is a 17-18% increase in the fraction of owners receiving no positive income over the entire decade after the buyout, a sign that owners are leaving the labor force and not starting new businesses.

Figure 7, panel (c) plots estimates of the level impact of PE acquisitions on owners capital gains realizations and business income. The average owner realizes a capital gain of $2.59 million dollars in the year of the buyout and an additional $100,750.5 in each of the four years after the buyout. On the other hand, business income quickly declines relative to the year of the buyout, by $579,235.4 per year in the four years after the buyout and $477,595.6 per year in the following five years. Panel (d) plots percent changes, showing that wages decline by roughly 20%, total sole proprietor income declines or does not change, and business income immediately declines by roughly 80%, remaining more than 50% below baseline after a decade. These results emphasize that owners appear to enjoy very large capital gains realizations along with precipitous drops in business income and wages, consistent with large liquidity events leading to declines in labor supply.[15]

While owners elect to sell their firms to PE, for illustrative purposes, if we interpret the estimates in Tables 10 and 11 as driven by liquidity effects, a $2,586,909.5 increase in captain gains in the year of the buyout leads to a 12.52 percentage point decrease in employment, a $579,235.4 reduction in business income and a $40,965.7 reduction in wage income on average over the four years after the buyout. Scaling these estimates, a $1 million increase in liquidity from the capital gains realizations leads to a 4.8 percentage

[15]That business income falls by as much as the capital gain realization is likely explained by three factors. First, the value of the business stake is likely understated as we measure *net* capital gains, not the *gross* capital gain before subtracting the basis, and we further cannot rule out that owners are realizing other losses to offset any gain. Second, business valuations may be driven by a very high discount rate. Third, control owners may be making additional investments in their firms whereas treated owners may be making fewer investments in their firms, directing liquidity towards alternative asset classes that yield other types of income.

point reduction in employment, a $223,910 reduction in business income and a $15,836 reduction in wage income on average over the four years after the buyout.[16]

Appendix Tables C.7 and C.8 show that while magnitudes change, our results are similar if we instead restrict the owner panel to owners with equity stakes exceeding 5% or 10%. For owners with equity stakes exceeding 10%, average capital gains in the year of the buyout are $3.7 million, business income quickly declines relative to the year of the buyout, by $801,023.3 per year in the four years after the buyout and $682,874.8 per year in the following five years, while wages drop by $40-50 thousand per year for the entire decade after buyouts.

5.3 Serial Entrepreneurship

It is possible that increases in liquidity following buyouts lead owners to found new firms, as discussed in section 2. Large influxes of monetary resources following buyouts could free up capital for owners to invest in new projects. However, our results yield little evidence of serial entrepreneurship or owners founding new firms. Panel (c) of Figure 7 shows that there are large and persistent reductions in the level of owners' business income, even nine years after the capital gains associated with the buyout.

We further explore new business activity and serial entrepreneurship in Figure 8. Panel (a) shows extensive margin estimates of the impacts of PE acquisition on owners' wages and business income from the firm they were matched to in the year before the PE acquisition, and all other firms. The frequency of business income and wages from the original firm decline by 20% and 10-15% respectively, while there are initially larger increases in the frequency of wage income from other firms of 20% and business income from other firms of 10%. These effects both shrink towards 5% increases over a decade. Panel (b) shows the smaller level estimates of sole proprietor income, and wages from the original and different firms, while panel (c) shows business income from the original and different firms. The magnitude of average estimated effects on sole proprietor income are economically very small. The immediate $75,000 decline in wages from the target firm is partially offset by a roughly $37,000 increase in wages from other firms.

[16]While not directly comparable, the reductions in employment and wage earnings are similar to those found in the wealth shock literature. For example, Bulman et al. (2022) find that an additional $1 million increase in after-tax wealth leads to a $18,000 reduction in earnings.

However, in panel (c) we show there is very little income from other firms to offset the $400,00-$500,000 per year decline in business income. Indeed, in percentage terms, business income from the original firm declines by close to 100% in the year after the deal, with the effect size fading towards 50% after a decade, while business income from different firms also declines by roughly 50%. In sum, the results point to little evidence of successful serial entrepreneurship following buyouts for owners selling to PE relative to their matched controls.

The persistent negative employment and earnings effects are largely driven by older firm owners. Figure 9 and Appendix Table C.9 display estimates of the impacts of PE buyouts on owners by age. Figure 9, panel (a) shows the frequency of owners having no positive income from any source jumps by 30 pp following PE acquisitions for owners over 65, and only 10% for owners under 45. Panel (b) shows that the size of business income declines, and to some extent the spike in capital gains realizations in the year of the deal, are both consistent across age groups. Panel (c) shows that wages decline by more for most older owners, while a decade after buyouts we cannot reject zero wage changes for the youngest owners.[17] All three panels paint a similar portrait: older owners appear to leave the labor force and enjoy the quiet life, while owners under the age of 45 continue to work but do not start or invest in new businesses.

6 Heterogeneity by Deal Type

6.1 Private Equity Add-ons

We next explore heterogeneity by deal type, specifically focusing on a category of deals which has received significant attention among policy makers: add-on acquisitions or roll-ups. In a roll-up deal, a PE fund acquires multiple small companies in a market, which are then merged or integrated into the same existing portfolio company. Roll-ups can increase efficiency through economies of scale, but detractors worry that these types of deals can increase product market power, raising prices for consumers, and labor market power, lowering wages for workers.

[17]The muted long-run wage impacts on owners above the age of 65 for bought-out relative to not-bought-out owners likely reflects that control group owners are also very likely to retire and stop receiving wage income as they age into their 70s.

Approximately two-thirds of the deals in our sample are add-ons, in which a PE firm acquired an additional target. These tend to be smaller firms, so they account for slightly less than half of the employees in our sample. The frequency of add-ons has been increasing in recent years. Effects for add-ons may differ from other deals for several reasons, so employment and wage effects could go in either direction. First, there may be economies of scale. Redundant workers may be terminated, reassigned, or may have less bargaining power within the firm. On the other hand, add-ons could generate profits for firms by increasing market power. If some of these rents are shared with workers, this could increase wages. At the same time, labor market power might reduce wages for workers at the firm but also in the labor market more generally.

6.2 Add-ons, Managers, and Owners

Figure 10 displays estimates of β_e from equation (1), splitting employment and wage effects by the add-on status of deals, excluding owners. Panels (a) and (c) display aggregate and manager employment impacts, while panels (b) and (d) display aggregate and manager wage impacts. The results indicate that aggregate employment effects are marginally significantly greater for add-on deals for a couple years, with no significant employment effects for non-add-on deals. Indeed, after excluding owners and add-on deals, aggregate employment effects are marginally positive a decade after acquisitions. For earnings, we cannot reject equivalent impacts for add-on and other deals, though effects of add-on deals are statistically significantly different from zero for the first several years.

Appendix Figure D.1 displays similar estimates including owners. Differences between the employment and earnings impacts of add-ons and other deals are more pronounced when we include owners, demonstrating that owners play a substantial role in driving negative employment and wage effects of add-ons, particularly for managers. Nonetheless, we still observe statistically significant declines in manager employment in Figure 10, suggesting that PE is not just leading to owner separations paired with large liquidity realizations, but also eliminating some existing managers after add-on deals. We see similar patterns for high-wage and older workers in Appendix Figures D.2 and D.3. Negative employment impacts for highly affected groups are larger when owners

are included in the estimation sample, but persist after we exclude owners.

Table 12 presents further regression estimates of equation (1) excluding owners, splitting the sample into the most sensitive groups, older, higher-wage, and managerial employees. The top rows show earnings, the middle rows show employment and the bottom rows show long-term capital gains. In column 1, which displays estimates for the full worker sample excluding owners, during the first four-years post buyout we estimate a statistically significant $1,619.6 wage decline after add-ons, 51% larger than the insignificant $1,075.4 decline we estimate after other deals in column 5. In the following five years wage effects for add-on deals recede and are similar to wage effects for other deals, with neither effect statistically significant. The lack of positive wage gains following roll-up deals suggests that increased profits through greater market power are not passed on to workers or that that they are offset by increases in labor market power.

The aggregate estimates from deals that are not add-ons in Table 12, column 5 demonstrate the surprising fact that, once owners and add-on deals are excluded, we estimate no significant earnings or employment effects of PE buyouts on workers. Indeed, there are even positive effects on long-term capital gains and the coefficients on employment are positive between five and nine years post-acquisition.

Turning to older, higher-wage, and managerial employees, Table 12 shows more pronounced differences between the impacts of add-ons and other deals. Add-on deals generally have larger effects on these groups, with much more substantial employment effects in the pooled year 1-4 estimates, and larger wage effects for higher-wage and managerial employees. For example, employment declines by 1.9% for non-owner managers in the first four years after add-on acquisitions, but only by an insignificant 0.2% for non-owner managers after other acquisitions.

Appendix Table D.1 displays similar estimates including owners in the sample. When we include owners, estimated aggregate employment and wage effects are more negative for add-ons and other deals. Impacts on older, higher-wage, and managerial employees are also more pronounced, in this case with statistically significant declines in wages and employment after add-ons up to nine years following buyouts that are larger than estimates after other deals. Differences between add-ons and other deals are particularly stark for managers. Overall, owners play a larger role in driving the magnitudes of the estimated impacts of add-ons because the deals target smaller businesses.

Two channels through which roll-ups may affect workers are increasing firms' product market power, or through increasing firms' labor market power. We expect both channels to be relevant for non-tradable industries if present, although they would likely affect workers in opposite directions. If product market power increases, firm profits will rise and some of these additional earnings may be passed on to workers in the form of higher earnings. If labor market power increases, employers will be able to pay workers lower wages. Which channel dominates, if any, is an empirical question. On the other hand, we would mainly expect only the labor market power channel to be relevant in the tradable sector given the less local nature of the sector's product market.

Appendix Figure D.4 displays estimates of employment and wage impacts separately for deals in tradable and non-tradable industries, following the geography-based classification in Mian and Sufi (2014).[18] The results do not indicate a clear pattern, with effects generally statistically indistinguishable. That said, if taking the point estimates at face value, they indicate that employment goes down more for add-on deals in the non-tradable sector compared to the tradable sector. And among non add-on deals, employment rises more for the non-tradable sector. These results are not consistent with rents being shared with workers from increases in product market power. However, the fact that employment does decline more for non-tradable add-on deals is consistent with efficiency gains.

Finally, we consider heterogeneity in the effect of add-on acquisitions by (endogenous) changes in labor market concentration. Antitrust scrutiny usually focuses on deals that lead to high levels and cause large changes in concentration. To calculate concentration in each market and year, we compute the Herfindahl–Hirschman Index $HHI_{ict} = \sum_{f=1}^{N} s_{ifct}^2$, where s_{ifct}^2 is firm f's employment share in six-digit industry i in commuting zone c at time t. We then flag add-ons in markets with high levels of and changes in concentration as the deals in markets exceeding the 75th HHI percentile that experience above median positive changes in HHI from the year before to the year after PE deals.

Appendix Figure D.6 shows separate estimates of the effects of add-on deals that do and do not occur in markets that meet these concentration criteria. Taking the point es-

[18] Appendix Figure D.5 repeats the analysis for the most sensitive groups. However, these results generally lack precision and have wide confidence intervals.

timates at face value, we see more negative earnings effects in the first several years post buyout for deals associated with larger levels of and changes in concentration, which is what we would expect if monopsony power is an important channel driving impacts on workers. However, confidence intervals are large, and we do not find statistically or economically significant differences in employment effects. We thus treat our results as suggestive of some small potential monopsony power effects for rollup deals.

We can draw three implications from the results in this section. First, once owners and add-on deals are excluded, for rank-and-file employees, PE buyouts have little or even positive effects on labor market outcomes. Second, the small employment and wage effects of non-add-on deals are concentrated amongst owners, managers, high-earners and older employees. For the latter three groups, we observe effects even for those without substantial ownership stakes. Third, negative employment effects are concentrated among these three groups for add-on deals, but perhaps most notable, we do not see differences in earnings effects overall (or for any groups once owners are excluded), indicating that add-on deals are not particularly harmful to workers. This suggests that concerns about increased labor market concentration from rollups may be overstated.

7 Conclusion

This paper builds a novel administrative dataset to demonstrate that much of the previously documented labor market effects of PE acquisitions are driven by owners leaving the firm following liquidity events substituting for leisure. Otherwise, small effects of PE buyouts are concentrated among relatively affluent groups: managers, high-earners and older employees, all of which significantly overlap. For the vast majority of employees, PE buyouts have at most modest negative effects on employment and earnings. Even add-on acquisitions appear to have limited effects on earnings.

While our study documents new facts using administrative data, there remain several promising avenues for future research. First, future work can try to quantify the efficiency benefits of removing entrenched management. Another fruitful avenue for further research is on add-on acquisitions. We rely on a classification of these deals provided by Pitchbook. Uncovering the full ownership structure of PE funds may allow for

the construction of a more complete measure of roll-up activity.

Finally, our results have implications for policy. Regulators have, on average, little reason to fear large negative labor market impacts of PE acquisitions on workers, even for add-on acquisitions. However, future work should also consider the impacts of PE acquisitions on product markets to better quantify the overall welfare consequences of these transactions (Chevalier 1995; Eaton et al. 2020; Fracassi et al. 2022; Asil et al. 2024).

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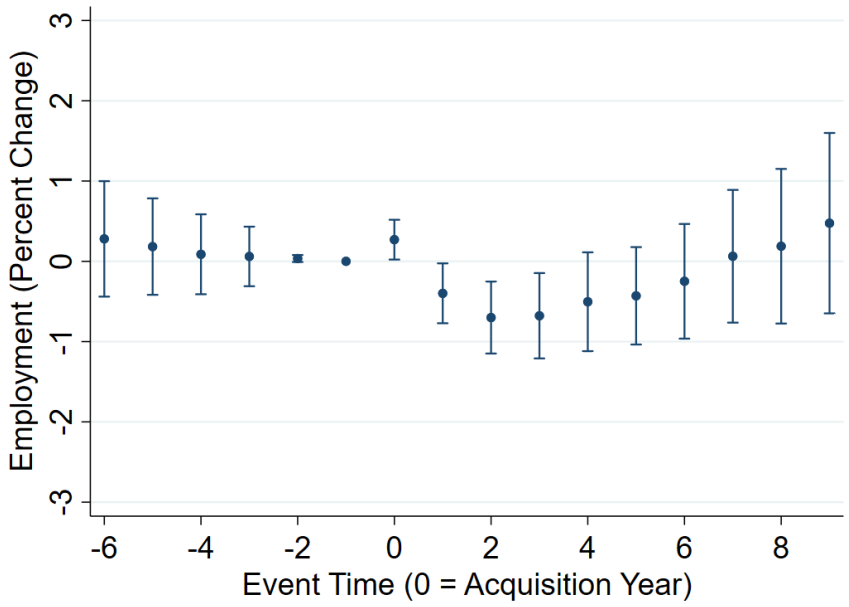
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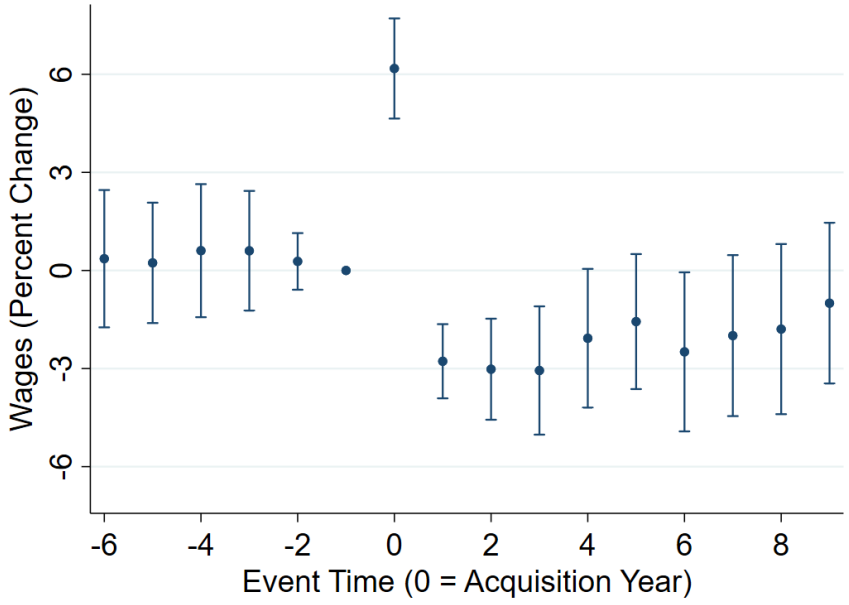
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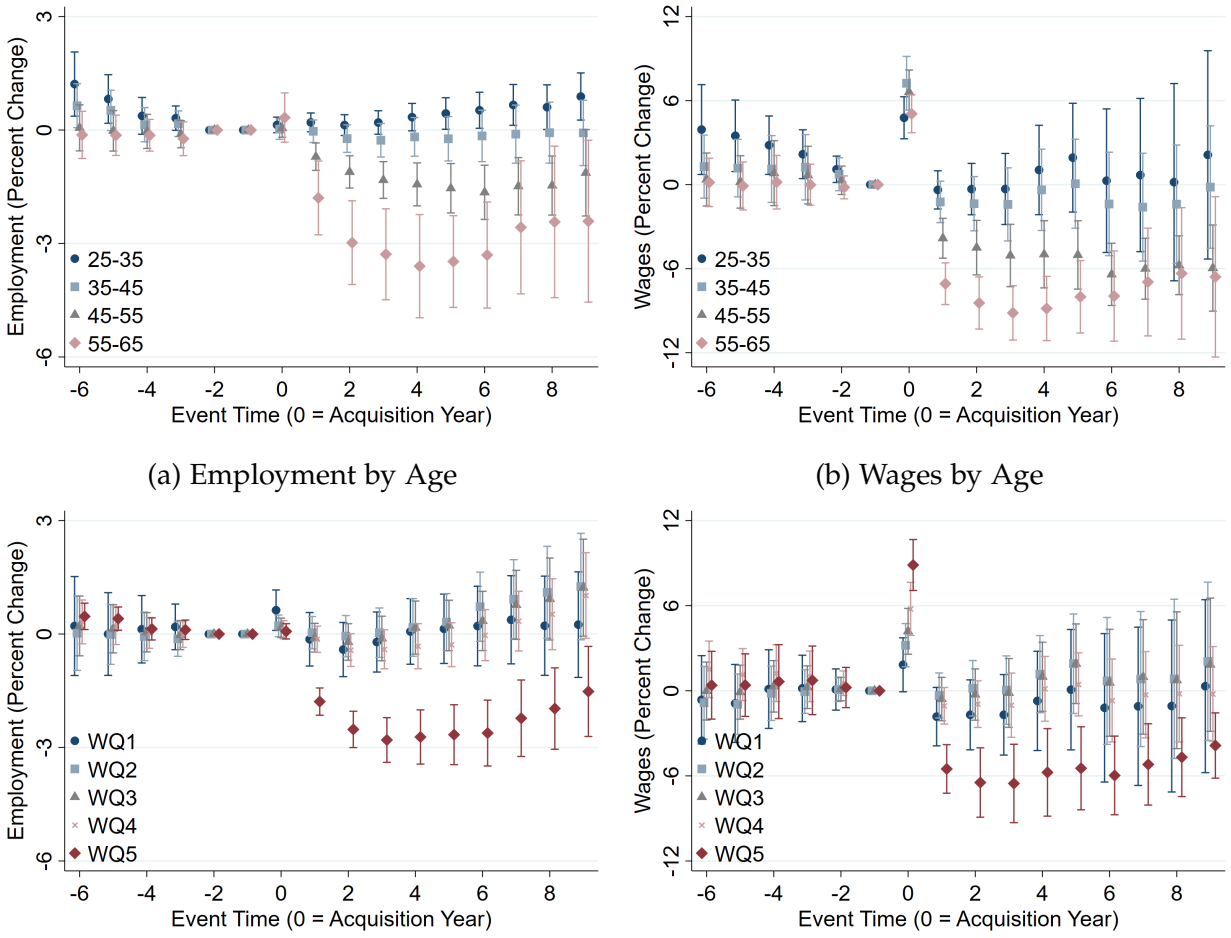
(a) Employment



(b) Wages

Figure 1: Impacts of PE Acquisitions

Notes: This figure displays difference-in-difference estimates of the β_e coefficients from equation (1) using a dummy variable equal to 1 if a worker is employed as the outcome in Panel (a), and wages as the outcome in Panel (b). To scale estimates into percent changes we divide coefficient estimates by the average value of the outcome variable for workers employed at acquired firms in the year before the acquisition. 95% confidence intervals are constructed from standard errors clustered at the firm level.



(a) Employment by Age

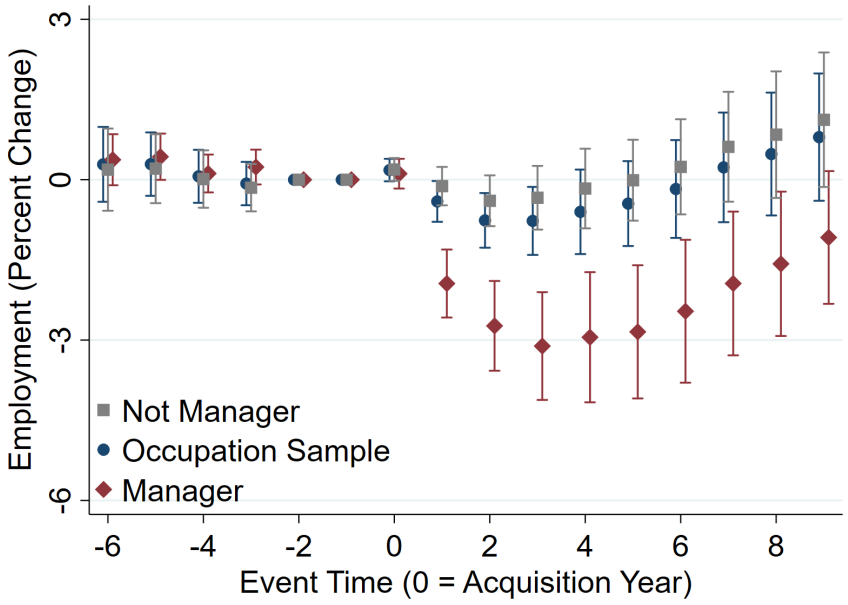
(b) Wages by Age

(c) Employment by Wage Quintile

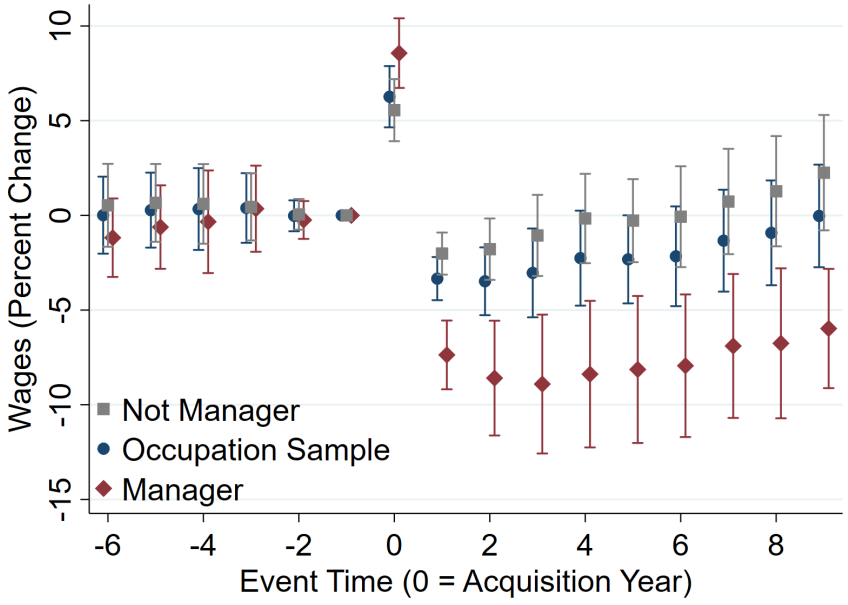
(d) Wages by Wage Quintile

Figure 2: Impacts of PE Acquisitions by Age or Wage Quintile

Notes: This figure displays difference-in-difference estimates of the β_e coefficients from equation (1) using a dummy variable equal to 1 if a worker is employed as the outcome in Panels (a) and (c), and wages as the outcome in Panels (b) and (d). Each series estimates equation (1) using only workers in the specified age range or wage quintile in the year before the PE acquisition. To scale estimates into percent changes we divide coefficient estimates by the average value of the outcome variable for workers employed at acquired firms in the specified age range in the year before the acquisition. 95% confidence intervals are constructed from standard errors clustered at the firm level.



(a) Employment



(b) Wages

Figure 3: Impacts of PE Acquisitions by Managerial Status

Notes: This figure displays difference-in-difference estimates of the β_e coefficients from equation (1) using a dummy variable equal to 1 if a worker is employed as the outcome in Panel (a), and wages as the outcome in Panel (b). Each series utilizes a different estimation sample, restricting to the set of workers we can measure occupation for, and then sub-setting to workers in managerial and non-managerial occupations. To scale estimates into percent changes we divide coefficient estimates by the average value of the outcome variable for workers employed at acquired firms in the relevant estimation sample in the year before the acquisition. 95% confidence intervals are constructed from standard errors clustered at the firm level.



(a) Manager Employment

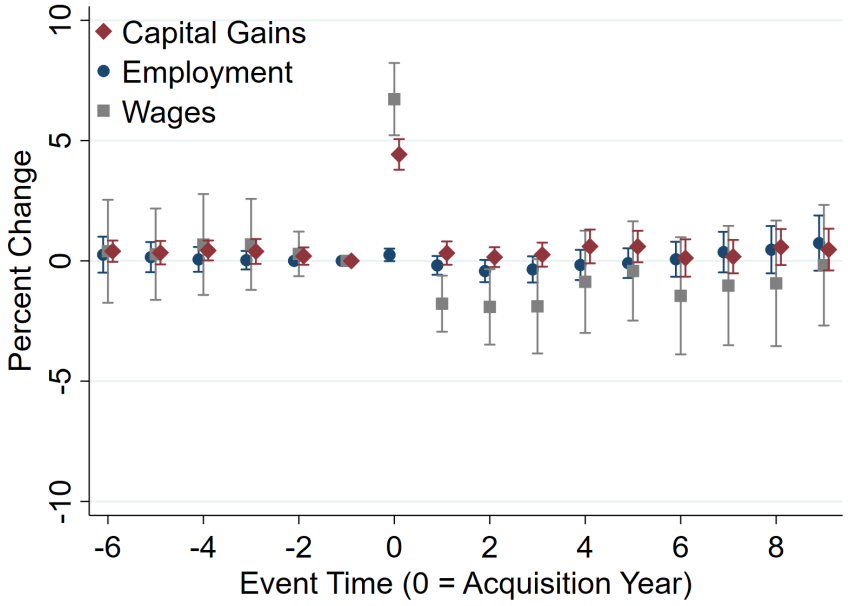
(b) Manager Wages

(c) Not Manager Employment

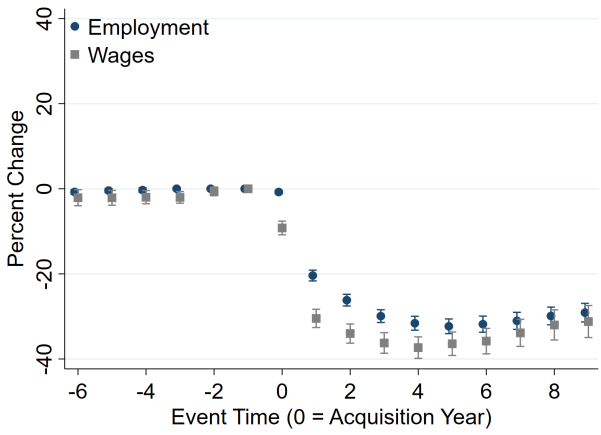
(d) Not Manager Wages

Figure 4: Impacts of PE Acquisitions by Managerial Status, Age, and Wage Quintile

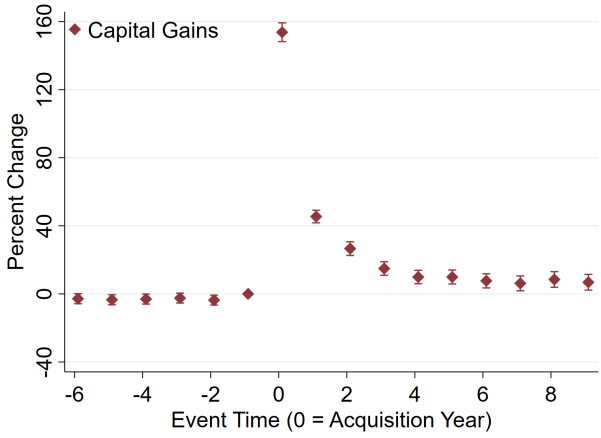
Notes: This figure displays difference-in-difference estimates of average post-reform coefficient estimates from equation (1), replacing post-match year indicators with an indicator for an observation being in any post-match year, and using a dummy variable equal to 1 if a worker is employed as the outcome in Panels (a) and (c), and wages as the outcome in Panels (b) and (d). Panels (a) and (b) are estimated on our sample of workers in managerial occupations, while panels (c) and (d) are estimated on workers in all other occupations. To scale estimates into percent changes we divide coefficient estimates by the average value of the outcome variable for workers employed at acquired firms in the relevant estimation sample in the year before the acquisition. 95% confidence intervals are constructed from standard errors clustered at the firm level.



(a) Non-Owners



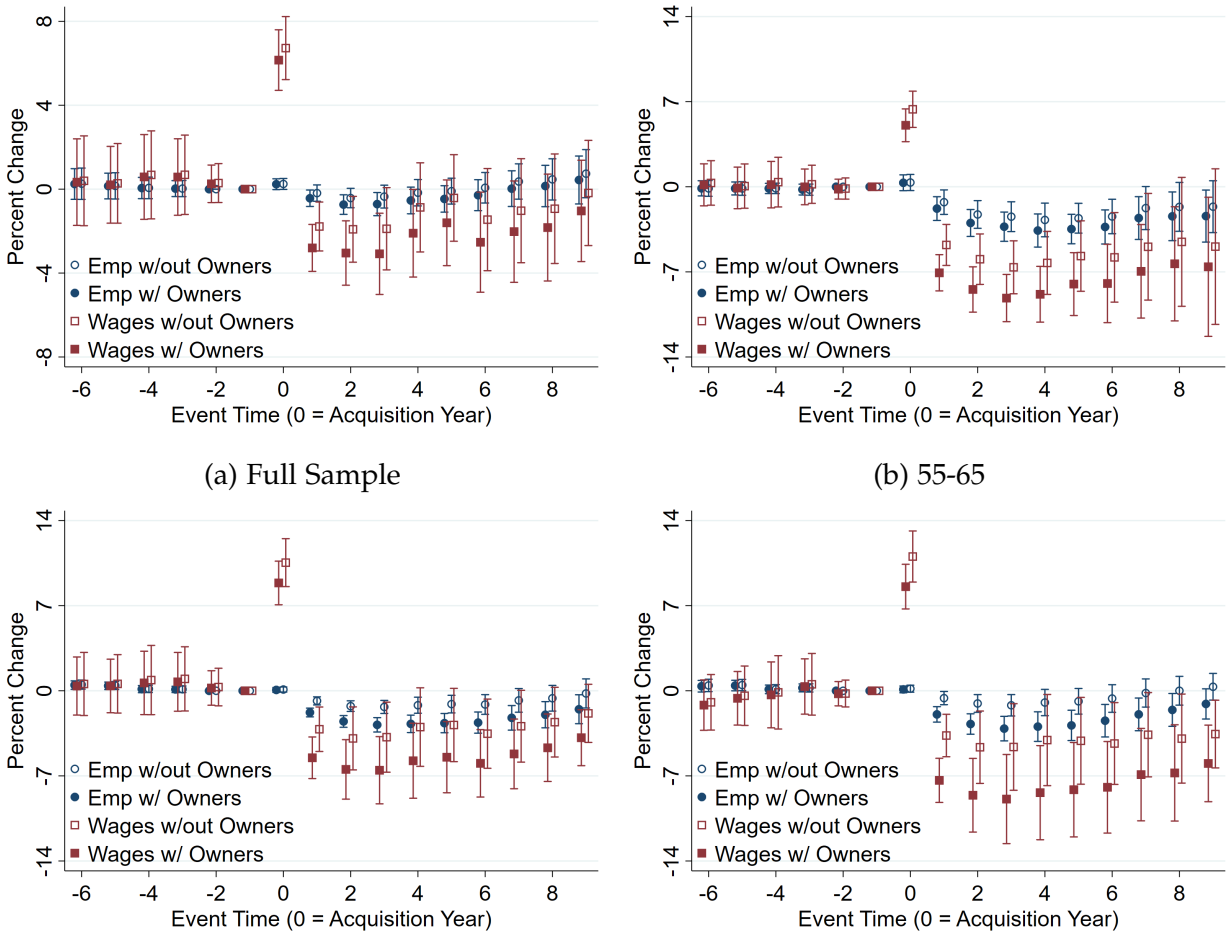
(b) Owners Employment and Wages



(c) Owners Capital Gains

Figure 5: Impacts of PE Acquisitions On Owner-Employees and Non-Owner-Employees

Notes: This figure displays difference-in-difference estimates of the β_e coefficients from equation (1) using long-term capital gains, a dummy variable equal to 1 if a worker is employed, and wages as outcome variables. Panel (a) displays estimates from a regression on all non-owners in our worker sample, while panels (b) and (c) displays estimates from a regression on all owners in our worker sample. To scale estimates into percent changes we divide coefficient estimates by the average value of the outcome variable for workers employed at acquired firms in the relevant estimation sample in the year before the acquisition. Capital gains are scaled as a fraction of base year wages because they are large and infrequent. 95% confidence intervals are constructed from standard errors clustered at the firm level.



(a) Full Sample

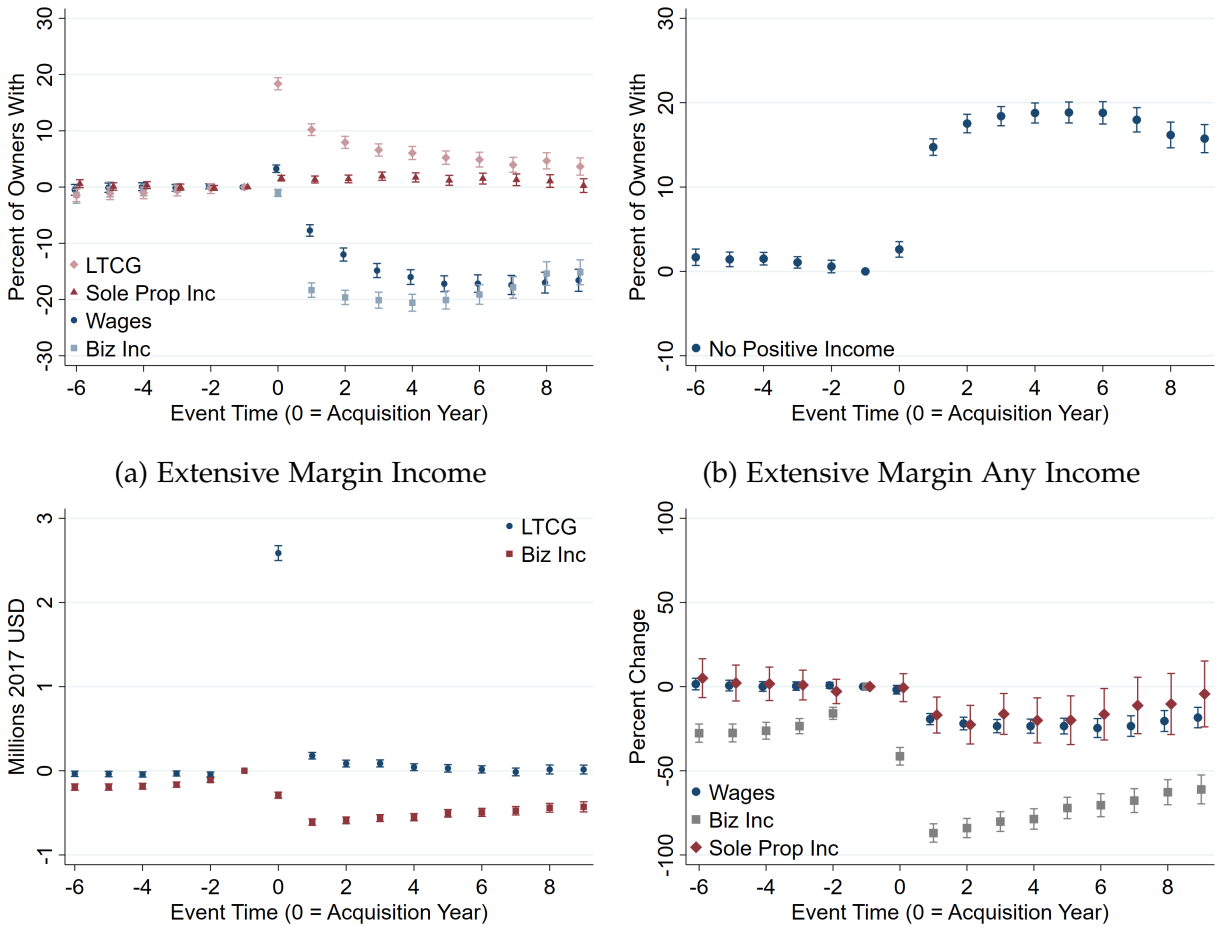
(b) 55-65

(c) Top Wage Quintile

(d) Managers

Figure 6: Impacts of PE Acquisitions With and Without Owner-Employees

Notes: This figure displays difference-in-difference estimates of the β_e coefficients from equation (1) using a dummy variable equal to 1 if a worker is employed and wages as outcome variables. Each panel displays four series of estimates, wages and employment including and excluding owners from the worker panel. Each panel displays these estimates for a different subsample of our data. Panel (a) uses the full worker sample, panel (b) uses workers ages 55-65, panel (c) uses top wage quintile workers, and panel (d) uses workers in managerial occupations. To scale estimates into percent changes we divide coefficient estimates by the average value of the outcome variable for workers employed at acquired firms in the relevant estimation sample in the year before the acquisition. 95% confidence intervals are constructed from standard errors clustered at the firm level.



(a) Extensive Margin Income

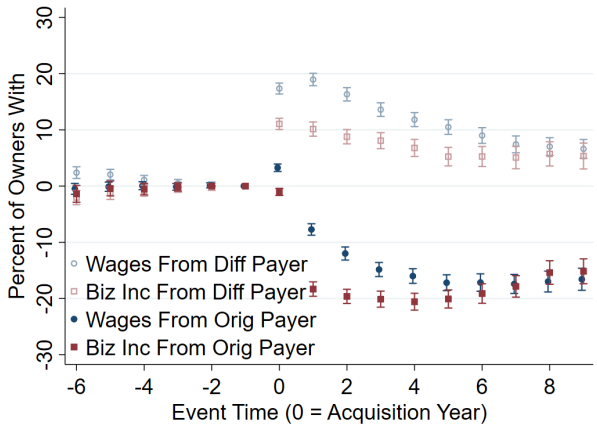
(b) Extensive Margin Any Income

(c) Level Change in Compensation

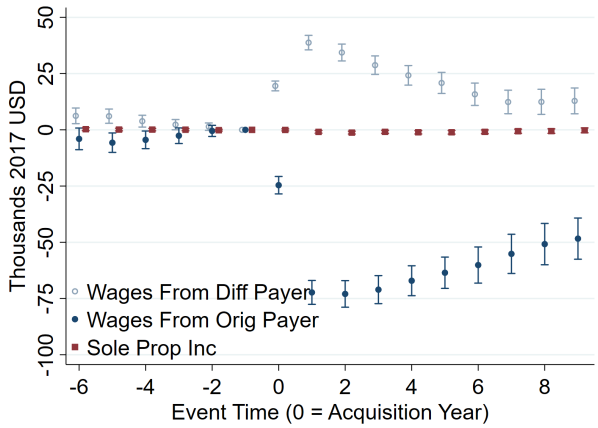
(d) Percent Change in Compensation

Figure 7: Impacts of PE Acquisitions on Owners

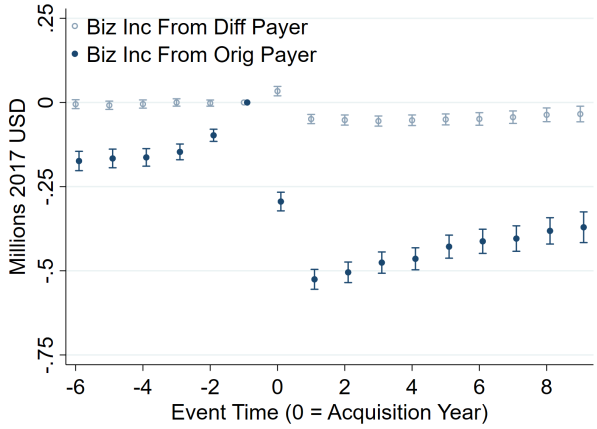
Notes: This figure displays difference-in-difference estimates of the β_e coefficients from equation (1) using our owner sample. Panels (a) and (b) displays estimates using dummy variables equal to 1 if an owner receives each type of income as outcome variables. Panel (c) displays estimates using variable levels as outcome variables. Panel (d) scales estimates into percent changes by dividing level coefficient estimates by the average value of the outcome for owners of acquired firms in the year before the acquistion. "Biz Inc" is ordinary business income, "LTCG" is long-term capital gains, and "Sole Prop Inc" is sole proprietorship income. 95% confidence intervals are constructed from standard errors clustered at the firm level.



(a) Extensive Margin Income



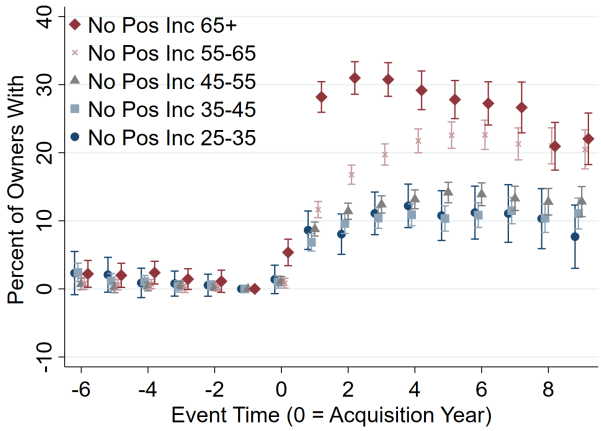
(b) Wages and Sole Prop Income



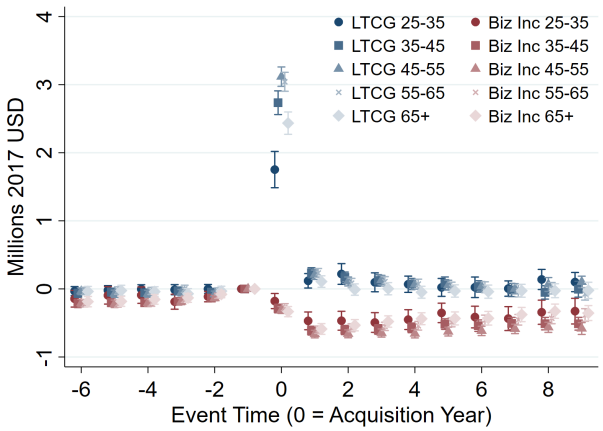
(c) Business Income

Figure 8: Impacts of PE Acquisitions on Owners New Business Activity

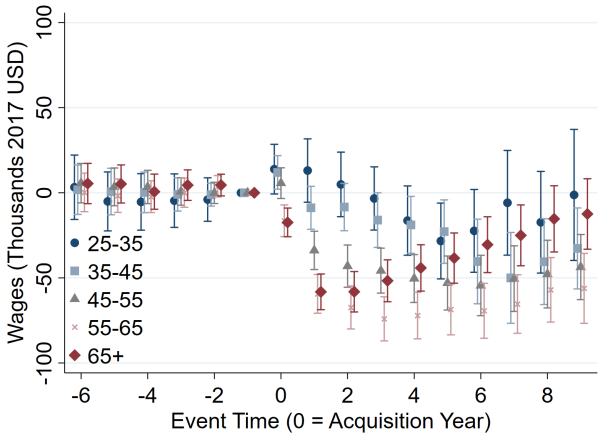
Notes: This figure displays difference-in-difference estimates of the β_e coefficients from equation (1) using our owner sample. The separate series in each panel are estimated using compensation outcome variables making distinctions between owner's compensation from the acquired entity and all other entities. Panel (a) displays estimates using dummy variables equal to 1 if an owner receives each type of income as outcome variables. Panels (b) and (c) display estimates using variable levels as outcome variables. 95% confidence intervals are constructed from standard errors clustered at the firm level.



(a) Extensive Margin Income



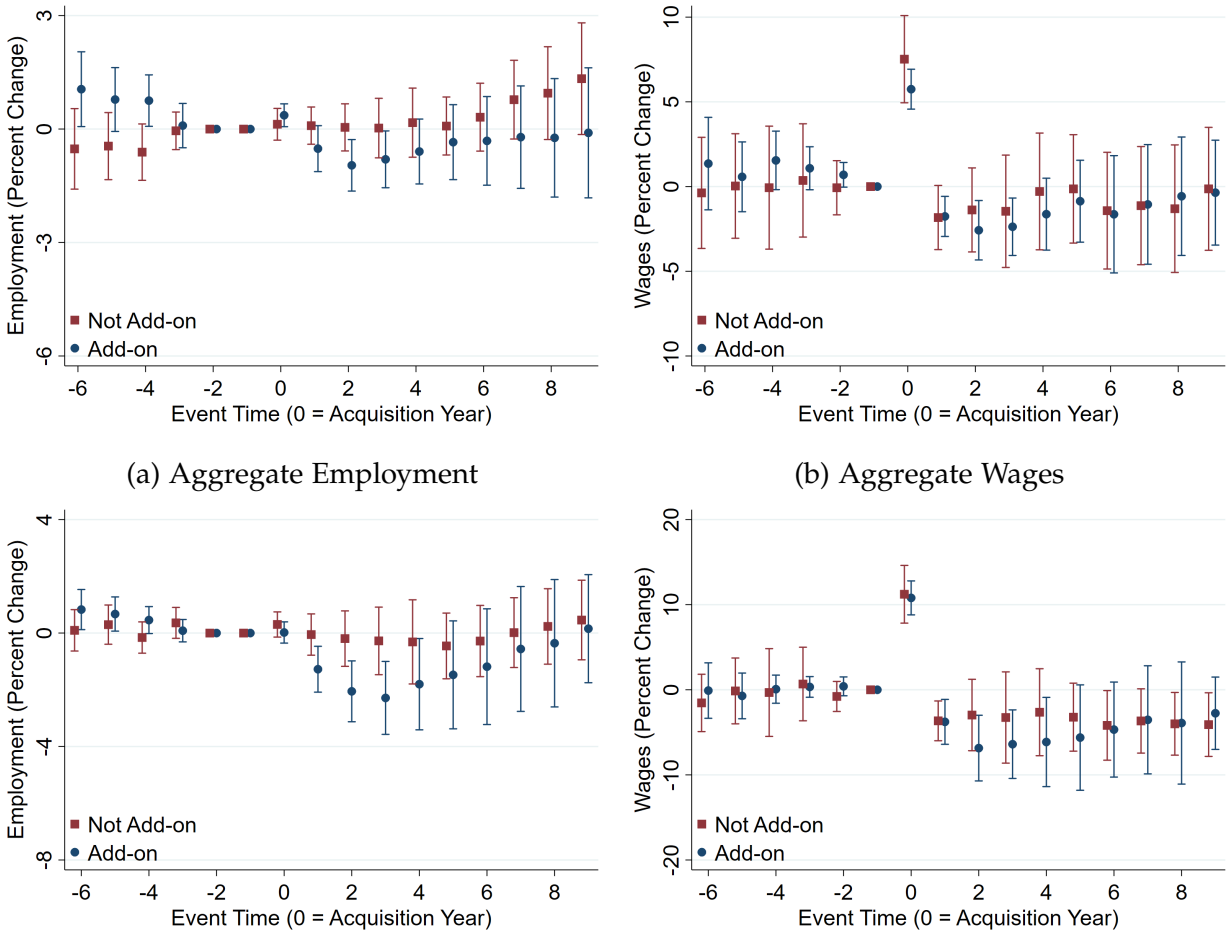
(b) Level Change in Biz Inc and LTCG



(c) Level Change in Wages

Figure 9: Impacts of PE Acquisitions on Owners by Age

Notes: This figure displays difference-in-difference estimates of the β_e coefficients from equation (1) using our owner sample. Panels (a) displays estimates using a dummy variable equal to 1 if an owner receives any type of income as an outcome variable. Panels (b) and (c) display estimates using variable levels as outcomes. "Biz Inc" is ordinary business income, and "LTCG" is long-term capital gains. 95% confidence intervals are constructed from standard errors clustered at the firm level.



(a) Aggregate Employment

(b) Aggregate Wages

(c) Manager Employment

(d) Manager Wages

Figure 10: Impacts of Add-on PE Acquisitions Without Owners

Notes: This figure displays difference-in-difference estimates of the β_e coefficients from equation (1) using a dummy variable equal to 1 if a worker is employed as the outcome in Panels (a) and (c) and wages as the outcome in panels (b) and (d). Each panel splits workers into groups that are and are not associated with an add-on acquisition. Panels (a) and (b) use the full sample of workers without owners, while panels (c) and (d) restrict to managers without owners. To scale estimates into percent changes we divide coefficient estimates by the average value of the outcome variable for workers employed at acquired firms in the relevant estimation sample in the year before the acquisition. 95% confidence intervals are constructed from standard errors clustered at the firm level.

Table 1: PE Deal Summary Statistics

Deal Type	Deal Count	Worker Count	Worker Snapshot Count
Matched Deals	11,155	2,007,409	1,318,573
Manufacturing	2,769	545,744	419,055
Service	2,742	365,587	270,124
Wholesale Trade	1,259	126,578	92,529
Health	723	105,438	73,924
Add-on	7,473	955,401	632,087

Notes: This table reports summary statistics for all private equity deals identified in the tax data, for which we succesfully find a matched control, where both the acquired firm and the matched control have more than 10 employees in the year of the buyout.

Table 2: Worker Sample Summary Statistics

	Mean	Std Dev	P10	P50	P90	Observations	Workers
Panel A: Intensive Margin							
Wages	76.1	84.7	2.6	53.1	159.2	27,841,160	1,956,890
Capital Gains	2.2	38.8	0.0	0.0	0.0	27,841,160	1,956,890
Sole Proprietor Income	0.6	9.6	0.0	0.0	0.0	27,841,160	1,956,890
Business Income	1.6	20.2	0.0	0.0	0.0	27,841,160	1,956,890
Unemployment	0.5	2.5	0.0	0.0	0.0	27,841,160	1,956,890
Disability	0.2	2.0	0.0	0.0	0.0	27,841,160	1,956,890
Social Security	1.1	5.1	0.0	0.0	0.0	27,841,160	1,956,890
Taxes	18.6	39.0	0.0	6.9	40.3	26,848,860	1,956,762
Age	43	20	28	43	59	27,757,186	1,950,896
Panel B: Extensive Margin							
Men	0.65					27,741,452	1,949,796
Managers	0.15					12,738,033	917,723
Add-on	0.48					14,391,697	1,010,253
Employed	0.91					27,841,160	1,956,890
Capital Gains	0.08					27,841,160	1,956,890
Sole Proprietor Income	0.04					27,841,160	1,956,890
Unemployment	0.08					27,841,160	1,956,890
Disability	0.01					27,841,160	1,956,890
Social Security	0.06					27,841,160	1,956,890

Notes: This table reports summary statistics for our entire worker panel data set spanning 1999-2023. Statistics are reported in thousands of 2017 US dollars. To preserve taxpayer anonymity, percentile statistics are reported as the means of all observations in the (P-1,P+1)th percentiles.

Table 3: Owner Sample Summary Statistics

	Mean	Std Dev	P10	P50	P90	Observations	Owners
Panel A: Intensive Margin							
Business Income	385.4	1,245.3	−64.5	26.7	1,107.1	654,856	47,545
Capital Gains	287.6	1,397.5	−33.9	0.0	371.2	654,856	47,545
Wages	164.3	300.1	0.0	58.0	421.1	654,856	47,545
Sole Proprietor Income	5.5	31.7	0.0	0.0	4.6	654,856	47,545
Age	55	49	37	53	71	623,762	45,294
Panel B: Extensive Margin							
Men	0.77					620,715	45,073
No Positive Income	0.15					654,856	47,545

Notes: This table reports summary statistics for our entire owner panel data set spanning 1999-2023. Statistics are reported in thousands of 2017 US dollars. To preserve taxpayer anonymity, percentile statistics are reported as the means of all observations in the (P-1,P+1)th percentiles.

Table 4: Impacts of PE Acquisitions on Workers

	(1) Employment	(2) Wages	(3) LTCG	(4) Sched C Inc	(5) UI	(6) DI	(7) OASI
Panel A: Baseline Specification							
β_0	0.0024	5136.9	8250.4	68.1	19.7	−2.7	−19.3
	(0.0013)	(615.1)	(444.6)	(15.8)	(66.5)	(3.3)	(10.3)
$\beta_{1\text{-}4}$	−0.0061	−2316.7	991.3	56.9	115.8	0.0	−59.4
	(0.0025)	(654.6)	(183.0)	(25.5)	(25.7)	(11.7)	(34.7)
$\beta_{5\text{-}9}$	−0.0008	−1542.1	581.5	43.9	37.1	5.9	−155.9
	(0.0041)	(906.8)	(262.4)	(41.6)	(40.6)	(26.1)	(87.1)
Panel B: Industry by Year Fixed Effects							
β_0	0.0021	5342.5	8386.0	80.3	21.0	−3.8	−22.5
	(0.0013)	(639.4)	(407.6)	(14.8)	(65.4)	(2.5)	(10.0)
$\beta_{1\text{-}4}$	−0.0060	−2224.8	1001.3	76.5	109.3	−2.2	−61.2
	(0.0024)	(666.8)	(170.9)	(22.3)	(25.5)	(10.2)	(32.5)
$\beta_{5\text{-}9}$	−0.0003	−1391.4	630.0	74.6	28.0	1.4	−154.3
	(0.0041)	(934.6)	(260.4)	(38.0)	(40.6)	(25.5)	(77.0)
Obs	27,841,160	27,841,160	27,841,160	27,841,160	27,841,160	27,841,160	27,841,160
Clusters	20,075	20,075	20,075	20,075	20,075	20,075	20,075
Pre-Deal Mean	1.0000	83,487.4	934.2	195.7	223.4	16.8	485.2

Notes: This table reports difference in difference estimates of β from equation (1), replacing post-match year indicators with indicators for an observation being in the acquisition year, the first 4 years after the acquisition, and years 5–9 after the acquisition. The estimation sample includes our entire worker panel data set. Estimates in panel B add 2 digit NAICS industry by year fixed effects to the baseline specification in panel A. Standard errors are clustered at the firm level and reported in parentheses. The pre-reform mean is the average value of the outcome variable in each column for workers employed at acquired firms in the year before the acquisition.

Table 5: Impacts of PE Acquisitions on Workers by Age Group

Outcome		(1) 25-35	(2) 35-45	(3) 45-55	(4) 55-65
Wages	β_0	3104.0	6594.6	6416.2	4635.0
		(497.1)	(891.2)	(777.6)	(631.0)
	$\beta_{1–4}$	−11.4	−1011.7	−4444.2	−7668.4
		(683.0)	(940.3)	(927.6)	(847.8)
	$\beta_{5–9}$	675.9	−802.8	−5622.3	−6675.2
		(1708.1)	(1660.3)	(1003.6)	(1710.3)
Employment	β_0	0.0013	0.0002	0.0005	0.0033
		(0.0011)	(0.0014)	(0.0013)	(0.0033)
	$\beta_{1–4}$	0.0021	−0.0018	−0.0114	−0.0289
		(0.0014)	(0.0020)	(0.0022)	(0.0055)
	$\beta_{5–9}$	0.0060	−0.0014	−0.0147	−0.0291
		(0.0025)	(0.0036)	(0.0034)	(0.0081)
LTCG	β_0	1392.0	5893.1	10606.2	16404.5
		(115.0)	(434.4)	(722.3)	(913.9)
	$\beta_{1–4}$	−5.7	728.5	1322.1	2461.2
		(136.0)	(210.7)	(282.9)	(349.9)
	$\beta_{5–9}$	−141.2	677.4	875.9	1134.2
		(164.0)	(289.4)	(420.1)	(436.3)
Obs		6,439,226	7,325,744	7,198,396	4,451,920
Clusters		19,504	19,801	19,611	18,692
Pre-Deal Wage Mean		64,988.7	91,090.8	97,051.0	91,668.1
Pre-Deal Employment Mean		1.0000	1.0000	1.0000	1.0000
Pre-Deal LTCG Mean		228.1	707.5	1,031.7	1,545.2

Notes: This table reports difference in difference estimates of β from equation (1), replacing post-match year indicators with indicators for an observation being in the acquisition year, the first 4 years after the acquisition, and years 5–9 after the acquisition. We assign workers to an age group based on their age in the year before acquisitions and each column displays estimates from regressions performed separately on workers in each age group. Standard errors are clustered at the firm level and reported in parentheses.

Table 6: Impacts of PE Acquisitions on Workers by Wage Quintile

Outcome		(1) WQ1	(2) WQ2	(3) WQ3	(4) WQ4	(5) WQ5
Wages	β_0	660.9	1690.4	2822.7	5119.0	15083.5
		(350.3)	(408.6)	(555.4)	(845.7)	(1557.9)
	$\beta_{1–4}$	−542.1	126.3	−19.8	−658.0	−10333.6
		(478.6)	(541.1)	(650.4)	(770.3)	(2077.0)
	$\beta_{5–9}$	−217.7	663.1	811.8	−151.1	−8740.4
		(935.9)	(1179.6)	(1257.2)	(1221.0)	(2280.3)
Employment	β_0	0.0063	0.0022	0.0016	0.0011	0.0007
		(0.0027)	(0.0015)	(0.0013)	(0.0012)	(0.0010)
	$\beta_{1–4}$	−0.0018	0.0004	−0.0007	−0.0032	−0.0245
		(0.0037)	(0.0029)	(0.0026)	(0.0023)	(0.0026)
	$\beta_{5–9}$	0.0023	0.0081	0.0064	0.0024	−0.0226
		(0.0056)	(0.0050)	(0.0044)	(0.0037)	(0.0046)
LTCG	β_0	735.9	549.6	1159.8	2692.9	35408.9
		(83.6)	(85.1)	(164.4)	(285.5)	(1785.3)
	$\beta_{1–4}$	−70.0	−82.2	−151.9	−53.8	5215.5
		(66.1)	(82.5)	(121.5)	(170.3)	(669.1)
	$\beta_{5–9}$	−54.9	35.9	−132.7	−76.0	3081.9
		(89.5)	(131.0)	(208.0)	(293.1)	(752.5)
Obs		5,456,669	5,567,213	5,568,952	5,562,552	5,685,773
Clusters		20,011	20,039	20,034	20,022	20,075
Pre-Deal Wage Mean		36,093.8	52,580.7	67,333.8	88,624.1	170,092.9
Pre-Deal Employment Mean		1.0000	1.0000	1.0000	1.0000	1.0000
Pre-Deal LTCG Mean		232.8	133.5	161.1	328.1	3,743.6

Notes: This table reports difference in difference estimates of β from equation (1), replacing post-match year indicators with indicators for an observation being in the acquisition year, the first 4 years after the acquisition, and years 5–9 after the acquisition. We assign workers to a quintile of the worker wage distribution in the year before acquisitions and each column displays estimates from regressions performed separately on workers in each quintile of the distribution. Standard errors are clustered at the firm level and reported in parentheses.

Table 7: Impacts of PE Acquisitions on Workers by Managerial Status

Outcome		(1) Occupation Sample	(2) Manager	(3) Not Manager
Wages	β_0	5534.3	11294.4	4445.6
		(728.8)	(1236.3)	(669.0)
	$\beta_{1–4}$	−2701.9	−10954.4	−1039.5
		(815.0)	(1991.2)	(676.3)
	$\beta_{5–9}$	−1323.9	−9580.9	496.5
		(1119.0)	(2421.5)	(1039.6)
Employment	β_0	0.0018	0.0011	0.0019
		(0.0011)	(0.0014)	(0.0011)
	$\beta_{1–4}$	−0.0064	−0.0267	−0.0026
		(0.0028)	(0.0045)	(0.0026)
	$\beta_{5–9}$	0.0010	−0.0208	0.0049
		(0.0048)	(0.0064)	(0.0048)
LTCG	β_0	10475.3	31948.1	6340.2
		(653.7)	(2098.5)	(461.6)
	$\beta_{1–4}$	1377.0	5580.0	519.2
		(251.1)	(638.9)	(237.4)
	$\beta_{5–9}$	1063.8	3754.7	429.3
		(301.6)	(677.1)	(277.7)
Obs		12,738,033	1,912,256	10,825,777
Clusters		18,807	16,501	18,745
Pre-Deal Wage Mean		88,320.6	131,821.5	79,999.2
Pre-Deal Employment Mean		1.0000	1.0000	1.0000
Pre-Deal LTCG Mean		1,195.2	3,619.0	731.5

Notes: This table reports difference in difference estimates of β from equation (1), replacing post-match year indicators with indicators for an observation being in the acquisition year, the first 4 years after the acquisition, and years 5–9 after the acquisition. We assign workers to an occupation in the year before acquisitions. Column 1 displays estimates from a regression performed on all workers that we can assign an occupation code to. Columns 2 and 3 display estimates from separate regressions splitting workers with occupations into managers and not managers. Standard errors are clustered at the firm level and reported in parentheses.

Table 8: Impacts of PE Acquisitions on Working Owners and Non-Owners

	(1)	(2)	(3)	(4)	(5)	(6)
	Owners			Not Owners		
	Wages	Emp	LTCG	Wages	Emp	LTCG
β_0	−23,343.9	−0.0076	389,678.7	5,474.1	0.0025	3,602.0
	(2,074.3)	(0.0024)	(7,140.2)	(624.1)	(0.0013)	(264.7)
$\beta_{1–4}$	−87,227.2	−0.2686	62,675.3	−1,329.0	−0.0029	266.1
	(2,792.6)	(0.0066)	(4,266.6)	(649.9)	(0.0025)	(165.2)
$\beta_{5–9}$	−87,015.9	−0.3107	20,839.8	−672.0	0.0026	311.9
	(3,781.4)	(0.0088)	(4,644.1)	(902.9)	(0.0040)	(265.1)
Obs	341,673	341,673	341,673	27,499,486	27,499,486	27,499,486
Clusters	11,284	11,284	11,284	20,070	20,070	20,070
R^2	0.67	0.51	0.41	0.73	0.36	0.31
Pre-Deal Mean	253,454.2	1.0000	37,810.7	81,410.1	1.0000	483.5

Notes: This table reports difference in difference estimates of β from equation (1), replacing post-match year indicators with indicators for an observation being in the acquisition year, the first 4 years after the acquisition, and years 5–9 after the acquisition. The estimation sample in the first 3 columns includes all owners in our workers panel, while the estimation sample in columns 4-6 includes all non-owners in our workers panel. Standard errors are clustered at the firm level and reported in parentheses. The pre-reform mean is the average value of the outcome variable in each column for workers at acquired firms in the year before the acquisition.

Table 9: Impacts of PE Acquisitions on Workers With and Without Owners

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		With Owners				Without Owners			
Outcome		Full	WQ5	55-65	Manager	Full	WQ5	55-65	Manager
Wages	β_0	5,136.9	15,083.5	4,635.0	11,294.4	5,474.1	17,307.0	5,560.2	13,758.1
		(615.1)	(1,557.9)	(631.0)	(1,236.3)	(624.1)	(1,651.5)	(663.7)	(1,335.1)
	$\beta_{1–4}$	−2,316.7	−10,333.6	−7,668.4	−10,954.4	−1,329.0	−5,720.1	−5,144.4	−5,304.9
		(654.6)	(2,077.0)	(847.8)	(1,991.2)	(649.9)	(2,105.1)	(911.8)	(1,808.5)
	$\beta_{5–9}$	−1,542.1	−8,740.4	−6,675.2	−9,580.9	−672.0	−4,605.2	−4,581.0	−4,920.4
		(906.8)	(2,280.3)	(1,710.3)	(2,421.5)	(902.9)	(2,255.3)	(1,851.5)	(2,042.0)
Employment	β_0	0.0024	0.0007	0.0033	0.0011	0.0025	0.0010	0.0035	0.0017
		(0.0013)	(0.0010)	(0.0033)	(0.0014)	(0.0013)	(0.0011)	(0.0034)	(0.0015)
	$\beta_{1–4}$	−0.0061	−0.0245	−0.0289	−0.0267	−0.0029	−0.0115	−0.0217	−0.0095
		(0.0025)	(0.0026)	(0.0055)	(0.0045)	(0.0025)	(0.0024)	(0.0057)	(0.0039)
	$\beta_{5–9}$	−0.0008	−0.0226	−0.0291	−0.0208	0.0026	−0.0082	−0.0208	−0.0034
		(0.0041)	(0.0046)	(0.0081)	(0.0064)	(0.0040)	(0.0044)	(0.0081)	(0.0054)
LTCG	β_0	8,250.4	35,408.9	16,404.5	31,948.1	3,602.0	15,181.5	6,416.3	11,349.0
		(444.6)	(1,785.3)	(913.9)	(2,098.5)	(264.7)	(1,009.2)	(497.5)	(950.8)
	$\beta_{1–4}$	991.3	5,215.5	2,461.2	5,580.0	266.1	1,932.7	740.5	2,011.7
		(183.0)	(669.1)	(349.9)	(638.9)	(165.2)	(601.3)	(289.7)	(434.9)
	$\beta_{5–9}$	581.5	3,081.9	1,134.2	3,754.7	311.9	1,858.2	435.7	2,181.0
		(262.4)	(752.5)	(436.3)	(677.1)	(265.1)	(782.0)	(418.7)	(607.1)
Obs		27,841,160	5,685,773	4,451,920	1,912,256	27,499,486	5,392,906	4,344,591	1,813,885
Clusters		20,075	20,075	18,692	16,501	20,070	19,798	18,464	15,871
Pre-Deal Wage Mean		83,487.4	170,092.9	91,668.1	131,821.5	81,410.1	164,196.4	87,245.7	124,591.6
Pre-Deal Employment Mean		1.0000	1.0000	1.0000	1.0000	1.0000	1.0000	1.0000	1.0000
Pre-Deal LTCG Mean		934.2	3,743.6	1,545.2	3,619.0	483.5	1,960.6	730.4	1,709.3

Notes: This table reports difference in difference estimates of β from equation (1), replacing post-match year indicators with indicators for an observation being in the acquisition year, the first 4 years after the acquisition, and years 5–9 after the acquisition. The estimation sample in the first 4 columns includes all workers in our workers panel, while the estimation sample in columns 5-8 excludes all owners from the workers panel. Standard errors are clustered at the firm level and reported in parentheses. The pre-reform mean is the average value of the outcome variable in each column for workers at acquired firms in the year before the acquisition.

Table 10: Impacts of PE Acquisitions on Owners

	(1) Wages	(2) LTCG	(3) Sched C Inc	(4) Biz Inc
Panel A: Baseline Specification				
β_0	−3,422.0	2,586,909.5	−34.1	−290,261.6
	(2,445.0)	(44,919.6)	(233.1)	(18,708.5)
$\beta_{1–4}$	−40,965.7	100,750.5	−1,039.0	−579,235.4
	(3,323.9)	(16,002.4)	(288.5)	(19,133.9)
$\beta_{5–9}$	−41,994.8	14,563.9	−760.1	−476,726.5
	(4,830.8)	(17,306.7)	(387.7)	(22,537.9)
Panel B: Industry by Year Fixed Effects				
β_0	−2,937.8	2,586,322.0	−56.8	−289,599.5
	(2,428.2)	(44,568.1)	(234.5)	(18,460.0)
$\beta_{1–4}$	−40,298.5	100,595.5	−993.8	−577,763.9
	(3,287.0)	(15,998.1)	(288.6)	(19,012.1)
$\beta_{5–9}$	−41,702.1	11,855.6	−693.0	−477,595.6
	(4,727.9)	(17,103.5)	(384.8)	(22,317.4)
Obs	654,856	654,856	654,856	654,856
Clusters	15,742	15,742	15,742	15,742
Pre-Deal Mean	186,251.9	186,732.5	5,495.6	701,356.0

Notes: This table reports difference in difference estimates of β from equation (1), replacing post-match year indicators with indicators for an observation being in the acquisition year, the first 4 years after the acquisition, and years 5–9 after the acquisition. The estimation sample includes our entire owner panel data set. Estimates in panel B add 2 digit NAICS industry by year fixed effects to the baseline specification in panel A. Standard errors are clustered at the firm level and reported in parentheses. The pre-reform mean is the average value of the outcome variable in each column for owners of acquired firms in the year before the acquisition.

Table 11: Extensive Margin Impacts of PE Acquisitions on Owners

	(1) Employment	(2) LTCG	(3) Sched C Inc	(4) Biz Inc	(5) No Income
Panel A: Baseline Specification					
β_0	0.0326	0.1838	0.0158	−0.0104	0.0261
	(0.0032)	(0.0055)	(0.0025)	(0.0032)	(0.0047)
$\beta_{1–4}$	−0.1252	0.0776	0.0161	−0.1963	0.1731
	(0.0054)	(0.0049)	(0.0031)	(0.0060)	(0.0049)
$\beta_{5–9}$	−0.1707	0.0463	0.0114	−0.1802	0.1781
	(0.0074)	(0.0058)	(0.0045)	(0.0084)	(0.0061)
Panel B: Industry by Year Fixed Effects					
β_0	0.0340	0.1857	0.0159	−0.0102	0.0255
	(0.0033)	(0.0054)	(0.0025)	(0.0031)	(0.0047)
$\beta_{1–4}$	−0.1223	0.0792	0.0165	−0.1975	0.1713
	(0.0052)	(0.0049)	(0.0031)	(0.0057)	(0.0048)
$\beta_{5–9}$	−0.1672	0.0484	0.0121	−0.1810	0.1760
	(0.0072)	(0.0058)	(0.0045)	(0.0080)	(0.0060)
Obs	654,856	654,856	654,856	654,856	654,856
Clusters	15,742	15,742	15,742	15,742	15,742
Pre-Deal Mean	0.7027	0.6094	0.1883	0.9999	0.0519

Notes: This table reports extensive margin difference in difference estimates of β from equation (1), replacing post-match year indicators with indicators for an observation being in the acquisition year, the first 4 years after the acquisition, and years 5–9 after the acquisition. All outcomes are dummy variables indicating whether each worker received a W2, realized any long-term capital gains, received any schedule C income, received any business income, or received any positive income of these four types. The estimation sample includes our entire owner panel data set. Estimates in panel B add 2 digit NAICS industry by year fixed effects to the baseline specification in panel A. Standard errors are clustered at the firm level and reported in parentheses. The pre-reform mean is the average value of the outcome variable in each column for owners of acquired firms in the year before the acquisition.

Table 12: Impacts of Add-on and Non-Add-on PE Acquisitions on Workers Without Owners

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Add-on				Not Add-on			
Outcome		Full	WQ5	55-65	Manager	Full	WQ5	55-65	Manager
Wages	β_0	4,430.4	15,110.4	5,023.2	12,628.7	6,422.1	19,283.6	6,005.1	14,683.6
		(462.9)	(1,342.7)	(608.6)	(1,190.3)	(1,121.5)	(2,899.0)	(1,125.3)	(2,262.6)
	$\beta_{1–4}$	−1,619.6	−6,150.2	−5,625.0	−6,750.1	−1,075.4	−5,383.6	−4,856.9	−4,112.5
		(607.4)	(2,489.6)	(1,019.6)	(2,222.9)	(1,119.9)	(3,343.2)	(1,445.6)	(2,703.4)
	$\beta_{5–9}$	−735.1	−2,995.8	−3,731.3	−4,994.8	−706.6	−6,112.5	−5,634.7	−4,980.3
		(1,122.7)	(3,297.9)	(2,756.9)	(3,431.1)	(1,391.0)	(3,181.1)	(2,546.1)	(2,419.0)
Employment	β_0	0.0036	0.0016	0.0028	0.0002	0.0013	0.0004	0.0042	0.0030
		(0.0015)	(0.0010)	(0.0054)	(0.0019)	(0.0021)	(0.0018)	(0.0042)	(0.0023)
	$\beta_{1–4}$	−0.0072	−0.0175	−0.0377	−0.0186	0.0008	−0.0063	−0.0065	−0.0021
		(0.0035)	(0.0037)	(0.0068)	(0.0058)	(0.0034)	(0.0030)	(0.0078)	(0.0053)
	$\beta_{5–9}$	−0.0026	−0.0139	−0.0381	−0.0080	0.0063	−0.0039	−0.0062	−0.0005
		(0.0063)	(0.0078)	(0.0089)	(0.0101)	(0.0051)	(0.0050)	(0.0118)	(0.0059)
LTCG	β_0	4,508.1	19,176.1	7,524.4	14,474.3	2,745.2	11,503.2	5,274.2	8,635.4
		(409.8)	(1,562.1)	(791.9)	(1,139.4)	(334.5)	(1,281.8)	(610.6)	(1,348.7)
	$\beta_{1–4}$	381.1	2,219.2	665.4	2,590.9	154.9	1,674.8	810.9	1,358.9
		(228.1)	(1,003.2)	(481.9)	(494.7)	(241.3)	(708.1)	(332.4)	(687.6)
	$\beta_{5–9}$	43.7	990.7	−278.2	1,715.6	507.5	2,488.9	1,089.4	2,142.3
		(327.6)	(1,191.2)	(591.7)	(908.4)	(400.9)	(1,038.3)	(564.8)	(876.0)
Obs		12,924,503	2,514,803	2,100,768	819,343	14,416,692	2,846,526	2,218,716	984,902
Clusters		13,039	12,822	11,879	10,184	6,834	6,782	6,412	5,545
Pre-Deal Wage Mean		77,092.3	153,322.7	83,709.1	116,982.4	85,419.8	174,144.2	90,950.6	130,830.5
Pre-Deal Employment Mean		1.0000	1.0000	1.0000	1.0000	1.0000	1.0000	1.0000	1.0000
Pre-Deal LTCG Mean		446.9	1,645.0	677.7	1,464.8	517.5	2,249.5	785.5	1,909.8

Notes: This table reports difference in difference estimates of β from equation (1), replacing post-match year indicators with indicators for an observation being in the acquisition year, the first 4 years after the acquisition, and years 5–9 after the acquisition. The estimation sample in the first 4 columns includes all workers in our workers panel except for owners that are associated with add-on acquisitions, while the estimation sample in columns 5-8 includes all workers except for owners that are associated with acquisitions that are not add-ons. Standard errors are clustered at the firm level and reported in parentheses. The pre-reform mean is the average value of the outcome variable in each column for workers at acquired firms in the year before the acquisition.

A Data Appendix

A.1 Matching Pitchbook Portfolio Companies to Tax Records

In this appendix we describe our procedure to match portfolio companies acquired in leveraged buyouts in Pitchbook with the U.S. administrative corporate tax records. We implement a conservative fuzzy matching procedure that utilizes company names, zip codes, and street addresses, information that appears in Pitchbook and on tax forms 1120, 1120-S and 1065 for the universe of C-corporations, S-corporations and partnerships.

First, we filter Pitchbook deal data to only private equity deals that are categorized as "Buyout/LBO". Pitchbook defines LBOs as any deal where a private equity fund purchases a controlling percentage of a company's capital stock to take over its assets and operations while borrowing money to finance a portion of the purchase price.

Second, we clean and standardize company names and addresses in both data sets to remove common stop words, punctuation, and legal suffixes (such as "inc" or "llc").

Next, we pursue an iterative matching procedure between Pitchbook portfolio companies and business tax records using names, zip codes, and street addresses. Our four step matching procedure prioritizes matches for which we have the highest degree of confidence and uses conservative criteria to avoid potentially spurious matches. First, we take the set of portfolio companies and find all exact matches in the tax data based on name, zip code, and street. In the second step, we find all exact matches on name and zip code. In the third step, we find all exact matches on zip code that fuzzy match on company name, requiring that non-identical names have n-gram based similarity scores exceeding 0.9 on a 0-1 scale. In the fourth step, we find all exact matches on name that have different zip codes, but are one-to-one matches. In each subsequent step of the matching procedure, we only focus on the remaining portfolio companies that do not yet have a match.

To avoid potentially spurious matches, we make three conservative filtering decisions. First, we exclude potential matches that appear in the tax data more than two years before Pitchbook founding dates. Second, we exclude portfolio companies with exact matches on name in different zip codes that are many-to-many matches. Third, we exclude a small number of cases where multiple Pitchbook portfolio companies match

to a single taxpayer.

This process leaves us with 31,451 firms in the tax data that are acquired in private equity deals in the years 2002-2020. To succesfully match these firms to control firms that are not acquired in private equity deals, we require that we observe firm's entity type (C-corporation, S-Corporation, or partnership), census region, 2 digit NAICS industry, assets, and based on the number of W2s the firm issues, the number of employees and average wage. After these restrictions, we are left with the 15,767 firms that we reference in section 3.

A.2 Occupation Codes

In this appendix, we describe how we classify over 47 percent of workers from our main sample into occupations.

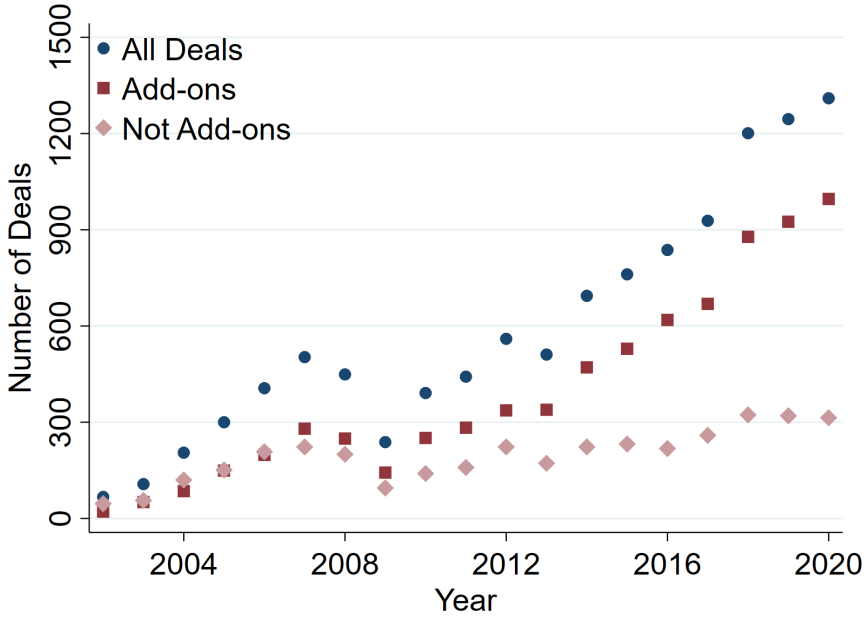
When filing taxes, the primary filer reports their occupation at the bottom of their Form 1040 next to the signature line. If the primary filer is filing taxes jointly, the secondary filer (i.e. the spouse) also reports their occupation. Each occupation is reported as a string variable. Data is available for this string variable beginning in 2005 for electronically filed returns, which have been steadily growing in usage; 53 percent of tax returns were electronically filed in 2005 while 93 percent were electronically filed in 2023, the most recently completed tax year. In some years, the string variable is only available for the primary filer and in other years it is available for both the primary and secondary filer.

To convert the string variables into a usable format, we take advantage of a new crosswalk created internally at the Treasury Department to successfully merge the vast majority of available occupation strings to 2010 three-digit Standard Occupational Classification (SOC) codes from the Bureau of Labor Statistics (BLS). We classify workers as managers versus non-manager on the basis of whether the first two digits of the matched SOC code in the year before the deal are "11", which contains all "Management Occupations" according to the BLS. We use occupations listed in the year before PE acquisitions to classify workers so that their occupations are exogenous to any possible effects of the acquisitions.

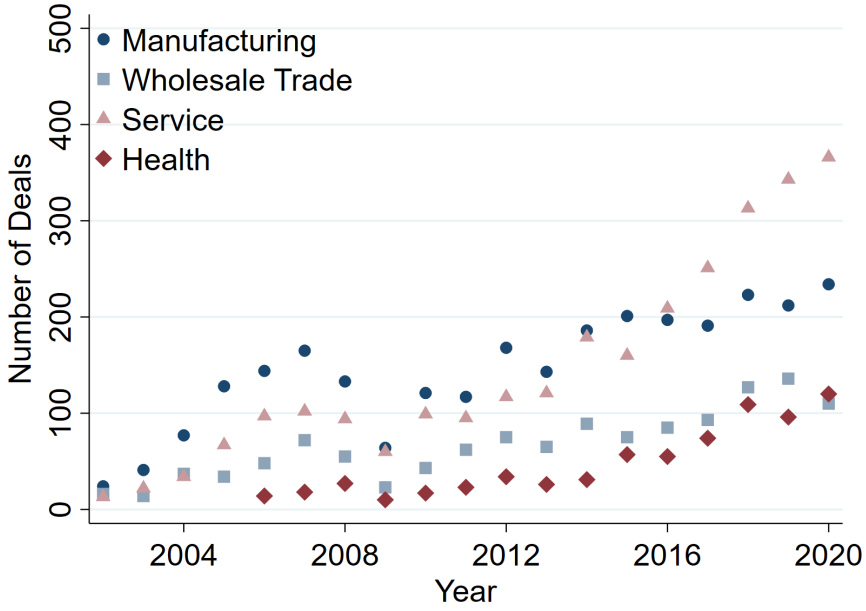
While our occupation measure is available for just under half of our worker sample,

we show that our aggregate results are similar in both the entire workers sample, and the subsample for which we can observe occupations in the year before PE deals. This provides strong evidence that our results by occupation are likely generalizable to the full sample. Moreover, observing occupation for a sample this size affords us more than enough statistical power to detect modestly sized differences in outcomes between managers and non-managers.

B Additional Summary Statistics



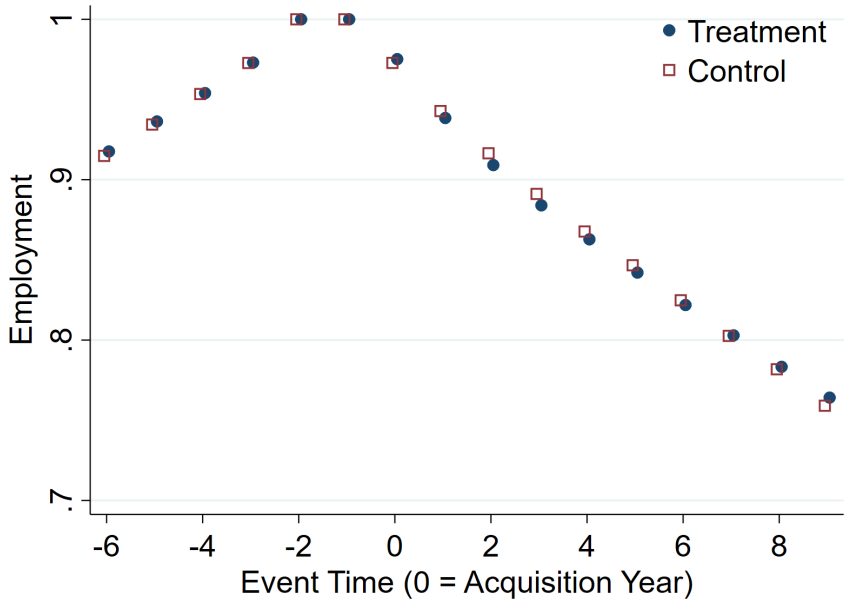
(a) By Deal Type



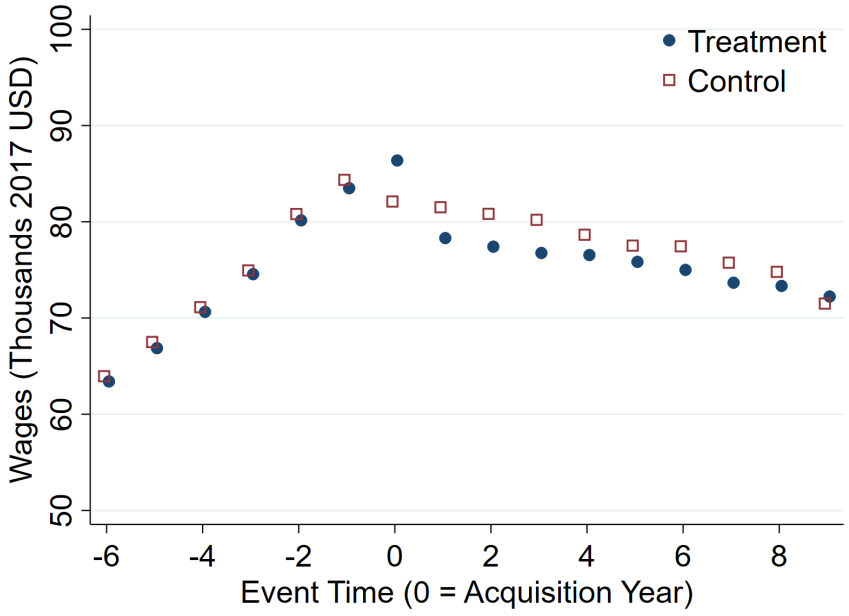
(b) By Industry

Figure B.1: Number of PE Deals

Notes: This figure displays the number of matched private equity deals in each year in our sample.



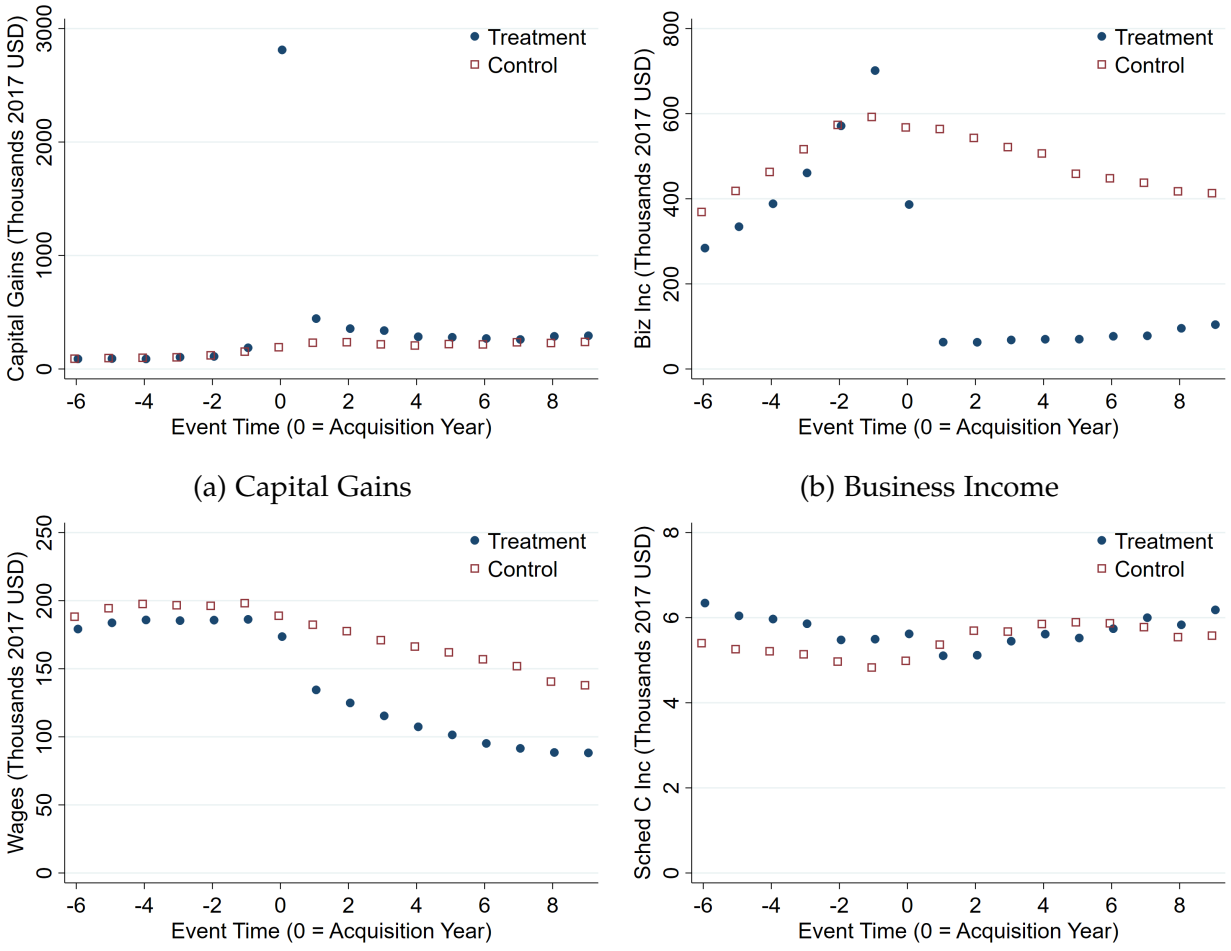
(a) Employment



(b) Wages

Figure B.2: Worker Treatment and Control Raw Means

Notes: This figure displays average raw means of employment and wages by event time for all treatment and control workers in our workers panel. Workers are matched conditional on employment in the two years prior to the buyout.



(a) Capital Gains

(b) Business Income

(c) Wages

(d) Sole Proprietor Income

Figure B.3: Owner Treatment and Control Raw Means

Notes: This figure displays average raw means of long-term capital gains, business income, wages, and sole proprietor income by event time for all treatment and control owners in our owners panel.

Table B.1: Worker Sample Balance

	Treatment		Control	
	Mean	Median	Mean	Median
Wages	83,487	59,749	84,357	58,346
LTCG	934	0	1327	0
Sched C Inc	196	0	224	0
Unemployment	223	0	233	0
Disability	17	0	16	0
OASI	485	0	599	0
Taxes	17,781	7,054	18,285	6,794
Age	42.9	42.5	43.7	43.5
Observations	1,015,380		955,640	
% Male	65.0		64.8	
% Manager	16.1		14.0	
% Add-on	48.4		.	

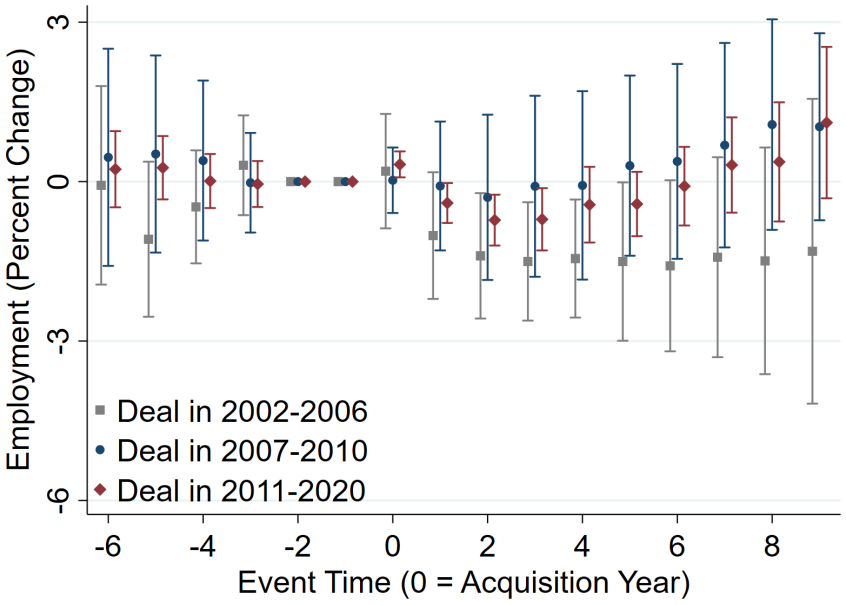
Notes: This table reports means and medians separately for the treatment and control groups in a cross section of the worker sample from the year before private equity acquisitions. Statistics are expressed in 2017 USD, except for counts, ages, and explicitly stated percentages. The bottom section of the table reports the number of observations in the treatment and control groups and the percent of the treatment and control groups that are male, that work in managerial occupations, and that are employed at a firm acquired in an addon acquisition. As discussed in the text, our occupation flag is only available for a subset of workers. To preserve taxpayer anonymity, percentile statistics are reported as the means of all observations in the (P-1,P+1)th percentiles.

Table B.2: Owner Sample Balance

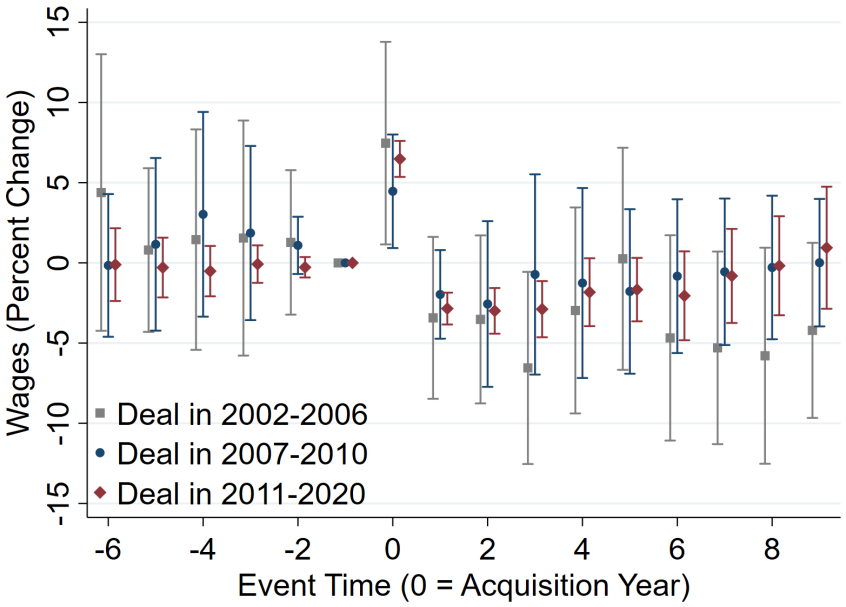
	Treatment		Control	
	Mean	Median	Mean	Median
Wages	186,252	94,327	198,157	86,860
Business Inc	701,356	209,210	592,305	151,805
LTCG	186,733	0	153,173	0
Sched C Inc	196	0	224	0
Taxes	342,260	101,090	305,765	86,817
Age	55.7	53.5	54.5	53.5
Observations	23,002		24,969	
% Male	77.4		76.7	

Notes: This table reports means and medians separately for the treatment and control groups in a cross section of the owner sample from the year before private equity acquisitions. Statistics are expressed in 2017 USD, except for counts, ages, and explicitly stated percentages. The bottom section of the table reports the number of observations in the treatment and control groups and the percent of the treatment and control groups that are male. To preserve taxpayer anonymity, percentile statistics are reported as the means of all observations in the (P-1,P+1)th percentiles.

C Heterogeneity and Robustness for Workers and Owners



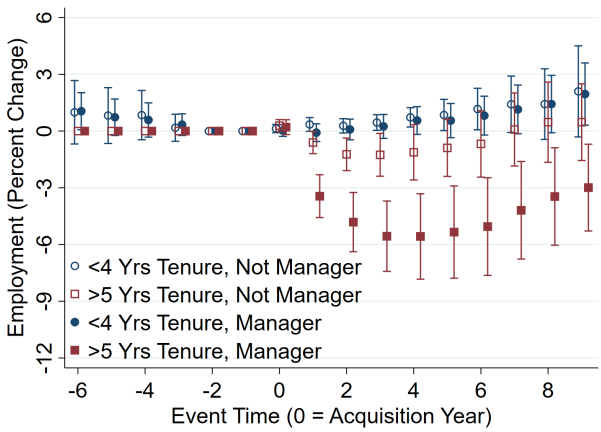
(a) Employment



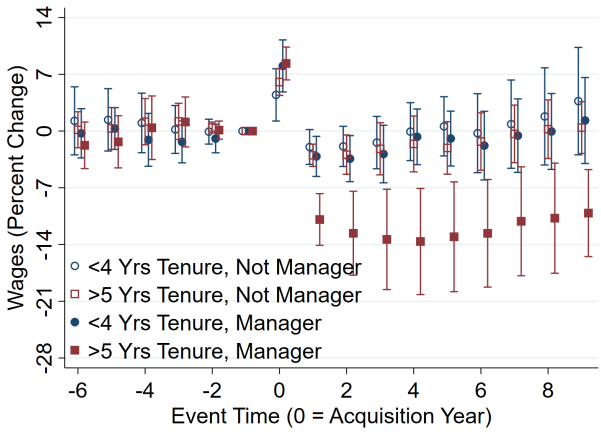
(b) Wages

Figure C.1: Impacts of PE Acquisitions by Time Period

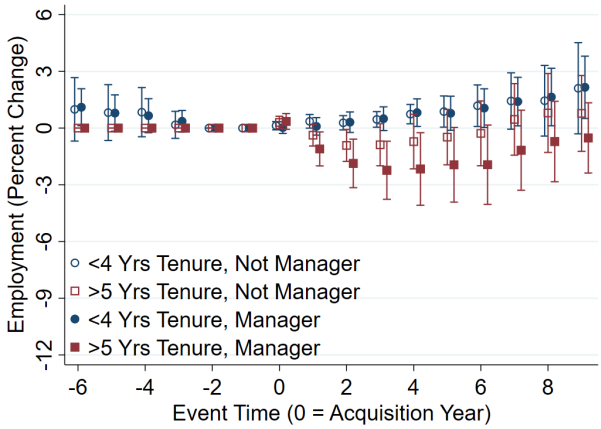
Notes: This figure displays difference-in-difference estimates of the β_e coefficients from equation (1) using employment as an outcome variable in panel (a) and wages in panel (b). The three series in each panel display estimates from separate regressions restricting to the sample of workers associated with deals taking place in different time periods. 95% confidence intervals are constructed from standard errors clustered at the firm level.



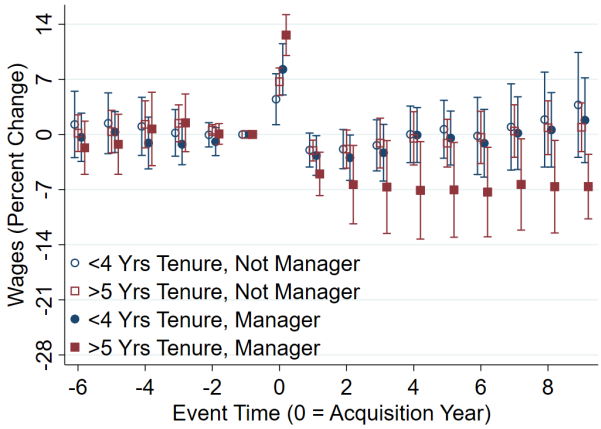
(a) Employment With Owners



(b) Wages With Owners



(c) Employment Without Owners



(d) Wages Without Owners

Figure C.2: Impacts of PE Acquisitions by Managerial Status and Tenure

Notes: This figure displays difference-in-difference estimates of the β_e coefficients from equation (1) using employment as an outcome variable in panels (a) and (c) and wages in panels (b) and (d). The four series in each panel display estimates from separate regressions restricting to the sample of workers that are and are not managers, with less than 4 and more than 5 years of tenure. Panels (a) and (b) show estimates from our worker sample including owners, while panels (c) and (d) exclude owners from the sample. 95% confidence intervals are constructed from standard errors clustered at the firm level.

Table C.1: Extensive Margin Impacts of PE Acquisitions on Workers

	(1) LTCG	(2) UI	(3) DI	(4) OASI
Panel A: Baseline Specification				
β_0	0.0182	0.0032	−0.0001	−0.0007
	(0.0029)	(0.0074)	(0.0002)	(0.0005)
$\beta_{1\text{-}4}$	0.0056	0.0145	−0.0000	−0.0025
	(0.0036)	(0.0045)	(0.0005)	(0.0016)
$\beta_{5\text{-}9}$	0.0053	0.0102	0.0004	−0.0066
	(0.0051)	(0.0057)	(0.0012)	(0.0043)
Panel B: Industry by Year Fixed Effects				
β_0	0.0187	0.0032	−0.0001	−0.0007
	(0.0028)	(0.0072)	(0.0001)	(0.0005)
$\beta_{1\text{-}4}$	0.0064	0.0139	−0.0001	−0.0026
	(0.0036)	(0.0045)	(0.0005)	(0.0015)
$\beta_{5\text{-}9}$	0.0066	0.0090	0.0001	−0.0068
	(0.0049)	(0.0055)	(0.0012)	(0.0039)
Obs	27,841,160	27,841,160	27,841,160	27,841,160
Clusters	20,075	20,075	20,075	20,075
Pre-Deal Mean	0.0658	0.0515	0.0015	0.0255

Notes: This table reports extensive margin difference in difference estimates of β from equation (1), replacing post-match year indicators with indicators for an observation being in the acquisition year, the first 4 years after the acquisition, and years 5–9 after the acquisition. All outcomes are dummy variables indicating whether each worker realized any long-term capital gains, or received UI, DI, or OASI payments. The estimation sample includes our entire worker panel data set. Estimates in panel B add 2 digit NAICS industry by year fixed effects to the baseline specification in panel A. Standard errors are clustered at the firm level and reported in parentheses. The pre-reform mean is the average value of the outcome variable in each column for workers employed at acquired firms in the year before the acquisition.

Table C.2: Net Present Value Impacts of PE Acquisitions on Workers

	(1) Full Sample	(2) WQ5	(3) 55-65	(4) Manager	(5) Rank & File
			Panel A: 5% Discount Rate		
Wages	−8,480.7	−52,235.4	−46,021.8	−61,244.6	2,715.3
	(5,116.9)	(15,114.5)	(8,816.1)	(15,223.0)	(5,975.7)
	[−2.9, 0.2]	[−6.7, −1.9]	[−12.6, −5.7]	[−9.2, −3.2]	[−1.7, 2.7]
LTCG	13,868.9	64,943.6	29,183.5	65,054.1	619.5
	(1,530.4)	(5,229.8)	(3,002.5)	(5,725.6)	(962.7)
	[50.3, 78.1]	[71.3, 98.0]	[70.1, 105.5]	[75.9, 107.6]	[−18.0, 35.6]
			Panel B: 7% Discount Rate		
Wages	−7,482.6	−46,925.6	−41,943.9	−55,442.6	2,477.7
	(4,641.7)	(13,824.8)	(7,925.0)	(13,883.4)	(5,382.9)
	[−2.8, 0.3]	[−6.5, −1.8]	[−12.2, −5.6]	[−8.9, −3.1]	[−1.7, 2.7]
LTCG	13,481.0	62,910.1	28,385.2	62,677.3	609.2
	(1,411.1)	(4,887.9)	(2,795.6)	(5,405.5)	(871.7)
	[54.0, 81.8]	[75.5, 102.6]	[74.8, 110.5]	[79.7, 112.1]	[−17.1, 36.0]
			Panel B: 9% Discount Rate		
Wages	−6,597.5	−42,239.1	−38,349.5	−50,326.8	2,283.5
	(4,229.0)	(12,693.8)	(7,159.5)	(12,711.1)	(4,871.1)
	[−2.7, 0.3]	[−6.3, −1.6]	[−11.8, −5.5]	[−8.7, −2.9]	[−1.6, 2.6]
LTCG	13,138.1	61,109.9	27,673.6	60,577.4	602.7
	(1,309.5)	(4,596.3)	(2,617.9)	(5,129.7)	(793.8)
	[57.6, 85.6]	[79.8, 107.4]	[79.5, 115.7]	[83.5, 116.7]	[−16.1, 36.5]

Notes: This table reports the net present value of cumulative post-reform difference in difference estimates of β from equation (1) for each year from the year of the acquisition through 9 years after the acquisition, using a discount rate of 5% in Panel A, 7% in Panel B, and 9% in Panel C. The estimation sample in column 1 includes our entire worker panel data set. Columns 2, 3, and 4 restrict to top wage quintile workers, 55-65 year old workers at the time of the acquisition, and managers. Column 5 restricts to all workers that are not in the top wage quintile, are less than 55 years old at the time of the acquisition, and are not managers. Standard errors are clustered at the firm level and reported in parentheses. We report 95% confidence intervals in percentage terms in brackets, scaling the coefficient estimate by the net present value of the sum of the outcome variable for the control group from the year of the acquisition through 9 years after the acquisition.

Table C.3: Impacts of PE Acquisitions on Workers Without Wage Restriction

	(1) Employment	(2) Wages	(3) LTCG
β_0	0.0015	3960.3	6405.4
	(0.0024)	(483.8)	(361.5)
$\beta_{1\text{-}4}$	−0.0062	−1287.0	728.5
	(0.0041)	(558.7)	(150.3)
$\beta_{5\text{-}9}$	0.0013	−175.7	340.3
	(0.0060)	(872.4)	(235.2)
Obs	34,784,260	34,784,260	34,784,260
Clusters	21,812	21,812	21,812
Pre-Deal Mean	1.0000	70,093.9	750.4

Notes: This table reports difference in difference estimates of β from equation (1), replacing post-match year indicators with indicators for an observation being in the acquisition year, the first 4 years after the acquisition, and years 5–9 after the acquisition. The estimation sample includes all workers from an alternative version of the workers panel constructed without any wage restrictions. Standard errors are clustered at the firm level and reported in parentheses. The pre-reform mean is the average value of the outcome variable in each column for workers employed at acquired firms in the year before the acquisition.

Table C.4: Sample Overlap for High Incidence Groups

	(1) Full Samp	(2) Owners	(3) WQ5	(4) 55-65	(5) Managers
Panel A: Full Worker Sample					
Total Count	1,971,020	25,024	402,726	319,762	
% Owners	1	100	5	2	
% WQ5	20	86	100	27	
% 55-65	16	32	21	100	
Panel B: Occupation Sample					
Total Count	921,463	15,164	202,162	152,232	138,685
% Owners	2	100	6	3	5
% WQ5	22	87	100	29	45
% 55-65	17	33	22	100	18
% Managers	15	49	31	17	100

Notes: This table reports the total number of workers, owners, top wage quintile workers, 55-65 workers, and managers, and the fraction of each group that are owners, top wage quintile, 55-65, and managers. Panel A reports statistics for the entire worker sample, while Panel B reports statistics for our occupation sample.

Table C.5: Impacts of PE Acquisitions on Workers With and Without Low Stake Owners

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		With Owners				Without Owners			
Outcome		Full	WQ5	55-65	Manager	Full	WQ5	55-65	Manager
Wages	β_0	5,136.9	15,083.5	4,635.0	11,294.4	5,425.3	17,184.1	5,460.6	13,614.3
		(615.1)	(1,557.9)	(631.0)	(1,236.3)	(625.0)	(1,661.0)	(663.4)	(1,339.8)
	$\beta_{1–4}$	−2,316.7	−10,333.6	−7,668.4	−10,954.4	−1,316.4	−5,699.5	−5,102.8	−5,237.2
		(654.6)	(2,077.0)	(847.8)	(1,991.2)	(650.4)	(2,116.7)	(917.3)	(1,812.4)
	$\beta_{5–9}$	−1,542.1	−8,740.4	−6,675.2	−9,580.9	−648.7	−4,543.3	−4,445.6	−4,938.4
		(906.8)	(2,280.3)	(1,710.3)	(2,421.5)	(904.9)	(2,262.1)	(1,865.4)	(2,035.3)
Employment	β_0	0.0024	0.0007	0.0033	0.0011	0.0025	0.0011	0.0035	0.0016
		(0.0013)	(0.0010)	(0.0033)	(0.0014)	(0.0013)	(0.0011)	(0.0034)	(0.0015)
	$\beta_{1–4}$	−0.0061	−0.0245	−0.0289	−0.0267	−0.0028	−0.0112	−0.0214	−0.0093
		(0.0025)	(0.0026)	(0.0055)	(0.0045)	(0.0025)	(0.0024)	(0.0057)	(0.0040)
	$\beta_{5–9}$	−0.0008	−0.0226	−0.0291	−0.0208	0.0027	−0.0078	−0.0204	−0.0032
		(0.0041)	(0.0046)	(0.0081)	(0.0064)	(0.0041)	(0.0044)	(0.0081)	(0.0054)
LTCG	β_0	8,250.4	35,408.9	16,404.5	31,948.1	3,341.7	14,136.6	6,018.7	10,687.1
		(444.6)	(1,785.3)	(913.9)	(2,098.5)	(255.4)	(971.9)	(480.7)	(909.3)
	$\beta_{1–4}$	991.3	5,215.5	2,461.2	5,580.0	316.2	2,182.1	854.1	1,972.7
		(183.0)	(669.1)	(349.9)	(638.9)	(150.6)	(490.0)	(244.5)	(433.8)
	$\beta_{5–9}$	581.5	3,081.9	1,134.2	3,754.7	318.3	1,872.8	429.2	2,176.4
		(262.4)	(752.5)	(436.3)	(677.1)	(263.7)	(775.3)	(410.3)	(607.1)
Obs		27,841,160	5,685,773	4,451,920	1,912,256	27,418,096	5,342,871	4,324,999	1,801,925
Clusters		20,075	20,075	18,692	16,501	20,070	19,758	18,446	15,842
Pre-Deal Wage Mean		83,487.4	170,092.9	91,668.1	131,821.5	81,165.2	163,692.4	86,833.9	124,123.9
Pre-Deal Employment Mean		1.0000	1.0000	1.0000	1.0000	1.0000	1.0000	1.0000	1.0000
Pre-Deal LTCG Mean		934.2	3,743.6	1,545.2	3,619.0	457.8	1,875.0	678.3	1,618.8

Notes: This table reports difference in difference estimates of β from equation (1), replacing post-match year indicators with indicators for an observation being in the acquisition year, the first 4 years after the acquisition, and years 5–9 after the acquisition. The estimation sample in the first 4 columns includes all workers in our workers panel, while the estimation sample in columns 5-8 excludes all owners from the workers panel, defining owners as anyone with a nonzero ownership stake in an S-corporation or partnership, and anyone receiving $> 0.1\%$ of a dividend distribution or ownership stake on an 1120 Schedule G for C-corporations. Standard errors are clustered at the firm level and reported in parentheses. The pre-reform mean is the average value of the outcome variable in each column for workers at acquired firms in the year before the acquisition.

Table C.6: Net Present Value Impacts of PE Acquisitions on Workers Excluding Owners

	(1) Full Sample	(2) WQ5	(3) 55-65	(4) Manager	(5) Rank & File
			Panel A: 5% Discount Rate		
Wages	−1,583.3	−19,103.0	−28,794.2	−22,414.3	2,794.0
	(5,078.7)	(15,121.0)	(9,619.0)	(13,106.6)	(5,978.4)
	[−1.8, 1.3]	[−4.0, 0.9]	[−9.5, −2.0]	[−4.8, 0.3]	[−1.7, 2.8]
LTCG	5,667.3	28,636.3	10,556.0	26,276.9	370.2
	(1,403.4)	(4,589.2)	(2,434.2)	(3,726.1)	(874.0)
	[13.5, 39.0]	[25.6, 49.1]	[17.4, 46.1]	[26.8, 47.4]	[−19.1, 29.6]
			Panel B: 7% Discount Rate		
Wages	−1,110.7	−16,284.6	−26,002.9	−19,461.7	2,546.9
	(4,606.9)	(13,841.9)	(8,651.6)	(11,978.4)	(5,385.5)
	[−1.7, 1.3]	[−3.8, 1.0]	[−9.1, −1.9]	[−4.6, 0.4]	[−1.6, 2.7]
LTCG	5,481.8	27,518.4	10,268.7	24,988.2	349.3
	(1,274.9)	(4,203.3)	(2,227.7)	(3,432.0)	(787.7)
	[15.0, 40.2]	[27.3, 50.6]	[19.3, 47.8]	[27.9, 48.5]	[−18.6, 29.4]
			Panel B: 9% Discount Rate		
Wages	−689.0	−13,793.6	−23,544.1	−16,863.9	2,344.5
	(4,197.5)	(12,719.7)	(7,820.0)	(10,990.3)	(4,873.5)
	[−1.6, 1.4]	[−3.7, 1.1]	[−8.7, −1.8]	[−4.5, 0.5]	[−1.6, 2.6]
LTCG	5,319.8	26,536.6	10,013.0	23,859.7	333.8
	(1,164.6)	(3,872.4)	(2,050.0)	(3,180.1)	(713.9)
	[16.6, 41.4]	[29.0, 52.3]	[21.1, 49.5]	[29.1, 49.7]	[−18.0, 29.3]

Notes: This table reports the net present value of cumulative post-reform difference in difference estimates of β from equation (1) for each year from the year of the acquisition through 9 years after the acquisition, using a discount rate of 5% in Panel A, 7% in Panel B, and 9% in Panel C. The estimation sample in column 1 includes our entire worker panel data set. Columns 2, 3, and 4 restrict to top wage quintile workers, 55-65 year old workers at the time of the acquisition, and managers. Column 5 restricts to all workers that are not in the top wage quintile, are less than 55 years old at the time of the acquisition, and are not managers. All samples exclude owners. Standard errors are clustered at the firm level and reported in parentheses. We report 95% confidence intervals in percentage terms in brackets, scaling the coefficient estimate by the net present value of the sum of the outcome variable for the control group from the year of the acquisition through 9 years after the acquisition.

Table C.7: Impacts of PE Acquisitions on Owners With Large Stakes

	(1) Wages	(2) LTCG	(3) Sched C Inc	(4) Biz Inc
Panel A: > 5% Ownership Stakes				
β_0	−9,574.3	3,320,207.8	−51.7	−362,997.0
	(2,955.1)	(53,239.8)	(276.2)	(22,235.1)
$\beta_{1–4}$	−46,281.5	115,855.8	−1,176.9	−722,087.0
	(3,823.2)	(19,268.7)	(326.5)	(23,046.3)
$\beta_{5–9}$	−42,919.7	8,852.2	−520.3	−606,732.4
	(5,114.4)	(21,051.5)	(414.8)	(27,439.7)
Obs	485,867	485,867	485,867	485,867
Clusters	15,602	15,602	15,602	15,602
R^2	0.66	0.38	0.46	0.53
Pre-Deal Mean	198,336.2	198,850.8	4,774.1	855,019.8
Panel B: > 10% Ownership Stakes				
β_0	−15,396.5	3,739,142.5	−70.1	−399,044.7
	(3,253.1)	(60,186.8)	(311.4)	(24,301.7)
$\beta_{1–4}$	−52,571.9	125,370.8	−1,282.9	−801,023.3
	(4,193.2)	(21,485.8)	(354.5)	(25,154.9)
$\beta_{5–9}$	−48,345.4	−9,033.5	−562.5	−682,874.8
	(5,571.0)	(24,041.2)	(445.6)	(30,432.1)
Obs	402,446	402,446	402,446	402,446
Clusters	15,318	15,318	15,318	15,318
R^2	0.66	0.39	0.44	0.54
Pre-Deal Mean	206,809.7	203,161.3	4,331.3	944,580.2

Notes: This table reports difference in difference estimates of β from equation (**??**), replacing post-match year indicators with indicators for an observation being in the acquisition year, the first 4 years after the acquisition, and years 5–9 after the acquisition. The estimation sample includes our entire owner panel data set, requiring owners have 5% ownership stakes in Panel A and 10% ownership stakes in Panel B. Standard errors are clustered at the firm level and reported in parentheses. The pre-reform mean is the average value of the outcome variable in each column for owners of acquired firms in the year before the acquisition.

Table C.8: Extensive Margin Impacts of PE Acquisitions on Owners With Large Stakes

	(1) Employment	(2) LTCG	(3) Sched C Inc	(4) Biz Inc	(5) No Income
Panel A: > 5% Ownership Stakes					
β_0	0.0382	0.1955	0.0233	−0.0104	0.0221
	(0.0035)	(0.0054)	(0.0030)	(0.0031)	(0.0035)
$\beta_{1–4}$	−0.1446	0.0933	0.0226	−0.2012	0.1864
	(0.0055)	(0.0051)	(0.0036)	(0.0052)	(0.0046)
$\beta_{5–9}$	−0.1911	0.0623	0.0199	−0.1892	0.1929
	(0.0074)	(0.0061)	(0.0050)	(0.0070)	(0.0060)
Obs	485,867	485,867	485,867	485,867	485,867
Clusters	15,602	15,602	15,602	15,602	15,602
R^2	0.66	0.60	0.57	0.42	0.47
Pre-Deal Mean	0.7306	0.6165	0.1820	0.9998	0.0447
Panel A: > 10% Ownership Stakes					
β_0	0.0386	0.2032	0.0267	−0.0108	0.0227
	(0.0037)	(0.0058)	(0.0033)	(0.0030)	(0.0034)
$\beta_{1–4}$	−0.1660	0.0989	0.0264	−0.2044	0.1969
	(0.0058)	(0.0055)	(0.0040)	(0.0051)	(0.0046)
$\beta_{5–9}$	−0.2151	0.0708	0.0269	−0.1942	0.2031
	(0.0077)	(0.0065)	(0.0055)	(0.0071)	(0.0062)
Obs	402,446	402,446	402,446	402,446	402,446
Clusters	15,318	15,318	15,318	15,318	15,318
R^2	0.65	0.59	0.56	0.41	0.47
Pre-Deal Mean	0.7565	0.6260	0.1788	0.9999	0.0381

Notes: This table reports extensive margin difference in difference estimates of β from equation (**??**), replacing post-match year indicators with indicators for an observation being in the acquisition year, the first 4 years after the acquisition, and years 5–9 after the acquisition. All outcomes are dummy variables indicating whether each worker received a W2, realized any long-term capital gains, received any schedule C income, received any business income, or received any positive income of these four types. The estimation sample includes our entire owner panel data set, requiring owners have 5% ownership stakes in Panel A and 10% ownership stakes in Panel B. Standard errors are clustered at the firm level and reported in parentheses. The pre-reform mean is the average value of the outcome variable in each column for owners of acquired firms in the year before the acquisition.

Table C.9: Impacts of PE Acquisitions on Owners by Age Group

Outcome		(1) 25-35	(2) 35-45	(3) 45-55	(4) 55-65	(5) 65+
Wages	β_0	13,873.0	11,968.2	5,660.1	−16,444.1	−17,408.4
		(7,435.9)	(5,024.4)	(4,596.7)	(4,742.3)	(4,301.0)
	$\beta_{1–4}$	5.5	−12,846.3	−43,026.6	−68,079.5	−53,402.8
		(8,691.1)	(6,770.9)	(5,931.8)	(6,000.1)	(5,729.2)
	$\beta_{5–9}$	−16,931.8	−36,322.1	−50,647.0	−64,715.3	−27,403.3
		(12,309.2)	(10,567.7)	(8,291.7)	(7,915.9)	(8,163.8)
Emp	β_0	0.0703	0.0610	0.0481	0.0275	0.0002
		(0.0147)	(0.0076)	(0.0054)	(0.0053)	(0.0057)
	$\beta_{1–4}$	−0.0104	−0.0485	−0.0961	−0.1922	−0.2205
		(0.0180)	(0.0108)	(0.0085)	(0.0091)	(0.0104)
	$\beta_{5–9}$	−0.0452	−0.1062	−0.1633	−0.2740	−0.1990
		(0.0239)	(0.0140)	(0.0112)	(0.0126)	(0.0140)
Biz Inc	β_0	−178,551.3	−288,225.9	−310,851.9	−267,810.0	−333,723.8
		(56,666.1)	(32,326.0)	(25,615.6)	(26,215.7)	(38,255.0)
	$\beta_{1–4}$	−471,060.0	−597,072.6	−657,275.8	−566,701.2	−512,705.2
		(66,928.8)	(31,286.1)	(27,753.4)	(27,947.2)	(39,228.5)
	$\beta_{5–9}$	−377,668.2	−518,939.6	−600,927.9	−453,091.2	−397,379.4
		(71,461.6)	(36,846.8)	(32,004.4)	(34,596.7)	(47,015.4)
LTCG	β_0	1,752,534.2	2,734,622.5	3,117,834.5	3,042,059.2	2,434,644.5
		(135,844.1)	(88,830.2)	(72,484.0)	(70,994.5)	(83,781.4)
	$\beta_{1–4}$	127,346.4	153,261.7	130,557.9	127,539.0	15,335.4
		(46,316.9)	(30,677.2)	(25,298.2)	(27,700.0)	(35,891.6)
	$\beta_{5–9}$	49,503.3	19,057.9	49,331.9	650.2	−29,732.4
		(50,566.9)	(33,100.4)	(27,301.9)	(30,713.0)	(40,038.1)
Obs		26,684	102,823	182,483	169,479	114,177
Clusters		1,360	4,524	7,363	7,347	5,298
Pre-Deal Wage Mean		100,876.9	198,501.0	227,050.5	236,436.6	152,963.4
Pre-Deal Emp Mean		0.7779	0.7968	0.8139	0.8154	0.5785
Pre-Deal Biz Inc Mean		471,772.2	608,183.4	733,498.4	732,917.6	696,326.4
Pre-Deal LTCG Mean		81,529.7	174,369.8	196,131.6	208,279.1	269,563.9

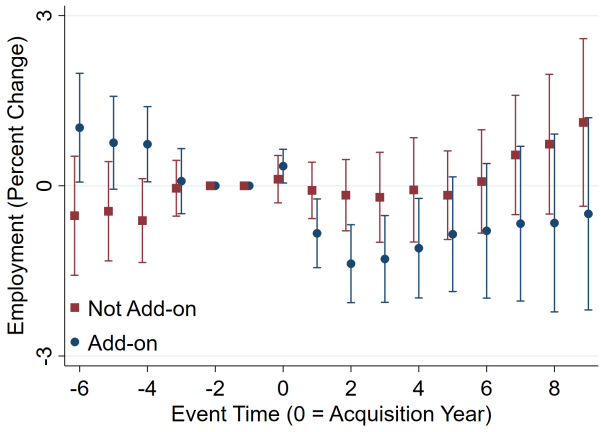
Notes: This table reports difference in difference estimates of β from equation (1), replacing post-match year indicators with indicators for an observation being in the acquisition year, the first 4 years after the acquisition, and years 5–9 after the acquisition. The estimation sample includes our entire owner panel data set. We assign owners to an age group based on their age in the year before acquisitions and each column displays estimates from regressions performed separately on owners in each age group. Standard errors are clustered at the firm level and reported in parentheses. The pre-reform mean is the average value of the outcome variable in each column for owners of acquired firms in the year before the acquisition.

Table C.10: Net Present Value Impacts of PE Acquisitions on Owners

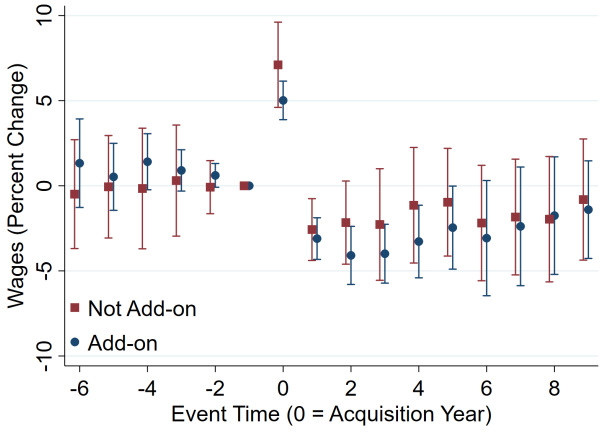
	(1) Full Sample	(2) 55-65	(3) 65+	(4) <55
		Panel A: 5% Discount Rate		
Wages	−295,755.4	−484,125.3	−295,575.2	−238,129.4
	(28,773.4)	(50,023.8)	(49,738.1)	(39,406.3)
	[−26.2, −17.8]	[−39.8, −26.4]	[−39.6, −20.0]	[−19.6, −10.0]
LTCG	2,991,032.0	3,474,153.5	2,384,739.2	3,427,138.5
	(132,946.8)	(230,379.1)	(298,954.7)	(162,153.6)
	[152.2, 181.2]	[136.4, 177.2]	[70.9, 117.1]	[193.6, 233.1]
Biz Inc	−4,021,617.5	−3,863,742.0	−3,532,390.5	−4,383,654.5
	(153,428.3)	(229,139.7)	(321,252.2)	(171,127.9)
	[−107.6, −92.6]	[−111.4, −88.2]	[−104.8, −73.1]	[−122.5, −105.1]
		Panel B: 7% Discount Rate		
Wages	−271,410.9	−445,790.1	−277,438.1	−215,557.4
	(26,219.6)	(45,728.7)	(45,327.0)	(35,873.2)
	[−25.8, −17.6]	[−39.1, −26.0]	[−39.3, −20.2]	[−19.1, −9.7]
LTCG	2,973,159.8	3,462,700.2	2,398,467.8	3,393,043.5
	(124,173.0)	(214,664.2)	(277,867.0)	(151,664.9)
	[164.4, 193.7]	[148.7, 189.8]	[78.5, 124.7]	[208.9, 249.0]
Biz Inc	−3,729,488.0	−3,585,009.0	−3,290,072.8	−4,053,754.8
	(141,024.6)	(210,119.1)	(295,492.3)	(157,259.4)
	[−106.9, −92.1]	[−110.6, −87.8]	[−104.1, −72.9]	[−121.8, −104.6]
		Panel B: 9% Discount Rate		
Wages	−250,004.2	−412,048.1	−261,189.0	−195,825.8
	(24,005.0)	(42,001.7)	(41,502.8)	(32,810.3)
	[−25.4, −17.3]	[−38.5, −25.7]	[−38.9, −20.4]	[−18.7, −9.4]
LTCG	2,956,778.2	3,451,305.8	2,410,271.8	3,362,417.2
	(116,597.9)	(201,068.5)	(259,608.9)	(142,645.0)
	[176.8, 206.5]	[161.1, 202.6]	[86.2, 132.4]	[224.5, 265.2]
Biz Inc	−3,471,953.2	−3,339,071.0	−3,076,072.2	−3,763,516.0
	(130,229.0)	(193,586.6)	(273,041.0)	(145,190.6)
	[−106.2, −91.7]	[−109.8, −87.4]	[−103.4, −72.8]	[−121.0, −104.0]

Notes: This table reports the net present value of cumulative post-reform difference in difference estimates of β from equation (1) for each year from the year of the acquisition through 9 years after the acquisition, using a discount rate of 5% in Panel A, 7% in Panel B, and 9% in Panel C. The estimation sample in column 1 includes our entire owner panel data set. The other columns restrict to owners in the given age ranges at the time of the acquisition. Standard errors are clustered at the firm level and reported in parentheses. We report 95% confidence intervals in percentage terms in brackets, scaling the coefficient estimate by the net present value of the sum of the outcome variable for the control group from the year of the acquisition through 9 years after the acquisition.

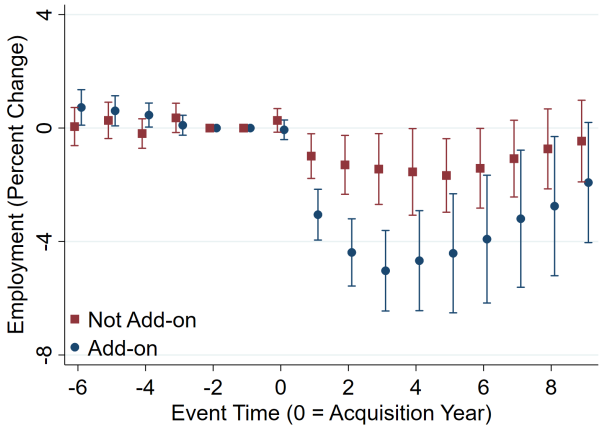
D Heterogeneity and Robustness for Add-Ons



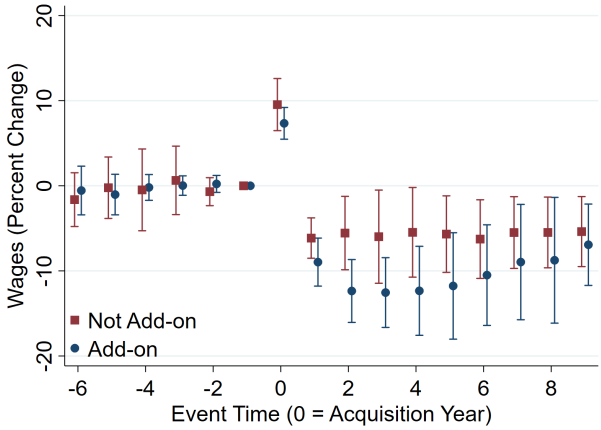
(a) Aggregate Employment



(b) Aggregate Wages



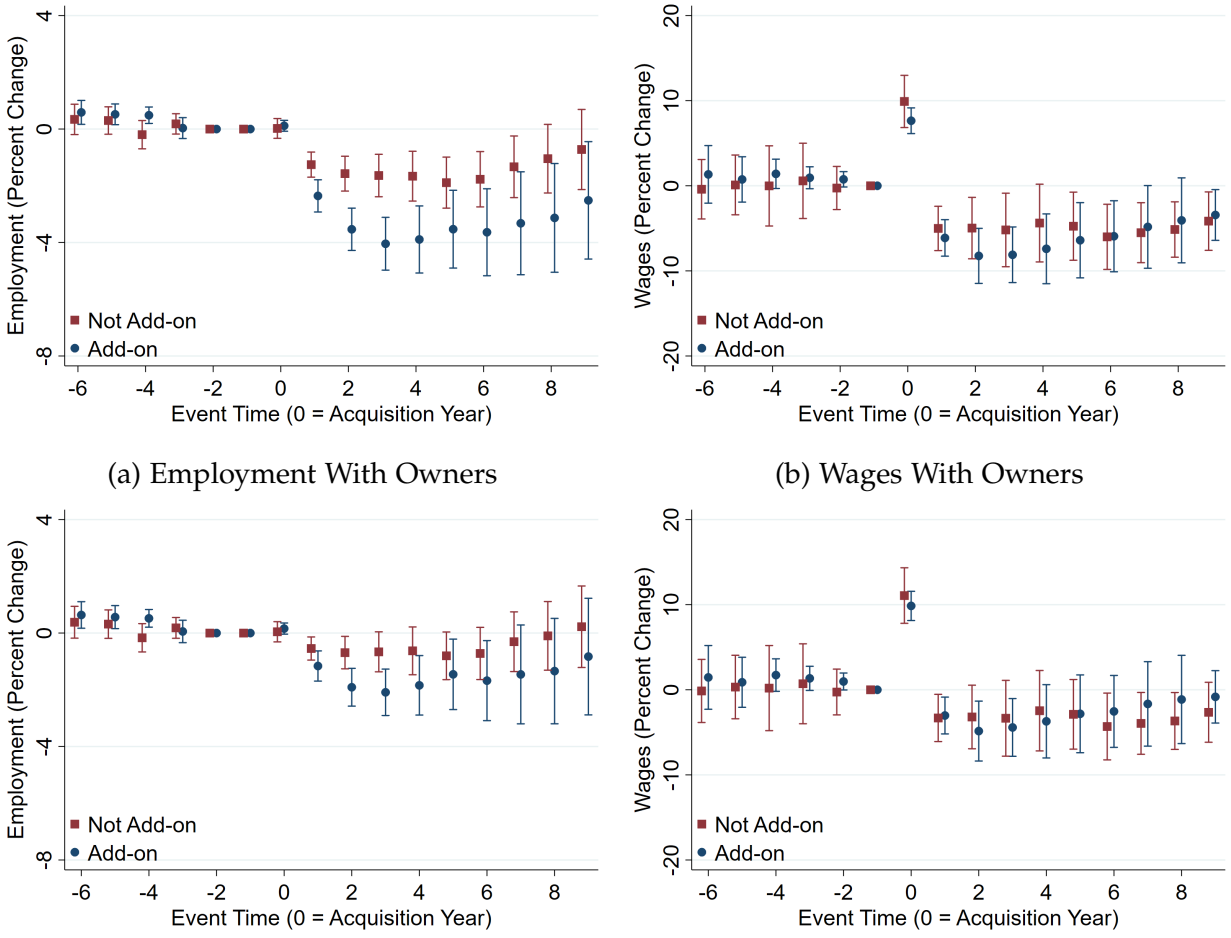
(c) Manager Employment



(d) Manager Wages

Figure D.1: Impacts of Add-on PE Acquisitions With Owners

Notes: This figure displays difference-in-difference estimates of the β_e coefficients from equation (1) using a dummy variable equal to 1 if a worker is employed as the outcome in Panels (a) and (c) and wages as the outcome in panels (b) and (d). Each panel splits workers into groups that are and are not associated with an Add-on acquisition. Panels (a) and (b) use the full sample of workers, while panels (c) and (d) restrict to managers. To scale estimates into percent changes we divide coefficient estimates by the average value of the outcome variable for workers employed at acquired firms in the relevant estimation sample in the year before the acquisition. 95% confidence intervals are constructed from standard errors clustered at the firm level.



(a) Employment With Owners

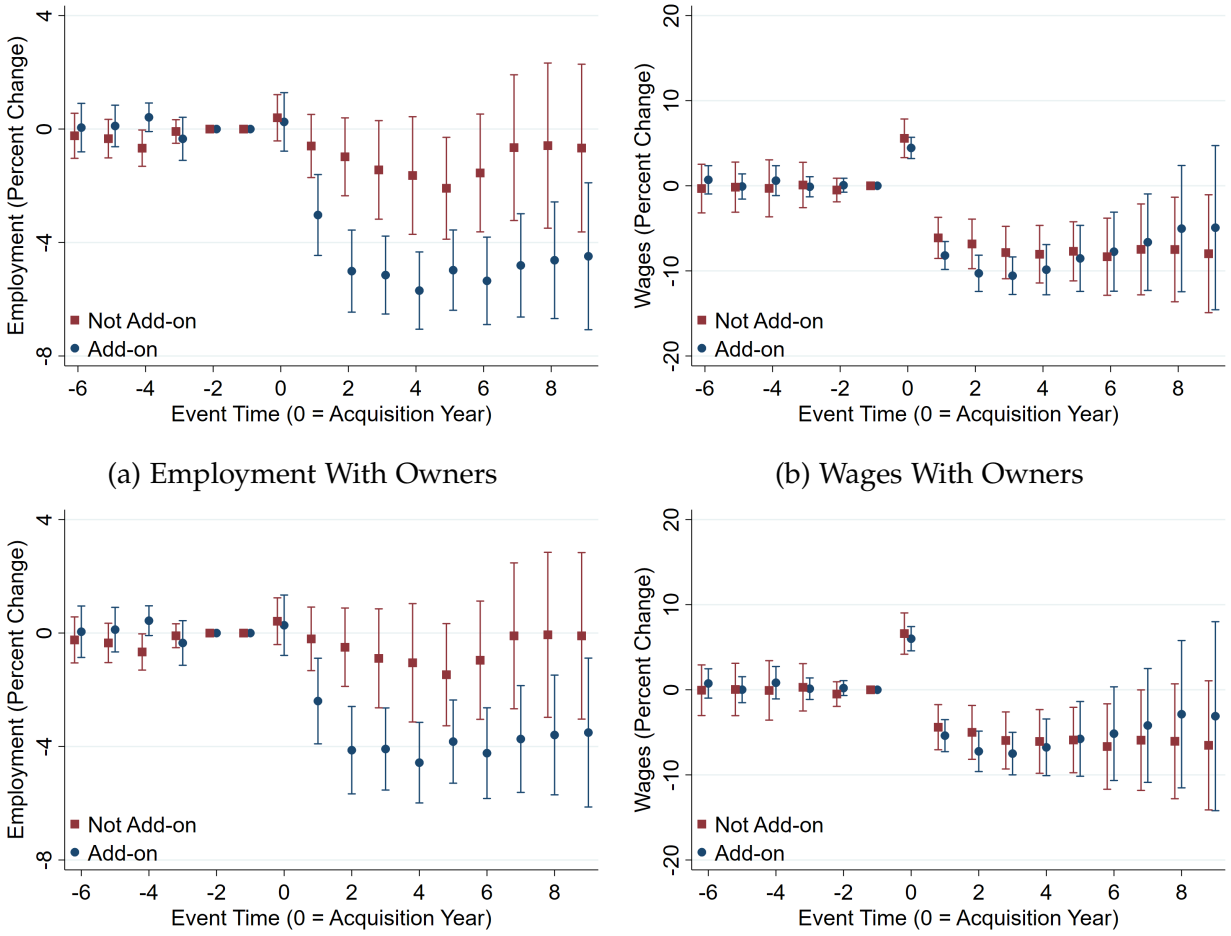
(b) Wages With Owners

(c) Employment Without Owners

(d) Wages Without Owners

Figure D.2: Employment Impacts of Add-On PE Acquisitions on High Wage Workers

Notes: This figure displays difference-in-difference estimates of the β_e coefficients from equation (1) using a dummy variable equal to 1 if a worker is employed as an outcome variable in panels (a) and (c), and wages as an outcome variable in panels (b) and (d). Each panel displays separate regression estimates performed on the sample of top wage quintile workers in our worker sample that are and are not associated with add-on acquisitions. Panels (a) and (b) include owners, while panels (c) and (d) exclude owners. To scale estimates into percent changes we divide coefficient estimates by the average value of the outcome variable for workers employed at acquired firms in the specified age range in the year before the acquisition. 95% confidence intervals are constructed from standard errors clustered at the firm level.



(a) Employment With Owners

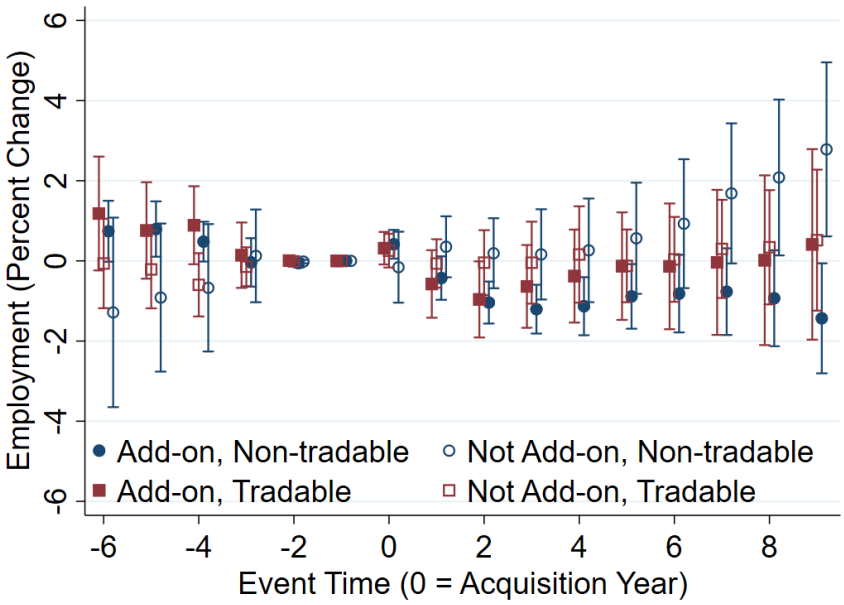
(b) Wages With Owners

(c) Employment Without Owners

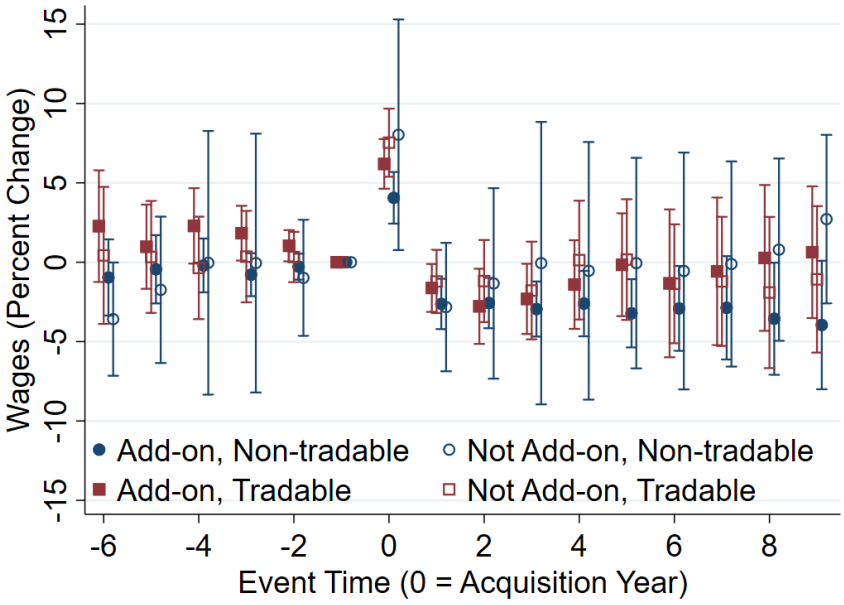
(d) Wages Without Owners

Figure D.3: Employment Impacts of Add-On PE Acquisitions on Older Workers

Notes: This figure displays difference-in-difference estimates of the β_e coefficients from equation (1) using a dummy variable equal to 1 if a worker is employed as an outcome variable in panels (a) and (c), and wages as an outcome variable in panels (b) and (d). Each panel displays separate regression estimates performed on the sample of age 55-65 workers in our worker sample that are and are not associated with add-on acquisitions. Panels (a) and (b) include owners, while panels (c) and (d) exclude owners. To scale estimates into percent changes we divide coefficient estimates by the average value of the outcome variable for workers employed at acquired firms in the specified age range in the year before the acquisition. 95% confidence intervals are constructed from standard errors clustered at the firm level.



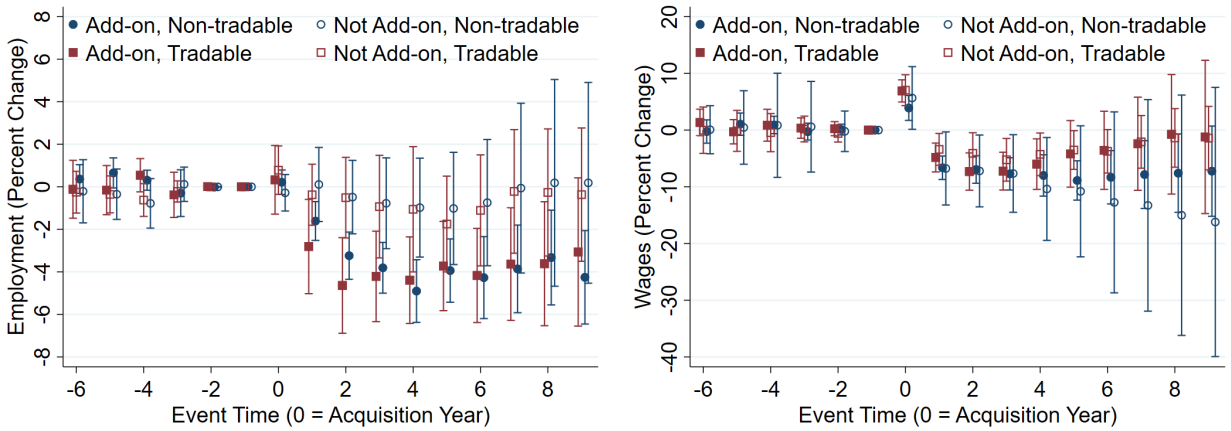
(a) Employment



(b) Wages

Figure D.4: Impacts of Add-on PE Acquisitions by Sector

Notes: This figure displays difference-in-difference estimates of the β_e coefficients from equation (1) using employment as an outcome variable in panel (a) and wages in panel (b). The four series in each panel display estimates from separate regressions restricting to the sample of works involved in add-on deals or not, in tradable industries or not, using the geographic tradable definition from Mian and Sufi (2014). 95% confidence intervals are constructed from standard errors clustered at the firm level.



(a) 55-65 Employment

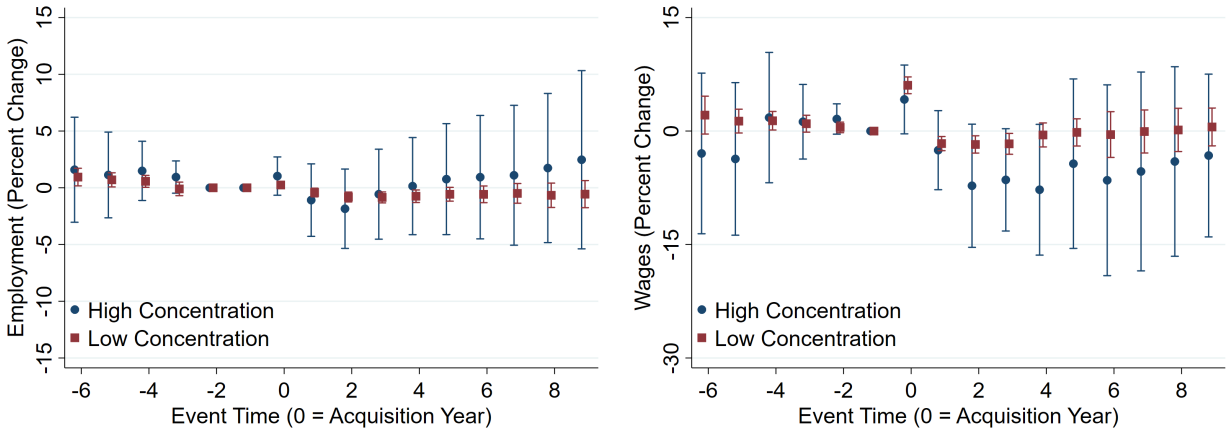
(b) 55-65 Wages

(c) WQ5 Employment

(d) WQ5 Wages

Figure D.5: Impacts of Add-on PE Acquisitions by Sector, Age, and Wage Quintile

Notes: This figure displays difference-in-difference estimates of the β_e coefficients from equation (1) using employment as an outcome variable in panels (a) and (c) and wages in panels (b) and (d). Panels (a) and (b) restrict to workers ages 55-65 in the year before acquisitions, and panels (c) and (d) restrict to workers in the top quintile of the wage distribution in the year before acquisitions. The four series in each panel display estimates from separate regressions restricting to the sample of workers involved in add-on deals or not, in tradable industries or not, using the geographic tradable definition from Mian and Sufi (2014). 95% confidence intervals are constructed from standard errors clustered at the firm level.



(a) Employment (b) Wages

Figure D.6: Impacts of Add-on PE Acquisitions by Concentration

Notes: This figure displays difference-in-difference estimates of the β_e coefficients from equation (1) using employment as an outcome variable in panel (a) and wages in panel (b). The two series in each panel display estimates from separate regressions restricting to the sample of workers involved in add-on deals in markets with above median HHI increases (exceeding 64) from the year before to after the deal, and starting at an HHI exceeding the 75th percentile (1300). 95% confidence intervals are constructed from standard errors clustered at the firm level.

Table D.1: Impacts of Add-on and Non-Add-on PE Acquisitions on Workers With Owners

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Add-on				Not Add-on			
Outcome		Full	WQ5	55-65	Manager	Full	WQ5	55-65	Manager
Wages	β_0	4,005.5	12,320.2	3,972.0	9,309.6	6,178.0	17,689.4	5,252.6	12,978.8
		(461.2)	(1,241.5)	(569.3)	(1,208.2)	(1,110.3)	(2,793.5)	(1,089.9)	(2,124.9)
	$\beta_{1–4}$	−2,895.0	−12,047.5	−8,681.7	−14,612.9	−1,788.7	−8,746.7	−6,771.9	−7,887.3
		(636.1)	(2,498.6)	(957.6)	(2,439.7)	(1,120.7)	(3,304.1)	(1,361.4)	(2,909.6)
	$\beta_{5–9}$	−1,843.4	−8,314.1	−6,148.6	−12,298.3	−1,358.1	−9,191.7	−7,345.5	−7,744.3
		(1,146.9)	(3,413.9)	(2,504.3)	(3,943.4)	(1,390.5)	(3,189.5)	(2,384.1)	(2,881.1)
Employment	β_0	0.0035	0.0011	0.0025	−0.0006	0.0012	0.0002	0.0040	0.0027
		(0.0015)	(0.0010)	(0.0053)	(0.0018)	(0.0021)	(0.0018)	(0.0042)	(0.0022)
	$\beta_{1–4}$	−0.0115	−0.0344	−0.0469	−0.0427	−0.0013	−0.0153	−0.0115	−0.0131
		(0.0036)	(0.0042)	(0.0064)	(0.0067)	(0.0035)	(0.0032)	(0.0077)	(0.0059)
	$\beta_{5–9}$	−0.0072	−0.0329	−0.0489	−0.0341	0.0039	−0.0141	−0.0120	−0.0113
		(0.0064)	(0.0082)	(0.0086)	(0.0117)	(0.0052)	(0.0052)	(0.0118)	(0.0068)
LTCG	β_0	10,899.4	46,414.7	20,632.0	43,646.8	5,751.1	24,926.9	11,892.0	21,840.6
		(741.1)	(2,948.4)	(1,598.1)	(2,803.4)	(504.4)	(2,042.7)	(976.4)	(2,522.0)
	$\beta_{1–4}$	1,359.0	6,552.5	2,929.2	7,763.7	643.0	3,951.0	1,932.7	3,609.1
		(256.3)	(1,058.0)	(574.6)	(750.1)	(259.5)	(817.2)	(400.9)	(915.2)
	$\beta_{5–9}$	243.1	1,887.9	251.0	3,489.5	821.6	3,937.6	1,918.9	3,503.2
		(318.8)	(1,077.7)	(595.2)	(862.8)	(397.3)	(1,037.4)	(581.2)	(1,060.9)
Obs		13,148,466	2,704,640	2,171,677	882,765	14,532,266	2,947,669	2,254,406	1,019,217
Clusters		13,042	13,042	12,049	10,664	6,836	6,836	6,466	5,690
Pre-Deal Wage Mean		79,839.8	161,226.8	89,298.2	126,889.6	86,905.1	178,512.3	94,193.7	136,044.4
Pre-Deal Employment Mean		1.0000	1.0000	1.0000	1.0000	1.0000	1.0000	1.0000	1.0000
Pre-Deal LTCG Mean		1,044.8	3,939.2	1,685.9	4,134.8	830.5	3,557.9	1,395.3	3,177.4

Notes: This table reports difference in difference estimates of β from equation (1), replacing post-match year indicators with indicators for an observation being in the acquisition year, the first 4 years after the acquisition, and years 5–9 after the acquisition. The estimation sample in the first 4 columns includes all workers in our workers panel that are associated with add-on acquisitions, while the estimation sample in columns 5-8 includes all workers that are associated with acquisitions that are not add-ons. Standard errors are clustered at the firm level and reported in parentheses. The pre-reform mean is the average value of the outcome variable in each column for workers at acquired firms in the year before the acquisition.

Table D.2: Net Present Value Impacts of Add-on PE Acquisitions on Workers Excluding Owners

	(1) Full Sample	(2) WQ5	(3) 55-65	(4) Manager	(5) Rank & File
			Panel A: 5% Discount Rate		
Wages	−3,799.1	−16,701.9	−27,645.8	−28,546.2	−3,056.4
	(5,877.9)	(20,909.1)	(13,586.6)	(20,595.3)	(6,041.3)
	[−2.5, 1.3]	[−5.0, 2.1]	[−11.3, −0.2]	[−7.5, 1.3]	[−2.8, 1.7]
LTCG	6,014.3	30,567.2	8,811.4	29,855.5	392.7
	(1,881.7)	(7,328.8)	(3,801.5)	(4,557.4)	(931.0)
	[14.7, 61.1]	[29.4, 81.4]	[5.8, 69.9]	[45.3, 83.9]	[−23.8, 36.8]
			Panel B: 7% Discount Rate		
Wages	−3,257.1	−14,572.7	−25,241.6	−25,391.8	−2,799.0
	(5,318.3)	(19,105.6)	(12,118.3)	(18,755.0)	(5,437.1)
	[−2.4, 1.3]	[−4.9, 2.1]	[−10.8, −0.3]	[−7.2, 1.3]	[−2.8, 1.6]
LTCG	5,941.2	29,819.6	8,872.5	28,696.5	432.5
	(1,718.4)	(6,714.2)	(3,494.3)	(4,124.7)	(858.0)
	[17.6, 63.8]	[32.8, 84.6]	[9.4, 73.4]	[48.3, 86.2]	[−22.6, 38.2]
			Panel B: 9% Discount Rate		
Wages	−2,771.1	−12,663.0	−23,104.4	−22,594.5	−2,564.9
	(4,834.4)	(17,525.8)	(10,858.2)	(17,147.5)	(4,916.3)
	[−2.3, 1.3]	[−4.7, 2.2]	[−10.4, −0.4]	[−7.0, 1.4]	[−2.7, 1.6]
LTCG	5,874.8	29,156.3	8,917.4	27,680.7	467.3
	(1,578.3)	(6,186.6)	(3,228.9)	(3,757.7)	(795.4)
	[20.6, 66.5]	[36.3, 87.9]	[13.1, 76.9]	[51.4, 88.7]	[−21.4, 39.7]

Notes: This table reports the net present value of cumulative post-reform difference in difference estimates of β from equation (1) for each year from the year of the acquisition through 9 years after the acquisition, using a discount rate of 5% in Panel A, 7% in Panel B, and 9% in Panel C. The estimation sample in column 1 includes our entire worker panel data set. Columns 2, 3, and 4 restrict to top wage quintile workers, 55-65 year old workers at the time of the acquisition, and managers. Column 5 restricts to all workers that are not in the top wage quintile, are less than 55 years old at the time of the acquisition, and are not managers. All samples exclude owners and restrict to workers associated with add-on acquisitions. Standard errors are clustered at the firm level and reported in parentheses. We report 95% confidence intervals in percentage terms in brackets, scaling the coefficient estimate by the net present value of the sum of the outcome variable for the control group from the year of the acquisition through 9 years after the acquisition.

Table D.3: Net Present Value Impacts of Non-Add-on PE Acquisitions on Workers Excluding Owners

	(1) Full Sample	(2) WQ5	(3) 55-65	(4) Manager	(5) Rank & File
			Panel A: 5% Discount Rate		
Wages	74.7	−21,534.9	−31,271.4	−17,693.4	7,604.5
	(8,112.2)	(22,137.0)	(13,727.6)	(16,589.6)	(9,880.0)
	[−2.5, 2.5]	[−5.4, 1.8]	[−12.4, −0.9]	[−5.1, 1.5]	[−2.2, 5.1]
LTCG	5,103.0	26,233.6	11,990.5	21,013.6	190.6
	(2,067.3)	(5,793.3)	(2,953.7)	(5,798.4)	(1,424.8)
	[6.3, 55.1]	[27.2, 68.8]	[30.0, 86.0]	[19.9, 66.8]	[−34.9, 40.0]
			Panel B: 7% Discount Rate		
Wages	523.3	−18,083.9	−28,002.8	−14,877.9	6,975.7
	(7,368.8)	(20,278.7)	(12,412.6)	(15,204.4)	(8,911.0)
	[−2.3, 2.5]	[−5.2, 1.9]	[−11.9, −0.8]	[−4.9, 1.6]	[−2.1, 5.0]
LTCG	4,838.8	24,848.8	11,387.4	19,833.8	129.2
	(1,873.2)	(5,296.8)	(2,688.7)	(5,354.3)	(1,277.6)
	[7.7, 55.9]	[28.8, 70.2]	[32.2, 87.6]	[21.0, 68.1]	[−35.0, 38.8]
			Panel B: 9% Discount Rate		
Wages	920.3	−15,057.8	−25,141.4	−12,418.1	6,441.5
	(6,722.2)	(18,646.9)	(11,280.1)	(13,989.5)	(8,073.3)
	[−2.2, 2.5]	[−4.9, 2.1]	[−11.3, −0.7]	[−4.7, 1.8]	[−2.1, 4.9]
LTCG	4,610.1	23,637.7	10,858.6	18,800.1	80.5
	(1,706.4)	(4,871.4)	(2,461.6)	(4,971.0)	(1,151.1)
	[9.0, 56.8]	[30.5, 71.8]	[34.4, 89.3]	[22.1, 69.5]	[−35.0, 37.6]

Notes: This table reports the net present value of cumulative post-reform difference in difference estimates of β from equation (1) for each year from the year of the acquisition through 9 years after the acquisition, using a discount rate of 5% in Panel A, 7% in Panel B, and 9% in Panel C. The estimation sample in column 1 includes our entire worker panel data set. Columns 2, 3, and 4 restrict to top wage quintile workers, 55-65 year old workers at the time of the acquisition, and managers. Column 5 restricts to all workers that are not in the top wage quintile, are less than 55 years old at the time of the acquisition, and are not managers. All samples exclude owners and restrict to workers not associated with add-on acquisitions. Standard errors are clustered at the firm level and reported in parentheses. We report 95% confidence intervals in percentage terms in brackets, scaling the coefficient estimate by the net present value of the sum of the outcome variable for the control group from the year of the acquisition through 9 years after the acquisition.