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 show that, while PE buyouts lead to employment and earnings drops among incumbent employees, the majority of these impacts are driven by outsized effects on owner employees, who realize large capital gains in the buyout year. Many owners leave the labor force, and despite the large liquidity infusion, exhibit little evidence of follow-on entrepreneurship. The remaining employment and wage declines after PE buyouts are concentrated among older, high-wage, managerial, and longer-tenured workers, with relatively precise null impacts on rank-and-file workers, even after add-on acquisitions. This paper shows that ownership is a critical form of worker heterogeneity when considering the incidence of firm-level shocks. Outside of owners and a few highly compensated employees, PE acquisitions have at most small effects on workers.

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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, Sadun, and Van Reenen 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, Haltiwanger, Handley et al. 2014), but identifying which types of workers are affected is crucial for assessing the welfare implications of PE acquisitions. Progress answering these questions has been limited by a lack of comprehensive data that can enable classification of worker types (by ownership and occupation), capture different income sources (wages, payouts, non-cash benefits), and provide sufficient statistical power to detect potential heterogeneity.

We overcome this challenge by creating a new linkage between U.S. PE deal data and administrative tax records. These rich data allow us to explore with sufficient precision heterogeneity in outcomes among groups of incumbent workers differentiated by age, income, managerial status, and tenure, and consider effects on payouts and other income sources.[2] Crucially, we identify owner employees in the data and explore both worker outcomes excluding owners and the effects on owner employment and business activity. We find declines in employment and wages following PE acquisitions, but these declines are almost entirely driven by business owners. In contrast, negative effects of PE buyouts do not extend to rank-and-file employees.

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, employee benefits, and worker occupations, and allow us to create linkages between firms, employees, and own-

[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."

[2] We focus on workers who were present at the firm a year before the acquisition, hereafter referred to simply as workers for brevity.

ers. We match the administrative tax records to proprietary PE deal 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 of less than one percent following buyouts that reverse within approximately six years. Earnings drop in the years following PE buyouts, taking nearly a decade to return to trend. The earnings decreases are at least three times as large as the employment decreases, pointing to the possibility of heterogeneous effects for different types of workers. In the year of the acquisition, we find a spike in earnings, likely driven by stock options and phantom stock plans that are categorized as wages. While this earnings spike is more than offset by earnings decreases in subsequent years, we also find an immediate increase in capital gains realizations, highlighting a critical source of heterogeneity. Some workers have substantial ownership stakes in the acquired firms and elect to sell their businesses in exchange for payouts (Eisfeldt, Falato, and Xiaolan 2023).

The outcomes of owner employees diverge sharply from all other workers after buyouts. To systematically identify owners in our data, we use Form K-1 for pass-through entities along with Form 1120 schedule G and historical dividend payment records for C-corporations. We then split our worker panel into two groups: owner employees and non-owner workers. We find large and persistent decreases in employment and earnings among owner employees on the order of 30-40 percent, and even larger increases in capital gains realizations. On the other hand, we cannot reject zero cumulative earnings or employment effects for non-owner workers, who make up nearly 99 percent of our worker panel.

We then focus on a broader set of outcomes among all owners, not just those that were also employees of the acquired firms. We 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. The negative employment effects are larger among older owners, whereas younger owners tend to remain employed and experience smaller earnings changes over the ten-year sample period we study. Surprisingly, we find little evidence that buyout-driven liquidity fosters serial entrepreneurship: despite large capital gains, owners experience substantial 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.

Turning back to the worker panel but excluding owners, the economically small and statistically insignificant employment and earnings effects of PE buyouts among non-owner employees masks some heterogeneity by age, income, managerial status, and tenure. For workers that are younger, in the bottom four wage quintiles, in non-managerial occupations, and with shorter tenure, we cannot reject zero effects of buyouts on employment and earnings. Focusing on the rank-and-file workers in the first three groups, we rule out negative cumulative earnings effects exceeding 1.6 percent. On the other hand, we find larger declines in employment and earnings for workers that are older, in the top wage quintile, in managerial occupations, and with longer tenure. While large impacts on managers in part explain the impacts on older, higher-wage, and longer-tenured workers because these characteristics are correlated, the largest negative effects are concentrated among managers that are older, have higher earnings, and have longer tenure. While these groups experience declines in employment and earnings, they also tend to receive payouts triggered by PE acquisitions that can exceed the size of their earnings losses. 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.[3]

We also consider whether there are effects of PE acquisitions on incumbent employees that are missed by standard measures of cash earnings, as private equity may cut costs by reducing employee benefits after acquisitions or force employees to make costly moves. We find little evidence that employees are more likely to move across zip codes after an acquisition. When it comes to employee benefits, we actually see some increases in the

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

first several years after the acquisition. Employees at acquired firms are modestly more likely to participate in a retirement plan, driven by an increase in defined contribution plans. Both employer health insurance contributions and employer-sponsored insurance coverage meaningfully rise.

Finally, we 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, have grown in prevalence and given the potential to create monopsony power, have led to increased regulatory scrutiny. Consistent with some of this, we estimate differences in short-run employment impacts between roll-ups and other acquisitions for older, higher-wage, managerial, and longer-tenured workers, but this pattern does not extend to other workers. 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 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, while Colonnelli, Lerner, Cruz et al. (2025) explore PE in the developing country context. More specifically, this paper contributes to the literature on the impacts of PE acquisitions of firms on workers. Davis, Haltiwanger, Handley et al. (2014) conducted a seminal study, exploring job creation and destruction at the establishment and firm level in the United States. They find that PE acquisitions lead to job losses, but the nature of their employment data does not allow either individual workers to be tracked over time or the exploration of heterogeneous effects on workers.[4] Other papers have filled this gap by looking at effects on individual workers.[5] A number of studies have explored the effect of PE buyouts on firm and worker employment and earnings in various European countries, coming to somewhat differing conclusions (Olsson and Tåg 2017; Antoni, Maug, and Obernberger 2019; Fang,

[4]Building on the earlier paper, Davis, Haltiwanger, Handley et al. (2025) study the heterogeneous impact of buyouts across public and private targets, credit conditions, and private equity groups.

[5]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, Nestoriak, and Wardlaw (2021) find that PE acquisitions lead to fewer workplace accidents, and Gornall, Gredil, Howell et al. (2024) find decreased employer satisfaction following buyouts.

Goldman, and Roulet 2025). Herkenhoff, Lerner, Phillips 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. Cornelli, Simintzi, and Vig (2019) study the relationship between PE employment and performance.

Our contributions rest on the 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 while owners realize large capital gains when selling their business to PE, they do not appear to use their liquidity to found or invest in new businesses, regardless of their age. These results stand in contrast to the literature that emphasizes the importance of liquidity in fostering entrepreneurship (Holtz-Eakin, Joulfaian, and Rosen 1994), underscoring heterogeneity in the extent to which financial constraints bind across contexts.

Third, using deal type classifications 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.[6] These types of deals 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. Despite this heightened concern, we do not observe differentially 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. This suggests regulatory attention may be better spent on product rather than labor markets when considering add-ons.[7] While our analysis compares add-on to

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

[7] Increases in profits arising from more concentrated product markets could raise worker earnings in ex

non-add-on PE acquisitions, another useful benchmark is to compare add-ons to merger and acquisition (M&A) deals. We do not find evidence that workers fare worse after add-ons than they do after M&As in the U.S. or abroad (Lagaras 2017; Arnold 2021; Arnold, Milligan, Moon et al. 2025; He and le Maire 2025; Lagaras 2025; Ma, Ouimet, and Simintzi 2025). In fact, using a broad sample of changes in corporate control that excludes leveraged buyouts, we find little evidence that employees fare worse after private equity acquisitions than they do after other ownership changes.

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, managerial status, and tenure even after excluding owners, that are consistent with some 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, leading to negative consequences for 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 also consistent with PE removing entrenched management, research showing PE-owned firms employ better management practices, and evidence that better managerial education and corporate boards can improve financial decision making (Ahern and Dittmar 2012; Carlin and Robinson 2012; Bloom, Sadun, and Van Reenen 2015; Giorcelli 2019; 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 and other dimensions of worker compensation enable us to make several additional contributions. We observe employer provided benefits and moves, dimensions not considered in typical analyses of worker outcomes, and find, if anything, increases in the provision of fringe benefits. Estimating the impacts of PE buyouts on capital gains

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.

realizations, we find that most of the workers negatively affected in the labor market receive payouts from PE buyouts that exceed their earnings losses, even after excluding those with larger ownership shares. We also 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. Our last contribution is that 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.

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 for all workers, and differentiates between owner employees and non-owner workers. Section 5 presents owner-level results. Section 6 explores the impacts of PE buyouts on non-owner workers, while section 7 analyzes the impacts of add-on deals. Section 8 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. 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 buyouts and exits. High leverage levels after buyouts concentrate firm ownership and reduce agency frictions (Kaplan 1989; Axelson, Strömberg, and Weisbach 2009). 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, Sraer, and Thesmar 2011; Bloom, Sadun, and Van Reenen 2015). Moreover PE buyouts can create value through government links (Colonnelli, Li, and Liu 2024). 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, Haltiwanger, Handley et al. 2014; Herkenhoff, Lerner, Phillips et al. 2025) and internationally (Boucly, Sraer, and Thesmar 2011; Bloom, Sadun, and Van Reenen 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, resulting in the loss of firm specific human capital or under- or unemployment due to search frictions.

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, income, and tenure.

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 liquidity, and potentially increasing wealth. It is well known from many contexts that increasing wealth can 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 newfound liquidity to start or invest in new ventures. This serial entrepreneurship might lead to higher future earnings and capital gains for bought-out owners.[8]

2.4 Roll-ups

In a roll-up or add-on 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 workers and consumers.

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 (Ahern 2012). 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. Despite mounting concerns, empirical evidence regarding roll-ups remains scant.

[8]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.

3 Data, Empirical Strategy, 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, Yagan, Zidar et al. 2019; Arnold 2021; Arnold, Milligan, Moon 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.[9] We observe firm ownership for pass-through 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 pass-through entities to exclude owner employees from some worker analysis. However, not all owners receive wages, so some owners may not appear in the worker panel. The owner panel allows us to cleanly identify all owners of pass-through 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

[9]While our core analysis focuses on workers and owners, we also measure whether workers switch firms and business owners receive income from different business ventures, both of which require tracking firms over time. To follow the firms acquired in PE acquisitions over time we assign a single unique ID to the original acquired EIN, and any new EIN in the year of the acquisition or later issuing W2s to at least 30% of the original EIN's workers from the year before the acquisition. These unique firm IDs improve upon simply tracking EINs over time, and the 30% cutoff is chosen conservatively in an attempt to avoid missing firms that scale down significantly, but this method can still lead to incorrectly flagging worker moves across firms after divestitures, divisional sales, or mergers. Therefore, our estimates of quantities like workers moving across firms are likely to overshoot the true numbers, though we find similar results using 50 and 70% cutoffs. Challenges tracking firms through PE acquisitions are not unique to our paper (Davis, Haltiwanger, Handley et al. 2014). Common practice in the literature studying firm level impacts of PE acquisitions is to restrict analysis samples to firms that can be tracked over time using similar methods. Our results do not depend on such a restriction because we are able to track workers and owners across firms.

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 Form W-2s in each year, setting wage income to zero in years in which workers do not receive a Form 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.[10] 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 identify moves, we use the zip code reported annually on Form 1040, while move distance is calculated as the distance between new and old zip code centroid latitudes and longitudes. We use Form W2 to measure the presence of any employer-provided retirement plan, the presence of a defined contribution plan, and employer's contributions to employer-sponsored health coverage. We use forms 1095-A, 1095-B, and 1095-C to measure monthly health insurance coverage.[11]

To construct the owner panel, we identify all owners of pass-through 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.[12] 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 schedule C 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

[10] 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.

[11] Employer health insurance contributions are only required to be reported by select employers, namely those that have issued at least 250 Form W-2s in the prior year, which could be endogenous to PE control. While available for fewer years, the Form 1095s are not subject to this potential selection concern. We find similar effects on health insurance using both forms.

[12] 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.

owners in the owner 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.

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

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

Our main treatment variable of interest is PE_i, an indicator for whether a worker or owner's firm was acquired by PE in event time 0. Event time fixed effects γ_t absorb market-wide factors in the lead up to deals, such as worker wages growing as seniority increases. Individual by original payer tax identification number fixed effects δ_i absorb time-invariant individual specific factors, such as an individual's family background or college education pre-buyout.[13] Our main coefficient of interest is β_e, the average difference between treated and control outcomes in event time e relative to the average difference in outcomes in the year before the PE buyout. We use many outcome variables Y_{it} across our worker and owner panels, 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

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

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-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 and column 2 displays the total number of workers in the year before the buyout. 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 panel. 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 schedule C 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 owner panel. There are 654,856 owner-

year observations and 47,971 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, Yagan, Zidar et al. 2019), 63% of owner year observations have positive wages. Close to 80% of owners are men, and the average owner age is 54.

4 Effects of PE Acquisitions on Workers

4.1 Effects of PE Acquisitions on All 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, Haltiwanger, Handley et al. (2014), we estimate muted employment effects.[14] 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.[15] Following

[14]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 work with a more recent sample of acquisitions, as tax data is not available in earlier years. 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.

[15]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

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) on the full sample of workers. 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.

Table 4 also presents results for a broader range of outcome variables. Column 3 shows that worker's decline in employment and wages 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. There is little evidence of substitution towards gig work or other self-employment. Column 4 shows a very small $56.9 average increase in Schedule C income over the four years after the deal. Consistent with earlier work on U.S. PE acquisitions focused on establishment net employment changes (Davis, Haltiwanger, Handley et al. 2014), we find workers do separate from the acquired firms at elevated rates, but they also find new jobs rapidly and rarely need to rely on benefit or transfer systems to bridge the gap. In the first four years after acquisitions, Column 5 shows 15.2% more workers leave their firms[16], but there are only very modest or null average increases in unemployment, disability, or social security income ($115.8, $0.0, $-59.4 respectively).

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 using outcome variables focused on the extensive margin of capital gains realizations

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 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.

[16]As discussed in section 3, our estimates of the prevalence of worker moves across firms may overstate true values because of challenges tracking firms through PE acquisitions. Our estimates are also larger than those in other papers because we do not restrict our analysis sample to only include workers associated with firms that we are able to track over time.

and benefit receipt. On the extensive margin, there is only an increase in capital gains realizations of 1.8% in the year of the deal. Any UI receipt increases by an economically small 1.5% in the four years after buyouts. Therefore, only small groups of workers are realizing capital gains or receiving UI benefits. 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 panel, and shows we rule out cumulative wage declines of more than 2.8% when using a 7% discount rate. 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 panel constructed without any wage restrictions. The estimates are similar to our baseline estimates.

While the aggregate employment and wage effects of PE that we estimate are somewhat muted and in line with previous studies, these small average effects mask significant heterogeneity. Despite the small increase in the fraction of workers realizing capital gains in the year of the buyout, there are large average increases in capital gains realizations across all workers, pointing to the fact that a small minority of workers have substantial ownership stakes in their firms. In the remainder of this section, we examine these workers directly, and show that the key drivers of small average employment and wage effects on all workers are very large effects on the narrow group of workers that are also owners. On the other hand, employment and wage effects are in many cases indistinguishable from zero for non-owner employees.

4.2 Accounting for Owner Employees

Previous studies of the labor market impacts of private equity have generally been unable to distinguish between owners and non-owners within firm workforces, but in this section we take advantage of our unique data to explore whether the impacts of buyouts differ for owner employees and non-owner workers. As discussed in section 2, acquisi-

tions may distinctly increase or decrease the future earnings of owners through several non-mutually exclusive channels. Owner employees are a small, highly-compensated group relative to non-owner workers. Owner employees make up 1.3% of all workers in the year before PE acquisitions, but receive 3.9% of all worker wages. Average wages for owner employees are $253,454.2 in the year before PE acquisitions, triple the average of $81,410.1 for non-owner employees.[17]

Figure 2 displays estimates of the impacts of PE acquisitions on workers, splitting the worker panel into owner employees and non-owner workers. The estimates demonstrate that changes in the employment, wages, and capital gains of owner employees are radically different than changes for non-owner workers after buyouts. Panel (a) shows that non-owner capital gains realizations 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 drastic changes in the employment, wages, and capital gains realizations of owner employees. Employment declines roughly 30% and wages decline close to 40% four years after acquisitions, while both declines remain above 30% a decade after buyouts. Capital gains realizations increase by 160% of baseline wages in the year of acquisitions, 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 when they sell their business to PE; the payout from the buyout.

Table 5 presents level estimates of equation (1), splitting the worker panel into owner employees in the left panel and non-owner workers in the right panel. In the first four years following the buyout there are no significant employment effects for non-owner workers, while non-owner earnings decline by a significant $1,329.0 per year and non-owner capital gains realizations increase by $3,602.0 in the year of the deal. On the other hand, owner employee earnings decline by $87 thousand per year for the nine years following buyouts, and capital gains realizations increase by $389,678.7 in the year of the

[17] Although owner employees have high wages on average, the top earner or top five earners within a firm are poor proxies for ownership status. In the year before PE buyouts only 28.7% of owner employees are the highest wage employee at their firm while 71.9% of owners are among the five highest wage employees.

deal.

Across employment, wages, and capital gains the outcomes of owner employees and non-owner workers diverge sharply after buyouts. Appendix Table C.4 summarizes the impacts of PE buyouts on non-owner workers on a net present value basis to facilitate comparison with similar estimates for the full worker panel including owners in Appendix Table C.2. Despite owner employees making up only 1.3% of all workers, removing them from the worker panel reduces the net present value of worker wage declines by 85% and capital gains realizations by 59% from the year of PE buyouts through nine years after the acquisitions.

5 Effects of Buyouts on Owners

In the previous section, we showed that owner employees play a strong role in driving the impacts of PE buyouts on workers. However, our previous analysis does not paint a complete picture of the impacts of PE buyouts on owners because not all business owners receive wage income, and therefore not all owners appear in our worker panel. To explore the trajectories of owners after PE buyouts more comprehensively, we turn to our owner panel, which tracks all owners of pass-through businesses acquired by private equity and their matched control firms, restricting to owners with equity stakes exceeding 1%. As discussed in section 2, buyouts may lead to large liquidity realizations that could foster new entrepreneurship and continued work for owners. On the other hand, substantial payouts may facilitate owners leaving the labor force.

Figure 3 plots estimates of β_e from equation (1) using the owner panel, tracking owner outcomes across extensive and intensive margins. Panel (a) of Figure 3 uses indicator variables for whether owners realize any capital gains, and have any schedule C income, wages, and business income, and Table 6 present analogous extensive margin regression estimates. Like owner employees, the owners in our owner panel experience a spike in capitals gains realizations, a large and persistent decline in the frequency of receiving wages and business income, and little change in schedule C income. The frequency of capital gains realizations spikes by 18% in the year of buyouts, employment declines by 13% in the four years after the buyout and 17% in the following five years, the frequency of owners receiving any business income immediately declines by 20% in the four years

after the buyout, and there is a slight uptick of a few percentage points in the frequency of owners receiving any schedule C income. As a summary measure we also define an indicator variable for owners not receiving any positive wages, business income, or schedule C income. There is an 18-19% increase in the fraction of owners receiving no positive income over the entire decade after the buyout, a first sign that owners are leaving the labor force after buyouts, not starting new businesses or continuing to work.

Panel (b) of Figure 3 displays intensive margin estimates, while Table 7 present analogous regression estimates.[18] The average owner realizes a capital gain of $2.6 million 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 and wages quickly decline after buyouts. In the four years after buyouts business income declines by an average of $579,235.4 per year and wages decline by an average of $40,965.7 per year. In the following five years business income declines by $476,726.5 per year and wages decline by $41,994.8 per year. 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 coming along with declines in labor supply.[19]

Adding to the evidence that owners are leaving the labor force, the final column of Table 6 shows that after buyouts, owners move to Florida at higher rates. The magnitude of the effect is large. In the year before buyouts, 8.8% of owners live in Florida. In the first four years after buyouts, owners that sold to PE are on average 1.1 percentage points (13.0%) more likely to reside in Florida, and the effect increases to 1.5 percentage points (17.6%) in the subsequent five years.[20] This result provides further suggestive evidence of the mechanism driving the large changes in labor and business activity among owners, that they are retiring to Florida following large liquidity events.

While owners elect to sell their firms to PE, for illustrative purposes, if we interpret

[18]We also plot the raw means of capital gains realizations, business income, wages, and schedule C income for treatment and control owners by year in Appendix Figure B.3.

[19]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.

[20]We find similar effects for larger groups of frequent retirement destination states like Florida, California, Maine, North Carolina, Hawaii, Colorado, New York, and Arizona.

the estimates in Tables 6 and 7 as driven by an exogenous liquidity shock, a $1 million increase in liquidity from a capital gain leads to a 4.8 percentage 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.[21] While not directly comparable, these reductions in employment and wage earnings are similar to those found in the wealth shock literature. For example, Bulman, Goodman, and Isen (2022) find that an additional $1 million increase in after-tax wealth leads to an $18,000 reduction in earnings.

Appendix Tables C.5 and C.6 show that while magnitudes change, our results are qualitatively 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 roughly $50 thousand per year for the entire decade after buyouts.

5.1 Serial Entrepreneurship

Increases in liquidity following buyouts may facilitate owners founding new firms if large influxes of monetary resources free up capital to invest in new projects. However, our results yield little evidence of serial entrepreneurship or owners founding new firms. Figure 3 shows 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 Table 8. Each set of columns plots estimates of β_e from equation (1) using the owner panel but focusing on wages and business income from the specific business each owner was matched to in columns 1 and 2, from all other businesses in columns 3 and 4, and from other businesses whose first year in operation is in the year before the acquisition or later in columns 5 and 6. Column 7 shows schedule C income estimates to capture any sole proprietor or self-employment activity. On average, owner wages from the business they elect to

[21] Capital gains increase by $2,586,909.5 in the year of the buyout, leading 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.

sell to PE decline by $61,834.2 over the first four years after the sale. Some owners do work at other businesses and new businesses after the sale, average wages from all other businesses and new businesses increase by $22,687.0 and $5,117.1 respectively. However, these increases do not come close to offsetting the decline in wages from the original business, and these patterns remain stable over nine years after the buyouts.

While we already established that owners total business income declines after selling to PE, Table 8 establishes that owners are not pivoting to new business ventures after selling to PE. Not only do owners experience a precipitous drop in business income from the businesses they sell, owners also receive on average $52,360.6 less business income per year from all other businesses and $5,530.7 less business income from explicitly new businesses over the four years after buyouts. These numbers do not reflect temporary losses from new businesses, they remain negative over the entire decade after buyouts. Owners also do not appear to replace lost wages or business income with self-employment, as schedule C income declines by an average of $1,039.0 per year over the four years after buyouts and $760.1 over the subsequent five years.

5.2 Heterogeneity by Owner Age

Thus far, our evidence shows that on average owners realize large capital gains, experience sharp and persistent declines in wages and business income, do not appear to start new businesses, and are more likely to move to Florida. Owners have an average age of 54 in the year before PE acquisitions, so it is not surprising that they might choose to sell their business and leave the labor force. However, some owners are substantially younger and may have whole careers ahead of them at the time of sales. In this section we explore whether owner trajectories after PE acquisitions differ by age.

Figure 4 plots estimates of β_e from equation (1), splitting the owner panel by owner age. Panel (a) confirms that older owners appear more likely to leave the labor force than younger owners. For owners age 65 and above in the year before the sale of their business, there is an immediate 30 percentage point increase in the frequency of receiving no positive wages, business income, or schedule C income. For younger owners below the age of 45 there is only a 10-15 percentage point increase.

Figure 4, panels (b) and (c) display intensive margin estimates for capital gains re-

alizations, business income, and wages, and Appendix Table C.7 displays analogous regression estimates. For owners between 25-45 at the time of the acquisition, we cannot reject zero average wage declines in the first four or next five years after acquisitions. On the other hand, owners between 45-65 experience persistent declines in wages of $40-$70 thousand per year. The series of estimates for workers age 65 and older in panel (c) confirms that many of the oldest owners are likely to retire sometime soon, even absent a sale to PE. In the first few years after PE acquisitions selling owner's wages decline relative to matched control owners, but that gap shrinks to zero by nine years after the acquisitions, reflecting that older control owners are also highly likely to stop working as they age into their seventies.

While Figure 4, panel (c) shows that younger owners continue to work after sales while older owners are less likely to do so, panel (b) shows similar large spikes in capital gains realizations and declines in business income across age groups. Appendix Table C.8 presents cumulative results for owners on a net present value basis over the year of PE acquisitions and the following nine years. Capital gains realizations rise by $3.0 million, while business income declines by $3.7 million. 95% confidence intervals cannot reject a 100% decline in business income over the 9 years after buyouts, rejecting declines for any age group smaller than 73%.

Our analysis of workers across the age distribution paints a consistent picture. 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 successful new businesses over the decade following PE buyouts.

6 Effects of Buyouts on Non-Owner Workers

Our previous results suggest that much of the effect of PE acquisitions on workers is driven by owners. However this owner-driven effect may still mask significant heterogeneity among other workers. Non-owner workers make up nearly 99% of total workers, and the impacts of PE acquisitions on this large group is interesting in its own right. We explore the impacts of PE acquisitions on non-owner workers in this section, using our worker panel while excluding owners.

6.1 Worker Heterogeneity

First, we explore whether the null impacts of PE acquisitions on non-owner workers mask heterogeneous impacts across worker types. Theories of PE value creation suggest private equity may break implicit contracts with highly compensated and longer-tenured senior employees who may be paid more than their marginal product (Lazear 1979; Shleifer and Summers 1988). Furthermore, as part of operational changes, PE may remove or replace senior and middle level managers.

To estimate heterogeneous employment and wage impacts of PE acquisitions on different subgroups of workers, we split our sample by worker age, wage quintile, managerial status, and tenure in the year before PE buyouts. Figures 5 and 6 display β_e coefficient estimates from equation (1) using employment and wages as outcome variables, with each series estimated separately within decade-wide age groups, within-firm wage quintiles, for managerial and non-managerial occupations, and by worker tenure at their firm.[22]

We first discuss heterogeneity by worker age. The estimates in Figure 5, panel (a) show that, 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 ages 45 and above we see substantial declines in employment. The greatest effect is for workers between 55 and 65, who experience close to a 3% reduction in employment in the four years after the buyout. This effect shrinks slightly but is persistent, with a 2% decline in employment nine years after the buyout. Our results are thus consistent with buyouts leading to early retirement for some workers, not just owners. The wage effects in Figure 5, panel (b) mirror the employment patterns, with zero or positive effects for younger workers, but persistent wage declines of roughly 6% for workers ages 55-65.

Panels (c) and (d) of Figure 5 show employment and wage effects by wage quintiles, defined within firms in the year prior to the buyout. For workers in the bottom 80% of the wage distribution we estimate null 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. On the other hand, top wage quintile employment declines persis-

[22]Our decade-wide age groups include the lowest but exclude the highest age, so the age 25-35 group spans workers over the age range [25,35).

tently by between 1 and 2%, and top wage quintile workers experience a persistent 3.5% decline in wages. 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, Goldman, and Roulet 2025).

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, Sadun, and Van Reenen 2015). Panels (a) and (b) of Figure 6 display employment and wage estimates for workers that are managers, workers in non-managerial positions, and all workers for which we can observe occupation in the year before buyouts. 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: slight declines in employment and wages after buyouts, which are marginally or not significant, followed by later increases that eventually reach or surpass pre-buyout levels. On the other hand, for managers, we estimate a 1% drop in employment over the first four years after buyouts that shrinks somewhat in later years, and a sharp and persistent wage drop of 4% over a decade after buyouts.

To explore heterogeneous impacts by worker tenure, we restrict our sample to all non-owner workers associated with PE acquisitions occurring in 2006 or later, matching the earliest deal year for the set of workers for whom we can observe occupation and yielding sufficient employment history to establish at least five years of tenure for all workers. Panels (c) and (d) of Figure 6 display estimates for this sample of workers, and split the sample into groups with tenure at their firm for < 5 years and ≥ 5 years.[23] Aggregate impacts on the sample of workers associated with these deals appear similar to estimates based on all workers, and longer-tenured workers experience larger declines in employment and wages than workers with short tenure, though the magnitude of the difference is smaller than when comparing across ages, the wage distribution, or managerial status. In summary, across all four worker characteristics that we split the sample on, we find statistically significant differences in earnings in the years after the

[23]Using 3 or 4 years to divide workers into shorter and longer tenure groups yields similar results.

acquisitions in both absolute and relative terms, and this holds even after accounting for differences in employment.

Our estimated effects by age, worker wage quintile, managerial status, and tenure are correlated because there is significant overlap between these different groups of workers. Appendix Table C.9 documents the rate of overlap between the different highly impacted worker groups. Higher wage workers are more likely to be older, more likely to be managers, and more likely to have longer tenure. Restricting to workers for whom we can observe occupations, 45% of managers are in the top wage quintile, 18% of managers are ages 55-65, and 60% of managers have tenure ≥ 5 years.

PE may replace managers in order to implement operational changes, targeting managers irrespective of their age, wage, or tenure. 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, managers, or longest-tenured workers, 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 understand whether the negative employment and wage effects we estimate for these groups are driven by occupation or other characteristics, we triply split our results by age group, wage quintile, and managerial status, and doubly split our results by tenure and managerial status.

Figure 7 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 high-wage non-managerial workers, the largest effects are concentrated among older, higher wage managers. Appendix Figure C.4 displays estimates for manager employment and wages by tenure. The largest effects are concentrated among longer-tenured managers. While PE undoubtedly has negative impacts on managers, these impacts are largest for managers whose wage appears likely to exceed their marginal product, the older, higher wage, and longer-tenured managers. Therefore, we interpret the evidence as consistent with PE breaking implicit contracts for workers whose wages exceed their marginal products.[24]

[24]These patterns could also be explained by more adverse effects of job transitions for those with higher levels of firm-specific human capital.

In the appendix, we explore worker heterogeneity while including owners in the analysis. Appendix Figures C.2 and C.3 split the sample by age, wage, occupation and tenure while including owners. Appendix Figures C.4 and C.5 respectively split the sample including owners by age, wage quintile and occupation, and by tenure and occupation. The difference in estimated impacts between older, higher-wage, and longer-tenured managers and other workers expands when including owners, with effect sizes usually double or more for highly impacted groups.

Thus far we have focused on employment and wage impacts of PE acquisitions on non-owner workers, but the heterogeneous wage declines we estimate in the years after buyouts omit important pieces of workers compensation. In contrast to post-buyout wage declines, in the year of the buyout, wage spikes increase by worker wage quintile, and are larger for managers than non-managers. Equity and equity-like compensation that counts as wages appears more concentrated among higher-wage and managerial workers, underscoring the heterogeneous payouts associated with PE buyouts. Confirming the presence of heterogeneous payouts, we present wage, employment, and capital gains realization regression estimates for all non-owner workers, and older, top wage quintile, managerial, and longer-tenured workers in Table 9. Capital gains realizations are substantial for all groups, with the largest realizations among top wage quintile and managerial workers. On a net present value basis, Appendix Table C.4 shows that higher wage workers and managers realize capital gains that are larger than their wage declines, while older workers capital gains are roughly 40% of their wage declines.

Appendix Table C.10 repeats the analysis presented in Table 9, but includes owners. Comparing estimates across the tables including and excluding owners, a consistent pattern emerges that once owners are excluded both employment and earnings effects on highly impacted groups of workers decline by approximately 50%, but do not shrink to zero, with some variation across groups. Comparing estimates in Appendix Tables C.2 and C.4, excluding owners reduces cumulative wage declines and capital gains realizations by roughly one half to two thirds across highly impacted groups of workers on a net present value basis.

In summary, some non-owner workers, in particular older, highly paid, longer-tenured managers, experience employment and wage declines after PE acquisitions. However, their wage declines come along with substantial capital gains realizations. The story is

different for rank-and-file workers. When we turn attention to workers not in the top wage quintile, below the age of 55, that are not managers, there is very little evidence of negative labor market impacts. We estimate no decline in employment, precise null increases in capital gains, and on a net present value basis, we reject cumulative wage declines exceeding 1.6% (see Appendix Table C.4).

6.2 Employee Benefits and Moving

We next explore whether our wage, employment, and capital gains realization estimates miss some negative impacts of PE acquisitions for non-owner workers. In theory, workers may be forced to take jobs in far-away locations to maintain similar incomes following buyouts. Furthermore, PE may seek to cut costs after acquisitions by reducing employee benefits or new jobs might offer less generous benefits. Table 10 examines these hypotheses, presenting estimates of β_e from equation (1) exploring moves and worker benefits. Columns 1 and 2 use indicator variables as outcomes for whether a worker moves to a new zip code, or moves to a new zip code with a centroid more than 50 miles away from their zip code in the year before buyouts.[25] Evidence is scant for PE buyouts leading to substantial numbers of worker moves. We cannot reject zero effects in the four years immediately following buyouts for either type of move or in the subsequent years for any move, though workers are 0.6% more likely to move more than 50 miles away between five and nine years following buyouts. Given their timing, this increase in long distance moves does not appear to be directly driven by relocations or layoffs imposed by PE acquisitions, and regardless, the economic magnitude of the increase is small.

The next four columns directly explore employee retirement and healthcare benefits. Column 3 uses an indicator for whether individuals are enrolled in a defined benefit plan as an outcome variable, while column 4 uses an indicator for whether individuals are enrolled in any retirement plan. We estimate increases in the take-up of retirement plans in the year of the buyout, with effects indistinguishable from zero in subsequent years.

[25]To retain the differences-in-differences nature of the design, for the pre- versus post-acquisition difference we compare moves after the acquisition from the base year relative to moves over a similar length of time before the acquisition. Specifically, the second difference compares zip code moves in years 0, 1-4, and 5-9 after the acquisition from their location 1 year before the acquisition to moves 5 years before the acquisition, 2 years before the acquisition, and the year before the acquisition relative to the zip code from 6 years before the acquisition, respectively.

While not statistically significant, we see suggestive evidence that PE moves workers away from defined benefit plans towards defined contribution plans. Column 5 shows employer dollar contributions to employer sponsored health insurance plans, while column 6 shows the number of months in the past year the individual was covered by an employer sponsored health insurance plan. Both employer insurance contributions and coverage appear to increase in the years following a buyout, although the effect on months of coverage is small.[26] The results thus suggest that significant numbers of non-owner employees are not forced into involuntary moves after PE buyouts, while they are more likely to have a retirement plan and receive more health insurance coverage and larger contributions from their employers.

7 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.

Approximately two-thirds of the deals in our sample are add-ons, in which a PE firm acquired an additional target. Appendix Figure B.1 shows that the frequency of add-ons has been increasing in recent years. Add-on targets tend to be smaller firms, so they account for slightly less than half of the employees in our sample. The effects of add-ons on workers may differ from other deals for several reasons, so employment and wage effects could be larger or smaller than other deals. First, add-ons may introduce 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 and efficiency. If some of these profits are shared with workers, this could increase wages. At the same time, increased labor market power

[26]The number of observations is significantly lower for the insurance outcomes in columns 5 and 6 of Table 10 because this information is only reported to the IRS following the passage of the Affordable Care Act.

might reduce wages.

7.1 Add-ons and Worker Heterogeneity

Figure 8 displays estimates of β_e from equation (1), using our sample of all non-owner workers, and splitting the sample by the add-on status of deals. Panels (a) and (c) display employment estimates for all non-owner workers and managers, while panels (b) and (d) display wage estimates for all non-owner workers and managers. The results indicate that overall 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 8, 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. Overall, negative employment impacts for highly affected groups are larger when owners are included in the estimation sample, but persist after we exclude owners.

Table 11 presents further regression estimates of equation (1) excluding owners, displaying estimates for the whole sample, and older, higher-wage, managerial, and longer-tenured employees. The top rows show earnings, the middle rows show employment and the bottom rows show capital gains realizations. Columns 1-5 show results for add-on deals, while columns 6-10 show results for other PE deals. In column 1, which displays estimates for the full worker panel excluding owners, during the first four-years post buyout we estimate a statistically significant average wage decline after add-ons of \$1,663.7. In column 6 we estimate an insignificant \$1,022.0 decline after other deals. In the following five years oint estimates of wage effects for add-on deals recede and are

similar to estimates for other deals, with neither effect statistically significant. The lack of positive wage gains following add-on 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.

Turning to older, higher-wage, managerial, and longer-tenured employees, Table 11 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 somewhat 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.

As is the case with all deals in our sample, highly impacted groups of workers realize substantial capital gains after add-ons that often exceed earnings losses. Appendix Table D.1 shows that on a net present value basis, cumulative capital gains realizations exceed wage declines for high wage and managerial workers after add-ons. On the other hand, rank-and-file workers realize an insignificant $447.6 in capital gains, and we can rule out cumulative wage declines for these workers exceeding 2.8%. Appendix Table D.2 documents similar patterns after non-add-on acquisitions, ruling out wage declines for rank-and-file workers exceeding 2.0%.

Appendix Table D.3 presents further regression estimates of equation (1) including owners, displaying estimates for the whole sample, and older, higher-wage, managerial, and longer-tenured employees. When we include owners, estimated aggregate employment and wage effects are more negative for add-ons and other deals. Impacts on older, higher-wage, managerial, and longer-tenured 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, including owners in the analysis yields more negative estimates of the effect of add-ons on all workers, because owners may be particularly likely to depart their firm after such acquisitions, and because add-ons target smaller businesses where owners mechanically make up more of the workforce. On the other hand, when we consider typical, non-owner workers, we cannot reject null effects of add-ons.

7.2 Add-on Effects by Tradability and Concentration

Add-on acquisitions may affect workers by increasing firms' product market and 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.2 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).[27] The results do not indicate a clear pattern, with effects generally statistically indistinguishable from one another. That said, if taking the point estimates at face value, they indicate that employment declines more after 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 median HHI that experience above median changes in HHI from the year before to the year after PE deals.

Appendix Figure D.4 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

[27] Appendix Figure D.3 repeats the analysis for the most sensitive groups. However, these results generally lack precision and have wide confidence intervals.

estimates 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 overlap, 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 some roll-up deals.

We can draw three implications from the previous sections. 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 among owners, as well as older, higher-earning, managerial and longer-tenured employees. For the latter groups, we observe effects even when excluding owner employees with substantial ownership stakes. Third, we do not see clear evidence of differences in earnings effects between add-ons and other deals when considering all workers, indicating that add-on deals are not particularly harmful to workers. This suggests that concerns about increased labor market concentration from add-on acquisitions may be overstated, although we cannot rule out non-trivial affects after deals associated with the largest concentration levels and changes.

7.3 Comparing LBOs to Other Deals

A natural question, given our consistent findings of muted employment and earnings effects, is whether there is something special about private equity acquisitions, or whether the impacts of LBOs on workers are similar to the impacts of other changes in corporate control. To address this question directly, we identify a large set of non-LBO changes in control in our data, using all instances where a pass-through business has a new majority owner who appears likely to originate from an arms length transaction.[28] We then match these businesses to control firms following the approach outlined in section 3, and construct a panel of workers associated with firms undergoing non-LBO changes in

[28]We focus on all pass-through businesses with a new majority owner after excluding i) any firms ever acquired in an LBO, ii) any transactions where the new majority owner is an estate or trust, and iii) any transactions where the new and old owner last names share the same first four characters.

control and their matched controls.

Appendix Table D.4 displays regression estimates of equation (1), where columns 1 and 2 reproduce our main PE acquisition results, while columns 3 and 4 show estimates from the non-LBO change in control worker panel. We observe similar small declines in the propensity to work after PE and other acquisitions in the first few years, and cannot reject no declines in employment over the first four and next five years after the non-PE acquisitions. We do not observe a wage spike following non-PE changes in control. Instead, there is a significant decline in wages in the year of these changes in control. In the following years, we observe similar wage declines after PE and non-PE acquisitions. The results in Table D.4 suggest that, despite significant attention from policymakers, PE buyouts do not appear to have more negative employment and wage effects on workers than other changes in corporate control, and if anything, PE buyouts may have slightly more favorable impacts on workers.

8 Conclusion

This paper provides new facts on the labor market impacts of private equity. We show that employment and wage declines after PE acquisitions are largely driven by owners voluntarily retiring after receiving a large payout. Remaining employment and wage declines are driven by highly compensated, equity-holding senior management who also often realize capital gains that exceed earnings declines following PE buyouts. When we turn our attention to rank-and-file workers, private equity ownership leaves their long-term employment and earnings largely unharmed.

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 acquisitions on product markets, the degree of tax arbitrage behavior, the efficiency benefits of removing entrenched management, and other dimensions through which private equity may generate value for investors to better quantify the overall welfare consequences of these transactions.

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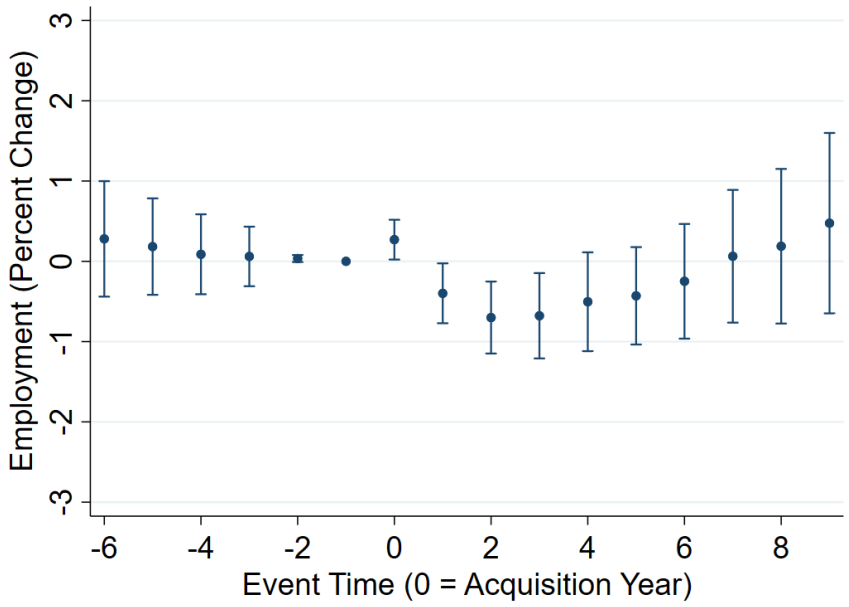
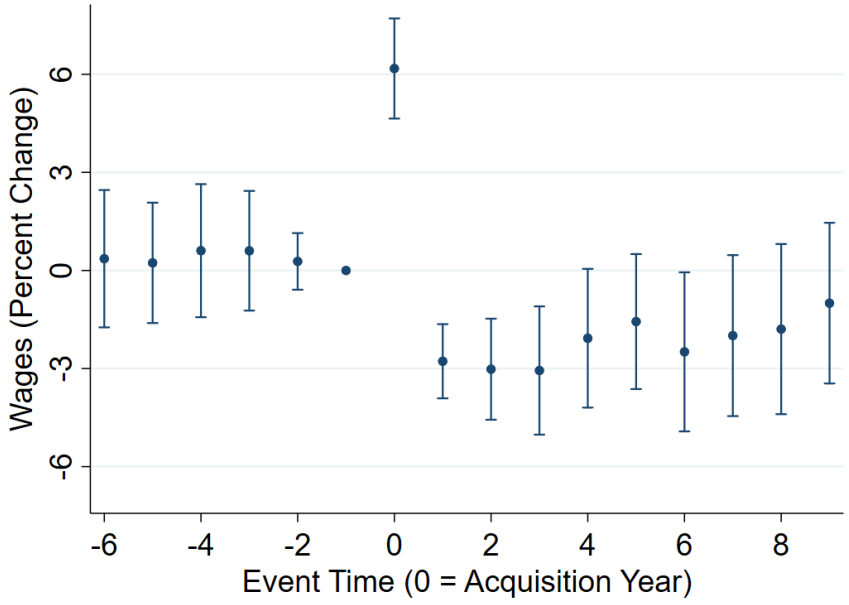


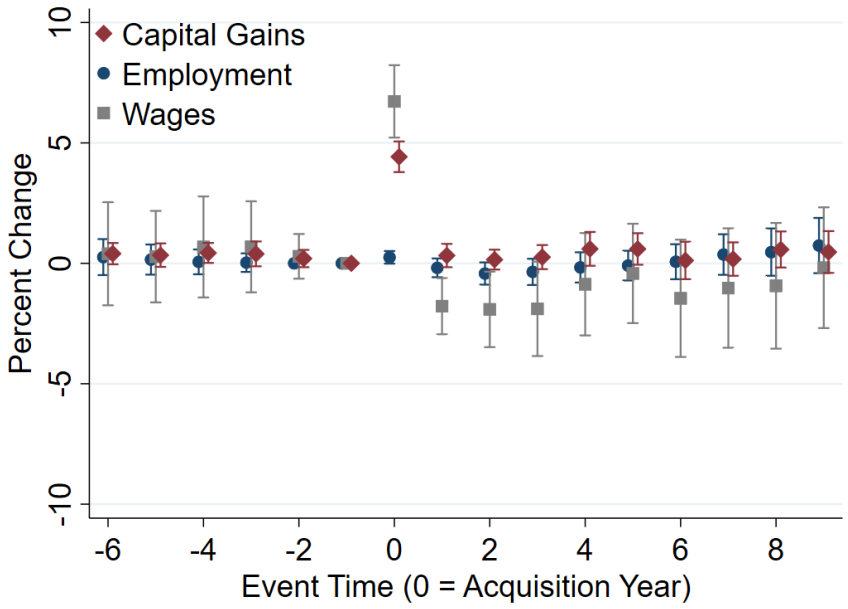
(a) Employment



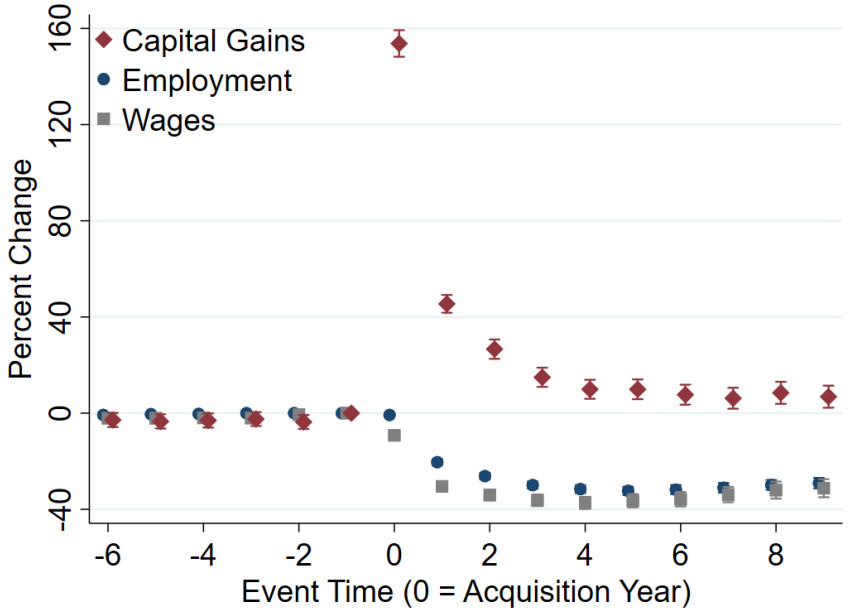
(b) Wages

Figure 1: Impacts of PE Acquisitions on All 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 the outcome in Panel (a), and wages as the outcome in Panel (b). The estimation sample is the entire worker panel. 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.



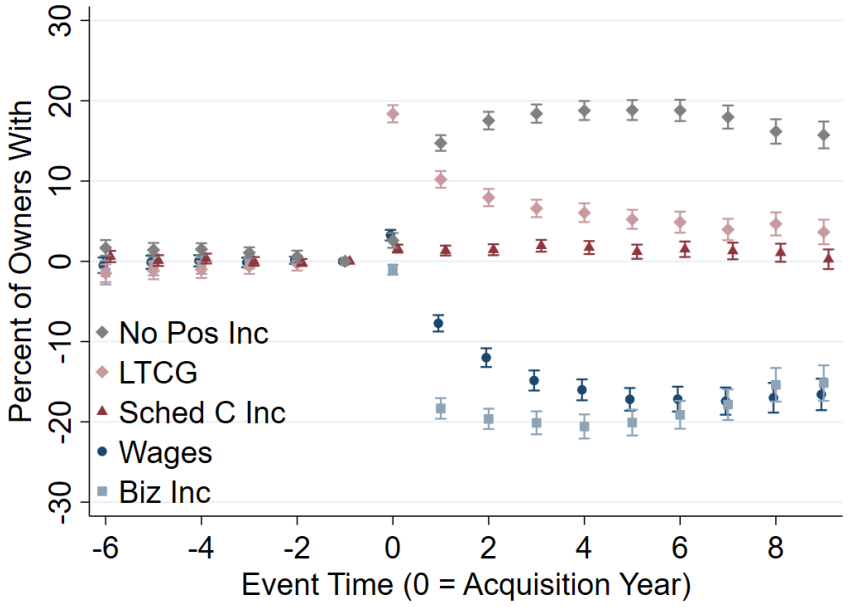
(a) Non-Owner Employees



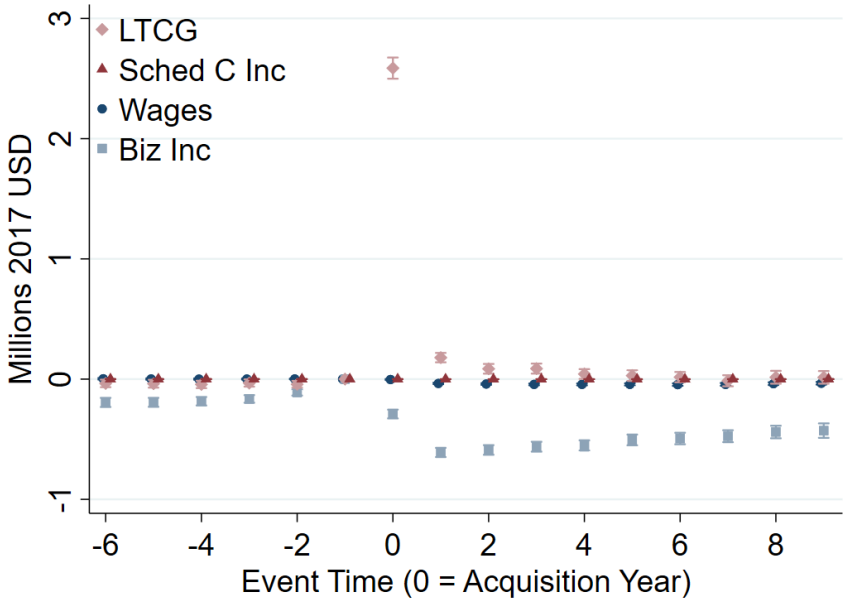
(b) Owner Employees

Figure 2: 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 panel, while panel (b) displays estimates from a regression on all owners in our worker panel. 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.



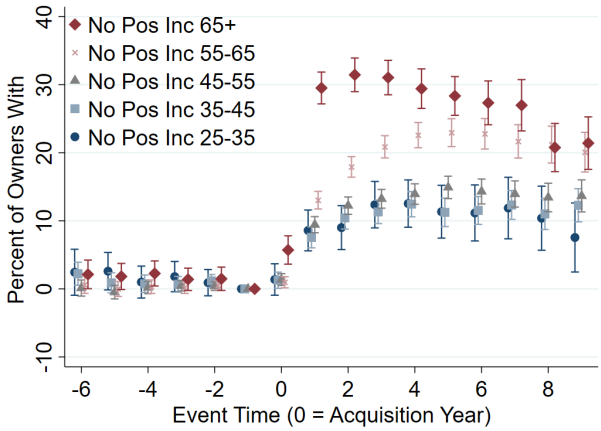
(a) Extensive Margin Income



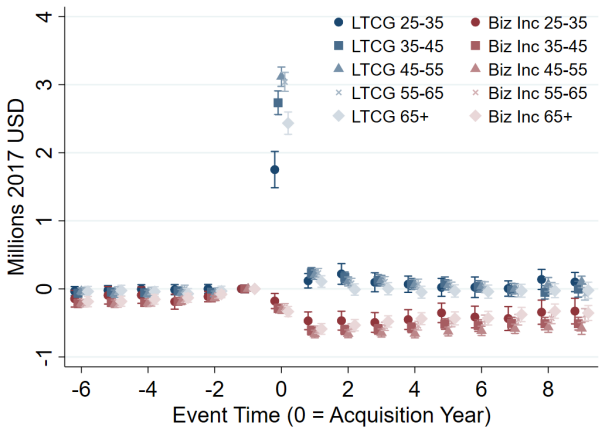
(b) Level Change in Compensation

Figure 3: Impacts of PE Acquisitions on Owners

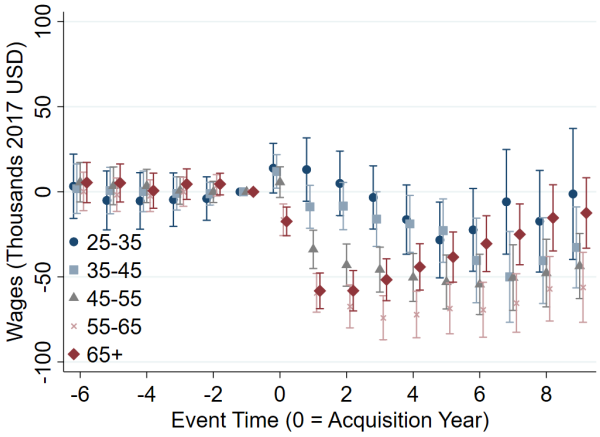
Notes: This figure displays difference-in-difference estimates of the β_e coefficients from equation (1) using our owner sample. Panel (a) displays estimates using dummy variables equal to 1 if an owner receives each type of income as outcome variables, where no positive income corresponds to owners having no positive wages, business income, or schedule C income. Panel (b) displays estimates using variable levels as outcome variables. "Biz Inc" is ordinary business income, "LTCG" is long-term capital gains, and "Sched C Inc" is schedule C income. The estimation sample is the entire owner panel. 95% confidence intervals are constructed from standard errors clustered at the firm level.



(a) Extensive Margin Income



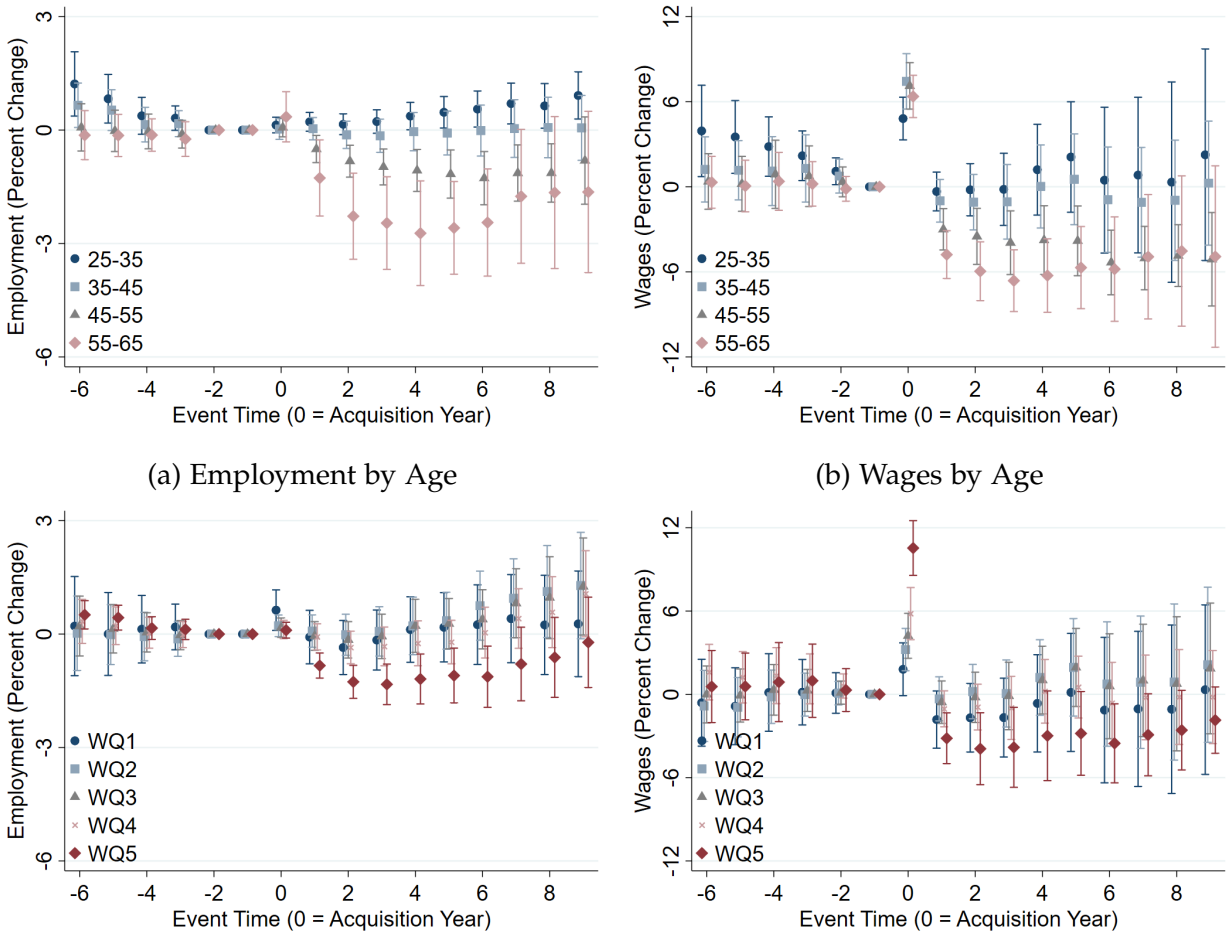
(b) Level Change in Biz Inc and LTCG



(c) Level Change in Wages

Figure 4: 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. Each series of estimates is from a separate regression performed on owners in the owner panel in the specified age range in the year before PE acquisitions. Panel (a) displays estimates using an indicator variable equal to 1 if an owner receives no positive wages, business income, or schedule C 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) Employment by Age

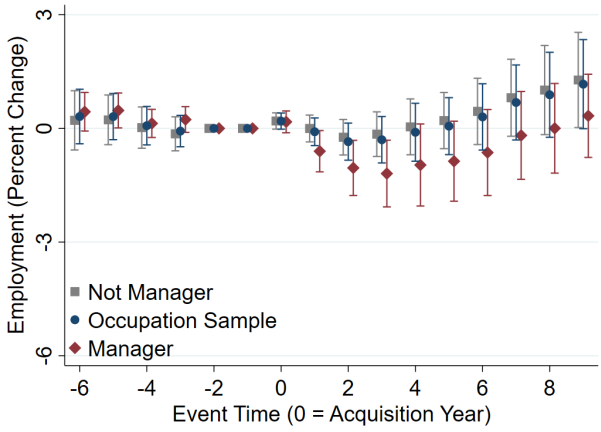
(b) Wages by Age

(c) Employment by Wage Quintile

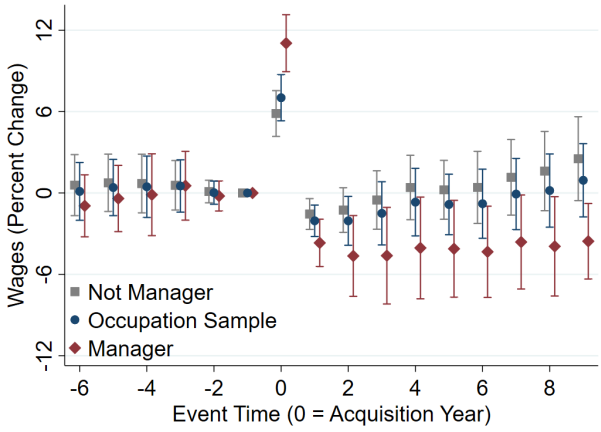
(d) Wages by Wage Quintile

Figure 5: Impacts of PE Acquisitions by Age or Wage Quintile

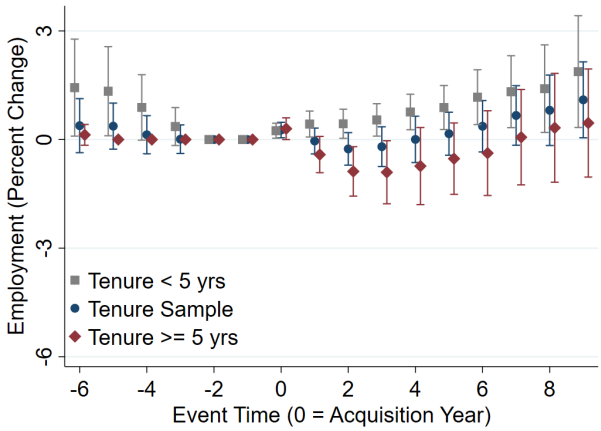
Notes: This figure displays difference-in-difference estimates of the β_e coefficients from equation (1) using a dummy variable equal to 1 if a worker is employed as the outcome in Panels (a) and (c), and wages as the outcome in Panels (b) and (d). Each series of estimates is from a separate regression performed on the specified subsample of the worker panel, excluding owners. Panels (a) and (b) display estimates for workers in different age ranges in the year before the PE acquisition. Panels (c) and (d) display estimates for workers in different within-firm wage quintiles 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 worker group in the year before the acquisition. 95% confidence intervals are constructed from standard errors clustered at the firm level.



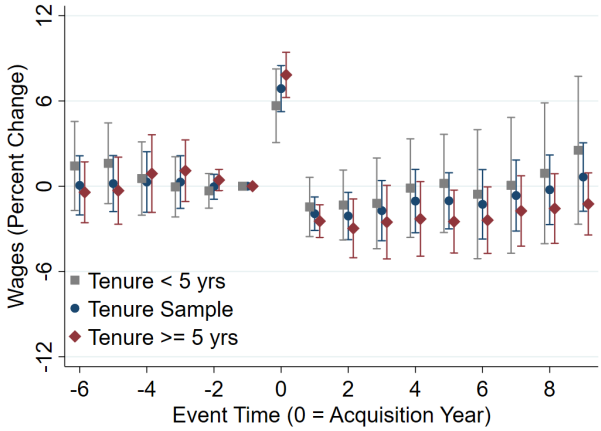
(a) Employment by Managerial Status



(b) Wages by Managerial Status



(c) Employment by Tenure



(d) Wages by Tenure

Figure 6: Impacts of PE Acquisitions by Managerial Status or Tenure

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 of estimates is from a separate regression performed on the specified subsample of the worker panel, excluding owners. Panels (a) and (b) display estimates for all workers for whom we observe occupation, as well as dividing workers into managerial and non-managerial occupations. Panels (c) and (d) display estimates for all workers associated with deals in 2006 and later, matching the first year of deals for which we can observe occupation, and yielding sufficient pre-period years to measure worker tenure. Panels (c) and (d) also split this sample of workers into those with < 5 years and ≥ 5 years of tenure at their firm in the year before the 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 subgroup in the year before the acquisition. 95% confidence intervals are constructed from standard errors clustered at the firm level.

(a) Manager Employment

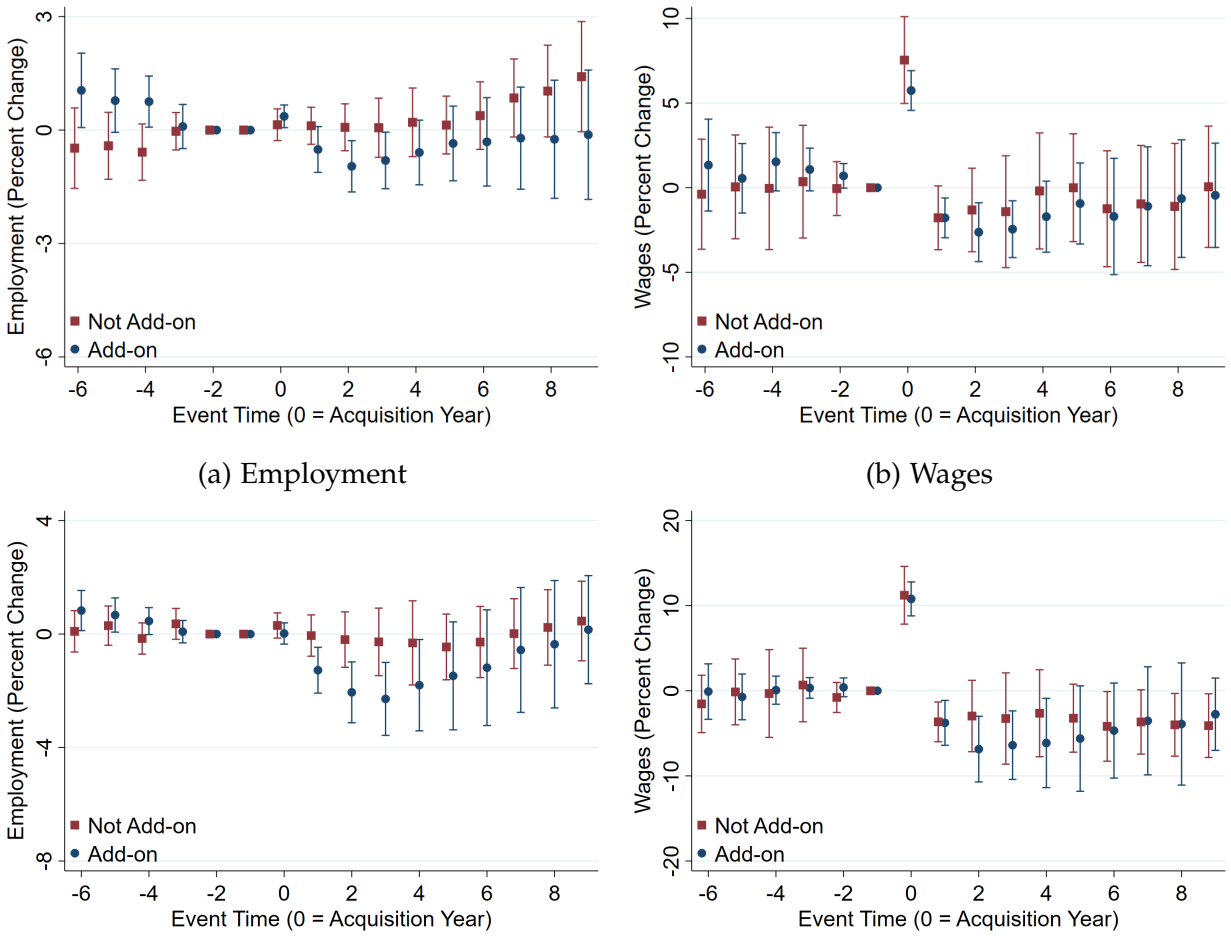
(b) Manager Wages

(c) Not Manager Employment

(d) Not Manager Wages

Figure 7: Impacts of PE Acquisitions by Managerial Status, Age, and Wage Quintile

Notes: This figure displays difference-in-difference estimates of average post-buyout 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). Each estimate is from a separate regression performed on the specified subsample of the worker panel, excluding owners. 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) Employment

(b) Wages

(c) Manager Employment

(d) Manager Wages

Figure 8: Impacts of Add-on 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 Panels (a) and (c) and wages as the outcome in panels (b) and (d). Each series of estimates is from a separate regression performed on the specified subsample of the worker panel, excluding owners, that are or are not associated with add-on acquisitions. 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
Matched Deals	11,155	2,007,409
Manufacturing	2,769	545,744
Service	2,742	365,587
Wholesale Trade	1,259	126,578
Health	723	105,438
Add-on	7,473	955,401

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 Panel 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	27,841,159	1,971,020
Capital Gains	2.2	38.8	0.0	0.0	0.0	27,841,159	1,971,020
Schedule C Income	0.6	9.6	0.0	0.0	0.0	27,841,159	1,971,020
Business Income	1.6	20.2	0.0	0.0	0.0	27,841,159	1,971,020
Unemployment	0.5	2.5	0.0	0.0	0.0	27,841,159	1,971,020
Disability	0.2	2.0	0.0	0.0	0.0	27,841,159	1,971,020
Social Security	1.1	5.1	0.0	0.0	0.0	27,841,159	1,971,020
Age	43	12	28	43	59	27,642,093	1,957,030
Panel B: Extensive Margin							
Owner Employees	0.01					27,841,159	1,971,020
Men	0.65					27,741,452	1,963,901
Managers	0.15					12,738,033	921,463
Add-on	0.48					14,391,697	1,015,380
Employed	0.91					27,841,159	1,971,020
Capital Gains	0.08					27,841,159	1,971,020
Schedule C Income	0.04					27,841,159	1,971,020
Unemployment	0.08					27,841,159	1,971,020
Disability	0.01					27,841,159	1,971,020
Social Security	0.06					27,841,159	1,971,020

Notes: This table reports summary statistics for our entire worker panel data set spanning 1999-2023. Intensive margin statistics in Panel A are reported in thousands of 2017 US dollars. Extensive margin statistics in Panel B are based on indicator variables. 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 Panel 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	47,971
Capital Gains	287.6	1,397.5	−33.9	0.0	371.2	654,856	47,971
Wages	164.3	300.1	0.0	58.0	421.1	654,856	47,971
Schedule C Income	5.5	31.7	0.0	0.0	4.6	654,856	47,971
Age	54	13	38	53	70	602,952	44,216
Panel B: Extensive Margin							
Men	0.77					620,715	45,480
No Positive Income	0.16					654,856	47,971

Notes: This table reports summary statistics for our entire owner panel data set spanning 1999-2023. Intensive margin statistics in Panel A are reported in thousands of 2017 US dollars. Extensive margin statistics in Panel B are based on indicator variables. 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 All Workers

	(1) Employment	(2) Wages	(3) LTCG	(4) Sched C Inc	(5) Leave Firm	(6) UI	(7) DI	(8) OASI
Panel A: Baseline Specification								
β_0	0.0024	5136.9	8250.4	68.1	0.0103	19.7	−2.7	−19.3
	(0.0013)	(615.1)	(444.6)	(15.8)	(0.0057)	(66.5)	(3.3)	(10.3)
$\beta_{1\text{-}4}$	−0.0061	−2316.7	991.3	56.9	0.1520	115.8	0.0	−59.4
	(0.0025)	(654.6)	(183.0)	(25.5)	(0.0111)	(25.7)	(11.7)	(34.7)
$\beta_{5\text{-}9}$	−0.0008	−1542.1	581.5	43.9	0.1323	37.1	5.9	−155.9
	(0.0041)	(906.8)	(262.4)	(41.6)	(0.0126)	(40.6)	(26.1)	(87.1)
Panel B: Industry by Year Fixed Effects								
β_0	0.0021	5342.5	8386.0	80.3	0.0108	21.0	−3.8	−22.5
	(0.0013)	(639.4)	(407.6)	(14.8)	(0.0041)	(65.4)	(2.5)	(10.0)
$\beta_{1\text{-}4}$	−0.0060	−2224.8	1001.3	76.5	0.1531	109.3	−2.2	−61.2
	(0.0024)	(666.8)	(170.9)	(22.3)	(0.0101)	(25.5)	(10.2)	(32.5)
$\beta_{5\text{-}9}$	−0.0003	−1391.4	630.0	74.6	0.1305	28.0	1.4	−154.3
	(0.0041)	(934.6)	(260.4)	(38.0)	(0.0109)	(40.6)	(25.5)	(77.0)
Obs	27,841,159	27,841,159	27,841,159	27,841,159	27,841,159	27,841,159	27,841,159	27,841,159
Clusters	20,075	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	0.0000	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-deal 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 Owner Employees and Non-Owner Workers

	(1)	(2)	(3)	(4)	(5)	(6)
	Owner Employees			Non-Owner Workers		
	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 worker panel, while the estimation sample in columns 4-6 includes all non-owners in our worker panel. Standard errors are clustered at the firm level and reported in parentheses. The pre-deal mean is the average value of the outcome variable in each column for workers at acquired firms in the year before the acquisition.

Table 6: Extensive Margin Impacts of PE Acquisitions on Owners

	(1) Employment	(2) LTCG	(3) Sched C Inc	(4) Biz Inc	(5) No Inc	(6) Florida
Panel A: Baseline Specification						
β_0	0.0326	0.1838	0.0158	−0.0104	0.0280	0.0061
	(0.0032)	(0.0055)	(0.0025)	(0.0032)	(0.0051)	(0.0009)
$\beta_{1–4}$	−0.1252	0.0776	0.0161	−0.1963	0.1812	0.0114
	(0.0054)	(0.0049)	(0.0031)	(0.0060)	(0.0052)	(0.0015)
$\beta_{5–9}$	−0.1707	0.0463	0.0114	−0.1802	0.1822	0.0154
	(0.0074)	(0.0058)	(0.0045)	(0.0084)	(0.0065)	(0.0026)
Panel B: Industry by Year Fixed Effects						
β_0	0.0340	0.1857	0.0159	−0.0102	0.0273	0.0061
	(0.0033)	(0.0054)	(0.0025)	(0.0031)	(0.0051)	(0.0009)
$\beta_{1–4}$	−0.1223	0.0792	0.0165	−0.1975	0.1793	0.0112
	(0.0052)	(0.0049)	(0.0031)	(0.0057)	(0.0051)	(0.0014)
$\beta_{5–9}$	−0.1672	0.0484	0.0121	−0.1810	0.1803	0.0150
	(0.0072)	(0.0058)	(0.0045)	(0.0080)	(0.0064)	(0.0025)
Obs	654,856	654,856	654,856	654,856	654,856	571,076
Clusters	15,742	15,742	15,742	15,742	15,742	15,236
Pre-Deal Mean	0.7027	0.6094	0.1883	0.9999	0.0595	0.0876

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, received any positive wages, business, or schedule C income, or filed a 1040 as a Florida resident. 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-deal mean is the average value of the outcome variable in each column for owners of acquired firms in the year before the acquisition.

Table 7: 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-deal mean is the average value of the outcome variable in each column for owners of acquired firms in the year before the acquisition.

Table 8: Impacts of PE Acquisitions on Owner New Business Activity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Matched Biz		All Other Biz		New Biz		
	Wages	Biz Inc	Wages	Biz Inc	Wages	Biz Inc	Sched C Inc
β_0	−19,090.0	−294,333.8	13,833.1	33,624.3	3,378.2	19,603.0	−34.1
	(1,929.0)	(14,178.1)	(1,013.0)	(7,107.6)	(294.1)	(2,082.3)	(233.1)
$\beta_{1–4}$	−61,834.2	−493,742.2	22,687.0	−52,360.6	5,117.1	−5,530.7	−1,039.0
	(2,861.0)	(15,058.4)	(1,661.8)	(6,544.5)	(426.5)	(1,265.3)	(288.5)
$\beta_{5–9}$	−52,623.5	−405,127.1	11,315.5	−44,544.8	4,290.1	−5,753.5	−760.1
	(3,890.0)	(17,827.3)	(2,281.0)	(7,812.4)	(2,281.0)	(1,556.2)	(387.7)
Obs	654,856	654,856	654,856	654,856	654,856	654,856	654,856
Clusters	15,742	15,742	15,742	15,742	15,742	15,742	15,742
Pre-Deal Mean	131,794.1	582,436.5	43,467.3	81,549.9	2,325.9	−1,453.8	5,495.6

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. The estimation sample includes our entire owner panel data set. Columns 1 and 2 display estimates for wages and business income from the original business we match the owner to. Columns 3 and 4 display estimates for wages and ordinary income from all other businesses. Columns 5 and 6 display estimates for wages and business income from all businesses whose first year in operation is in the year before the acquisition or later. Column 7 displays estimates for schedule C income. Standard errors are clustered at the firm level and reported in parentheses. The pre-deal mean is the average value of the outcome variable in each column for owners of acquired firms in the year before the acquisition.

Table 9: Heterogeneous Impacts of PE Acquisitions on Workers

Outcome		(1) Full	(2) WQ5	(3) 55-65	(4) Manager	(5) Ten≥5
Wages	β_0	5,474.1	17,307.0	5,560.2	13,758.1	7,006.8
		(624.1)	(1,651.5)	(663.7)	(1,335.1)	(726.3)
	β_{1-4}	−1,329.0	−5,720.1	−5,144.4	−5,304.9	−2,298.8
		(649.9)	(2,105.1)	(911.8)	(1,808.5)	(889.7)
	β_{5-9}	−672.0	−4,605.2	−4,581.0	−4,920.4	−1,765.5
		(902.9)	(2,255.3)	(1,851.5)	(2,042.0)	(973.2)
Employment	β_0	0.0025	0.0010	0.0035	0.0017	0.0030
		(0.0013)	(0.0011)	(0.0034)	(0.0015)	(0.0015)
	β_{1-4}	−0.0029	−0.0115	−0.0217	−0.0095	−0.0073
		(0.0025)	(0.0024)	(0.0057)	(0.0039)	(0.0039)
	β_{5-9}	0.0026	−0.0082	−0.0208	−0.0034	−0.0008
		(0.0040)	(0.0044)	(0.0081)	(0.0054)	(0.0062)
LTCG	β_0	3,602.0	15,181.5	6,416.3	11,349.0	5,874.4
		(264.7)	(1,009.2)	(497.5)	(950.8)	(554.9)
	β_{1-4}	266.1	1,932.7	740.5	2,011.7	648.7
		(165.2)	(601.3)	(289.7)	(434.9)	(230.8)
	β_{5-9}	311.9	1,858.2	435.7	2,181.0	736.8
		(265.1)	(782.0)	(418.7)	(607.1)	(376.7)
Obs		27,499,486	5,392,906	4,344,591	1,813,885	12,880,068
Clusters		20,070	19,798	18,464	15,871	16,888
Pre-Deal Wage Mean		81,410.1	164,196.4	87,245.7	124,591.6	89,423.3
Pre-Deal Employment Mean		1.0000	1.0000	1.0000	1.0000	1.0000
Pre-Deal LTCG Mean		483.5	1,960.6	730.4	1,709.3	795.6

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. The estimation sample in Column 1 includes all workers in our worker panel after excluding owners. Columns 2–5 restrict the sample to top wage quintile workers, age 55–65 workers, managers, and workers with tenure $\geq$ 5 years. Standard errors are clustered at the firm level and reported in parentheses. The pre-deal 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: PE Acquisition Impacts on Worker Location, Retirement, and Health Insurance

	(1)	(2)	(3)	(4)	(5)	(6)
	Location		Retirement		Insurance	
	Zip Move	Far Move	Only DB	Any Plan	Emp Contrib	ESI Coverage
β_0	0.0014	−0.0011	−0.0071	0.0185	359.2	0.0334
	(0.0019)	(0.0010)	(0.0041)	(0.0051)	(115.7)	(0.0327)
$\beta_{1\text{-}4}$	−0.0022	0.0019	−0.0092	0.0139	998.2	0.0783
	(0.0028)	(0.0015)	(0.0060)	(0.0087)	(148.5)	(0.0302)
$\beta_{5\text{-}9}$	0.0005	0.0057	0.0168	−0.0067	947.8	0.0863
	(0.0031)	(0.0022)	(0.0195)	(0.0199)	(175.9)	(0.0330)
Obs	23,720,830	23,720,830	27,279,706	27,103,573	13,359,786	7,092,945
Clusters	20,069	20,069	20,070	20,070	14,435	9,857
Pre-Deal Mean	0.2231	0.0691	0.0772	0.6211	3,998.9	11.3221

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 in our workers panel after excluding owners. Standard errors are clustered at the firm level and reported in parentheses. Zip Move is an indicator for filing a 1040 as a resident of a different zip code than the one in the year before the acquisition. Far Move is an indicator for filing a 1040 as a resident of a different zip code than the one in the year before the acquisition only if the new zipcode centroid is at least 50 miles from the original zipcode centroid. The move variables are mechanically 0 in the year before the acquisition. Therefore, to retain the difference in difference nature of the design, for the pre- versus post-acquisition difference we compare moves after the acquisition from the base year relative to moves over a similar length of time before the acquisition. Specifically, the second difference compares zip code moves in years 0, 1-4, and 5-9 after the acquisition from their location 1 year before the acquisition to moves 5 years before the acquisition, 2 years before the acquisition, and the year before the acquisition relative to the zip code from 6 years before the acquisition, respectively. Only DB is an indicator variable for workers only having a defined benefit plan, while Any Plan is an indicator for workers making contributions to any retirement plan. Emp Contrib are the employer dollar contributions to a health plan. ESI coverage is the number of months in the past year the individual was covered by an employer sponsored health insurance plan. The pre-deal 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 11: Impacts of Add-on and Non-Add-on PE Acquisitions on Workers

		(1)	(2)	(3) Add-on	(4)	(5)	(6)	(7)	(8) Not Add-on	(9)	(10)
Outcome		Full	WQ5	55-65	Manager	Ten ≥ 5	Full	WQ5	55-65	Manager	Ten ≥ 5
Wages	β_0	4,427.2	15,096.0	5,030.8	12,628.7	6,475.5	6,442.4	19,312.3	5,993.9	14,683.6	7,513.3
		(461.3)	(1,337.0)	(607.0)	(1,190.3)	(691.8)	(1,118.0)	(2,889.5)	(1,119.7)	(2,262.6)	(1,226.9)
	β_{1-4}	−1,663.7	−6,217.2	−5,617.6	−6,750.1	−3,282.9	−1,022.0	−5,290.1	−4,859.2	−4,112.5	−1,344.2
		(601.6)	(2,463.9)	(1,017.0)	(2,222.9)	(919.3)	(1,115.2)	(3,324.5)	(1,437.6)	(2,703.4)	(1,469.5)
	β_{5-9}	−786.2	−3,060.2	−3,723.7	−4,994.8	−1,857.1	−560.0	−5,905.4	−5,651.9	−4,980.3	−1,631.7
		(1,114.7)	(3,264.6)	(2,746.4)	(3,431.1)	(1,291.0)	(1,380.7)	(3,162.0)	(2,542.0)	(2,419.0)	(1,465.8)
Employment	β_0	0.0036	0.0016	0.0027	0.0002	0.0020	0.0014	0.0005	0.0042	0.0030	0.0039
		(0.0015)	(0.0010)	(0.0054)	(0.0019)	(0.0016)	(0.0021)	(0.0018)	(0.0042)	(0.0023)	(0.0026)
	β_{1-4}	−0.0072	−0.0175	−0.0377	−0.0186	−0.0149	0.0011	−0.0061	−0.0068	−0.0021	−0.0000
		(0.0035)	(0.0037)	(0.0068)	(0.0058)	(0.0050)	(0.0034)	(0.0030)	(0.0077)	(0.0053)	(0.0058)
	β_{5-9}	−0.0027	−0.0138	−0.0381	−0.0080	−0.0078	0.0070	−0.0034	−0.0062	−0.0005	0.0057
		(0.0063)	(0.0077)	(0.0089)	(0.0101)	(0.0097)	(0.0051)	(0.0050)	(0.0117)	(0.0059)	(0.0079)
LTCG	β_0	4,496.8	19,125.7	7,504.9	14,474.3	7,404.0	2,771.8	11,578.6	5,269.0	8,635.4	4,379.4
		(409.7)	(1,561.9)	(791.9)	(1,139.4)	(846.7)	(334.0)	(1,280.2)	(610.1)	(1,348.7)	(695.0)
	β_{1-4}	372.4	2,181.5	662.8	2,590.9	753.5	167.9	1,701.2	787.6	1,358.9	544.5
		(226.5)	(995.4)	(479.1)	(494.7)	(349.2)	(239.6)	(705.1)	(332.0)	(687.6)	(298.0)
	β_{5-9}	32.5	919.9	−296.6	1,715.6	420.0	536.3	2,585.5	1,110.0	2,142.3	1,005.5
		(326.0)	(1,185.1)	(589.3)	(908.4)	(483.4)	(398.0)	(1,032.7)	(560.8)	(876.0)	(567.3)
Obs		12,991,204	2,527,719	2,110,624	819,343	6,253,844	14,508,282	2,865,187	2,233,967	984,902	6,626,224
Clusters		13,157	12,937	11,978	10,184	11,327	6,913	6,861	6,486	5,545	5,561
Pre-Deal Wage Mean		77,092.3	153,322.7	83,709.1	116,982.4	86,214.8	85,419.8	174,144.2	90,950.6	130,830.5	92,563.8
Pre-Deal Employment Mean		1.0000	1.0000	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	764.2	517.5	2,249.5	785.5	1,909.8	826.4

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 5 columns includes all workers in our workers panel except for owners that are associated with add-on acquisitions, while the estimation sample in columns 6-10 includes all workers except for owners that are associated with acquisitions that are not add-ons. Columns 2–5 and 7–10 restrict these samples to top wage quintile workers, age 55–65 workers, managers, and workers with tenure ≥ 5 years. Standard errors are clustered at the firm level and reported in parentheses. The pre-deal 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.

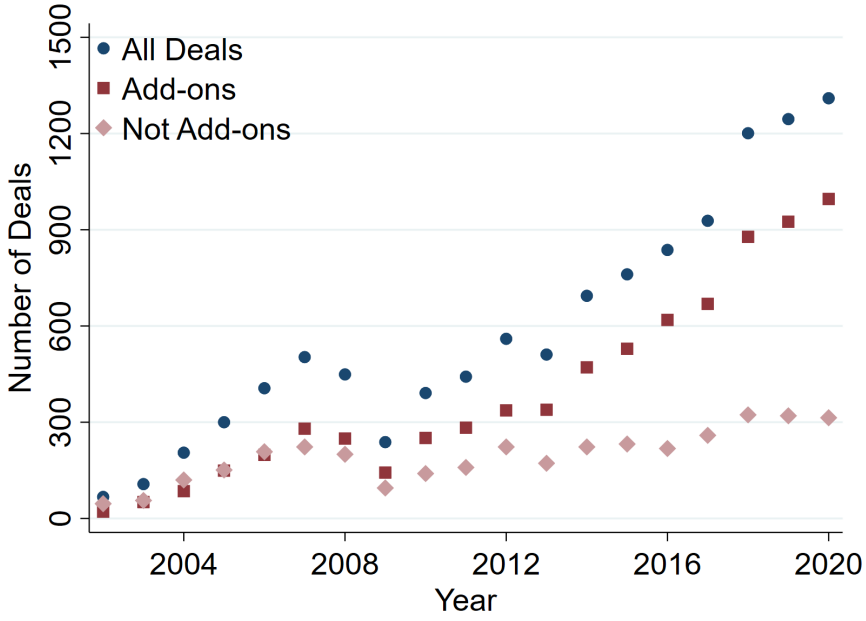
When filing taxes, the primary filer reports their occupation at the bottom of their Form 1040 next to the signature line. If the primary filer is filing taxes jointly, the secondary filer (i.e. the spouse) also reports their occupation. Each occupation is reported as a string variable. Data is available 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.

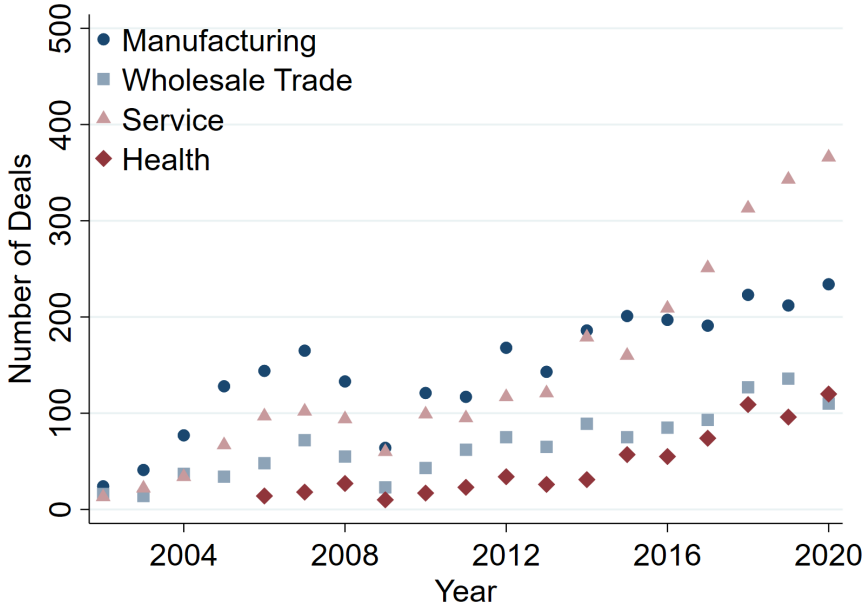
While our occupation measure is available for just under half of our worker panel,

we show that our aggregate results are similar in both the entire worker panel, 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



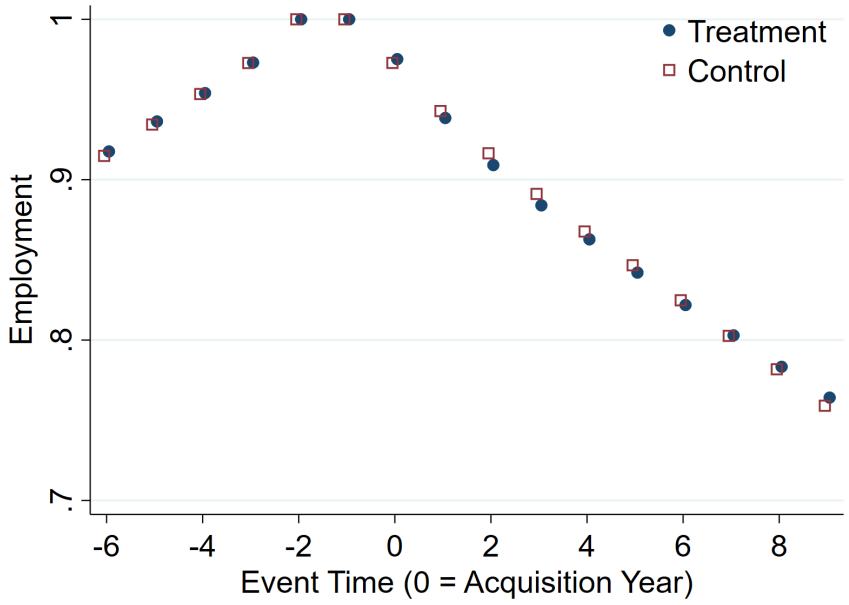
(a) By Deal Type



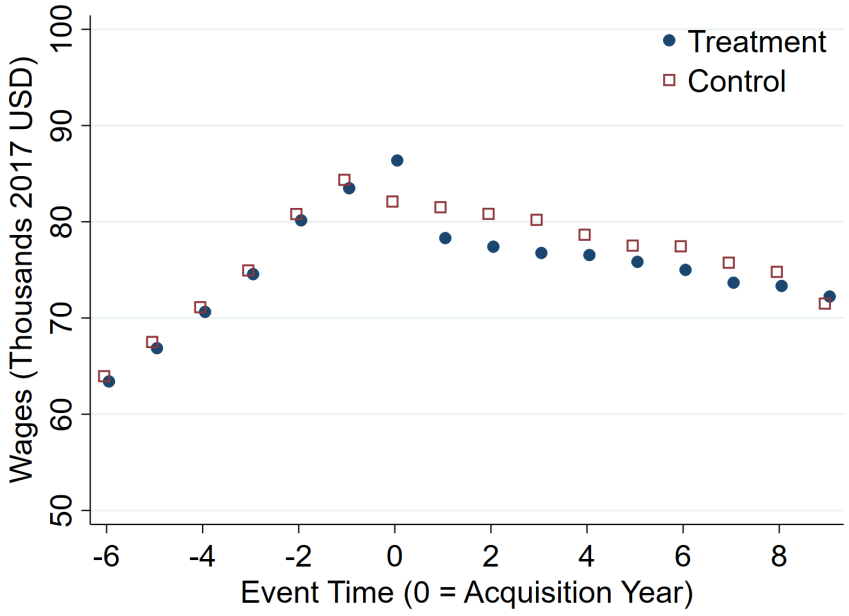
(b) By Industry

Figure B.1: Number of PE Deals

Notes: This figure displays the number of matched private equity deals in each year in our sample, dividing deals by deal type in panel (a) and by industry in panel (b).



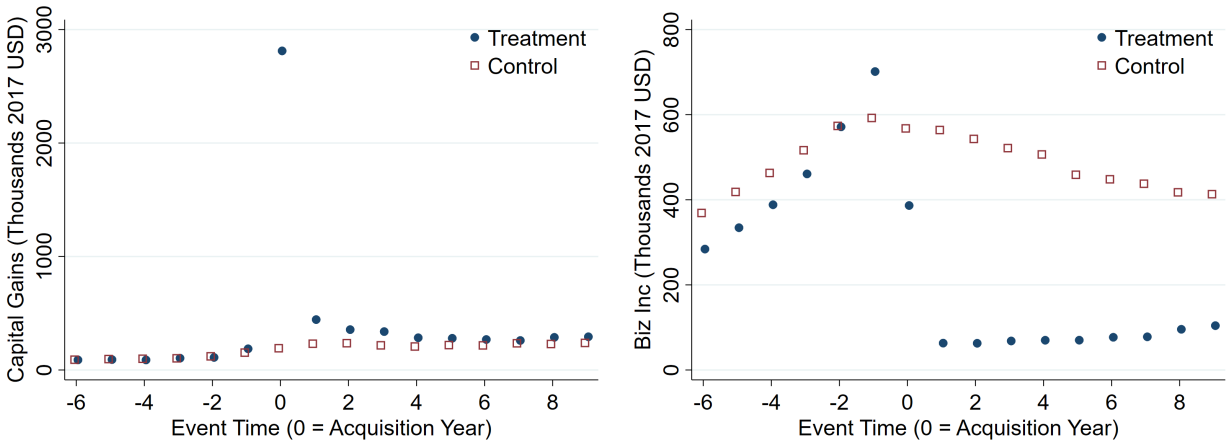
(a) Employment



(b) Wages

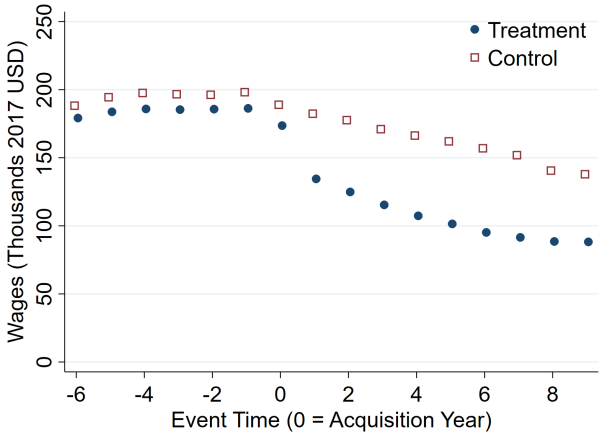
Figure B.2: Worker Treatment and Control Raw Means

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

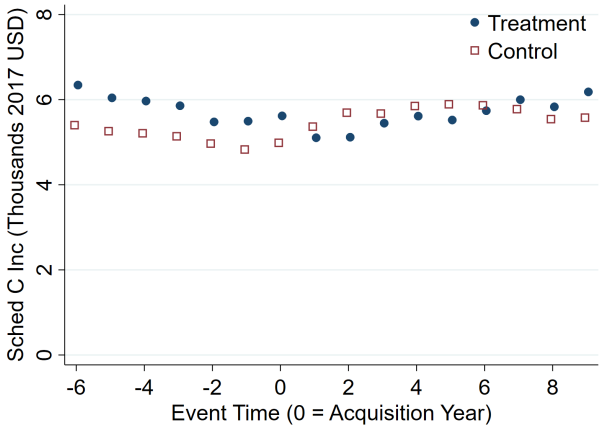


(a) Capital Gains

(b) Business Income



(c) Wages



(d) Schedule C 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 schedule C income by event time for all treatment and control owners in our owner panel.

Table B.1: Worker Panel 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
Age	42.9	42.5	43.6	43.5
Observations	1,015,380		955,640	
% Male	65.0		64.8	
% Manager	16.1		14.0	
% Add-on	48.4		.	

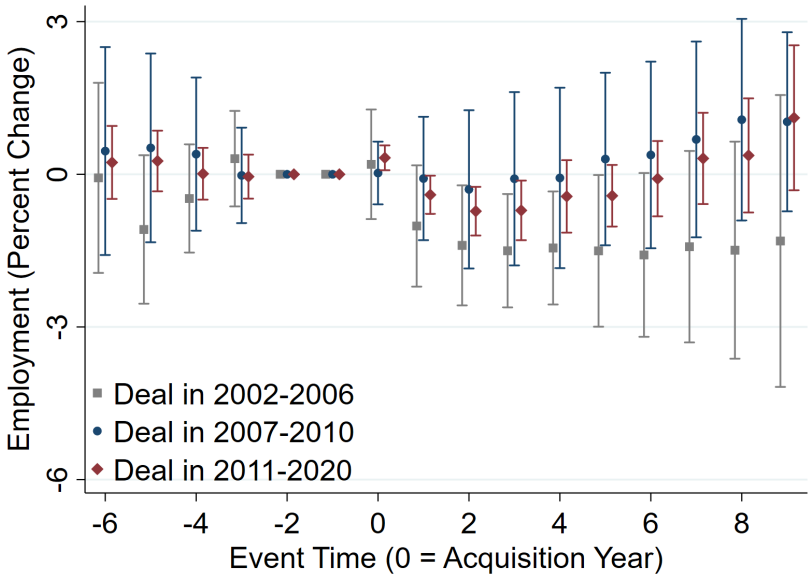
Notes: This table reports means and medians separately for the treatment and control groups in a cross section of the worker sample from the year before private equity acquisitions. Statistics are expressed in 2017 USD, except for counts, ages, and explicitly stated percentages. The bottom section of 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 Panel Balance

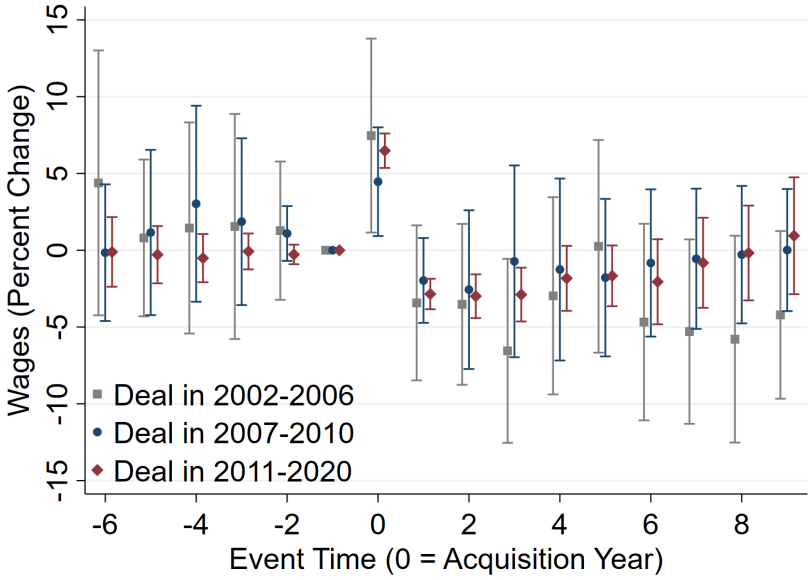
	Treatment		Control	
	Mean	Median	Mean	Median
Wages	186,252	94,327	198,157	86,860
Business Inc	701,356	209,210	592,305	151,805
LTCG	186,733	0	153,173	0
Sched C Inc	196	0	224	0
Age	54.1	54.0	54.1	53.5
Observations	23,002		24,969	
% Male	77.4		76.7	

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



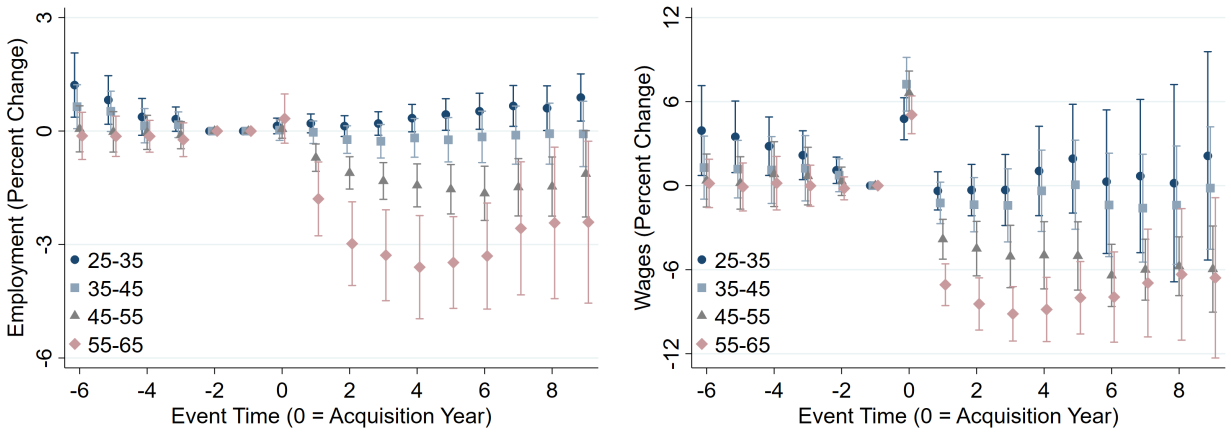
(a) Employment



(b) Wages

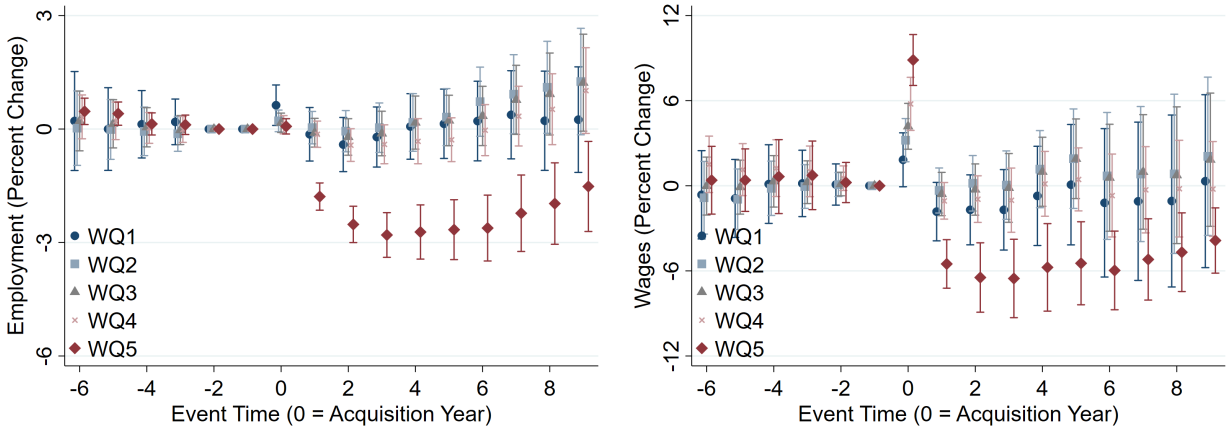
Figure C.1: Impacts of PE Acquisitions by Time Period

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 by Age

(b) Wages by Age

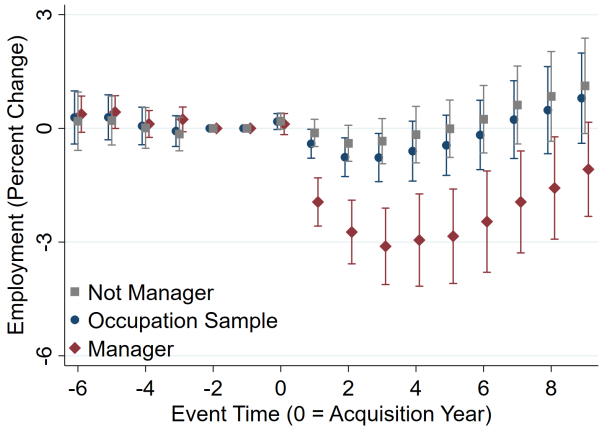


(c) Employment by Wage Quintile

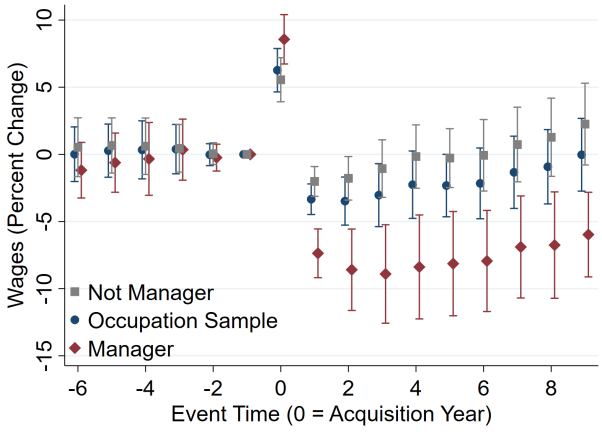
(d) Wages by Wage Quintile

Figure C.2: Impacts of PE Acquisitions by Age or Wage Quintile Including Owners

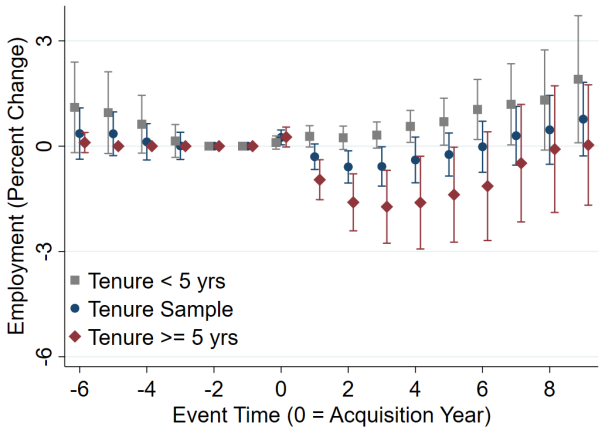
Notes: This figure displays difference-in-difference estimates of the β_e coefficients from equation (1) using a dummy variable equal to 1 if a worker is employed as the outcome in Panels (a) and (c), and wages as the outcome in Panels (b) and (d). Each series of estimates is from a separate regression performed on the specified estimation sample. Panels (a) and (b) display estimates for workers, including owners, in different age ranges in the year before the PE acquisition. Panels (c) and (d) display estimates for workers, including owners, in different within-firm wage quintiles 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.



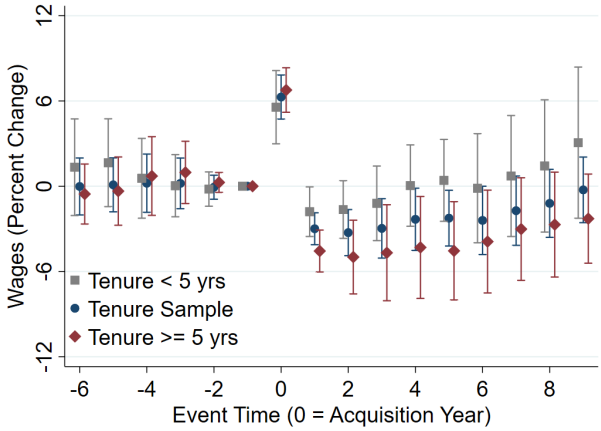
(a) Employment by Managerial Status



(b) Wages by Managerial Status



(c) Employment by Tenure



(d) Wages by Tenure

Figure C.3: Impacts of PE Acquisitions by Managerial Status or Tenure Including 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 series of estimates is from a separate regression performed on the specified estimation sample. Panels (a) and (b) display estimates for all workers for whom we observe occupation, as well as dividing workers into managerial and non-managerial occupations. Panels (c) and (d) display estimates for all workers associated with deals in 2006 and later, matching the first year of deals for which we can observe occupation, and yielding sufficient pre-period years to measure worker tenure. Panels (c) and (d) also split this sample of workers into those with < 5 years and ≥ 5 years of tenure at their firm in the year before the 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 subgroup in the year before the acquisition. 95% confidence intervals are constructed from standard errors clustered at the firm level.

(a) Manager Employment

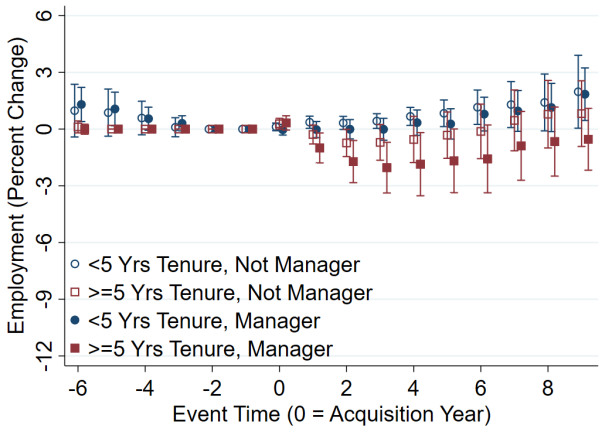
(b) Manager Wages

(c) Not Manager Employment

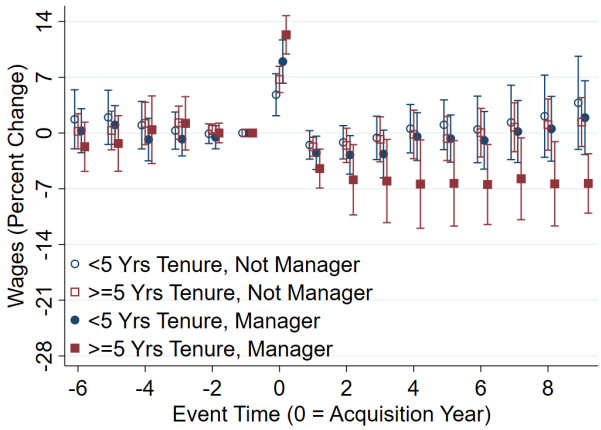
(d) Not Manager Wages

Figure C.4: Impacts of PE Acquisitions by Managerial Status, Age, and Wage Quintile Including Owners

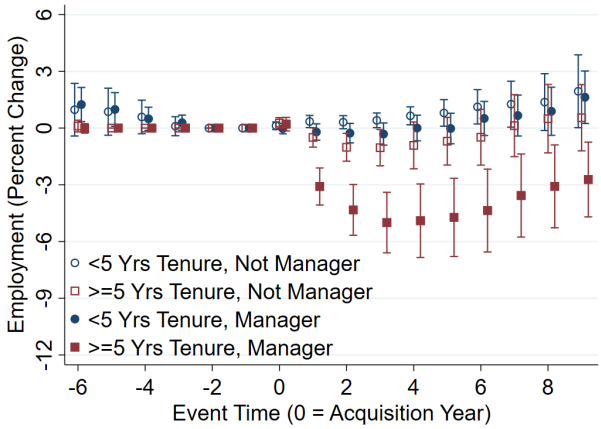
Notes: This figure displays difference-in-difference estimates of average post-reform coefficient estimates from equation (1), replacing post-match year indicators with an indicator for an observation being in any post-match year, and using a dummy variable equal to 1 if a worker is employed as the outcome in Panels (a) and (c), and wages as the outcome in Panels (b) and (d). Each estimate is from a separate regression performed on the specified estimation sample. 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.



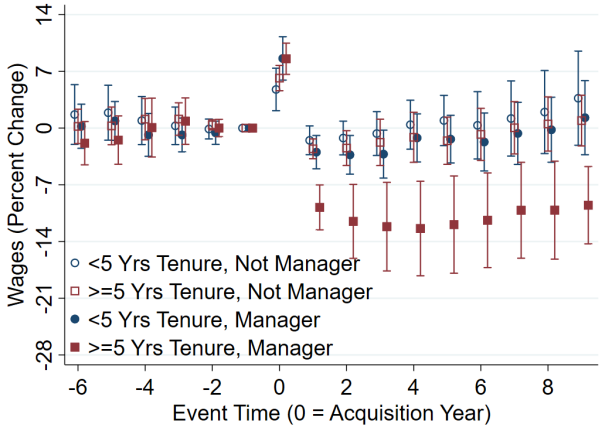
(a) Employment Without Owners



(b) Wages Without Owners



(c) Employment With Owners



(d) Wages With Owners

Figure C.5: 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 <5 and ≥ 5 years of tenure. Panels (a) and (b) show estimates from our worker panel excluding owners, while panels (c) and (d) use the entire worker panel including owners. 95% confidence intervals are constructed from standard errors clustered at the firm level.

Table C.1: Extensive Margin Impacts of PE Acquisitions on All 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	27,841,159	27,841,159	27,841,159	27,841,159
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-deal 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 All 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 C: 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: 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	−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 C: 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 after excluding owners. 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.5: 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 (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, 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-deal 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.6: Extensive Margin Impacts of PE Acquisitions on Owners With Large Stakes

	(1) Employment	(2) LTCG	(3) Sched C Inc	(4) Biz Inc	(5) No Inc	(6) Florida
Panel A: > 5% Ownership Stakes						
β_0	0.0382	0.1955	0.0233	−0.0104	0.0242	0.0063
	0.0035)	(0.0054)	0.0030)	(0.0031)	(0.0038)	(0.0011)
$\beta_{1–4}$	-0.1446	0.0933	0.0226	−0.2012	0.1967	0.0128
	0.0055)	(0.0051)	0.0036)	(0.0052)	(0.0049)	(0.0017)
$\beta_{5–9}$	-0.1911	0.0623	0.0199	−0.1892	0.2010	0.0168
	0.0074)	(0.0061)	0.0050)	(0.0070)	(0.0062)	(0.0030)
Obs	485,867	485,867	485,867	485,867	485,867	428,758
Clusters	15,602	15,602	15,602	15,602	15,602	14,978
R^2	0.66	0.60	0.57	0.42	0.46	0.86
Pre-Deal Mean	0.7306	0.6165	0.1820	0.9998	0.0515	0.0922
Panel B: > 10% Ownership Stakes						
β_0	0.0386	0.2032	0.0267	−0.0108	0.0254	0.0070
	0.0037)	(0.0058)	0.0033)	(0.0030)	(0.0037)	(0.0012)
$\beta_{1–4}$	-0.1660	0.0989	0.0264	−0.2044	0.2085	0.0141
	0.0058)	(0.0055)	0.0040)	(0.0051)	(0.0049)	(0.0019)
$\beta_{5–9}$	-0.2151	0.0708	0.0269	−0.1942	0.2130	0.0176
	0.0077)	(0.0065)	0.0055)	(0.0071)	(0.0064)	(0.0033)
Obs	402,446	402,446	402,446	402,446	402,446	358,332
Clusters	15,318	15,318	15,318	15,318	15,318	14,596
R^2	0.65	0.59	0.56	0.41	0.46	0.86
Pre-Deal Mean	0.7565	0.6260	0.1788	0.9999	0.0443	0.0955

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, received any positive wages, business, or schedule C income, or filed a 1040 as a Florida resident. 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-deal 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.7: 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-deal 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: 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 C: 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.

Table C.9: Sample Overlap for High Incidence Groups

	(1) Full Samp	(2) Owners	(3) WQ5	(4) 55-65	(5) Managers	(6) Ten≥5
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	496,399
% Owners	2	100	6	3	5	3
% WQ5	22	87	100	29	45	27
% 55-65	17	33	22	100	18	21
% Managers	15	49	31	17	100	17
% Ten≥5	54	90	66	69	60	100

Notes: This table reports the total number of workers, owners, top wage quintile workers, 55-65 workers, managers, workers with >= 5 years of tenure at their firm, and the fraction of each group that are owners, top wage quintile, 55-65, managers, and have >= 5 years of tenure. Panel A reports statistics for the entire worker sample, while Panel B reports statistics for our occupation sample.

Table C.10: Heterogeneous Impacts of PE Acquisitions on Workers With Owners

Outcome		(1) Full	(2) WQ5	(3) 55-65	(4) Manager	(5) Ten≥5
Wages	β_0	5,136.9	15,083.5	4,635.0	11,294.4	6,311.0
		(615.1)	(1,557.9)	(631.0)	(1,236.3)	(692.1)
	β_{1-4}	−2,316.7	−10,333.6	−7,668.4	−10,954.4	−4,236.9
		(654.6)	(2,077.0)	(847.8)	(1,991.2)	(964.2)
	β_{5-9}	−1,542.1	−8,740.4	−6,675.2	−9,580.9	−3,461.0
		(906.8)	(2,280.3)	(1,710.3)	(2,421.5)	(1,118.6)
Employment	β_0	0.0024	0.0007	0.0033	0.0011	0.0027
		(0.0013)	(0.0010)	(0.0033)	(0.0014)	(0.0015)
	β_{1-4}	−0.0061	−0.0245	−0.0289	−0.0267	−0.0134
		(0.0025)	(0.0026)	(0.0055)	(0.0045)	(0.0040)
	β_{5-9}	−0.0008	−0.0226	−0.0291	−0.0208	−0.0072
		(0.0041)	(0.0046)	(0.0081)	(0.0064)	(0.0064)
LTCG	β_0	8,250.4	35,408.9	16,404.5	31,948.1	14,449.9
		(444.6)	(1,785.3)	(913.9)	(2,098.5)	(1,063.2)
	β_{1-4}	991.3	5,215.5	2,461.2	5,580.0	1,992.3
		(183.0)	(669.1)	(349.9)	(638.9)	(291.5)
	β_{5-9}	581.5	3,081.9	1,134.2	3,754.7	1,281.7
		(262.4)	(752.5)	(436.3)	(677.1)	(372.8)
Obs		27,841,159	5,685,773	4,451,920	1,912,256	13,169,381
Clusters		20,075	20,075	18,692	16,501	16,950
Pre-Deal Wage Mean		83,487.4	170,092.9	91,668.1	131,821.5	93,111.1
Pre-Deal Employment Mean		1.0000	1.0000	1.0000	1.0000	1.0000
Pre-Deal LTCG Mean		483.5	3,743.6	1,545.2	3,619.0	1,646.5

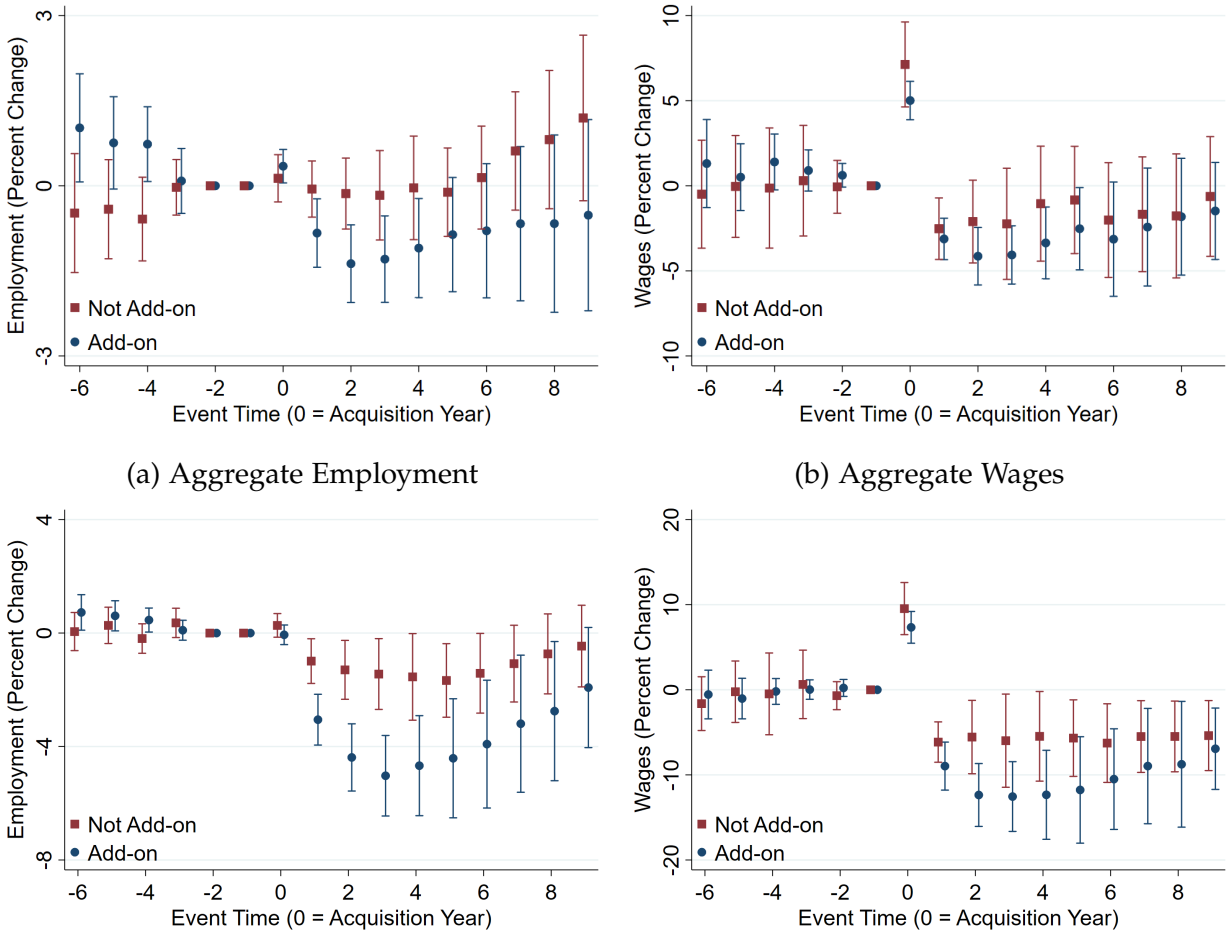
Notes: This table reports difference in difference estimates of β from equation (1), replacing post-match year indicators with indicators for an observation being in the acquisition year, the first 4 years after the acquisition, and years 5–9 after the acquisition. The estimation sample in Column 1 includes all workers in our worker panel including owners. Columns 2–5 restrict the sample to top wage quintile workers, age 55–65 workers, managers, and workers with tenure $\geq$ 5 years. Standard errors are clustered at the firm level and reported in parentheses. The pre-deal 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.11: Impacts of PE Acquisitions on Workers With and Without Low Stake Owners

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
				With Owners					Without Owners		
Outcome		Full	WQ5	55-65	Manager	Ten ≥ 5	Full	WQ5	55-65	Manager	Ten ≥ 5
Wages	β_0	5,136.9	15,083.5	4,635.0	11,294.4	6,311.0	5,425.3	17,184.1	5,460.6	13,614.3	6,911.6
		(615.1)	(1,557.9)	(631.0)	(1,236.3)	(692.1)	(625.0)	(1,661.0)	(663.4)	(1,339.8)	(726.3)
	β_{1-4}	−2,316.7	−10,333.6	−7,668.4	−10,954.4	−4,236.9	−1,316.4	−5,699.5	−5,102.8	−5,237.2	−2,281.0
		(654.6)	(2,077.0)	(847.8)	(1,991.2)	(964.2)	(650.4)	(2,116.7)	(917.3)	(1,812.4)	(889.4)
	β_{5-9}	−1,542.1	−8,740.4	−6,675.2	−9,580.9	−3,461.0	−648.7	−4,543.3	−4,445.6	−4,938.4	−1,720.1
		(906.8)	(2,280.3)	(1,710.3)	(2,421.5)	(1,118.6)	(904.9)	(2,262.1)	(1,865.4)	(2,035.3)	(969.0)
Employment	β_0	0.0024	0.0007	0.0033	0.0011	0.0027	0.0025	0.0011	0.0035	0.0016	0.0030
		(0.0013)	(0.0010)	(0.0033)	(0.0014)	(0.0015)	(0.0013)	(0.0011)	(0.0034)	(0.0015)	(0.0015)
	β_{1-4}	−0.0061	−0.0245	−0.0289	−0.0267	−0.0134	−0.0028	−0.0112	−0.0214	−0.0093	−0.0072
		(0.0025)	(0.0026)	(0.0055)	(0.0045)	(0.0040)	(0.0025)	(0.0024)	(0.0057)	(0.0040)	(0.0039)
	β_{5-9}	−0.0008	−0.0226	−0.0291	−0.0208	−0.0072	0.0027	−0.0078	−0.0204	−0.0032	−0.0006
		(0.0041)	(0.0046)	(0.0081)	(0.0064)	(0.0064)	(0.0041)	(0.0044)	(0.0081)	(0.0054)	(0.0062)
LTCG	β_0	8,250.4	35,408.9	16,404.5	31,948.1	14,449.9	3,341.7	14,136.6	6,018.7	10,687.1	5,444.5
		(444.6)	(1,785.3)	(913.9)	(2,098.5)	(1,063.2)	(255.4)	(971.9)	(480.7)	(909.3)	(531.8)
	β_{1-4}	991.3	5,215.5	2,461.2	5,580.0	1,992.3	316.2	2,182.1	854.1	1,972.7	729.7
		(183.0)	(669.1)	(349.9)	(638.9)	(291.5)	(150.6)	(490.0)	(244.5)	(433.8)	(208.7)
	β_{5-9}	581.5	3,081.9	1,134.2	3,754.7	1,281.7	318.3	1,872.8	429.2	2,176.4	742.0
		(262.4)	(752.5)	(436.3)	(677.1)	(372.8)	(263.7)	(775.3)	(410.3)	(607.1)	(375.8)
Obs		27,841,159	5,685,773	4,451,920	1,912,256	13,169,381	27,418,096	5,342,871	4,324,999	1,801,925	12,815,741
Clusters		20,075	20,075	18,692	16,501	16,950	20,070	19,758	18,446	15,842	16,873
Pre-Deal Wage Mean		83,487.4	170,092.9	91,668.1	131,821.5	93,111.1	81,165.2	163,692.4	86,833.9	124,123.9	89,058.7
Pre-Deal Employment Mean		1.0000	1.0000	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	1,646.5	457.8	1,875.0	678.3	1,618.8	754.2

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 in the first 5 columns includes all workers in our workers panel, while the estimation sample in columns 6-10 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-deal mean is the average value of the outcome variable in each column for workers at acquired firms in the year before the acquisition.

D Heterogeneity and Robustness for Add-Ons



(a) Aggregate Employment

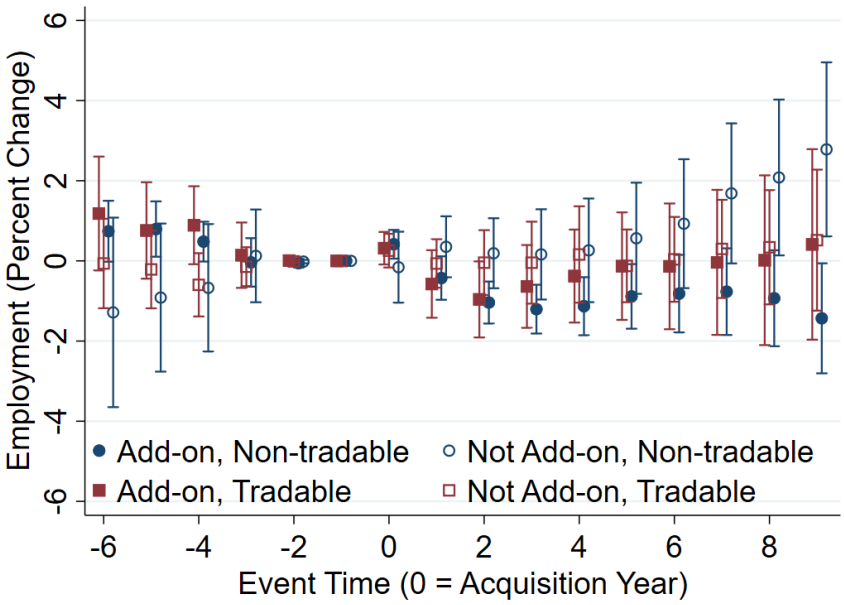
(b) Aggregate Wages

(c) Manager Employment

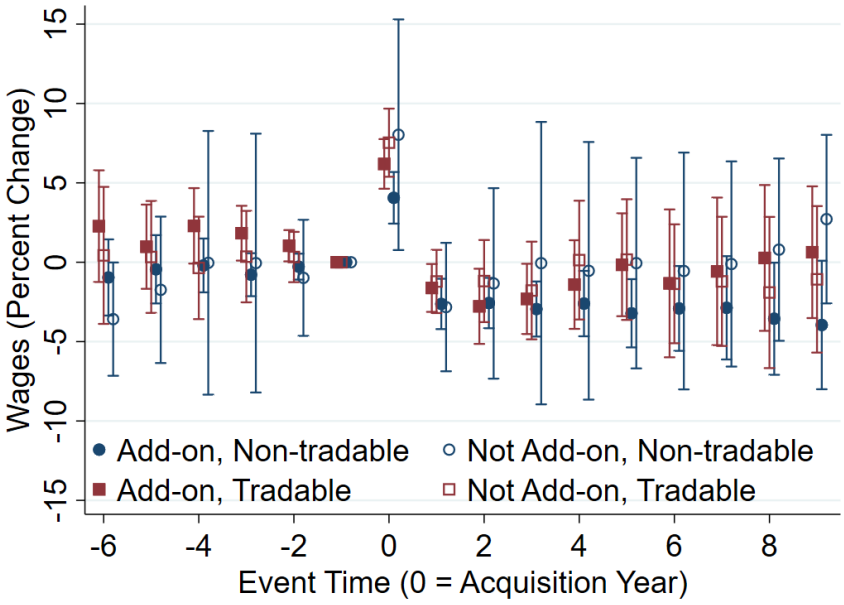
(d) Manager Wages

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.



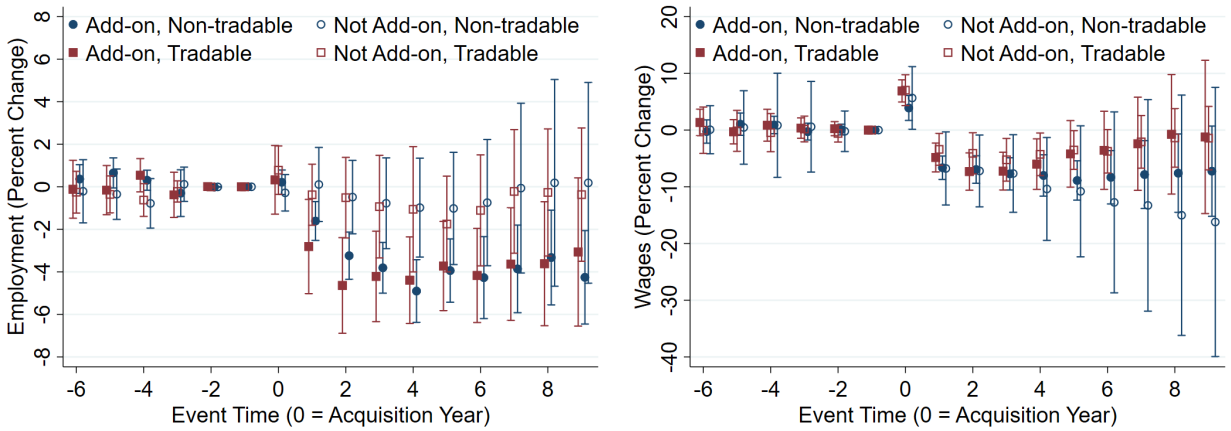
(a) Employment



(b) Wages

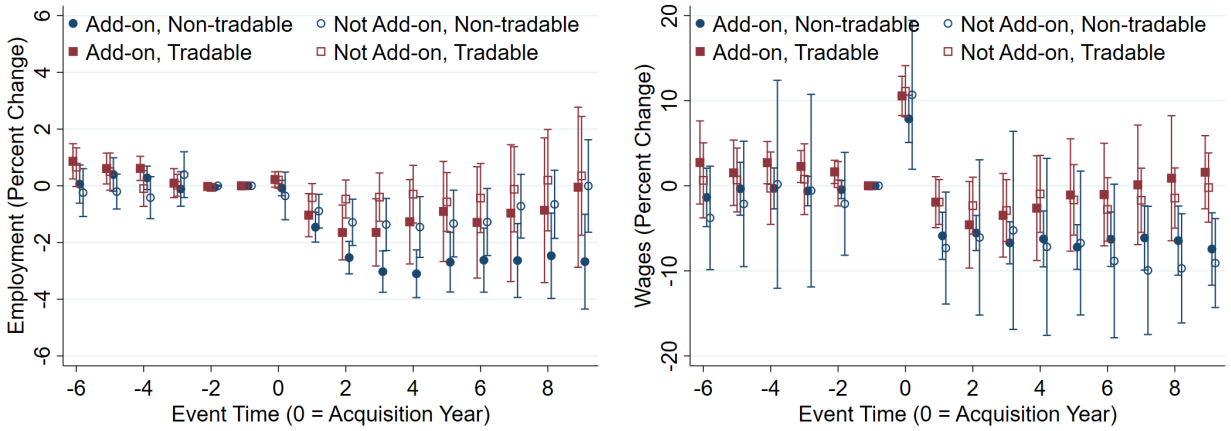
Figure D.2: Impacts of Add-on PE Acquisitions by Sector

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

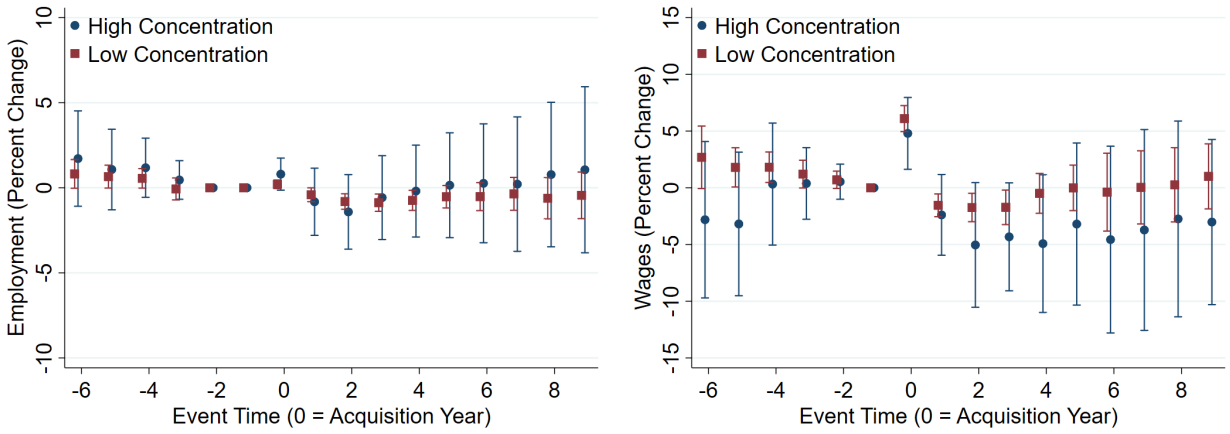


(c) WQ5 Employment

(d) WQ5 Wages

Figure D.3: 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.4: 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: Net Present Value Impacts of Add-on PE Acquisitions on Workers

	(1) Full Sample	(2) WQ5	(3) 55-65	(4) Manager	(5) Rank & File
			Panel A: 5% Discount Rate		
Wages	−4,141.8	−17,187.3	−27,602.2	−29,085.4	−3,175.3
	(5,828.0)	(20,692.2)	(13,542.5)	(20,384.1)	(6,006.0)
	[−2.6, 1.2]	[−5.0, 2.0]	[−11.3, −0.2]	[−7.5, 1.2]	[−2.9, 1.6]
LTCG	5,928.6	30,118.8	8,711.9	30,077.7	410.5
	(1,871.8)	(7,283.4)	(3,786.1)	(4,566.4)	(925.8)
	[14.2, 60.3]	[28.6, 80.0]	[5.5, 68.9]	[45.4, 83.9]	[−23.4, 37.1]
			Panel B: 7% Discount Rate		
Wages	−3,568.7	−15,015.9	−25,196.8	−25,891.3	−2,910.6
	(5,272.7)	(18,907.4)	(12,079.3)	(18,562.3)	(5,405.1)
	[−2.5, 1.2]	[−4.9, 2.1]	[−10.8, −0.3]	[−7.3, 1.2]	[−2.8, 1.6]
LTCG	5,862.8	29,412.0	8,782.0	28,894.6	447.6
	(1,709.4)	(6,672.8)	(3,480.3)	(4,133.1)	(853.2)
	[17.1, 62.9]	[32.0, 83.2]	[9.1, 72.4]	[48.4, 86.2]	[−22.2, 38.5]
			Panel C: 9% Discount Rate		
Wages	−3,055.4	−13,069.1	−23,058.9	−23,059.0	−2,669.9
	(4,792.6)	(17,343.9)	(10,823.5)	(16,971.1)	(4,887.1)
	[−2.4, 1.2]	[−4.7, 2.1]	[−10.4, −0.4]	[−7.0, 1.3]	[−2.7, 1.5]
LTCG	5,802.7	28,783.9	8,834.7	27,857.5	480.1
	(1,570.1)	(6,148.7)	(3,216.1)	(3,765.5)	(791.0)
	[20.1, 65.6]	[35.5, 86.5]	[12.7, 76.0]	[51.5, 88.6]	[−21.1, 40.0]

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 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.2: Net Present Value Impacts of Non-Add-on PE Acquisitions on Workers

	(1) Full Sample	(2) WQ5	(3) 55-65	(4) Manager	(5) Rank & File
			Panel A: 5% Discount Rate		
Wages	811.3	−20,434.3	−31,357.5	−16,632.9	8,150.5
	(8,061.6)	(21,985.5)	(13,692.4)	(16,510.9)	(9,796.0)
	[−2.3, 2.6]	[−5.3, 1.9]	[−12.4, −1.0]	[−5.0, 1.6]	[−2.1, 5.2]
LTCG	5,279.3	26,747.8	11,985.5	22,081.9	250.6
	(2,050.5)	(5,761.1)	(2,934.8)	(5,774.8)	(1,414.4)
	[7.6, 56.0]	[28.3, 69.6]	[29.9, 85.1]	[22.2, 69.0]	[−34.1, 40.9]
			Panel B: 7% Discount Rate		
Wages	1,185.1	−17,091.5	−28,077.4	−13,904.9	7,462.4
	(7,323.1)	(20,138.1)	(12,379.7)	(15,130.3)	(8,835.4)
	[−2.2, 2.6]	[−5.1, 2.0]	[−11.9, −0.9]	[−4.8, 1.7]	[−2.0, 5.1]
LTCG	5,000.6	25,316.6	11,375.1	20,817.1	184.7
	(1,857.9)	(5,267.6)	(2,671.9)	(5,333.0)	(1,268.1)
	[8.9, 56.8]	[29.9, 71.0]	[32.0, 86.7]	[23.3, 70.3]	[−34.1, 39.6]
			Panel C: 9% Discount Rate		
Wages	1,517.4	−14,159.2	−25,206.3	−11,521.7	6,876.9
	(6,680.6)	(18,515.9)	(11,249.1)	(13,919.4)	(8,005.0)
	[−2.1, 2.6]	[−4.8, 2.1]	[−11.3, −0.8]	[−4.6, 1.9]	[−1.9, 5.0]
LTCG	4,759.3	24,065.2	10,840.7	19,709.1	132.2
	(1,692.4)	(4,844.8)	(2,446.6)	(4,951.7)	(1,142.4)
	[10.3, 57.6]	[31.5, 72.5]	[34.1, 88.3]	[24.4, 71.7]	[−34.1, 38.4]

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.

Table D.3: Impacts of Add-on and Non-Add-on PE Acquisitions on Workers With Owners

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
				Add-on					Not Add-on		
Outcome		Full	WQ5	55-65	Manager	Ten ≥ 5	Full	WQ5	55-65	Manager	Ten ≥ 5
Wages	β_0	4,001.5	12,301.0	3,974.8	9,309.6	5,606.0	6,198.0	17,716.5	5,239.0	12,978.8	7,002.7
		(459.6)	(1,235.7)	(567.6)	(1,208.2)	(698.8)	(1,106.9)	(2,784.3)	(1,084.4)	(2,124.9)	(1,177.7)
	β_{1-4}	−2,939.9	−12,121.2	−8,671.6	−14,612.9	−5,696.5	−1,735.9	−8,654.8	−6,766.8	−7,887.3	−2,790.0
		(630.2)	(2,474.5)	(954.9)	(2,439.7)	(1,054.9)	(1,116.3)	(3,286.3)	(1,353.2)	(2,909.6)	(1,554.6)
	β_{5-9}	−1,892.2	−8,374.9	−6,123.6	−12,298.3	−3,969.9	−1,214.1	−8,996.6	−7,351.1	−7,744.3	−2,924.0
		(1,138.4)	(3,381.7)	(2,493.7)	(3,943.4)	(1,590.5)	(1,381.1)	(3,170.6)	(2,379.7)	(2,881.1)	(1,606.6)
Employment	β_0	0.0035	0.0012	0.0025	−0.0006	0.0017	0.0013	0.0003	0.0039	0.0027	0.0037
		(0.0015)	(0.0010)	(0.0052)	(0.0018)	(0.0015)	(0.0021)	(0.0018)	(0.0041)	(0.0022)	(0.0025)
	β_{1-4}	−0.0115	−0.0345	−0.0468	−0.0427	−0.0228	−0.0010	−0.0151	−0.0118	−0.0131	−0.0042
		(0.0036)	(0.0041)	(0.0064)	(0.0067)	(0.0054)	(0.0035)	(0.0032)	(0.0077)	(0.0059)	(0.0059)
	β_{5-9}	−0.0073	−0.0329	−0.0489	−0.0341	−0.0162	0.0046	−0.0136	−0.0120	−0.0113	0.0011
		(0.0064)	(0.0082)	(0.0086)	(0.0117)	(0.0102)	(0.0051)	(0.0051)	(0.0117)	(0.0068)	(0.0081)
LTCG	β_0	10,889.8	46,374.7	20,619.3	43,646.8	18,924.5	5,777.5	24,997.0	11,881.5	21,840.6	9,998.0
		(741.1)	(2,948.2)	(1,598.1)	(2,803.4)	(1,746.2)	(504.0)	(2,041.6)	(976.1)	(2,522.0)	(1,195.7)
	β_{1-4}	1,346.8	6,503.4	2,924.9	7,763.7	2,513.9	658.2	3,987.9	1,905.1	3,609.1	1,469.3
		(254.9)	(1,051.5)	(572.4)	(750.1)	(434.7)	(257.8)	(814.4)	(400.5)	(915.2)	(368.6)
	β_{5-9}	223.9	1,791.1	214.8	3,489.5	829.8	849.7	4,029.0	1,924.4	3,503.2	1,640.8
		(317.3)	(1,072.3)	(592.9)	(862.8)	(456.4)	(394.2)	(1,031.2)	(577.4)	(1,060.9)	(569.0)
Obs		13,216,651	2,718,901	2,182,070	882,765	6,448,032	14,624,508	2,966,872	2,269,850	1,019,217	6,721,349
Clusters		13,160	13,160	12,152	10,664	11,378	6,915	6,915	6,540	5,690	5,572
Pre-Deal Wage Mean		79,839.8	161,226.8	89,298.2	126,889.6	91,018.9	86,905.1	178,512.3	94,193.7	136,044.4	95,192.3
Pre-Deal Employment Mean		1.0000	1.0000	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	1,892.2	830.5	3,557.9	1,395.3	3,177.4	1,402.1

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 5 columns includes all workers in our workers panel that are associated with add-on acquisitions, while the estimation sample in columns 6-10 includes all workers that are associated with acquisitions that are not add-ons. Columns 2–5 and 7–10 restrict these samples to top wage quintile workers, age 55–65 workers, managers, and workers with tenure ≥ 5 years. Standard errors are clustered at the firm level and reported in parentheses. The pre-deal 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.4: Impacts of Changes in Control on All Workers

	(1)	(2)	(3)	(4)
	LBO		New Majority Owner	
	Employment	Wages	Employment	Wages
β_0	0.0024	5136.9	−0.0019	−2135.7
	(0.0013)	(615.1)	(0.0013)	(919.1)
$\beta_{1\text{-}4}$	−0.0061	−2316.7	−0.0055	−1884.8
	(0.0025)	(654.6)	(0.0042)	(1092.5)
$\beta_{5\text{-}9}$	−0.0008	−1542.1	−0.0047	−729.6
	(0.0041)	(906.8)	(0.0092)	(2960.3)
Obs	27,841,159	27,841,159	23,084,362	23,084,362
Clusters	20,075	20,075	35,938	35,938
Pre-Deal Mean	1.0000	83,487.4	1.0000	75,771.7

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. In columns 1 and 2, the estimation sample is our entire worker panel data set, replicating Panel A, columns 1 and 2 in Table 4. In columns 3 and 4, the estimation sample is an alternative worker panel data set built to track all workers at passthrough businesses with a new majority owner from an arms length transaction and all workers at those passthrough businesses matched control firms, following the same matching procedure described in section 3. This alternative sample excludes i) any firms ever acquired in an LBO, ii) any transactions where the new majority owner is an estate or trust, and iii) any transactions where the new and old owner last names share the same first four characters. 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.