The Labor Market Consequences of Acquisitions

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Abstract

We study the effects of corporate acquisitions on workers using Swedish administrative data and document substantial, persistent earnings losses following acquisitions. These losses reflect both displacement and wage cuts among stayers from target firms. We find no evidence that increased monopsony power accounts for these wage cuts. Instead, they are concentrated in acquisitions where the acquiring-firm CEO sat on the board of the target prior to the transaction. Such acquisitions increase acquiring-firm profits and CEO pay, without affecting total employment or revenue, consistent with rent redistribution. Overall, acquisitions reduce wages and disrupt employment, with profit gains partly extracted from workers.

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1 Introduction

When one firm acquires another, the effects may extend beyond shareholders to consumers and workers. Traditional antitrust policy centers on consumer welfare, guarding against the risk that acquirers restrict output and raise prices. By contrast, the impact on workers is theoretically ambiguous, and the empirical literature offers limited consensus.[1] On the one hand, wages may rise if acquisitions improve profitability or efficiency and firms share these gains with employees. On the other hand, the gains from acquisitions may come at labor's expense—either through lower wages, potentially due to increased market power in the labor market, or through layoffs tied to post-acquisition restructuring.

In this paper, we provide new evidence on the effects of corporate acquisitions on workers' earnings and employment. Using comprehensive administrative data on Swedish workers, firms, and their acquisition activity, we document substantial earnings losses following acquisitions. These losses reflect both layoffs and pay cuts for incumbent workers. We do not find evidence that these pay cuts stem from acquisition-induced increases in monopsony power. Instead, they occur in acquisitions in which the acquirer CEO sits on the board of the target firm pre-acquisition. In such acquisitions, acquiring-firm profits and CEO pay increase, while total revenue and employment remain constant, suggesting that these CEOs possess critical information about the target firm that enables a redistribution of rents away from workers towards shareholders and managers.

To estimate the impact of acquisitions, we use a stacked difference-in-differences design, comparing workers and firms who experience an acquisition at a given point in time to units who experience such an event a few years later. By exploiting differences in event timing, we can account for unobservable differences between acquiring and target firms—and their workers—and firms that never acquire (Fadlon and Nielsen, 2021; Nekoei and Seim, 2023). This approach restricts the control group to not-yet-treated units, thereby avoiding "forbidden comparisons" (Borusyak et al., 2024).

Corporate acquisitions lead to substantial and persistent earnings losses: In the five years following the event, average earnings are 5 percent lower. These earnings losses reflect both wage cuts and layoffs. Among incumbent workers who remain at the acquiring firm, the decline is driven almost entirely by pay cuts for target-firm workers, whose earnings fall by about 3 percent. Employment effects are also sizable: five years post-acquisition, workers are 4 percentage points less likely to be employed in the acquiring firm, largely reflecting layoffs among target-firm employees. These acquisitions thus account for a large share of displacement in the economy, and the resulting earnings losses—around 15 percent for separating workers—closely mirror those documented in the literature on mass layoffs (Jacobson et al., 1993; Seim, 2019; Lachowska et al., 2020; Schmieder et al., 2023).

[1] Recent studies on the effects of mergers and acquisitions on workers include Arnold (2021), Arnold et al. (2024), and Lagaras (2024). For a broader overview, see citations therein.

Decomposing the earnings losses—accounting for both within-group average effects and relative group sizes—we find that 17 percent can be attributed to wage cuts among incumbent workers. The remaining losses reflect displacement: 66 percent from non-employment and 17 percent from transitions to other firms. The new employers offer lower pay premia, are smaller and less profitable, and provide lower overall job quality than the firms workers leave, implying that the displacement effect in part reflects transitions to lower-quality jobs.[2]

Our findings provide little support for an efficiency-enhancing channel of acquisitions. Although acquiring firms initially expand in employment and revenue as they absorb the target, we find that they subsequently contract as they scale down output and lay off workers. Overall, the combined employment at both firms falls by about 30 percent post-acquisition, and revenue by about 40 percent. Moreover, acquirers do not experience profit gains on average, and profits for the combined entity decline post-acquisition.[3]

Our results are not driven by acquisitions of poorly performing target firms that would likely have ceased operations absent the acquisition. To show this, we estimate both worker and firm-level effects of acquisitions depending on the growth trajectories of target firms in the years preceding acquisitions. We find similar and statistically indistinguishable effects across target firms on upward versus downward trajectories.

Why do earnings of stayers decline following acquisitions? A natural hypothesis is that acquisitions *increase* firms' labor market power, enabling them to reduce wages.[4] Acquisitions may increase firms' labor market power by reducing the number of labor market competitors or narrowing workers' set of outside options (Berger et al., 2022; Jarosch et al., 2024). However, we do not find evidence in support of this hypothesis in our setting, in contrast with recent evidence from the U.S. (Arnold, 2021; Prager and Schmitt, 2021). First, the estimated wage reductions are concentrated among target-firm workers, with no effect on the wages of acquirer workers, even within the same local labor markets. Second, firms rarely acquire their direct labor market competitors. And even when they do, effects are similar. Third, earnings declines are not larger in acquisitions predicted to increase local labor market concentration, regardless of how concentration is defined. Fourth, we find that acquisitions have no effect on the wages of new hires, suggesting that the observed wage effects are not driven by potentially narrower outside options induced by acquisitions.

An alternative hypothesis is that acquisitions trigger changes in firms' pay policies, even without altering their degree of monopsony power. To the extent that rents exist, potentially due

[2]We do not find evidence that worker-firm specific match effects drive these results, in contrast with recent evidence from Canada (Arnold et al., 2024).

[3]The effects on firm performance are consistent with a large literature in corporate finance and industrial organization on the impact of mergers and acquisitions on revenue and profits (e.g. Schoar, 2002; Davis et al., 2014; Malmendier et al., 2018). Possible motivations include strategic killer acquisitions—aimed at preserving long-run market power (Cunningham et al., 2021), or managerial motives such as empire building (Jensen, 1986) or overconfidence (Malmendier and Tate, 2005).

[4]For a recent discussion on the role that mergers and acquisitions (M&As) may play in suppressing wages through increases in labor market concentration, see, e.g., Marinescu and Posner (2019); Marinescu and Hovenkamp (2019)

to the presence of labor or product market power, an acquisition-induced change in how rents are shared between firms and workers can reduce wages and increase profits.[5] As emphasized by Shleifer and Summers (1988), acquisitions may be situations where a new manager is able to overcome the costs of lowering wages—such as reputation and trust. Such wage cuts can occur, for example, when new managers renege on implicit contracts between previous managers and the workers. Importantly, this relies on the new manager having information about the target-firm's wage policy and the potential room for lowering wages (Cai and Sevilir, 2012; He and le Maire, 2022).

To investigate this hypothesis, we identify acquisitions where the acquiring-firm CEO or a top manager was on the board of the target firm prior to the acquisition and compare these events to other acquisitions. These cases are of particular interest because the acquiring manager is likely to possess detailed information about the target firm, including its wage policies. One motivation for the acquisition may therefore be to take over firms where wages can be reduced to raise profits. Consistent with this, we first show that CEOs are more likely to acquire firms on whose boards they sit when those firms pay a high wage premium. These acquisitions account for the entirety of the wage reductions we find among target-firm workers. By contrast, acquisitions without prior board connections have no effect on the wages of stayers.

Acquisitions involving prior board connections also have important implications for firm performance and CEO compensation. Such acquisitions do not significantly alter the scale of the combined firm: total revenue and employment remain broadly unchanged. Instead, they are associated with increased CEO pay and higher acquiring-firm profits. We estimate that lower wages of target workers can explain about one third of the profit gains. These results suggest that such acquisitions redistribute rents, shifting them from target-firm workers to shareholders and CEOs.

One set of alternative mechanisms for reduced stayers' earnings is increased monopoly power or scale economies. These mechanisms could reduce labor demand and lower wages as firms move down their labor supply curve. Two pieces of evidence suggest they are unlikely to explain our findings for common-manager acquisitions. First, unlike acquisitions without common managers, such acquisitions show no decline in the size of the combined firm. Second, we find no increase in sales or profits per worker post-acquisition, regardless of whether common managers are involved. Explanations based on increased product-market power or scale economies would typically be accompanied by observable changes in output or productivity proxies at the combined firm level, which we do not find.

Our findings have implications for antitrust policy. Conventional arguments for mergers and acquisitions emphasize efficiency gains, with productivity improvements generating higher profits and potential benefits for workers through rent sharing (Williamson, 1968; Asker and

[5] Earlier work has documented evidence of rent-sharing, showing that workers in industries and firms with higher profits receive higher wages (Blanchflower et al., 1996; Van Reenen, 1996; Hildreth and Oswald, 1997; Kline et al., 2019).

Nocke, 2021). In contrast, we find no evidence of productivity increases in Sweden. If such gains were present, at least some workers would be expected to share in the benefits. Instead, acquisitions appear to be events marked by layoffs and wage cuts, with profits reallocated from workers to shareholders and CEOs.

Related Literature Our paper connects to three main bodies of research. First, we contribute to the literature evaluating the labor market consequences of mergers and acquisitions (M&As) (Brown and Medoff, 1988; Siegel and Simons, 2010; Dessaint et al., 2017; Ouimet and Zarutskie, 2020). In particular, Lagaras (2024) and Arnold et al. (2024) document large earnings losses in Brazil and Canada, driven by worker displacement, and negative effects on firm performance, while Arnold (2021) and Prager and Schmitt (2021) estimate that merger-induced increases in labor market power lead to wage reductions among workers in U.S. firms and hospitals. In contrast to these studies, we estimate wage reductions only among target firm workers who remain employed by the acquirer. We find no evidence that increased monopsony power explains these wage cuts.

Second, we contribute to a literature on the role of managers in shaping firm policies and influencing their outcomes (e.g. Bertrand and Schoar, 2003). Recently, Acemoglu et al. (2023) and He and le Maire (2022) have studied the role of managers in shaping firms' pay policies, documenting the role of manager-specific pay premiums that transfer between firms and how managers with business education change firm rent-sharing practices. In line with this work, we find that managers play a crucial role in acquisitions.

Our paper connects to the literature on common ownership and board interlocks (Mizruchi, 1996; Haunschild and Beckman, 1998)—where firms share at least one common board member—on the likelihood and success of mergers and acquisitions (e.g. Renneboog and Zhao, 2014; de Sousa Barros et al., 2020). Notably, Cai and Sevilir (2012) find higher announcement returns for acquiring firms in M&As where acquiring and target firms share a common board member, possibly due to better information acquisition. Azar (2012) documents that firms with overlapping institutional investors are more likely to share directors, showing that board interlocks are systematically related to shared ownership. Complementing work showing that common ownership can soften product-market competition (Azar et al., 2018), our findings underscore the importance of common managers in shaping post-acquisition wage declines.

The remainder of our paper is organized as follows. In Section 2, we describe our data and estimation samples. Section 3 outlines our empirical strategy. Section 4 presents the worker- and firm-level results and explores underlying mechanisms. Section 5 concludes.

2 Empirical Setting and Data

Our sample covers Swedish firms and workers from 1997 to 2019 and draws on several linked administrative data sources. We merge these data with data on corporate acquisitions from the

Serrano database which covers all acquisitions registered at the Swedish Companies Registration Office (*Bolagsverket*). All acquisitions are complete takeovers where the acquirer absorbs the target, which ceases to exist as a separate legal entity. We construct two estimation samples: one focused on worker outcomes and the other on firm outcomes.

Worker Sample. We merge employer-employee data with other administrative sources (see Appendix Section A1). We keep the main employment of all workers in prime working age between 23 and 60 who work at firms that, on average, have at least 5 employees over our sample period. We assign workers to firms based on their employment in the year before the acquisition and restrict the sample to acquisitions where both firms appear in the employer-employee data. If a worker experiences more than one acquisition, we keep one of the events at random.

Firm Sample. In our firm sample, we merge firms' employment, measured by the number of employed workers according to the employer-employee data, with balance sheet information for all non-financial, limited liability companies in Sweden. All our results on acquiring firms' outcomes and on the joint employment of acquirer and target firms refer to this sample. Results on joint revenues and profits of acquiring and target firms are based on a subsample where both firms are observed in the employer-employee data and the balance sheet data.[6] If an acquirer has multiple acquisitions, we keep one at random. We balance our sample by ensuring that the acquiring firm is observed throughout the full event window surrounding the acquisition.

We complement the firm sample with data on board members and CEOs from the Serrano data, linked to the administrative data. We define firms as having a common manager if, in the year before the acquisition, an executive employee of the acquiring firm sits on the target's board.

Empirical Setting. Between 1997 and 2019, there were 37,624 corporate acquisitions. Over this period, roughly 23 percent of Swedish workers experienced at least one acquisition. Restricting the sample to acquisitions in which both acquiring and target firms employed at least five workers in Sweden results in 6,147 events. Around 11 percent of workers were affected by at least one such acquisition. To ensure that we can follow workers and firms over a sufficiently wide window before and after the acquisition, we restrict our samples to acquisitions between 2002 and 2012. Our worker sample includes 3,521 acquisitions, and the firm sample 1,692 acquisitions. For further descriptions, see Appendix Section A2.

[6] Target firms are less likely to be covered in the balance sheet data. Therefore, the sample for joint outcomes is smaller than the one covering only acquiring firms.

3 Empirical Strategy

We use a difference-in-differences (DiD) design around acquisitions to study effects on workers and firms. Because workers and firms (units) involved in acquisitions may differ from non-acquiring units in both levels and, more importantly, trends of outcomes, we compare units that undergo acquisitions to units that will do so but have not yet experienced one. This design exploits the *timing* of acquisitions as the event (Fadlon and Nielsen, 2021; Nekoei and Seim, 2023). Specifically, we compare units treated in year t to those treated in $t + \delta$. Thereby, control units will be not-yet-treated during the event window, avoiding "forbidden comparisons" (Borusyak et al., 2024).

We estimate the following event-study specification:

$$y_{it} = \sum_{k \neq -1} \beta_k \times \mathbb{1}\{s = k\} \times D_i + \gamma_i + \gamma_{s \times c} + \varepsilon_{it}, \tag{1}$$

where y_{it} is the outcome of interest for observation i in year t, where i refers to a unit in a given treatment year cohort c since the same unit can appear in both treatment and control group. $\mathbb{1}s = k$ equals 1 if event time $s = k$, D_i is a treatment group indicator, and γ_i and $\gamma_{s \times c}$ are unit-by-cohort and event-time-by-cohort fixed-effects. The coefficients β_k capture the difference between treatment and control group in event year k relative to the baseline year before the event, $s = -1$. In our baseline, we set $\delta = 7$ and follow units for five years after acquisitions. In order to evaluate the robustness of our results to this choice, we also present results where we vary δ. Standard errors are two-way clustered at the worker and event level for worker outcomes and at the firm level for firm outcomes. To account for differences in the life cycle of workers and firms, we reweight control units to match the distribution of treated units. In the worker sample, we reweight on 10 age bins interacted with gender and educational degree, measured in period $s = -5$. In the firm sample, we reweight on 2-digit industry and 20 employment-size bins, also measured in $s = -5$.

The identifying assumption is that treated units would have followed the same trend as not-yet-treated units absent acquisition. To evaluate this assumption, we plot the pre-event coefficients, β_k, in our event-study graphs. Across a wide range of outcomes for both workers and firms, we find no evidence of differential pre-trends. A further implication is that acquirers do not systematically purchase firms that would otherwise fail. To examine this, we split the sample by whether the target firm exhibited an upward or downward trend in the years preceding the acquisition. Reassuringly, the results for both workers and firms are very similar across the two groups, supporting the validity of our design.

When analyzing multiple outcomes, we report average post-event effects estimated from the following static DiD specification

$$y_{it} = \beta \times P_t \times D_i + \gamma_i + \gamma_{s \times c} + \varepsilon_{it} \tag{2}$$

where P_t is an indicator for post-acquisition periods. β captures the average difference between treated and control units in the five years following the acquisition.

4 Effect of Acquisitions on Workers and Firms

4.1 Effect on Workers

In this section, we examine the effects of acquisitions on the labor market outcomes of incumbent workers at acquiring and target firms.

Effect on labor income. Figure 1a shows that labor incomes of affected workers fall relative to that of control workers by an average of 4.9 percent over five years. The losses are substantially larger for workers at target firms than for those at acquiring firms (7 percent vs. 4 percent). To illustrate the empirical design, Appendix Figure A5.4 plots average incomes of the treatment and control groups before and after acquisition. The figure shows that income growth among the treated group lags behind that of the control group following the acquisition.[7]

The decline may reflect the intensive margin (lower wages growth) and the extensive margin (reduced employment). Figure 1b shows that income declines are concentrated among target-firm workers, whose earnings fall by 3.8 percent relative to matched controls five years after the transaction. By contrast, we find little evidence of pay reductions among acquiring-firm stayers.

The decline in income also reflects increased separations from the acquiring firm. In Appendix Figure A5.7a, we show that, for acquiring-firm workers, the likelihood of remaining employed at the acquiring firm drops by 1.5 percent on average, though the effect is not statistically distinguishable from zero. In contrast, the effect for target-firm workers is substantially larger, at 10.4 percent. While acquirer workers do not separate at higher rates than controls, income losses upon separation are large. Appendix Figure A5.7b reports the effect on income among workers who switch employers post-acquisition, revealing an average labor income loss of approximately 5 percent among both acquirer and target workers.[8] In Appendix Section A4 we show that these declines in labor income are driven by switches to lower-quality jobs while worker-firm match effects do not play an important role.

Overall, these findings indicate that corporate acquisitions negatively impact labor income trajectories of incumbent workers, with the adverse effects borne predominantly by those at target firms.

How do the labor income losses from acquisition-induced job displacement compare to estimates from the mass layoff literature (Jacobson et al., 1993)? In Figure 1c, we follow workers

[7] We also find negative effects of acquisitions on both hourly wages and working hours for those workers who we observe in the Structure of Earnings dataset (Appendix Figure A5.8). The decline in overall labor income is somewhat smaller in this sample than in our main estimation sample (Appendix Table A6.2).

[8] For further analysis of heterogeneity in effects, see Appendix Section A3.

who separate from the acquiring firm at different points in time—either entering unemployment or transitioning to another firm—and examine their labor income losses. Like stayers, separating workers experience gradual relative income declines before separation. In the year of the separation, workers incur a large drop in income of about 10 percent to 17 percent. The magnitude of these losses is comparable to, though at the upper end of, estimates from the literature on mass layoffs in Sweden (e.g. Seim, 2019; Athey et al., 2024), other European countries (e.g. Bertheau et al., 2023), and the US (e.g. Jacobson et al., 1993). Unlike the mass layoff literature, losses remain stable throughout our sample, with no recovery.

Robustness. To evaluate the robustness of our estimated effects on income, we estimate the difference-in-differences regression varying the choice of our control group. Specifically, we vary the number of years (δ) until control workers experience an acquisition. This naturally affects the number of post-event periods for which we can estimate effects. As Appendix Figure A5.5 documents, the estimates remain robust to the choice of control group. In Appendix Figure A5.6, we split the sample by whether the target firm experienced growth in employment or profits in the years preceding the acquisition. The estimates are similar and statistically indistinguishable across target firms on upward versus downward trajectories. This provides strong evidence that our results are not driven by acquisitions of firms that would otherwise have gone bankrupt.

Decomposing the average effects. Our baseline estimate of average labor income decline post-acquisition reflects different margins of adjustment—transitions into non-employment, to other employers, and income declines among stayers. To quantify the contributions of these margins, we implement the approach of Nekoei et al. (2024). We partition our sample into three disjoint groups $\pi_{it} = p$ for some $p \in P$. The groups are:

a. *Extensive margin*: workers who are not employed in a given year;

b. *Intensive margin - switchers*: employed workers who no longer work at the acquiring (or pre-acquisition target) firm in a given year;

c. *Intensive margin - stayers*: workers who remain employed at the acquiring (or pre-acquisition target) firm in a given year.

For each worker i with outcome y_{it} in year t, we assign a control outcome $y_{c(i)t}$, which is the average outcome of control workers in the respective treatment cohort, and generate

$$y_{it}^{p} = y_{it}\mathbb{1}\{\pi_{it} = p\} + y_{c(i)t}\mathbb{1}\{\pi_{it} \neq p\}. \tag{3}$$

We then decompose average treatment effects along the three margins, as

$$\frac{1}{N_t}\sum_{i} y_{it} - y_{c(i)t} = \sum_{p \in P} w_{pt}\bar{\Delta}_{pt} \tag{4}$$

where N_t is the number of observations, $w_{pt} = \frac{N_{pt}}{N_t}$ is the share of workers in partition p, and

$\bar{\Delta}_{pt} = \frac{1}{N_{pt}} \sum_{i:\pi_{it}=p} y_{it}^{p} - y_{c(i)t}$ is the average effect within each partition. This approach estimates each margin's contribution, capturing both within-partition effects and group shares.

We plot the results of this decomposition in Figure 1d. In the first year after the acquisition, the extensive margin accounts for nearly the entire decline in labor income. Over time, the intensive margin becomes more important. After five years, 66 percent of the overall effect can be attributed to the extensive margin, 17 percent to income losses among switchers, and 17 percent to lower pay of workers who stay at the acquiring firm.

4.2 Effects on Firm Outcomes

We next examine the effects of acquisitions on firms' employment, revenue, and profits. Employment and revenue capture changes in firm scale, while profits measure performance, potentially reflecting shifts in productivity, prices, or wages.

Figure 2 reports two sets of estimates.[9] First, we estimate effects on the acquiring firm, which absorbs the target firm during the acquisition. Second, we analyze combined outcomes for the acquiring and target firms, as targets cannot be tracked separately after the transaction.

Employment at the acquiring firm jumps by 37 percent in the acquisition year (Figure 2a), then declines to 18 percent above pre-acquisition levels. Combined employment (Figure 2b) falls steadily after the acquisition. Revenue shows a similar pattern: acquiring firm revenue rises by 20 percent initially (Figure 2c), but combined revenues drop sharply and remain 37 percent below baseline after five years (Figure 2d). These patterns imply that acquisitions expand the scale of the acquirer while overall activity contracts, consistent with consolidation and reduced aggregate firm output.

We find no evidence that, on average, acquisitions raise profits. Acquiring-firm profits fall slightly, with small and insignificant effects (Figure 2e), while combined profits decline more substantially (Figure 2f).[10] These patterns run counter to common claims that acquisitions generate efficiency gains through synergies, as we find no evidence of higher profitability for either acquiring or target firms in our Swedish setting.

Robustness. We perform robustness checks analogous to those in the worker sample. Appendix Figure A5.11 shows that the firm-level results are stable across alternative control groups when varying the number of years (δ) until control firms undergo an acquisition. Moreover, the effects on acquiring firms' outcomes are very similar whether target firms were on an upward or downward trajectory prior to the acquisition (Appendix Figure A5.12).

[9] Appendix Figures A5.10 display mean outcomes for treated and control firms, before and after the event.
[10] See Appendix Figure A5.13 for results on different measures of firm profitability.

4.3 Effect of Acquisitions on Pay: Monopsony Power vs. Rent-Sharing

A central finding of our analysis is that workers from the target firm who remain employed at the acquiring firm after an acquisition experience systematic pay cuts. In this section, we seek to understand the mechanisms behind this result. We focus on two possibilities. The first is that acquisitions increase concentration in local labor markets, enabling firms to suppress earnings through enhanced monopsony power (Marinescu and Hovenkamp, 2019; Marinescu and Posner, 2019). This channel is consistent with U.S. evidence showing that hospital mergers reduce wage growth for stayers (Prager and Schmitt, 2021) and that large mergers and acquisitions depress earnings at the labor-market level (Arnold, 2021). The second mechanism is that acquiring firms may have different wage-setting and rent-sharing policies than target firms, leading to systematic pay cuts for target-firm workers post-acquisition, holding constant firms' monopsony power.

Increased firm monopsony power. We use two complementary approaches to assess whether changes in monopsony power can account for the observed pay declines.

First, we examine whether acquiring firms typically purchase target firms that are direct competitors in the labor market. We measure direct competition as the share of worker transitions from the acquirer to the target in the seven years preceding the acquisition, relative to all workers who left the acquirer for another employer. In roughly 83 percent of acquisitions, we observe no such transitions. Appendix Figure A5.14 shows the distribution of transition shares for the remaining 17 percent. Figure 3a compares post-acquisition earnings effects for stayers in acquisitions with and without prior worker transitions. If wage cuts reflected a reduction in outside options, we would expect larger declines when both firms had competed for the same workers. Instead, the estimates are nearly identical.

Second, we turn to the local labor market level, defined as the intersection of a 3-digit occupation and a municipality. We restrict the sample to workers where the acquirer and target operate in the same local labor market and group them into tertiles based on the predicted increase in the acquirer's employment share, defined as the target firm's pre-acquisition share. If monopsony power were driving our worker effects, we would expect larger income declines in acquisitions generating greater increases in concentration (Azar et al., 2020; Jarosch et al., 2024). Figure 3b plots the effects on two main outcomes—employment and labor income conditional on employment—by tercile. We find no evidence that acquisitions leading to greater concentration produce larger employment or income losses. These results are robust to alternative definitions of local labor markets (Appendix Figure A5.15a). Appendix Figure A5.15b shows a similar null result when restricting to stayers, and Appendix Figure A5.16 shows no post-acquisition changes in the incomes of new hires at the acquiring firm, measured in levels or residualized by worker characteristics.

Product-market power and scale economies. An alternative interpretation is that post-acquisition wage declines reflect increased monopoly power or scale economies, which reduce labor de-

mand and move firms down their labor supply curve. These mechanisms imply changes in output and productivity at the combined firm level. However, acquisitions involving common managers show no evidence of expansion or productivity gains: the size of the combined firm does not increase (Figure 5c), and sales and profits per worker remain unchanged. In contrast, acquisitions without common managers exhibit a decline in sales per worker. The absence of output or productivity gains in common-manager acquisitions is difficult to reconcile with explanations based on increased market power or scale economies.

Decline in rent-sharing. An alternative explanation for the decline in target-firm stayers' earnings is that acquisitions change firms' pay policies—specifically, the extent to which profits are shared with workers—even absent changes in monopsony power. While firms may possess monopsony power, they may not be able to fully exploit it, e.g., when workers have some collective bargaining power (Wong, 2025). Therefore, acquisitions may affect worker earnings through changes in monopsony power or changes in the extent to which firms are able to exploit such power. We provide evidence consistent with the latter interpretation. Corporate acquisitions typically involve a change in upper management, creating an opportunity for new managers to reduce wages more readily than their predecessors, who may have faced relational costs from lowering pay for long-standing employees (Shleifer and Summers, 1988). Moreover, high labor costs may themselves be a motive for acquisitions: firms with relatively high labor costs may become attractive targets because acquirers anticipate profiting from reducing compensation. In this view, acquisitions serve not only to reorganize production but also to reshape the division of rents between firms and workers. Crucially, such post-acquisition wage reductions require that the new managers possess detailed knowledge of the target's operations, including its wage-setting practices and scope for cuts (Cai and Sevilir, 2012).

We operationalize this idea by studying cases when there are *board interlocks* between the acquirer and target before the acquisition. We identify cases in which the CEO or other top executives of the acquiring firm served on the target's board before the acquisition—what we refer to as common-manager acquisitions. Such interlocks plausibly provide acquiring-firm executives with insider knowledge about the target's pay policies, facilitating wage reductions after the acquisition. If rent redistribution is an important channel, we should expect larger earnings declines for target-firm stayers in acquisitions involving such interlocks (Mizruchi, 1996).[11]

We begin by documenting a positive correlation between the likelihood of a common-manager acquisition and residualized pay in the target firm, which we use to measure the target's pay policy, in Figure 4a.[12] This pattern suggests that CEOs may strategically target firms where

[11] See Appendix Figure A5.17 for differences in characteristics of acquisitions with and without common managers.

[12] We estimate residual pay by regressing log income on fixed effects for the interaction of calendar year, age bins, gender, region of residence, and educational degree, and then averaging the residual at the firm level in the year before the acquisition.

wages are above market levels, possibly with the intention of reducing labor costs. Consistent with this interpretation, we find that post-acquisition pay cuts of workers who remain employed at the acquiring firm after the acquisition occur predominantly in high-paying target firms (Figure 4b).

Figure 4c shows that earnings losses of about 3 percent for target-firm stayers arise only in acquisitions involving a common manager, while the effect for acquirer stayers is small and statistically insignificant; acquisitions without a common manager show no effect at all. This pattern is consistent with a rent-sharing interpretation, in which new management uses insider knowledge of the target's pay policies to reallocate rents away from workers. In line with models of implicit contracts and backloaded compensation (e.g. Lazear, 1979), we find that wage cuts fall disproportionately on older workers, while there is no evidence that they target employees with lower productivity, measured by residualized pay (Appendix Figure A5.18).

While workers who remain at the acquiring firm experience a loss in labor income, the opposite holds for the acquiring firm's CEO. Figure 5a shows CEO pay in acquisitions with and without a common manager. When the acquirer's CEO sits on the target's board, CEO pay increases by about 14 percent in the five years following the acquisition. In acquisitions without a common manager, CEO pay instead declines by roughly 27 percent.[13]

Firm outcomes mirror those of CEOs. Figure 5b compares revenue, employment, and profits in acquiring firms across the two types of acquisitions. In cases without a common manager, we find little change in scale, as measured by revenue and employment, but a decline in profits. By contrast, in acquisitions with a common manager, both scale and profits rise. Figure 5c shows similar patterns for combined acquirer–target outcomes: performance remains broadly stable with a common manager but deteriorates substantially otherwise.

To support a causal interpretation of the differences between common-manager acquisitions and those without a common manager, Appendix Figure A5.20 replicates the results in Figures 4c and 5, reweighting the non–common-manager sample to match the distribution of observable acquisition characteristics in the common-manager sample. The results are similar, indicating that the differences in outcomes are not driven by differences in observables.

The inverse relationship between the effects on workers' pay, on the one hand, and CEO earnings and firm profits, on the other, indicates a redistribution of rents from workers to executives and shareholders. Since overall employment and revenue remain roughly constant following common-manager acquisitions, much of the profit gain can be attributed to wage cuts. Back-of-the-envelope calculations suggest that wage cuts among target workers may account for up to 33 percent of the increase in acquiring-firm profits.[14]

[13] In Appendix Figure A5.19, we investigate the role of CEO characteristics and quality for firm-level outcomes, finding limited evidence that these characteristics substantially influence acquisition outcomes.

[14] The absolute increase in acquiring firms' profits after common-manager acquisitions is 556,000 SEK. Total payroll for target workers at the average firm in the year before the acquisition is 7.2 million SEK, with an average pay reduction of 2.56 percent. Given that overall employment stays constant (see Figure 5c), these reductions can account for 33 percent of the increase in profits, assuming no effects on productivity and prices. Taking the (insignificant) decline of acquiring workers' income into account, we can explain 55 percent of the increase in profits.

Taken together, common-manager acquisitions are characterized by (i) wage declines among stayers, (ii) no expansion in the combined firm size, and (iii) no clear productivity gains in the combined firm. This joint pattern is difficult to reconcile with scale or product-market power explanations but is consistent with a redistribution of rents from workers to CEOs and shareholders.

5 Conclusion

We study the consequences of corporate acquisitions for workers and find that they are far from neutral: earnings decline through both job displacement and wage cuts among stayers from target firms. These wage cuts do not reflect increased monopsony power. Instead, they are concentrated in acquisitions where the acquirer's CEO sat on the target's board prior to the transaction, suggesting insider knowledge of pay policies. Consistent with this, such CEOs disproportionately acquire high-paying firms and subsequently reduce wages. The result is rent redistribution from workers to shareholders and managers, as seen in higher profits and CEO pay. These findings imply that the labor-market risks of acquisitions arise not only from greater market concentration, as documented in previous work, but also from managerial incentives that conventional antitrust policies are ill-equipped to address.

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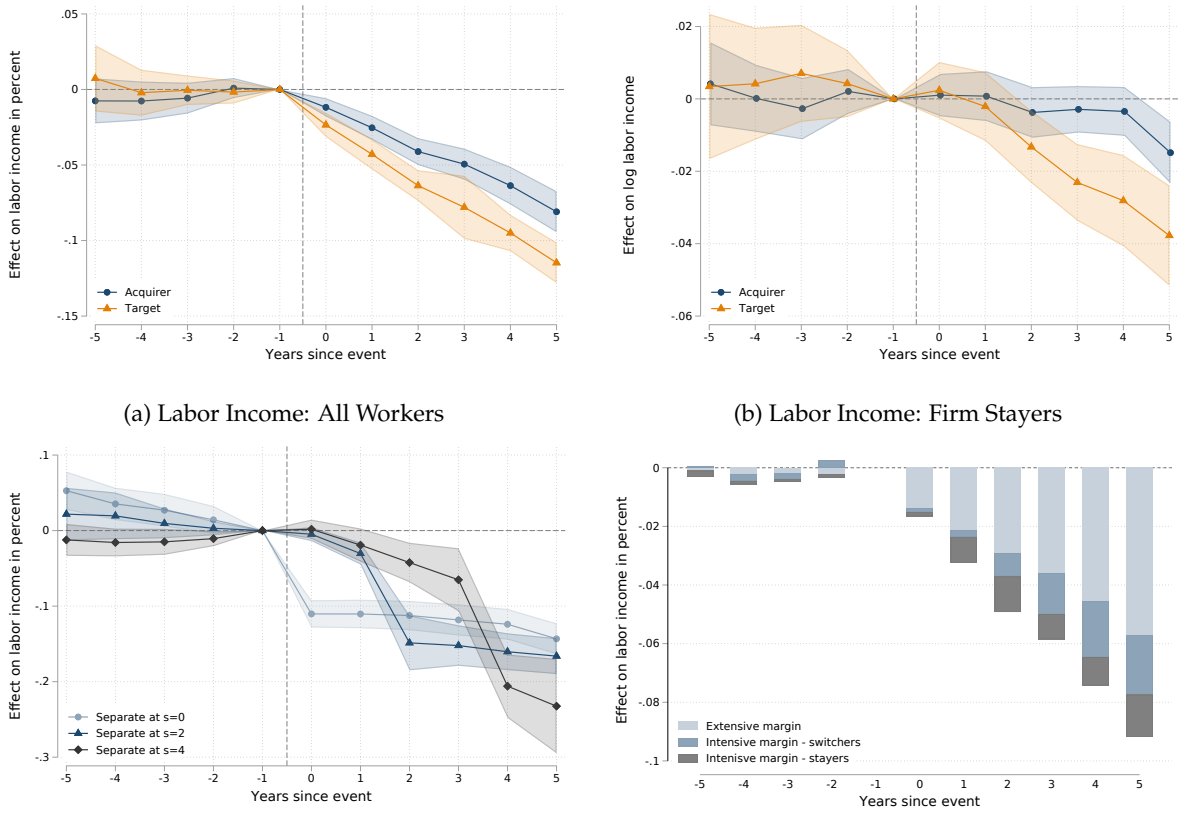
6 Figures

Figure 1: Effects of Acquisitions on Labor Income



(a) Labor Income: All Workers

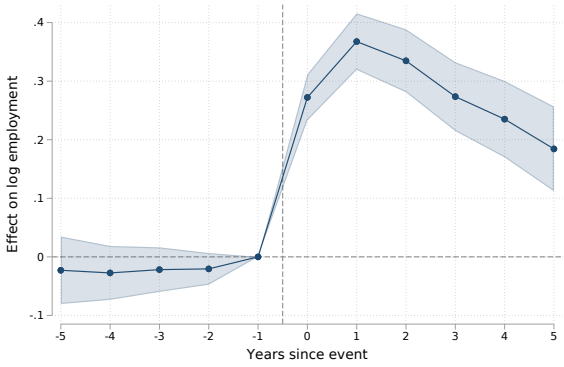
(b) Labor Income: Firm Stayers

(c) By Year of Separation

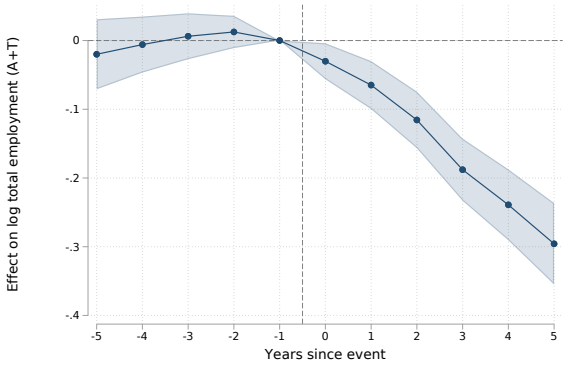
(d) By Margin of Adjustment

Notes: This figure shows the effects of acquisitions on labor market outcomes. Coefficient estimates are based on stacked DiD event studies according to specification (1), estimated in the worker sample. Control units are workers who experience an acquisition seven years after the treated units. We reweight control units to match the distribution of treatment units based on gender, age, and education, measured in period $s = -5$. **Panel (a)** shows the estimated effects on annual labor market income divided by average labor income of treated units in the year before the acquisition. We report estimates separately for workers who were employed at the acquirer firm in the year prior to the acquisition and those who were employed at the target firm. **Panel (b)** shows the effects on log annual labor income for those workers who remain at the acquiring firm (or, pre-acquisition, target firm) throughout the sample period. **Panel (c)** shows the relative effects of acquisitions on annual labor income for workers who separate from the acquiring firm into unemployment or transition to another firm. We split the sample based on the year of separation of treated workers. Shaded areas are 95% confidence intervals where standard errors are two-way clustered at the worker and event level. **Panel (d)** plots the dynamic decomposition of annual labor income relative to the year before the event according to equation (4). We partition the sample into workers who are not employed (extensive margin), workers who remain employed, but switch to other firms (intensive margin - switchers), and workers who remain employed at the acquiring (or pre-event target) firm (intensive margin - stayers). Employment is defined as having annual labor income exceeding 10,000 SEK in a given year.

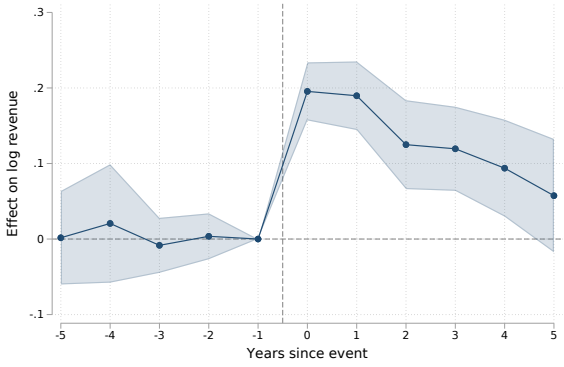
Figure 2: Effects of Acquisitions on Firm Outcomes



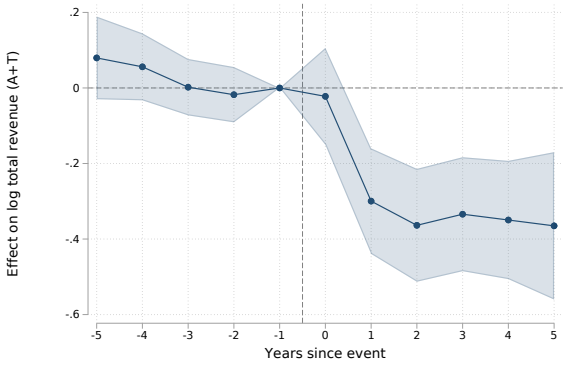
(a) Employment: Acquirer



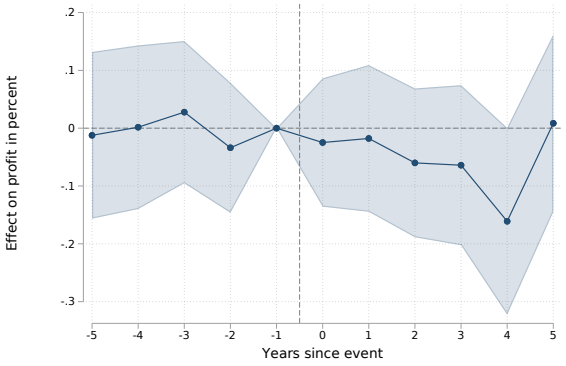
(b) Employment: Acquirer + Target



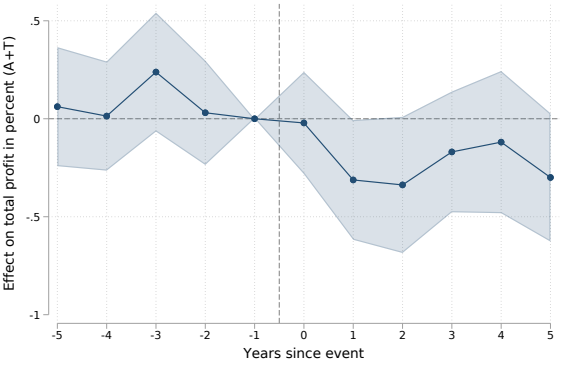
(c) Revenue: Acquirer



(d) Revenue: Acquirer + Target



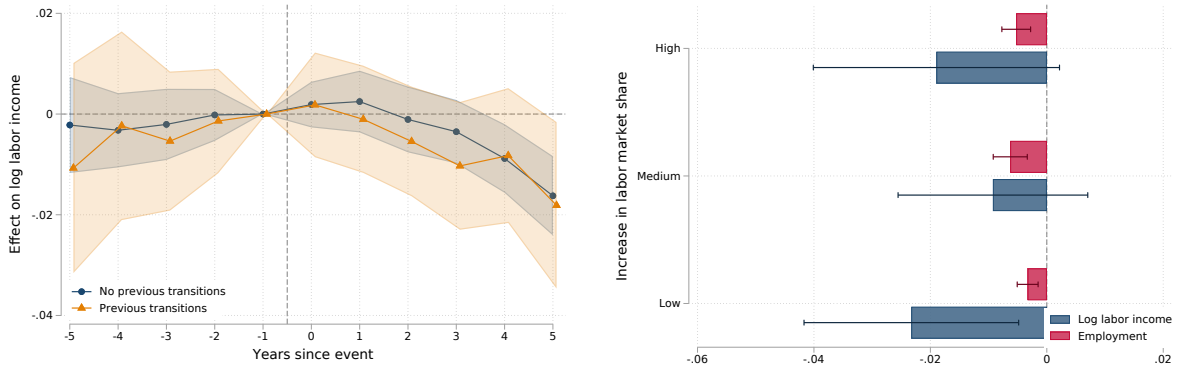
(e) Profit: Acquirer



(f) Profit: Acquirer + Target

Notes: This figure shows the effects of acquisitions on firm outcomes, where coefficient estimates are based on stacked DiD event studies according to specification (1), estimated in the firm sample. Control units are firms that undergo an acquisition seven years after the treated units. **Panel (a)** plots estimated effects on the acquiring firm's log employment in a given year, and **panel (b)** the same for joint employment of the acquiring and target firms. **Panel (c)** plots estimated effects on the acquiring firm's log revenue in a given year, and **panel (d)** the same for joint revenue of the acquiring and target firms. **Panel (e)** plots estimated effects on the acquiring firm's profit relative to average profits of treatment units in the year before the event, and **panel (f)** the same for joint profit of the acquiring and target firms. The effects on total revenue and total profit are estimates from the balance sheet firm sample in which we observe both acquiring and target firms' balance sheet information. We reweight control units to match the distribution of treatment units based on 2-digit industries and 20 employment bins, measured in period $s = -5$. Shaded areas are 95% confidence intervals where standard errors are clustered at the firm level.

Figure 3: Firm Monopsony Power and Worker Outcomes

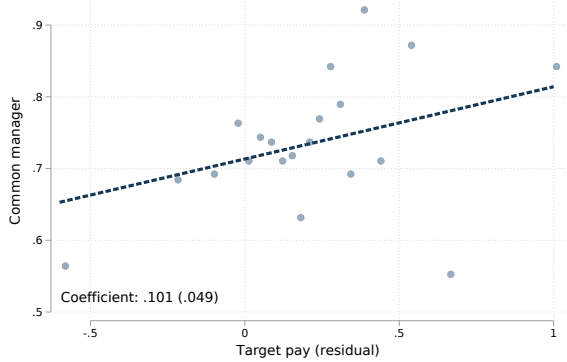


(a) Effects by Previous Worker Transitions

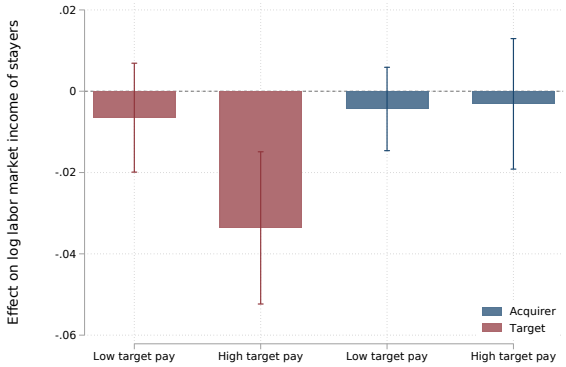
(b) Effects by Predicted Increase in Labor Market Share

Notes: **Panel (a)** shows the effects of acquisitions on labor income, where coefficient estimates are based on stacked DiD event studies according to specification (1), estimated in the worker sample. We restrict the sample to workers who remain at the acquiring firm after the acquisition and then split the sample into two subsamples. The first subsample contains 83% of events in which there were no worker transitions from the acquiring firm to the target firm in the seven years leading up to the acquisition. The second subsample contains the remaining 17% of events in which at least one worker transitioned from the acquiring firm to the target firm in the seven years leading up to the acquisition. **Panel (b)** shows the effects of acquisitions on labor market outcomes, where coefficient estimates are based on stacked DiD models according to specification (2), estimated in the worker sample. The outcomes are log annual labor income, conditional on employment, and an indicator for employment, defined as having annual labor income exceeding 10,000 SEK. The sample is split into tertiles by the predicted increase in the acquiring firm's employment share in the worker's labor market. We define this increase as the target firm's employment share in the respective labor market. A labor market is the intersection between one out of 290 municipalities and one out of 148 3-digit occupations. Control units in both panels are workers who experience an acquisition seven years after the treated units. We reweight control units to match the distribution of treatment units based on gender, age, and education, measured in period $s = -5$. Shaded areas and whiskers are 95% confidence intervals where standard errors are two-way clustered at the worker and event level.

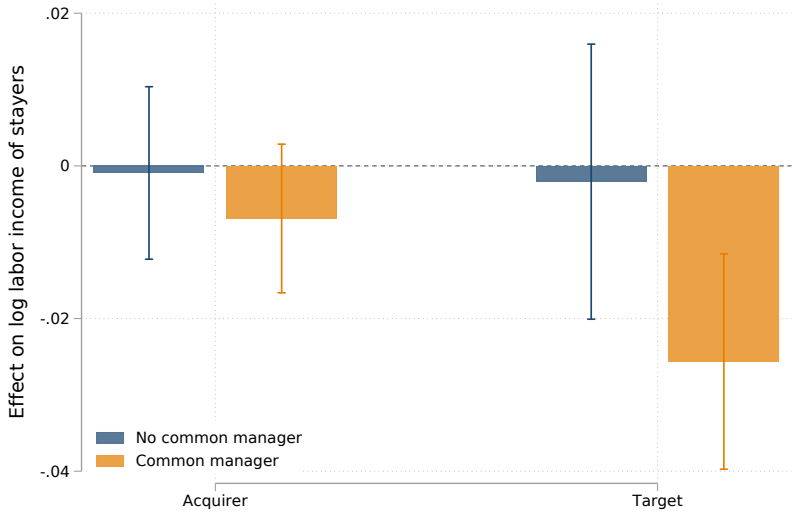
Figure 4: Common Managers and Target Firms' Pay Policies



(a) Target Pay and Common Managers



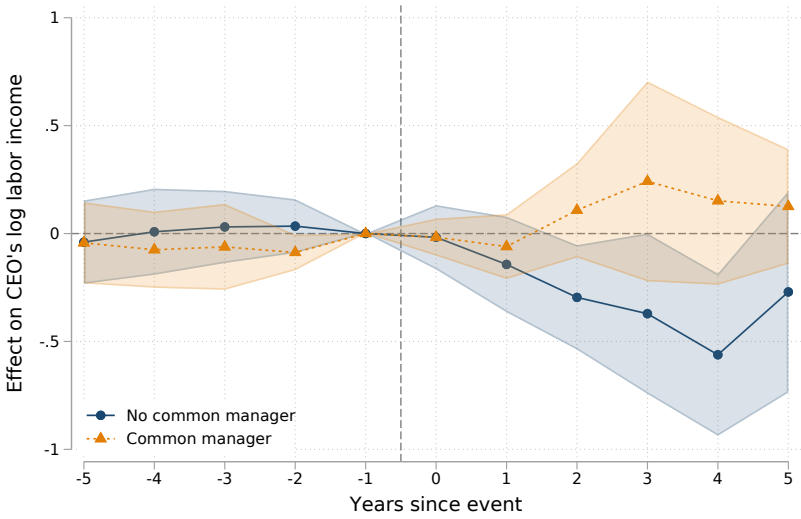
(b) By Initial Target Pay



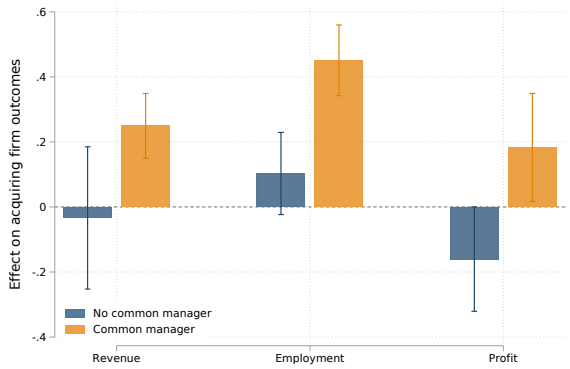
(c) Labor Income: Stayers by common manager

Notes: **Panel (a)** shows a binscatter of the correlation between the probability of an acquisition having a common manager and the target firms' initial pay policies. Common manager acquisitions are defined as events for which an acquiring firm manager was on the board of the target firm in the year before the acquisition. To evaluate pay policies, we regress log annual labor income in the full employer-employee data on fixed effects for the interaction of calendar year, 10 age bins, gender, region of residence, and educational degree to obtain residuals and compute the average residual by firm in the year prior to the acquisition. In **Panel (b)**, we split the sample of acquirer and target-workers, who remain at the acquiring firm after the event, into firms with above- and below-median average residuals and estimate the effects of acquisitions on log annual labor income. **Panel (c)** shows the effects of acquisitions on annual log labor market income of stayers based on whether the acquisition had a common manager or not. The estimates in the last two panels follow from a stacked DiD regression according to specification (2), estimated in the worker sample. Control units are workers that undergo an acquisition seven years after the treated units. We reweight control units to match the distribution of treatment units based on gender, age, and education, measured in period $s = -5$. Whiskers are 95% confidence intervals where standard errors are two-way clustered at the worker and event level.

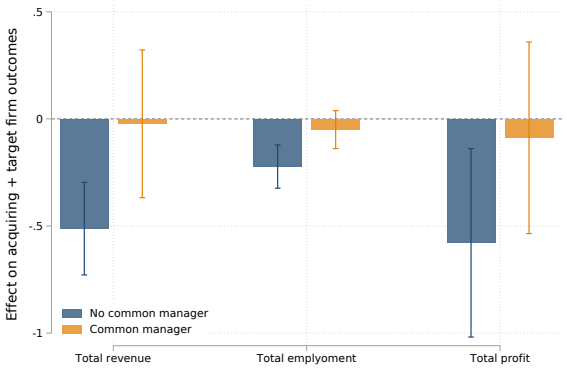
Figure 5: Effects of Acquisitions on Firm and CEO Outcomes by Common Manager



(a) Labor Income: Acquirer CEO



(b) Firm Outcomes: Acquirer



(c) Firm Outcomes: Acquirer + Target

Notes: This figure shows the effects of acquisitions on firm and CEO outcomes. Control units are units that experience an acquisition seven years after the treated units. In all panels, we split the sample based on whether an acquiring firm manager was on the board of the target firm in the year before the acquisition. **Panel (a)** shows the effects of acquisitions on the acquiring firm CEO's annual log labor income. CEOs are defined as the acquiring firm's CEO in the year before the acquisition. Coefficient estimates are based on stacked DiD models according to specification (2), estimated in the firm sample. The average effect is 14% (p-val. 0.16) for events with common managers and -27% (p-val. 0.01) for events without common managers. **Panel (c)** shows the effects of acquisitions on the following outcomes of the acquiring firm: log revenue, log employment, and profit, divided by the average profit of treated firms in the year before the acquisition. **Panel (d)** does the same, but shows the effects on joint acquirer and target outcomes. The effect on total employment is estimated from the main firm sample and the effects on total revenue and total profit from the balance sheet firm sample in which we observe both acquiring and target firms' balance sheet information. Coefficient estimates are based on stacked DiD models according to specification (2), estimated in the firm sample. We reweight control units to match the distribution of treatment units based on 2-digit industries and 20 employment bins, measured in period $s = -5$. Shaded areas and whiskers are 95% confidence intervals where standard errors are two-way clustered at the firm level.

Online Appendix

The Labor Market Consequences of Acquisitions

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February 24, 2026

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A1 Data Sources

Worker Sample. Information on labor income and workers' employment as well as the geographic location and industry of the workplace are derived from the matched employer-employee data (RAMS). Industries follow the Swedish Standard Industrial classification (SNI). We link these to demographic information on age, gender, and education from the Longitudinal Integrated Database for Health Insurance and Labor Market Studies (LISA), covering the full adult population of Swedish residents. We merge this sample with data on full-time equivalent monthly wages, contracted working hours, and occupations from the Structure of Earnings Survey (SES). The SES covers the full set of public sector employees in Sweden and a random subsample of around 50 percent of the private sector workforce, stratified by industry and firm size, and is typically collected in September - November every year. Labor income and wages are reported in 2018 Swedish Krona (SEK). The main employment of a worker is defined as the worker-firm spell with the highest earnings in a given year. Employment status is defined as having annual labor income exceeding 10,000 SEK. Occupation codes follow Statistics Sweden's (SCB) 4-digit SSYK classification. In a robustness check, we define local labor markets based on educational degrees. These degrees follow SCB's SUN group classification and contain 97 different categories, combining both vertical (e.g. vocational school vs. university) and horizontal (e.g. STEM vs. business) aspects of degrees.

Firm Sample. In our firm sample, we merge firms' employment, measured by the number of employed workers according to the employer-employee data, with information on revenues and operating profits from the Structural Business Statistics (FEK), which contain annual balance sheet data for all non-financial, limited liability companies in Sweden since 1997. Operating profit equals net sales plus other operating income, minus personnel expenses, depreciation and amortization, and other operating expenses. This corresponds to earnings before interest and taxes (EBIT). We winsorize balance sheet outcomes at the 0.5th and 99.5th percentiles for each year. All monetary variables are expressed in 2018 SEK.

We keep all firms with, on average, five employees over our sample period. In our main estimation sample, we restrict the data to cases where we observe both the acquiring and the target firms in the employer-employee data and the acquiring firm in the balance sheet data. All our results on acquiring firms' outcomes and on the joint employment of acquirer and target firms refer to this sample. Results on joint revenues and profits of acquiring and target firms are based on a subsample where both firms are observed in the employer-employee data and the balance sheet data.[1] If an acquiring firm undergoes more than one acquisition, we keep one of the events at random.[2] We balance our sample by ensuring that the acquiring firm is observed throughout the full event window surrounding the acquisition.

[1] Target firms are on average substantially smaller than acquiring firms, both in terms of employment and revenue, and less likely to be a limited liability company. Therefore, the sample with balance sheet information for acquiring and target firms is smaller than the one with such information for at least the acquiring firm.

[2] Relaxing this restriction does not meaningfully change our results in both the worker and firm sample.

We complement the firm sample with data on board members and CEOs from the Serrano data, linked to the administrative data. We define acquiring and target firms as having a common manager if, in the year before the acquisition, an employee of the acquiring firm sits on the target's board. Among these employees, 34 percent are classified as CEOs of the acquiring firm. Of the remainder, 58 percent are classified as managers based on their 1-digit occupation code, followed by high-skilled professionals (17 percent). Their median within-firm income rank is at the 96th percentile, suggesting they hold senior managerial or executive positions. For brevity, we refer to these individuals as managers or, sometimes, CEOs.

A2 Descriptives Sample

In Appendix Table A6.1, we report average firm and worker characteristics in the firm sample, measured in the year before the acquisition. Columns 1 and 2 report descriptives for the full sample of acquisitions, while 3 and 4 report descriptives for the sample where we observe both acquirer and target firms in the balance sheet data. Acquiring firms are around four times larger than target firms in terms of employment and nearly twice as large in terms of revenue. They pay, on average, 17 percent higher salaries, but value-added per worker and average educational attainments are similar. For comparison, in column 5, we report the characteristics of the average Swedish firm not involved in acquisitions. We reweight the calendar time distributions of these firms to match the distribution of acquisition events. Overall, both acquiring and target firms are substantially larger, more productive and profitable, and pay higher salaries than firms not involved in acquisitions. Appendix Figure A5.1 shows the annual distribution of events in the worker and the firm sample. In Appendix Figure A5.2, we report the 2-digit industries of acquiring firms in both samples. The two most common industries are Retail and Manufacturing. In 57 percent of all acquisitions, the acquiring and target firms are within the same 2-digit industry, and in 39 percent of cases, the two firms are within the same 5-digit industry. In about 60 percent of cases, workers initially employed at the acquiring and target firms remain in separate plants after the acquisition (see Appendix Figure A5.3).

A3 Heterogeneity Worker Results

Which types of workers are most affected by acquisitions? To the extent that acquisitions lead to corporate restructuring, one would expect the worst-performing workers to be most at risk of being laid off. Similarly, to the extent that wages are backloaded, high-tenure, older workers are likely most at risk. To examine differences between workers in the effects of acquisitions, we split the sample based on workers' characteristics and estimate our main effects separately within each group. Appendix Figure A5.9 presents the respective average effects on employment for all workers and labor income for those who stay at the acquiring firm post-acquisition. Older workers above age 50 experience larger income losses of about 1.3 percent,

conditional on staying at the acquiring firm—twice as large as for younger peers. Employment effects differ even more: the likelihood of remaining employed falls by 4.8 percentage points for older workers, compared to 2.5 percentage points for younger workers. We then split the sample by residual pay in the year before the acquisition.[3] Effects on labor income for stayers are similar across high and low residual pay groups, but employment losses are much larger for low residual pay workers (5.6 percentage points) than for high residual pay workers (1.5 percentage points).[4] We then split the sample into four broad occupational groups. Managers and professional workers experience little or no decline in labor income, conditional on staying, and face somewhat smaller risks of unemployment. In contrast, declines are larger for blue-collar workers and for those in administrative and customer service roles. Finally, women are somewhat more likely to become unemployed (3.4 percentage points) than men (2.6 percentage points), while income losses conditional on staying are similar across genders.

A4 What Explains Income Declines Among Displaced Workers?

Declines in labor income among switchers might reflect changes in firm quality, i.e., transition to lower-paying and less-productive employers. On the other hand, these declines could result from a reduction in worker-firm match quality or a loss in firm-specific productivity. To investigate this, we first study four firm-level outcomes. First, using the universe of workers and firms in the Swedish data, we estimate firm pay premia as firm fixed effects in the AKM model (Abowd et al., 1999)

$$y_{it} = \phi_i + \psi_{j(i,t)} + X_{it}^{'}\delta + u_{it} \tag{5}$$

where ϕ_i are worker fixed effects, $\psi_{j(i,t)}$ are firm effects of the firm j where worker i is employed in year t, and X_{it} includes year fixed effects and a squared term of workers' labor market experience.[5] We then rank firms according to the estimated fixed effects $\hat{\psi}_{j(i,t)}$ from 0 to 100, where the firm with the lowest firm premium is assigned the value 0 and the firm with the highest pay premium is assigned rank 100. Second, we follow Sorkin (2018) and estimate overall job quality provided by firms from voluntary firm-to-firm worker transitions.[6] This measure captures both pay-related factors as well as non-pay amenities such as flexible working hours, training opportunities, or pleasant colleagues. As before, we rank firms from 0 (worst) to 100 (best). Finally, we examine the effects on firm size in terms of employment and firms' profits. Appendix Figure A5.7c plots the effects of acquisitions on these four firm-level measures for firm switchers. We find that acquisitions lead to transitions to firms that score

[3] Residual pay is obtained from a Mincer regression of log income on fixed effects for the interaction of calendar year, age bins, gender, region of residence, and educational degree.

[4] Older and less educated workers are also found to experience larger income losses and slower recovery following mass layoffs (e.g. Davis and von Wachter, 2011; Athey et al., 2024).

[5] We use the largest connected set of workers and firms in the employer-employee data to estimate firm fixed effects.

[6] We exclude transitions following mass layoffs, acquisitions, and firm closures. See Sorkin (2018) for further details.

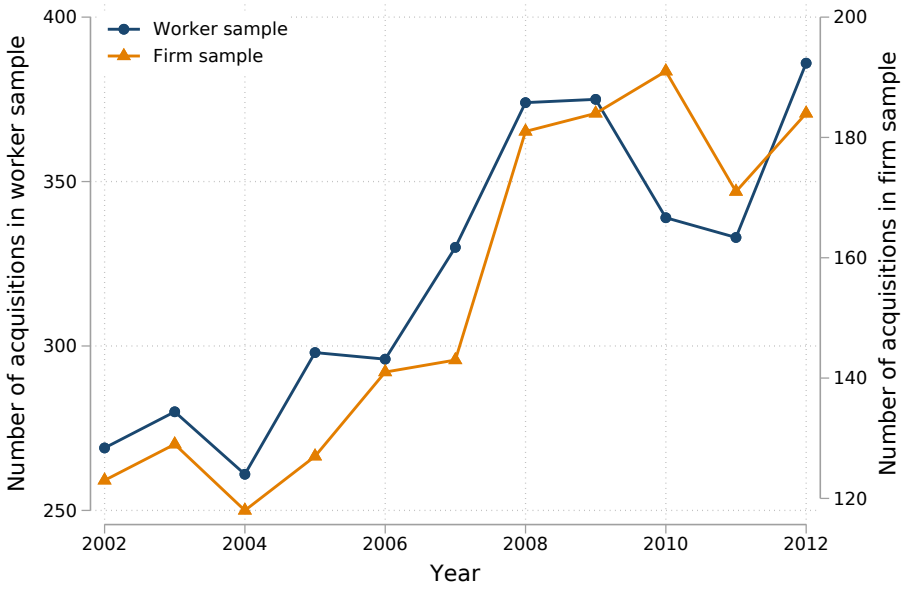
lower on all measures. The firm pay premium rank drops by 3.1, the overall job value rank decreases by 4, firm size drops by 33.2 percent, and profits by 16.3 percent.

To investigate the role of firm-specific human capital, we follow Woodcock (2015) and estimate worker-firm match effects as average spell-level residuals, after controlling for worker and firm fixed effects.[7] We find that acquisitions have a small and statistically insignificant effect on worker-firm match effects, especially when compared to the impact of acquisitions on firm fixed effects (see Appendix Table A6.3). Our results are consistent with (Lagaras, 2024) for Brazil, but differ from (Arnold et al., 2024) for Canada, who find that losses of firm-specific human capital are an important driver of earnings losses following layoffs triggered by mergers and acquisitions. In the mass layoff literature, European studies tend to find a limited role of worker-firm match effects, compared to firm effects (Schmieder et al., 2023; Bertheau et al., 2023) while US studies find the opposite (Lachowska et al., 2020). Overall, these results suggest that workers who separate from the acquiring firm transition to substantially lower-quality employers, contributing to the large losses we find.

[7] Specifically, match effects are estimated as $\hat{\mu}_{ij} = \bar{y}_{ij} - \hat{\phi}_i - \hat{\psi}_{j(i,t)}$ where $\bar{y}_{ij}$ is a worker's average log labor income within an employment spell. See also e.g. Lachowska et al. (2020) for more details.

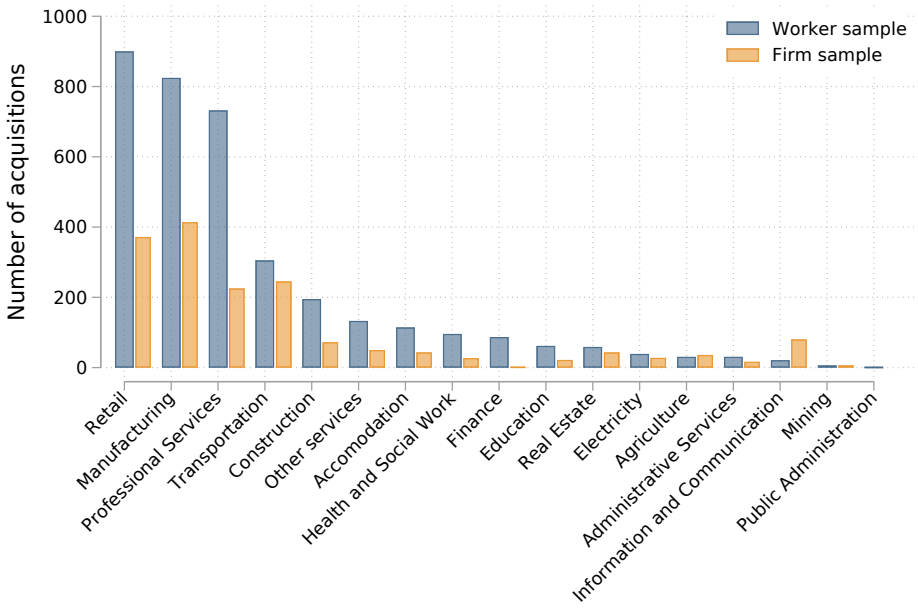
A5 Supplementary Figures

Figure A5.1: Acquisitions over Time



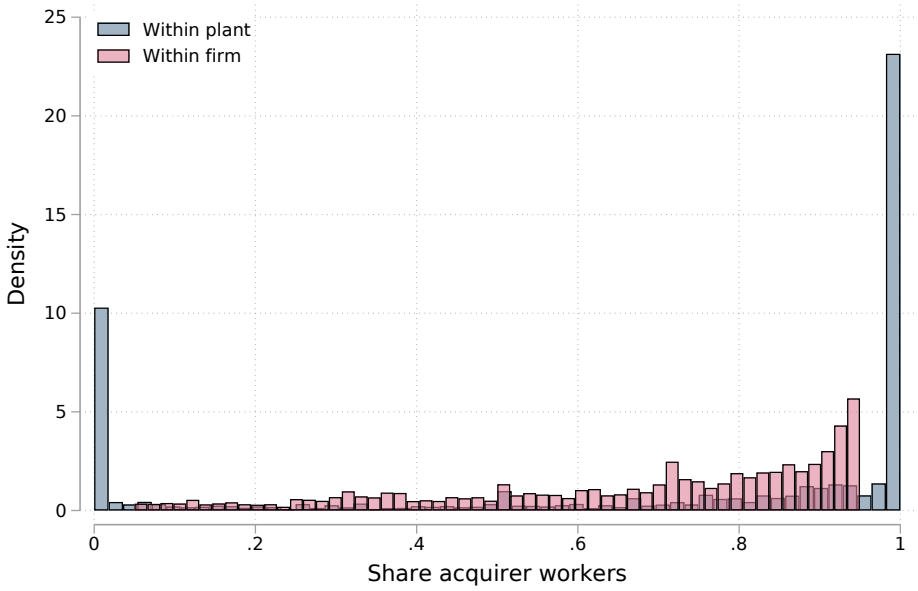
Notes: This figure shows the annual distribution of acquisition events in the worker sample and the firm sample.

Figure A5.2: Acquisitions across 2-digit Industries



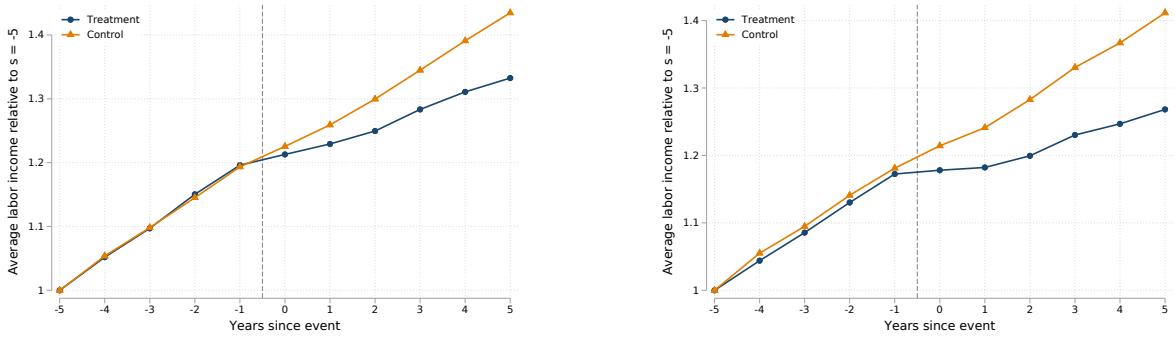
Notes: This figure shows the distribution of acquisition events across 2-digit industries in the worker sample and the firm sample.

Figure A5.3: Share of acquirer workers within firms and plants



Notes: This figure plots the share of workers initially employed at the acquiring firm, relative to the combined initial workforce of acquirer and target, over the five years following the acquisition. The blue bars show the same share within plant. The sample is restricted to cases where this acquirer firm-share lies between 5% and 95%.

Figure A5.4: The Empirical Design

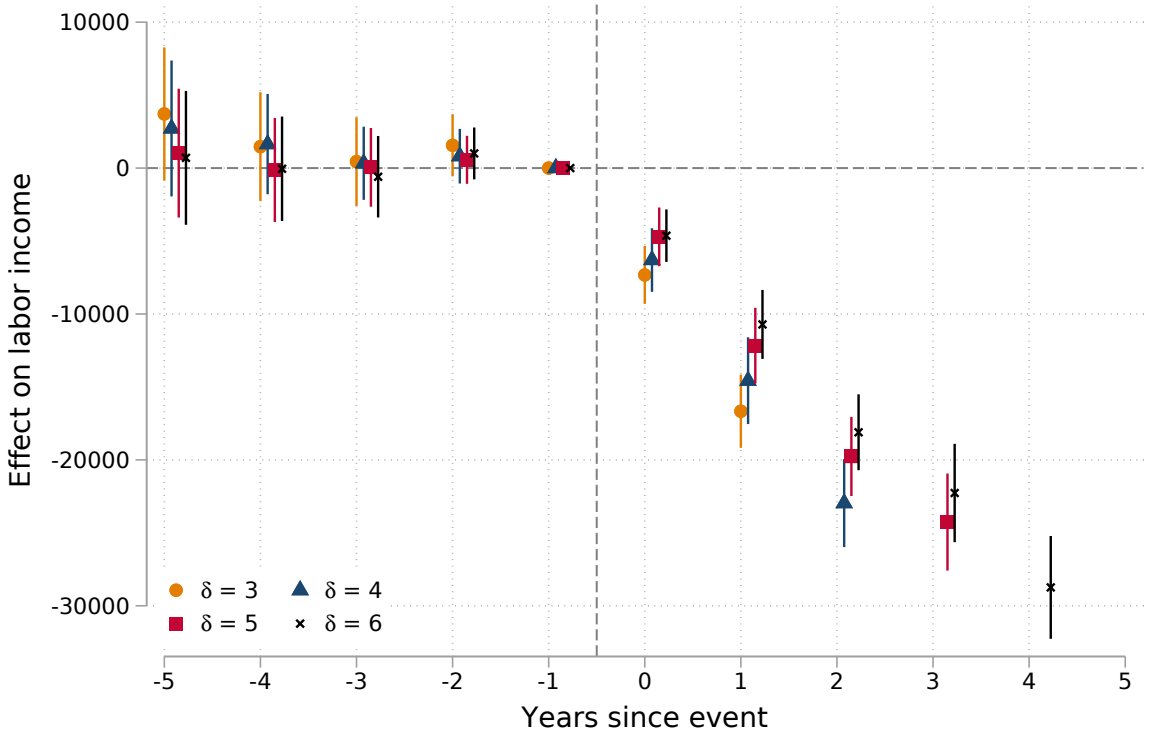


(a) Labor Income: Acquirer Workers

(b) Labor Income: Target Workers

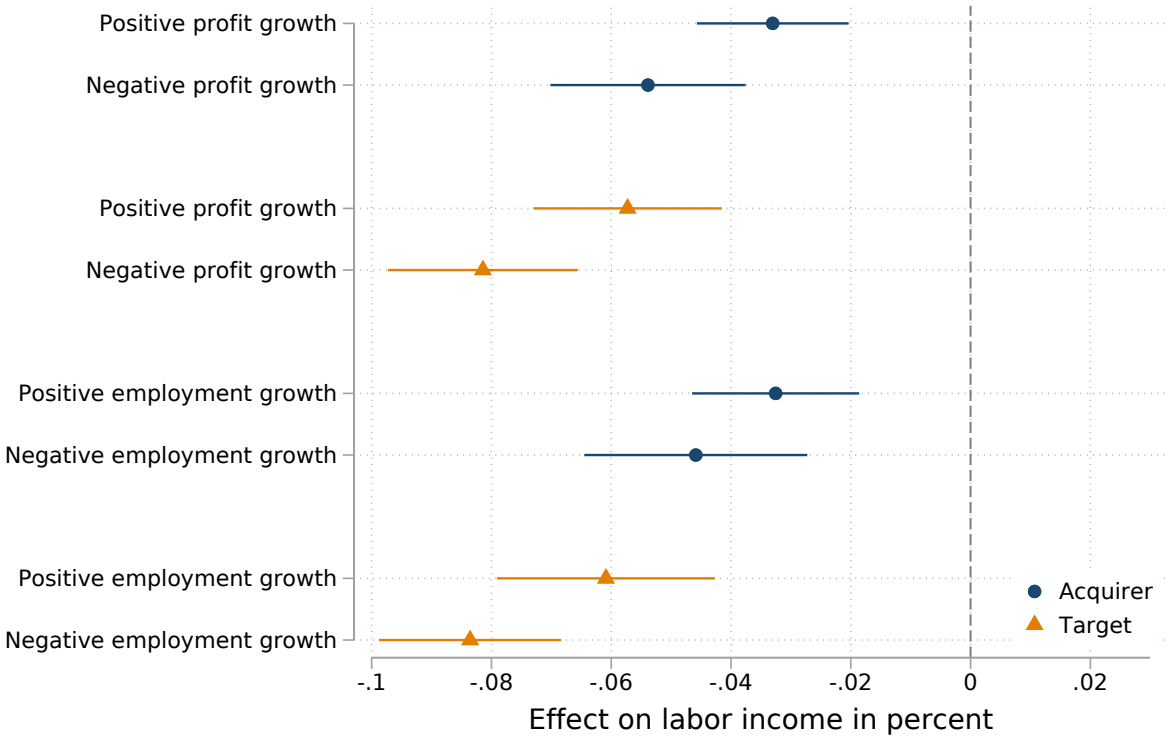
Notes: This figure illustrates our empirical design, plotting average labor income of treatment —which experiences an acquisition at time 0— and the control group which experiences an acquisition at time 7. We normalize labor income by dividing by average income in $s = -5$ for both treated and control workers, respectively **Panel (a)** plots averages for workers who were initially employed at the acquiring firm, and **panel (b)** for those who were initially employed at the target firm.

Figure A5.5: Effects on Labor Income and Robustness to Timing



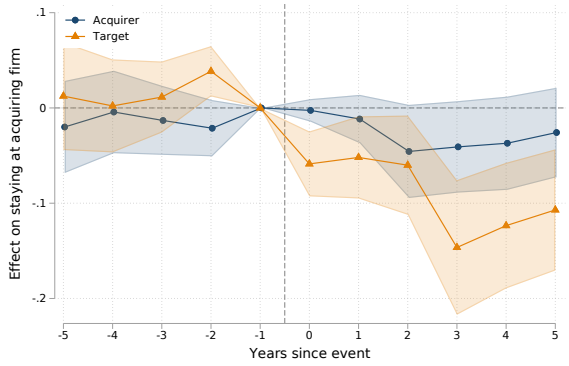
Notes: This figure plots the effects of acquisitions on workers' labor market income. Coefficient estimates are based on stacked DiD event studies according to specification (1), estimated in the worker sample. Control units are workers who experience an acquisition δ years after the treated units. In each panel, we vary δ from 3 to 6 and plot post-event estimates for all periods up to and including two years before control workers undergo the acquisition. Whiskers are 95% confidence intervals and standard errors are two-way clustered at the worker and event level. In all panels, we reweight control units to match the distribution of treatment units based on gender, age, and education, measured in period $s = -5$.

Figure A5.6: Effects on Labor Income by Target Firm Growth

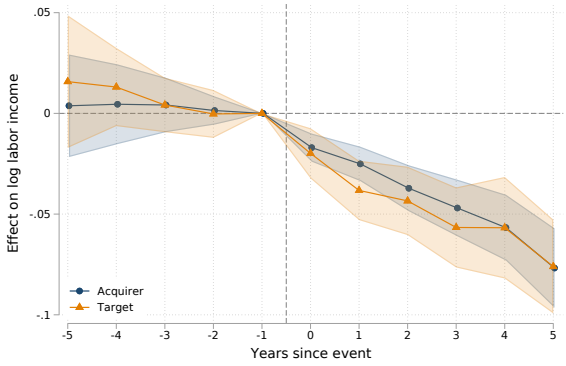


Notes: This figure shows the effects of acquisitions on annual labor market income divided by average labor income of treated units in the year before the acquisition. Coefficient estimates are based on stacked DiD models according to specification (2), estimated in the worker sample. Control units are workers who experience an acquisition seven years after the treated units. We reweight control units to match the distribution of treatment units based on gender, age, and education, measured in period $s = -5$. We report estimates separately for workers who were employed at the acquirer firm in the year prior to the acquisition and those who were employed at the target firm. We split the sample based on whether the target firm experienced negative or positive growth in employment or profit between period $s = -3$ and period $s = -1$. The split by profit growth is only available for events where we observe the target firm in the balance sheet data. Whiskers are 95% confidence intervals where standard errors are two-way clustered at the worker and event level.

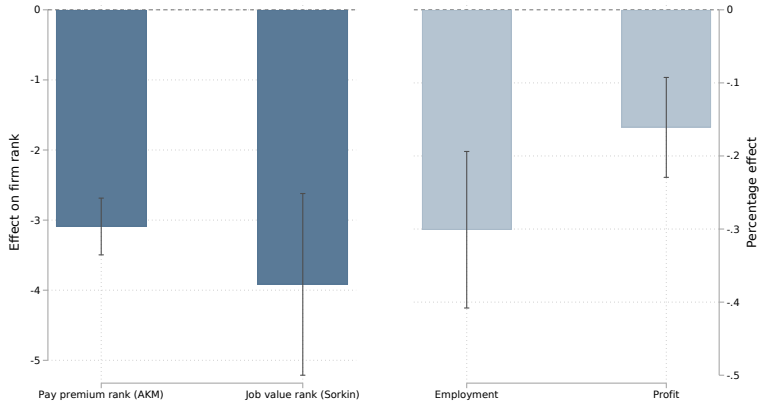
Figure A5.7: Firm Switching



(a) Staying at the Firm



(b) Labor Income of Firm Switchers



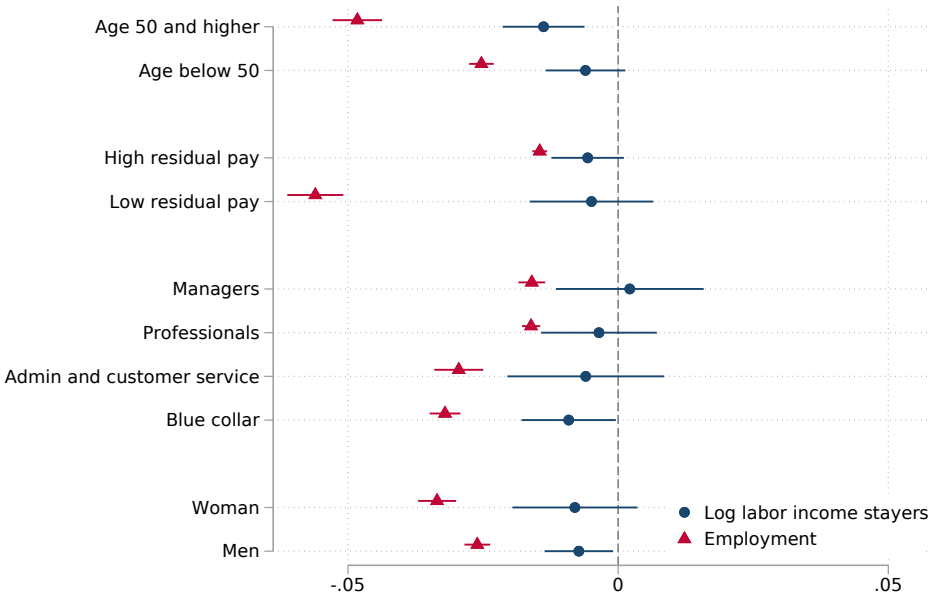
(c) Characteristics of New Employers

Notes: This figure shows the effects of acquisitions on workers who separate from the acquiring firm post-acquisition. Coefficient estimates are based on stacked DiD event studies according to specifications (1) and (2). Control units are workers who experience an acquisition seven years after the treated units. We reweight control units to match the distribution of treatment units based on gender, age, and education, measured in period $s = -5$. **Panel (a)** shows effects on staying at the acquiring firm. In **Panel (b)**, we plot the estimated effects on annual log labor market income for workers who switched to another firm at some point after the acquisition, conditional on employment. **Panel (c)** shows the effects of characteristics of switchers' new employers. The rightmost bar shows the estimate of AKM firm fixed effect ranks (Abowd et al., 1999) of workers' employers, ranging from 0 (lowest) to 100 (highest). The second bar shows the estimate of job value rank based on Sorkin (2018), ranging from 0 (lowest) to 100 (highest). The third bar displays the coefficient on workers' employers' log employment. The last bar shows the estimate on profits of workers' employers, divided by the average profits of treated units in the year before the acquisition. Shaded areas and whiskers are 95% confidence intervals where standard errors are two-way clustered at the worker and event level.

Figure A5.8: Wages and Hours

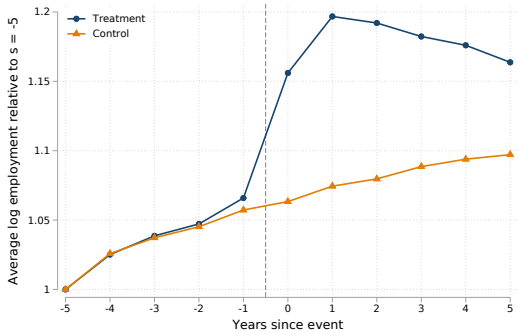
Notes: This figure shows the effects of acquisitions on log hourly wages and log contracted working hours. Coefficient estimates are based on stacked DiD event studies according to specification (1), estimated in the worker sample for workers we observe in the Structure of Earnings dataset (see Appendix Section A1 for details). See also Appendix Table A6.2 for differences between this sample and our main estimation sample. Control units are workers who experience an acquisition seven years after the treated units. Shaded areas are 95% confidence intervals where standard errors are two-way clustered at the worker and event level.

Figure A5.9: Heterogeneity in Effects on Labor Market Outcomes

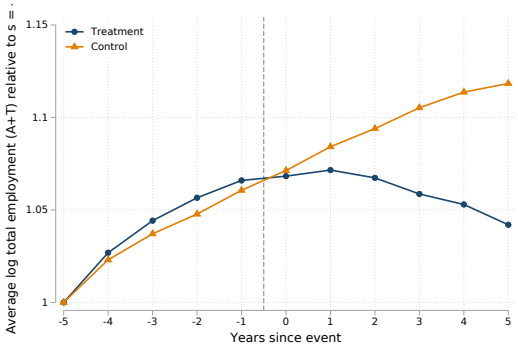


Notes: This figure shows heterogeneity in effects of acquisitions on labor market outcomes. Coefficient estimates are based on the stacked DiD models according to specification (2), estimated in the worker sample. Control units are workers who experience an acquisition seven years after the treated units. The outcomes are log annual labor income for stayers who remain at the acquiring firm after the event, conditional on having worked throughout the sample period, and an indicator for employment, defined as having annual labor income exceeding 10,000 SEK for all workers involved in the acquisition. In the first two rows, we split the sample by age in the year before the acquisition. Then, we split the sample based on whether the worker's mincer residual in the year before the acquisition was above the 67th percentile or below the 33rd percentile. We estimate Mincer residuals by regressing log labor income in the total population on fixed effects for the interaction of calendar year, 10 age bins, gender, region of residence, and educational degree. In the next four rows, we split the sample by broad occupational categories, and in the last two rows, we split the sample by gender. Whiskers are 95% confidence intervals where standard errors are two-way clustered at the worker and event level.

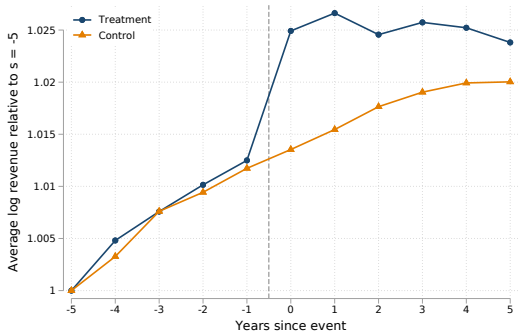
Figure A5.10: Average Firm Outcomes



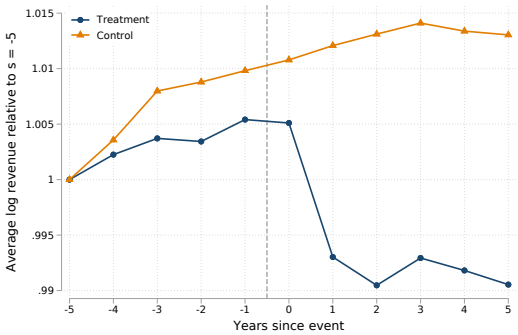
(a) Employment: Acquirer



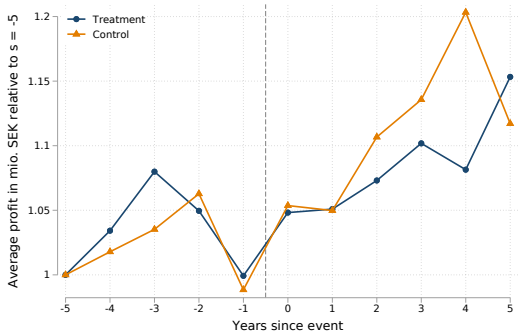
(b) Employment: Acquirer + Target



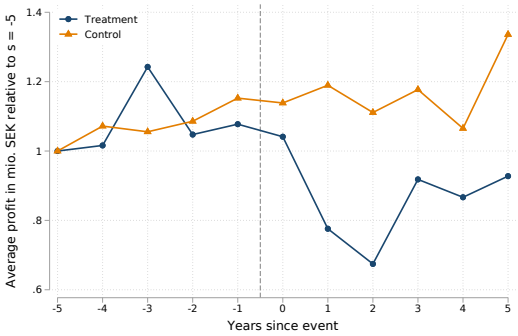
(c) Revenue: Acquirer



(d) Revenue: Acquirer + Target



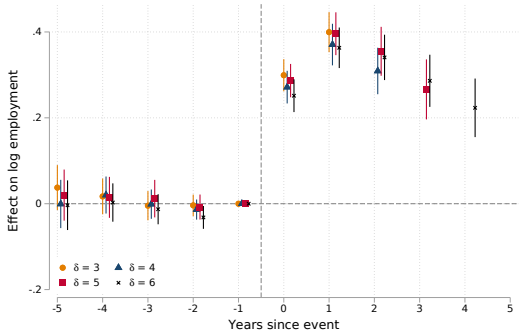
(e) Profit: Acquirer



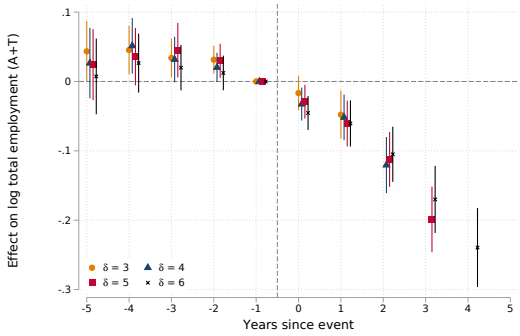
(f) Profit: Acquirer + Target

Notes: This figure shows average firm outcomes for treated and control units in the years before and after an acquisition. We normalize each outcome by dividing by the average outcome for treated and control firms, respectively. **Panel (a)** plots average log employment of acquiring firms, and **panel (b)** the same for joint employment of the acquiring and target firms. **Panel (c)** plots average log revenue of acquiring firms, and **panel (d)** the same for joint revenues of acquiring and target firms. **Panel (e)** plots average profit of acquiring firms in millions of SEK, and **panel (f)** the same for joint profit of acquiring and target firms. We reweight control units to match the distribution of treatment units based on 2-digit industries and 20 employment bins, measured in period $s = -5$.

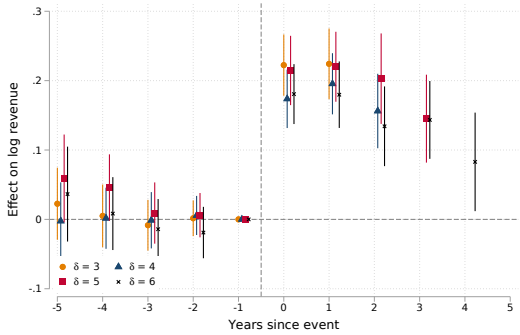
Figure A5.11: The Effects of Acquisitions on Firm Outcomes - Robustness to Timing



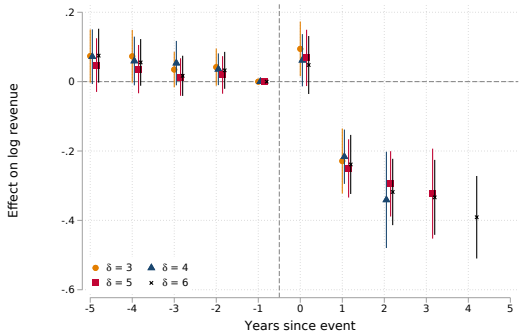
(a) Employment: Acquirer



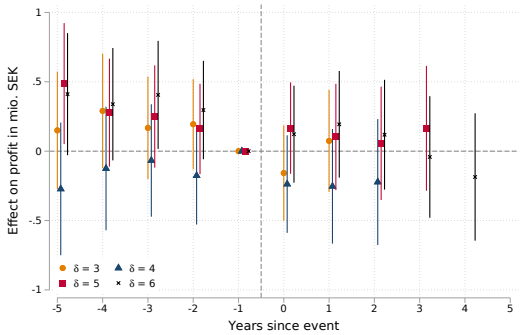
(b) Employment: Acquirer + Target



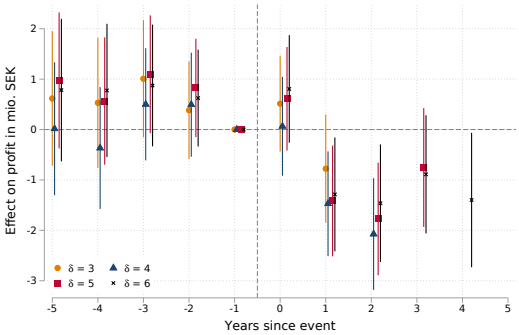
(c) Revenue: Acquirer



(d) Revenue: Acquirer + Target



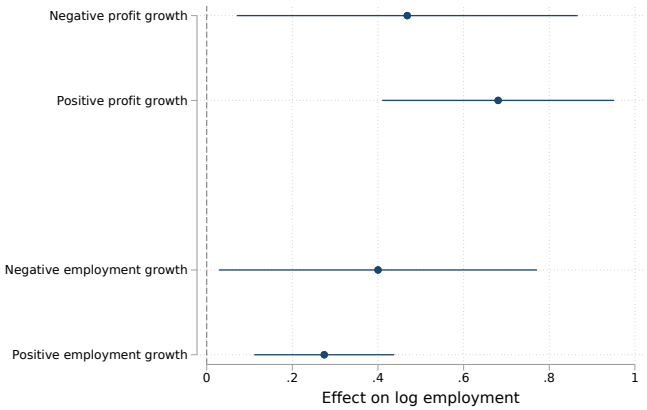
(e) Profit: Acquirer



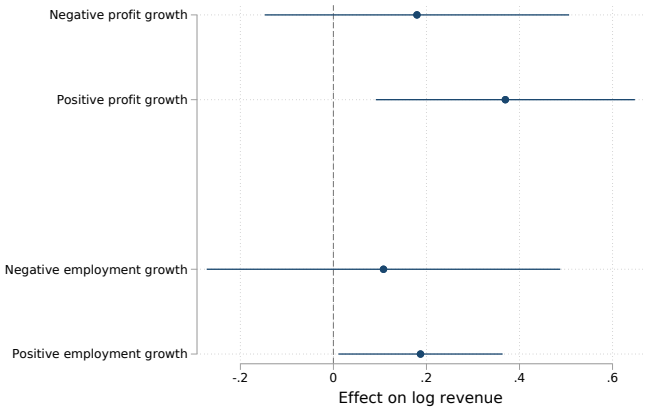
(f) Profit: Acquirer + Target

Notes: This figure shows the effects of acquisitions on firm outcomes, where coefficient estimates are based on stacked DiD event studies according to specification (1), estimated in the firm sample. Control units are firms that undergo an acquisition δ years after the treated units. In each panel, we vary δ from 3 to 6 and plot post-event estimates for all periods up to and including two years before control firms undergo the acquisition. **Panel (a)** plots estimated effects on the acquiring firm's log employment, and **panel (b)** the same for joint employment of the acquiring and target firms. **Panel (c)** plots estimated effects on the acquiring firm's log revenue, and **panel (d)** the same for joint revenues of acquiring and target firms. **Panel (e)** plots estimated effects on the acquiring firm's profit in millions of SEK, and **panel (f)** the same for joint profit of acquiring and target firms. We reweight control units to match the distribution of treatment units based on 2-digit industries and 20 employment bins, measured in period $s = -5$. Whiskers are 95% confidence intervals and standard errors are clustered at the firm level.

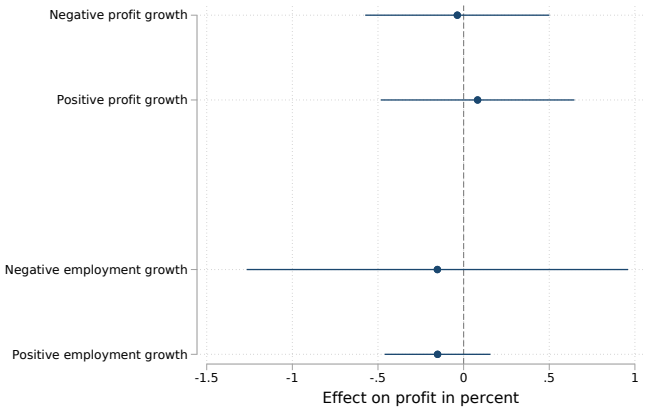
Figure A5.12: Effects on Firm Outcomes by Target Firm Growth



(a) Employment: Acquirer



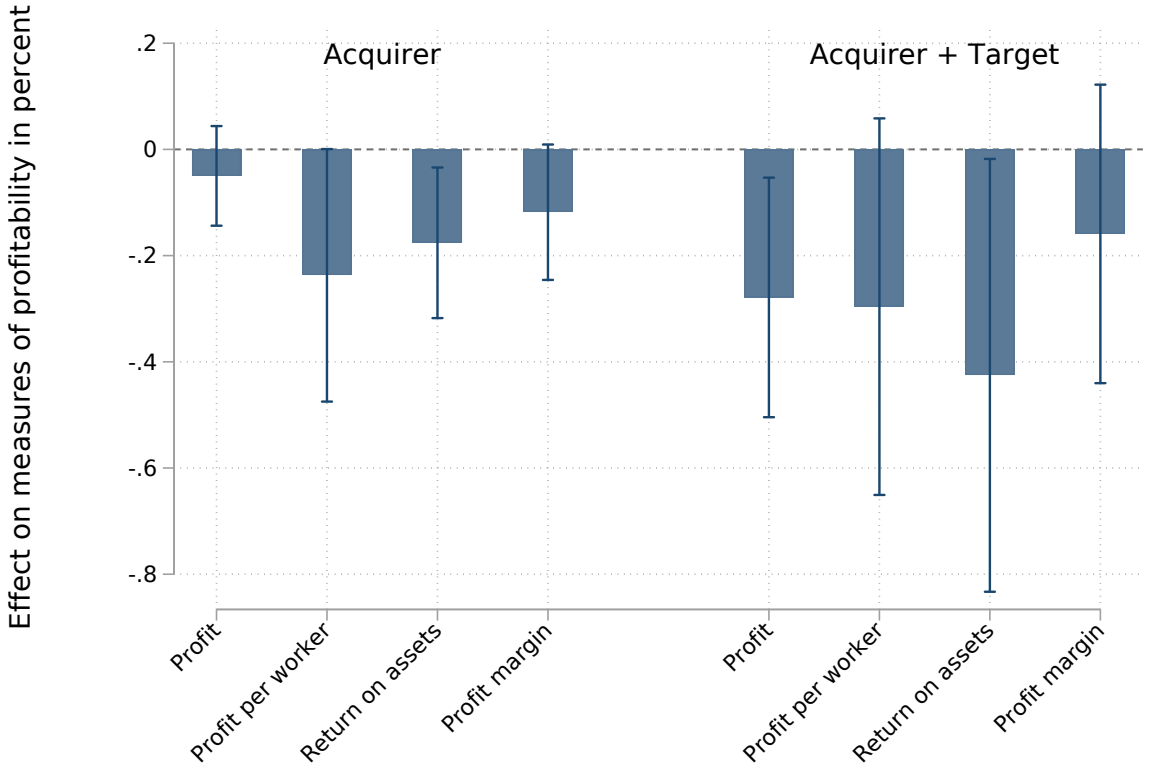
(b) Revenue: Acquirer



(c) Profit: Acquirer

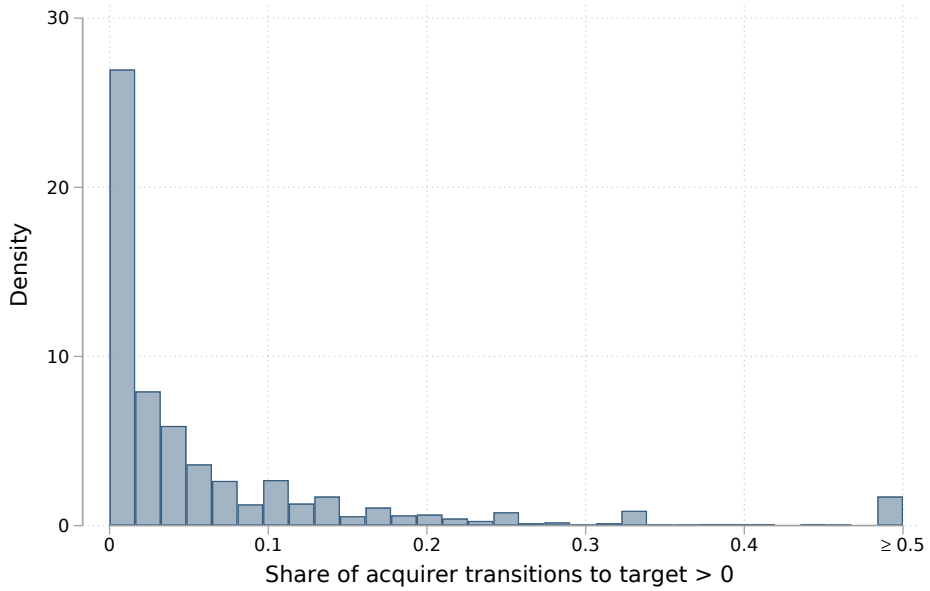
Notes: This figure shows the effects of acquisitions on firm outcomes. Coefficient estimates are based on stacked DiD models according to specification (2), estimated in the firm sample. Control units are firms who undergo an acquisition seven years after the treated units. We reweight control units to match the distribution of treatment units based on 2-digit industries and 20 employment bins, measured in period $s = -5$. **Panel (a)** shows results for the acquiring firm's log employment, **panel (b)** for the acquiring firm's log revenue, and **panel (c)** for the acquiring firm's profit divided by the average profit of treated firms in the year before the acquisition. We split the sample based on whether the target firm experienced negative or positive growth in employment or profit between period $s = -3$ and period $s = -1$. The split by profit growth is only available for events where we observe the target firm in the balance sheet data. Whiskers are 95% confidence intervals where standard errors are clustered at the firm level.

Figure A5.13: Different Measures of Profitability



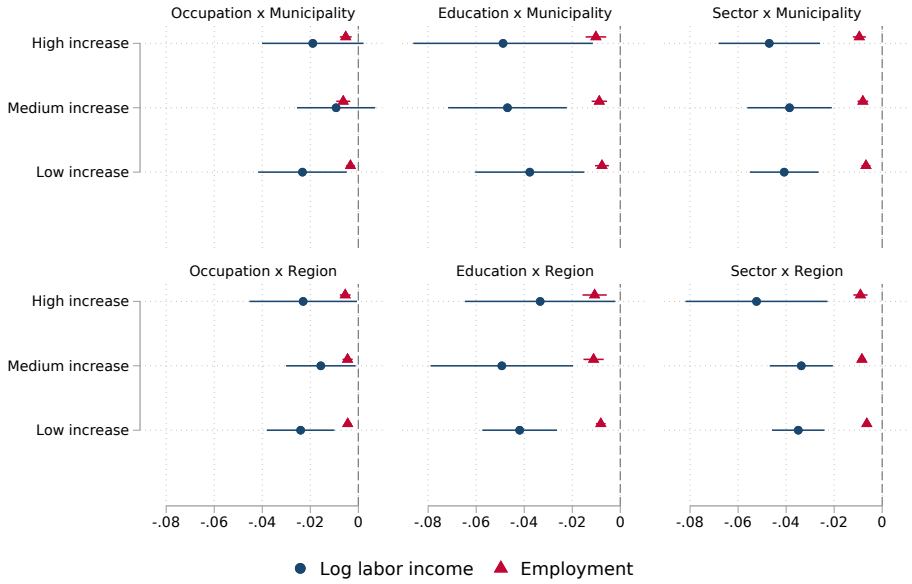
Notes: This figure shows the effects of acquisitions on different measures of firm profitability. These measures are: (operating) profit, profit per worker, return on assets – defined as profit divided by total assets, and profit margin – defined as profit divided by revenue. All outcomes are evaluated relative to the average outcome of treated firms in the year before the acquisition. Coefficient estimates are based on stacked DiD models according to specification (2), estimated in the firm sample. Control units are firms that undergo an acquisition seven years after the treated units. We reweight control units to match the distribution of treatment units based on 2-digit industries and 20 employment bins, measured in period $s = 5$. The first four estimates show results for the acquiring firm and the last four estimates for joint acquiring and target firm outcomes. To account for some severe outliers, we residualize profit margins for the acquiring firm at the 1st and 99th percentile. Whiskers are 95% confidence intervals where standard errors are two-way clustered at the worker and event level.

Figure A5.14: Acquirer-Target Transitions Before Acquisition

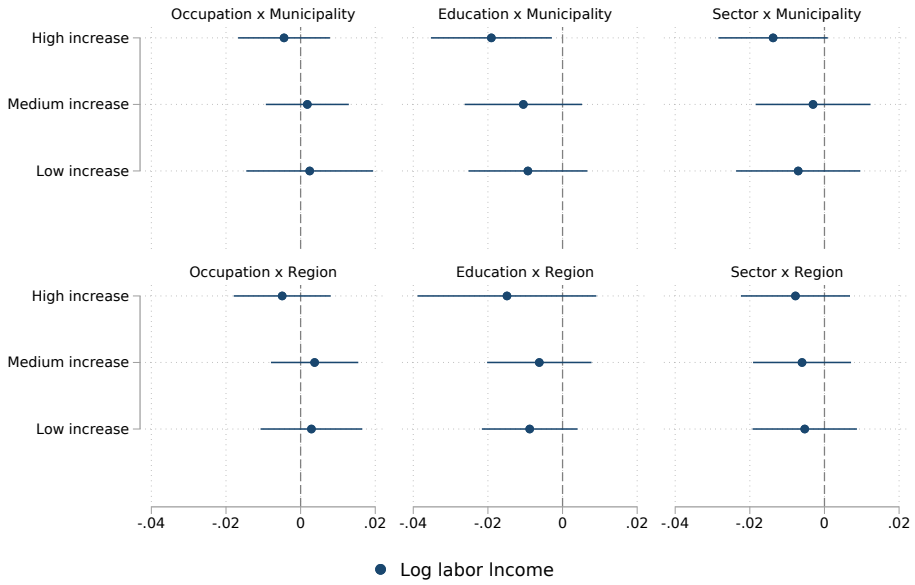


Notes: This figure shows the distribution of worker transitions from the acquiring firm to the target firm, relative to all transitions from the acquiring firm in the seven years leading up to the acquisition. We restrict the distribution to the 17% of events in which there were any acquirer-target transitions. The final bin contains all values greater or equal to 0.5.

Figure A5.15: By Increase in Labor Market Concentration - Alternative Market Definitions



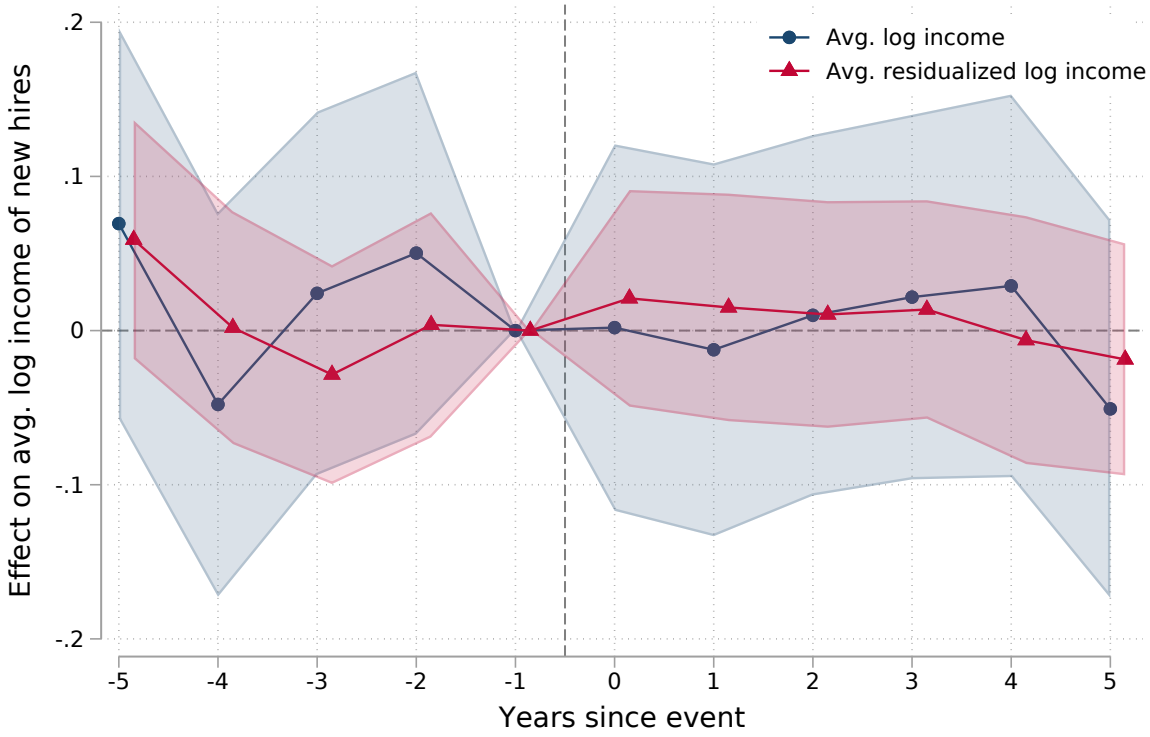
(a) Labor Income and Employment: All Workers



(b) Labor Income: Stayers

Notes: This figure shows the effects of acquisitions on labor market outcomes, where coefficient estimates are based on stacked DiD models according to specification (2), estimated in the worker sample. Control units are workers who experience an acquisition seven years after the treated units. The outcomes in **panel (a)** are log annual labor market income, conditional on employment, and an indicator for employment, defined as having annual labor market earnings above 10,000 SEK. The sample is split into tertiles by the predicted increase in the acquiring firm's employment share in the worker's labor market. We define this increase as the target firm's employment share in the respective labor market. The six panels plot results for different definitions of local labor markets. We interact a geographic identifier with a worker type. As geographic identifiers we use either one out of 290 municipalities or one out of 21 regions in Sweden. Worker characteristics are either one out of 97 educational degrees, one out of 148 3-digit occupations or one out of 16 2-digit sectors the worker is employed in. **Panel (b)** repeats this exercise for workers who stay at the acquiring firm post-acquisition. Whiskers are 95% confidence intervals and standard errors are two-way clustered at the worker and event level.

Figure A5.16: Average incomes of new hires



Notes: This figure shows the effects of acquisitions on average log incomes of new hires at the acquiring firm. Coefficient estimates are based on stacked DiD event studies according to specification (1), estimated in the firm sample. Control units are firms who experience an acquisition seven years after the treated units. We report two outcomes: the average log income of all workers who join the acquiring firm in a given year and the average of residualized log incomes of new hires. We residualize log incomes in the full employer-employee sample by regressing log income on fixed effects for calendar year, gender, age, and educational degrees. Shaded areas are 95% confidence intervals where standard errors are clustered at the firm level.

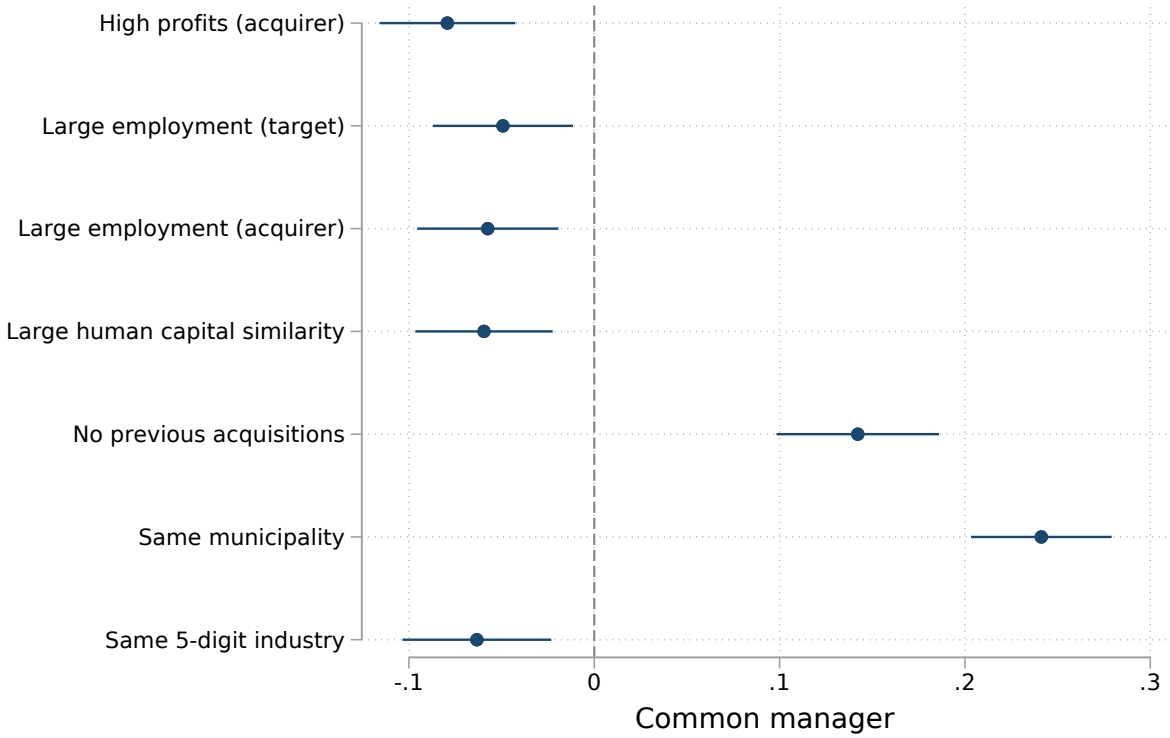
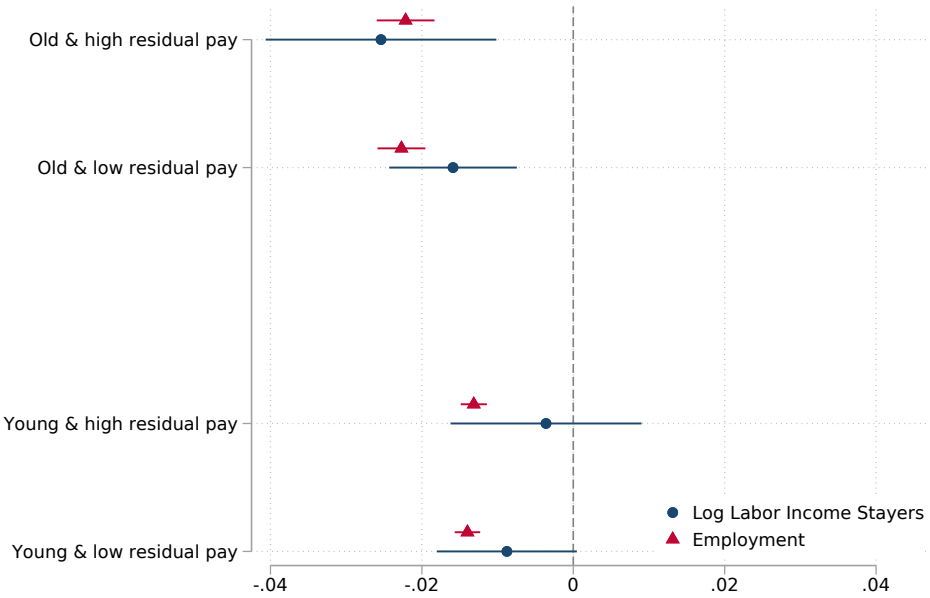


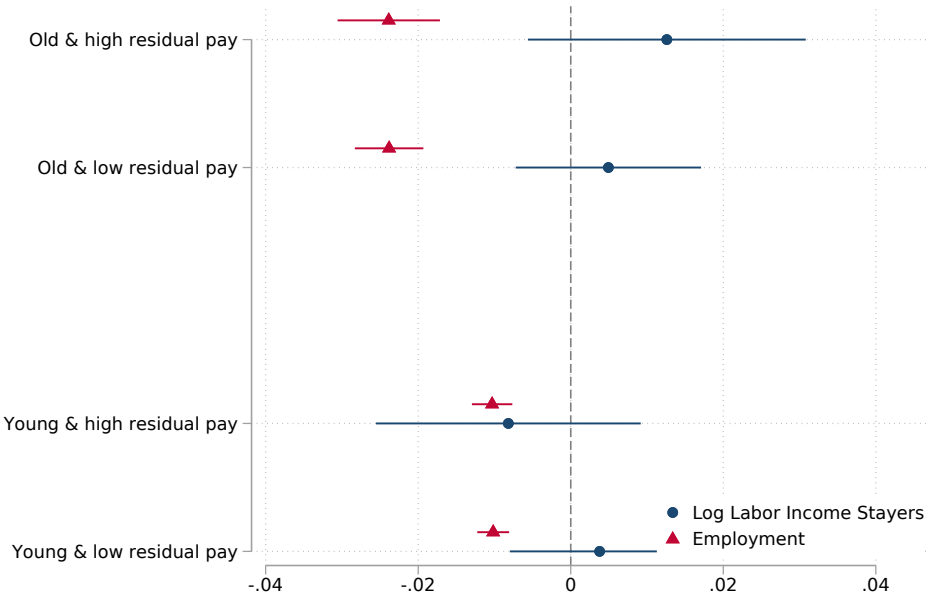
Figure A5.17: Common Manager - by Event Characteristics

Notes: This figure shows the difference in the probability of an acquisition having a common manager in the year prior to the acquisition by various event characteristics. These characteristics include above-median profits at the acquirer, above-median employment at the target, above-median employment at the acquirer, above-median human capital similarity between firms, no prior acquisitions by the acquirer, same municipality, and same 5-digit industry. Whiskers are 95% confidence intervals where standard errors are clustered at the firm level.

Figure A5.18: Effects by Worker Age and Pay



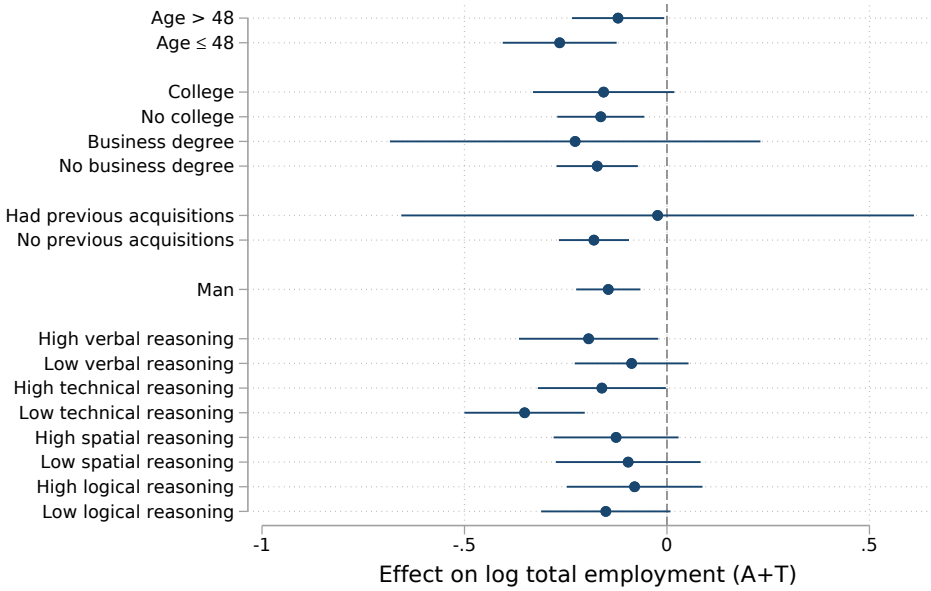
(a) Common manager



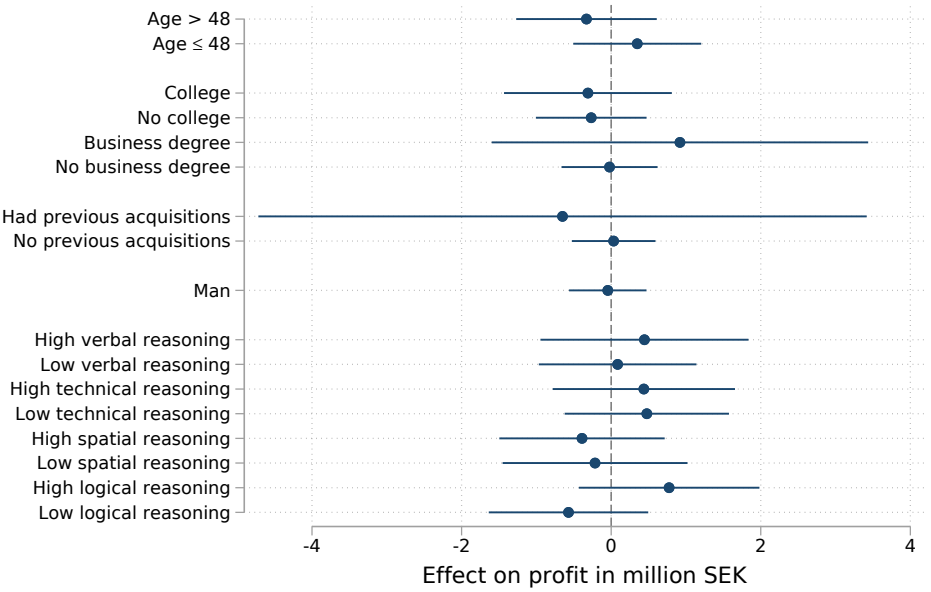
(b) No common manager

Notes: This figure shows heterogeneity in effects of acquisitions on labor market outcomes. Coefficient estimates are based on the stacked DiD models according to specification (2), estimated in the worker sample. Control units are workers who experience an acquisition seven years after the treated units. The outcomes are log annual labor income for stayers who remain at the acquiring firm after the event, conditional on having worked throughout the sample period, and an indicator for employment, defined as having annual labor income exceeding 10,000 SEK for all workers involved in the acquisition. We split the sample into four disjoint groups based on whether workers were old (50 years and above) or young (below 50 years), and based on whether they had high (above 66th percentile) or low (below 33rd percentile) residual pay in the year prior to the acquisition. We estimate Mincer residuals by regressing log labor income in the total population on fixed effects for the interaction of calendar year, 10 age bins, gender, region of residence, and educational degree. **Panel (a)** shows estimates for acquisitions with common manager and **panel (b)** for those without common manager. Common manager acquisitions are defined as events for which an acquiring firm manager was on the board of the target firm in the year before the acquisition. Whiskers are 95% confidence intervals where standard errors are two-way clustered at the worker and event level.

Figure A5.19: The Effects of Acquisitions on Firm Outcomes by CEO Characteristics



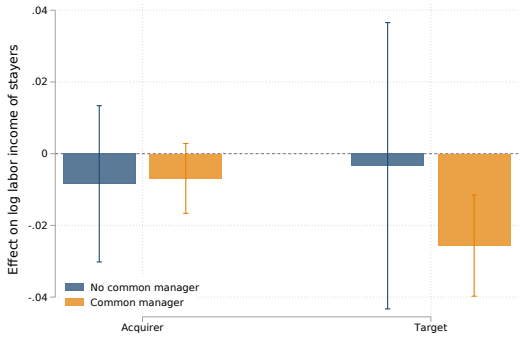
(a) Employment: Acquirer + Target



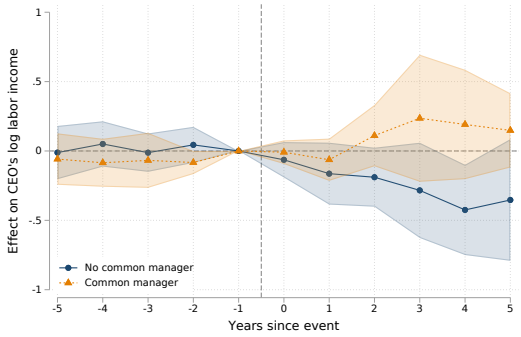
(b) Profit: Acquirer

Notes: This figure shows the effects of acquisitions on firm outcomes, where coefficient estimates are based on stacked DiD models according to specification (2), estimated in the firm sample. Control units are firms that undergo an acquisition δ years after the treated units. The outcome in **panel (a)** is the log joint employment of acquiring and target firms. In **panel (b)**, we plot estimates for acquiring firms' profits in millions of SEK. In both panels, we split the sample based on characteristics of the acquiring firm's CEO in the year before the acquisition: age (above or below 48), college degree, business college degree, prior acquisition experience as CEO, gender, and cognitive skills (above or below median verbal, technical, spatial, or logical reasoning scores from military enlistment data). We do not display coefficients for female CEOs due to small sample size. We reweight control units to match the distribution of treatment units based on 2-digit industries and 20 employment bins, measured in period $s = -5$. Whiskers are 95% confidence intervals and standard errors are clustered at the firm level.

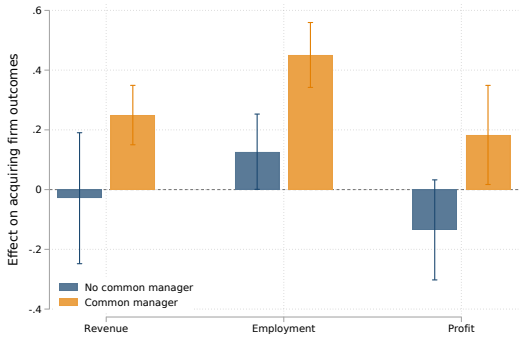
Figure A5.20: Results by Common Manager: Reweighted



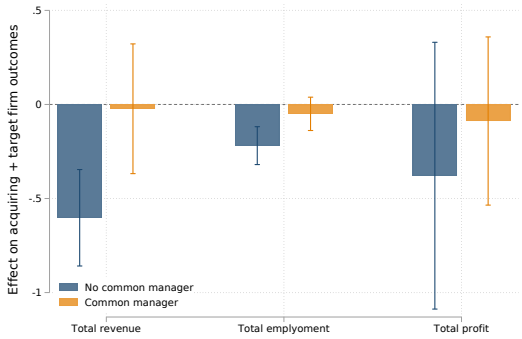
(a) Labor Income: Stayers



(b) Labor Income: Acquirer CEO



(c) Firm Outcomes: Acquirer



(d) Firm Outcomes: Acquirer + Target

Notes: This figure replicates the results in Figure 4c and Figure 5, but reweights the non–common-manager sample to match the characteristics of the common-manager sample. We construct disjoint bins defined over observable characteristics and assign weights to observations in the non–common-manager sample so that the distribution across bins matches that of the common-manager sample. For the worker-level results in **panel (a)**, bins are defined by the interaction of indicators for the acquirer and target being located in the same region, a horizontal acquisition (2-digit industry), whether the acquirer had prior acquisitions, and three employment size categories for each of the target and acquirer firms. For the CEO- and firm-level results in **panels (b)–(d)**, bins are defined analogously, but include three employment size categories for the acquirer firm only. For additional details, see the notes to Figure 4c and Figure 5.

A6 Supplementary Tables

Table A6.1: Firm Sample: Descriptives

	Full sample		Balance sheet sample		
	Acquirer	Target	Acquirer	Target	Other Firms
Profit in mio. SEK	3.36	–	3.18	1.55	0.94
Revenue in mio. SEK	78.43	–	82.32	45.37	22.96
VA per worker in 1000 SEK	1,450	–	1,887	1,831	957
Number of employees	155.63	32.42	171.46	41.16	27.82
Avg. earnings	307,120	275,259	320,193	273,844	215,921
Avg. years of educ.	11.79	11.67	11.93	11.72	11.50
Avg. age	40.36	42.14	40.29	40.20	37.51

Note: This table reports average firm and worker characteristics in the year before the acquisition. **Columns 1** and **2** show acquiring and target firms in the full sample, where we observe all firms in the employer-employee data and acquiring firms in the balance sheet data. **Columns 3** and **4** restrict to the subsample where acquirer and target firms appear in both data sources. **Column 5** reports characteristics of all firms in the balance sheet data, regardless of acquisition status, reweighted to match the calendar time distribution of acquiring firms.

Table A6.2: Full Worker Sample vs. Wage Sample

	Full sample	**Wage sample**		
	(1) Log Labor Income	(2) Log Labor Income	(3) Log Wage	(4) Log Hours
Post x Treatment	-0.0287***	-0.00587	-0.00534	-0.00875***
	(0.00469)	(0.00450)	(0.00387)	(0.00185)
Constant	12.69***	12.76***	10.33***	4.557***
	(0.00134)	(0.00151)	(0.00130)	(0.000622)
Person FE	Yes	Yes	Yes	Yes
Event-time FE	Yes	Yes	Yes	Yes
Observations	3714832	1904131	1904131	1904131

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table reports effects of acquisitions on workers' labor income, wages and working hours. Coefficient estimates are based on stacked DiD models according to specification (2), estimated in the worker sample. Control units are workers who experience an acquisition seven years after the treated units. **Column 1** reports the estimated effect on log annual labor income in the full worker sample, conditional on being employed throughout the sample period. In **columns 2 - 4**, we restrict the sample to those workers whom we observe in the Structure of Earnings data (see Section 2) and report estimated coefficients on log labor income, log wages, and log working hours. Standard errors are two-way clustered at the worker and event level.

Table A6.3: Effects on Firm and Match Effects of Firm Switchers

	(1) Log Labor Income	(2) Firm Effect	(3) Match Effect
Post x Treatment	-0.0456***	-0.0632***	-0.0145
	(0.00662)	(0.00523)	(0.0125)
Constant	12.66***	0.231***	0.0625***
	(0.00194)	(0.00153)	(0.00367)
Person FE	Yes	Yes	Yes
Event-time FE	Yes	Yes	Yes
Observations	2121537	2121537	2121537

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table reports effects of acquisitions on switchers' labor income, firm effects and firm-worker match effects. Coefficient estimates are based on stacked DiD models according to specification (2), estimated in the worker sample. Control units are workers who experience an acquisition seven years after the treated units. We estimate firm effects from an AKM model (Abowd et al., 1999) and worker-firm match effects as average spell-level residuals following Woodcock (2015). Standard errors are two-way clustered at the worker and event level.

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