Firm Pay Policy and the Personal Labor Market Experiences of Business Owners*

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Abstract

This paper studies how the personal economics experiences of owner-managers shape firm pay policy. Leveraging detailed survey data on the management roles of U.S. business owners, I show that firms operated by dominant owner-managers with greater past local unemployment rate exposure (URE) offer higher pay on average. Difference-in-differences estimates centered on firm ownership transitions show that high URE managers increase average worker earnings by 3.4%, while firm employment and revenue is unchanged. These results are not driven by contemporaneous labor market conditions, differences in the financial resources of new owner-managers, or age cohort or location-specific trends. I conduct further firm and worker-level analyses to explore how labor market experiences inform personnel strategies at the firm. I argue that my results are consistent with URE increasing the salience of worker turnover costs to the firm, leading owner-managers to better internalize a pay-turnover trade-off. Consequently, high URE managers retain a greater share of incumbent employees and offer greater relative pay premia to new hires.
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1 Introduction

Average worker earnings varies substantially across firms, with a wide-ranging literature attributing these differences to various factors, including worker sorting across firms (Song, Price, Guvenen, Bloom and Wachter, 2019), productivity dispersion (Barth, Bryson, Davis and Freeman, 2016), and the presence of non-wage amenities at the workplace (Sorkin, 2018). In light of growing evidence on the prevalence of firm labor market power (Card, 2022), other studies have framed these patterns in terms of the ability of firms to capture rents via the employment relationship (Card, Cardoso, Heining and Kline, 2018; Lamadon, Mogstad and Setzler, 2022).

Though these existing theories are useful for explaining many observed labor market features, direct evidence on the nature of firm-level pay conduct has been more scarce. While a lack of competition from other employers may lead firms to set lower wages on average, individual firms may still vary in how readily they reduce wages below what would be offered in a perfectly competitive setting. For example, firms may pay higher wages to retain certain workers (Bassier, Dube and Naidu, 2021), to encourage effort in the workplace (Ruffini, 2022), or due to preferences for certain types of workers (Biasi and Sarsons, 2021). As a result, evidence on the drivers of firm wage-setting conduct is important for understanding both the sources of labor market power and how such conduct relates to dispersion in firm-level pay.

This paper studies firm pay conduct by focusing directly on *who* sets pay at the firm level. The role of individual managers in determining firm pay has been recognized since at least Slichter (1950). Here, I present evidence on a novel driver of managerial discretion over pay policy: the past labor market experiences of business owners. My approach uses detailed to survey data to identify U.S. business owners that are actively involved in the operations of their firm, finding that U.S. firms operated by owner-managers who were previously more exposed to elevated local unemployment rates offer higher pay to workers (Malmendier and Shen, 2019). These differences cannot be explained by differential firm employment or revenue, or by direct effects on the financial resources of potential owners.

Establishing a causal link between owners' past labor market experiences and the pay policy of firms is challenging for a number of reasons. First, data linking the work histories of business owners to their firms is not readily available in many existing data sources. While worker-entrepreneur transitions can be inferred from matched employer-employee data, prior approaches do not identify which owners are actively involved in management and firm financial decision-making. These approaches also rely on administrative tax records to measure entrepreneurship, conflating ownership changes or reincorporation with new firm creation (Chow, Fort, Goetz, Goldschlag, Lawrence, Perlman, Stinson and White, 2021).

Second, differences in firm pay policy may reflected a variety of strategies that firms employ to maintain a productive workforce. For example, while firm rent-sharing may reflect normative beliefs about what constitutes "fair" compensation, it could also reflect efforts to retain workers

that are particularly essential to the operation of the firm. Similarly, worker pay across certain firms may be similar on average but vary dynamically depending on the implicit contracts under which worker compensation is determined (Beaudry and DiNardo, 1991). This feature of pay policy necessitates credible empirical designs paired with both firm and worker-level data to identify the mechanisms underlying firm pay-setting heterogeneity.

Finally, it is unclear how prior economic shocks shape the behavior of owner-managers based on existing evidence on experience effects. In closely related work, He and le Maire (2022) provide evidence that differences in manager-specific wage premia can be explained by managers' prior exposure to inequality.[1] Malmendier (2021) emphasizes that empirically documented experience effects are typically *domain specific* rather than generalized shocks to beliefs or preferences. In other words, owner-managers may respond differently than households to salient labor market shocks if they are informative of the particular costs that firms face in the market to retain and attract workers. For this reason, the application of one experience measure, such as local unemployment exposure, to a different domain, such as firm management, requires a careful consideration of the ways that such exposure may influence managerial conduct.

I address these challenges by creating a novel data linkage that uses survey data to identify owner-managers in matched employer-employee data from the US Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) program. This linkage identifies the work histories of individuals who report involvement in the day-to-day management of their firms. In my baseline sample, these firms are relatively small, averaging thirty-four employees, and disproportionately comprise "closely-held" S-corporations where owners are likely to be particularly influential in the operation of their firms (Smith, Yagan, Zidar and Zwick, 2019). This feature is particularly appropriate for studying how managerial discretion affects pay policy, since larger firms may be more likely to institute standardized pay structures (Hazell, Patterson, Sarsons and Taska, 2022). I generate measures of owner-managers' cumulative, lifetime unemployment rate exposure (URE) up to the year in which they acquire their firm (Malmendier and Shen, 2019). I classify owners by whether their calculated URE is above the median value in my sample, which yields a "treatment" group comprising owners whose average URE was 6.89% when they acquired their firm relative to a "control" group whose average URE was 5.27%. I then relate these measures to both firm- and worker-level outcomes using multiple research designs that clarify the economic forces that mediate the relationship between owner experiences and labor market outcomes.

My baseline empirical design estimates worker earnings, employment, and revenue responses to changes in firm ownership. To abstract from mechanical effects associated with ownership transfers, my difference-in-differences design compares firms acquired by high URE owners to

[1]These results are consistent with past experience shaping managers' fairness views, following work on the relationship between inequality and belief formation (Hvidberg, Kreiner and Stantcheva, 2023). However, prior studies have found somewhat mixed evidence on the effects of recession exposure on normative beliefs around fairness or preferences for redistribution (Cotofan, Dur and Meier, 2021; Hansen and Stutzer, 2021).

firms acquired by low URE managers. In my preferred specification, which controls for contemporaneous unemployment rates, firms acquired by high URE owner-managers increase mean earnings by 4.7% five years after the ownership change, with an average earnings effect of 3.4% over the same period. These results are not accompanied by significant effects on employment or revenue. Event study estimation reveals stable trends in each of these outcomes prior to firm acquisition, supporting the common trends assumption that treatment effects are driven by the underlying characteristics of the firms acquired by owner-managers with different unemployment rate exposure. Alternative specifications show that these results are not confounded by differences in acquiring owners' financial resources, contemporaneous local economic conditions, or differences in how acquired forms are incorporated. I also conduct placebo tests showing that neither age cohort nor recent local economic conditions alone can explain my core results.

In worker-level analysis, I find that incumbent workers experience earnings increases that mirror the firm-level results, with a statistically significant 3.2% increase in earnings for workers that come under the ownership of high URE owner-managers five years after the acquisition event. I also find that workers hired within the three year period after ownership changes also receive large, persistent earnings increases relative to their prior job. Together, these results suggest that the firm-level responses estimated in the previous analysis reflect changes in pay policy rather than a compositional shift toward different kinds of workers, and that these gains accrue to both incumbent and new hires alike.

To explore the potential mechanisms through which manager URE influences firm pay policy, I conduct several heterogeneity analyses across firm and labor market characteristics. While my results are consistent with high URE owner-managers placing greater value on fairness in the workplace, I focus on several alternative hypotheses that may reflect economic rationales for offering higher pay. First, my results could reflect differences in the degree to which owner-managers provide implicit insurance contracts against local shocks via employment contracts (Beaudry and DiNardo, 1991). If prior URE shifts how owner-managers value such insurance provision, my results on firm earnings could be instead driven by underlying heterogeneity in local labor market conditions following acquisitions. To test this hypothesis, I estimate heterogeneous effects across the both the average local unemployment rate in the post-acquisition period, and changes in the local unemployment rate over time. In both cases, I find little evidence that contemporaneous unemployment rates drive estimates of pay differentials at high URE firms.

I also test the degree to which owner-manager URE influences the degree of rent-sharing at the firm while controlling for time-varying commuting zone level shocks (Card, Devicienti and Maida, 2014). These regressions estimate worker-level earnings responses to exogenous shocks to firm revenue-per-worker using an instrumental variable (IV) design. I estimate firm passthrough elasticities of 0.064 and 0.219 for workers employed at firms operated by low and high URE owners, respectively, over the 1997-2007 period. This rent-sharing behavior translate to higher

average earnings for workers employed by high URE owner-managers, as demonstrated in event study regressions centered on job-to-job transitions. While greater rent-sharing could reflect a variety of "primitive" motives, these results help link my estimates to those in related work (e.g. Acemoglu, He and le Maire (2022)).

Lastly, prior unemployment rate exposure could alter owner-managers' preferences for worker turnover at the firm. A recent literature on the spatial distribution of unemployment shows elevated local unemployment is explained primarily by high job separation rates rather than low job finding rates (Bilal, 2023*a*). At the same time, recent evidence on the sizable costs of worker turnover to the firm (Bertheau, Cahuc, Jäger and Vejlin, 2022; Giupponi, Landais and Lapeyre, 2022) suggests there is scope for managers to differ in their evaluation of the costs of job separations to the firm. Given the well-founded empirical relationship between job turnover and firm pay, differences in managers' preferences toward turnover could result in differences in firm pay (Burdett and Mortensen, 1998).

I test this possibility by estimating heterogeneous effects according to firm-level turnover prior to the acquisition events in my event study design. The primary result from this analysis is that the estimated effects on firm-level earnings are almost entirely driven by differential pay across firms with relatively high worker turnover in the years prior to firm ownership transfer. This is consistent with high URE managers being particularly sensitive to salient measures of firm-level turnover likelihood and raising wages to better retain workers. In line with this interpretation, high URE firms retain a 2.3 percentage point higher share of incumbent workers following the acquisition event. Additionally, I find that the earnings effects among incumbents are strongest for older and college-educated workers. Older workers are also more likely to remain at the firm, suggesting high URE owner-managers may raise earnings to retain key personnel and further bolstering the interpretation of my results as reflecting a shift in manager preferences toward better worker retention.

This paper provides new evidence on the role of managers as a driver of firm-level pay heterogeneity. Prior research has shown that characteristics and practices of managers are important for explaining different dimensions of firm heterogeneity (Bloom and Van Reenen, 2007). I show that the personal labor market experiences of owners influences firm pay policy, and I argue that these differences can be explained by past labor market experiences shifting managers' views of the economic costs associated with firm hiring and pay policy. My work is closely related to that of He and le Maire (2022), who use Danish matched employer-employee data to incorporate managers into a two-way fixed effect wage decomposition (Abowd, Kramarz and Margolis, 1999). They find that managers explain 34% of wage inequality between firms, and that these patterns are consistent with prior work and personal experience shaping managers' fairness views. In a follow-up paper, Acemoglu, He and le Maire (2022) find that publicly-traded companies under the leadership of CEOs with MBAs offer lower earnings. Though similar in its focus on worker

outcomes, my paper differs to these studies in that I study smaller, closely held firms that have received relatively less attention in the literature on firm pay-setting. As in the latter paper, I find that differences in rent-sharing can partly explain how managers influence firm pay premia.

This paper also relates to a recent literature exploring why workers transition into entrepreneurship (Babina, 2020; Wallskog, 2022). My primary contribution to this literature is methodological, as I demonstrate how survey data on owners can be used to identify entrepreneurs and the role they perform at their new firm. I also show how survey data can be used to identify firm ownership changes, a non-negligible channel for entrepreneurial entry that previous methods could conflate with true new firm entry.

Finally, by focusing on relatively smaller firms, my paper links the literature on firm pay policy to a distinct literature on the role of small firms in labor markets. New firms play an outsized role in creating new jobs (Haltiwanger, Hyatt, Kahn, Erica and McEntarfer, 2018). Likely due to limit data availability, there is relatively little evidence on the pay-setting behavior of small firms (Babina, Ma, Moser, Ouimet and Zarutskie, 2019). I provide new estimates of firm labor supply and rent-sharing elasticities that can be used to inform fiscal and labor market policies that are likely to affect worker outcomes at small firms.

2 Data

I link detailed survey responses from the US Census Bureau's Survey of Business Owners (SBO) to administrative employer-employee records from the US Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) program. This section describes these source data in detail and outlines the matching procedure implemented to construct the samples used in the empirical analyses.

The Survey of Business Owners (SBO) is a large randomly sampled survey of US business owners that was conducted every five years over the 1972 to 2012 period.[2] The survey selects businesses from a list of all firms reporting receipts of at least $1,000 during the survey year, excluding select industries.[3] These data include economic and demographic characteristics of up to four business owners linked to a firm Employment Identification Number (EIN). Importantly for my analysis, the survey reports the role that each owner performs at their firm and the timing and means by which they acquired ownership of the firm. The survey also reports a number of other outcomes, such as information on the sources of startup and expansion capital used for the

[2]Due to declining response rates, the U.S. Census discontinued the SBO following the 2012 survey. In its place, the Census conducted the Annual Survey of Entrepreneurs (ASE) from 2014 to 2016. This survey in turn evolved into the Annual Business Survey (ABS), first conducted in 2018.

[3]The excluded industries are: Crop and Animal Production (NAICS 111, 112), Scheduled Passenger Air Transportation (NAICS 481111), Rail Transportation (NAICS 482), Postal Service (NAICS 491), Funds, Trusts, and Other Financial Vehicles (NAICS 525), Religious, Grantmaking, Civic, Professional, and Similar Organizations (NAICS 813), Private Households (NAICS 814), and Public Administration (NAICS 92).

firm and whether the firm provides certain employee benefits.

The Longitudinal Employer-Household Dynamics (LEHD) administrative files are a matched employer-employee dataset that provides quarterly earnings and employment information at the worker-job level. These data are derived from state unemployment insurance (UI) system wage records and the ES-202 program, jointly administered with the Bureau of Labor Statistics. Earnings records encompass various forms of compensation, such as bonuses, stock options, profit distributions, and, in some states, gratuities (Acemoglu, He and le Maire, 2022). This project uses data from twenty-two states and the District of Columbia.[4] For these states, my data covers over 90% of private sector workers over the 1990-2015 period, though time coverage varies across states. The LEHD also provides the age, sex, race, ethnicity, education level, and country of birth of workers. Firms in the the LEHD are defined at the state level using the UI-based State Employer Identification Number (SEIN). This definition typically comprises all establishments operating under a single firm in a given state. The LEHD also provided a federal Employer Identification Number (EIN) which permits linkages across states and to other Census data products.

I supplement these core data sources with firm-level revenue data from the US Census Bureau's Longitudinal Business Database (LBD), which is available over the 1997 to 2015 period. The US Census Bureau's Business Register (BR) maintains the EIN identifiers that link firms across these three data sources.

2.1 Identifying Firm Acquisitions

I link survey responses from the 2002-2012 waves of the SBO to administrative records using EIN firm identifiers derived from tax records and available across Census datasets. Throughout the paper, I refer to "firm acquisition" as the first year in which a firm EIN appears in my data. In practice, new EINs can appear for a variety of reasons beyond the birth of a new firm, most notably when a firm undergoes an ownership change. As a result, EIN changes alone may be misleading when attempting to identify entrepreneurial activity. To address this shortcoming, I verify the timing and nature of acquisition using the SBO, which reports the year in which each owner acquired the firm and whether the specified owner founded, purchased, inherited, or acquired the firm as a gift. For simplicity, I limit my analyses to firms that were either founded or purchased, which comprise over 90% of firms in the public-use 2007 SBO microdata.

While the EIN is a reliable identifier for firms that continue to be owned and operated by their original founder, changing EINs poses a significant challenge to tracking firm outcomes across ownership changes.[5] I manually create consistent firm identifiers for SBO firms that undergo

[4]The states used in this analysis are: Arizona, Arkansas, California, Colorado, Delaware, Florida, Illinois, Indiana, Iowa, Kansas, Maine, Maryland, Montana, New Mexico, Oklahoma, Oregon, Pennsylvania, South Carolina, Tennessee, Washington, West Virginia, and the District of Columbia.

[5]Constructing ownership-invariant establishment identifiers is a primary goal of the US Census Bureau's

ownership changes using a combination of Business Register data on EIN births and deaths and LEHD data on worker flows across firms. I first limit attention to purchased SBO firms where the reported acquisition year matches the birth of the firm EIN. Next, I match these EINs to all EINs that disappeared in the same or previous year and operated in the same state. I then examine the SEIN to which these EINs are attached in the LEHD. Because the LEHD reports employment and payroll at the quarterly frequency, I further refine these matches by limiting attention to SEIN-to-SEIN matches where apparent the apparent birth-death occurs in the same or consecutive quarters. Finally, I examine whether workers attached to the candidate successor SEIN in the LEHD eventually appear at the SBO firm SEIN. Specifically, I define a valid EIN-to-EIN match as those where more than 50% of the workers attached to the successor SEIN at its death appear at the SBO firm within one quarter.[6]

I make additional sampling restrictions for both purchased and founded SBO firms to ensure that my analyses focus on owners for which I can accurately measure prior labor market experience and that are likely to have a significant role in the management of firms that maintain a reasonable pool of employees. First, I discard all firms that never employee more than five workers (including owners receiving labor compensation) in a year. Second, I drop firms for which I do not observe at least five years of owner employment history prior to firm acquisition. Third, I only consider firms where the primary owner reports their role at the firm as either "managing day-to-day operations" or "financial control with the authority to sign loans, leases, and contracts." This ensures that the owners I identify can be reasonably characterized as firm managers. This approach naturally fits with my sampling procedure, which implicitly selects owners that report nonzero labor earnings at their firm and thus are likely to be involved in its day-to-day functioning. Fourth, while I impose no explicit maximum firm size, I do not consider EINs that operate in more than one state, as identified using the comprehensive firm and establishment records in the LBD. This restriction limits potential noise introduced by the inclusion of firms with more complex management structures in which the owners are less likely to exercise direct control over pay policy. This restriction also reduces the possibility of spurious demographic matches in the LEHD if an owner receives labor compensation in a state outside my LEHD sample. Finally, I do not consider firms that undergo ownership changes driven by M&A activity by existing firms, as defined by enterprise-level firm identifiers available in the LBD, over the sample period.

LBD program. The LBD creates annual, ownership-invariant establishment identifiers through a combination of the comprehensive Economic Census (EC), conducted every five years, and the annual Report of Organization. However, because the latter source only surveys large firms, the LBD is unable to provide consistent identifiers across time for many smaller firms like those reported in SBO (Chow, Fort, Goetz, Goldschlag, Lawrence, Perlman, Stinson and White, 2021).

[6]This Census project did not gain access to the LEHD Successor-Predecessor File (SPF), which conducts a similar procedure to link SEIN births and deaths.

2.2 Linking Owner-Manager Labor Market Histories

Using the firm matches identified using the criteria above, I link owners from the SBO to their LEHD worker histories using the demographic information available in both data sources. Because assigning owners to spurious LEHD worker identifiers could significantly bias my estimates, I adopt a fairly conservative approach to flagging valid owner-worker matches. Following recent literature tracking entrepreneurs in matched employer-employee data, I first restriction attention to workers at match firms that are among the top three earners at the time of acquisition (either via purchase or founding).[7] I then flag all owner-worker pairs with the same age, sex, race, ethnicity, and place of birth. I discard all other owner-worker pairs, as well as those where the LEHD reports an unimputed education level that does not match the education level reported in the SBO. I further eliminate owner-worker pairs where the worker did not report nonzero earnings at the SBO firm for each year between the acquisition year and the survey year in which the SBO owner match was flagged. For firms acquired via a purchase, I also exclude owner-worker matches where the worker appears at the firm predecessor EIN identified in the previous section. While it is likely that some firms are acquired by individuals that were previously employed by the firm, such matches are otherwise difficult to parse from spurious matches. In certain cases, these refinements could yield firms for which two or more workers may be a demographic match to two or more owners reported in the SBO. I keep all such matches as long as the number of workers is equal to or less than the number owners in the match pool. I drop all other firms where I'm unable to distinguish owners from potentially spurious matches.

3 Empirical Strategy

This section presents the core firm-level empirical strategy used to estimate the effects of past labor market experiences on owner-manager behavior. This analysis shows that firms operated by managers with greater past exposure to elevated local unemployment rates offer higher pay on average despite similar revenue and employment trends.

3.1 Measuring Local Unemployment Experience

I follow Malmendier and Shen (2019) to compute measures of local unemployment exposure of owner-managers when they gain ownership of their firm. They model the effects of past unemployment rate experience as a weighted average of past realized unemployment rate exposure parameterized to capture the degree of relative recency bias. While their experience-based learning model implies life-cycle dynamics as new experiences are gained, I fix an individual owner-manager's experiences at the time of firm acquisition to ensure that I do not conflate

[7] See Babina (2020) and Wallskog (2022) for recent examples.

post-acquisition firm-level shocks with experience-based learning.

Specifically I define cumulative, lifetime unemployment rate exposure at time t_{Entry} as a function of past local realizations UR_{t-k} in periods $\{0, t-1\}$

$$E_{t_{Entry}} = \sum_{k=0}^{t-1} w(\lambda, t, k) W_{t-k}. \tag{1}$$

where the weighting function $w(\lambda, t, k)$ is given by

$$w(\lambda, t, k) = \frac{(t-k)^{\lambda}}{\sum_{t=0}^{t-1}(t-k)^{\lambda}}.$$

The parameter $\lambda > 0$ thus captures the degree of recency bias under which an individual internalizes new experiences. Following Malmendier and Shen (2019), I use linear weights given by $\lambda = 1$ in my baseline analysis while also showing robustness to larger values of λ.

To operationalize these measures, I define an owner-manager's location as their county of residence as identified using the LEHD-SBO merge described in Section 2. I obtain data on county- and state-level unemployment rates from the U.S. Bureau of Labor Statistics' (BLS) Local Area Unemployment (LAU) Statistics program. A limitation of this data is that county-level LAU data is only available after 1989, while state-level data is only available after 1976. I construct experience measures using the smallest geographic data available in a given year. For years prior to 1976, I use national unemployment rate data obtained from BLS.

While the LEHD provides data on individuals date of birth, I only observe county of residence in a limited window of years preceding firm acquisition. I thus impute residency in missing years using the most recently available location observed in the LEHD. This naturally introduces measurement error for individuals that move locations. As long as this error is not systematically correlated with both experience and unobserved owner characteristics, it will only produce attenuation bias in my difference-in-differences results. Additionally, given the available data coverage in both the BLS and LEHD data, an advantage of my approach is that I use progressively more conservative measures of unemployment exposure as measurement error increases.

Throughout my analysis, I categorize owner-managers as either "high URE" or "low URE" based on whether their calculate unemployment exposure is above or below the median value of $E_{t_{Entry}}$ in my sample of owner-firm matches. This binary treatment variables reduces the sensitivity of my estimates to outliers in $E_{t_{Entry}}$ while also avoiding challenges associated with interpreting coefficients in difference-in-differences settings with continuous treatment (Callaway, Sant'Anna and Goodman-Bacon, 2021).

3.2 Firm Acquisitions Design

My baseline empirical analysis estimates the differential effects of firm ownership transfers on firm and worker outcomes based on the URE of the acquiring owner-managers. I implement a "stacked" difference-in-differences that compares firms in event time relative to the time at which an ownership change occurs (Cengiz, Dube, Lindner and Zipperer, 2019). This empirical strategy enables me to isolate changes in outcomes due to the arrival of new ownership rather than due to unobserved differences in firm- and market-level characteristics. By restricting my difference-in-differences comparisons to acquired firms only, I also ensure my results are not driven by any mechanical effects of ownership transfers relative to firms whose ownership remains unchanged.

For firm outcomes Y_{jt}, I estimate regressions of the form:

$$Y_{jt} = \sum_{k=-3, k\neq -1}^{5} \beta_k [E_{it} \times 1(e = k)] + \gamma \mathbf{X_{jt}} + \mu_j + \varepsilon_{it}, \tag{2}$$

where subscripts i, j, and t denote owner-managers, firms, and years relative to an ownership transfer, respectively. The treatment variable E_{it} is equal to one if a firm is acquired by a high URE owner-manager. The coefficients of interest, β_k, thus describe the average difference in outcomes across owner-manager URE status in event time k relative to the difference in outcomes in the year preceding the acquisition event. All regressions include firm fixed effects μ_j and event-time fixed effects interacted with various controls, given by $\mathbf{X_{jt}}$. My baseline specification includes (1) firm size quartiles-by-3-digit NAICS industry-by-event study fixed effects and (2) acquisition year-by-event time fixed effects. These controls allow me to account for common industry and time-varying shocks, in particular exposure to the post-2008 recession, while also leaving sufficient variation to estimate parameters of interest. Because variation in URE primarily varies according to age and location, I cluster standard errors at the birth year-county level (Cameron and Miller, 2015).

The identifying assumption underlying my empirical strategy is that outcomes at firms acquired by high URE owners would have evolved in a similar fashion to those acquired by low URE owners if the acquiring individuals had the same prior local unemployment rate exposure. There are two primary threats to the causal interpretation of model estimates as the effect of experiences on managerial pay policy. The first potential threat to identification is that, because treatment is defined by geographic exposure, owner URE could be correlated with contemporaneous unemployment rates and other market-level shocks that are likely to affect firm and work outcomes. While estimating effects on within-firm outcomes ensures that time-invariant differences in labor market characteristics do not confound my results, time-varying shocks could interact with the timing of acquisition. My empirical design allows me to evaluate this concern by testing pretrends on firm outcomes prior to acquisition. I also consider specifications that

further interact $\mathbf{X_{jt}}$ with indicators for whether local unemployment rates were above or below the sample median in the four years preceding firm acquisition.

The second threat to identification worthy of consideration is that URE drives selection into entrepreneurship in a way that correlates with managerial ability. If this were the case, my results could be driven by an increase in firm productivity rather than a behavioral difference in managerial policy. I conduct several tests that help rule out selection on unobservables as a driver of my results. I consider specifications that control for owners' financial resources prior to acquisition, arguably the most direct way that URE may induce selection into entrepreneurship. Specifically, I interact $\mathbf{X_{jt}}$ with controls for owners' average labor income in the five years prior to firm acquisition, and for indicators denoting whether the firm acquisition was funded via debt financing.

Beyond the direct effects on entrepreneurs' financial resources, the direction of further selection bias in my empirical design is ex ante ambiguous. If elevated past exposure to high LAU periods shocks entrepreneurs' preferences toward greater risk-aversion, then it is possible that relatively lower-ability workers select out of entrepreneurship due to LAU exposure, biasing my treated sample toward higher-productivity owner-managers. Conversely, Babina (2020) finds that financial distress at the firm level induces entry into entrepreneurship for affected workers. If this effect dominates, greater LAU exposure could induce less-capable individuals to enter into ownership, biasing firm outcomes downward.[8] I discuss selection bias in the context of my empirical results in Section 3.4.

3.3 Estimation Results: Firm Acquisitions

I estimate these regressions on a balanced, 10-year panel firms. To ensure that my firms are informative of managerial policy toward worker pay, I only consider firms that maintain at least 5 employees throughout the four year period preceding the ownership change. Table 2 presents select owner and firm characteristics across my treatment categories for the 2,450 firms for which I identify ownership changes. On average, owner-managers classified as "low URE" have a URE value of 5.27 percent unemployment, while high URE owner-managers have URE of 6.89 percent unemployment. Owners and firms are otherwise similar across treatment groups. High URE owner-managers are slightly younger on average, but with identical prior experience in self-employment and business ownership. Acquired firms were of similar size prior to acquisition, averaging about 34 employees, and are equally likely to provide nonwage amenities such as health insurance and profit sharing arrangements under the acquiring ownership.

Figure 1 presents my headline result that firms acquired by high URE owners increase average

[8] He and le Maire (2022) uses manager retirements and sudden deaths to generate plausibly exogenous variation in manager turnover. While such a design is conceptually possible in my setting, the relatively smaller sample size produced by the SBO data makes empirical implementation unfeasible.

worker compensation relative to those acquired by low URE owners. Log mean worker earnings at firms acquired by high URE owners display no differential trends prior to ownership transfer, and I estimate a precise zero effect in the year in which the transfer occurs. In the first full year of new ownership, mean earnings are 2.1% greater at high URE firms relative to low URE. This effect holds five years after the ownership transfer, when mean earnings are 3.4% higher on average. Figure 1 also demonstrates that the estimated effects on worker earnings are similar across several alternative controls, each of which is interacted with acquisition year-by-event time fixed effects. Specifically, I consider (1) owner financial resource proxies, defined above, (2) firm S-Corp status, (3) decile bins of post-period average LAU rate, and (4) quintiles of changes in LAU over the five year post period. In particular, the fact that post-period LAU and owner financial controls yield slightly larger effects suggests that these confounders would otherwise attenuate the estimated effects on worker earnings toward zero.

In contrast, I find no evidence that owner-manager URE affects firm-level employment or revenue post-acquisition. Panel (A) of Figure 2 shows no differential trend in firm-level log employment prior to ownership change, and a small, insignificant positive effect for firms acquired by high URE owners. Panel (B) shows that, while revenue effects are much noisier, I estimate a similar null effect on average. This noise potentially reflects the fact that revenue data from the LBD must be imputed for some years, particularly in earlier years. Difference-in-differences estimates of these outcomes in Table 4 show an approximate zero effect on log employment, and a noisy 1.9% effect on revenue in my baseline specification. Given the aforementioned data quality issues, I interpret this revenue effect cautiously. These effects are again similar across the alternative empirical specifications tested in Figure 1.

3.4 Robustness and Empirical Validation

I conduct several additional analyses to rule out plausible alternative explanations for these results. Figure 3 re-estimates the firm-level earnings effects while re-defining treatment according to either the average LAU in the four-year pre-period or by age cohort. This figure demonstrates that neither of these factors alone explain the positive earnings result documented in Figure 1 and reproduced here. I additionally test for whether these earnings effects are driven by the largest firms, firms that experience large declines in employment, or high-growth firms. Table B.1 presents estimates from specifications that interact difference-in-differences terms with indicators corresponding to each of these categories. Large and high-growth firms exhibit no statistical difference difference in mean earnings, while earnings differences are approximately zero (though again noisy) for declining firms. These diagnostic tests allay any concern that selection on the growth trends of acquiring firms may explain results otherwise attributed to acquiring owner-managers.

I also show that my results are not sensitive to calibration of URE treatment variation. Figure

B.2 shows that my results are similar when using a continuous rather than discrete treatment definition. Panel (A) of Figure B.3 shows that these effects are very similar when calibrating $E_{t_{Entry}}$ such that $\lambda \in 1, 3$. Panel (B) of Figure B.3 estimates a version of Equation 2 that defines an owner-manager's URE as the average unemployment exposure from ages 18 to 25 (e.g. Cotofan, Dur and Meier (2021)). Though small and noisy, I find that such experiences predict a 2.5% increase in average earnings five years after the ownership transfer. The fact that I find larger and more precise effects using the full lifetime exposure measure suggests that the experienced-based learning model of Malmendier and Shen (2019) is more informative of managerial behavior.

Finally, while these estimates can not rule out the possibility that URE induces selection on manager characteristics, the fact that manager URE is associated with no significant employment or revenue effects limits the scope for selection to meaningfully explain my pattern of results. In particular, URE would have to select specifically on manager pay strategies rather than, for example, biasing the population of acquiring managers toward higher ability individuals, to simultaneously explain these patterns. Section 4.3 provides additional support for the hypothesis that the relationship between owner-manager URE and pay policy is not confounded by significant selection bias.

4 Manager Experiences and Firm Personnel Strategies

This section explores the mechanisms underlying my top-line result that elevated local unemployment rate exposure leads owner-managers to offer workers higher earnings on average. Prior research documenting manager-specific firm pay components provides some suggestive evidence on the role of manager characteristics and personal experiences (He and le Maire, 2022; Acemoglu, He and le Maire, 2022). Although the results presented in section 3.3 are consistent with the notion that exposure to high local unemployment shifts managers' fairness views toward offering higher pay, this section presents empirical tests of several alternative mechanisms reflecting the economic rationale underlying different approaches to firm pay-setting.

I first test whether the firm-level differences in mean earnings reflect meaningful earnings effects for incumbent workers at acquiring firms. This analysis reveals that, while incumbent workers do experience earnings growth on average, these effects are largely confined to older and college educated workers, the former of whom are also significantly more likely to remain at the firm. Motivated by these findings, I next explore how perceptions about worker turnover costs to the firm mediated my effects, finding significant scope for this as a key mechanism. Third, I utilize a separate sample of firms owned and operated by their original founder to test whether high URE manager pay premia reflect greater rent-sharing with workers. Finally, I test the possibility that correlation between owner-manager experiences and implicit insurance provision

at the firm explains my results.

4.1 Effects of Ownership Changes on Incumbent Workers

Though my prior results are suggestive of a change in firm-level pay policy, they do not rule out the possibility that changes in worker composition may fully explain these effects (Abowd, Kramarz and Margolis, 1999). To explore whether my results represent a change in pay policy conditional on worker conditions, I estimate worker-level analogues of Equation 2 on incumbent workers. I define incumbent workers as those who were employed at acquired firms at least two years prior to the ownership change, and who report positive earnings in the year after the ownership change occurred. Conditional on this incumbency status, I consider the total earnings of workers, including from other firms post-acquisitions, since changes in firm management may also affect worker mobility.

Figure 4 presents the effects of owner-manager URE on the earnings of these workers following ownership changes. The earnings of incumbent workers across firms exhibit no differential trends prior to firm acquisitions. These earnings effects largely mirror the dynamics of my firm-level estimates, though the five-year effect of 3.2% is slightly smaller than the largest firm-level estimates in Figure 1. Column (1) of Table 5 reports the corresponding difference-in-differences earnings estimate, showing that incumbent workers at high URE firms experience on average a 2.4% earnings increase.

A natural question arising from these results is which incumbent workers benefit most from increases in firm-level pay following transitions to new owner-managers. I investigate this margin by estimating my difference-in-differences specifications while interacting URE indicators with indicators for worker age and education. Specifically, I use a binary variable for whether incumbent workers are above the age of 44 (the average age of new firm owners in my sample), and an indicator for whether workers possess at least a bachelor's degree or equivalent. This heterogeneity analysis reveals that the earnings effects on incumbent workers are entirely driven by older and college-educated workers. These results are presented graphically in Figure 5.[9] Earnings effects for younger workers are approximately zero, while older workers receive a large, statistically significant earnings increases of 4.1% $(0.044 - 0.003)$. Similarly, workers without a college degree receive a small positive earnings increase that is statistically indistinguishable from zero. In contrast, college educated workers receive large average increases of around 5.4% $(0.040 + 0.015)$.

These effects are also accompanied by a increased probability of remaining at the firm for relatively older workers. [10] Specifically, such workers are 1.5% ($p = 0.097$) more likely to remain

[9] Point estimates, standard errors, and p-values are reported in Appendix Table B.2.

[10] Heterogeneity by worker education, in contrast, yields precise zero effects on retention. Education variables in the LEHD are derived from the ACS and Decennial Censuses and are thus imputed for many workers. For

at their firm over the post-period, and 2.4% ($p = 0.065$) more likely to remain five years after the acquisition event. Taken together, these results could suggest that these results reflect the retention of "key personnel" in the acquisition of the firm.

4.2 Worker Turnover Costs

These worker-level estimates point to a potentially significant role for perceptions about worker turnover costs to explain the relationship between owner-manager URE and firm pay premia. Bilal (2023*b*) demonstrates that spatial differences in local unemployment rates are largely explained by differences in job losing rates rather than job finding rates. If high URE owner-managers internalize these salient signals of job turnover costs, the may respond by offering higher worker compensation to reduce turnover at acquired firms (Burdett and Mortensen, 1998). This hypothesis is made more plausible given empirical estimates of the costs of job turnover to the firm, which suggest broad scope for job turnover to meaningfully affect firms in the form of forgone productivity, increased training costs, and recruiting efforts.[11] Finally preserving the worker-firm match during recessionary periods is a primary motive for many popular fiscal policies (Giupponi, Landais and Lapeyre, 2022).

To generate a direct "first stage" test of this hypothesis, Figure 6 demonstrates that firm-level earnings effects in Figure 1 also accompany greater retention of incumbent workers. As a more direct test for the turnover cost hypothesis, I estimate the heterogeneous effects of high URE managers on employment and earnings by whether the acquired firm had previously experienced average job turnover above the sample median. Figure 7 presents these results graphically for both DiD and long difference effects. [12] Interestingly, I find that worker earnings effects are almost entirely driven by effects at acquiring firms with high prior turnover. This result is consistent with high URE owners being particularly sensitive to signals of turnover risk at acquiring firms, and raising worker compensation as a result. At the same time, I do not find evidence of greater retention at these firms, perhaps reflecting the noise generated by elevated job turnover rates at these firms.

4.2.1 Earnings Effects for New Hires

Owner-managers may also set pay to reduce the costs associated with recruiting and retaining new workers (Bertheau, Cahuc, Jäger and Vejlin, 2022). To test this possibility, I track workers

this reason, it is possible that my earnings effect on "college" educated workers instead reflects demographic characteristics that predict higher education levels.

[11] Bertheau, Cahuc, Jäger and Vejlin (2022) estimates that unexpected worker deaths lead to reduced profits equivalent to the average workers' yearly salary, primarily through reduced value-added and higher pay among incumbent workers. Dube, Freeman and Reich (2010) find smaller reported turnover costs (approximately a month of mean worker earnings) from a survey of firms in California.

[12] Tables B.3 and B.4 present point estimates and standard errors for difference-in-differences and long difference coefficients, respectively.

that undergo job-to-job transitions to the firms in my sample in the five years following the acquisition event. As in the preceding analyses, I compare workers hired at high URE firms to those hired at low URE firms. Because this event-study design compares worker earnings to those at their previous firm, I interpret these estimates as reflecting the relative pay premia offered to new hires by owners with different URE.

Figure 8 presents event-study regression estimates from these specifications, indicating that workers hired at high URE firms experience large, immediate, and persistent earnings increases over their previous jobs (relative to those hired at low URE firms). These results also hold when including controls for workers' earnings growth at their prior firm, which otherwise may induce selection on which kinds of workers are hired across firms. Columns (2) and (3) of Table 5 show that the average post-hire effects are significant and largely in line with those for incumbents. To confirm that these differences reflect the input of new owner-managers, Figure 9 demonstrates that any such relative new hire pay premium is absent prior to the firm ownership transfer. Overall, these results show that high URE firms also offer relative pay premia to new hires.

4.3 Firm Rent-Sharing

I next test whether high URE manager pay premia reflect greater passthrough of firm-level rents. Firm rent-sharing has been studied extensively as a driver of firm-level pay discrepancies (e.g. Card, Cardoso, Heining and Kline (2018); Kline, Petkova, Williams and Zidar (2019); and Lamadon, Mogstad and Setzler (2022)). Additionally, estimates of rent-sharing elasticities across owner-managers allow me to unpack whether pay differences are due to differences in firm-level rents (i.e. conditional on the same pass-through elasticity) or in differences in rent-sharing *conduct*, conditional on similar firm-level rents.

I estimate pass-through elasticities using worker-level regressions that model the relationship between worker earnings and revenue-per-worker at the firm. Following Card, Devicienti and Maida (2014), I instrument for firm-level revenues using industry-specific revenue variation in other commuting zones. To gain greater empirical traction, I estimate these regressions using a large sample of firms owned and operated by their original founders. This yields a sample of approximately 255,000 worker-year observations. Appendix A describes this sample construction and estimation strategy in detail.

Table 6 presents both OLS and IV estimates of equation A.1 over the 1997-2007 period. As my baseline estimates, I initially exclude the 2008-2013 recession years because my IV variation might otherwise be confounded by large, aggregate shocks which may induce firm closure or contraction. Column (1) shows no difference in firm pass-through by owner URE status using OLS. While unreliable as a measure of rent-sharing, since OLS likely conflates differences in worker productivity with differences in firm conduct, these results are still useful as descriptive measures of the firm-level labor share. In contrast, my IV design reveals a significant difference

in pass-through elasticities according to owner experiences: high URE owner-managers increase worker earnings by approximately 0.22 dollars for every 1 dollar increase in firm rents, as defined by my IV measure, while low URE managers only increase worker earnings by 0.064 dollars for an equivalent increase in rents. These differences are statistically different at the 5% level. Together, these results provide evidence that my prior estimates reflect changes in conduct consistent with greater passthrough of firm revenue shocks.

To make these results more comparable to those in previous sections, I estimate how the earnings of new hires at firms in this sample evolve relative to prior earnings, displayed in Figure B.4. These specifications, which replicate the specifications used to generate Figure 8, indicate less immediate earnings effects, but still translate to significant positive earnings growth over time, as would be expected under greater rent-sharing.

4.4 Labor Market Conditions and Firm Insurance Provision

As a final test of the mechanisms underlying the relationship between owner-manager URE and firm pay policy, I consider the possibility that past exposure to elevated unemployment rates induces managers to offer more generous implicit insurance contracts to workers via earnings (Beaudry and DiNardo, 1991; Guiso, Pistaferri and Schivardi, 2005). If LAU exposure is correlated with future adverse shocks, my results could reflect high URE managers offering better implicit insurance against shocks via the wage contract. I test this possibility by estimating heterogeneous treatment effects across three continuous measures of local labor market conditions: (1) average pre-period county unemployment rates, (2) average post-period county unemployment rates, and (3) changes in county unemployment rates across the post-period. Figure 10 demonstrates this heterogeneity analysis graphically. I find little evidence for heterogeneity by contemporaneous labor market conditions, suggesting differences in implicit insurance provision do not meaningfully drive post-acquisition earnings differences across managers.

As an alternative test of the insurance provision hypothesis, I also repeat the analysis in section 4.3 using data from the 2008-2013 Great Recession in Appendix Table B.6. While more suggestive, since the recession likely induces selection on firm and job survival, I interpret these IV estimates as reflecting how worker earnings responded to large, negative shocks. Interestingly, I find here that rent-sharing patterns reverse across owner URE status. This result is broadly consistent with the hypothesis that high URE owners may differ in how they provide implicit insurance contracts to workers to insulate them from shocks. Bolstering this interpretation is the fact that the relative labor share (i.e. the OLS estimates in column (1)) is slightly larger (though not significantly different) in firms operated by high URE managers.

5 Conclusion

This paper investigates how the personal labor market experiences of owner-managers influence pay policy, finding that owners with greater exposure to elevated local unemployment rates offer higher pay on average. I do so using a unique data linkage that improves upon existing methods used to study how entrepreneurs acquire firms. While my headline result on worker earnings are consistent with prior work showing that fairness norms shape how managers set wages, I instead argue that my effects suggest that my results are also reflective of economic strategies reflected in prevailing theories of the labor market. In particular, I find that the effects of owner experiences on firm pay policy are consistent with high URE managers preferring lower worker turnover, particularly among key personnel. Future work can use this data linkage to improve our understanding of both how workers select into firm ownership and how these forces shape pay and hiring policy.

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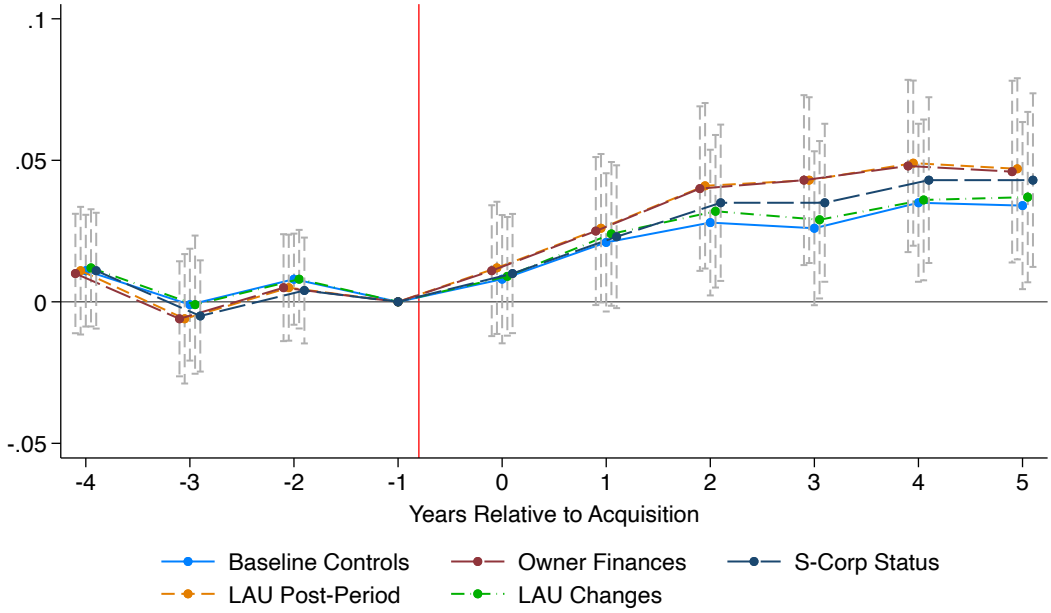
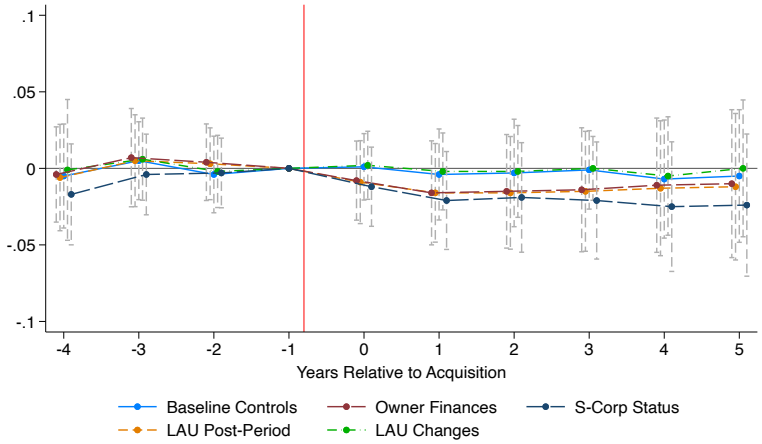
Figure 1: Effects of Managers' URE on Firm Mean Worker Earnings after Ownership Transfer



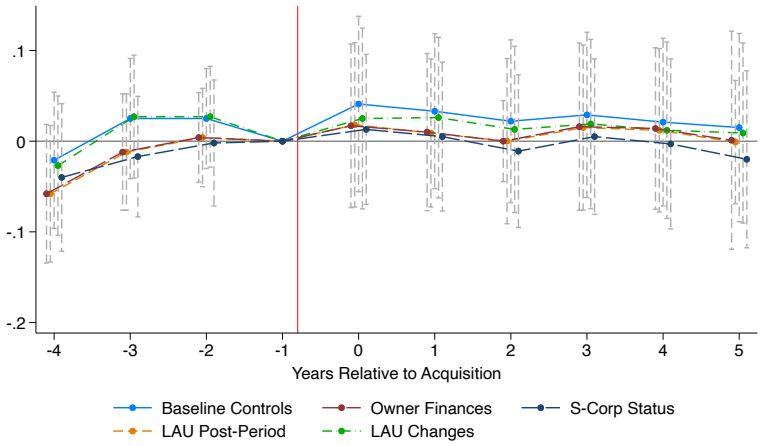
Notes: Figure 1 plots event-study coefficient estimates of the differential effects of firm ownership changes on average worker earnings across acquiring owner URE. *Source:* Authors' calculations based on LEHD and SBO data.

Figure 2: Effects of Managers' URE on Firm Outcomes after Ownership Transfer

(A) Log Employment

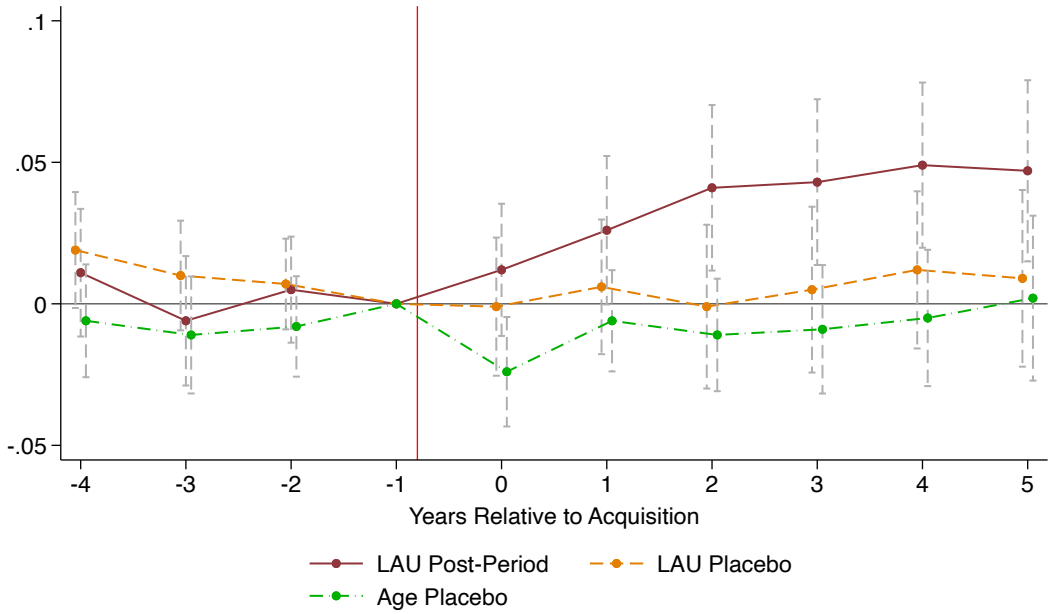


(B) Log Revenue



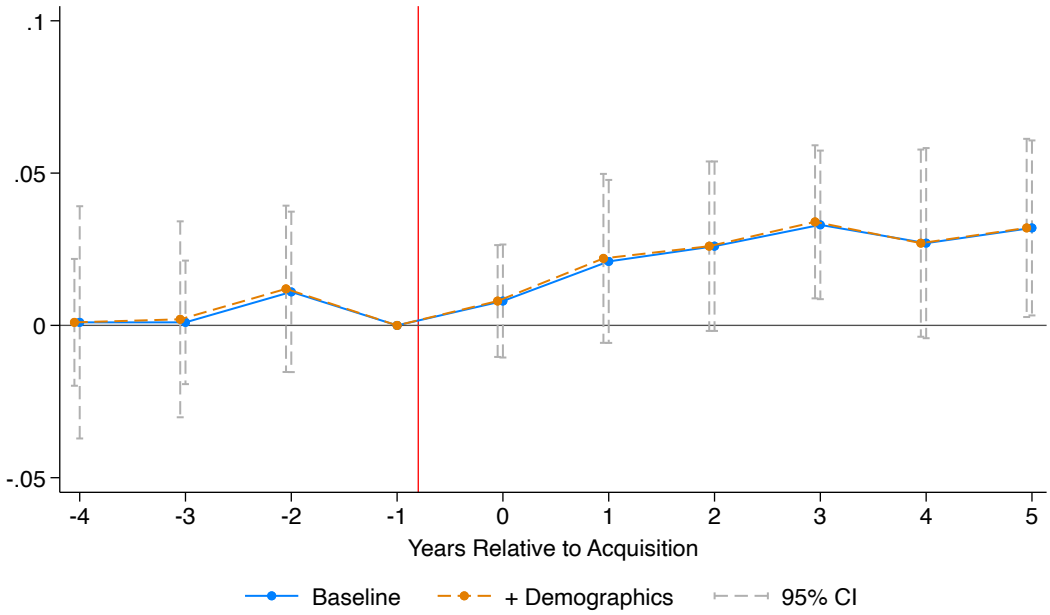
Notes: Figure 2 plots event-study coefficient estimates of the differential effects of firm ownership changes on firm revenue and employment across acquiring owner URE. *Source:* Authors' calculations based on LEHD and SBO data.

Figure 3: Effects of Managers' URE on Mean Earnings: Location and Age Cohort Placebos



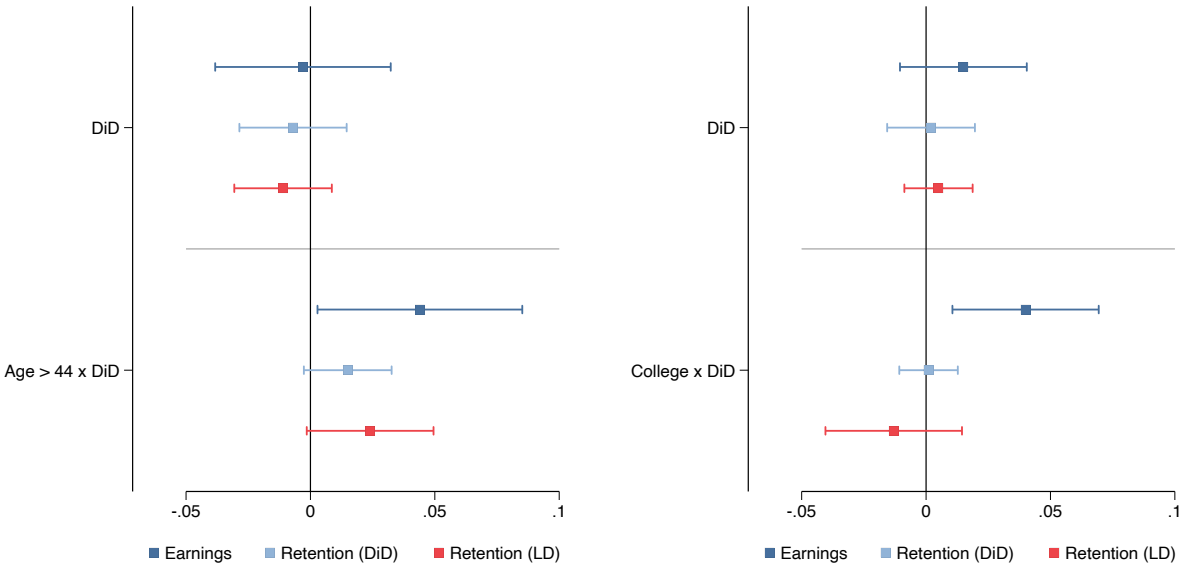
Notes: Figure 3 plots event-study coefficient estimates of the differential effects of firm ownership changes on average worker earnings using two placebo treatments. "LAU Placebo" denotes treatment defined as firms where the average unemployment rate in the four years preceding the acquisition event was above the sample median. "Age Placebo" denotes treatment defined as firms acquired by new owners over the age of 44, the sample mean, at the time of the ownership transfer. *Source:* Authors' calculations based on LEHD and SBO data.

Figure 4: Effects of Owners' URE on Log Incumbent Worker Earnings



Notes: Figure 4 plots event-study coefficient estimates of the differential effects of firm ownership changes on the earnings of incumbent workers across acquiring owner URE. Incumbency status is defined as workers that were employed at the firm at least two years prior to the ownership change, and who report some positive earnings in the year after the change. *Source:* Authors' calculations based on LEHD, SBO, and BLS data.

Figure 5: Effects of Owners' URE on Worker Outcomes: Demographic Heterogeneity

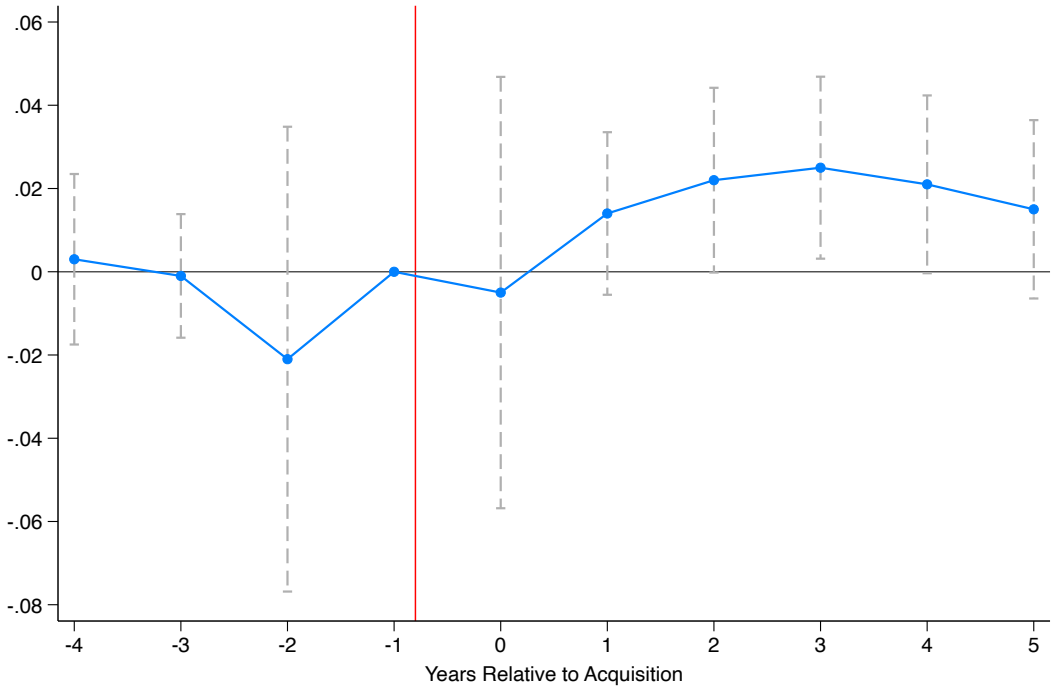


Panel A: Worker Age

Panel B: Worker Education

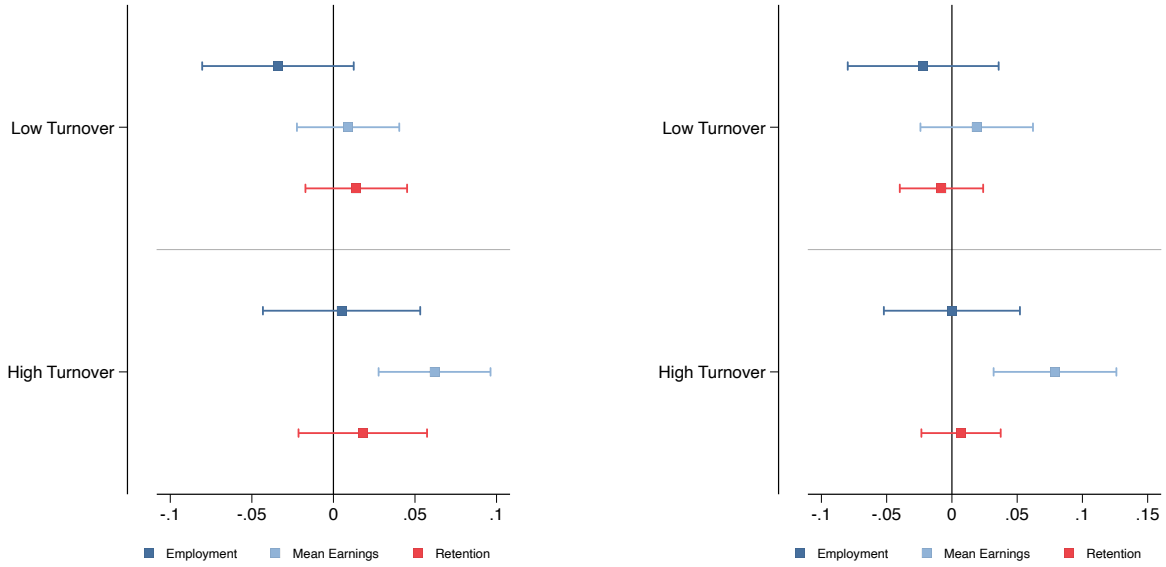
Notes: Figure 5 reports heterogeneous coefficient estimates of the differential effects of firm ownership changes on the earnings and retention probability of incumbent workers across acquiring owner URE. Panel A reports heterogeneous effects according to worker age. Panel B reports heterogeneous effects according to imputed worker education. *Source:* Authors' calculations based on LEHD, SBO, and BLS data.

Figure 6: Effects of Managers' URE on Incumbent Retention



Notes: Figure 6 plots event-study coefficient estimates of the differential effects of firm ownership changes on the share of event time t-1 employees that remain at the firm relative to total employment. *Source:* Authors' calculations based on LEHD and SBO data.

Figure 7: Effects of Owners' URE on Mean Worker Earnings: Firm-Level Turnover Heterogeneity

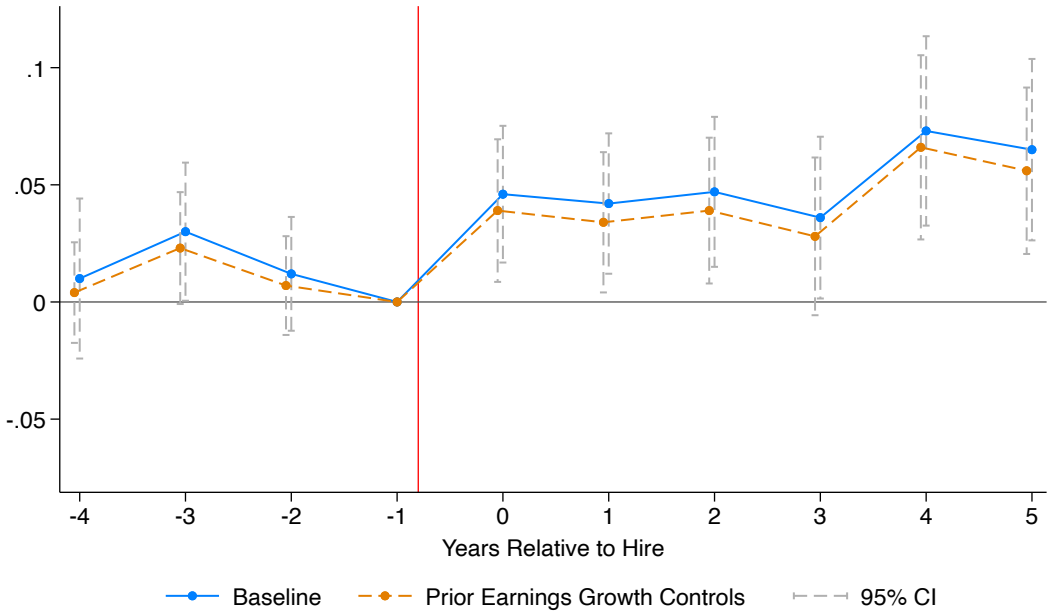


Panel A: DiD

Panel B: Long Difference

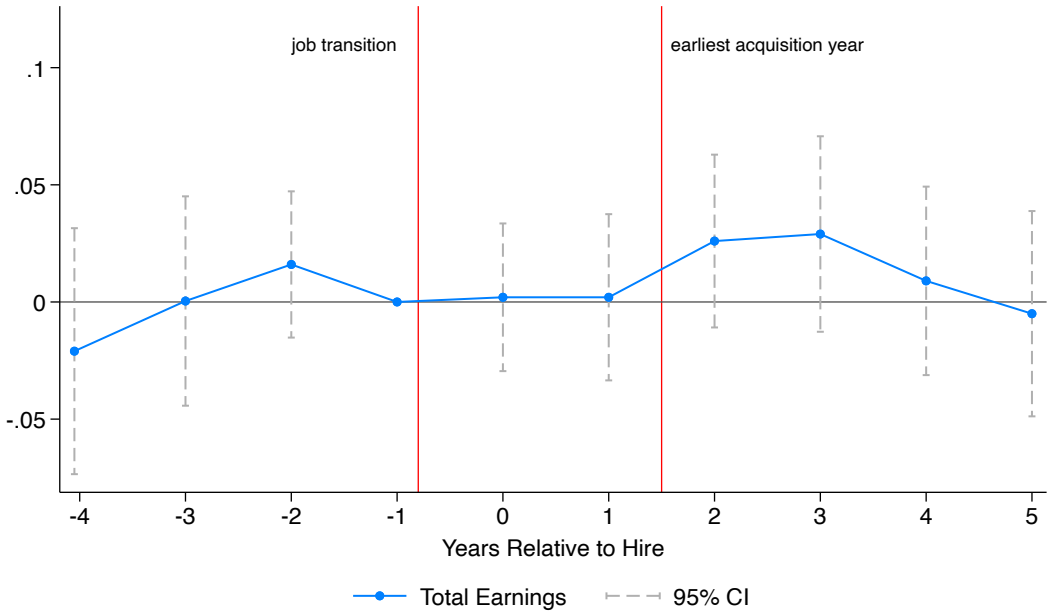
Notes: Figure 7 reports heterogeneous difference-in-differences coefficient estimates associated with the differential effects of firm ownership changes across acquiring owner URE on mean worker earnings at the firm level. These coefficient estimates are interacted with firm-level indicators for whether worker turnover at the firm was above the sample median in the pre-acquisition period. *Source:* Authors' calculations based on LEHD, SBO, and BLS data.

Figure 8: Effects of Job Transitions on Log Worker Earnings: Post Acquisition



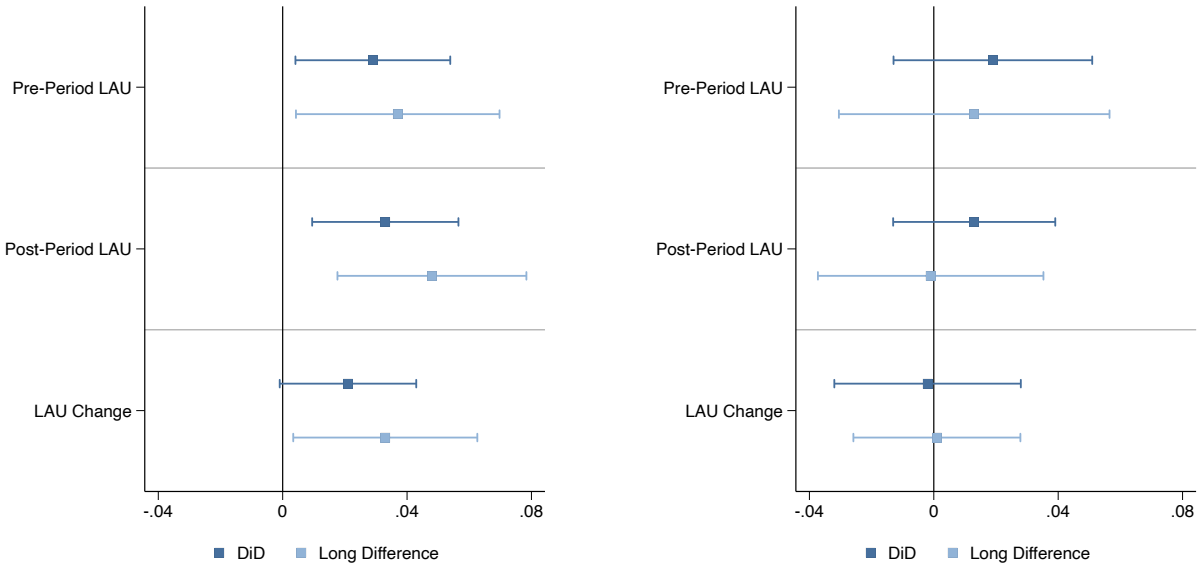
Notes: Figure 8 reports event-study coefficient estimates of the differential earnings effects of job-to-job transitions of workers hired at acquired firms depending on the URE of new owners. *Source:* Authors' calculations based on LEHD, SBO, and BLS data.

Figure 9: Effects of Job Transitions on Log Worker Earnings: Pre- Acquisition



Notes: Figure 8 reports event-study coefficient estimates of the differential earnings effects of job-to-job transitions of workers hired before firms undergo ownership changes. These effects thus represent the relative earnings premia experienced by hires at firms that will go on to be acquired by high URE owners relative to those at firms that will soon be acquired by low URE owners. *Source:* Authors' calculations based on LEHD, SBO, and BLS data.

Figure 10: Effects of Owners' URE on Mean Worker Earnings: LAU Heterogeneity



Panel A: Mean DiD Effect

Panel B: Heterogeneous Effects

Notes: Figure 10 reports heterogeneous difference-in-differences coefficient estimates associated with the differential effects of firm ownership changes across acquiring owner URE on mean worker earnings at the firm level. These coefficient estimates are interacted with different measures of contemporaneous local unemployment rates. Panel A reports the mean DiD effect associated with each interaction variable, and Panel B reports the continuous interaction term associated with the heterogeneous effects with respect to pre-acquisition mean LAU, post-acquisition LAU, and the percentage point change in LAU over the five-year post-acquisition period. *Source:* Authors' calculations based on LEHD, SBO, and BLS data.

Table 1: New Firm Entry by Sector: 2007 Survey of Business Owners

	Share
Professional, Scientific, Technical Services	0.146
Construction	0.144
Health Care and Social Assistance	0.107
Retail Trade	0.105
Accommodation and Food Services	0.087
Administrative, Support, Waste Management	0.074
Manufacturing	0.053
Wholesale Trade	0.051
Finance and Insurance	0.043
Transportation and Warehousing	0.040
Observations	58923

Table 1 shows the share of new firms in the 2007 SB0 in the listed industries. New firms are defined as those founded after 2004. *Source:* Authors' calculations based on SBO data.

Table 2: Characteristics of Owner-Mangers and Acquired Firms by URE Exposure

	(1) URE	(2) Acq. Year	(3) Owner Age	(4) Prior Own.
Low Exp.	5.27	2004	43.95	0.46
	(0.46)	(2.62)	(8.67)	(0.5)
High Exp.	6.89	2003	42.48	0.46
	(1.09)	(2.84)	(8.2)	(0.5)
	(5) Firm Size	(6) S-Corp	(7) Health Ins.	(8) ProfitShare
Low Exp.	35.1	0.63	0.74	0.12
	(58.3)	(0.48)	(0.44)	(0.33)
High Exp.	33.2	0.61	0.74	0.13
	(52.7)	(0.49)	(0.44)	(0.34)

Table 2 reports characteristics of owner-managers of acquired firms in my baseline empirical sample. *Source:* Authors' calculations based on LEHD and SBO data.

Table 3: Effects of Owners' URE on Firm Earnings after Ownership Transfer

	(1)	(2)	(3)
	Panel A: Log Mean Worker Earnings		
HighExp × Post	0.020	0.032	0.033
	(0.039)	(0.004)	(0.002)
	[0.071]	[0.011]	[0.006]
	Panel B: Log Mean Total Earnings		
HighExp × Post	0.025	0.02	0.027
	(0.012)	(0.01)	(0.012)
	[0.037]	[0.046]	[0.024]
Base FE	✓		
Owner Finances FE		✓	
Unemp. Rate FE			✓
N	24,500	24,500	24,500

Table 3 reports difference-in-differences coefficient estimates corresponding to my firm-level event study designs.
Source: Authors' calculations based on LEHD and SBO data.

Table 4: Effects of Owners' URE on Firm Employment and Revenue after Ownership Transfer

	(1)	(2)	(3)
	Panel A: Log Employment		
HighExp × Post	-0.002	-0.014	-0.014
	(0.5615)	(0.6708)	(0.6364)
	[0.885]	[0.477]	[0.465]
	Panel B: Log Revenue		
HighExp × Post	0.019	0.026	0.025
	(0.710)	(0.7411)	(0.738)
	[0.609]	[0.501]	[0.513]
Base FE	✓		
Owner Finances		✓	
Unemp. Rate FE FE			✓
N	24,500	24,500	24,500

Table 4 reports difference-in-differences coefficient estimates corresponding to my firm-level event study designs. *Source:* Authors' calculations based on LEHD and SBO data.

Table 5: Earnings Effects for Incumbent Workers and New Hires

	(1) Incumbents	(2) New Hires	(3) New Hires
HighExp × Post	0.024**	0.033**	0.026**
	(0.012)	(0.014)	(0.013)
	[0.046]	[0.018]	[0.046]
Pre-Period Earn. Growth FE			✓
Observations	332,000	277,000	277,000

Table 5 reports difference-in-differences estimates corresponding to my worker-level event-study designs. *Source:* Authors' calculations based on LEHD and SBO data.

Table 6: Rent-Sharing Elasticity Estimates: 1997-2007

	(1) OLS	(2) IV
ξ_{LowExp}	0.106***	0.064
	(0.013)	(0.109)
$\xi_{HighExp}$	0.102***	0.219*
	(0.012)	(0.116)
p[$\xi_{HighExp}$ - ξ_{LowExp}]	0.796	0.021
Weak IV F-stat		183.9
Observations	255,000	255,000

Table 6 reports OLS and IV estimates of equation A.1 over the 1997-2007 period. *Source:* Authors' calculations based on LEHD and SBO data.

Online Appendix: Not for Publication

A Pass-through Estimation

This appendix describes the empirical analysis in section 4.3 in greater detail. Firm rent-sharing has been studied extensively as a driver of firm-level pay discrepancies (e.g. Card, Cardoso, Heining and Kline (2018); Kline, Petkova, Williams and Zidar (2019); and Lamadon, Mogstad and Setzler (2022)). Estimates of rent-sharing elasticities across owner-managers helps address whether pay differences are due to differences in firm-level rents (i.e. conditional on the same pass-through elasticity) or in differences in rent-sharing *conduct*, conditional on similar firm-level rents.

This analysis also helps to address several potential concerns regarding the interpretation of the preceding results as reflecting changes in managerial discretion around pay policy. First, while the firm-level revenue effects from Section 3.3 are noisy, they could reflected differences in the underlying productivity of acquiring owner-managers, via selection on prior labor market experience, that translates to higher worker earnings via higher marginal products of labor. Second, my prior results could reflect geographic heterogeneity that is correlated with recent local labor market conditions but not captured by the local unemployment rate controls and heterogeneity considered throughout the paper. I address these two issues by estimating pass-through elasticities for fixed changes in revenue and flexibly controlling for local labor market trends, respectively.

I estimate regressions of the following form at the worker-level:

$$\ln Earn_{it} = \xi_{OwnExp_j} \ln \frac{Rev_{jt}}{Emp_{jt}} + \gamma_i + \alpha_{jkt} + \varepsilon_{ikt}. \tag{A.1}$$

where i, j, k, and t index workers, firms, commuting zones, and years, respectively. The parameters of interest ξ_{OwnExp_j} dictate how firm-level revenue-per-worker relates to worker earnings and is allowed to vary by whether the firm's owner-manager was classified as high or low URE at the time of founding. In all regressions, I include worker fixed effects γ_i. I also include commuting zone-by-year fixed effects, which help isolate variation in firm-level conduct due to owner experiences that is uncorrelated with local time-varying shocks. Finally, I also interact these location controls with quartile bins of firm employment growth in their first five years of operation to ensure that my results are not biased toward high-growth firms.

Because firm level revenue-per-worker is likely to correlated with worker productivity, I follow Card, Devicienti and Maida (2014) and instrument for firm-level rents-per-worker using the average value of $\frac{Rev_{jt}}{Emp_{jt}}$ at other firms in the same 4-digit NAICS industry but operating exclusively in separate commuting zones. Because I control for CZ-by-year shocks, my identification restricts

attention to firms in different industries but operating in the same commuting zone in the same year, whose owners had different past URE exposure when they founded their firm. Identification of ξ_{OwnExp_j} parameters requires the assumption that the instrument variation only affects worker earnings via changes in firm-level rents, not, for example via labor market adjustments driven by same industry firms exposed to the same shock. Though potentially a strong assumption, I can interpret the difference in ξ_{OwnExp_j}'a across owner experiences by imposing the less restrictive assumption that these sources of bias are common across firms in the same commuting zone.

I estimate these regressions using an alternative sample of workers employed at firms that do not undergo ownership changes. I restrict attention to workers attached to their firm for at least four consecutive quarters and who earn at least 5000 in 2001 U.S. dollars in each year they are attached to the firm. This procedure yields 255,000 worker-year observations over the 1997 to 2007 period. As in the firm acquisitions sample, I only consider firms that maintain at least five employees and exclude owner-managers from this analysis.

B Elasticity of Labor Supply to the Firm

Following Manning (2011), I use estimates from the firm acquisitions sample to calculate the elasticity of labor supply to the firm. By equating separation and recruiting elasticities, this parameter can be defined as two times the separation elasticity with respect to a given change in pay. In my setting, this translates to:

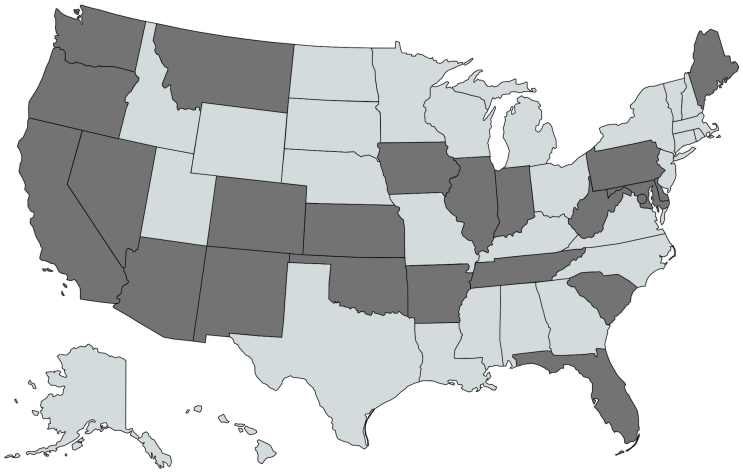
$$\varepsilon_{LS} = -2 \times \frac{-\hat{\gamma}_{HighExp}}{\hat{\phi}_{HighExp}},$$

where $\hat{\phi}_{HighExp}$ is the firm-level percent change in pay policy, as identified by differences in owner-manager URE, while $\hat{\gamma}_{HighExp}$ is my estimated effect on worker retention for subgroups. Because younger workers and those without a college degree have very small estimated retention elasticities, I calculate ε_{LS} using the effects for "key personnel" in Table B.2 ($\hat{\gamma}_{HighExp.}$) Because demographic-specific pay increases may reflect selection on unobserved worker characteristics, I follow Bassier, Dube and Naidu (2021) and use my "firm-level" estimates of changes in pay policy in column (1) of Table 5 ($\hat{\phi}_{HighExp}$). Across values of $\hat{\gamma}_{HighExp}$, I calculate values of ε_{LS} ranging from 1.1-1.8. These estimates indicate high degrees of monopsony power at acquired firms and are very close to values found in the meta analysis of Sokolova and Sorensen (2021). While more recent work by Bassier, Dube and Naidu (2021) finds larger elasticities (indicating less firm monospony power) my results can be interpreted as being specific to the setting of firm acquisitions, where worker preferences might dictate stronger attachments to their firms when an acquisition occurs.

As a final test relating my estimates to the literature on firm labor market power, I also

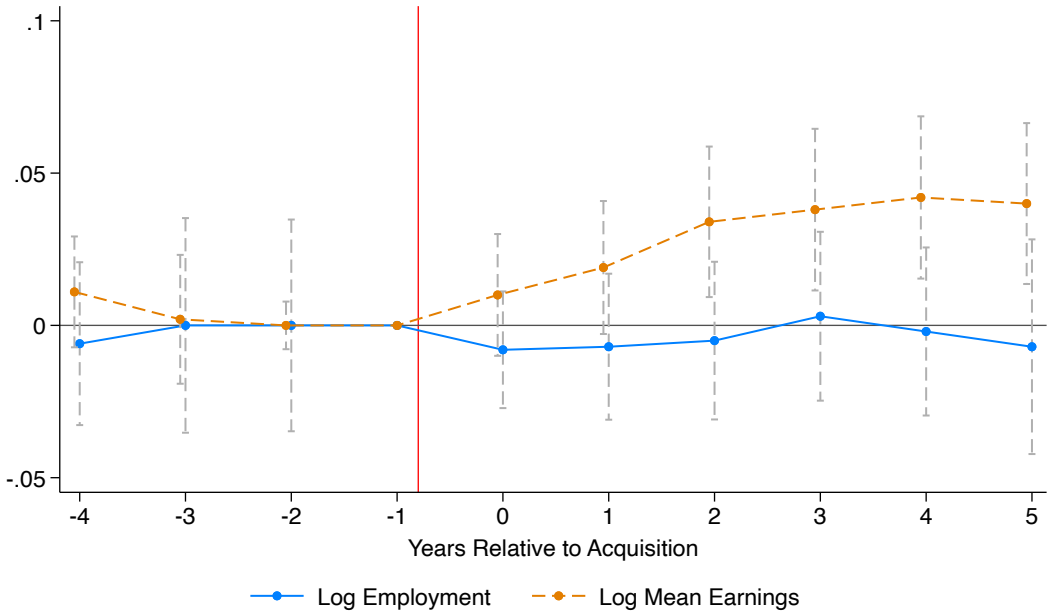
estimate heterogeneous effects according to local labor market concentration, displayed in Table B.5. I follow Arnold (2020) and use job flows across industries within the same commuting zone to calculate the Herfindahl-Hirschmann Index (HHI) of labor market concentration. I then interact an indicator denoting whether acquired firms where in a local labor market in the top quartile of HHI at the time of acquisition. Here, I find that the largest earnings and retention effects are at firms in highly concentrated markets. Though somewhat counter-intuitive, the fact that the largest earnings effects occur in such markets is consistent with the idea that managerial discretion is most likely to shape firm pay when firms have market power. In contrast, labor markets featuring greater competition for workers may apply greater discipline to firms to set wages. Additionally, these relative effects again suggest smaller labor supply elasticities to the firm in concentrated markets.

Figure B.1: LEHD Sample Coverage



Notes: Figure B.1 shows the U.S. states comprising the LEHD analysis throughout the paper. The included states are: Arizona, Arkansas, California, Colorado, Delaware, Florida, Illinois, Indiana, Iowa, Kansas, Maine, Maryland, Montana, Nevada, New Mexico, Oklahoma, Oregon, Pennsylvania, Tennessee, Washington, and West Virginia.

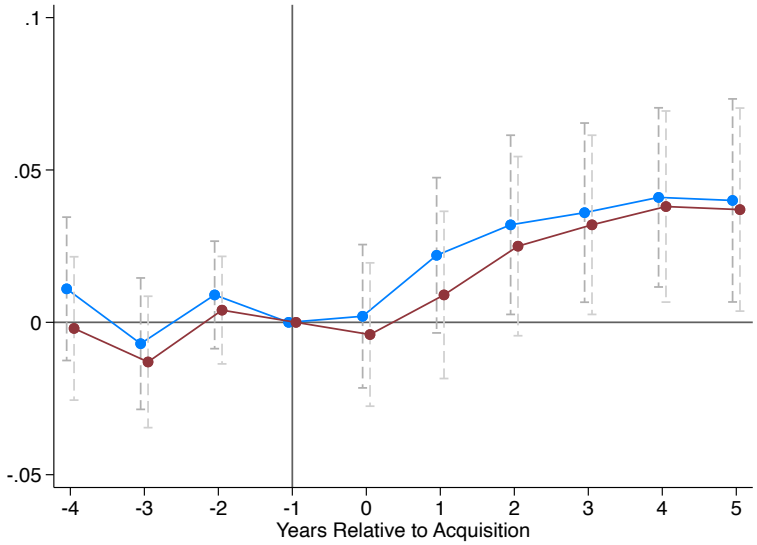
Figure B.2: Effects of Managers' URE: Continuous Treatment



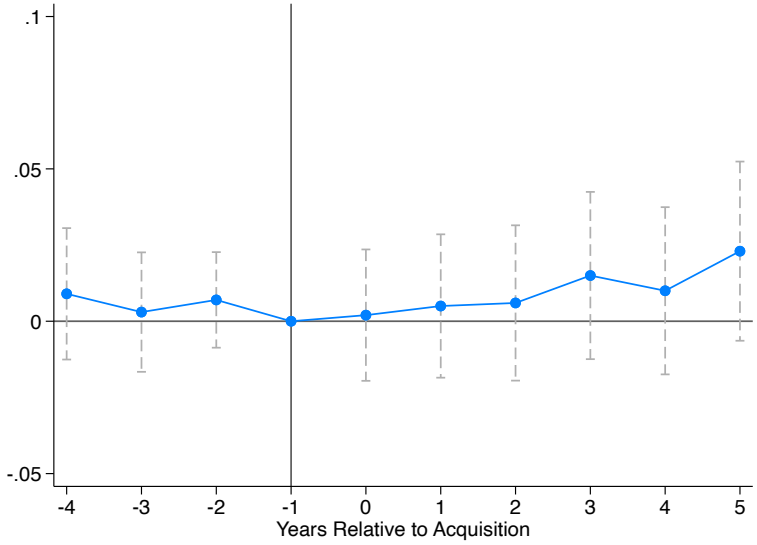
Notes: Figure B.2 plots event-study coefficient estimates of the differential effects of firm ownership changes on average worker earnings and employment using a continuous treatment definition. Event-study coefficients correspond to the average linear effect associated with moving from the mean URE of acquiring managers in the "Low Exp." group in Table 2 to the mean URE of acquiring managers in the "High Exp." group. *Source:* Authors' calculations based on LEHD and SBO data.

Figure B.3: Effects of Managers' URE on Firm Mean Worker Earnings after Ownership Transfer: Alternative Treatments

(A) Log Mean Earnings: Alternative Calibrations

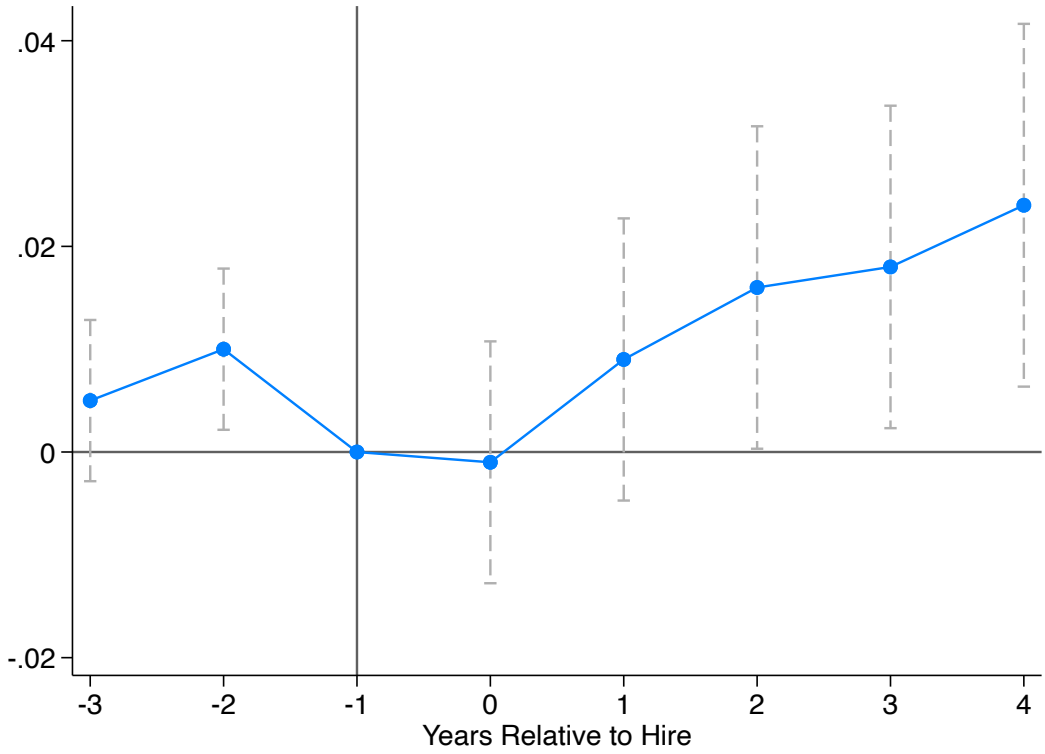


(B) Log Mean Earnings: Formative Experiences



Notes: Figure B.3 plots event-study coefficient estimates of the differential effects of firm ownership changes on average worker earnings across acquiring owner URE using alternative treatment definitions. Panel (A) plots versions that calibrate $\lambda = 1$ (blue) and $\lambda = 3$ (red), respectively. Panel B only considers average URE during owner ages 18 to 25. *Source:* Authors' calculations based on LEHD and SBO data.

Figure B.4: Effects of New Hires on Log Worker Earnings: Founder Firms



Notes: Figure B.4 reports event-study coefficient estimates of the differential earnings effects of job-to-job transitions of workers hired at founder firms across owner URE. *Source:* Authors' calculations based on LEHD, SBO, and BLS data.

Table B.1: Mean Earnings Heterogeneity by Firm Growth Characteristics

	(1) Large Firm	(2) Emp. Decline	(3) Emp. Boom
HighExp	0.034	0.044	0.032
	(0.010)	(0.003)	(0.004)
	[0.022]	[0.001]	[0.011]
Int. × HighExp	-0.003	-0.041	0.026
	(7.495)	(0.082)	(1.159)
	[0.904]	[0.126]	[0.603]
Base + Int. FE	✓	✓	✓
N	24,500	24,500	24,500

Table B.1 reports difference-in-differences coefficient estimates corresponding to my firm-level event study designs, with treatment variables interacted with the specified firm characteristics. I define large firms as those in the top quartile of firm employment at the time of acquisition, "decline" firms as those that experience at least a 25% decline in employment over the five year post-acquisition period, and "boom" firms as those that experience employment growth of at least 100% over the same time period. *Source:* Authors' calculations based on LEHD and SBO data.

Table B.2: Heterogeneity in Incumbent Outcomes by Worker Characteristics

	(1) Log(Earn)	(2) Tenure
	Panel A: Age	
HighExp	-0.003	-0.007
	(0.018)	(0.011)
	[0.868]	[0.519]
Age44 × HighExp	0.044	0.015
	(0.021)	(0.009)
	[0.032]	[0.097]
	Panel B: Education	
HighExp	0.015	0.002
	(0.013)	(0.009)
	[0.244]	[0.824]
College × HighExp	0.040	0.001
	(0.015)	(0.006)
	[0.007]	[0.874]
Observations	332,000	332,000

Table B.2 reports difference-in-differences estimates of the differential effects of firm ownership changes on incumbent worker earnings and retention probabilities, with treatment indicators interacted with the specified worker demographic characteristics. *Source:* Authors' calculations based on LEHD and SBO data.

Table B.3: Mean Earnings Heterogeneity by Prior Firm Job Turnover

	(1) Log(Emp)	(2) Log(Earn)
HighExp	-0.034	0.009
	(0.0237)	(0.0113)
	[0.152]	[0.574]
$\text{Turn}_{High}\times$ HighExp	0.039	0.053
	(0.0333)	(0.0228)
	[0.242]	[0.020]
Observations	24,500	24,500

Table B.1 reports difference-in-differences coefficient estimates corresponding to my firm-level event study designs, with treatment variables interacted with the indicators for whether average job turnover at the firm in event time t-4 to t-1 was above the sample median. *Source:* Authors' calculations based on LEHD and SBO data.

Table B.4: Mean Earnings Heterogeneity by Prior Firm Job Turnover: Long Difference Effects

	(1) Log(Emp)	(2) Log(Earn)
HighExp_{t+5}	-0.022	0.019
	(0.0295)	(0.022)
	[0.456]	[0.387]
$\text{Turn}_{High} \times \text{HighExp}_{t+5}$	0.022	0.060
	(0.0258)	(0.0299)
	[0.607]	[0.045]
Observations	24,500	24,500

Table B.1 reports coefficient estimates of event study term t+5 corresponding to my firm-level event study designs, with treatment variables interacted with the indicators for whether average job turnover at the firm in event time t-4 to t-1 was above the sample median. *Source:* Authors' calculations based on LEHD and SBO data.

Table B.5: Heterogeneity in Incumbent Worker Outcomes by Local Labor Market Concentration

	(1) Log(Earn)	(2) Tenure
HighExp × Post	0.012	-0.002
	(0.013)	(0.008)
HHI × HighExp × Post	0.048**	0.010
	(0.022)	(0.014)
Observations	332,000	332,000

Table B.5 reports difference-in-differences estimates of the differential effects of firm ownership changes on incumbent worker earnings and retention probabilities, with treatment indicators interacted with local labor market HHI in the year of acquisition. *Source:* Authors' calculations based on LEHD and SBO data.

Table B.6: Rent-Sharing Elasticity Estimates: 2008-2013

	(1) OLS	(2) IV
ξ_{LowExp}	0.059***	0.581***
	(0.014)	(0.177)
$\xi_{HighExp}$	0.078***	0.263*
	(0.011)	(0.142)
p[$\xi_{HighExp}$ - ξ_{LowExp}]	0.283	0.045
Weak IV F-stat		136.7
Observations	223,000	223,000

Table B.6 reports OLS and IV estimates of equation A.1 over the 2008-2013 period. *Source:* Authors' calculations based on LEHD and SBO data.