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

Alex Xi He   Daniel le Maire

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Abstract

This paper shows that some managers pay higher wage premiums to their workers and these managers are targets of M&As. We use a manager-firm-worker matched dataset covering the population of Denmark from 1995 to 2011 and develop a novel framework to measure manager styles in wage-setting by tracking workers and managers across 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. 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 acquisitions. Lower wages have little effect on firms' productivity, and therefore represent a transfer from workers to shareholders. The replacement of high-paying managers accounts for almost all of the wage decline and 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

*Alex Xi He: University of Maryland. Email: ahe@rhsmith.umd.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 Josh Angrist, Tania Babina, Sydnee Caldwell, Andrew Garin, Sabrina Howell, Holger Mueller, Simon Jäger, David Matsa, Nancy Rose, Antoinette Schoar, Kathryn Shaw, Geoffrey Tate, John Van Reenen, as well as seminar participants at MIT, University of Chicago, University of Utah, University of Toronto, LBS, HKUST, University of Hong Kong and University of Maryland 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 wage premiums conditional on productivity and other firm characteristics,[1] 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 first estimate managers' styles using a two-step approach. 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

[1] Accordingly, "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.

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.

We then test whether M&As discipline managers who pay high wages. M&As often involve replacing the management team, and is a major form of governance mechanism that disciplines manager behaviors. We first find that mergers and acquisitions on average 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, 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

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

persistent 2% wage decline relative to workers staying at control establishments, and effects are larger for young and low-skilled workers.[3] The negative wage effect *cannot* be accounted for by the selection of worker exits, and holds for various alternative matching estimators.

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.

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

[3] We 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.

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 also contributes to the labor economics literature on firm-specific wage premiums by introducing the manager dimension in wage setting. While competitive models of labor markets are predicated on the notion that firms have no power to set wages, recent works using matched employer-employee datasets show that firm-specific wage premiums contribute substantially to wage inequality (Card, Heining, and Kline 2013; Song et al. 2015). While other papers have looked at how wages are affected by firm profitability (Van Reenen 1996; Kline et al. 2017) and workers' outside options (Caldwell and Harmon 2018), we show that the identity of managers is an important determinant of how surpluses are allocated in wage bargaining. Our findings are consistent with an imperfectly competitive labor market, but also suggest that profit-maximizing financial activities such as M&As reduce managerial discretion and firms' power to set wages. The rent extraction in M&As is similar to outsourcing activities (Goldschmidt and Schmieder 2017) in which firms redraw boundaries to exclude outsourced workers from sharing rents.

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; Betton et al. 2008),[4] 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.[5] Several papers suggest

[4]Another 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).

[5]See, for example, Farrell and Shapiro (1990), Hoberg and Phillips (2010), Ashenfelter et al. (2014) on the effects of mergers on monopoly power; Ordover, Saloner, and Salop (1990), Hortaçsu and Syverson (2007) and Houde (2012) on vertical foreclosures; and Braguinsky et al. (2005), Devos et al. (2008), Siegel and Simons (2010), and Li (2013) 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),

that a large part of synergy 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 labor adjustments.

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

2 Data and Empirical Setting

2.1 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.[6]

and talent and innovation acquisition (Ouimet and Zarutskie 2011) as motivations for M&As.

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

Each employed worker is matched to her establishment. An establishments is a unique physical work location, such as an office, store, or 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.[7]

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.

2.2 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

[7] The 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.

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).[8] The bargaining at the firm or establishment level is between the managers and shop stewards, and the majority of agreements cover wage increases and not employment levels.[9]

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.

2.3 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.[10] 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

[8]Industry-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 observations of all firms that was in the standard rate system before 2001.

[9]The 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).

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

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.[11] 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.[12]

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 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. 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.[13] Very few mergers were chal-

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

[12] The 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 Figure A11 that M&As in the public sector do not reduce wages.

[13] A 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.

lenged and nearly none of the mergers were blocked.

3 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.

3.1 Estimation of Manager Styles in Wage-Setting

3.1.1 Empirical Methodology

We start by identifying individual managers' styles regarding wage-setting. We define "manager" as the top manager of each establishment.[14] A manager is "softer" if she pays higher wages to the same worker at the same firm and "tougher" if she pays lower wages. 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.[15]The two-step procedure can be derived from a wage bargaining model, which we show in the online Appendix.

In the first step, we estimate 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:

$$y_{ijt} = \psi_{jt} + \xi_i + \beta X_{ijt} + \epsilon_{ijt} \tag{1}$$

where ξ_i is worker fixed effects and X_{ijt} are time-varying worker characteristics, including

[14] Alternatively 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.

[15] One can also combine the two steps into a single step, which yields the same estimates of manager fixed effects. We choose to do two steps here for simplicity of the variance decomposition later.

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 and post-merger observations of target establishments in the estimation.

In the second step, we estimate an establishment-level regression with establishment fixed effects and manager fixed effects. This is 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:

$$\hat{\psi}_{jt} = \lambda_{m(j,t)} + \alpha_t + \gamma_j + \beta X_{jt} + \varepsilon_{jt} \tag{2}$$

where $\hat{\psi}_{jt}$ is the establishment-year fixed effect for establishment j in year t estimated from step 1, λ_m is manager fixed effect, α_t is year fixed effects, γ_j is establishment fixed effects. X_{jt} are time-varying establishment characteristics, including firm age, 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.[16] 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.

Our identification using manager fixed effects relies on the assumption that manager mobil-

[16] We 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.

ity 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 address these concerns in 3.1.3.

3.1.2 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.[17]

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. (2019) 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 establish-

[17] The 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.

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

3.1.3 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 Figure 1. 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 Appendix Figure A4, 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 (2) 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 Figure 2 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 Figure 2, 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 Appendix Figure A5 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.

3.2 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. Figure 3 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. Appendix Figure A6 shows that when a soft manager or a tough manager leaves, firms experience very little change in productivity both before and after the manager retires.

3.3 Correlations with Manager-specific Productivity

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 aspects of firms. We estimate manager styles in other dimensions using a manager fixed effects approach as in equation (2) with productivity or other firm-level variables on the left hand side. We use two measures of firm productivity: total factor productivity (TFP) and log value added per worker.[18] To measure TFP, we follow Schoar (2002) and estimate the following OLS regression separately for each three-digit standard industrial classification industry and year:

$$\log(y_{jkt}) = \alpha_{kt} + \beta_{kt}\ln(K_{jkt}) + \gamma_{kt}\ln(L_{jkt}) + \delta_{kt}\ln(M_{jkt}) + \epsilon_{jkt} \tag{3}$$

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

[18] We 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.

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).

Figure 4 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). Appendix Figure A8 shows that there is no correlation between manager effects in wage premiums and manager effects in leverage. Consistent with theory predictions in the Appendix, we find that soft managers hire less and have lower quit rates. Interestingly, there is a non-monotonic relationship between managers' wage fixed effects and managers' worker quality fixed effects, suggesting that 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, Appendix Figure A7 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 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.

4.1 The Effect of M&As on Wages and Employment

4.1.1 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 region[19] 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,

[19] There 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.

we select the comparison establishment with the closest propensity score calculated based on a rich set of establishment characteristics.[20] 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 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.[21] Appendix 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

[20] The 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.

[21] One 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.

over time.

4.1.2 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:

$$y_{jt} = \alpha_j + \gamma_t + \sum_{\tau=-3}^{5} \lambda_\tau D_{jt}(\tau) + \sum_{\tau=-3}^{5} \delta_\tau D_{jt}(\tau) \times Target_j + \epsilon_{it} \quad (4)$$

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 weighted by employment.[22] 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.

Figure 5(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 (Appendix Figure A8). Both job inflows and job outflows increase following M&As, and cancel out each other (Appendix Figure A9). Figure 5(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 equa-

[22] This 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.

tion at the worker level:

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

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.

Figure 6 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. Figure 6(b) shows that workers staying in target establishments lose 3-4% of their initial annual earnings. 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 (Appendix Figure A10).

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 $\tau = 2$).[23] Assuming that the loss of wage premium is 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 (5), adding interactions of worker covariates with period dummies, as well as interactions of covariates with period dummies and treatment. We find that young and low-skilled workers experience the largest wage declines after an acquisition (Appendix Figure A12).

[23] The 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 nominal wage growth.

4.1.3 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 (Appendix Figure A13). Second, Appendix Table A2 shows that the increase in the departure rate is quite uniform along the wage distribution.[24] Third, the average worker quality at target establishments measured by worker fixed effects[25] does not change significantly after mergers (Appendix Figure A14). 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.[26] Panel C of Table 3 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 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 3, we conduct a series of robustness tests on the wage results. We define

[24] The 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.

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

[26] The 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 Appendix Table A2 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.

"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. Column 6 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 show the effects of failed mergers in Appendix Figure A15. 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 A3. 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 Figure A16). Details of these estimators are in the online Appendix.

4.2 Mergers Target Firms with Soft Managers

Using the manager fixed effects estimated above, we test whether target establishments have softer managers. We estimate the regression

$$\hat{\lambda}_{mjt} = \gamma_1 Target_{jt} + \beta X_{jt} + \varepsilon_{jt} \tag{6}$$

where $\hat{\lambda}$ is manager fixed effects estimated from regression (2) 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 Panel A of Table 4 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:

$$\hat{\psi}_{jt} = \gamma_1 Target_{jt} + \beta X_{jt} + \varepsilon_{jt} \tag{7}$$

where $\hat{\psi}$ is the establishment-year fixed effect from equation (1). Column 2 in Table 4 shows that target establishments pay workers 2.3% higher wages on average conditional on productivity, industry, and region. These results are consistent with Davis et al. (2019), who shows that target firms of private equity buyouts pay about 2% higher average wages. According to equation (2), 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,[27] 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

[27] One 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.

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 4 shows that acquirers have on average 0.8% lower manager fixed effects and 1.1% lower wage premiums than comparable firms.

In Panel C 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. 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.

4.3 Manager Turnover Around Mergers

We have shown 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 Figure 7 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 channel for eliminating soft managers.

4.4 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 4.1, so that we can compare wage changes based on ex ante manager characteristics. We estimate the following equation:

$$
\begin{aligned}
w_{ijt} = & \sum_{\tau=-3}^{5} \lambda_\tau D_{ijt}(\tau) + \sum_{\tau=-3}^{5} \eta_\tau D_{ijt}(\tau) \times \text{SoftManager}_j + \sum_{\tau=-3}^{5} \delta_\tau D_{ijt}(\tau) \times MA_j \times \text{SoftManager}_j \\
& + \sum_{\tau=-3}^{5} \gamma_\tau D_{ijt}(\tau) \times MA_j \times (1 - \text{SoftManager}_j) + \alpha_{ij} + \beta X_{ijt} + \mu_t + \epsilon_{it} \qquad (8)
\end{aligned}
$$

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.[28]

Figure 8(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 and statistically insignificant wage cut of less than 1%.[29]

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 1) 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. Figure 8(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 man-

[28]One 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.

[29]To 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 (Appendix Figure A17).

ager is replaced by year 3 in Figure 8(c). Only workers in establishments that had soft managers and replaced those managers experience large wage declines. We also show in Appendix Figure A18 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 3.2).

Appendix Figure A19 shows that employment tends to increase or decline less in target establishments with soft managers. Appendix Figure A20 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.[30]

4.5 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 firm[31] 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. Figure 9 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,

[30] Appendix Figure A20 shows that the average worker quality of the newly hired workers increases at target establishments with soft managers, although the difference is not statistically significant. This suggests that reducing wage premiums due to soft managers could potentially allow firms to hire higher-quality workers.

[31] We 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.

i.e. replacement of soft managers.[32] As shown in Figure 8(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. By the envelope theorem,[33] 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:

$$\Delta = p(\beta\phi_{target} - \beta\phi_{replacement})^{+} \frac{(wL)_{target}}{A_{acquirer} + A_{target}} \tag{9}$$

where p is the probability of replacing a soft manager (which equals 0.56, according to Figure 7), the second part is the positive part of the difference between the target's manager fixed effects and the replacement managers' fixed effects, 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.[34] 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).

We consider two scenarios in Table 5: replacing target managers with an average manager from the pool and replacing target managers with managers similar to the acquirers' managers.[35] In the first scenario, the sample average of the term Δ among all M&As is 0.63 percentage points. 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

[32] Here we use changes in manager fixed effects and not actual wage changes since actual wage changes are a result of rent extraction and many other factors, like productivity changes.

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

[34] The 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.

[35] Since manager fixed effects are estimated without post-merger observations, we usually don't have the manager fixed effects for the actual replacement managers.

establishments. Under the second scenario, the average impact on ROA is even bigger (0.72 percentage points), since acquirers on average have tougher-than-average managers. Given that 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%).[36] 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.[37]

4.6 Industry Concentration and Rent-Extracting Mergers

In less competitive industries, firms have higher profits, and there is more room for managerial discretion in wage setting. 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. Proposition 3 in the Theory Appendix 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,

[36]The 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.

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

and M&As will lead to larger wage declines.

Consistent with our theoretical prediction, we find that there are more soft managers in highly concentrated industries. Appendix Figure A21 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.

To test whether target firms have softer managers in more concentrated industries, we estimate the following extended version of equation (6):

$$\hat{\lambda}_{mjt} = \gamma_1 Target_{jt} + \gamma_2 Target_{jt} \times HighConcentration_j + \beta X_{jt} + \varepsilon_{jt} \qquad (10)$$

where $HighConcentration_j$ is a dummy variable that equals one if firm j is in a industry with HHIs over 1000.[38] Column 3 of Table 4 shows that manager fixed effects are significantly larger only in concentrated industries.

We find large wage declines following mergers in concentrated industries, and no significant wage changes in competitive industries (Appendix Figure A22). 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, where mergers lead to slightly more positive employment changes in more concentrated industries due to rent extraction.[39]

[38] 1000 is close to the median HHI for all 127 industries. 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.

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

5 Robustness and Discussion

5.1 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.[40] 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 Appendix Figure A23 shows that cross-city 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 Appendix Figure A23 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

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

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 Appendix Figure A23 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.

5.2 Are Mergers Efficiency-Enhancing?

M&As may increase profits by replacing unproductive managers and raising productivity of the target firms (Lang et al. 1989; Wang and Xie 2009). We test whether acquirers target poorly managed firms in the last two columns of Table 4 Panel A. 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. This finding implies that on average M&As mainly 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 Figure A24 shows that target firms with less productive managers seem to have more positive wage changes following mergers. Since targets on average have slightly 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 reductions due to rent extraction.

5.3 Monopoly Power

Monopoly power is arguably the most cited motivation for mergers. In theory, 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.

5.4 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.

5.5 Why are managers soft?

The previous results show that soft managers reduce firm profits and are more likely to have their firms acquired and leave the managerial positions, but why are they soft despite seemingly negative consequences for their firms and themselves? While we do not take a stand on the underlying drivers of manager softness, our evidence is most consistent with the manager styles being fixed personal traits (e.g. a progressive notion of fairness), and we find little evidence that the the higher wage acts as an efficiency wage. Below we consider several other possibilities for explaining different manager styles.

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.

We use two measures to approximate ownership of managers. The first measure is family firms. Following Bennedsen et al. (2007), we classify a firm as a family firm if managers in different years are family members. The second measure is whether the manager is a founder, e.g. if the manager was at the firm in the year the firm was established. By both measures, the majority of target firms have managers that are not owners – about 21% of the target firms and 31% of all firms are family firms, and about 35% of target firms and 38% of all firms have managers that are founders. Appendix Figure A25 shows that family firms experience slightly larger wage reductions following M&As, and wage effects are similar for firms with founder managers and non-founder managers. These results show that managers may set generous wages even when they are the owners of the firm, and suggests that soft managers are likely not due to agency conflict. This is also consistent with soft managers performing as well as tough managers on other dimensions, like productivity and input costs.

Compensation for risks

Another possibility is that soft managers pay higher wages to compensate for the higher risks of job loss associated with M&As. However, in terms of magnitudes the risks are too small to explain the excess wage premiums. Suppose that workers are risk averse, since the average cumulative unemployment spell following M&As is less than 0.02 years (Appendix Figure ?) and the probability of takeovers is less than 1%, the amount of wage increase required to compensate for the risk would be negligible compared to the average annual wage premium of 2% observed in the target firms.

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 Figure A26), which is consistent with Pontiff, Shleifer, and Weisbach (1990). Anecdotal evidence also suggests that amenities often worsen after mergers.[41]

6 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 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.

[41]For 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.

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; Ma, Sraer, and Thesmar 2018). Our results indicate that with increasing market power (De Loecker and Eekout 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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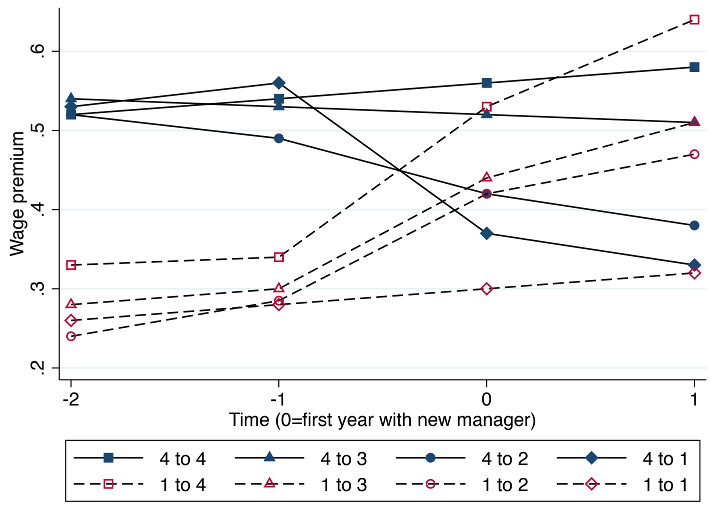
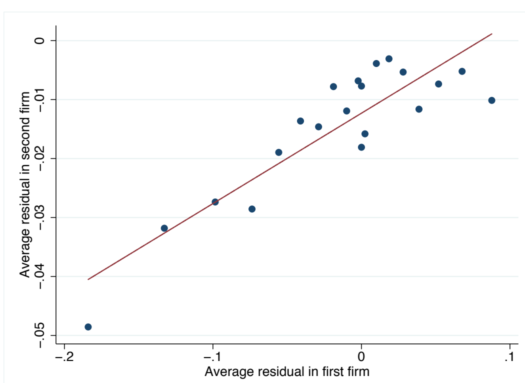
Figure 1: Mean wage premiums of firms that switch managers classified by quartile of manager effects for departing and entering managers

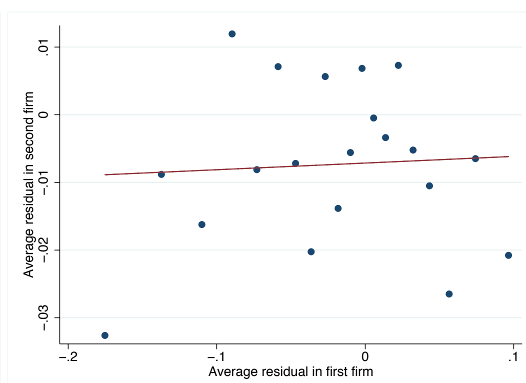


Notes: Figure shows mean wage premium of establishments that change managers. Managers are classified into quartiles based on their estimated manager fixed effects $\hat{\lambda}_m$.

Figure 2: Correlation of managers' wage residuals across establishments



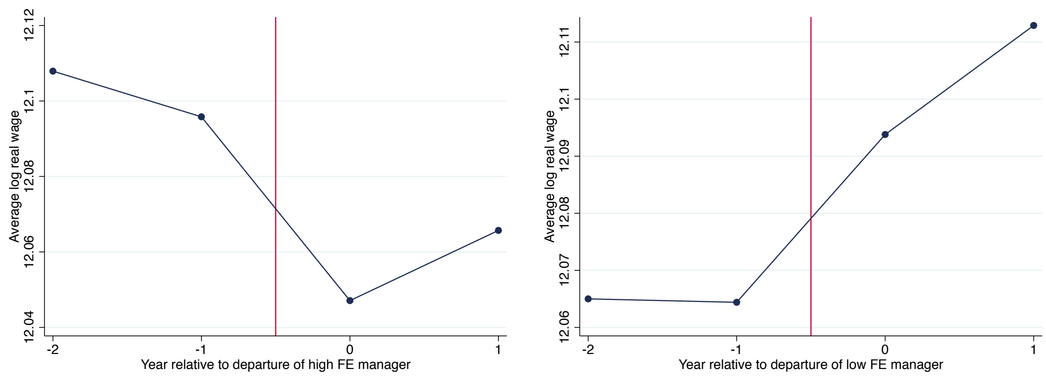
(a) Data



(b) Placebo

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 0.1533 with t-statistic of 8.9; the regression coefficient in the right figure is 0.0098 with t-statistic of 0.03.

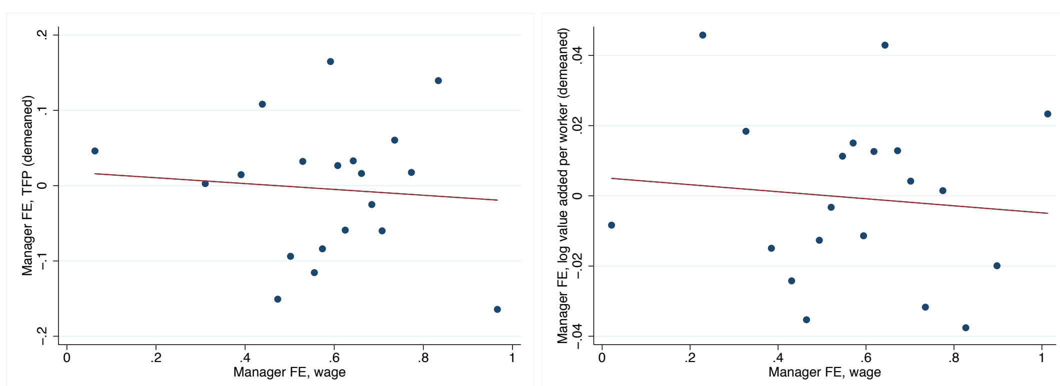
Figure 3: Event study of exogenous manager departures



(a) High FE manager departure (b) Low FE manager departure

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.

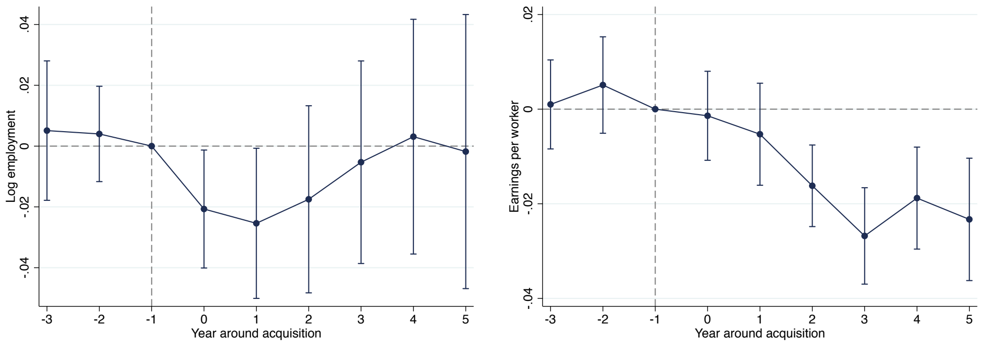
Figure 4: Correlation between manager styles in wage setting and productivity



(a) Value-added per worker (b) TFP

Notes: The graph shows the binscatter plots of manager fixed effects in measures of productivity 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 log value-added per worker. In (b), 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 (measured in number of hours), capital and materials, with separate regressions for each three-digit industry. The regression coefficient in the left figure is -0.010 with t-statistic of 0.7; the regression coefficient in the right figure is -0.036 with t-statistic of 0.8.

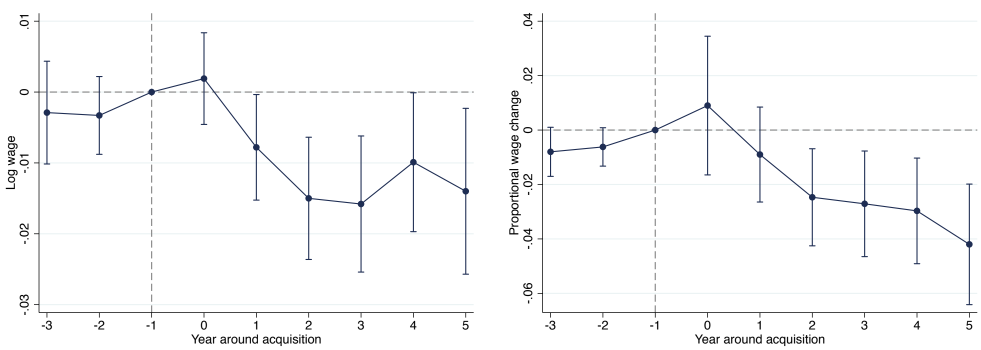
Figure 5: Target establishments' employment and wages following M&As



(a) Employment (b) Log Earnings per worker

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 (4). 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 log of total wage bill divided by employment. The vertical lines denote 95% confidence intervals based on standard errors clustered at the establishment level.

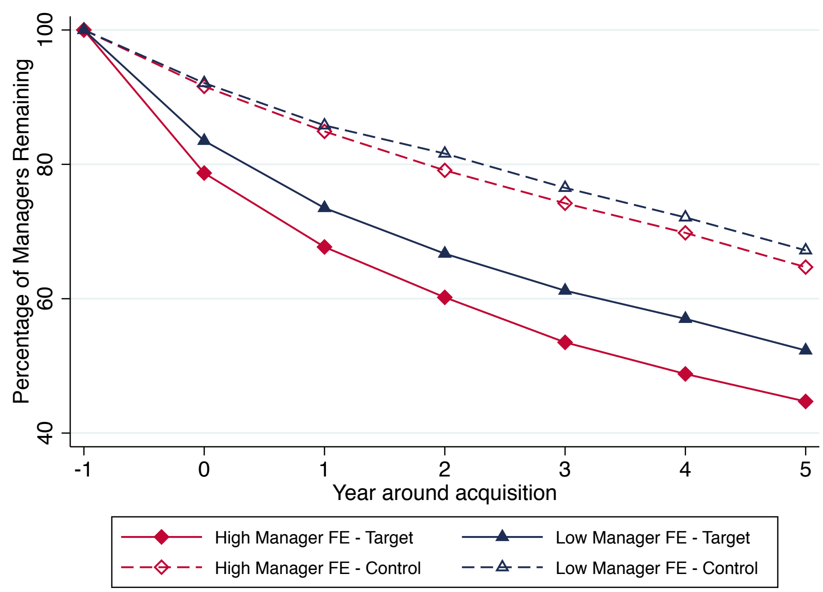
Figure 6: Changes in staying workers' wages after M&As



(a) Log annual wage (b) Proportional wage change

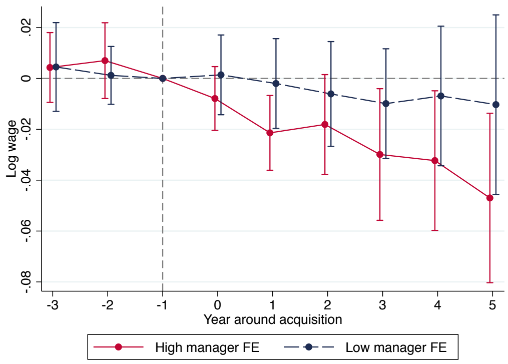
Notes: The figure shows regression coefficients and associated confidence intervals for the difference between staying workers at target and control establishments, i.e., the δ_τ from the difference-in-differences model in (5). The coefficient in $\tau = -1$ is normalized to zero. All regressions in 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 (including zero) normalized by the average annual earnings from $\tau = -3$ to $\tau = -1$. The vertical lines denote 95% confidence intervals based on standard errors clustered at the establishment level.

Figure 7: Manager turnover around M&As

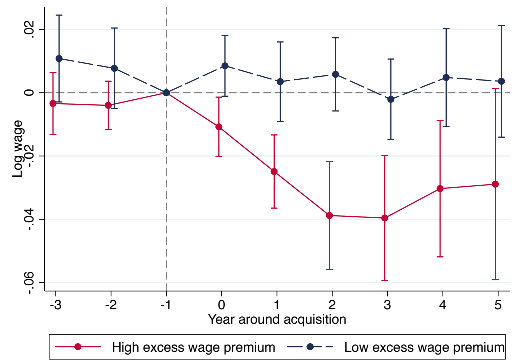


Notes: This figure plots the percentage of managers who are at treatment or control establishments in the year before acquisition that remain in the same establishment for each year after the acquisition. By definition, in year -1, 100% of managers remain in their initial establishment. Managers are 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.

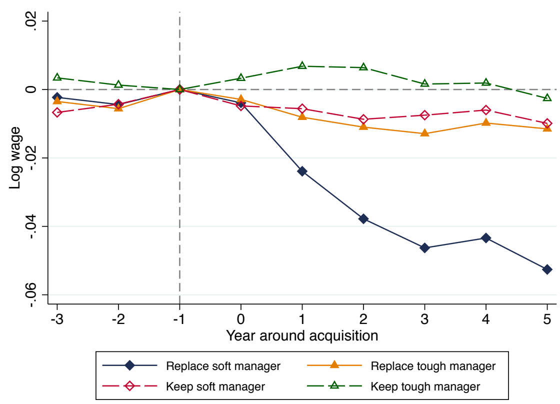
Figure 8: Heterogeneity of wage effects by pre-merger wage premium and manager FE



(a) By pre-merger manager FE



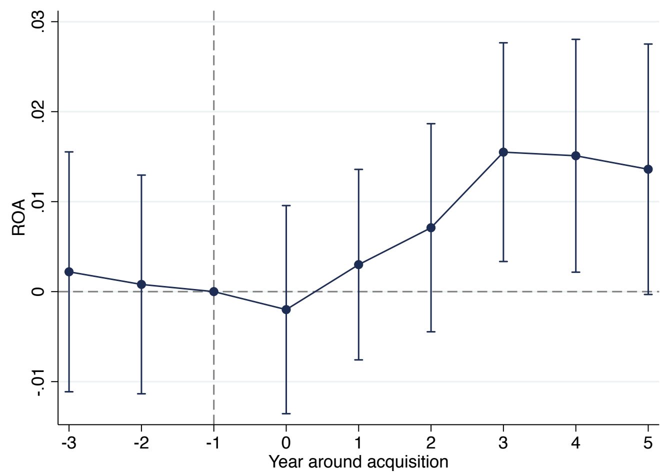
(b) By pre-merger excess wage premium



(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 (8). 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 ($\hat{\psi}_{jt}$ 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 9: 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: $y_{jt} = \alpha_j + \gamma_t + \sum_{\tau=-3}^{5} \delta_\tau D_{jt}(\tau) + \beta X_j t + \epsilon_{jt}$, 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.

Table 1 Summary Statistics

Firm Data

	All firms		Target firms		Acquirer firms	
Observations (firm-year)	2,206,320		5,244		3,483	
Variable	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
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		Target firm employees		Acquirer firm employees	
Observations (worker-year)	41,706,676		286,114		1,739,780	
Variable	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
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

	OLS (Plug in)	*Leave Out (Kline et al. 2018)*
Step 1: Estimation of Establishment Year Fixed Effects		
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 3.1. The first step estimates equation (1). 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 (2), 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 (2019). 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 Wages

	(1)	(2)	(3)	(4)	(5)	(6)
	A. Log wage of staying workers					
Dependent variable	Log annual wage	Log annual wage	Log annual wage	Log annual wage	Log annual wage	Log hourly wage
Short-run effect	-0.008	-0.007	-0.005	-0.010	-0.005	-0.014
	(0.004)	(0.004)	(0.003)	(0.006)	(0.004)	(0.005)
Long-run effect	-0.014	-0.012	-0.010	-0.018	-0.015	-0.017
	(0.005)	(0.004)	(0.004)	(0.009)	(0.005)	(0.006)
Person*firm FE	X	X	X	X	X	X
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	1,016,647
	B. Bouding exercise in Lee (2009) for wage effects of staying workers					
Year relative to M&A	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: 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 (5). 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 (B) 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 4 Manager Style in Target and Acquiring Firms

	(1)	(2)	(3)	(4)	(5)
Dependent Var.	Manager FE in wage	Establishment year FE	Manager FE in wage	Manager FE in TFP	Manager FE in VA per worker
			A. Target firms		
Target	0.017	0.023	0.006	-0.012	-0.005
	(0.007)	(0.004)	(0.004)	(0.021)	(0.015)
Target * High concentration			0.032		
			(0.015)		
			B. Acquirer firms		
Acquirer	-0.008	-0.011	-0.005	0.013	0.000
	(0.005)	(0.002)	(0.006)	(0.009)	(0.007)
Acquirer * High concentration			-0.013		
			(0.008)		
Industry*Year FE	X	X	X	X	X
Region*Year FE	X	X	X	X	X
No. of estab.	53,748	324,390	53,748	53,748	53,748

C. Propensity to be acquired

Average log wage	Estab. year FE	Average worker FE	Manager FE	Estab. FE	Log VA per worker
0.0015	0.0024	-0.0003	0.0019	0.0006	-0.0003
(0.0002)	(0.0003)	(0.0003)	(0.0005)	(0.0004)	(0.0002)
# of obs					
1,396,573	1,396,403	1,388,607	413,277	413,277	699,741

Notes: Panel A and B 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 3.1. 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 (2), with dependent variables being TFP and log value-added per worker respectively. Total factor productivity (TFP) is estimated by equation (3), and since labor input is measured by number of workers, the wage level does not affect TFP directly. Panel C 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. The mean of dependent variable is 0.006. 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 5 Measuring Impact of Rent Extraction on Profitability

Dependent variable	Mean	Standard deviation	Q1	Median	Q3
Replace soft with average (N=1425)					
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%
Replace soft with acquirer (N=1425)					
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%
Mergers with both acquier and target publicly listed (N=87)					
Target CAR	12.3%	31.7%	-1.8%	8.6%	27.8%
Acqurier 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 4.5). Wage bill and total assets are calculated in the year before merger. Manager fixed effects are estimated in Section 3.1. 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 6 Alternative Mechanisms

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

Notes: This table presents the effects of mergers on wages of staying workers in the target establishments (based on equation (5)) 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.