

Mergers and Managers: Manager-Specific Wage Premiums and Rent Extraction in M&As*

Alex Xi He
MIT

Daniel le Maire
University of Copenhagen

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Abstract

This paper studies how the market for corporate control disciplines managers who pay high wages. We construct a manager-firm-worker matched panel data set covering the population of Denmark from 1995 to 2011 and develop a framework to measure manager styles in wage-setting by tracking workers and managers across different firms over time. We find that individual managers do matter for wages, and variation in manager fixed effects can explain a significant part of wage differences between firms. Using a comprehensive sample of over 3000 M&As, we show that mergers target high-paying managers and reduce wage premiums but not employment at target firms, and that the effect is stronger in less competitive industries. Establishments with high wage premiums due to generous managers are more likely to be acquired, and experience higher manager turnover and larger wage declines after acquisition. Lower wages have little effect on firms' productivity, and therefore represent a transfer from workers to shareholders. We show that increased market power in product markets or labor markets cannot account entirely for these facts. The reduction in wages accounts for about half the shareholder gains in all M&As, suggesting that rent extraction might be a major motive for merger transactions.

JEL codes: G34; G30; J31; M52; J50; D22

Key Words: Managerial style; mergers and acquisitions; rent sharing; wage policies; linked employer-employee data

*Alex Xi He: Department of Economics, MIT. Email: axhe@mit.edu. Daniel le Maire: Department of Economics, University of Copenhagen. Email: daniel.le.maire@econ.ku.dk. We thank David Autor, Daron Acemoglu, and David Thesmar for their invaluable advice and generous help at all stages of this project. We also thank Isha Agarwal, Josh Angrist, Tania Babina, Sydnee Caldwell, David Eil, Paul Goldsmith-Pinkham, Jon Gruber, Sabrina Howell, Mitsuru Igami, Simon Jäger, Pat Kline, Holger Mueller, Paige Ouimet, Christopher Palmer, Jim Poterba, Nancy Rose, Antoinette Schoar, Kathryn Shaw, Camille Terrier, John Van Reenen, Mike Whinston, as well as seminar participants at MIT and the WEAI graduate student workshop for helpful suggestions. Alex Xi He gratefully acknowledges financial support from the George and Obie Shultz Fund.

1 Introduction

A growing literature suggests that manager-specific preferences and styles play an important role in shaping a variety of corporate decisions (Bertrand and Schoar 2003). Some managerial preferences can lead to diverging interests between managers and value-maximizing shareholders. In particular, managers often enjoy private benefits from paying workers higher wages (Bertrand and Mullainathan 2003; Cronqvist et al. 2009). In this case, managers who tend to pay workers higher wages may result in lower shareholder value, and thus one would expect the market for corporate control to discipline those managers with non-value-maximizing styles (Manne 1965; Martin and McConnell 1991). The link between manager styles and wage setting has important implications for the labor market and corporate governance, yet we have little empirical evidence on whether there are manager-specific styles in setting wages and how market forces discipline them.

In this paper, we show that some managers are “soft” and pay all workers higher wages than other managers conditional on productivity, and that these soft managers are the targets of mergers and acquisitions (M&As). The novel contribution of our paper is to introduce the manager dimension in wage setting and to demonstrate that it is a major driver of M&As. Manager fixed effects on wages partly explain differences in wages between firms highlighted in recent studies such as Card, Heining, and Kline (2013) and Song et al. (2015), and are a principal component of manager styles that are uncorrelated with manager fixed effects on firms’ productivity and capital structure that previous papers have studied. Our findings show that paying high wages is a major type of managers’ non-value-maximizing behavior targeted by corporate takeovers, and that replacing soft managers contributes to about half of the combined firm’s profit gains in M&As.

We start by outlining a theoretical framework with an imperfectly competitive labor market and managers who derive private benefits from higher wages. Managers differ both in productivity and in the degree of private benefits. We define “softer” managers as managers who pay workers higher wage premiums conditional on productivity and other firm characteristics.¹ We show that mergers that replace soft managers with tough managers reduce wages, whereas mergers that increase productivity or monopoly power increase wages. The model also predicts that firms with softer managers are more likely to be acquired and those soft managers to be replaced, which leads to wage declines after acquisitions.

Following predictions from the model, we estimate managers’ styles using a two-step approach.

¹Accordingly, “tougher” managers are managers who pay workers lower wages. Empirically the “softness” of a manager is measured by a continuous measure of manager fixed effects on wage premiums.

In the first step, we estimate time-varying establishment-specific wage premiums using a two-way fixed effects regression similar to Abowd, Kramarz, and Margolis (1999, AKM for short). In the second step, we estimate manager fixed effects in explaining the wage premiums conditional on firm fixed effects and productivity. Manager fixed effects are identified by manager mobility across firms as in Bertrand and Schoar (2003). We construct a manager-firm-worker matched panel data set covering all firms and all workers in Denmark from 1995 to 2011.² The largest connected set covers more than 75% of the workers and contains over 100,000 managers.

We find that individual managers do matter for wages and that manager styles are transferrable across firms. Wage residuals (above and beyond any firm effect) at a new employer are strongly correlated with residuals at the prior employer for a given manager. Manager fixed effects explain more than 30% of the variation in establishment-specific wage premiums, and a move from the 10th to the 90th percentile in the distribution of manager fixed effects is associated with a 21% increase in workers’ wages. To address the concern that manager mobility is correlated with time-varying shocks to firms, we conduct an event study of wage changes in companies experiencing exogenous manager turnovers due to natural retirements and find stable wages before the retirement and large wage losses (gains) after departures of soft (tough) managers.

Managers’ wage fixed effects are uncorrelated with managers’ productivity or financial policy. We measure productivity using total factor productivity (TFP) and value added per worker, and for both measures we do not find that soft managers are more or less productive. Soft managers also do not have higher financial leverage or fire workers more. This is consistent with our theoretical assumption that heterogeneity in manager styles in wage setting is due to heterogeneous private benefits. Consistent with the prediction that managers in less competitive industries can enjoy a “quiet life” more without incurring negative profits,³ we find that the distribution of manager fixed effects is wider and has a longer upper tail in less competitive industries.

We then test whether M&As discipline managers who pay high wages. We first find that mergers and acquisitions reduce wage premiums at target establishments. We identify M&As using firm identifiers following Smeets et al. (2016), and our sample covers over 3000 mergers and acquisitions within the universe of Danish firms from 1995 to 2011. We track the behavior of workers and establishments before and after merger deals, and compare them to a carefully constructed control group of similar establishments that are not acquisition targets during the period. Within our sample,

²Managers are defined as top managers of establishments (every establishment has one manager).

³“The best of all monopoly profits is a quiet life” (Hicks, 1935). Bertrand and Mullainathan (2003) suggest that managers enjoy a quiet life by paying high wages and therefore buying peace with their workers.

following mergers the employment at target firms declined by 2-3% initially, but grew back to the original level afterward. However, workers staying at target establishments experienced a persistent 2% wage decline relative to workers staying at control establishments.⁴ The negative wage effect cannot be accounted for by the selection of worker exits, and holds for various alternative matching estimators. Young and low-skilled workers experienced the largest wage declines. There was little change in wages at the acquiring firms after mergers.

We show that the lower wages at target firms after M&As are due to the replacement of soft managers. First, both wage premiums and manager fixed effects at target establishments are about 2% higher than at establishments of similar productivity, industry, and region. This indicates that mergers target establishments with high wage premiums resulting from soft managers. In contrast, acquirers on average have lower wage premiums and tougher managers. Second, soft managers are much more likely to be replaced when their firms are acquired, whereas at control establishments turnover rates are similar for soft managers and tough managers. Third, workers experienced large wage declines at target establishments that had soft managers before the merger, especially after soft managers were replaced. In contrast, workers at target establishments that had tough managers prior to the merger experienced no significant wage changes regardless of whether managers were replaced or not. Fourth, since there is a larger room for wage discretion in less competitive industries, target firms in less competitive industries have softer managers and reduce wages more after being acquired.

The reduction in wages accounts for a major part of profit gains in merger transactions. We combine the balance sheets of target firms and acquirer firms before mergers to create a firm panel, and find that merging firms experience an increase in return on assets (ROA) of 1 to 1.5 percentage points. Assuming that target firms replace soft managers with average managers with a probability equal to the observed manager turnover rate, we infer that this leads to a 0.63 percentage point increase in the ROA of the joint firm, accounting for 42% to 63% of the total increase in profitability.

We consider a number of alternative explanations for our results. First, efficiency-enhancing mergers may target firms with unproductive and inefficient managers who happen to be soft. We show that the differences in manager productivity measured by TFP or value added per worker are statistically insignificant from zero between target firms and acquirer firms. This indicates that M&As discipline managers paying high wages but not managers with low productivity. Second,

⁴We focus on wage changes of stayers in the firm to control for changes in worker composition. Previous studies that have studied wage effects of M&As include Conyon et al. (2002), Huttunen (2007), Li (2013), Lichtenberg and Siegel (1990), Davis et al. (2014), and Ma et al. (2017). However, most of these studies used firm- or establishment-level average wages, which is also affected by changes in worker composition.

mergers may reduce wages due to increases in labor market concentration and monopsony power. We take several approaches to measure the impact of mergers on monopsony power⁵ and find that the majority of mergers in our sample has little effect on monopsony power. We also find large and significant wage declines, even for mergers that supposedly have no impact on labor market power. Third, the wage decline could result from “value-destroying” mergers or mergers that eliminate product market competitors to preempt competition. However, we find similar wage declines for horizontal and non-horizontal mergers, and for production and non-production workers. We also find little change in target establishments’ employment levels and exit rates, and an increase in the profitability of combined firms, which suggests that value-destroying mergers are exceptions rather than the norm. Fourth, manager styles and wage reductions are not driven by firms’ different propensities to automate or outsource, since we observe large wage declines even for workers whose work could not be automated or outsourced.

Our paper builds on previous literature on how managers affect various corporate practices (Bertrand and Schoar 2003; Bennedsen et al. 2017) and worker productivity (Lazear et al. 2015; Frederiksen et al. 2017). Our paper is closest to Cronqvist et al. (2009) and Bach and Serrano-Velarde (2015), both of which study the effects of CEO characteristics on workers’ pay. Our paper is the first to systematically study the role of individual manager styles in redistributing rents between workers and shareholders, and to show that manager styles are disciplined by the market for corporate control.

Our paper offers a new perspective on how M&As create shareholder value and provides empirical support for the claim that M&As transfer rents from workers to shareholders (Shleifer and Summers 1988). While most existing studies find higher stock market returns and better performance for the combined company following mergers (Bradley et al. 1988; Andrade et al. 2001; Moeller et al. 2004; Betton et al. 2008),⁶ the sources of this value creation are less clear. A large literature in industrial organization and corporate finance studies the effects of mergers on monopoly power, productive efficiency, and vertical foreclosures.⁷ Several papers suggest that a large part of synergy

⁵We measure the impact of mergers on firms’ monopsony power using several approaches following Naidu et al. (2018).

⁶Another strand of literature finds that most mergers fail to create shareholder value for the bidding company (see, e.g., Malmendier et al. 2016). The two findings are not mutually exclusive, since the acquirer often overbids, but mergers still create value on average when taking into account the effect on the acquired company (Kaplan 2016).

⁷See, for example, Farrell and Shapiro (1990), Kim and Singal (1993), Dafny (2009), Hoberg and Phillips (2010), Ashenfelter et al. (2014), Blonigen and Pierce (2016), Miller and Weinberg (2017) on the effects of mergers on monopoly power; Ordoover, Saloner, and Salop (1990), Hortaçsu and Syverson (2007) and Houde (2012) on vertical foreclosures; and Pesendorfer (2003), Braguinsky et al. (2005), Devos et al. (2008), Siegel and Simons (2010), Li (2013), and Sheen (2014) on productivity and efficiency. Other papers have looked at automation (Olsson and Tåg 2016; Ma et al. 2017), financial constraints (Erel et al. 2015), preempting competition (Cunningham et al. 2018),

in mergers comes from replacing management in poorly managed targets (Lang et al. 1989; Wang and Xie 2009). Our paper is the first to empirically disentangle the redistribution of wealth among workers and shareholders from efficiency improvements using a comprehensive data set of mergers. Our paper also complements the literature on the negative relationship between labor protection and merger returns (John et al. 2015; Dessaint et al. 2017) by providing direct evidence of how acquirers benefit from employment and wage adjustments.

The rest of the paper is organized as follows. Section 2 presents the theoretical framework that motivates our empirical analysis. Section 3 describes the empirical setting and data sets used in the analysis. Section 4 discusses identification of manager styles and presents results on how managers affect wages. Section 5 presents empirical tests of whether mergers discipline soft managers. Section 6 discusses threats to validity and alternative explanations, and Section 7 concludes.

2 Theoretical Framework

In this section, we introduce a simple two-period wage bargaining model between a firm’s manager and a homogeneous group of workers. We use the model to show how individual managers’ styles—by which we mean their propensity to pay above-market wages—affect wage setting and profits. We then show how mergers create value by replacing inefficient managers, and disentangle the effects of various channels—efficiency, monopoly power, and rent extraction—on employment and wages. Finally, we develop an empirical measure of manager styles based on the relationship between manager turnovers and changes in wages and productivity.

The economy has a large number of firms and a large number of workers. Each firm has one manager. For simplicity, we assume that firms operate a production technology that uses labor as the only input. The production function is $Y_j = T_{jm}F(L_j)$, where L_j is the number of workers employed at firm j . T_{jm} denotes firm j ’s total factor productivity (TFP) when it is managed by manager m . We assume that $F(\cdot)$ is strictly increasing, strictly concave, and continuously differentiable in L . The timing is as follows: in period 1 managers bargain over wages with the workers. The managers then make production decisions and hire workers in period 2. We solve for a subgame perfect equilibrium of this model.

At period 2, demand for firm j is given by $q_j(p_j)$, and the corresponding inverse demand is $p_j(L_j) = q_j^{-1}(T_{jm}F(L_j))$. Firms solve the following profit maximization problem (note that wage

talent and innovation acquisition (Ouimet and Zarutskie 2011, Phillips and Zhdanov 2013), and growth options and investment opportunities (Levine 2017) as motivations for M&As.

rate is assumed to be exogenous in this period):

$$\max_{L_j} \pi_j = p_j(L_j)T_{jm}F(L_j) - w_jL_j \quad (1)$$

The first-order condition is:

$$w_j = \left(1 - \frac{1}{\varepsilon_j}\right) p_j T_{jm} F'(L_j) \quad (2)$$

where $\varepsilon_j = -\frac{\partial q_j}{\partial p_j} \frac{p_j}{q_j}$ is the price elasticity of demand and $\varepsilon_j > 1$.

At period 1, managers bargain with their respective unions over wages. We assume that the solution is characterized by a generalized Nash bargaining outcome given by the following program:

$$\max_{w_j} (w_j - b_j)^\beta (\pi_j + \phi_m w_j \overline{L}_j)^{1-\beta}$$

where β is workers' bargaining power at firm j and b_j is the outside option of workers at firm j . The union maximizes the wages paid to workers.⁸ We assume that managers maximize the sum of firm profits and their private benefits from higher wages. The reason is that managers have agency costs and prefer to enjoy a “quiet life” (Bertrand and Mullainathan 2003), or they use high wages as a substitute for monitoring efforts (Krueger 1991; Acemoglu and Newman 2002). They may also simply enjoy paying some workers high wages (Landier, Nair and Wulf 2009; Yonker 2017). The term $\phi_m w_j \overline{L}_j$ captures the private benefits to managers, where ϕ_m is a manager-specific parameter, and \overline{L}_j is the past average employment at firm j .⁹ Private benefit is proportional to the firm's average employment but does not depend on employment during the current period, and therefore it captures managers' preferences for higher wages but *not* higher employment. Since the term does not contain current-period employment, it does not enter the maximization problem in period 2. The resulting outcome is:

$$w_j = (1 - \beta)b_j + \beta \frac{p_j T_{jm} F'(L_j)}{L_j - \phi_m \overline{L}_j} \quad (3)$$

⁸There is no employment in the union's utility function because (1) the majority of firm-level bargaining agreements cover only wage increases and no employment-related outcomes; (2) since most of our sample is before the Great Recession, we assume that employment is mainly adjusted along the hiring margin, and involuntary separations do not depend on bargained wage levels, which we verify in the data later. The absence of employment in bargaining agreements is also inconsistent with the class of efficient bargaining models, in which managers and workers bargain to Pareto-efficient employment and wage levels.

⁹The term is scaled by average employment level such that the private benefit is proportional to firm size (for example, the cost of bargaining may be higher in bigger firms). Nevertheless, our qualitative predictions remain the same when private benefit is $\phi_m w_j$.

Equations (??) and (??) jointly determine wage w_j and employment L_j . The following proposition summarizes how manager styles in wage setting ϕ_m affect firm outcomes:

Proposition 1. *Wage w_j is increasing in $\phi_{m(j)}$, and employment L_j and profit π_j are decreasing in $\phi_{m(j)}$, where $m(j)$ indexes the manager working at firm j .*

Managers also differ in their productivity T_{jm} , and the following proposition summarizes how managers' productivity affects firm outcomes:

Proposition 2. *Wage w_j , employment L_j and profit π_j are increasing in manager's productive efficiency T_{jm} .*

To ensure that profits are nonnegative, the maximum ϕ_m must satisfy:

$$\phi_{m(j)} \leq 1 - \frac{\beta}{1 - (1 - \beta) \frac{b_j L_j}{p_j T_{jm} F(L_j)}} \quad (4)$$

The right-hand side of this equation depends on the ratio of average productivity $\frac{p_j A_j F(L_j)}{L_j}$ to outside option b_j . When the outside option is low relative to productivity, the right-hand side is close to $1 - \beta$; when the outside option is close to productivity, the right-hand side is almost zero.

Proposition 3. *Let $\bar{\phi}_j$ be the maximum ϕ_m such that firm j has nonnegative profits. $\bar{\phi}_j$ is increasing in productivity T_j and decreasing in demand elasticity ε_j . In other words, $\bar{\phi}_j$ is higher in industries with higher concentration.*

Managers with higher ϕ_m ("soft" managers) lead to higher wages and lower profits, which provides opportunities for acquiring firms to extract rents. Managers with low TFP lead to lower profits and provide opportunities for productivity-enhancing mergers. The following corollary describes how different channels of mergers affect employment, wages, and productivity in the target firms.

Corollary. *Mergers raise profits and create value through the following channels:*

- (1) *Rent-extracting mergers: mergers replace soft managers, i.e., $\phi_{m(j)}$ decreases, which reduces wages, increases employment, and does not affect TFP at target firms;*
- (2) *Productivity-enhancing mergers: mergers replace inefficient managers, i.e., T_{jm} increases, which increases wages and TFP at target firms, and has ambiguous effects on employment;*
- (3) *Market-power-increasing mergers: mergers increase market power and markups in the product market, i.e., ε_j decreases, which increases wages and TFP and reduces employment.*

Among all the channels, only the rent extraction channel reduces wages at the target firms. The reason is that, holding the bargaining power and manager preferences fixed, mergers that increase productivity through efficiency improvements or market power will lead to higher wages.

The model predictions allow us to estimate manager styles and manager productivity from the data. We can approximate the wage rule as:

$$\log w_j = (1 - \beta) \log b_j + \beta \log \left(\frac{p_j T_{jm} F(L_j)}{L_j} \right) + \beta \phi_{m(j)} \quad (5)$$

In this equation log wage is the sum of three parts: the first part is reservation wage, the second part is sharing of average log value added per worker, and the third part is due to manager discretion. Therefore, in the panel data, when we include both firm fixed effects and manager fixed effects and control for productivity, the manager fixed effects would identify the term $\beta \phi_m$. Since the two-way fixed effects model requires a lot of manager mobility across firms, we also take a complementary approach of regressing wages on productivity, and industry and region fixed effects interacted with year fixed effects, and the residual from this regression is $\beta \phi_m$ if the error terms are uncorrelated with manager styles.¹⁰ We discuss the estimation of manager styles in more detail in Section 4.1.¹¹

Similarly, we can estimate manager productivity using the two-way fixed effects framework with the dependent variable being the TFP. Assume that the TFP can be decomposed into a firm-specific component and a manager-specific component ($\log T_{jm} = \log A_j + \log \theta_m$), then manager fixed effects from the two-way fixed effects regression identifies individual managers' productivity θ_m . The log value added per worker is: $\log(p_j T_{jm} F(L_j)/L_j) = \log A_j + \log \theta_m + \log(p_j F(L_j)/L_j)$. Conditional on one firm, a more efficient manager also increases value added per worker, but a 1% increase in θ_m increases value added per worker by less than 1% due to decreasing returns to scale.

In our stylized model, soft managers get higher private benefits from paying higher wages; alternatively, soft managers may give workers higher bargaining power β . In a more general model with both heterogeneous private benefits and bargaining power among managers, the treatment effect of managers we identify combines various structural parameters. However, our approach still identifies the correct ranking of managers' effect on wage premiums as long as managers' bargaining power is

¹⁰The interactions of industry and region fixed effects with year fixed effects control for the outside option b_j . We assume that the outside option is not affected by managers. Otherwise changing the outside option b_j has the same effects on wages and employment as changing ϕ_m , but only changes the interpretation: "soft" managers give workers better outside options in wage negotiations instead of enjoying private benefits from high wages.

¹¹Although workers are homogeneous here, the model can easily incorporate worker heterogeneity by having workers with different productive units, and wage is price per productive unit. In that case, there is an additional term for worker ability in equation ??, which can be identified by worker mobility in the first step of our empirical approach.

uncorrelated with their productivity. Our qualitative results also remain unchanged: soft managers raise wages and lower profits, and therefore are replaced in M&As.

Our identification using manager fixed effects relies on the assumption that manager mobility is uncorrelated with the time-varying residual components of wage residuals. Importantly, this assumption is *not* violated by systemic patterns of manager mobility related to fixed manager characteristics. For example, soft managers may be more likely to be fired, but this does not violate the assumption, because our fixed effects estimator is conditioned on the actual sequence of establishments at which each manager is observed. However, the assumption would be violated if shocks to wage residuals of workers predict the firing of soft managers. For instance, if soft managers are more likely to be fired when firms experience negative shocks to productivity and wages, this will lead to an over-statement of the importance of manager styles. Another violation is sorting based on match effects. For example, firms may tend to select as managers the family members of the owners or founders, who have a higher stake in the company and stronger incentives to maximize profits by paying low wages. We discuss how to address these concerns in Section 4.1.

3 Data and Empirical Setting

3.1 Data Sources

The main data sets used in this paper are drawn from administrative registers in Statistics Denmark. Our firm data come from the Firm Statistics Register, or FirmStat, which covers the universe of private-sector Danish firms for the years 1995 to 2011. FirmStat associates each firm with a unique identifier, and provides annual data on many of the firm’s activities, such as number of full-time employees, value added and industry affiliation. We also match with other firm registers to obtain firms’ balance sheet information, including profits and dividends.

The worker data are extracted from the Integrated Database for Labor Market Research, or IDA, which covers the entire Danish population aged 15 to 74, including the unemployed and those who do not participate in the labor force. The IDA associates each person with her unique identifier, and provides annual data on many of the individual’s socioeconomic characteristics, such as hourly wage, education, and occupation. We measure the hourly wage rate as annual labor income plus mandatory pension fund payments divided by annual hours.¹² Each employed worker is matched to her establishment. An establishment is a unique physical work location, such as an office, store, or

¹²The annual hours are imputed using the supplementary mandatory pension contributions (ATP), which takes four values based on four intervals of the hours worked.

factory, and each establishment has a unique identifier that is consistent over time.

To match our firm data with our worker data we draw on the Firm-Integrated Database for Labor Market Research, or FIDA, which links every firm in FirmStat with every worker in IDA who is employed by that firm in the last week of November, including temporary workers. Using our matched worker-firm data, we can consistently track virtually every person in the Danish economy over time regardless of her employment status or employer identity.¹³

We identify managers using the occupation codes of workers following Friedrich (2017). In cases where an establishment has multiple managers, we select the highest-ranked manager based on occupation codes, hierarchy, and wages. We discuss the construction of manager variables in greater detail in the Data Appendix.

3.2 Danish Labor Market

We first highlight several key features of the labor market in Denmark to provide context for our following analysis. Denmark has a flexible labor market with low hiring and firing costs. Botero et al. (2004) classified Denmark as one of the most flexible labor markets in the world, comparable to the United States. Unlike in many countries in continental Europe, employment protection is very weak in Denmark, and it is easy for Danish firms to hire and fire employees. In 1995 the average tenure in Denmark was the lowest in continental Europe at 7.9 years, similar to the level in UK (7.8 years) and lower than in Germany (9.7 years). Unemployed workers receive generous unemployment benefits, but are also incentivized to search for jobs through active labor market policies, which together form what is called the “flexicurity” model.

Like other Scandinavian countries, Denmark used to have an industry-level standard rate wage bargaining system until the 1980s, but since then wage bargaining has been decentralized to the individual or establishment level. By the start of our sample in 1995, only 16% of the private labor market was still covered by the standard rate system, whereas the majority of wage contracts were and still are negotiated at the worker-firm level (Dahl et al. 2013).¹⁴ The bargaining at the firm or establishment level is between the managers and shop stewards, and the majority of agreements

¹³The high quality of the match derives from two features of the data. One, IDA and FIDA are administrative data and the worker identifier used in each remains unchanged throughout 1995 to 2011. Two, the informal sector is almost nonexistent in Denmark, unlike in some developing countries such as Brazil and Mexico that have been used in previous matched worker-firm studies.

¹⁴Industry-level bargaining agreements usually specify a wage floor, which is not binding in most cases. Segments that remained under the centralized standard rate system are largely characterized by routine tasks (e.g., transport, warehouse work, and production line work), where it makes less sense to differentiate wages across workers. In our data we can only observe the bargaining system before 2001. Our results are robust to excluding all firms in the standard rate system before 2001.

cover wage increases and not employment levels.¹⁵

Although wage structure in Denmark is still more compressed than in the United States, it has experienced a significant increase in wage inequality: between 1980 and 2011, the 90/10 wage ratio in Denmark increased from 2.1 to 2.8, similar to Germany, whose 90/10 wage ratio increased from 2.4 to 3.0.

3.3 Construction of the M&A Sample

We identify mergers and acquisitions using the changes in *firm* identifiers of establishments because establishment identifiers remain constant despite changes in ownership.¹⁶ We identify a merger if two establishments with different firm identifiers in a given year had the same firm identifier in the next year. For example, if establishment 1 had firm identifier A and establishment 2 had firm identifier B in year 0, and then, in year 1, they shared the same firm identifier C (which could be A or B or a new one), this suggests that firm A merged with firm B between year 0 and year 1. The establishment whose firm identifier remained the same both before and after the merger is the acquirer firm. In cases where a new firm identifier was created after the merger, we don't know which was the acquirer. In 93% of mergers, we can clearly identify which establishments were in the acquirer and target firms.

We take a few steps to restrict the sample and make sure we identify the mergers correctly. First, we drop partial mergers, that is, we only consider mergers where all establishments in the target firm are acquired by the same acquirer firm. Second, we drop mergers where the firm identifier of the target firm still exists at any time after the merger. These two steps help to avoid picking up changes in firm identifiers unrelated to ownership changes.¹⁷ Finally, throughout our analysis we focus only on mergers between private firms in private industries. The reason is that, Danish municipalities merged in 2007, which resulted in many mergers of government agencies, and these mergers are very different in nature from the corporate mergers we consider in this study.¹⁸

We also merge the administrative data with an external data set on M&As to verify the validity of our approach to identifying mergers. The data we use is transaction-level data on mergers and

¹⁵The scope of bargaining varies from company to company, and the most common agreement concerned annual wage increases (77% of agreements) and individual supplements (43% of agreements). Management possesses a right to hire and fire, that cannot be questioned by shop stewards except in a few exceptional cases (Ilsøe 2012).

¹⁶Our approach to identifying mergers is similar to Smeets et al. (2016), who used the same data set but for different time periods.

¹⁷In robustness checks we also include these partial mergers in our analysis and find very similar results.

¹⁸The public sector in Denmark is large compared to most other countries and accounts for nearly a third of employment. Around 10% of all mergers and acquisitions involve a firm in the public sector. We show in Appendix ?? that M&As in the public sector do not reduce wages.

acquisitions from Zephyr at Bureau Van Dijk. Zephyr is a comprehensive source of data on M&As, covering both public and private transactions. We then match all the merging firms in Denmark during our sample period in the Zephyr data set to firms in our administrative firm data using the firm name and address. In the Data Appendix we compare mergers in the Zephyr data sets with mergers in our data sets. Almost all the matched target firms from Zephyr data sets are also identified as target firms by our approach, but we are able to identify more mergers in the earlier years of the sample period.

We identify around 3700 mergers within Denmark from 1995 to 2011. ?? plots the number of mergers between Danish firms in each year. ?? plots the percentage of workers in Denmark working in acquirer or target firms in each year. On average about 1% of workers each year work in one of the target firms, and about 5% of workers work in one of the acquirer firms. This indicates that mergers affect a large proportion of workers in the economy. Table 1 reports summary statistics for this sample. On average, acquirer firms are larger and more productive than target firms. Target firm employees are on average younger, less educated, and less experienced, whereas workers at acquirer firms are older, more educated, and more experienced than the average worker in the economy.

The Danish merger control regime was implemented in 2000. Most firms in our sample have turnovers below the threshold subject to merger control.¹⁹ Very few mergers were challenged and nearly none of the mergers were blocked.

4 Do Individual Managers Matter for Wages?

In this section we establish that individual managers matter for wage premiums of workers. We develop a novel framework to measure manager fixed effects on wage premiums using both manager and worker mobility, and verify that manager styles are transferrable across firms. We then investigate the correlation of manager effects on wages with other measures of manager style and the interaction of manager style with industry concentration.

¹⁹A merger is required to notify the antitrust authority if: the combined turnover in Denmark is more than 900 million DKK and the aggregate turnover in Denmark of each firm is more than 100 million DKK; or the aggregate turnover in Denmark of at least one firm is more than 3.8 billion DKK and the aggregate worldwide turnover of at least one firm is more than 3.8 billion DKK.

4.1 Estimation of Manager Styles in Wage-Setting

4.1.1 Empirical Methodology

We start by identifying individual managers' styles regarding wage-setting. We define "manager" as the top manager of each establishment.²⁰ A manager is "softer" if she has a higher ϕ_m and "tougher" if she has a low ϕ_m . We estimate managers' styles in wage setting using a two-step procedure: in the first step we estimate an establishment-specific, time-varying wage premium using worker mobility across firms, and in the second step we identify managers' styles in determining the wage premium using manager mobility across firms.

We start by estimating a two-way fixed effects regression at the worker level with log hourly wage on the left-hand side and person fixed effects and establishment-year fixed effects on the right-hand side:

$$y_{ijt} = \psi_{jt} + \xi_i + \beta X_{ijt} + \epsilon_{ijt} \quad (6)$$

where ξ_i is worker fixed effects and X_{ijt} are time-varying worker characteristics, including quadratic and cubic terms in age fully interacted with educational attainment. The estimated establishment-year fixed effect, ψ_{jt} , provides a measure of establishment-specific time-varying wage premiums, and indicates how much the same worker gets paid at establishment j in year t relative to at other establishments in other years. This specification is similar to the AKM regression (for example in Card, Heining, and Kline 2013), except that here we allow the establishment-specific wage premium to vary over time. Worker mobility across establishments allows us to separately identify the establishment-year fixed effects and person-fixed effects within the largest connected set of establishments. We exclude all managers in the estimation of wage premiums.

We then estimate an establishment-level regression with establishment fixed effects and manager fixed effects similar to the manager fixed effects regression in Bertrand and Mullainathan (2003). We impose the requirement that managers have to be at each firm for at least two years, to ensure that managers are given a chance to exert their influence at a given company. Our specification is as follows:

$$\hat{\psi}_{jt} = \lambda_{m(j,t)} + \alpha_t + \gamma_j + \beta X_{jt} + \varepsilon_{jt} \quad (7)$$

where $\hat{\psi}_{jt}$ is the establishment-year fixed effect for establishment j in year t estimated from step 1, λ_m

²⁰Alternatively one can define manager as the top manager of each *firm*. Since most of the target firms in our sample have a single establishment, the two approaches yield very similar results. We also find similar results when including only single-establishment firms.

is manager fixed effect, α_t is year fixed effects, γ_j is establishment fixed effects. X_{jt} are time-varying establishment characteristics, including the share of female workers, the share of workers in each education group, the average age and experience of workers, and dummies for each decile of value added per worker.²¹ Similar to the AKM regression, the identification comes from managers changing establishments. Establishment fixed effects and manager fixed effects are separately identifiable within the largest connected set of establishments linked by manager movements.

4.1.2 Estimation Results

Table 2 presents the estimation results from both steps. As shown in the top three rows, our sample includes 34 million person-year observations, 3.6 million workers and 380,000 firms. Mobility rates are high for workers: the largest connected set linked by worker movements includes 96% of the establishments and 99.7% of the person-year observations. There is also a lot of mobility of managers between the biggest firms: the largest connected set linked by manager mobility contains 75% of the workers and 59% of the person-year observations.²²

To summarize our findings, for each step we report the standard deviations of the estimated fixed effects, the correlations between the two fixed effects, and the adjusted R^2 statistic. The fixed effects are unbiased but inconsistent estimates of the unobserved effects; therefore the variance and covariance of the fixed effects will be biased due to estimation error. We adopt the leave-out estimator in Kline et al. (2018) to adjust for this problem and to obtain unbiased estimates for variance and covariance terms in models with two-way fixed effects and unrestricted heteroscedasticity. The bias-adjusted correlation between manager and establishment fixed effects is quite small (-0.03), suggesting that there is not much systematic manager sorting across establishments based on fixed wage premium differences.

How big is the variation in manager effects on wages? The estimated manager fixed effects have a corrected standard deviation of 0.082, which is economically significant and bigger than the standard deviation of the estimated establishment fixed effects (0.075). The variance in manager effects accounts for 31% of the between-firm wage variation. A move from the 10th to the 90th

²¹We also ran robustness tests using log value added and mean lagged value added over the previous three years as controls, and all our main results remain unchanged. Since firms may insure workers against temporary productivity shocks but not against permanent productivity shocks (Guiso et al. 2005), our estimates of manager fixed effects may capture manager effects on permanent productivity that are not controlled for by our measures of worker productivity. As we show later, managers' productivity is not systematically correlated with managers' wage effects, which alleviates this concern.

²²The establishments in the connected set are usually larger. More than 40% of target establishments and 70% of target establishments with more than 50 employees are in the connected set of managers.

percentile in the distribution of manager fixed effects, assuming that it is normally distributed, is associated with a 21% increase in workers' wages.

4.1.3 Robustness

To address the concern that sorting based on time-varying shocks to wages might bias our estimates, we consider an event study of wage premiums for establishments that change managers in ???. We split the set of departing managers and their successors into quartiles based on the manager fixed effects, and plot the average wage premium in the two years before and the two years after the firms switch managers as a function of origin and destination manager category. There is little pre-trend before the manager turnover. If instead firms systematically replace soft managers during downturns, we would expect to see a negative trend before turnovers. In addition, the effects of turnover on wage premiums are symmetric across different types of moves and of roughly similar magnitude, which alleviates some of the concern about sorting.

To further address the concern about sorting based on match effects, we compare the double-fixed-effects model with a model with unrestricted match effects (i.e., separate dummies for each manager-establishment combination). The adjusted R^2 increases, but only by a modest amount. We plot the mean residuals for the two-way fixed effects model by manager and establishment decile in ??, and in each cell the mean residuals are small and less than 1%, except for some larger deviations of 1-2% for the softest and the toughest managers. This suggests that the basic specification with additively separable manager and establishment effects provides a good characterization of the data.

To test the joint significance of manager fixed effects, we compare regression (??) in Step 2 with a regression model with only establishment and year fixed effects and time-varying establishment controls. Including manager fixed effects increases the adjusted R^2 of the estimated models from 0.50 to 0.87. The F statistic is close to 10, which allows us to reject the null hypothesis that manager fixed effects are jointly zero.

We apply several additional tests to see whether managers have different styles of wage setting that are transferrable across firms. First, we follow Bertrand and Schoar (2003) and regress the estimated wage residual (above and beyond any firm effect) at a new employer against the estimated wage residual at the prior employer. The wage residual is from regressing establishment-year fixed effects on year dummies and establishment fixed effects. This directly tests whether manager styles are portable across employers. Using a different sample from Bertrand and Schoar (2003), Fee et al. (2013) found an insignificant relationship using this test despite rejecting the null of zero manager

fixed effects using the F-test. The left figure of ?? is a binscatter plot of wage residual at the first employer against wage residual at the second employer for all managers. There is a significant positive relationship between the two wage residuals (t -statistic = 13.2), confirming that individual managers display durable styles that they transfer across employers.

Second, we test whether managers actively affect wage levels at their firms. An alternative interpretation would be that a manager may by coincidence be involved in a period of lower wages at her firm, and would be perceived as having a style of setting low wages although that manager does not actively influence wages. Under this alternative interpretation, we would see lower wages in the firm right before the manager joins the firm. In the right figure of ??, we regress the wage residual at the second employer against the average wage residual of three years preceding the manager’s arrival at the first employer. We find a nearly zero relationship between the residuals. This result is consistent with the interpretation that managers actively shape the wage levels at their firms.

What are the characteristics of soft managers? In ?? we regress the estimated manager fixed effects on the characteristics of the managers. Women are on average more generous in wage setting, whereas older and more experienced managers tend to be less generous. Managers who are married are also less generous in wage setting than unmarried managers. The earnings of the managers themselves are negatively correlated with generosity in wage setting, suggesting that toughness in wage setting may be valued in the managerial labor market.

4.2 Event Study of Exogenous Manager Departures

The tests above show that manager styles are transferrable across employers. They do not, however, rule out the possibility that managers are sorted to firms based on unobservable shocks to the firm. For example, firms that change managers may also make a simultaneous set of major changes, like investment, financing, or hiring decisions.

Motivated by these concerns, we conduct an event study of exogenous manager departures due to retirement. We identify natural retirements of managers based on age. The prior literature establishes that CEOs often retire, either voluntarily or because of their employer’s retirement policies, once they reach certain age thresholds (Jenter and Lewellen 2015). Based on this observation, we identify a set of departures where the manager leaves the firm at an age greater than 62 and remain unemployed thereafter (this also includes manager departures due to health reasons or death). While manager retirements may have been anticipated by the board or firm owners, this offers a test for the presence of style effects resulting from a new draw from the style distribution in the absence of

major organizational stress.

We re-estimate the manager fixed effects for all managers using data *outside* the four-year window used for the event studies. The reason is that manager fixed effects are measured with error, and if we defined a soft manager as one who happened to have positive wage shocks in the firm within the event study, we would have found a spurious relationship between wages and the exit of such a manager even if she had no causal impact on wages. ?? plots the impact of the retirement of managers on real wages. The left figure includes retirements of managers with manager FE in the top quartile, and the bottom figure includes retirements of managers with manager FE in the lowest quartile. We find that retirements of a high-FE (or low-FE) manager lead to a decrease (or an increase) in real wages of 3-5%. This supports our interpretation that manager styles play a causal role in wages.

One caveat of the analysis is that firms may experience major changes or distress when powerful managers retire. ?? shows that when a soft manager or a tough manager leaves, firms experience very little change in productivity despite large changes in wages. This suggests that the average productivity of managers paying very high or very low wages is close to the average productivity of all managers.

4.3 Correlations with Manager Productivity and Leverage

The previous section documents a wide degree of heterogeneity in the way managers set wages. In this section we investigate whether managers' wage effects reflect other underlying differences in manager practices. For example, do managers who have higher productivity also pay higher wages? Or do managers who are more financially aggressive pay higher wages?

To answer these questions, we analyze the correlation between manager fixed effects in wages and manager fixed effects in productivity and other firm policies. We estimate manager styles in other dimensions using a manager fixed effects approach similar to equation (??). We use two measures of firm productivity: total factor productivity (TFP) and log value added per worker.²³ To measure TFP, we follow Schoar (2002) and estimate the following OLS regression separately for each three-digit standard industrial classification industry and year:

$$\log(y_{jkt}) = \alpha_{kt} + \beta_{kt} \ln(K_{jkt}) + \gamma_{kt} \ln(L_{jkt}) + \delta_{kt} \ln(M_{jkt}) + \epsilon_{jkt} \quad (8)$$

²³We did not use profits because there are many negative values and the size of the connected set becomes much smaller. Since log value-added per worker and TFP are only available at the firm level and not the establishment level, we keep only single-establishment firms and multi-establishment firms for which we can clearly identify the CEO in the estimation sample.

where y_{jkt} is total value of shipments of firm j in industry k at year t , L_{jkt} is the number of full-time equivalents, K_{jkt} is the value of the capital stock, and M_{jkt} is the cost of material shipments. The specification allows for different factor intensities across industries, and since we measure labor using only labor quantity and not wage bill, the wage level does not affect the estimation of TFP directly.

Theoretically the correlation between wage fixed effects and productivity fixed effects is ambiguous. On the one hand, higher wages may improve the productivity of the firm by encouraging more efforts or the accumulation of firm-specific human capital among workers (Akerlof 1982; Kahneman, Knetsch, and Thaler 1986). The high level of wages represents an implicit contract, and a breakdown in trust between employers and employees may lead to employee retaliation and huge losses in productivity, as is shown by the case in Krueger and Mas (2004). High wages also help firms to attract and retain high-skilled and productive workers. On the other hand, if soft managers have higher agency costs and thus prefer to enjoy a quiet life with the workers, they are also likely to enjoy a quiet life in other corporate decisions, which can be detrimental to productivity (Bertrand and Mullainathan 2003; Gormley and Matsa 2016).

?? presents a binscatter plot of managers' productivity fixed effects against managers' wage fixed effects for all managers in Denmark. For both measures of TFP and log value-added per worker, the absolute value of the correlation is lower than 0.01, and therefore we do not find evidence that soft managers systematically outperform or underperform tough managers in terms of productivity.

The wage differences between managers may be also due to differences in risk-taking. For example, some managers may take fewer risks and provide greater job security to workers, which allows them to pay lower wages (Sraer and Thesmar 2010). Debt may also be used in bargaining with workers and their unions to keep wages down (Matsa 2010). ?? shows that there is no correlation between manager effects in wage premiums and manager effects in leverage. Consistent with theory predictions, we find that soft managers hire less and have lower quit rates. Interestingly, there is a non-monotonic relationship between the wage fixed effects and the worker quality fixed effects, and managers who are neither too soft nor too tough have the most skilled workers.

Lastly, one explanation for soft managers is that they are soft bargainers and are incapable of keeping the wage costs down. If this is true, we would expect that soft managers also have higher input costs. However, ?? shows that managers who pay higher wages do not have higher input costs. This is consistent with the finding that soft managers do not have lower value added per worker.

Overall the evidence in this section indicates that manager-specific effect on wages is one important component of manager style that is uncorrelated with manager productivity and financial

policy, and can have a large impact on firms' profitability and human capital.

4.4 Industry Concentration and Manager Discretion

Our theoretical framework shows that in less competitive industries, firms have higher profits, and there is more room for managerial discretion in wage setting. Similarly, Giroud and Mueller (2010) find that managers in less competitive industries tend to enjoy a “quiet life” more. Dube, Manning and Naidu (2018) show that firms fail to set optimal wages when they have market power. In this section we investigate whether competition reduces manager discretion in wage setting.

To examine the role of industry concentration, we calculate the Herfindahl-Hirschman Index using data on sales for the universe of Danish firms and divide the 127 three-digit industries into two groups based on the average HHI over all years. For example, industries with high concentration include financial intermediaries, research and development, and production of meat, and industries with low concentration include the sale and repair of motor vehicles, hotels and restaurants, and architecture and engineering.

Consistent with our theoretical prediction, we find that there are more soft managers in highly concentrated industries. ?? shows the 25th, 50th, and 75th percentiles of the distribution of manager wage fixed effects in unconcentrated industries (HHIs less than 1500), moderately concentrated industries (HHIs between 1500 and 2500), and highly concentrated industries (HHIs above 2500). The median manager fixed effect in highly concentrated industries is 3.5% higher than the median manager fixed effect in unconcentrated industries, and the range from the 25th to 75th percentiles is 37% wider. This suggests that product market competition mitigates agency costs and eliminates the upper tail of soft managers.

5 Manager Styles and Wage Changes Following M&As

In this section we first show that M&As reduce wages but not employment at target establishments. We then show that this occurs because mergers target and replace soft managers at target firms, and that this rent extraction channel accounts for a major part of shareholder gains in M&As.

5.1 The Effect of M&As on Wages and Employment

5.1.1 Empirical strategy

To analyze the impact of mergers, we implement a dynamic difference-in-differences design in which we compare target establishments in merger transactions to similar firms that did not take part in a merger or acquisition.

We select one control establishment for each target establishment such that the comparison establishment did not experience any M&A transactions but had lagged characteristics similar to the target establishment. We implement a matched sampling procedure: for every target establishment in the year right before the merger, we select a comparison establishment similar to the target establishment in the same year. This approach is motivated by Rosenbaum and Rubin (1985) and Imbens and Rubin (2015, chapter 15), who describe how matched sampling can be used to find a comparison group of similar size and with similar observed characteristics as the treatment group. For each target establishment acquired in year t , we select a control establishment that did not involve any acquisition in the sample period and satisfied the following criteria in year $t - 1$:

- It belongs to the same two-digit sector as the target;
- It is located in the same geographical region²⁴ as the target;
- It is in the same quintile of employment and average establishment wage as the target.

For 92.5% of the target establishments we can find at least one comparison establishment that satisfies the three criteria above. When multiple matches for a target establishment are found, we select the comparison establishment with the closest propensity score calculated based on a rich set of establishment characteristics.²⁵ Each control establishment is matched to at most one target establishment in every year, but can be matched to multiple targets in different years. Later in this section, we show the robustness of our results to alternative matching strategies.

The key identifying assumption is that workers' wages in target and control establishments would have followed parallel trends in $\tau > 0$ if no merger had occurred in the treated establishment. Admittedly mergers and acquisitions are not exogenous events, but our estimation strategy is still valid if the target is selected based on the level of wages or productivity. Potential threats to

²⁴There are five geographical regions in Denmark, and each geographical region is close to a commuting zone in the US: it usually takes less than two hours to travel between places within a geographical region.

²⁵The propensity score is estimated using a linear probability model, and the independent variables include log employment, average log wage, establishment age, log value-added per worker, log sales per worker, share of workers with higher education, share of workers with vocational training, share of female workers, average age and experience of workers, as well as industry and year dummies.

identification would be unobserved shocks that affect both the outcomes and the timing of merger in the treated establishments. For instance, acquirer firms could target firms on the verge of wage reductions. Importantly, we do *not* match on pre-merger employment or wage growth, as the three criteria above and all covariates used in estimating the propensity score are measured for the year before the merger, so the pre-trends can be used to evaluate the common trends assumption. As we will see, the fact that wage decline occurs precisely at the moment of merger mitigates this concern. As a robustness check we also match target establishments to controls two years before the merger and get similar results.²⁶ Table A1 shows that control establishments and target establishments had similar characteristics one year before the merger.

We examine the effects of failed mergers to further shed light on the causal effect of mergers. Failed mergers are mergers in the Zephyr M&A data set that were announced but were eventually withdrawn. We exclude target firms in cancelled deals that got eventually acquired by a different firm, and end up with a sample of 365 failed mergers. We match each establishment in this sample to a control establishment and compare their wage and employment trends over time.

5.1.2 M&As reduce wages but not employment at target establishments

We start by looking at how employment and wages change at the establishment level by estimating the following event-study framework:

$$y_{jt} = \alpha_j + \gamma_t + \sum_{\tau=-3}^5 \lambda_{\tau} D_{jt}(\tau) + \sum_{\tau=-3}^5 \delta_{\tau} D_{jt}(\tau) \times Target_j + \epsilon_{it} \quad (9)$$

where y_{jt} is outcome variable at establishment j in year t . We denote $\tau = t - d$ as the number of years relative to the merger occurring in year d . The model includes establishment fixed effects, α_j , calendar year fixed effects, γ_t , and leads and lags around event time $D_{jt}(\tau)$. $Target_j$ is a dummy variable indicating whether establishment j is a target or control. The coefficients of interest are δ_{τ} , which capture the effect of a merger in year τ in the target establishments and are normalized to zero in $\tau = -1$. The standard errors are clustered at the establishment level. The observations are

²⁶One might be concerned that our approach would violate the stable unit treatment value assumption (SUTVA): applying the treatment to one unit has no effect on outcomes at other units. This assumption fails if, for example, treatment effects on target establishments systematically alter equilibrium wages at control establishments. Given that we only restrict the control establishments to being in the same two-digit industry and region, both of which are sufficiently broad, any one firm usually constitutes a very small fraction of the two-digit industry or geographical region. Furthermore, we obtain similar results when selecting control establishments outside the target's industry and region.

weighted by employment.²⁷ The event study approach allows us to estimate the dynamic treatment effects of mergers on establishment outcomes over time as well as to use the effects in the pre-period to evaluate the common trends assumption.

??(a) shows the changes in employment at target establishments. Although employment declines right after merger, it reverts to the original level after three years. The initial drop in employment is less than 3%, and there is little employment decline after year 0. M&As also have little effect on establishment exits (??). In Appendix ?? we show that following a merger both job inflows and job outflows increase, and cancel out each other. ??(b) shows that target establishments' earnings per worker (EPW) decline by 2-3%, and this decline persists over time.

To control for changes in worker composition, we look at the wage changes of workers remaining in the target establishments following acquisitions. We estimate the following equation at the worker level:

$$w_{ijt} = \alpha_{ij} + \gamma_t + \sum_{\tau=-3}^5 \lambda_{\tau} D_{it}(\tau) + \sum_{\tau=-3}^5 \delta_{\tau} D_{it}(\tau) \times Target_j + \beta X_{it} + \epsilon_{it} \quad (10)$$

The model includes job spell fixed effects, α_{ij} . $Target_i$ equals one if worker i 's employer at $\tau = -1$ is acquired. Included in the control covariates X_{it} are experience and its interactions with gender and education level to control for changes in productivity of workers. The coefficients of interest are δ_{τ} , which capture the effect of the merger on wages of stayers over time.

?? shows the effects of mergers on stayers' wages. It is reassuring for our design that there is no pre-trend before mergers. After merger, the wage growth of workers remaining in target establishments declines by nearly 2 percentage points compared to employees remaining in control establishments. These differences are persistent, lasting for at least five years after the merger. ??(b) shows that workers staying in target establishments lose 3-4% of their initial annual earnings. ??(c) shows that hourly wages also fall by 2% after mergers, suggesting that the reduction in annual earnings is not driven by a reduction in hours worked. We use the same matching method to estimate wage changes at acquirer firms, and find that workers in acquiring firms do not experience any wage cuts after mergers (??).

How big is this effect? Since real wages grow by 1% per year on average, a wage decline of 2% means that an average worker in the target firm has zero real wage growth during the first two years after the merger (between $\tau = 0$ and $\tau = 2$).²⁸ Assuming that the loss of wage premium is

²⁷This is to ensure that the treatment effects are comparable to the worker-level regressions. The weight is the average employment during the three years before merger, and is therefore fixed for each unit.

²⁸The downward rigidity of nominal wages may also explain why real wages keep falling for two to three years after the merger, since some groups may experience a real wage decline of 5-6%, which is about two to three years of

permanent, and that careers last 20 years and the discount rate is 5 percent, a 2% wage decline implies a loss in present discounted value equal to 26% of annual earnings.

The wage effects on staying workers are heterogeneous among worker groups. To assess heterogeneity of treatment effects, we estimate variations of equation (??), adding interactions of worker covariates with period dummies, as well as interactions of covariates with period dummies and treatment. ?? shows that young and low-skilled workers experience the largest wage declines after a merger.

5.1.3 Robustness

The main challenge in interpreting wage effects on stayers is that their wage declines may be due to differential selection of who stays. For example, if workers with more negative future wage changes are more likely to leave the firm, then estimates of wage changes would be upward biased. First, we show that all initially employed workers at the target firm experience a wage decline of 2-3% regardless of whether they stay at the firm (??). Second, Table 3 shows that the increase in the departure rate is quite uniform along the wage distribution.²⁹ Third, ?? shows that the average worker quality at target establishments measured by worker fixed effects³⁰ does not change significantly after mergers. Finally, we use the trimming approach in Lee (2009) to bound the effects of selection without imposing any assumptions on which workers would stay. In particular, given that the proportion of workers staying in target establishments is smaller than the proportion of workers staying in control establishments in each period, we trim the sample of workers staying in the control establishments such that the proportion of staying workers is the same for targets and controls. We then estimate an upper (or lower) bound of the unbiased treatment effect by trimming the upper (or lower) part of the distribution of wage changes among remaining workers in control establishments.³¹ Panel C of Table 4 shows that the upper bounds of wage effects are still negative for four years after merger, and the entire confidence interval is below zero for the second year after nominal wage growth.

²⁹The only exception is that workers in the highest wage quartile are slightly more likely to leave after mergers. If anything, this would lead to a downward bias in our estimates of wage declines for staying workers, because workers in top wage quartile usually have more experience and slower wage growth. Also, as we will show in the next subsection, workers in lower wage quartiles experience the largest wage cuts following mergers, so the negative wage effect is even stronger when we exclude the highest wage quartile.

³⁰The estimated worker fixed effects arise from wage regressions with both establishment fixed effects and worker fixed effects as in Abowd, Kramarz, and Margolis (1999).

³¹The only assumption is monotonicity of selection, which says that workers who leave in target establishments will also leave in control establishments. Since nearly all coefficients in Table 3 are positive, meaning that all subgroups of workers experience an increase in the departure rate in target establishments, the monotonicity assumption is not violated here.

merger. This evidence indicates that the bias due to differential selection of staying workers is likely to be small and cannot explain all of the negative effects on staying workers’ wages.

As shown in [Table 4](#), we conduct a series of robustness tests on the wage results. We define “short-run effect” to be the effect of mergers in one year after the merger (δ_1), and “long-run effect” to be the average effect in the five years after the merger ($\frac{1}{5} \sum_{\tau=1}^5 \delta_\tau$). Panel A presents the wage effects for staying workers, and Panel B presents the wage effects for all initially employed workers. To address the concern that the differences between target and control establishments are driven by different industry or occupation structures, in Columns 2 and 3, we add industry-by-year fixed effects and occupation-by-year fixed effects, and get similar wage effects. In Column 4 we allow the treatment and control establishments to have different linear wage trends, and the wage effects become even more negative. In Column 5 we control for labor productivity at the firm level, and wage declines remain significant. In Columns 6 and 7, we run our regressions separately for years before and after 2004. Although post-merger wage declines are slightly smaller post-2004, all the effects remain significant in both periods, and not statistically different across subperiods.

We show the effects of failed mergers in [Appendix ??](#). The wage effects for mergers that were never completed are not statistically significant, suggesting that the observed wage cuts following mergers are not due to unobserved heterogeneous wage trends between targets and controls.

We obtain similar results using various matching estimators, reported in [Appendix Table A4](#). These include variations of the baseline matching estimators in which firms were matched: to firms outside their industry and region; at two years before the merger date; and to two control firms for each treated firm based on propensity score. In addition, we use nonparametric matching as in Davis et al. (2014) and find that only target establishments and not control establishments experience wage declines. The results still hold when we compare targets to a synthetic control group constructed using only information up to two years before the merger ([Appendix ??](#)). Details of these estimators are in [Appendix A.1](#).

5.2 Mergers Target Firms with Soft Managers

Our simple theory model predicts that firms with soft managers are more likely to become acquisition targets. Using the manager fixed effects estimated above, we test whether target establishments have softer managers. We estimate the regression

$$\hat{\lambda}_{mjt} = \gamma_1 Target_{jt} + \beta X_{jt} + \varepsilon_{jt} \quad (11)$$

where $\hat{\lambda}$ is manager fixed effects estimated from regression (??) and $Target_{jt}$ is a dummy variable indicating whether the establishment is acquired within the next three years, and we control for time-varying establishment characteristics as well as industry-year and region-year fixed effects. The regression is weighted by the standard error of the estimated manager fixed effects.

Column 1 in Table 5 shows that manager fixed effects at target establishments are 1.7% higher than other establishments. This means that managers in target establishments pay workers 1.7% more than managers in comparable establishments. It is important to note that we exclude all post-merger observations of the target establishments when estimating manager fixed effects, so any change in wages after mergers does not enter into the estimated manager fixed effects. Nonetheless, the magnitude of wage decline after mergers in the event studies is very close to the premium of manager fixed effects at target establishments, suggesting that the wage cut represents the loss of wage premiums due to soft managers.

We then test whether target establishments have higher wage premiums than establishments in the same industry and local labor market and with similar productivity by estimating:

$$\hat{\psi}_{jt} = \gamma_1 Target_{jt} + \beta X_{jt} + \varepsilon_{jt} \quad (12)$$

where $\hat{\psi}$ is the establishment-year fixed effect from equation (??). Column 2 in Table 5 shows that target establishments pay workers 2.3% higher wages on average conditional on productivity, industry, and region. According to equation (??), the higher wage premium could be due to a higher establishment-specific component γ_j (e.g., amenities), or higher manager-specific component λ_m , or higher error term ε_{jt} . Both establishment-year fixed effects and manager fixed effects are about 2% higher in target establishments,³² implying that the majority of the wage premium is due to soft managers. In other words, target establishments pay workers higher wages because they have managers who actively implement a high-wage policy, and therefore the wage premium can be eliminated when the target firms replace their managers.

Are managers of acquiring firms softer or tougher? Since the acquirers are targeting firms with soft managers, it is very likely that the acquirers themselves have tough managers. Consistent with this idea, Panel B of Table 5 shows that acquirers have on average 0.8% lower manager fixed effects and 1.1% lower wage premiums than comparable firms.

³²One might be concerned that the estimation sample is different in Column 1 and Column 2 of Table 5, since the connected set of managers where manager fixed effects can be identified contains fewer establishments. We estimate the regression in Column 2 on the sample where manager fixed effects can be identified, and results are similar, with a coefficient of 0.025.

In [Appendix Table A2](#) we look at what types of firms have a higher propensity to be acquired. Column 1 shows that establishments with higher average wages are more likely to be a target. Higher wages may be due to higher wage premiums or due to more high-skilled workers. Column 2 and Column 3 show that a higher establishment wage premium is associated with a higher likelihood of being acquired, while a more skilled workforce (measured by average worker fixed effects) is not. A one-standard-deviation increase in establishment fixed effects increases the likelihood of being a target by approximately 0.1%, which is a 14% increase over the average probability of 0.7%. The establishment wage premium contains manager fixed effects, establishment fixed effects, and productivity. We further show that establishments with higher manager fixed effects are also more likely to be acquired (Column 4), but establishments with higher establishment fixed effects are not (Column 5), and establishments with higher productivity are less likely to be acquired (Column 6). Therefore the positive correlation between wage premiums and propensity to be acquired is driven by manager fixed effects. Column 7 shows that despite the positive effect of wage level, the change in wage levels does not predict acquisitions. This is consistent with the absence of pre-trends in wages before mergers. Although we should be cautious about interpreting these effects as causal, the evidence on propensity to be acquired supports our hypothesis that establishments that have soft managers and pay higher wages to workers are more likely to be targeted by acquirers.

5.3 Manager Turnover Around Mergers

Column 6 of [Table 3](#) shows that manager turnover increases significantly following mergers: whereas the departure rate of workers increases by 1% on average, the departure rate of managers increases by over 7%. This is consistent with Hartzell, Ofek, and Yermack (2004), who also found high turnover rates for CEOs at the time of acquisition and for several years thereafter. In our sample, about 43% of the managers in target firms joined different firms within three years after the merger. In contrast, only 20% of managers joined other firms within three years at control firms and only 21% of managers joined other firms within three years at firms that are neither targets nor controls. Martin and McConnell (1991) show that the high turnover is due to non-value-maximizing behaviors of managers at target firms: prior to the merger, target firms, which replaced their managers after the merger, underperformed target firms that did not replace their managers after merger.

We examine whether firms are more likely to replace soft managers after mergers by comparing manager turnover based on the estimated manager fixed effects in wages. In ?? the two solid lines plot manager turnover rates for target establishments with high or low manager fixed effects; for

comparison, the two dashed lines plot manager turnover rates for control establishments with high or low manager fixed effects. By year 5, target establishments with soft managers are 8 percentage points more likely to replace the managers than target establishments with tough managers, accounting for about 40% of the difference in manager turnover rates between target and control establishments. This indicates that managers' style in wage-setting is a major factor in deciding whether they remain in the firm after mergers. By contrast, for control establishments, the difference in turnover rates between soft managers and tough managers is almost negligible. Evidence therefore suggests that mergers and acquisitions are a key corrective mechanism for eliminating soft managers.

5.4 Are Wage Cuts Due to Replacing Soft Managers?

In our model, mergers reduce wages at target firms because they remove soft managers. Therefore we would expect wage cuts to be concentrated in target establishments with soft managers. To test this, we modify our empirical specification from Section 5.1, so that we can compare wage changes based on ex ante manager characteristics. We estimate the following equation:

$$\begin{aligned}
w_{ijt} = & \sum_{\tau=-3}^5 \lambda_{\tau} D_{ijt}(\tau) + \sum_{\tau=-3}^5 \eta_{\tau} D_{ijt}(\tau) \times \text{SoftManager}_j + \sum_{\tau=-3}^5 \delta_{\tau} D_{ijt}(\tau) \times MA_j \times \text{SoftManager}_j \\
& + \sum_{\tau=-3}^5 \gamma_{\tau} D_{ijt}(\tau) \times MA_j \times (1 - \text{SoftManager}_j) + \alpha_{ij} + \beta X_{ijt} + \mu_t + \epsilon_{it}
\end{aligned} \tag{13}$$

where we include interactions between treatment status, period dummies, and a dummy indicating whether an establishment has soft managers before a merger. We rematch the target establishments to control establishments such that target establishments and control establishments are in the same quartile of manager fixed effects. We define an establishment as *SoftManager*=1 if its manager fixed effect is above the median in year -1. The coefficients γ_{τ} indicate the treatment effects for target establishments with tough managers, and coefficients δ_{τ} indicate the treatment effects for target establishments with soft managers.³³

Table A.1(a) presents the results. We find that almost all of the wage cut is concentrated in establishments with soft managers. Workers in target establishments with soft managers experience a wage cut of 3-5%, whereas workers in target establishments with tough managers experience a modest

³³One might be concerned that the results are driven by mean reversion. Since we control for *SoftManager* dummy interacted with period dummies, they will absorb the effects of mean reversion.

and statistically insignificant wage cut of less than 1%.³⁴

Since manager fixed effects can be estimated only for firms in the largest connected set, we also use excess wage premium in a firm as a proxy for the manager style. The excess wage premium is defined as the residual from regressing the estimated establishment-year fixed effects ($\hat{\psi}_{jt}$ in equation ??) on productivity and on industry-year and region-year fixed effects. It can be estimated for all establishments, regardless of whether they are in the largest connected set linked by manager mobility. The excess wage premium measures how much a firm overpays its workers relative to a comparable firm. As shown in Table 5, the higher excess wage premium in target establishments is mostly due to soft managers. We define an establishment as *High Wage* if its excess wage premium is above the median in year -1. ??(b) shows that only workers in target establishments with high excess wage premiums experience wage declines after mergers.

To further investigate whether wage decline after mergers is entirely due to replacing soft managers, we plot the wage changes by establishment manager effects and whether the manager is replaced by year 3 in ??(c). Only workers in establishments that had soft managers and replaced those managers experience large wage declines. We also show in ?? that while wages decline when a firm with a soft manager is acquired by a firm with a tough manager, wages do not increase when a firm with a tough manager is acquired by a firm with a soft manager. This indicates that acquirers take over firms with soft managers and replace them with tougher managers to extract rents, but wages change very little when target firms already have tough managers. The magnitude of wage changes when replacing a soft manager after acquisition is close to the magnitude of wage changes when a manager whose manager FE is in the top quartile retires (Section 4.2).

Consistent with our theory, Appendix ?? shows that employment tends to increase in target establishments with soft managers. Appendix ?? shows that the target firms with soft managers experience a large increase in job inflows following mergers. This occurs because, by replacing soft managers and lowering labor costs, firms expand production and hire more workers.³⁵

³⁴To account for measurement error in the estimated manager fixed effects, we use a split-sample instrumental variables approach. We split the sample by even and odd years, and estimate manager fixed effects separately for each subsample. The estimation errors are uncorrelated across the two sets of estimates. For each subsample we define soft managers as managers with fixed effects above the median. We then instrument the soft manager dummy from one subsample with the soft manager dummy from the other subsample, and vice versa. This approach yields similar results although the estimates are noisier (??).

³⁵Appendix ?? shows that the average worker quality of the newly hired workers increases at target establishments with soft managers. Although we do not model heterogeneous worker types in our theoretical framework, this suggests that reducing wage premiums due to soft managers may also allow firms to hire higher-quality workers.

5.5 Gains from Mergers

How much value does rent extraction create in merger transactions? To estimate the impact of mergers on the profitability of the combined firm, we combine the balance sheets of each target firm with its acquirer firm before the merger and track the combined performance over time. We compare the return on assets (ROA) of the combined firm³⁶ with firms in the same industry over time using an event study approach. ROA is calculated as profits before taxes and interests divided by total assets. ?? plots the change in ROA of combined companies over time and shows that merged companies experienced an average increase in ROA of 1 to 1.5 percentage points within five years after the merger.

We then calculate how much of the increase in ROA can be attributed to rent extraction. As shown in ??(c), workers experience large wage declines only at target firms that replace soft managers. Suppose that acquirer firms replace the target firm managers with above-average manager effects with managers with average manager effects, and do not change the wage policy in the acquirer firms or target firms with below-median manager effects. From equation (??), by the envelope theorem,³⁷ the impact of a change in manager styles on firm profits is $w_j L_j \Delta(\beta \phi_m)$, where $w_j L_j$ is the wage bill of the target firm, and $\beta \phi_m$ is the identified manager fixed effects. Therefore the impact of replacing soft managers on ROA is:

$$\Delta = p(\beta \phi_{target} - \beta \bar{\phi})^+ \frac{(wL)_{target}}{A_{acquirer} + A_{target}} \quad (14)$$

where p is the probability of replacing a soft manager (which equals 0.56, according to ??), the second part is the positive part of the difference between the target's manager fixed effect and the average manager fixed effect, and the last part is the wage bill of the target divided by the total assets of the combined firm. To adjust for the estimation error in manager fixed effects, we use a simple empirical shrinkage procedure from the empirical Bayes literature and shrink the estimates toward the mean.³⁸ The relative weight that the estimate gets in the convex combination varies inversely with the noise of the estimate (which is based on the standard error of the manager fixed effect).

³⁶We did not use log profits since there are a significant number of observations with negative profits. To isolate the changes in profits from changes in asset levels, we use pre-merger total assets as denominators when calculating ROAs. Using contemporary assets as denominators does not alter the results.

³⁷Since firms maximize profits with respect to L in the second period, $\frac{\partial \pi}{\partial(\beta \phi_m)} = \frac{\partial w}{\partial(\beta \phi_m)} * L = \frac{\partial(\log w)}{\partial(\beta \phi_m)} * wL \approx wL$.

³⁸The intuition behind this is that when a manager's fixed effect is estimated to be far above (below) average, it is likely to suffer from a positive (negative) estimation error. Therefore, the expected level of manager fixed effect, given the estimated manager fixed effect, is a convex combination of the estimate and the mean of the underlying process.

The sample average of the term Δ among all mergers is 0.63 percentage points.³⁹ This indicates that 42% to 63% of the increase in profitability of 1 to 1.5 percentage points following mergers come from the rent extraction channel. The remaining gains are due to efficiency improvements and monopoly power, or changes at the acquirer’s establishments. Under the alternative scenario that acquirers replace all soft managers at the target firms with managers similar to the managers at the acquirer firms, the average impact on ROA is even bigger (0.72 percentage points), since acquirers on average have tougher-than-average managers. Given that only two-thirds of the target firms have above-average manager fixed effects, this suggests that many of these mergers would have created no value or even negative value if no rents were extracted from the workers.

An alternative measure for gains from mergers is abnormal stock market returns. Following Bradley, Desai, and Kim (1988), we compute the portfolio cumulative abnormal returns (PCAR), which is the cumulative abnormal return to a value-weighted portfolio of the target and acquirer, over an 11-day event window around the merger announcement. The average PCAR is 2.1%, which is smaller than the average percent increase in ROA (6.6%).⁴⁰ This suggests that the higher profits following mergers are partially reflected in the stock prices and confirms that rent extraction explains a large part of the increase in shareholder value.⁴¹

5.6 Industry Concentration and Rent-Extracting Mergers

Some recent studies highlight the interactive effects of industry concentration (as a proxy for product competition) and corporate governance. Giroud and Mueller (2010) show that anti-takeover laws have a more negative impact on shareholder value in non-competitive industries, suggesting that takeover pressure and product market competition work as substitutes. Brav, Jiang, and Kim (2015) find that hedge fund activism improves real productivity only in competitive industries and focuses on improving financial structure and agency conflicts in noncompetitive industries. Our theoretical framework suggests that the rent extraction channel of M&As and product market competition are substitutes: in more concentrated industries, firms have higher profits, and there is more room for managerial discretion in setting wages. Accordingly, in concentrated industries, target firms have softer managers, and M&As will lead to larger wage declines.

³⁹Details of the calculations are in [Table A3](#).

⁴⁰The 6.6% is calculated by dividing the average increase in ROA by the mean ROA of 19.1 percentage points. We calculate ROAs for 87 mergers in the SDC, and the small sample of listed firms precludes a one-to-one match to our worker-level data sets.

⁴¹Since cutting labor costs is less uncertain than investing in productivity improvements, rent extraction may account for a larger share of the stock price increase if future gains from rent extraction are discounted less than other types of gains from mergers.

To test whether target firms have softer managers in more concentrated industries, we estimate the following extended version of equation (??):

$$\hat{\lambda}_{mjt} = \gamma_1 Target_{jt} + \gamma_2 Target_{jt} \times HighConcentration_j + \beta X_{jt} + \varepsilon_{jt} \quad (15)$$

where *HighConcentration_j* is a dummy variable that equals one if firm *j* is in an industry with HHIs over 1000.⁴² Column 3 of Table 5 shows that manager fixed effects are significantly larger only in concentrated industries, which is consistent with our theoretical prediction.

?? shows large wage declines following mergers in concentrated industries, and no significant wage changes in competitive industries. In concentrated industries, the negative wage effects from rent extraction dominate the positive wage effects from real productivity increases and market power, whereas the wage effects of all channels are balanced for mergers in competitive industries. This can be also seen from the effects on employment in ??, where mergers lead to slightly more positive employment changes in more concentrated industries due to rent extraction.⁴³

6 Robustness

6.1 Monopsony Power and Labor Market Concentration

A growing literature shows that greater concentration in the labor market leads to lower wages (Azar et al. 2017; Benmelech et al. 2018). In a classic monopsony model, a bigger firm is a larger buyer in the labor market and hence has more market power and can pay lower wages. However, monopsony power cannot explain why target firms pay higher wages ex ante: if the acquirer firm pays significantly lower wages than the target, there is presumably little competitive pressure from the acquirer firm on the target’s wages.

To further test whether increases in monopsony power explain the wage declines, we take several approaches to construct measures of monopsony power created by mergers. The first approach is to measure changes in labor market concentration due to mergers. We first use municipalities to approximate local labor markets.⁴⁴ For nearly half of the mergers in our sample, the acquirer firm is not in the same municipality as the target. The top left figure of ?? shows that cross-city

⁴²1000 is close to the median HHI for all 127 industries. For example, the Horizontal Merger Guidelines classify markets as unconcentrated (HHI less than 1,500); moderately concentrated (HHI between 1,500 and 2,500); and highly concentrated (HHI above 2,500).

⁴³We would expect the opposite to be true if mergers in concentrated industries are primarily driven by product market power.

⁴⁴Over 75% of the workers have worked only in one municipality, and over 90% of the workers have worked in no more than two municipalities.

mergers seem to lead to even larger wage cuts than same-city mergers. An alternative measure of labor market is by occupation and region (Azar et al. 2017). We treat each four-digit occupation code combined with geographical region as a separate labor market, and calculate the change in the Herfindahl-Hirschman Index (HHI) induced by the merger. For about 15% of the workers in target firms, mergers lead to an increase in HHI of 100 points or more, which is the US government’s threshold for scrutinizing mergers (FTC/DOC, 2010). The top right figure of ?? shows that workers above the threshold and workers below the threshold experience almost identical wage declines.

The second approach is to measure the diversion ratio, which is the fraction of target firm employees that would move to acquirer firms when the target firm lowers wages (Naidu et al. 2018). We measure it by the fraction of job movers from target firms who moved to the acquirer firm before the merger. A higher ratio indicates that the acquirer is a more important competitor in the labor market. Only about a quarter of the target firms have positive diversion ratios, and the average diversion ratio is less than 5%, indicating that there is little competition between acquirer and target in the labor market. The bottom figure of ?? shows that mergers have similar wage effects for targets with positive diversion ratios and for targets with zero diversion ratios.

These results suggest that most mergers in our sample do not create large enough monopsony power to significantly suppress wages. While it is still possible that some very large mergers suppress wages by creating monopsony, monopsony power cannot explain the large negative wage effects of mergers in our data.

6.2 Are Mergers Efficiency-Enhancing?

As shown in our theoretical framework, mergers can increase profits by replacing unproductive managers and raising productivity of the target firms. We test whether acquirers target poorly managed firms in the last two columns of Table 5. Column 4 shows that target establishments on average have slightly lower manager fixed effects on TFP than comparable establishments, and acquirers have higher manager fixed effects on TFP, but the differences are not statistically significant. Column 5 looks at manager fixed effects in value added per worker, and the difference between targets and acquirers is almost zero. Therefore we cannot reject that managers at target firms are as productive as the average manager despite setting higher wages and making less profit, which is consistent with the lack of correlation between managers’ productivity and manager’s wage effects shown in Section 4.3. This finding implies that M&As discipline managers paying high wages rather than managers with low productivity.

According to our model, target firms with less productive managers will experience wage increases after mergers. Appendix ?? shows that target firms with less productive managers seem to have more positive wage changes following mergers. Since targets on average have less productive managers than acquirers, replacing unproductive managers (independent of replacing soft managers) will lead to wage increases after mergers, and this positive wage effect is dominated by wage cuts due to rent extraction.

6.3 Monopoly Power

Monopoly power is arguably the most cited motivation for mergers. As shown in our theoretical framework, market power should lead to higher profits and therefore increase wages. However, firms may also engage in “killer acquisitions,” where they acquire product market competitors as a way to reduce their production and to preempt future competition (Cunningham, Ederer, and Ma 2018). The lack of employment declines after mergers suggests that this motive is not very likely to be prevalent in our sample.

To further investigate whether post-merger wage declines can be explained by killer acquisitions, we divide the sample into horizontal mergers, in which the acquirer and target operate in the same industry, and non-horizontal mergers, in which they do not. The first two columns of Table 6 show that horizontal and non-horizontal mergers lead to nearly identical wage cuts. Column 3 and Column 4 show that production and non-production workers experience similar wage declines after mergers. This finding suggests that reduced competition in the product market cannot explain the negative effect of M&As on target firms’ wage premiums.

6.4 Discussion of Alternative Interpretations of Manager Styles

Automation and outsourcing

Since automation has large fixed costs, mergers might create economies of scale and induce more automation (Olsson and Tåg 2016; Ma et al. 2017). Similarly, larger firms are more likely to outsource their non-production activities. Goldschmidt and Schmieder (2017) show that firms outsource their FCSL (food, cleaning, security and logistics) workers to reduce their wage premiums. An alternative explanation for our finding is that tough managers have a greater propensity or capability to automate or outsource, and they use the threat of automating or outsourcing to bargain for lower wages. Increased automation and outsourcing may also reduce the labor demand for routine or FCSL workers and therefore reduce their wages. In Columns 5 to 8 of Table 6, we compare the

effects of mergers on wages of routine and FCSL workers versus on non-routine and non-FCSL workers. We find that non-routine and non-FCSL workers experience larger wage cuts, which does not support the explanation that the threat of automation or outsourcing depresses wages after mergers.

Manager entrenchment and ownership

Cronqvist et al. (2009) show that entrenched CEOs pay workers more, and CEOs who own more cash flow rights in their firms pay workers less. Since we do not have data on managers’ control rights and cash flow rights, we cannot test how manager styles interact with entrenchment and ownership. However, since our identification of manager styles is based on manager mobility across firms, the estimated manager styles would not capture wage effects due to employer-specific entrenchment and ownership.

In Appendix ?? we compare the wage effects of mergers for family firms and nonfamily firms. About 30% of the target firms in our sample are family firms. Following Bennedsen et al. (2007), we classify a firm as a family firm if managers in different years are family members. We find slightly larger wage cuts for family firms following mergers, suggesting that managers may set generous wages even when they are the owners of the firm and there is no agency conflict.

Nonwage benefits

Another interpretation of manager “softness” is compensating differentials for heterogeneous amenities across managers. For example, soft managers may pay higher wages because they provide workers with worse amenities and nonwage benefits (Sorkin 2018). Although we do not directly observe amenities, and therefore cannot identify manager styles concerning amenities separately from manager styles in setting wages, we do find that an important part of nonwage benefits—pension payments—decline by nearly 5% after mergers (Appendix ??), which is consistent with Pontiff, Shleifer, and Weisbach (1990). Anecdotal evidence also suggests that amenities often worsen after mergers.⁴⁵

7 Conclusion

Using a matched employer-employee data set from Denmark and analyzing the universe of M&A transactions from 1995 to 2011, we identify soft managers—managers with a tendency to set higher

⁴⁵For example, 3G Capital is famous for cutting even seemingly tiny employee benefits at the companies it acquires: free beer at AB InBev-owned Budweiser after its merger with SABMiller, free Timbits at Tim Hortons annual general meetings after its merger with Burger King, and free cheese sticks for Kraft employees after its merger with Heinz.

wages—and find that M&As target and replace these soft managers. Rent extraction from target firms with soft managers brings higher profits to the acquirer firms, explaining the majority of the rise in profitability of the merged firm. These findings suggest that rent extraction is a major driver of the market for corporate control and a key source of merger synergies.

Our paper contributes to the growing literature of how managerial biases and misoptimization affect firms’ real outcomes and the aggregate product and labor markets (DellaVigna and Gentzkow 2018; Dube, Manning, and Naidu 2017; Ma, Sraer, and Thesmar 2018). Our results indicate that with increasing market power (De Loecker and Eekhout 2017; Autor et al. 2017; Gutiérrez and Philippon 2017), managers’ nonvalue-maximizing behavior becomes more severe, and market forces and corporate governance practices that regulate manager behaviors become increasingly important. We explore the role of M&As in disciplining managers, but more work is needed to understand other forces driving manager behaviors and their aggregate consequences.

The rent extraction channel provides new insights into the costs and benefits of M&As. On the one hand, acquisitions provide market discipline, without which managers might indulge preferences and reduce profits and productivity were it not for the threat of acquisition (Bertrand and Mullainathan 1999, 2003). On the other hand, we find that manager styles in wage setting are uncorrelated with managers’ productivity and that mergers do not appear to improve managers’ productive efficiency. This suggests that the private gains of M&As to the shareholders of target and acquirer firms may exceed the social gains.

More broadly, our results suggest that ownership and management play an important role in the allocation of rents between shareholders and stakeholders. The financialization of firms, which puts more focus on maximizing shareholder value, may lead to large shifts in how rents are distributed. Studies have shown that targets of private equity buyouts and hedge fund activism experience stagnant or declining wages despite higher productivity (Davis et al. 2014; Brav, Jiang, and Kim 2015), and our evidence shows that in some merger transactions higher profits may be a result of lower wages. Exploring the impact of the rent-seeking components of firm activities on labor markets and how and when financial markets stimulate or alleviate these rent-seeking behaviors is an important area for future research.

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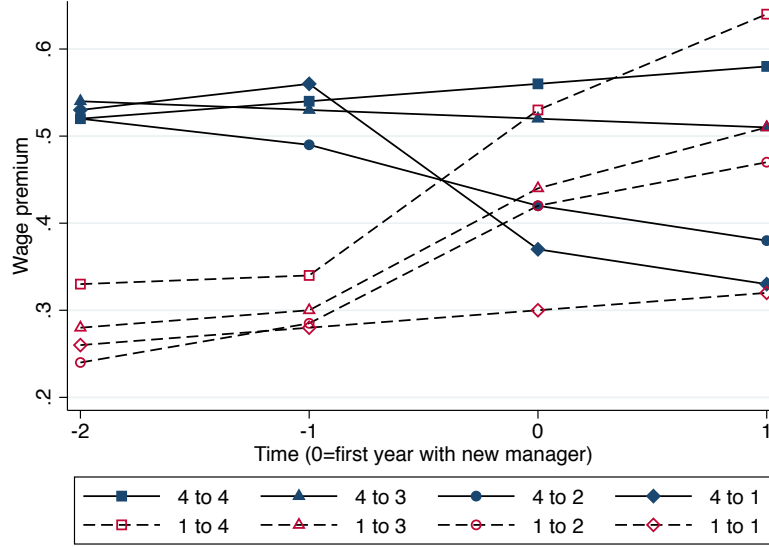
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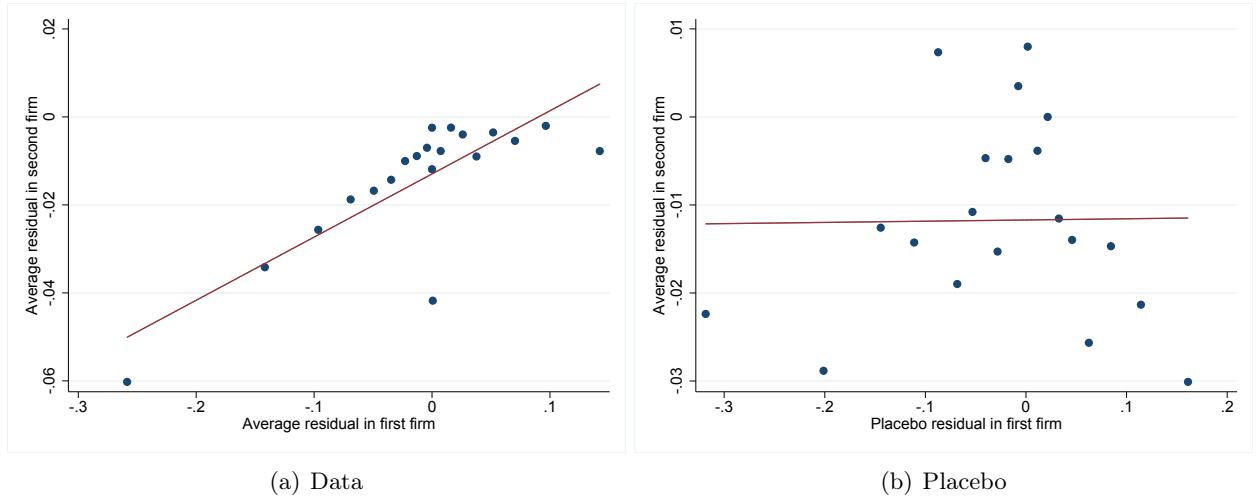
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Figure 1: Mean wage premiums of firms that switch managers classified by quartile of manager effects for departing and entering managers



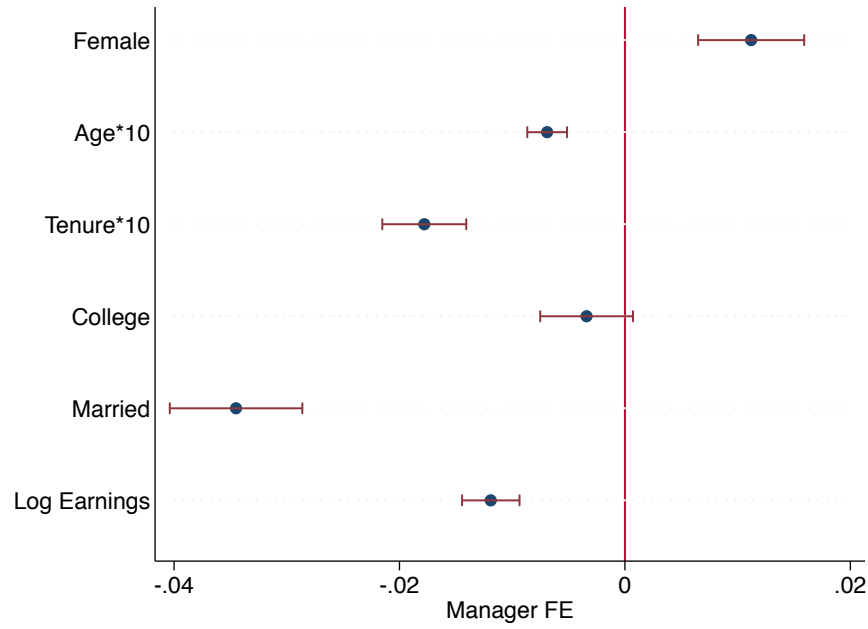
Notes: Figure shows mean wage premium of establishments that change managers. Managers are classified into quartiles based on their estimated manager fixed effects $\hat{\lambda}_m$.

Figure 2: Correlation of managers' wage residuals across establishments



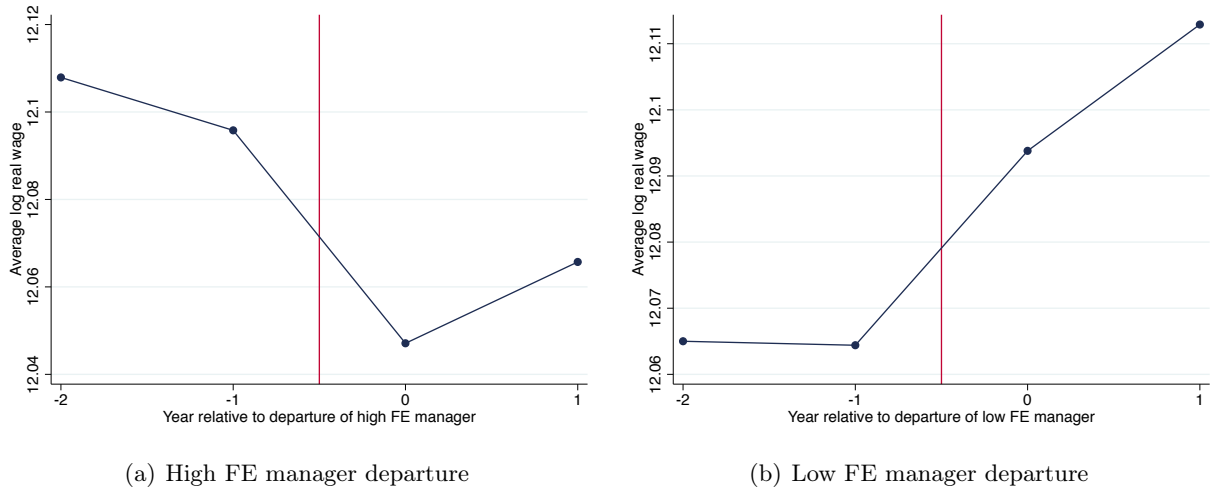
Notes: This figure shows the binscatter of wage residual in the manager's first employer against the wage residual in manager's second employer. This is similar to the test in Table V of Bertrand and Schoar (2003). The wage residual is from regressing establishment-year fixed effects on year dummies and establishment fixed effects. The number of observations is 69,641, and each dot in the binscatter contain the same number of observations. In the right figure, the variable on the x-axis is the placebo wage residual in the manager's first employer averaged over the three years before the manager joined the firm. The regression coefficient in the left figure is 0.1436 with t-statistic of 13.2; the regression coefficient in the right figure is 0.0001 with t-statistic of 0.01.

Figure 3: Characteristics of soft managers



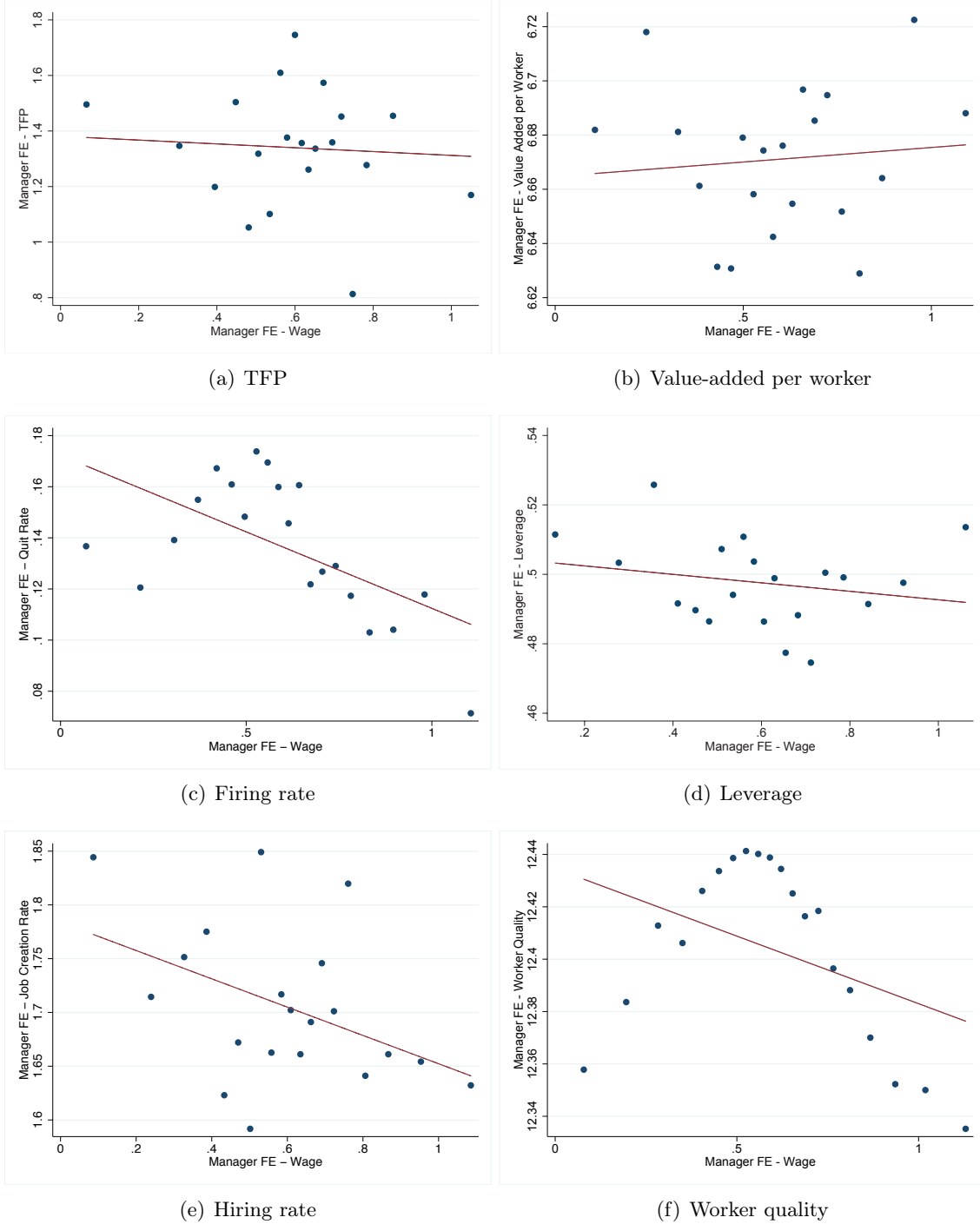
Notes: This figure shows the regression coefficients of manager fixed effects in wages on managers' characteristics. The dependent variable is manager fixed effects and the regression is weighted by the inverse standard errors of the estimated manager fixed effects. Each row is a separate regression. Ninety-five percent confidence intervals shown.

Figure 4: Event study of exogenous manager departures



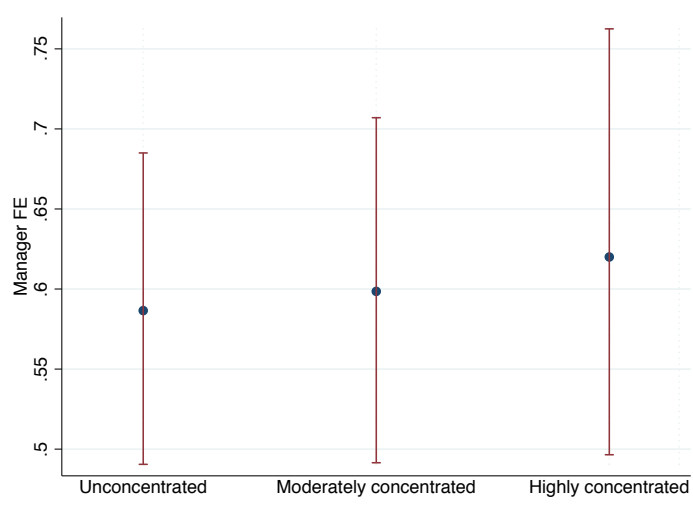
Notes: This figure shows the changed in log real wages around the departure of managers that are at least 62 years old. Year 0 is the year when the manager leaves, and we only include managers that had stayed in the same firm for at least three years before they retire and had never been employed since retirement. We reestimate the manager fixed effects for all managers using data outside the four-year window used for the event studies. The top figure includes retirements of managers with manager FE in the top quartile and has 1368 events, and the bottom figure includes retirements of managers with manager FE in the lowest quartile and has 1344 events.

Figure 5: Correlation between manager fixed effects



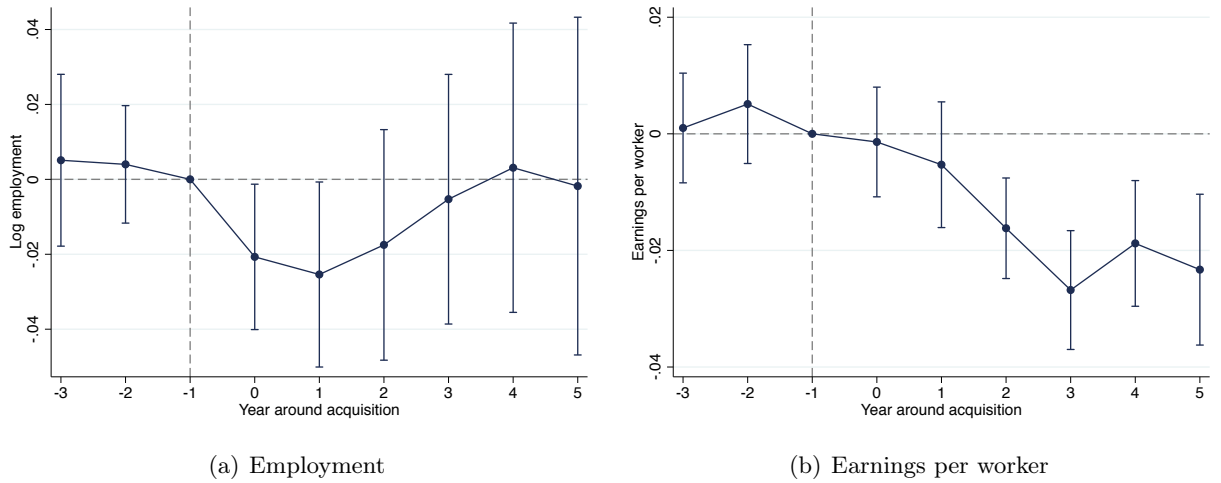
Notes: The graph shows the binscatter plots of manager fixed effects for various outcomes against manager fixed effects for wages. Each dot contains the same number of observations. In (a), on the y axis is manager FE in terms of TFP, where TFP is the residual from regressing log value-added on inputs, including labor, capital and materials, with separate regressions for each three-digit industry. In (b), on the y axis is manager FE in terms of log value-added per worker. In (c), on the y axis is manager FE in terms of the share of workers leaving the establishment in each year. In (d), on the y axis is manager FE in terms of leverage (total debt divided by book value of assets). In (e), on the y axis is manager FE in terms of share of new entrants every year. In (f), on the y axis is manager FE in terms of average worker quality, where worker quality is measured using person fixed effects in an AKM regression with person fixed effects and establishment fixed effects.

Figure 6: Industry Concentration and Manager Fixed Effects



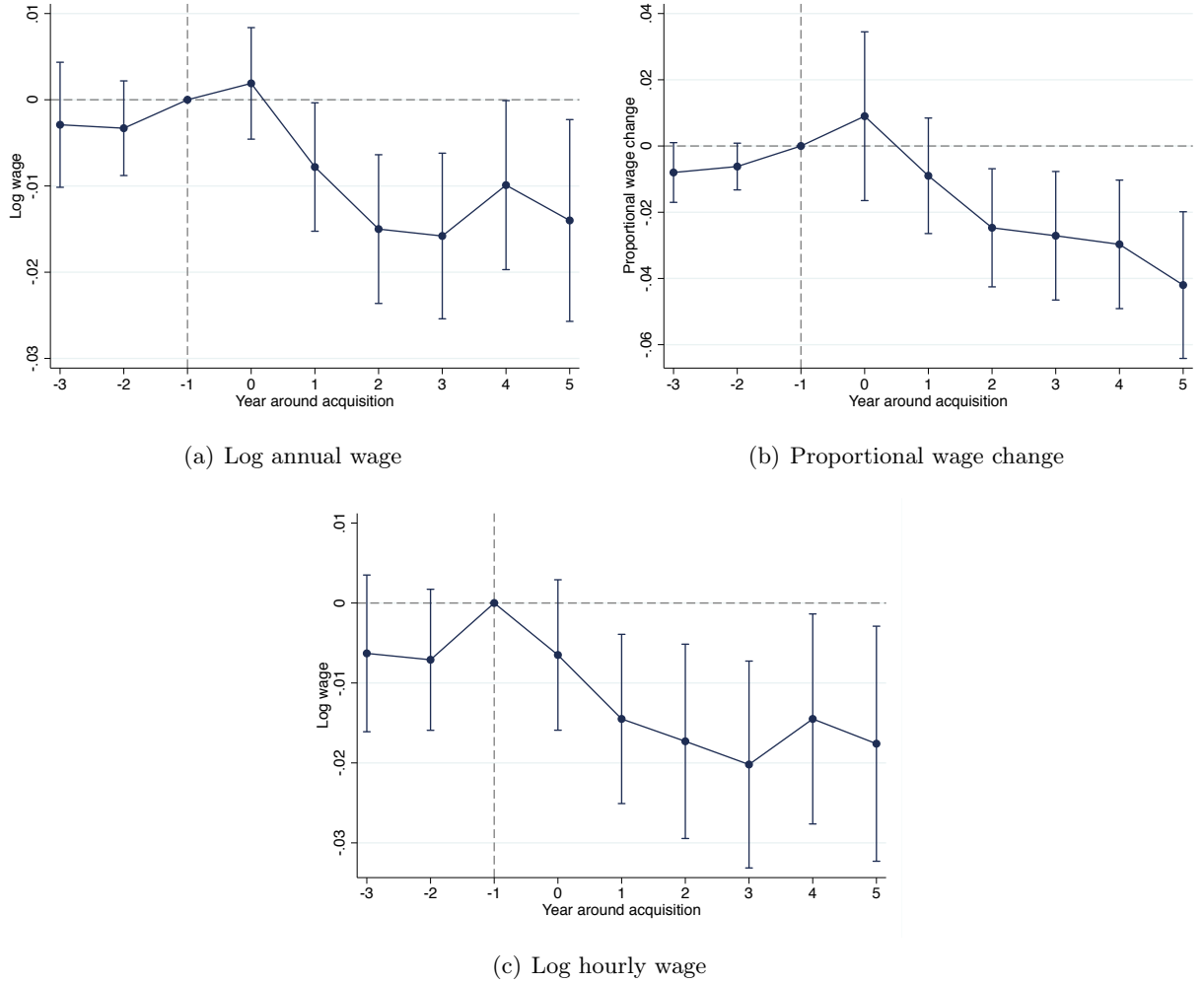
Notes: The figure plots the distribution of manager fixed effects in wage setting in industries with different levels of concentration. The dots are median manager fixed effects for each industry group, and the vertical bars denote the range from 25th percentile to 75th percentile of manager fixed effects for each industry group. Three-digit industries are defined as unconcentrated if its HHI is less than 1,500; moderately concentrated if HHI is between 1,500 and 2,500; and highly concentrated if HHI is above 2,500 (according to the Horizontal Merger Guidelines). Manager fixed effects measure managers' generosity in wage setting and the estimation is detailed in Section 4.1.

Figure 7: Target establishments' employment and wages following M&As



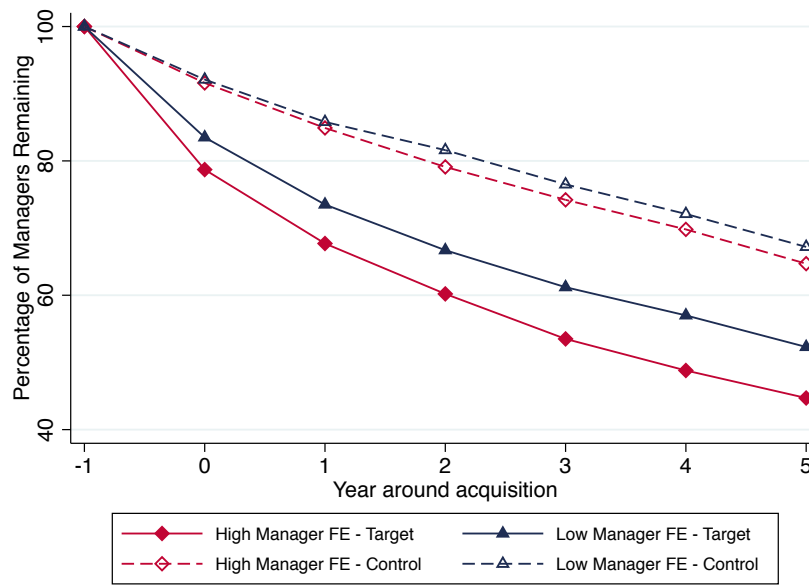
Notes: The figure shows regression coefficients and associated confidence intervals for the difference between treatment and comparison group in a given year τ relative to the year of acquisition in the treatment group establishments, i.e., the δ_τ from the difference-in-differences model in (9). The coefficient in $\tau = -1$ is normalized to zero. Regressions are weighted by average establishment employment between $\tau = -3$ and $\tau = -1$. The outcome variable in panel (a) is log employment. The outcome variable in panel (b) is proportional change in annual earnings relative to the initial annual earnings before merger ($w/w_0 - 1$). The vertical lines denote 95% confidence intervals based on standard errors clustered at the establishment level.

Figure 8: Changes in staying workers' wages after M&As



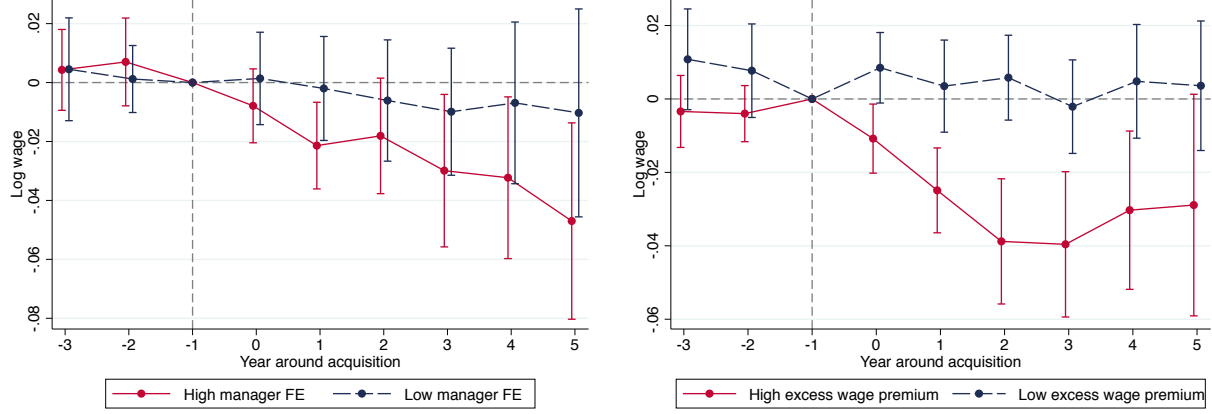
Notes: The figure shows regression coefficients and associated confidence intervals for the difference between staying workers at target and control establishments, i.e., the δ_τ from the difference-in-differences model in (10). The coefficient in $\tau = -1$ is normalized to zero. All regressions in this figure include person-establishment fixed effects, and the plotted coefficients show the effects of mergers on wage premiums for staying workers. The outcome variable in panel (a) is log annual labor earnings. The outcome variable in panel (b) is annual earnings normalized by the average annual earnings from $\tau = 3$ to $\tau = -1$. The outcome variable in (c) is log hourly wage, which is calculated as annual labor income divided by annual hours worked. The vertical lines denote 95% confidence intervals based on standard errors clustered at the establishment level.

Figure 9: Manager turnover around mergers



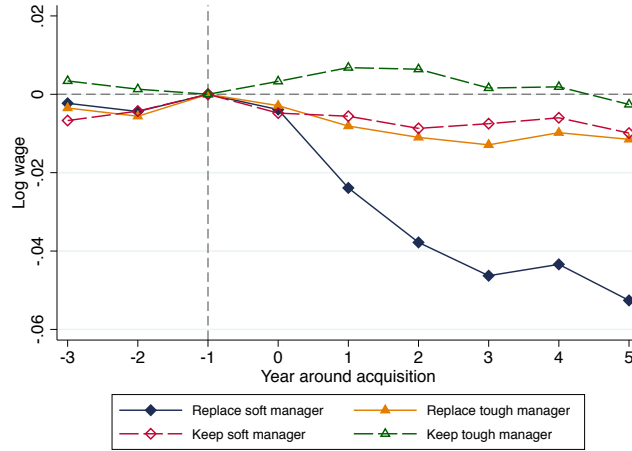
Notes: This figure plots the percentage of managers who are at treatment or control establishments in the year before acquisition that remain in the same establishment for each year after the acquisition. By definition, in year -1, 100% of managers remain in their initial establishment. Managers are defined using occupation codes (see Data Appendix for details) and each establishment has one manager in each year. For both treatment and control establishments we plot separately by manager fixed effects: the red lines are managers with above-median manager fixed effects, and navy lines are managers with below-median manager fixed effects.

Figure 10: Heterogeneity of wage effects by pre-merger wage premium and manager FE



(a) By pre-merger manager FE

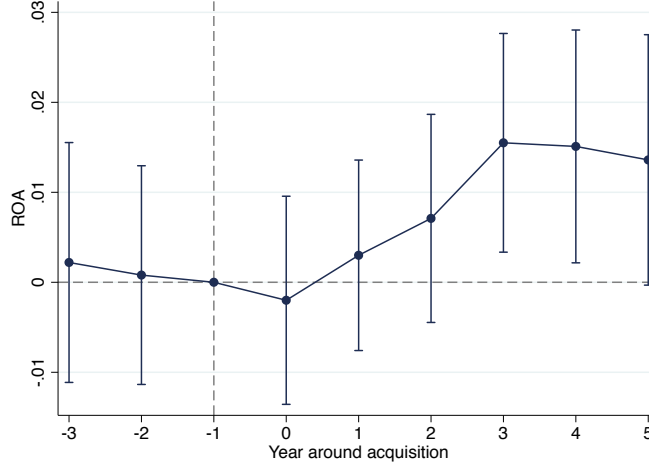
(b) By pre-merger excess wage premium



(c) By pre-merger manager FE and manager turnover

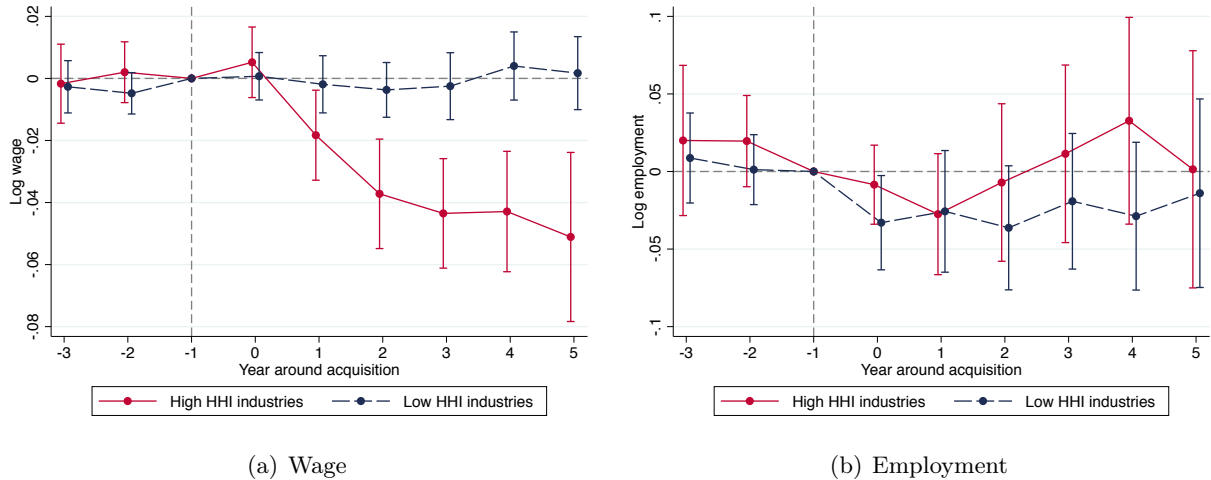
Notes: The figure shows regression coefficients and associated 95% confidence intervals for the difference between staying workers at target and corresponding control establishments separately by target establishments' pre-merger wage residual or manager softness. In top two figures the navy (red) line plots δ_τ (γ_τ) in regression (13). In figure (a), the treatment establishments are re-matched to control establishments such that they are in the same quartile of wage residual (manager FE). In figure (b) we define high manager FE as establishments with above-median manager FE (in wage setting) in the year before merger. In the right figure, high wage establishments are establishments with above-median wage residual in the year before merger, where the residual is from regressing establishment-year fixed effects (ψ_{jt} in equation 10) on productivity and industry-year and region-year fixed effects. The wage residual proxies for manager softness. Standard errors are clustered at the establishment level. In Figure (c), each line shows estimates from a separate regression, where treatment establishments in each subgroup are compared to corresponding control establishments. The four lines contain target establishments that (1) had above-median manager fixed effect and replaced the manager within 3 years after merger; (2) had below-median manager fixed effect and replaced the manager within 3 years after merger; (3) had above-median manager fixed effect and kept the same manager for at least 3 years after merger; (4) had below-median manager fixed effect and kept the same manager for at least 3 years after merger.

Figure 11: Effects of M&As on ROA of Merging Firms Over Time



Notes: The figure plots the regression coefficients and associated confidence intervals for the treatment effect of M&As on the return on assets (ROA) of the combined firm. ROA is equal to before-tax profits divided by total assets at the firm level (average ROA is 19 percentage points). For years before the merger took place, ROA of the combined firm is calculated as the sum of before-tax profits of the target and acquirer firms divided by the sum of total profits of the both firms. To isolate the changes in profits from changes in asset levels we use the pre-merger total assets as denominators when calculating the ROAs (using contemporary assets as denominators yield similar results). The plotted coefficients are δ_τ from the following firm-level event study including all firms in the economy: $y_{jt} = \alpha_j + \gamma_t + \sum_{\tau=-3}^5 \delta_\tau D_{jt}(\tau) + \beta X_{jt} + \epsilon_{jt}$, where $D_{jt}(\tau)$ equals one if firm j is a target in year $t - \tau$. The controls X_{jt} include three-digit industry-year fixed effects to control for industry-specific trends. Standard errors are clustered at the establishment level.

Figure 12: Heterogeneity by industry concentration



Notes: The figure shows regression coefficients and associated confidence intervals for the difference between wages of staying workers and employment at target and corresponding control establishments in high concentration and low concentration industries. There are 127 three-digit industries and concentration is defined by the Herfindahl-Hirschman index. High concentration industries have HHI above 1000. The left figure plots coefficients δ_τ from the worker-level difference-in-differences model in (10), and the right figure plots coefficients δ_τ from the establishment-level difference-in-differences model in (9). The coefficient in $\tau = -1$ is normalized to zero. The vertical lines denote 95% confidence intervals based on standard errors clustered at the establishment level.

Table 1 Summary Statistics

Firm Data

	All firms 2,206,320		Target firms 5,244		Acquirer firms 3,483	
Observations (firm-year)	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Variable						
Employment	16.6	252.3	264.0	2063	828.8	3089
Median employment	3		15		74	
Log value added	7.30	1.41	8.84	1.67	10.17	1.81
Log sales	8.20	1.56	9.99	1.82	11.25	1.98
Average log annual wage	11.96	0.79	12.12	0.51	12.13	0.51
Average log hourly wage	4.95	0.51	4.98	0.32	5.01	0.32
Log value added per worker	5.90	0.80	5.84	0.73	6.02	0.44
Number of establishments	1.13	2.77	3.41	18.4	12.02	38.4
Median no. of establishments	1		1		3	

Worker Data

	All workers 41,706,676		Target firm employees 286,114		Acquirer firm employees 1,739,780	
Observations (worker-year)	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Variable						
Age	39.6	12.9	37.6	13.2	40.1	12.9
Female (%)	48.3		45.5		49.8	
Married (%)	50.5		45.6		0.84	
Basic education (%)	37.0		43.0		33.6	
Vocational training (%)	36.3		36.1		33.6	
College education (%)	26.7		21.0		32.9	
Experience	15.4	11.0	14.5	11.1	16.0	11.3
Tenure	4.2	5.7	4.0	5.4	4.1	5.8
Average log annual wage	12.21	0.85	12.17	0.86	12.25	0.80
Average log hourly wage	5.05	0.53	5.01	0.52	5.08	0.45

Notes: All statistics reported are for the matched employer-employee data set described in Section 3.1. Firm-level balance sheet data are from the firm register and available from 1999. Worker-level information is from the income register and is available for the entire sample period (1995-2011). Mergers where we cannot distinguish between the target and acquirer are excluded from the merger sample. All monetary values are normalized to real 2010 Danish kroner. All ages refer to the age of an individual as of November within a given year. The classification of education groups relies on a Danish education code that corresponds to the International Standard Classification of Education (ISCED). “Higher education” basically corresponds to the two highest categories (5 and 6) in the ISCED; i.e., the individual has a tertiary education. “Vocational education” is defined as the final stage of secondary education encompassing programs that prepare students for direct entry into the labor market. Workers with just a high school or equivalent education or less are classified as having “basic education.” The medians are calculated as the average value of 10 observations around the median.

Table 2 Estimation of Manager Fixed Effects

	# of person/year obs.	# of establishments	# of workers
All population	34,000,350	379,780	3,655,779
Largest connected set in Step 1	33,906,527	364,349	3,621,302
Largest connected set in Step 2	19,992,506	60,301	2,673,937

Step 1: Estimation of Establishment Year Fixed Effects

	<i>OLS</i> (Plug in)	<i>Leave Out</i> (Kline et al. 2018)
Std. dev. of dependent variable	0.469	0.469
Std. dev. of person effects	0.269	0.224
Std. dev. of establishment year effects	0.165	0.138
Correlation of person/estab. effects	-0.01	0.16
Adjusted R-squared	0.923	0.853

Step 2: Estimation of Manager Fixed Effects

Std. dev. of dependent variable	0.147	0.147
Std. dev. of manager effects	0.106	0.082
Std. dev. of establishment effects	0.097	0.075
Correlation of manager/estab. effects	-0.22	-0.03
Adjusted R-squared	0.869	0.781

Comparison match model

Adjusted R-squared	0.873
Std. dev. of match effect	0.032

Model without manager FE

Adjusted R-squared	0.503
F statistic	9.99
Number of managers	109,252
p value	<0.001

Notes: This table summarizes the estimation details in estimating manager fixed effects in Section 4.1. The first step estimates equation (6). Establishment fixed effects ψ_{jt} and person fixed effects ξ_i are separately identified in the largest connected set linked by worker mobility. The control variables Xb include year dummies interacted with education dummies, and quadratic and cubic terms in age interacted with education dummies. The second step estimates equation (7), and manager fixed effects λ_m and establishment fixed effects are separately identified in the largest connected set linked by manager mobility. Managers are defined using occupation codes (see Data Appendix for details) and each establishment has one manager in each year. The control variables Xb include share of female workers, the share of workers in each education group, average age and experience of workers, and dummies for each decile of value-added per worker. The statistics reported in the second column under Step 1 and Step 2 are from the leave-out estimator in Kline et al (2018). The match model contains a dummy for each manager-establishment pair. Reported F-statistic and p value are from F-tests for the joint significance of the manager fixed effects.

Table 3 Effects of Mergers on Worker Departure

	Dependent Variable: Departure Rate					
	(1) All workers	(2) Wage Quartile 1	(3) Wage Quartile 2	(4) Wage Quartile 3	(5) Wage Quartile 4	(6) Managers
Year t=0	0.011 (0.008)	0.006 (0.017)	0.009 (0.010)	0.007 (0.007)	0.025*** (0.008)	0.030*** (0.007)
Year t=1	0.008 (0.015)	0.004 (0.026)	0.003 (0.019)	0.004 (0.005)	0.019 (0.016)	0.052*** (0.011)
Year t=2	0.014 (0.016)	0.001 (0.026)	0.002 (0.021)	0.007 (0.007)	0.026 (0.018)	0.071*** (0.013)
Year t=3	0.010 (0.017)	0.003 (0.028)	0.006 (0.023)	0.005 (0.019)	0.020 (0.020)	0.070*** (0.013)
Year t=4	0.005 (0.018)	0.002 (0.028)	0.006 (0.024)	0.000 (0.020)	0.007 (0.022)	0.066*** (0.014)
Year t=5	0.003 (0.020)	-0.007 (0.028)	0.008 (0.026)	-0.001 (0.020)	0.010 (0.025)	0.063*** (0.016)
No. of observations	1,121,850	278,339	277,318	282,790	283,233	50,534

Notes: (* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$) This table shows the effect of mergers on probability of leaving for workers in target establishments. The dependent variable is a dummy variable that equals one if the worker is not in the same establishment as in year -1 (the year before merger), and the coefficients are δ_τ in the difference-in-differences regression (16). All regressions control for person fixed effects and year fixed effects. The wage quartile of a worker is calculated at year $\tau = -1$ compared to all other workers in that year, and wage quartile 1 is the lowest wage quartile. Managers are defined using occupation codes (see Data Appendix for details) and each establishment has one manager in each year. Standard errors are clustered by establishment and reported in parentheses.

Table 4 Effects of Mergers on Wages

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable	Log wage	Log wage	Log wage	Log wage	Log wage	Before 2004	After 2004
<u>A. Staying workers</u>							
Short-run effect	-0.008* (0.004)	-0.007** (0.004)	-0.005 (0.003)	-0.010 (0.006)	-0.005 (0.004)	-0.010 (0.006)	-0.008 (0.005)
Long-run effect	-0.014*** (0.005)	-0.012*** (0.004)	-0.010** (0.004)	-0.018** (0.009)	-0.015*** (0.005)	-0.019*** (0.006)	-0.011* (0.007)
<u>B. All initially employed workers</u>							
Short-run effect	-0.019*** (0.004)	-0.017*** (0.004)	-0.012*** (0.004)	-0.019*** (0.007)	-0.020*** (0.005)	-0.022*** (0.005)	-0.015*** (0.005)
Long-run effect	-0.025*** (0.005)	-0.021*** (0.004)	-0.018*** (0.004)	-0.027** (0.012)	-0.024*** (0.006)	-0.028*** (0.006)	-0.020*** (0.007)
Industry*year FE		X	X				
Occupation*year FE			X				
Linear pre-trend				X			
Value added per worker					X		
No. of observations	1,095,058	1,095,058	1,095,058	1,095,058	881,952	636,271	458,787
<u>C. Bouding exercise in Lee (2009) for wage effects of staying workers</u>							
Year relative to merger	t=0	t=1	t=2	t=3	t=4	t=5	
Upper bound	0.005	-0.003	-0.009	-0.006	-0.002	0.004	
Standard error	0.003	0.004	0.004	0.004	0.005	0.005	
Lower bound	-0.009	-0.025	-0.035	-0.035	-0.035	-0.033	
Standard error	0.003	0.004	0.004	0.004	0.005	0.005	
Confidence interval (Imbens and Manski)	[-0.015, 0.010]	[-0.031, 0.004]	[-0.042, -0.002]	[-0.043, 0.001]	[-0.042, 0.006]	[-0.041, 0.012]	

Notes: (* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$) This table shows the effect of mergers on wages of workers in target establishments. Panel (A) shows the effects on wages of workers remaining in the target establishments, i.e. coefficients δ_τ in regression (10). Panel (B) shows the effects on wages of all workers initially employed in the target establishments, i.e. coefficients δ_τ in regression (16). Short-run effects refer to the difference-in-differences effects using year $\tau = 1$ post-merger as the post period; long-run effects refer to the specifications using years 1 through 5 post-merger as the post period. All regressions control for person fixed effects and year fixed effects. Column 2 controls for 4-digit industry*year fixed effects, and Column 3 controls for 4-digit occupation*year fixed effects. Standard errors are clustered by establishment and reported in parentheses. Panel (C) shows the upper bound and lower bound of the wage effects for remaining target firm employees accounting for selection using the trimming method in Lee (2009). The bounds are calculated separately for each year after the merger. To make the bounds narrower, we divide all workers in target and control establishments into three equal-sized groups based on the job tenure at year of merger, and apply the trimming procedure separately to each group. The bounds are the average of group specific bounds, and asymptotic variance is the average of the asymptotic variance for each group plus the weighted average squared deviation of each group's estimate from the mean. The confidence interval is based on Imbens and Manski (2004) and is [lower bound-1.645×s.e. of lower bound, upper bound+1.645×s.e. of upper bound].

Table 5 Manager Style in Target and Acquiring Firms

	(1)	(2)	(3)	(4)	(5)
Dependent Variable	Manager FE in wage	Establishment year FE	Manager FE in wage	Manager FE in TFP	Manager FE in value added per worker
<u>A. Target firms</u>					
Target	0.017** (0.007)	0.023*** (0.004)	0.006 (0.004)	-0.012 (0.021)	-0.005 (0.015)
Target * High concentration industry			0.032** (0.015)		
<u>B. Acquirer firms</u>					
Acquirer	-0.008 (0.005)	-0.011*** (0.002)	-0.005 (0.006)	0.013 (0.009)	0.000 (0.007)
Acquirer * High concentration industry			-0.013 (0.008)		
Control for value added	X	X	X		
Industry*Year FE	X	X	X	X	X
Region*Year FE	X	X	X	X	X
No. of establishments	53,748	324,390	53,748	53,748	53,748

Notes: (* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$) This table shows the wage premiums, manager styles and manager productivity in target establishments and acquirer establishments compared to other firms in the economy. All regressions include industry-year and region-year fixed effects. The dummy variable "Target" equals one if the establishment will become a target within the next three years but has not been acquired yet. The dummy variable "Acquirer" equals one if the establishment belongs to a firm that will acquire another firm in the next three years. All regressions are weighted by the inverse standard error of the estimated manager or establishment-year fixed effects. The estimation of manager fixed effects and establishment-year fixed effects are detailed in Section 4.1. In Column 2, manager fixed effects are estimated by excluding all managers in step 1 of the estimation procedure. High concentration industry is a dummy indicating that the firm is in a three-digit industry with HHI over 1000. In the last two columns, the dependent variables are manager fixed effects estimated from equation (11), with dependent variables being TFP and log value-added per worker respectively. Total factor productivity (TFP) is estimated by equation (15), and since labor input is measured by number of workers, the wage level does not affect TFP directly.

Table 6 Alternative Mechanisms

	(1)	(2)	(3)	(4)
Dependent variable:	Horizontal	Non- horizontal	Production workers	Non-production workers
log annual wage				
Short-run effect	-0.012*** (0.005)	-0.009* (0.006)	-0.014*** (0.006)	-0.008** (0.004)
Long-run effect	-0.014*** (0.005)	-0.013* (0.008)	-0.016*** (0.006)	-0.012** (0.005)
No. of observations	832,244	262,814	400,026	505,344
	(5)	(6)	(7)	(8)
	Routine workers	Non-routine workers	FCSL (food, cleaning, security logistics) workers	Non-FCSL workers
Short-run effect	-0.004 (0.007)	-0.004 (0.004)	-0.012 (0.010)	-0.007* (0.004)
Long-run effect	-0.003 (0.008)	-0.010** (0.005)	0.010 (0.012)	-0.014*** (0.005)
No. of observations	324,312	615,634	56,575	1,026,487

Notes: (* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$) This table presents the effects of mergers on wages of staying workers in the target establishments (based on equation (10)) for various worker groups and types of mergers. Short-run effects refer to the difference-in-differences effects using year $\tau = 1$ post-merger as the post period; long-run effects refer to the specifications using years 1 through 5 post-merger as the post period. In Column (1) and (2), horizontal mergers are defined as mergers in which target and acquirer firms are in the same four-digit industry. In Column (3) and (4), workers are classified into non-production or production category based on their detailed occupation information. The non-production category includes managers, professionals, technicians, clerks, sales and service workers. The production category includes operators, craft, and laborers. In Column (5) and (6) routine workers are workers in occupations that are in the top employment-weighted third of routine task intensity. In Column (7) and (8) FCSL workers are workers in food, cleaning, security and logistics occupations as defined in Goldschmidt and Schmieder (2017). All regressions control for person-establishment fixed effects and year fixed effects. Standard errors are clustered by establishment and reported in parentheses.

Appendix

A Additional Results

A.1 Alternative Matching Strategies

A.1.1 Non-parametric matching

We consider nonparametric comparisons that control for the cross-product of our categorical variables as in Davis et al. (2014). We construct cells using a fully saturated interaction of 127 three-digit industries, 8 establishment size groups and 4 establishment age groups. We estimate the following regressions for all stayers:

$$w_{ijt} = \alpha_{ij} + \gamma_t + \sum_{\tau=-3}^5 \delta_{\tau} D_{it}(\tau) + \beta X_{it} + \epsilon_{it}$$

where $D_{it}(\tau)$ is a dummy indicating the year relative to merger. For non-target firms we assign $\tau = -1$. The control variables X_{it} contains interaction of year dummies and dummies for each cell. The coefficients of interest are δ_{τ} , which captures the effect of merger in year τ in the target establishments and are normalized to zero in $\tau = -1$. We also run the same specification for the matched control establishments in our baseline propensity score matching procedure.

Angrist and Pischke (2008) argue that OLS and matching yield different results because of different weighting, but in general differences between matching and OLS are not of much empirical importance. Column 5 of [Table A4](#) shows that the OLS results are similar to our baseline matching method. Column 6 shows that the matched control firms do not exhibit different trends from other firms conditional on the covariates, suggesting that the spillover effects of mergers on the matched control firms are negligible.

A.1.2 Synthetic control

We test the robustness of our matching framework through an alternative strategy based on a synthetic control estimator (Abadie and Gardeazabal, 2003, Abadie et al., 2010).

We build a synthetic control for each establishment target using only average information in the years $[-4, -2]$ relative to the acquisition date. We create the synthetic control from a pool of pre-selected establishments, which we select as being in the same industry and having similar employment and wage levels three years before the audit to reduce computation. The synthetic control is obtained by weighting all establishments in the control pool so as to minimize the pre-treatment differences with the treated establishment. In particular, this methodology allows to flexibly control for unobserved factors that affect common trends in both the treatment and control groups (Abadie et al., 2010). While this empirical strategy is commonly used in cases of only one treated unit, we follow a strategy similar to Acemoglu et al. (2016) to extend the methodology to the case of multiple treated units. Hence, we first construct the synthetic control for each establishment, and we then aggregate the individual treatment effects through a re-weighting using the quality of each match. Our estimate is computed as follows:

$$\hat{\theta}(\tau) = \frac{\sum_{i \in \text{Treatment group}} \frac{y_{i\tau} - \hat{y}_{i\tau}}{\hat{\sigma}_i}}{\sum_{i \in \text{Treatment group}} \frac{1}{\hat{\sigma}_i}}$$

where $y_{i\tau}$ is the outcome of the synthetic control unit. $1/\hat{\sigma}_i$ measures the goodness of fit for each match, so that better matches are given more weight in the estimation. To construct the confidence intervals, we randomly draw 5,000 placebo treatment groups from the control group – with each group having the same size as the real treatment group. We compute the wage effect of M&As for these placebo treatment groups, and construct the confidence intervals for hypothesis testing of whether the coefficient is significantly different from zero. The effect is significant at 5% if it does not belong to the interval that contains the [2.5, 97.5] percentiles of the effect for placebo treatment groups. Result is shown in ??.

A.2 Job Inflow and Outflow

To examine how job inflow and outflow change around time of M&As, we define job inflow between year 0 and year 1 as the number of workers joining the firm during the period divided by the employment in year 0, and job outflow as the number of workers leaving the firm during the period divided by employment in year 0. We then estimate the equation:

$$y_{jt} = \gamma_t + \sum_{\tau=-3}^5 \lambda_{\tau} D_{jt}(\tau) + \beta X_{jt} + \epsilon_{jt}$$

where λ_{τ} captures the difference between treated and control establishment in terms of job inflow and outflow rates. We control for industry-year fixed effects to absorb industry-specific trends. ?? shows that both job inflow and job outflow increase following mergers.

How does cutting wages affect job flows? To answer this question, we look at the effects of mergers on job inflow and outflow separately for high-wage and low-wage establishments. In Section 5.3 we have established that all of the wage cuts are concentrated in high-wage establishments. ?? shows that both high-wage and low-wage establishments experience a rise in job outflow rates after mergers, while only high-wage establishments experience a large rise in job inflow rates after mergers. The average quality of joining workers in high-wage establishments increases, while the average average of joining workers in low-wage establishments does not change after mergers. This indicates that mergers lead to wage cuts in high-wage establishments, but also lead to more hiring of high-skilled workers in high-wage establishments. By reducing the wage premium in establishments with soft managers, mergers allow the target firms to hire more high-wage workers.

A.3 Wage Changes of All Initially Employed Workers

We investigate the selection issue by looking at the effects of mergers on the wages and departure rates for cohorts of workers initially employed in target firms at the time of acquisition. We estimate the following regression, which includes all workers who are in the target or control establishments in $\tau = -1$ regardless of whether they move to another establishment in $\tau > 0$:

$$y_{it} = \alpha_i + \gamma_t + \sum_{\tau=-3}^5 \lambda_{\tau} D_{it}(\tau) + \sum_{\tau=-3}^5 \delta_{\tau} D_{it}(\tau) \times Target_i + \beta X_{it} + \epsilon_{it} \quad (16)$$

This is the same as the cohort-based approach in Hummels, Munch and Xiang (2018).

?? shows that mergers reduce wages for workers initially employed in target firms at time of merger. The wage declines are slightly larger than those of workers staying in target firms due to

the additional negative effects of job displacement (Jacobson, LaLonde and Sullivan 1993). However, although the effect on unemployment peaks at one year after merger, the negative wage effects are persistent and seemingly irreversible, which is consistent with the loss of firm-specific wage premiums.

B Data Appendix

B.1 Identify Managers

We define managers using occupation codes (ISCO-88) and job hierarchy (PSTILL). A worker is defined as a manager if the occupation code belongs to the manager occupation group or if the worker is in the highest job hierarchy. About 6% of all the workers are managers, and we are able to identify at least one manager for about 60% of all establishment-year observations, and for 80% of the establishment-year observations with at least 10 employees.

For each firm we select one manager with the highest rank. The highest ranked occupation code is 1210 (directors and chief executives). If no worker has occupation code equal to 1210, then the highest ranked manager is someone with a managerial occupation code (between 1221 and 1319) and has highest job hierarchy (PSTILL=31). If no one satisfies the criteria, then the highest ranked manager is someone with a managerial occupation code or highest job hierarchy. If multiple individuals have the same highest ranked occupation code and the highest job hierarchy, then we select the manager with the highest total income as the top manager.

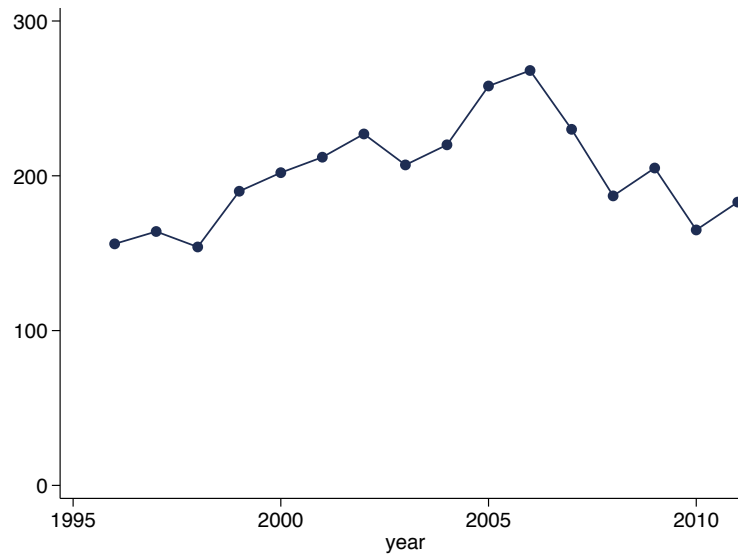
B.2 Comparing with External Datasets of M&As

?? compares the number of mergers in our administrative dataset with the number of mergers in Denmark in Zephyr and SDC datasets. In all datasets number of mergers is increasing before the financial crisis and declines afterwards. The administrative dataset has about 30-40% more mergers in years before the financial crisis.

References

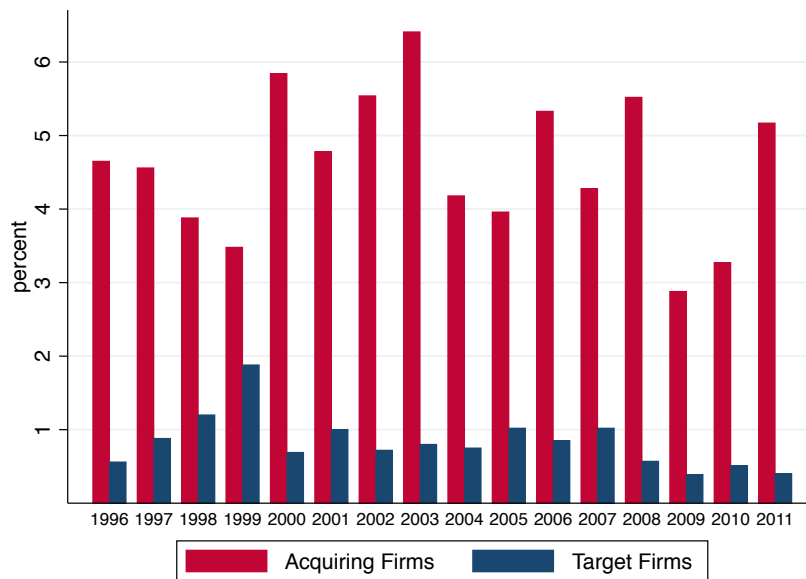
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Figure A1: Number of Mergers and Acquisitions in Denmark: 1995-2011



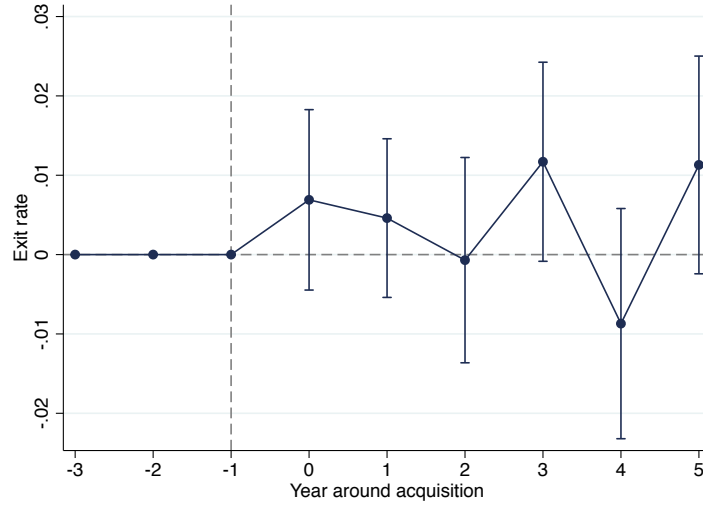
Notes: This figure shows the number of merger transactions in Denmark by year. Mergers are identified by firm and establishment identifiers (see Section 3.3 for details). Transactions in which one of the parties is a foreign company are not included. Mergers in the public sector are also excluded. For transactions involving multiple firms, each transaction is only counted once.

Figure A2: Percentage of employment in target or acquirer firms



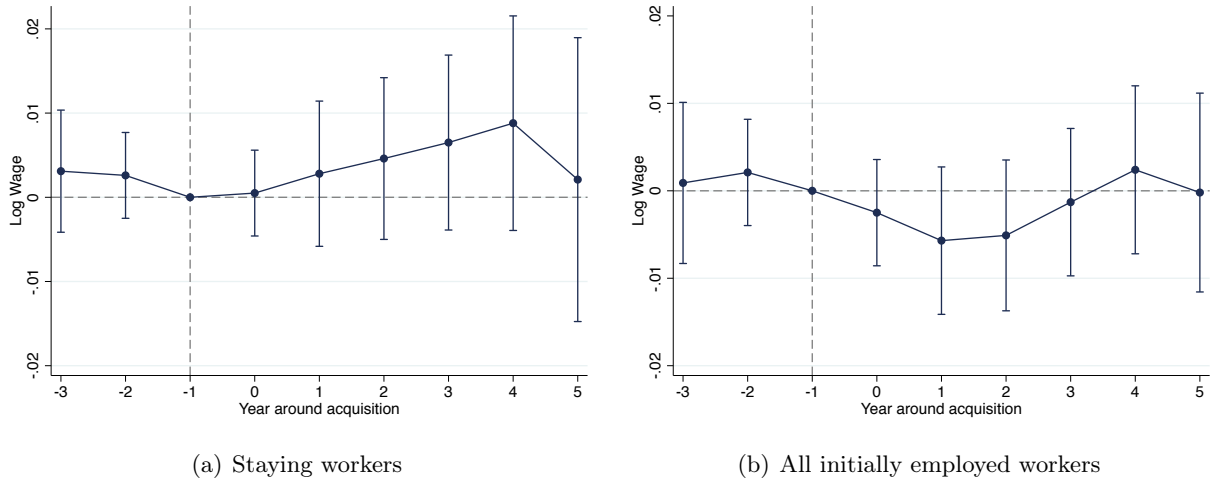
Notes: This figure shows the share of all employed workers in Denmark that works in acquired or acquiring firms in each year. We only include workers who are full-time employees and are between 25 and 60 years old.

Figure A3: Effects of merger on establishment exit



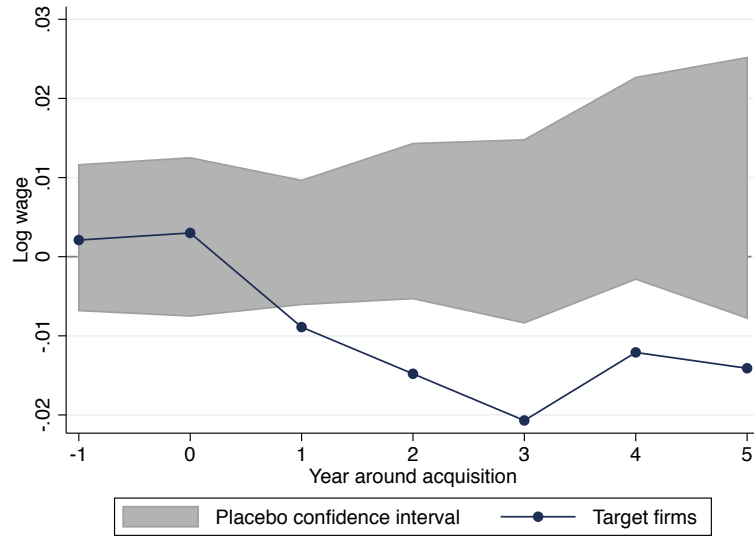
Notes: The figure shows regression coefficients and associated confidence intervals for the difference between treatment and comparison group in a given year τ relative to the year of acquisition in the treatment group establishments, i.e., the δ_τ from the difference-in-differences model in (9). The coefficient in $\tau = -1$ is normalized to zero. Regressions are weighted by average establishment employment between $\tau = -3$ and $\tau = -1$. The outcome variable is a dummy variable that equals one if the establishment exits in the following year. The vertical lines denote 95% confidence intervals based on standard errors clustered at the establishment level.

Figure A4: Effects of merger on wages of workers in acquiring establishments



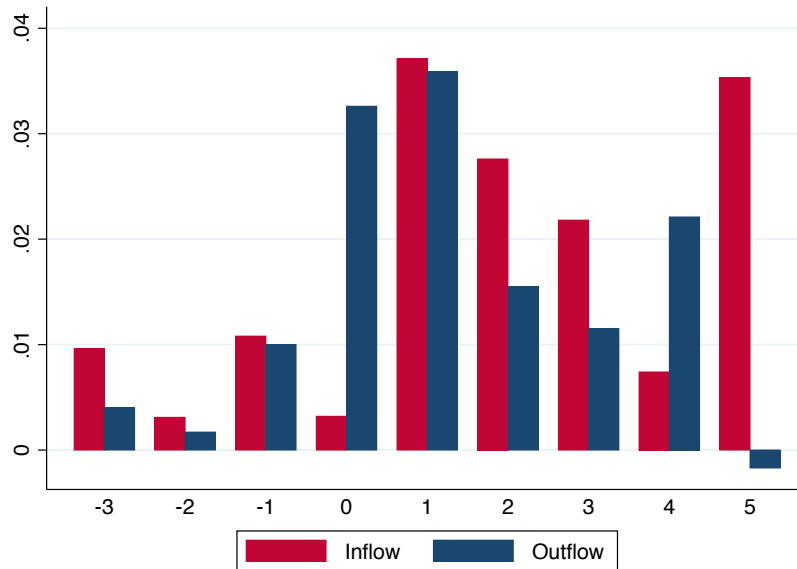
Notes: This figure shows the effect of mergers on workers' annual wages in acquiring firms. The left figure shows the effects on wages of all workers staying in acquiring establishments, and right figure shows the effects on wages of all workers employed in acquiring firms in the year before the merger. Establishments that have acquired multiple times are excluded. Ninety-five percent confidence intervals shown.

Figure A5: Synthetic control



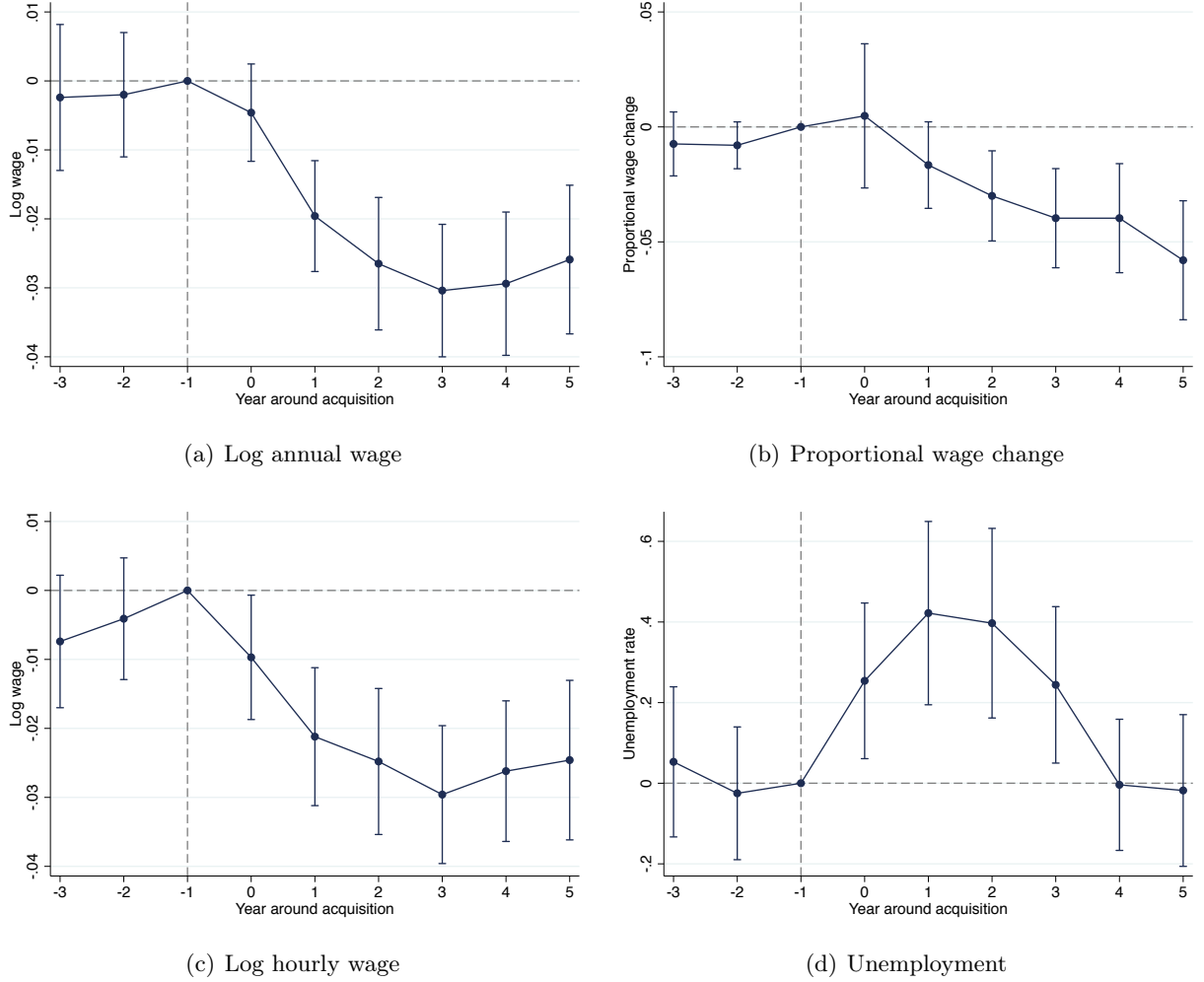
Notes: This figure shows the estimate of the effects of M&As on target establishments' earnings per worker using synthetic control. The shaded area is the [2.5, 97.5] confidence interval constructed using placebo treatment groups. See Appendix A.1.2 for details.

Figure A6: Changes in job inflow and outflow in target establishments around mergers



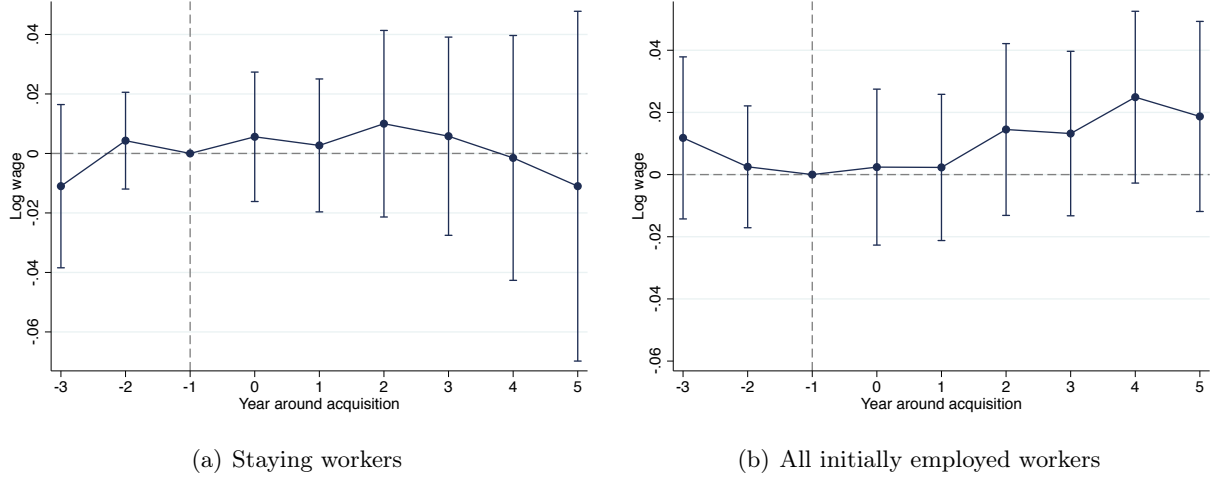
Notes: This figure shows differences in the inflow and outflow of workers between target establishments and control establishments. The regression controls for industry-year fixed effects. Inflow (outflow) at year τ is calculated as the number of entrants (leavers) between year $\tau - 1$ and year τ divided by employment in year $\tau - 1$.

Figure A7: Changes in all initially employed workers' wages after M&As



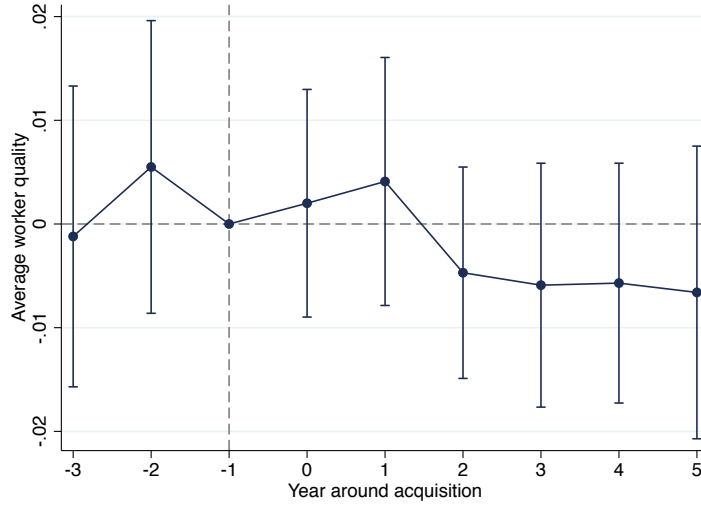
Notes: The figure shows regression coefficients and associated confidence intervals for the difference between workers initially employed at target and control establishments at time $\tau = -1$, i.e., the δ_τ from the difference-in-differences model in (16). The coefficient in $\tau = -1$ is normalized to zero. All regressions control for person fixed effects and year fixed effects. The outcome variable in panel (a) is log annual labor earnings. The outcome variable in panel (b) is proportional change in annual earnings relative to the initial annual earnings before merger ($w/w_0 - 1$). Observations with zero earnings are included in (b) and not in (a). The outcome variable in (c) is log hourly wage, which is calculated as annual labor income divided by annual hours worked. The outcome variable in (d) is a dummy variable for unemployment, where unemployment is defined as zero labor income. The vertical lines denote 95% confidence intervals based on standard errors clustered at the establishment level.

Figure A8: Wage effects of failed mergers



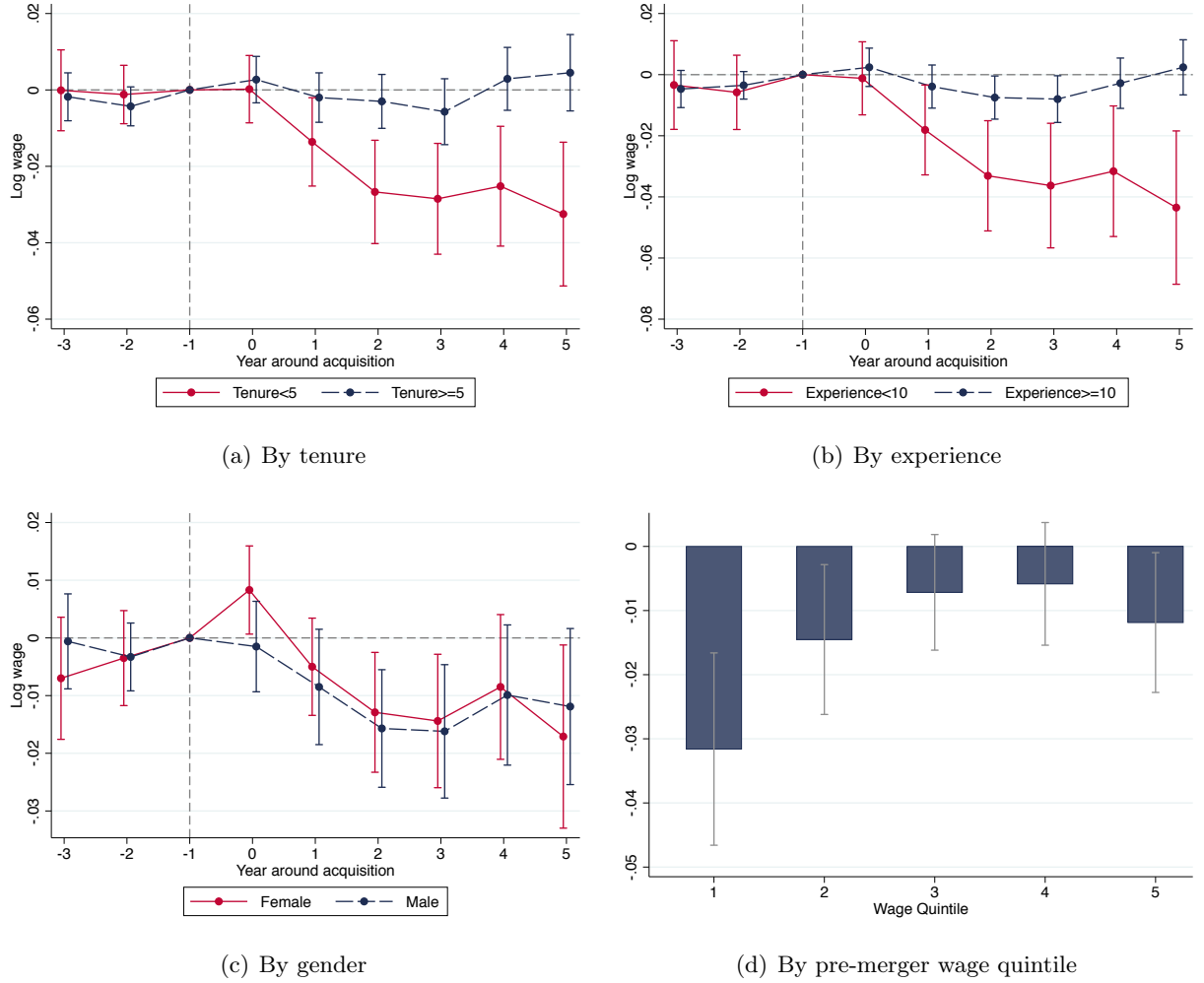
Notes: The figure shows regression coefficients and 95% confidence intervals for the difference between wages at failed target and control establishments. We match 365 targets of failed mergers from SDC Platinum to the administrative register data. We then match each failed target establishment to a control establishment using the same procedure in Section 5.1. The left figure plots wage effects for staying workers, i.e. δ_τ in equation (10); the right figure plots wage effects for all initially employed workers, i.e. δ_τ in equation (16).

Figure A9: Change in worker quality of target establishments around mergers



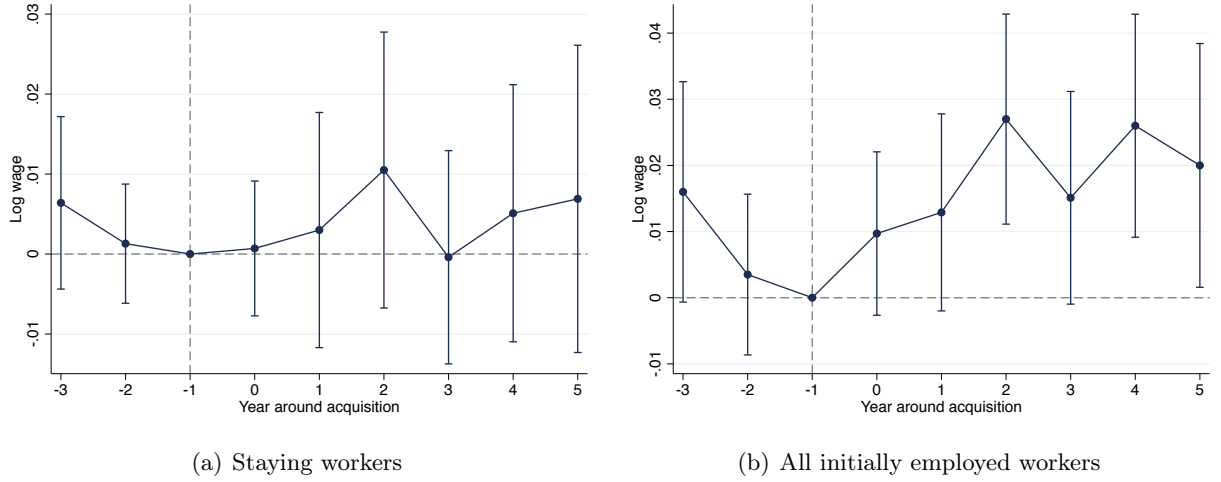
Notes: The figure shows regression coefficients and associated confidence intervals for the difference between treatment and comparison group in a given year τ relative to the year of acquisition in the treatment group establishments, i.e., the δ_τ from the difference-in-differences model in (9). The coefficient in $\tau = -1$ is normalized to zero. Regressions are weighted by average establishment employment between $\tau = -3$ and $\tau = -1$. The outcome variable is average worker quality measured by average worker fixed effects, where worker fixed effects are estimated from AKM-type regressions with worker fixed effects and establishment fixed effects. The vertical lines denote 95% confidence intervals based on standard errors clustered at the establishment level.

Figure A10: Heterogeneity by worker covariates



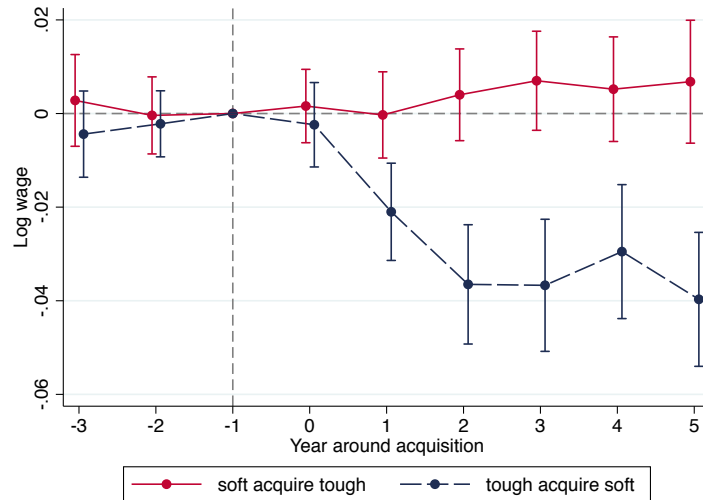
Notes: The figure shows regression coefficients and associated confidence intervals for the difference between staying workers at target and control establishments for different groups of workers. We estimate variations of equation (10) adding interactions of worker covariates with the period dummies, as well as interactions of covariates with period dummies and treatment status, and plot the coefficients for the interactions of covariates with period dummies and treatment status. The worker characteristics (tenure, experience and wage quintile) are calculated at year -1 (one year before the merger takes place). In the regression sample, the median experience is 15 years and the median tenure is 4 years, and 37% are female. The coefficient in $\tau = -1$ is normalized to zero. The vertical lines denote 95% confidence intervals based on standard errors clustered at the establishment level.

Figure A11: Wage effects of public sector mergers



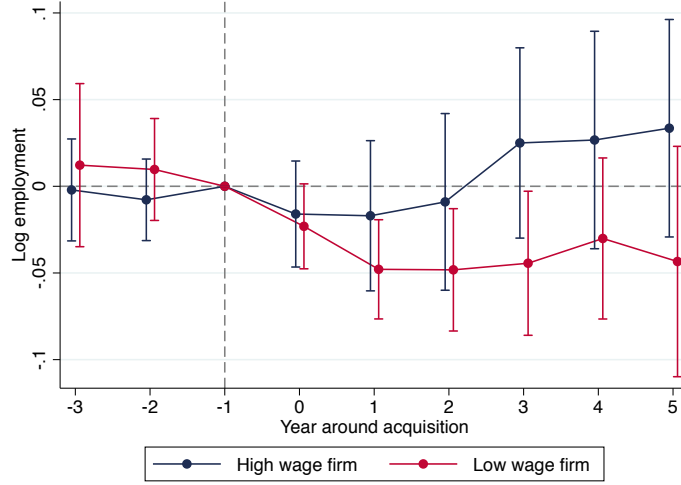
Notes: The figure shows regression coefficients and 95% confidence intervals for the difference between wages at target and control establishments in the public sector. Public sector industries are defined as industries comprising of firms owned by the government, including education, public administration, governments, utility services, health services, etc. The left figure plots wage effects for staying workers, i.e. δ_τ in equation (10); the right figure plots wage effects for all initially employed workers, i.e. δ_τ in equation (16).

Figure A12: Effects of mergers on wages based on difference in manager FE between acquirer and target



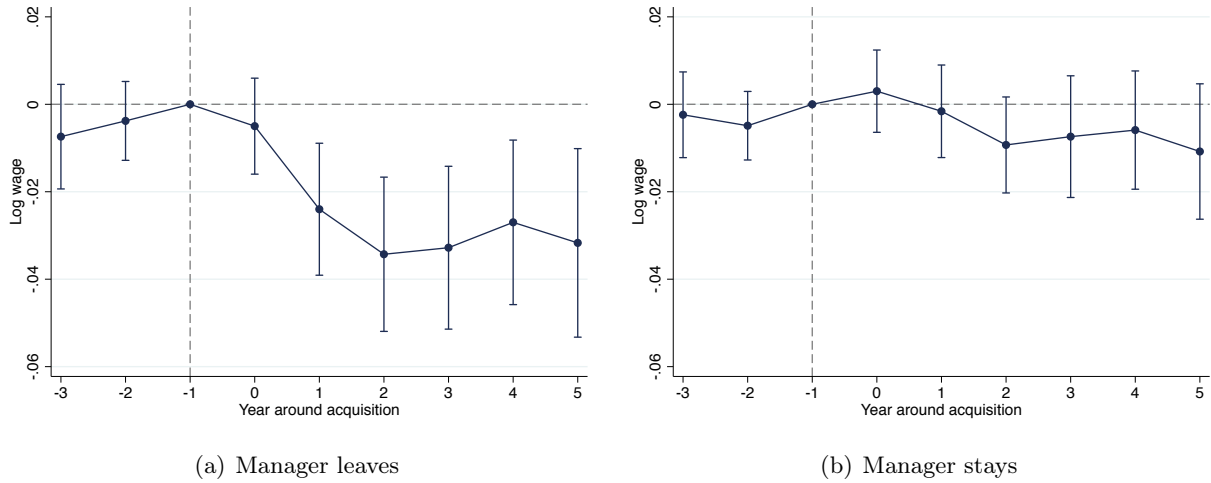
Notes: The figure shows regression coefficients and associated 95% confidence intervals for the difference between staying workers at target and corresponding control establishments. The treatment establishments are re-matched to control establishments such that they are in the same quartile of manager fixed effects. The red (navy) line contains target establishments with manager fixed effect lower (higher) than the manager fixed effect of its acquirer firm and the corresponding control establishments. Standard errors are clustered at the establishment level.

Figure A13: Effects of mergers on employment at high wage and low wage target establishments



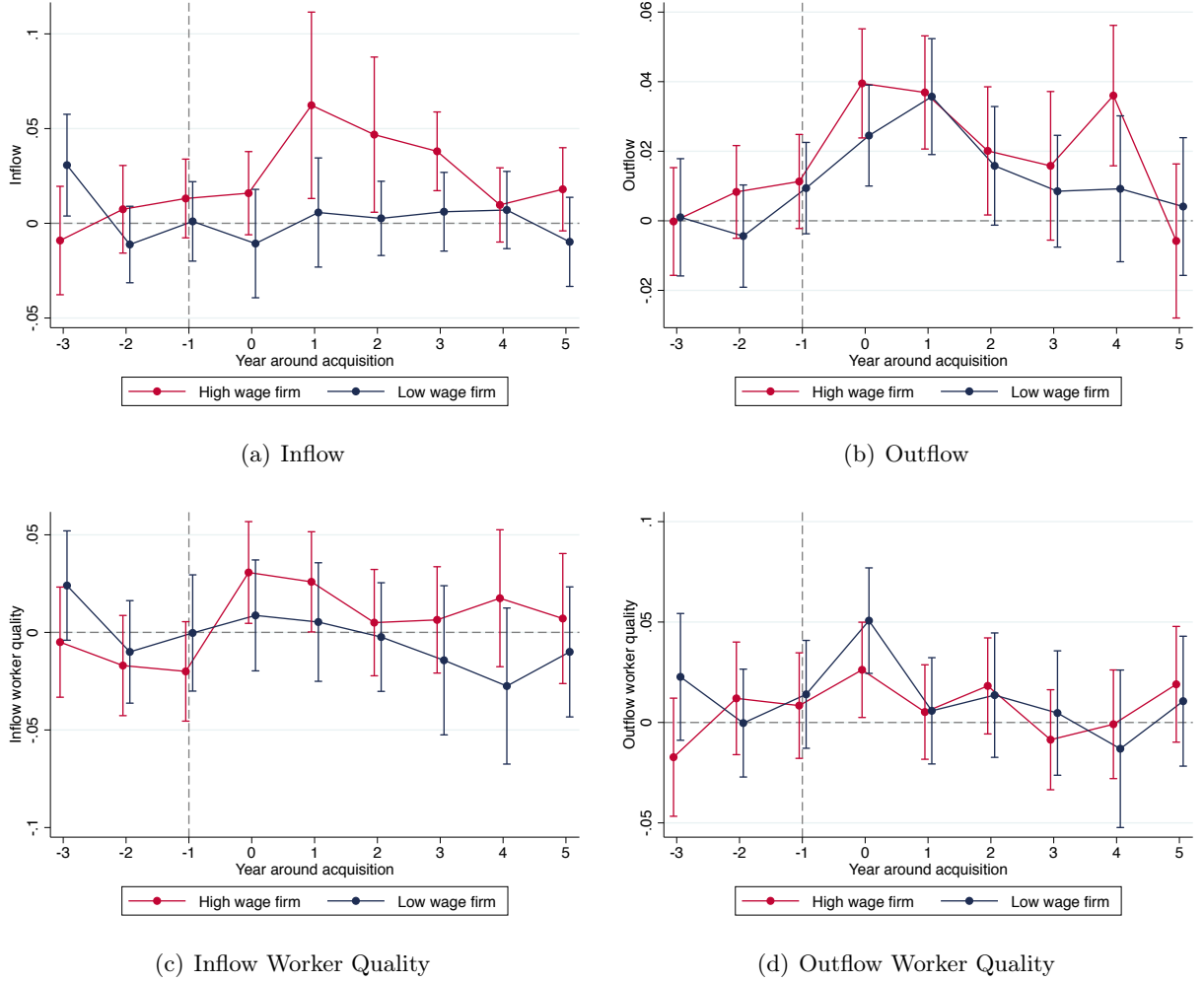
Notes: The figure shows regression coefficients and associated 95% confidence intervals for the difference between log employment at target and corresponding control establishments separately by target establishments' pre-merger wage residual. Inflow (outflow) at year τ is calculated as the number of entrants (leavers) between year $\tau - 1$ and year τ divided by employment in year $\tau - 1$. The treatment establishments are re-matched to control establishments such that they are in the same quartile of wage residual. The regression includes establishment fixed effects and year fixed effects. The coefficient in $\tau = -1$ is normalized to zero. High wage establishments are establishments with above-median wage residual in the year before merger, where the residual is from regressing establishment-year fixed effects on productivity and industry-year and region-year fixed effects. The wage residual proxies for manager softness. Standard errors are clustered at the establishment level.

Figure A14: Heterogeneity by manager turnover



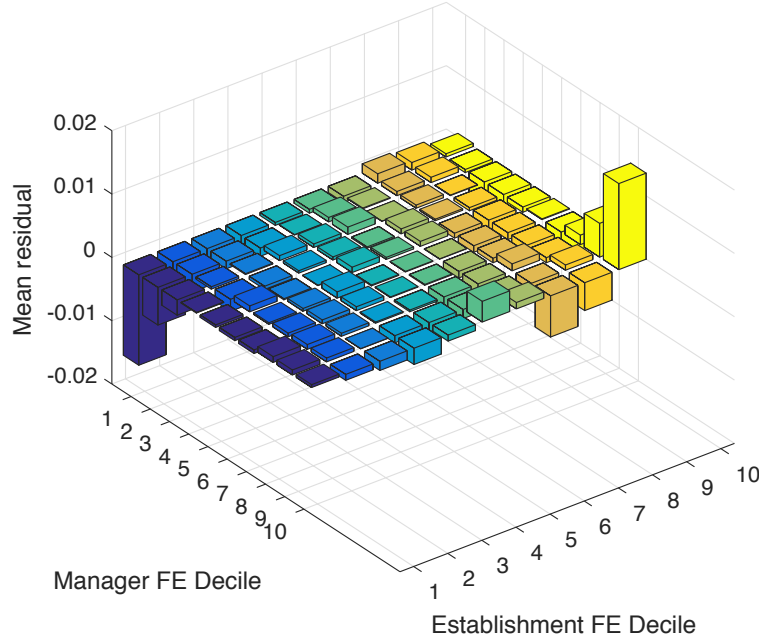
Notes: The figure shows regression coefficients and associated 95% confidence intervals for the difference between staying workers at target and control establishments. The two figures are two separate regressions, the left figure contains all target establishments which replace their managers at year $\tau = 2$ and the corresponding control establishments, and the right figure contains all target establishments which keep their managers at year $\tau = 2$ and the corresponding control establishments. Managers are defined using occupation codes (see Data Appendix for details) and each establishment has one manager in each year. Standard errors are clustered at the establishment level.

Figure A15: Inflow and outflow at high wage and low wage target establishments



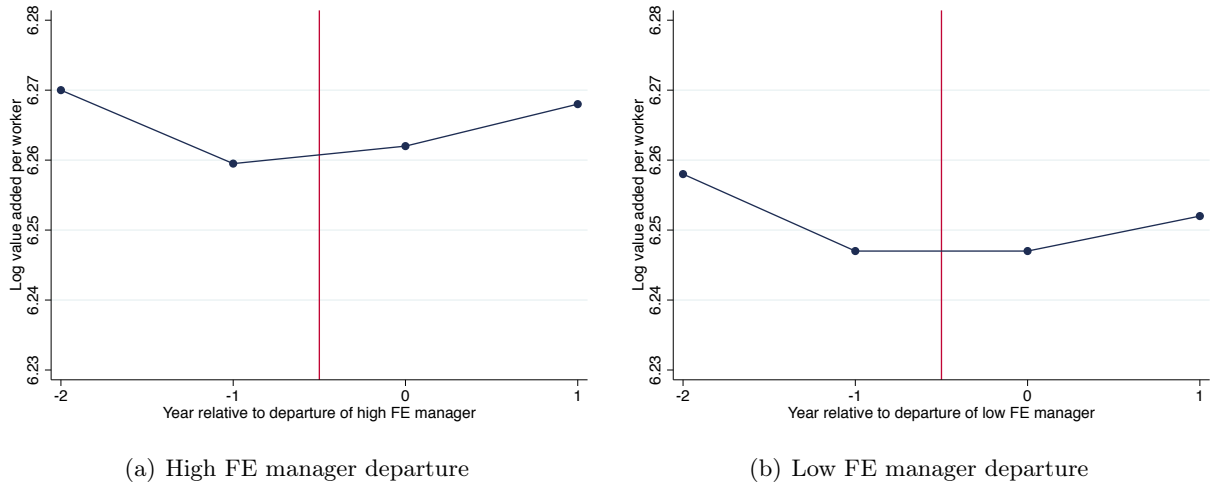
Notes: The figure shows regression coefficients and associated 95% confidence intervals for the difference between job inflows and outflows at target and corresponding control establishments separately by target establishments' pre-merger wage residual. Inflow (outflow) at year τ is calculated as the number of entrants (leavers) between year $\tau - 1$ and year τ divided by employment in year $\tau - 1$. Worker quality of inflow (outflow) at year τ is calculated as the average person fixed effects (estimated in step 1 of Section 4.1) of entrants (leavers) between year $\tau - 1$ and year τ divided by employment in year $\tau - 1$. The treatment establishments are re-matched to control establishments such that they are in the same quartile of wage residual. The regression includes industry by year fixed effects. High wage establishments are establishments with above-median wage residual in the year before merger, where the residual is from regressing establishment-year fixed effects on productivity and industry-year and region-year fixed effects. The wage residual proxies for manager softness. Standard errors are clustered at the establishment level.

Figure A16: Mean residuals by deciles of manager/establishment fixed effects



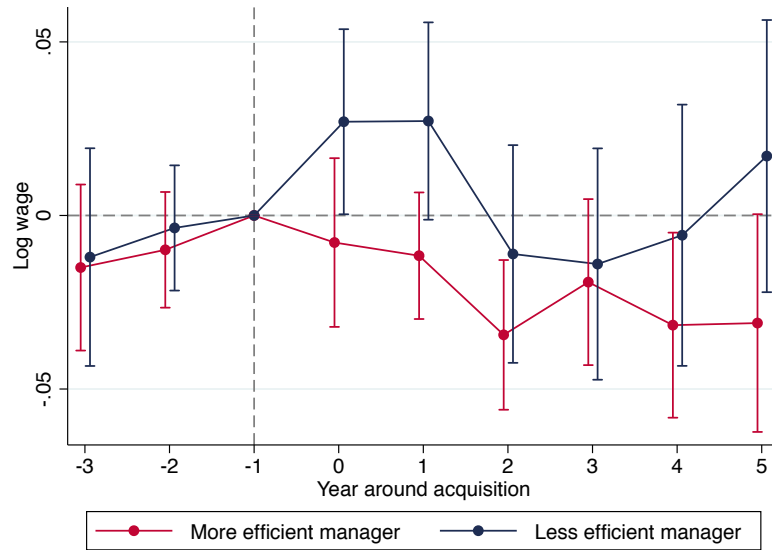
Notes: Figure shows mean residuals from estimating manager FE (equation 7) with cells defined by decile of estimated establishment effect, interacted with decile of estimated manager effect.

Figure A17: Event study of exogenous manager departures on productivity



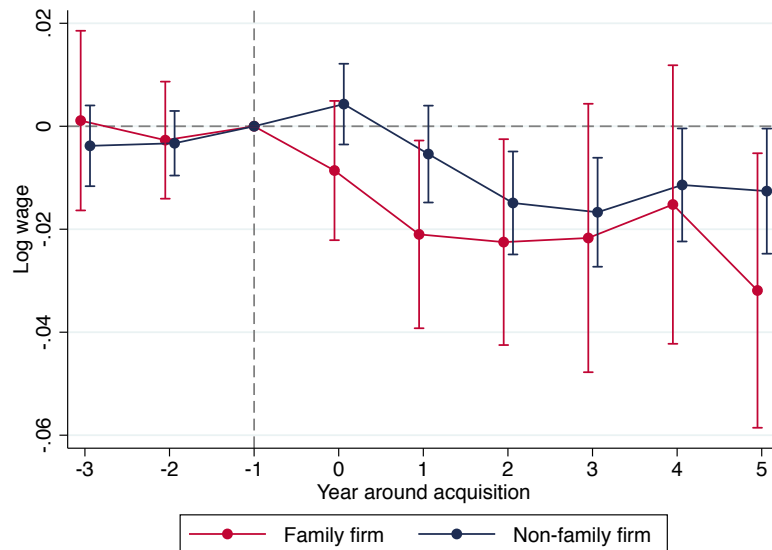
Notes: This figure shows the changed in log value-added per worker around the departure of managers that are at least 62 years old. Year 0 is the year when the manager leaves, and we only include managers that had stayed in the same firm for at least three years before they retire and had never been employed since retirement. We reestimate the manager fixed effects for all managers using data outside the four-year window used for the event studies. The top figure includes retirements of managers with manager FE in the top quartile and has 1368 events, and the bottom figure includes retirements of managers with manager FE in the lowest quartile and has 1344 events.

Figure A18: Manager productivity and effects of mergers on wages



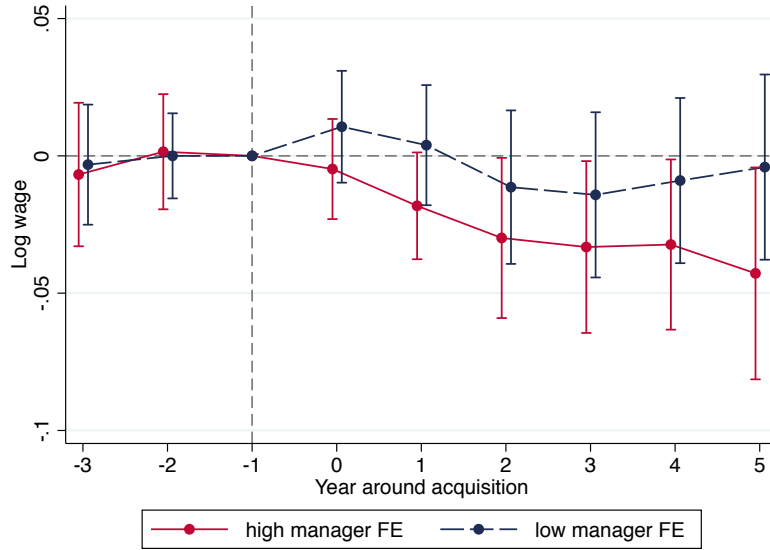
Notes: The figure shows regression coefficients and associated 95% confidence intervals for the difference between staying workers at target and corresponding control establishments separately by target establishments' pre-merger manager productivity. Manager productivity is estimated using equation (7) with TFP on the left hand side. The red (navy) line includes target establishments with above-median (below-median) manager productivity and their corresponding control establishments. Standard errors are clustered at the establishment level.

Figure A19: Family firms and effects of mergers on wages



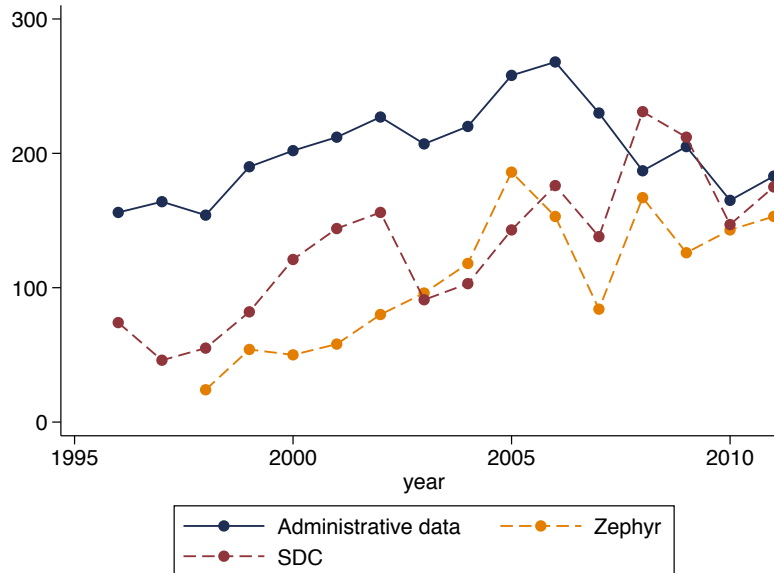
Notes: The figure shows regression coefficients and associated 95% confidence intervals for the difference between staying workers at target and corresponding control establishments for family firms and non-family firms respectively. We define family firms following Bennedsen et al. (2007) and a firm is a family firm if managers in different years are family members.

Figure A20: Heterogeneity of wage effects by manager FE: split-sample IV estimates



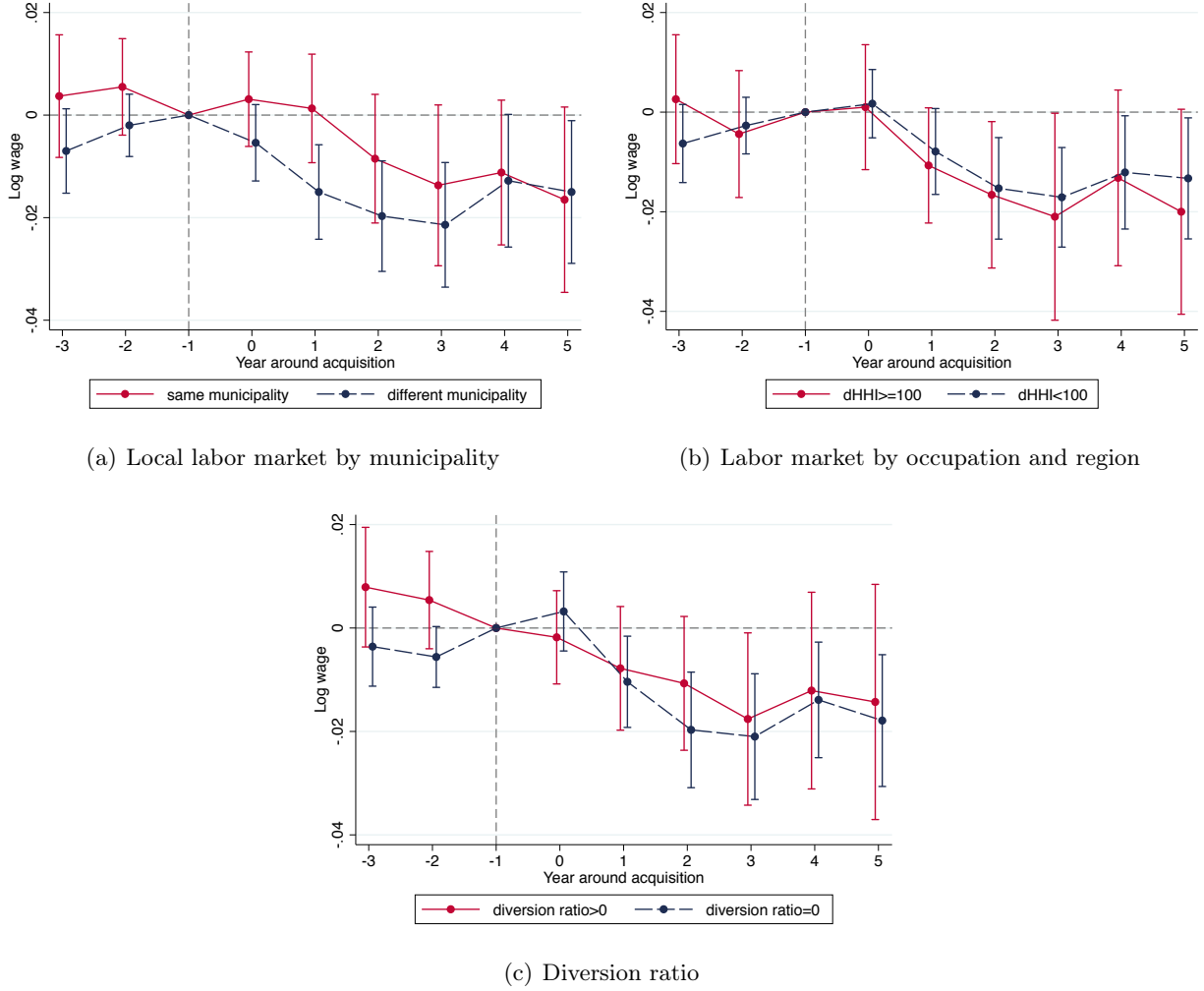
Notes: The figure shows regression coefficients and associated 95% confidence intervals for the difference between staying workers at target and control establishments separately by target establishments' manager FE. The sample is divided evenly by odd and even years and manager FE is estimated for each subsample. Manager FEs in odd years are instrumented with the manager FEs in even years and vice versa.

Figure A21: Compare Mergers in Administrative Datasets and External Datasets



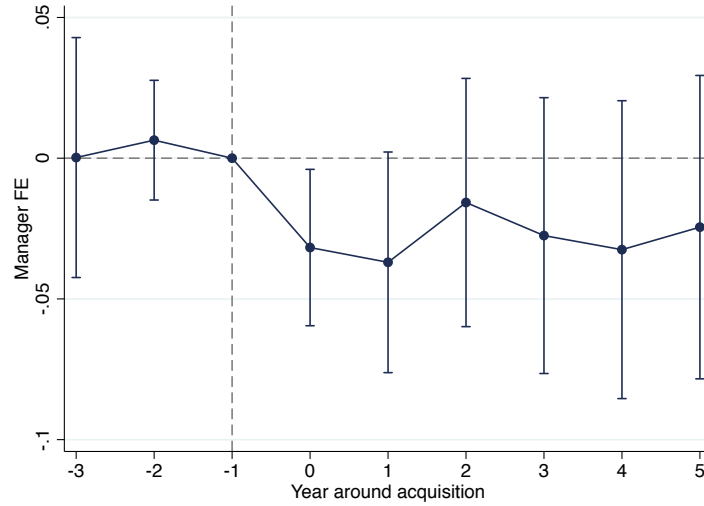
Notes: This figure shows the number of merger transactions in Denmark by year from 1996 to 2011. The solid line is the number of merger transactions in our data, the red dashed line is the number of transactions from the SDC Platinum data, and the orange dashed line is the number of transactions from BvD Zephyr data. Transactions in which one of the parties is a foreign company are not included. For transactions involving multiple firms each transaction is only counted once.

Figure A22: Testing Monopsony



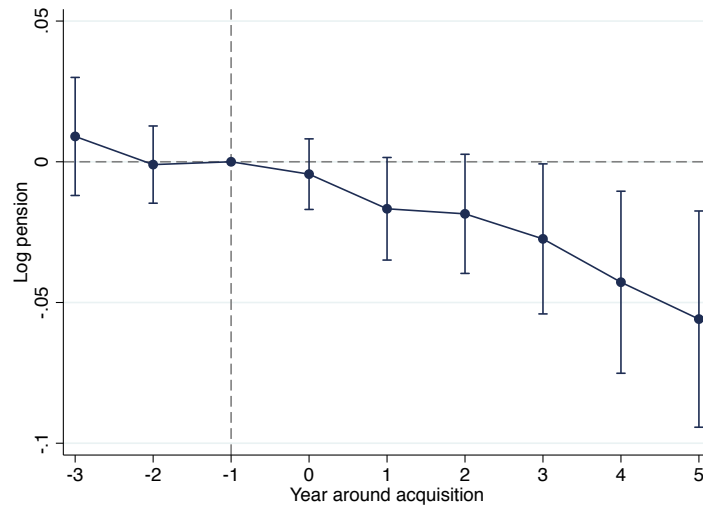
Notes: This figure tests whether negative wage effects of mergers are due to increased monopsony power in the labor market. Each figure plots the regression coefficients and associated 95% confidence intervals for the difference between staying workers at target and corresponding control establishments separately for mergers that have larger or smaller impact on monopsony power. In (a), monopsony power is calculated by concentration in the local labor markets defined by municipalities, and red (blue) line contains target establishments which are in same (or different) municipality as the acquirer and their corresponding control establishments. In (b), monopsony power is calculated by concentration in the local labor markets defined by geographical region (similar to commuting zones) and 4-digit occupation, and red (blue) line contains mergers that increased the labor market HHI by more (or less) than 100 points. In (c), monopsony power is calculated by the diversion ratio, which is measured by the fraction of job movers from target firms that move to the acquirer firm in the years before merger. Red (blue) line contains target establishments with positive (or zero) diversion ratios and their corresponding control establishments. Standard errors are clustered at the establishment level.

Figure A23: Effects of mergers on manager fixed effects



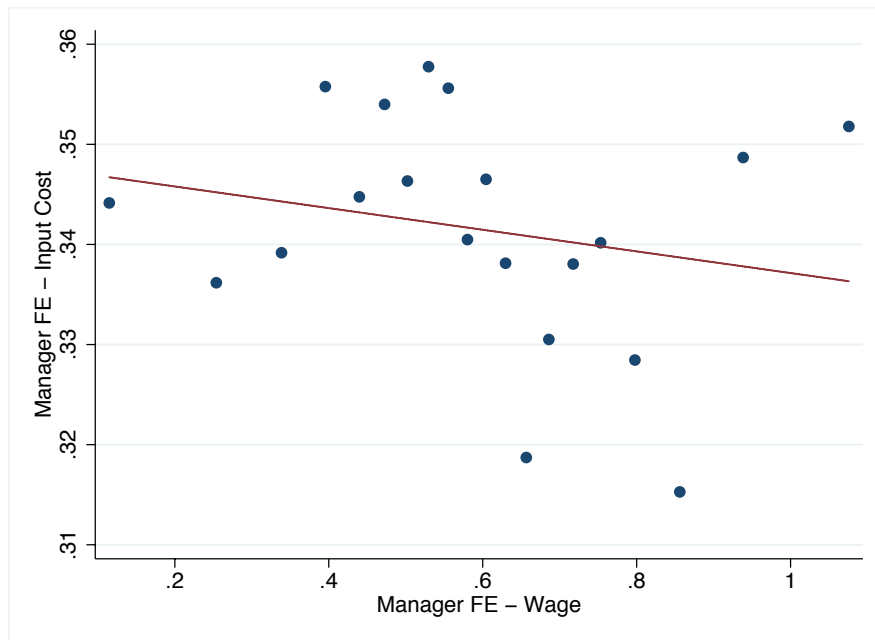
Notes: The figure shows regression coefficients and associated 95% confidence intervals for the difference between manager fixed effects at target and corresponding control establishments. The manager fixed effects measure managers' generosity in wage setting and the estimation is detailed in Section 4.1. Note that the estimation of manager FE excludes post-merger observations of the target firms, and the manager fixed effects for observations in $\tau \geq 0$ are identified only from other firms where the managers are employed.

Figure A24: Effects of mergers on pensions of target employees



Notes: The figure shows regression coefficients and associated confidence intervals for the difference between log pension payments of staying workers at target and control establishments, i.e., the δ_τ from the difference-in-differences model in (10). The coefficient in $\tau = -1$ is normalized to zero. The vertical lines denote 95% confidence intervals based on standard errors clustered at the establishment level.

Figure A25: Correlation between input costs and wage effects of managers



Notes: The graph shows the binscatter plots of manager fixed effects for input costs against manager fixed effects for wages. Each dot contains the same number of observations. On the y axis is manager FE in terms of log input costs.

Table A1 Characteristics of Treated and Control Establishments

Variables	Treated Establishments	Control Establishments
Median employment	11	11
Mean employment	25.0	22.8
Log hourly wage	5.002	4.987
Log annual income	12.131	12.126
Log employment growth from previous year	-0.009	-0.007
Log wage growth from previous year	0.021	0.025
Share of workers with higher education	0.184	0.202
Share of workers with vocational education	0.418	0.407
Share of female workers	0.468	0.512
Average worker age	38.74	38.64
Average worker experience	15.22	15.30
Log Value added per worker	6.048	6.054
Log Sales per worker	7.218	7.197
Establishment age	14.79	15.44
Number of establishments	5,875	5,875

Notes: This table presents summary statistics for all target establishments and control establishments. Each target establishment is matched to a control establishment using the matching approach detailed in Section 5.1. All the characteristics are calculated at one year before the merger occurs, and wage and employment growth is the growth rate from two years before the merger to one year before the merger. The medians are calculated as the average value of 10 observations around the median.

Table A2 Determinants of Being Target Firm

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Average log wage	0.0015*** (0.0002)						
Establishment year FE		0.0024*** (0.0003)					
Average worker FE			-0.0003 (0.0003)				
Manager FE				0.0019*** (0.0005)			
Establishment FE					0.0006 (0.0004)		
Log value added per worker						-0.0003* (0.0002)	
Growth in log wage							-0.0001 (0.0002)
No. of observations	1,396,573	1,396,403	1,388,607	413,277	413,277	699,741	1,122,048
Mean of Dep Var	0.0067	0.0067	0.0067	0.0062	0.0062	0.0054	0.0071
Mean of Indep Var	12.09	0.233	11.91	0.012	0.010	6.172	0.026
St. Dev. of Indep Var	0.609	0.258	0.304	0.172	0.145	0.499	0.336

Notes: (* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$) This table presents the linear probability model of the propensity to be a target firm. Dependent variable equals one if the establishment is acquired in the following year and zero otherwise. Establishment fixed effects and worker fixed effects are estimated from AKM regression. All regressions control for industry-year fixed effects and region-year fixed effects. Standard errors are clustered by establishment and reported in parentheses.

Table A3 Measuring Impact of Rent Extraction on Profitability

Dependent variable	Mean	Standard deviation	Q1	Median	Q3
<i>Replace soft with average (N=1425)</i>					
Adjusted difference between target manager FE and average manager FE	0.048	0.069	0	0.039	0.080
Target's wage bill/ Total asset of combined firm	0.21	0.23	0.06	0.17	0.28
Impact on ROA	0.63%	1.32%	0	0.43%	1.09%
<i>Replace soft with acquirer (N=1425)</i>					
Adjusted difference between target manager FE and acquirer manager FE	0.059	0.083	0	0.046	0.091
Target's wage bill/ Total asset of combined firm	0.21	0.23	0.06	0.17	0.28
Impact on ROA	0.72%	1.70%	0	0.46%	1.13%
<i>Mergers with both acquirer and target publicly listed (N=87)</i>					
Target CAR	12.3%	31.7%	-1.8%	8.6%	27.8%
Acquirer CAR	-0.3%	6.5%	-3.2%	-0.2%	2.6%
Portfolio CAR	2.1%	9.9%	-4.0%	2.4%	5.9%

Notes: This table calculates the contribution of rent extraction to the ROA of the combined firm and the cumulative abnormal returns of mergers between publicly listed firms. The difference in manager fixed effects between target and the mean adjusts for estimation error by shrinking the estimated manager FE towards the mean, where the weight varies inversely with the noise of the estimate. The contribution to ROA is calculated as difference in manager fixed effects multiplied by target's wage bill then divided by total assets of the combined firm (the formula is explained in Section 5.5). Wage bill and total assets are calculated in the year before merger. Manager fixed effects are estimated in Section 4.1. The cumulative abnormal return is calculated over an 11-day event window around the merger announcement. The data on stock prices of the merging firms are from SDC Platinum. The portfolio CAR refers to the cumulative abnormal return to a value-weighted portfolio of the target and acquirer. The medians and quantiles are calculated as the average value of 5 observations around the median/quantile.

Table A4 Wage Effects on Stayers: Alternative Matching Strategies

	Dependent Variable: Log Wage					
	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	No spillover	Match at year -2	Two controls per firm	Non-parametric: Target	Non-parametric: Control
Year t= -5	0.0002 (0.0056)	0.0001 (0.0051)	-0.0003 (0.0061)	-0.0009 (0.0057)	-0.0029 (0.0027)	-0.0001 (0.0026)
Year t= -4	0.0001 (0.0037)	-0.0014 (0.0037)	0.0039 (0.0046)	0.0041 (0.0048)	-0.0020 (0.0020)	0.0031 (0.0020)
Year t= -3	-0.0045 (0.0035)	-0.0021 (0.0036)	-0.0035 (0.0040)	0.0017 (0.0044)	-0.0006 (0.0017)	-0.0030 (0.0019)
Year t= -2	-0.0030 (0.0028)	-0.0040 (0.0028)	-0.0047 (0.0031)	0.0013 (0.0039)	0.0003 (0.0015)	0.0025 (0.0017)
Year t= 0	0.0018 (0.0033)	-0.0007 (0.0035)	-0.0055 (0.0039)	-0.0015 (0.0030)	0.0007 (0.0019)	0.0050*** (0.0016)
Year t= 1	-0.0077* (0.0040)	-0.0110*** (0.0040)	-0.0117*** (0.0042)	-0.0081** (0.0036)	-0.0062*** (0.0019)	0.0031* (0.0018)
Year t= 2	-0.0153*** (0.0043)	-0.0170*** (0.0043)	-0.0135*** (0.0043)	-0.0144*** (0.0046)	-0.0112*** (0.0022)	-0.0025 (0.0020)
Year t= 3	-0.0157*** (0.0049)	-0.0149*** (0.0045)	-0.0167*** (0.0057)	-0.0182*** (0.0055)	-0.0119*** (0.0024)	-0.0019 (0.0021)
Year t= 4	-0.0100** (0.0051)	-0.0111** (0.0047)	-0.0072 (0.0063)	-0.0094* (0.0057)	-0.0118*** (0.0027)	-0.0029 (0.0023)
Year t= 5	-0.0139** (0.0059)	-0.0068 (0.0051)	-0.0185*** (0.0071)	-0.0125** (0.0063)	-0.0049* (0.0029)	0.0009 (0.0026)
No. of observations	1,350,387	1,310,042	1,120,943	1,902,474	24,987,697	24,950,534

Notes: (* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$) This table shows the effect of mergers on wages of staying workers in target establishments from year -5 to year 5 relative to the merger (coefficients δ_τ in regression (10)). All regressions control for person fixed effects and year fixed effects. Column 1 is our baseline specification. Column 2 selects control establishments that have similar propensity score and wage and employment levels but are in different industry and different geographical region from the treated establishments. Column 3 matched treated establishments to controls based on covariates at year -2 instead of year -1. In Column 4 we choose two establishments as control for each target establishment based on the propensity score. Column 5 and Column 6 use the non-parametric estimator as in Davis et al. (2014) (see Appendix A.1.1 for details). Column 5 shows the wage effects for target establishments, and Column 6 shows the wage effects for control establishments of the baseline propensity score matching as a placebo test. Standard errors are clustered by establishment and reported in parentheses.