Essays on Finance and Labor Markets

by

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Abstract

This thesis consists of three chapters in corporate finance and labor economics. The first two chapters study the interaction of the financial sector and labor market, and the last chapter focuses on corporate R&D investment.

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

The second chapter (co-authored with Daniel le Maire) investigates the effects of liquidity constraints on employment and earnings by exploiting a mortgage reform in Denmark in 1992, which for the first time allowed homeowners to borrow against housing equity for non-housing purposes. Liquidity-constrained homeowners extracted housing equity, increased debt levels and experienced higher earnings growth after the reform. In contrast, the reform had little impact on employment and earnings of homeowners with high liquid asset holdings. Consistent with models of job search with risk aversion, the option to borrow against housing equity allows individuals to seek jobs that have higher earnings growth but higher unemployment risks. This effect is larger for low-income and older individuals. The results imply that relaxing liquidity constraints can increase output, and policies restricting mortgage refinancing during economic distress may backfire in recessions.

The third chapter studies the spillovers of corporate R&D investment across different technological fields. I build a measure of technological distance between firms using the citation-based innovation network, which incorporates knowledge spillovers from upstream technological fields to downstream technological fields. I then use this measure to estimate the impact of technology spillovers using panel data on U.S. firms. I find that spillovers from firms innovating in upstream fields are quantitatively as important as spillovers from firms innovating in same fields. Consistent with the idea that firms innovate more when there is more past upstream innovation to build on, firms' R&D investments respond positively to R&D investments of firms in upstream fields, but not to R&D investments of firms in downstream fields or in the same fields. Smaller firms on average operate in more upstream technological fields and generate more spillovers and higher social returns, which is contrary to the findings of previous research.

JEL Codes: G34, J30, D22

Thesis Supervisor: David Autor
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***

This thesis is dedicated to the memory of Cara Anne Nickolaus, whose kindness and dedication inspire me to be a better person every day.

Contents

Chapter 1

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

1.1 Introduction

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

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

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

Following predictions from the model, we estimate managers' styles using a two-step approach. In the first step, we estimate time-varying establishment-specific wage premiums using a two-way fixed effects regression similar to Abowd, Kramarz, and Margolis (1999, AKM for short). In the second step, we estimate manager fixed effects in explaining the wage premiums conditional on firm fixed effects and productivity. Manager fixed effects are identified by manager mobility across firms as in Bertrand and Schoar (2003). We construct a manager-firm-worker matched panel data set covering all firms and all workers in Denmark from 1995 to 2011.[2] The largest connected set covers more than 75% of the workers and contains over 100,000 managers.

[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.

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

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

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

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

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

sistent 2% wage decline relative to workers staying at control establishments.[4] The negative wage effect cannot be accounted for by the selection of worker exits, and holds for various alternative matching estimators. Young and low-skilled workers experienced the largest wage declines. There was little change in wages at the acquiring firms after mergers.

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

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

We consider a number of alternative explanations for our results. First, efficiency-enhancing

[4] 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.

mergers may target firms with unproductive and inefficient managers who happen to be soft. We show that the differences in manager productivity measured by TFP or value added per worker are statistically insignificant from zero between target firms and acquirer firms. This indicates that M&As discipline managers paying high wages but not managers with low productivity. Second, mergers may reduce wages due to increases in labor market concentration and monopsony power. We take several approaches to measure the impact of mergers on monopsony power[5] and find that the majority of mergers in our sample has little effect on monopsony power. We also find large and significant wage declines, even for mergers that supposedly have no impact on labor market power. Third, the wage decline could result from "value-destroying" mergers or mergers that eliminate product market competitors to preempt competition. However, we find similar wage declines for horizontal and non-horizontal mergers, and for production and non-production workers. We also find little change in target establishments' employment levels and exit rates, and an increase in the profitability of combined firms, which suggests that value-destroying mergers are exceptions rather than the norm. Fourth, manager styles and wage reductions are not driven by firms' different propensities to automate or outsource, since we observe large wage declines even for workers whose work could not be automated or outsourced.

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

Our paper offers a new perspective on how M&As create shareholder value and provides empirical support for the claim that M&As transfer rents from workers to shareholders (Shleifer and Summers 1988). While most existing studies find higher stock market returns and better perfor-

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

mance for the combined company following mergers (Bradley et al. 1988; Andrade et al. 2001; Moeller et al. 2004; Betton et al. 2008),[6] the sources of this value creation are less clear. A large literature in industrial organization and corporate finance studies the effects of mergers on monopoly power, productive efficiency, and vertical foreclosures.[7] Several papers suggest that a large part of synergy in mergers comes from replacing management in poorly managed targets (Lang et al. 1989; Wang and Xie 2009). Our paper is the first to empirically disentangle the redistribution of wealth among workers and shareholders from efficiency improvements using a comprehensive data set of mergers. Our paper also complements the literature on the negative relationship between labor protection and merger returns (John et al. 2015; Dessaint et al. 2017) by providing direct evidence of how acquirers benefit from employment and wage adjustments.

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

1.2 Theoretical Framework

In this section, we introduce a simple two-period wage bargaining model between a firm's manager and a homogeneous group of workers. We use the model to show how individual managers' styles—by which we mean their propensity to pay above-market wages—affect wage setting and

[6] 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).

[7] See, for example, Farrell and Shapiro (1990), Kim and Singal (1993), Dafny (2009), Hoberg and Phillips (2010), Ashenfelter et al. (2014), Blonigen and Pierce (2016), Miller and Weinberg (2017) on the effects of mergers on monopoly power; Ordover, Saloner, and Salop (1990), Hortaçsu and Syverson (2007) and Houde (2012) on vertical foreclosures; and Pesendorfer (2003), Braguinsky et al. (2005), Devos et al. (2008), Siegel and Simons (2010), Li (2013), and Sheen (2014) on productivity and efficiency. Other papers have looked at automation (Olsson and Tåg 2016; Ma et al. 2017), financial constraints (Erel et al. 2015), preempting competition (Cunningham et al. 2018), talent and innovation acquisition (Ouimet and Zarutskie 2011, Phillips and Zhdanov 2013), and growth options and investment opportunities (Levine 2017) as motivations for M&As.

profits. We then show how mergers create value by replacing inefficient managers, and disentangle the effects of various channels—efficiency, monopoly power, and rent extraction—on employment and wages. Finally, we develop an empirical measure of manager styles based on the relationship between manager turnovers and changes in wages and productivity.

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

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

$$\max_{L_j} \pi_j = p_j(L_j)T_{jm}F(L_j) - w_jL_j \tag{1.1}$$

The first-order condition is:

$$w_j = \left(1 - \frac{1}{\varepsilon_j}\right) p_jT_{jm}F'(L_j) \tag{1.2}$$

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

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

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

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

$$w_j = (1-\beta)b_j + \beta \frac{p_j T_{jm} F(L_j)}{L_j - \phi_m \overline{L_j}} \tag{1.3}$$

Equations (1.2) and (1.3) jointly determine wage w_j and employment L_j. The following proposition summarizes how manager styles in wage setting ϕ_m affect firm outcomes:

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

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

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

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

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

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

$$\phi_{m(j)} \leq 1 - \frac{\beta}{1 - (1-\beta)\frac{b_j L_j}{p_j T_{jm} F(L_j)}} \tag{1.4}$$

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

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

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

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

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

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

$$\log w_j = (1-\beta)\log b_j + \beta \log\left(\frac{p_j T_{jm} F(L_j)}{L_j}\right) + \beta\phi_{m(j)} \tag{1.5}$$

In this equation log wage is the sum of three parts: the first part is reservation wage, the second part is sharing of average log value added per worker, and the third part is due to manager discretion. Therefore, in the panel data, when we include both firm fixed effects and manager fixed effects and control for productivity, the manager fixed effects would identify the term $\beta\phi_m$. Since the two-way fixed effects model requires a lot of manager mobility across firms, we also take a complementary approach of regressing wages on productivity, and industry and region fixed effects interacted with year fixed effects, and the residual from this regression is $\beta\phi_m$ if the error terms are uncorrelated with manager styles.[10] We discuss the estimation of manager styles in more detail in Section 4.1.[11]

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

In our stylized model, soft managers get higher private benefits from paying higher wages; al-

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

[11]Although workers are homogeneous here, the model can easily incorporate worker heterogeneity by having workers with different productive units, and wage is price per productive unit. In that case, there is an additional term for worker ability in equation 1.5, which can be identified by worker mobility in the first step of our empirical approach.

ternatively, soft managers may give workers higher bargaining power β. In a more general model with both heterogeneous private benefits and bargaining power among managers, the treatment effect of managers we identify combines various structural parameters. However, our approach still identifies the correct ranking of managers' effect on wage premiums as long as managers' bargaining power is uncorrelated with their productivity. Our qualitative results also remain unchanged: soft managers raise wages and lower profits, and therefore are replaced in M&As.

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

1.3 Data and Empirical Setting

1.3.1 Data Sources

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

The worker data are extracted from the Integrated Database for Labor Market Research, or IDA, which covers the entire Danish population aged 15 to 74, including the unemployed and those who do not participate in the labor force. The IDA associates each person with her unique identifier, and provides annual data on many of the individual's socioeconomic characteristics, such as hourly wage, education, and occupation. We measure the hourly wage rate as annual labor income plus mandatory pension fund payments divided by annual hours.[12] Each employed worker is matched to her establishment. An 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.[13]

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

1.3.2 Danish Labor Market

We first highlight several key features of the labor market in Denmark to provide context for our following analysis. Denmark has a flexible labor market with low hiring and firing costs. Botero et al. (2004) classified Denmark as one of the most flexible labor markets in the world, comparable

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

[13]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.

to the United States. Unlike in many countries in continental Europe, employment protection is very weak in Denmark, and it is easy for Danish firms to hire and fire employees. In 1995 the average tenure in Denmark was the lowest in continental Europe at 7.9 years, similar to the level in UK (7.8 years) and lower than in Germany (9.7 years). Unemployed workers receive generous unemployment benefits, but are also incentivized to search for jobs through active labor market policies, which together form what is called the "flexicurity" model.

Like other Scandinavian countries, Denmark used to have an industry-level standard rate wage bargaining system until the 1980s, but since then wage bargaining has been decentralized to the individual or establishment level. By the start of our sample in 1995, only 16% of the private labor market was still covered by the standard rate system, whereas the majority of wage contracts were and still are negotiated at the worker-firm level (Dahl et al. 2013).[14] The bargaining at the firm or establishment level is between the managers and shop stewards, and the majority of agreements cover wage increases and not employment levels.[15]

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

1.3.3 Construction of the M&A Sample

We identify mergers and acquisitions using the changes in *firm* identifiers of establishments because establishment identifiers remain constant despite changes in ownership.[16] We identify a merger if

[14] 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 all firms in the standard rate system before 2001.

[15] 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).

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

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

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

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

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

[18] 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.

We identify around 3700 mergers within Denmark from 1995 to 2011. Figure A1 plots the number of mergers between Danish firms in each year. Figure A2 plots the percentage of workers in Denmark working in acquirer or target firms in each year. On average about 1% of workers each year work in one of the target firms, and about 5% of workers work in one of the acquirer firms. This indicates that mergers affect a large proportion of workers in the economy. Table 1 reports summary statistics for this sample. On average, acquirer firms are larger and more productive than target firms. Target firm employees are on average younger, less educated, and less experienced, whereas workers at acquirer firms are older, more educated, and more experienced than the average worker in the economy.

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

1.4 Do Individual Managers Matter for Wages?

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

[19] 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.

1.4.1 Estimation of Manager Styles in Wage-Setting

Empirical Methodology

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

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

$$y_{ijt} = \psi_{jt} + \xi_i + \beta X_{ijt} + \epsilon_{ijt} \tag{1.6}$$

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

We then estimate an establishment-level regression with establishment fixed effects and manager fixed effects similar to the manager fixed effects regression in Bertrand and Mullainathan (2003). We impose the requirement that managers have to be at each firm for at least two years, to

[20] 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.

ensure that managers are given a chance to exert their influence at a given company. Our specification is as follows:

$$\hat{\psi}_{jt} = \lambda_{m(j,t)} + \alpha_t + \gamma_j + \beta X_{jt} + \varepsilon_{jt} \quad (1.7)$$

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 the share of female workers, the share of workers in each education group, the average age and experience of workers, and dummies for each decile of value added per worker.[21] Similar to the AKM regression, the identification comes from managers changing establishments. Establishment fixed effects and manager fixed effects are separately identifiable within the largest connected set of establishments linked by manager movements.

Estimation Results

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

To summarize our findings, for each step we report the standard deviations of the estimated fixed effects, the correlations between the two fixed effects, and the adjusted R^2 statistic. The fixed effects are unbiased but inconsistent estimates of the unobserved effects; therefore the variance and

[21] 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.

[22] 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.

covariance of the fixed effects will be biased due to estimation error. We adopt the leave-out estimator in Kline et al. (2018) to adjust for this problem and to obtain unbiased estimates for variance and covariance terms in models with two-way fixed effects and unrestricted heteroscedasticity. The bias-adjusted correlation between manager and establishment fixed effects is quite small (-0.03), suggesting that there is not much systematic manager sorting across establishments based on fixed wage premium differences.

How big is the variation in manager effects on wages? The estimated manager fixed effects have a corrected standard deviation of 0.082, which is economically significant and bigger than the standard deviation of the estimated establishment fixed effects (0.075). The variance in manager effects accounts for 31% of the between-firm wage variation. A move from the 10th to the 90th percentile in the distribution of manager fixed effects, assuming that it is normally distributed, is associated with a 21% increase in workers' wages.

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 1-1. 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 Figure A16, 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 (1.7) 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 1-2 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 1-2, 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 Figure 1-3 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.

1.4.2 Event Study of Exogenous Manager Departures

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

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

We re-estimate the manager fixed effects for all managers using data *outside* the four-year window used for the event studies. The reason is that manager fixed effects are measured with error, and if we defined a soft manager as one who happened to have positive wage shocks in the firm within the event study, we would have found a spurious relationship between wages and the exit of such a manager even if she had no causal impact on wages. Figure 1-4 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. Figure A17 shows that when a soft manager or a tough manager leaves, firms experience very little change in productivity despite large changes in wages. This suggests that the average productivity of managers paying very high or very low wages is close to the average productivity of all managers.

1.4.3 Correlations with Manager Productivity and Leverage

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

To answer these questions, we analyze the correlation between manager fixed effects in wages and manager fixed effects in productivity and other firm policies. We estimate manager styles in other dimensions using a manager fixed effects approach similar to equation (1.7). We use two measures of firm productivity: total factor productivity (TFP) and log value added per worker.[23] To measure TFP, we follow Schoar (2002) and estimate the following OLS regression separately for each three-digit standard industrial classification industry and year:

$$\log(y_{jkt}) = \alpha_{kt} + \beta_{kt}\ln(K_{jkt}) + \gamma_{kt}\ln(L_{jkt}) + \delta_{kt}\ln(M_{jkt}) + \epsilon_{jkt} \tag{1.8}$$

[23] 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.

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

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

Figure 1-5 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). Figure 1-5 shows that there is no correlation between manager effects in wage premiums and manager effects in leverage. Consistent with theory predictions, we find that soft managers hire less and have lower quit rates. Interestingly, there is a non-monotonic relationship between the wage fixed effects and the worker quality fixed effects, and managers who are neither too soft nor too tough have the most skilled workers.

Lastly, one explanation for soft managers is that they are soft bargainers and are incapable of keeping the wage costs down. If this is true, we would expect that soft managers also have higher input costs. However, Figure A25 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.

1.4.4 Industry Concentration and Manager Discretion

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

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

Consistent with our theoretical prediction, we find that there are more soft managers in highly concentrated industries. Figure 1-6 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.

1.5 Manager Styles and Wage Changes Following M&As

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

1.5.1 The Effect of M&As on Wages and Employment

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[24] as the target;
- It is in the same quintile of employment and average establishment wage as the target.

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

The key identifying assumption is that workers' wages in target and control establishments would have followed parallel trends in $\tau > 0$ if no merger had occurred in the treated establishment. Admittedly mergers and acquisitions are not exogenous events, but our estimation strategy is still valid if the target is selected based on the level of wages or productivity. Potential threats to identification would be unobserved shocks that affect both the outcomes and the timing of merger in the treated establishments. For instance, acquirer firms could target firms on the verge of wage reductions. Importantly, we do *not* match on pre-merger employment or wage growth, as the three criteria above and all covariates used in estimating the propensity score are measured for the year before the merger, so the pre-trends can be used to evaluate the common trends assumption. As we will see, the fact that wage decline occurs precisely at the moment of merger mitigates this concern. As a robustness check we also match target establishments to controls two years before the merger and get similar results.[26] Table A1 shows that control establishments and target establishments had

[24]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.

[25]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.

[26]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.

similar characteristics one year before the merger.

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

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:

$$y_{jt} = \alpha_j + \gamma_t + \sum_{\tau=-3}^{5} \lambda_\tau D_{jt}(\tau) + \sum_{\tau=-3}^{5} \delta_\tau D_{jt}(\tau) \times Target_j + \epsilon_{it} \tag{1.9}$$

where y_{jt} is outcome variable at establishment j in year t. We denote $\tau = t - d$ as the number of years relative to the merger occurring in year d. The model includes establishment fixed effects, α_j, calendar year fixed effects, γ_t, and leads and lags around event time $D_{jt}(\tau)$. $Target_j$ is a dummy variable indicating whether establishment j is a target or control. The coefficients of interest are δ_τ, which capture the effect of a merger in year τ in the target establishments and are normalized to zero in $\tau = -1$. The standard errors are clustered at the establishment level. The observations are weighted by employment.[27] The event study approach allows us to estimate the dynamic treatment effects of mergers on establishment outcomes over time as well as to use the effects in the pre-period to evaluate the common trends assumption.

Figure 1-7(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 (Figure A3). In Appendix Figure A6 we show that following a

[27] 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.

merger both job inflows and job outflows increase, and cancel out each other. Figure 1-7(b) shows that target establishments' earnings per worker (EPW) decline by 2-3%, and this decline persists over time.

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

$$w_{ijt} = \alpha_{ij} + \gamma_t + \sum_{\tau=-3}^{5} \lambda_\tau D_{it}(\tau) + \sum_{\tau=-3}^{5} \delta_\tau D_{it}(\tau) \times Target_j + \beta X_{it} + \epsilon_{it} \tag{1.10}$$

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

Figure 1-8 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 1-8(b) shows that workers staying in target establishments lose 3-4% of their initial annual earnings. Figure 1-8(c) shows that hourly wages also fall by 2% after mergers, suggesting that the reduction in annual earnings is not driven by a reduction in hours worked. We use the same matching method to estimate wage changes at acquirer firms, and find that workers in acquiring firms do not experience any wage cuts after mergers (Figure A4).

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

[28]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.

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 (1.10), adding interactions of worker covariates with period dummies, as well as interactions of covariates with period dummies and treatment. Figure A10 shows that young and low-skilled workers experience the largest wage declines after a merger.

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 (Figure A7). Second, Table 3 shows that the increase in the departure rate is quite uniform along the wage distribution.[29] Third, Figure A9 shows that the average worker quality at target establishments measured by worker fixed effects[30] does not change significantly after mergers. Finally, we use the trimming approach in Lee (2009) to bound the effects of selection without imposing any assumptions on which workers would stay. In particular, given that the proportion of workers staying in target establishments is smaller than the proportion of workers staying in control establishments in each period, we trim the sample of workers staying in the control establishments such that the proportion of staying workers is the same for targets and controls. We then estimate an upper (or lower) bound of the unbiased treatment effect by trimming the upper (or lower) part of the distribution of wage changes among

[29]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.

[30]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).

remaining workers in control establishments.[31] Panel C of Table 4 shows that the upper bounds of wage effects are still negative for four years after merger, and the entire confidence interval is below zero for the second year after merger. This evidence indicates that the bias due to differential selection of staying workers is likely to be small and cannot explain all of the negative effects on staying workers' wages.

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

We show the effects of failed mergers in Appendix Figure A8. The wage effects for mergers that were never completed are not statistically significant, suggesting that the observed wage cuts following mergers are not due to unobserved heterogeneous wage trends between targets and controls.

We obtain similar results using various matching estimators, reported in Appendix Table A4. These include variations of the baseline matching estimators in which firms were matched: to firms outside their industry and region; at two years before the merger date; and to two control firms for each treated firm based on propensity score. In addition, we use nonparametric matching

[31]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 Table 3 are positive, meaning that all subgroups of workers experience an increase in the departure rate in target establishments, the monotonicity assumption is not violated here.

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 A5). Details of these estimators are in Appendix A.1.

1.5.2 Mergers Target Firms with Soft Managers

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

$$\hat{\lambda}_{mjt} = \gamma_1 Target_{jt} + \beta X_{jt} + \varepsilon_{jt} \tag{1.11}$$

where $\hat{\lambda}$ is manager fixed effects estimated from regression (1.7) and $Target_{jt}$ is a dummy variable indicating whether the establishment is acquired within the next three years, and we control for time-varying establishment characteristics as well as industry-year and region-year fixed effects. The regression is weighted by the standard error of the estimated manager fixed effects.

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

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

$$\hat{\psi}_{jt} = \gamma_1 Target_{jt} + \beta X_{jt} + \varepsilon_{jt} \tag{1.12}$$

where $\hat{\psi}$ is the establishment-year fixed effect from equation (1.6). Column 2 in Table 5 shows that target establishments pay workers 2.3% higher wages on average conditional on productivity, industry, and region. According to equation (1.7), the higher wage premium could be due to a higher establishment-specific component γ_j (e.g., amenities), or higher manager-specific component λ_m, or higher error term ε_{jt}. Both establishment-year fixed effects and manager fixed effects are about 2% higher in target establishments,[32] implying that the majority of the wage premium is due to soft managers. In other words, target establishments pay workers higher wages because they have managers who actively implement a high-wage policy, and therefore the wage premium can be eliminated when the target firms replace their managers.

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

In Appendix Table A2 we look at what types of firms have a higher propensity to be acquired. Column 1 shows that establishments with higher average wages are more likely to be a target. Higher wages may be due to higher wage premiums or due to more high-skilled workers. Column 2 and Column 3 show that a higher establishment wage premium is associated with a higher likelihood of being acquired, while a more skilled workforce (measured by average worker fixed effects) is not. A one-standard-deviation increase in establishment fixed effects increases the likelihood of being a target by approximately 0.1%, which is a 14% increase over the average probability of 0.7%. The establishment wage premium contains manager fixed effects, establishment fixed effects, and productivity. We further show that establishments with higher manager fixed effects are also more likely to be acquired (Column 4), but establishments with higher establishment fixed effects are not (Column 5), and establishments with higher productivity are less likely to be acquired (Column 6). Therefore the positive correlation between wage premiums and propensity to be ac-

[32]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.

quired is driven by manager fixed effects. Column 7 shows that despite the positive effect of wage level, the change in wage levels does not predict acquisitions. This is consistent with the absence of pre-trends in wages before mergers. Although we should be cautious about interpreting these effects as causal, the evidence on propensity to be acquired supports our hypothesis that establishments that have soft managers and pay higher wages to workers are more likely to be targeted by acquirers.

1.5.3 Manager Turnover Around Mergers

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

We examine whether firms are more likely to replace soft managers after mergers by comparing manager turnover based on the estimated manager fixed effects in wages. In Figure 1-9 the two solid lines plot manager turnover rates for target establishments with high or low manager fixed effects; for comparison, the two dashed lines plot manager turnover rates for control establishments with high or low manager fixed effects. By year 5, target establishments with soft managers are 8 percentage points more likely to replace the managers than target establishments with tough managers, accounting for about 40% of the difference in manager turnover rates between target and control establishments. This indicates that managers' style in wage-setting is a major factor in deciding whether they remain in the firm after mergers. By contrast, for control establishments, the difference in turnover rates between soft managers and tough managers is almost negligible. Evidence therefore suggests that mergers and acquisitions are a key corrective mechanism for eliminating soft managers.

1.5.4 Are Wage Cuts Due to Replacing Soft Managers?

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

$$w_{ijt} = \sum_{\tau=-3}^{5} \lambda_\tau D_{ijt}(\tau) + \sum_{\tau=-3}^{5} \eta_\tau D_{ijt}(\tau) \times \text{SoftManager}_j + \sum_{\tau=-3}^{5} \delta_\tau D_{ijt}(\tau) \times MA_j \times \text{SoftManager}_j$$
$$+ \sum_{\tau=-3}^{5} \gamma_\tau D_{ijt}(\tau) \times MA_j \times (1 - \text{SoftManager}_j) + \alpha_{ij} + \beta X_{ijt} + \mu_t + \epsilon_{it} \tag{1.13}$$

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

Figure 1-10(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%.[34]

[33]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.

[34]To account for measurement error in the estimated manager fixed effects, we use a split-sample instrumental vari-

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 1.6) 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 1-10(b) shows that only workers in target establishments with high excess wage premiums experience wage declines after mergers.

To further investigate whether wage decline after mergers is entirely due to replacing soft managers, we plot the wage changes by establishment manager effects and whether the manager is replaced by year 3 in Figure 1-10(c). Only workers in establishments that had soft managers and replaced those managers experience large wage declines. We also show in Figure A12 that while wages decline when a firm with a soft manager is acquired by a firm with a tough manager, wages do not increase when a firm with a tough manager is acquired by a firm with a soft manager. This indicates that acquirers take over firms with soft managers and replace them with tougher managers to extract rents, but wages change very little when target firms already have tough managers. The magnitude of wage changes when replacing a soft manager after acquisition is close to the magnitude of wage changes when a manager whose manager FE is in the top quartile retires (Section 4.2).

Consistent with our theory, Appendix Figure A13 shows that employment tends to increase in target establishments with soft managers. Appendix Figure A15 shows that the target firms with soft managers experience a large increase in job inflows following mergers. This occurs

ables 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 (Figure A20).

because, by replacing soft managers and lowering labor costs, firms expand production and hire more workers.[35]

1.5.5 Gains from Mergers

How much value does rent extraction create in merger transactions? To estimate the impact of mergers on the profitability of the combined firm, we combine the balance sheets of each target firm with its acquirer firm before the merger and track the combined performance over time. We compare the return on assets (ROA) of the combined firm[36] 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 1-11 plots the change in ROA of combined companies over time and shows that merged companies experienced an average increase in ROA of 1 to 1.5 percentage points within five years after the merger.

We then calculate how much of the increase in ROA can be attributed to rent extraction. As shown in Figure 1-10(c), workers experience large wage declines only at target firms that replace soft managers. Suppose that acquirer firms replace the target firm managers with above-average manager effects with managers with average manager effects, and do not change the wage policy in the acquirer firms or target firms with below-median manager effects. From equation (1.1), by the envelope theorem,[37] the impact of a change in manager styles on firm profits is $w_j L_j \Delta(\beta\phi_m)$, where $w_j L_j$ is the wage bill of the target firm, and $\beta\phi_m$ is the identified manager fixed effects. Therefore the impact of replacing soft managers on ROA is:

$$\Delta = p(\beta\phi_{target} - \beta\overline{\phi})^{+} \frac{(wL)_{target}}{A_{acquirer} + A_{target}} \tag{1.14}$$

[35] Appendix Figure A15 shows that the average worker quality of the newly hired workers increases at target establishments with soft managers. Although we do not model heterogeneous worker types in our theoretical framework, this suggests that reducing wage premiums due to soft managers may also allow firms to hire higher-quality workers.

[36] 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.

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

where p is the probability of replacing a soft manager (which equals 0.56, according to Figure 1-9), the second part is the positive part of the difference between the target's manager fixed effect and the average manager fixed effect, and the last part is the wage bill of the target divided by the total assets of the combined firm. To adjust for the estimation error in manager fixed effects, we use a simple empirical shrinkage procedure from the empirical Bayes literature and shrink the estimates toward the mean.[38] The relative weight that the estimate gets in the convex combination varies inversely with the noise of the estimate (which is based on the standard error of the manager fixed effect).

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

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

[38]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.

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

[40]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.

explains a large part of the increase in shareholder value.[41]

1.5.6 Industry Concentration and Rent-Extracting Mergers

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

To test whether target firms have softer managers in more concentrated industries, we estimate the following extended version of equation (1.11):

$$\hat{\lambda}_{mjt} = \gamma_1 Target_{jt} + \gamma_2 Target_{jt} \times HighConcentration_j + \beta X_{jt} + \varepsilon_{jt} \tag{1.15}$$

where $HighConcentration_j$ is a dummy variable that equals one if firm j is in a industry with HHIs over 1000.[42] Column 3 of Table 5 shows that manager fixed effects are significantly larger only in concentrated industries, which is consistent with our theoretical prediction.

Figure 1-12 shows large wage declines following mergers in concentrated industries, and no significant wage changes in competitive industries. In concentrated industries, the negative wage

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

[42] 1000 is close to the median HHI for all 127 industries. For example, the Horizontal Merger Guidelines classify markets as unconcentrated (HHI less than 1,500); moderately concentrated (HHI between 1,500 and 2,500); and highly concentrated (HHI above 2,500).

effects from rent extraction dominate the positive wage effects from real productivity increases and market power, whereas the wage effects of all channels are balanced for mergers in competitive industries. This can be also seen from the effects on employment in Figure 1-12, where mergers lead to slightly more positive employment changes in more concentrated industries due to rent extraction.[43]

1.6 Robustness

1.6.1 Monopsony Power and Labor Market Concentration

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

To further test whether increases in monopsony power explain the wage declines, we take several approaches to construct measures of monopsony power created by mergers. The first approach is to measure changes in labor market concentration due to mergers. We first use municipalities to approximate local labor markets.[44] For nearly half of the mergers in our sample, the acquirer firm is not in the same municipality as the target. The top left figure of Figure A22 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

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

[44] 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.

threshold for scrutinizing mergers (FTC/DOC, 2010). The top right figure of Figure A22 shows that workers above the threshold and workers below the threshold experience almost identical wage declines.

The second approach is to measure the diversion ratio, which is the fraction of target firm employees that would move to acquirer firms when the target firm lowers wages (Naidu et al. 2018). We measure it by the fraction of job movers from target firms who moved to the acquirer firm before the merger. A higher ratio indicates that the acquirer is a more important competitor in the labor market. Only about a quarter of the target firms have positive diversion ratios, and the average diversion ratio is less than 5%, indicating that there is little competition between acquirer and target in the labor market. The bottom figure of Figure A22 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.

1.6.2 Are Mergers Efficiency-Enhancing?

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

According to our model, target firms with less productive managers will experience wage increases after mergers. Appendix Figure A18 shows that target firms with less productive managers seem to have more positive wage changes following mergers. Since targets on average have less productive managers than acquirers, replacing unproductive managers (independent of replacing soft managers) will lead to wage increases after mergers, and this positive wage effect is dominated by wage cuts due to rent extraction.

1.6.3 Monopoly Power

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

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

1.6.4 Discussion of Alternative Interpretations of Manager Styles

Automation and outsourcing

Since automation has large fixed costs, mergers might create economies of scale and induce more automation (Olsson and Tåg 2016; Ma et al. 2017). Similarly, larger firms are more likely to outsource their non-production activities. Goldschmidt and Schmieder (2017) show that firms outsource their FCSL (food, cleaning, security and logistics) workers to reduce their wage premiums. An alternative explanation for our finding is that tough managers have a greater propensity or capability to automate or outsource, and they use the threat of automating or outsourcing to bargain for lower wages. Increased automation and outsourcing may also reduce the labor demand for routine or FCSL workers and therefore reduce their wages. In Columns 5 to 8 of Table 6, we compare the effects of mergers on wages of routine and FCSL workers versus on non-routine and non-FCSL workers. We find that non-routine and non-FCSL workers experience larger wage cuts, which does not support the explanation that the threat of automation or outsourcing depresses wages after mergers.

Manager entrenchment and ownership

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

In Appendix Figure A19 we compare the wage effects of mergers for family firms and nonfamily firms. About 30% of the target firms in our sample are family firms. Following Bennedsen et al. (2007), we classify a firm as a family firm if managers in different years are family members. We find slightly larger wage cuts for family firms following mergers, suggesting that managers may set generous wages even when they are the owners of the firm and there is no agency conflict.

Nonwage benefits

Another interpretation of manager "softness" is compensating differentials for heterogeneous amenities across managers. For example, soft managers may pay higher wages because they provide workers with worse amenities and nonwage benefits (Sorkin 2018). Although we do not directly observe amenities, and therefore cannot identify manager styles concerning amenities separately from manager styles in setting wages, we do find that an important part of nonwage benefits—pension payments—decline by nearly 5% after mergers (Appendix Figure A24), which is consistent with Pontiff, Shleifer, and Weisbach (1990). Anecdotal evidence also suggests that amenities often worsen after mergers.[45]

1.7 Conclusion

Using a matched employer-employee data set from Denmark and analyzing the universe of M&A transactions from 1995 to 2011, we identify soft managers—managers with a tendency to set higher wages—and find that M&As target and replace these soft managers. Rent extraction from target firms with soft managers brings higher profits to the acquirer firms, explaining the majority of the rise in profitability of the merged firm. These findings suggest that rent extraction is a major driver of the market for corporate control and a key source of merger synergies.

Our paper contributes to the growing literature of how managerial biases and misoptimization affect firms' real outcomes and the aggregate product and labor markets (DellaVigna and Gentzkow 2018; Dube, Manning, and Naidu 2017; Ma, Sraer, and Thesmar 2018). Our results indicate that with increasing market power (De Loecker and 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'

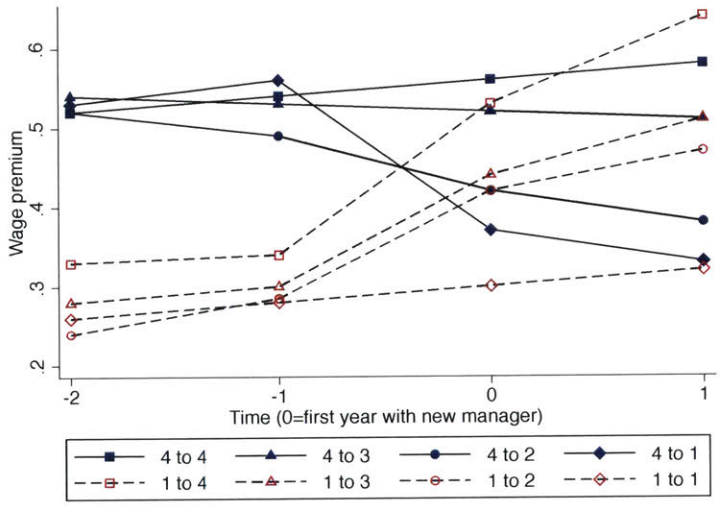
[45]For example, 3G Capital is famous for cutting even seemingly tiny employee benefits at the companies it acquires: free beer at AB InBev-owned Budweiser after its merger with SABMiller, free Timbits at Tim Hortons annual general meetings after its merger with Burger King, and free cheese sticks for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

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.

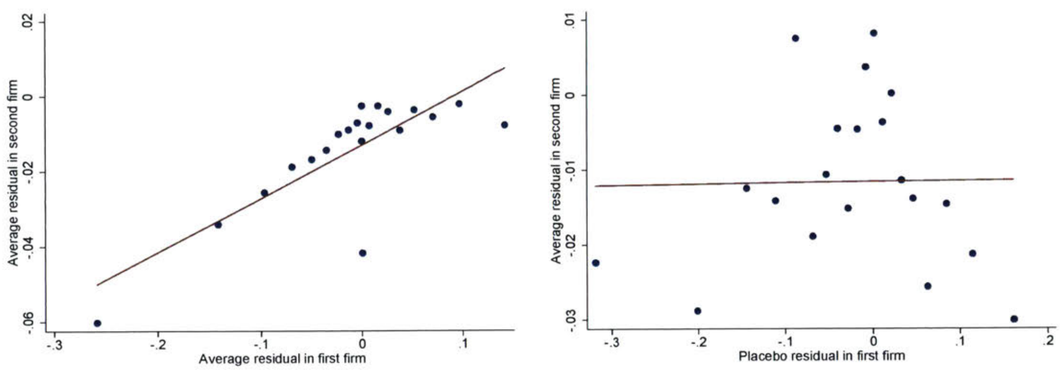
1.8 Figures

Figure 1-1: 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 $\hat{\lambda}_m$.

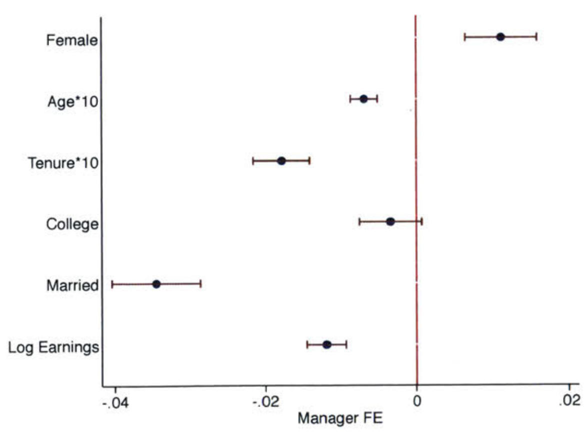
Figure 1-2: Correlation of managers' wage residuals across establishments



(a) Data (b) Placebo

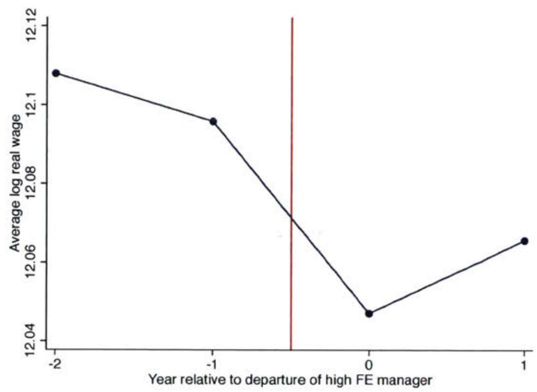
Notes: This figure shows the binscatter of wage residual in the manager's first employer against the wage residual in manager's second employer. This is similar to the test in Table V of Bertrand and Schoar (2003). The wage residual is from regressing establishment-year fixed effects on year dummies 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 0.1436 with t-statistic of 13.2; the regression coefficient in the right figure is 0.0001 with t-statistic of 0.01.

Figure 1-3: Characteristics of soft managers



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

Figure 1-4: Event study of exogenous manager departures

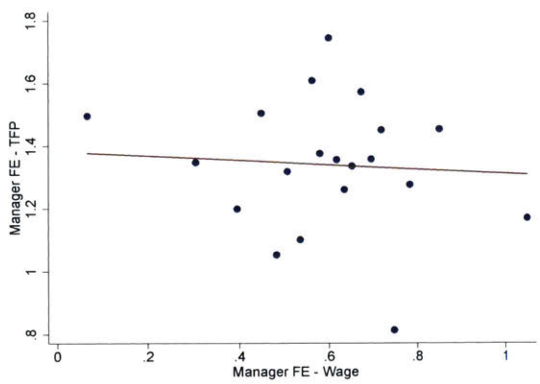


(a) High FE manager departure

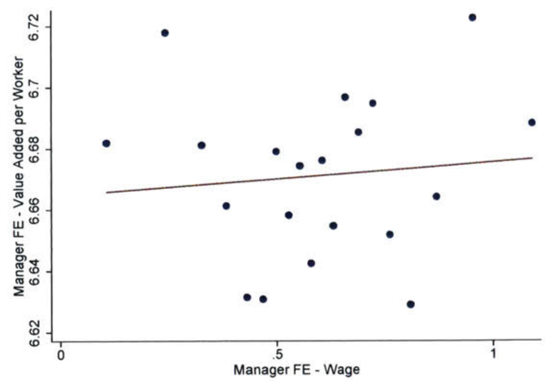
(b) Low FE manager departure

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.

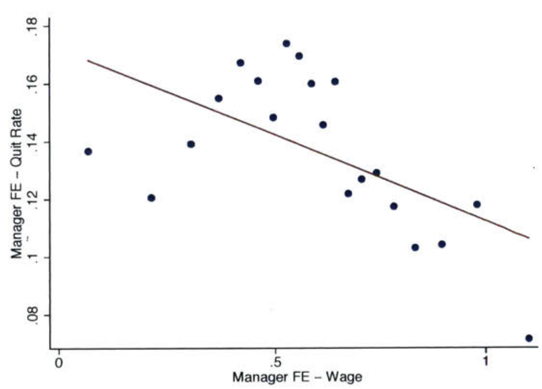
Figure 1-5: Correlation between manager fixed effects



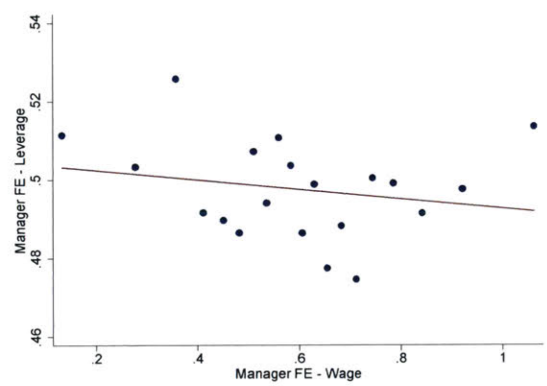
(a) TFP



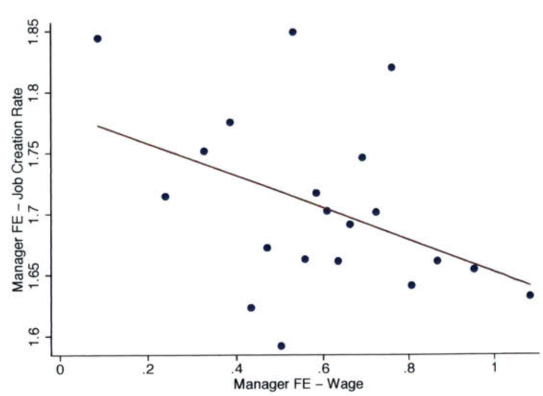
(b) Value-added per worker



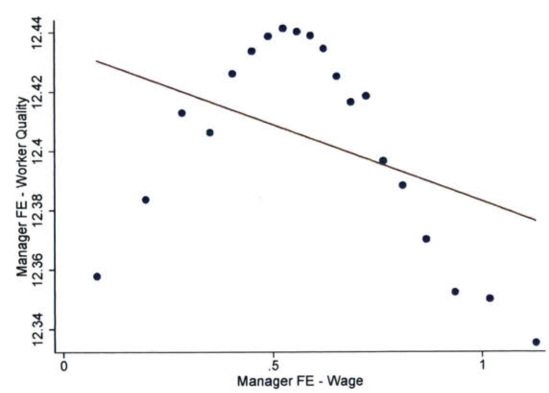
(c) Firing rate



(d) Leverage



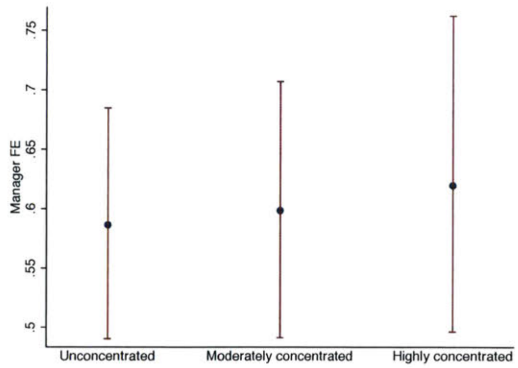
(e) Hiring rate



(f) Worker quality

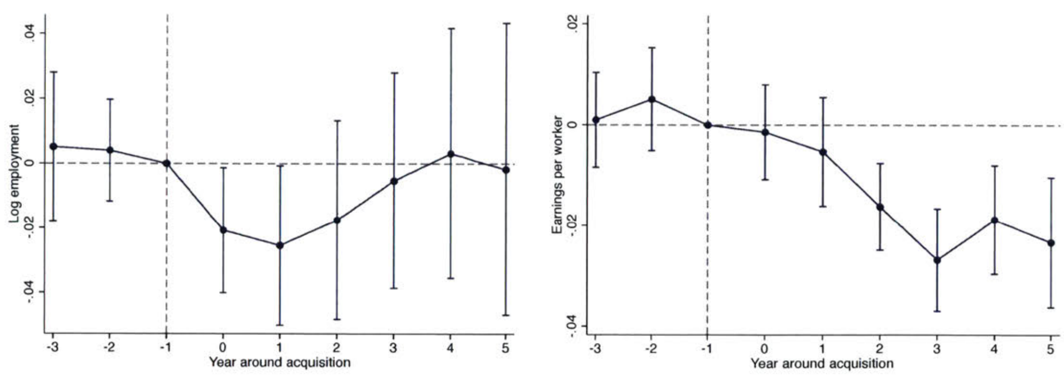
Notes: The graph shows the binscatter plots of manager fixed effects for various outcomes against manager fixed effects for wages. Each dot contains the same number of observations. In (a), on the y axis is manager FE in terms of TFP, where TFP is the residual from regressing log value-added on inputs,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

Figure 1-6: Industry Concentration and Manager Fixed Effects



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

Figure 1-7: Target establishments' employment and wages following M&As

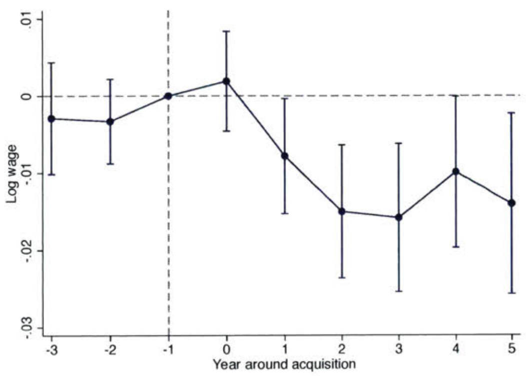


(a) Employment

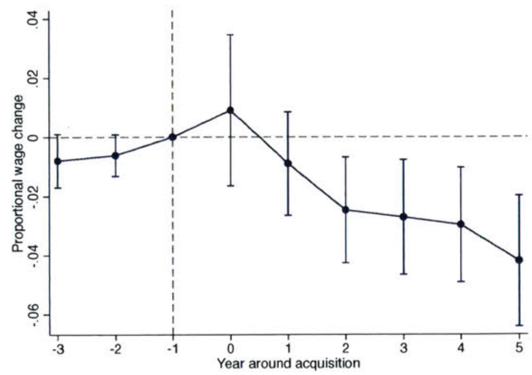
(b) Earnings per worker

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 (9). The coefficient in $\tau = -1$ is normalized to zero. Regressions are weighted by average establishment employment between $\tau = -3$ and $\tau = -1$. The outcome variable in panel (a) is log employment. The outcome variable in panel (b) is proportional change in annual earnings relative to the initial annual earnings before merger $(w/w_0 - 1)$. The vertical lines denote 95% confidence intervals based on standard errors clustered at the establishment level.

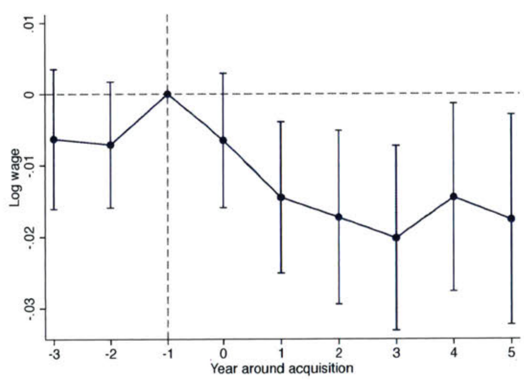
Figure 1-8: Changes in staying workers' wages after M&As



(a) Log annual wage



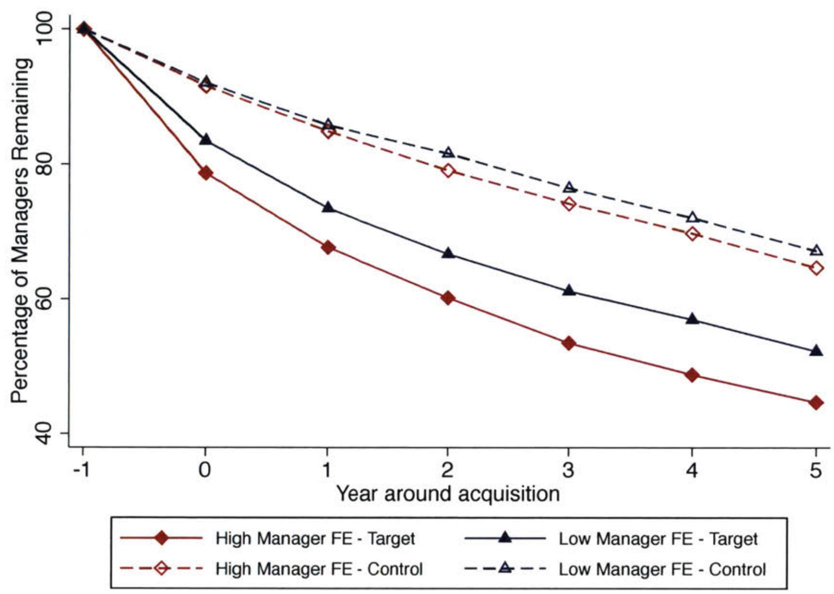
(b) Proportional wage change



(c) Log hourly wage

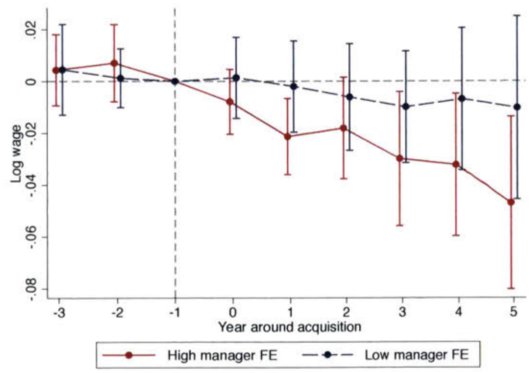
Notes: The figure shows regression coefficients and associated confidence intervals for the difference between staying workers at target and control establishments, i.e., the δ_τ from the difference-in-differences model in (10). The coefficient in $\tau = -1$ is normalized to zero. All regressions in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

Figure 1-9: Manager turnover around mergers

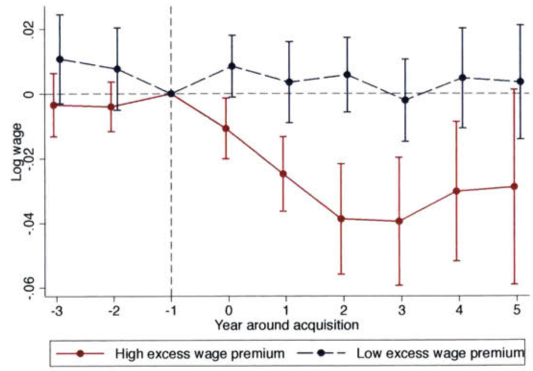


Notes: This figure plots the percentage of managers who are at treatment or control establishments in the year before acquisition that remain in the same establishment for each year after the acquisition. By definition, in year -1, 100% of managers remain in their initial establishment. Managers are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

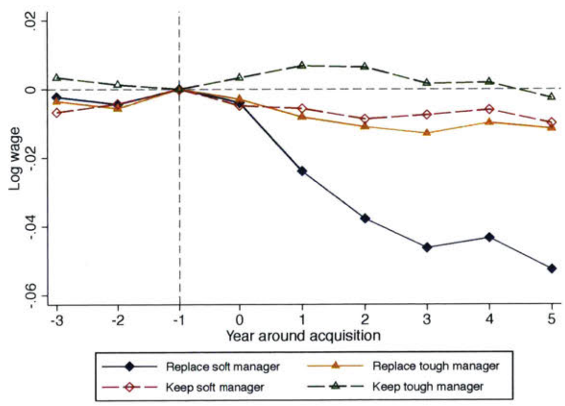
Figure 1-10: Heterogeneity of wage effects by pre-merger wage premium and manager FE



(a) By pre-merger manager FE



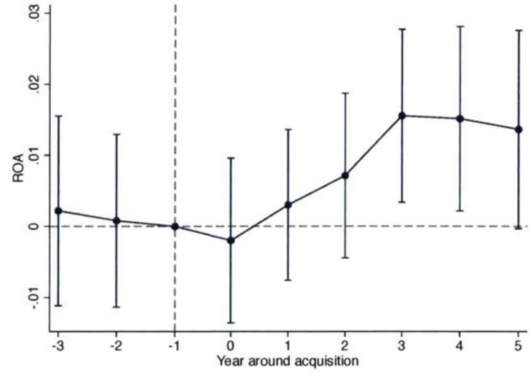
(b) By pre-merger excess wage premium



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

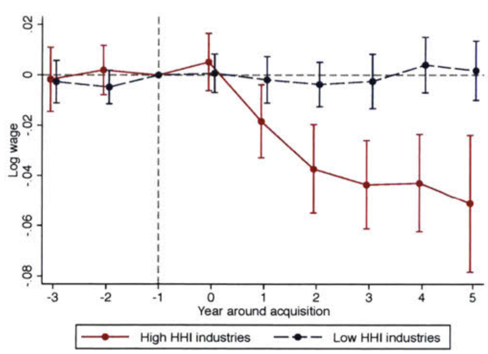
Notes: The figure shows regression coefficients and associated 95% confidence intervals for the difference between staying workers at target and corresponding control establishments separately by target establishments' pre-merger wage residual or manager softness. In top two figures the navy (red) line plots δ_τ (γ_τ) in regression (13). In figure (a), the treatment establishments are re-matched to control establishments such that they are in the same quartile of wage residual (manager FE). In figure (b) we define high manager FE as establishments with above-median manager FE (in wage setting) in the year before merger. In the right figure, high wage establishments are establishments with above-median wage residual in the year before merger, where the residual is from regressing establishment-year fixed effects ($\hat{\psi}_{jt}$ 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 1-11: Effects of M&As on ROA of Merging Firms Over Time

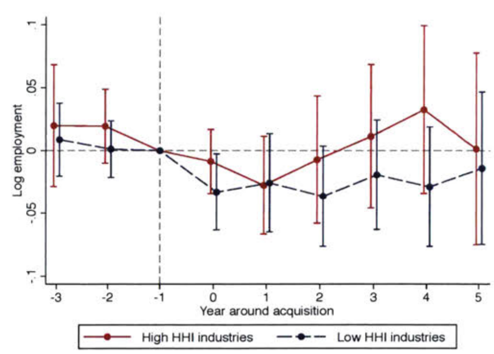


Notes: The figure plots the regression coefficients and associated confidence intervals for the treatment effect of M&As on the return on assets (ROA) of the combined firm. ROA is equal to before-tax profits divided by total assets at the firm level (average ROA is 19 percentage points). For years before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

Figure 1-12: Heterogeneity by industry concentration



(a) Wage



(b) Employment

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

1.9 Tables

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

Table 3 Effects of Mergers on Worker Departure

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

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

Table 4 Effects of Mergers on Wages

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

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

Table 5 Manager Style in Target and Acquiring Firms

	(1)	(2)	(3)	(4)	(5)
Dependent Variable	Manager FE in wage	Establishment year FE	Manager FE in wage	Manager FE in TFP	Manager FE in value added per worker
			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 industry			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 industry			-0.013		
			(0.008)		
Control for value added	X	X	X		
Industry*Year FE	X	X	X	X	X
Region*Year FE	X	X	X	X	X
No. of establishments	53,748	324,390	53,748	53,748	53,748

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

Table 6 Alternative Mechanisms

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

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1.10 Appendix

1.10.1 Alternative Matching Strategies

Non-parametric matching

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

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

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

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

Synthetic control

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

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

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

where $\hat{y_{i\tau}}$ is the outcome of the synthetic control unit. $1/\hat{\sigma_i}$ measures the goodness of fit for each match, so that better matches are given more weight in the estimation. To construct the confidence intervals, we randomly draw 5,000 placebo treatment groups from the control group – with each group having the same size as the real treatment group. We compute the wage effect of M&As for these placebo treatment groups, and construct the confidence intervals for hypothesis testing of whether the coefficient is significantly different from zero. The effect is significant at 5% if it does not belong to the interval that contains the [2.5, 97.5] percentiles of the effect for placebo treatment groups. Result is shown in Figure A5.

1.10.2 Job Inflow and Outflow

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

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

where λ_τ captures the difference between treated and control establishment in terms of job inflow and outflow rates. We control for industry-year fixed effects to absorb industry-specific trends. Figure A6 shows that both job inflow and job outflow increase following mergers.

How does cutting wages affect job flows? To answer this question, we look at the effects of mergers on job inflow and outflow separately for high-wage and low-wage establishments. In Section 5.3 we have established that all of the wage cuts are concentrated in high-wage establishments. Figure A15 shows that both high-wage and low-wage establishments experience a rise in job outflow rates after mergers, while only high-wage establishments experience a large rise in job inflow rates after mergers. The average quality of joining workers in high-wage establishments increases, while the average average of joining workers in low-wage establishments does not change after mergers. This indicates that mergers lead to wage cuts in high-wage establishments, but also lead to more hiring of high-skilled workers in high-wage establishments. By reducing the wage premium in establishments with soft managers, mergers allow the target firms to hire more high-wage workers.

1.10.3 Wage Changes of All Initially Employed Workers

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

$$y_{it} = \alpha_i + \gamma_t + \sum_{\tau=-3}^{5} \lambda_\tau D_{it}(\tau) + \sum_{\tau=-3}^{5} \delta_\tau D_{it}(\tau) \times Target_i + \beta X_{it} + \epsilon_{it} \tag{1.16}$$

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

Figure A7 shows that mergers reduce wages for workers initially employed in target firms at time of merger. The wage declines are slightly larger than those of workers staying in target firms due to the additional negative effects of job displacement (Jacobson, LaLonde and Sullivan 1993). However, although the effect on unemployment peaks at one year after merger, the negative wage effects are persistent and seemingly irreversible, which is consistent with the loss of firm-specific wage premiums.

1.10.4 Data Appendix

Identify Managers

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

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

Comparing with External Datasets of M&As

Figure A21 compares the number of mergers in our administrative dataset with the number of mergers in Denmark in Zephyr and SDC datasets. In all datasets number of mergers is increasing before the financial crisis and declines afterwards. The administrative dataset has about 30-40%

more mergers in years before the financial crisis.

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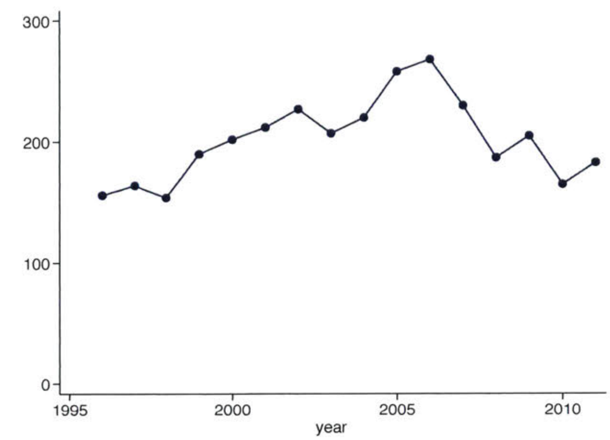
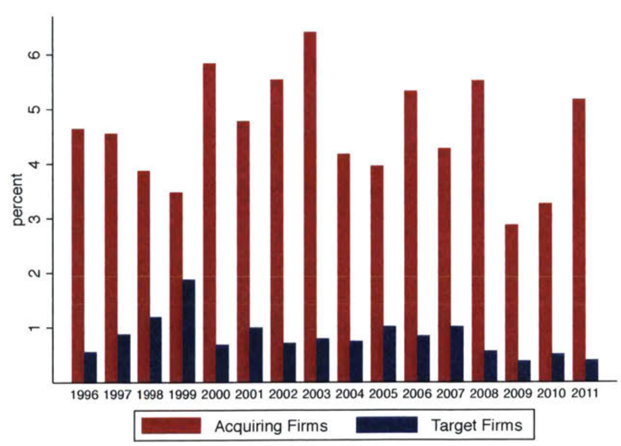
1.11 Appendix Figures and Tables

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



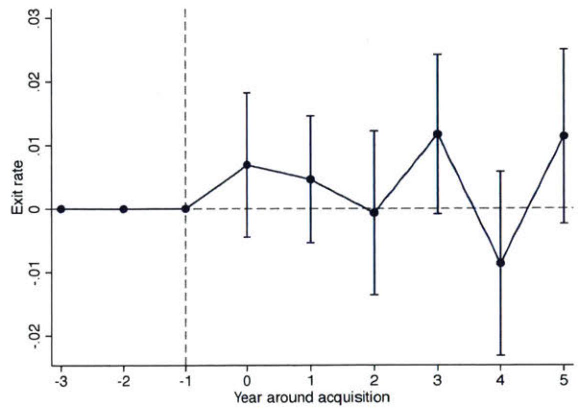
Notes: This figure shows the number of merger transactions in Denmark by year. Mergers are identified by firm and establishment identifiers (see Section 3.3 for details). Transactions in which one of the parties is a foreign company are not included. Mergers in the public sector are also excluded. For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

Figure A2: Percentage of employment in target or acquirer firms



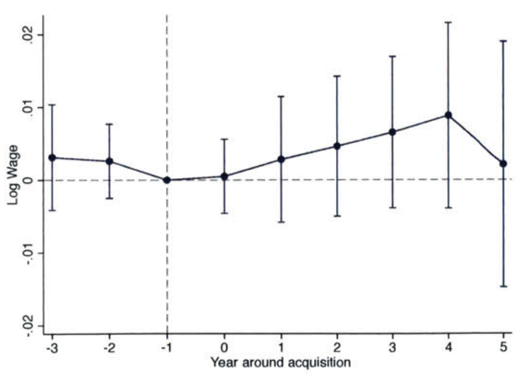
Notes: This figure shows the share of all employed workers in Denmark that works in acquired or acquiring firms in each year. We only include workers who are full-time employees and are between 25 and 60 years old.

Figure A3: Effects of merger on establishment exit

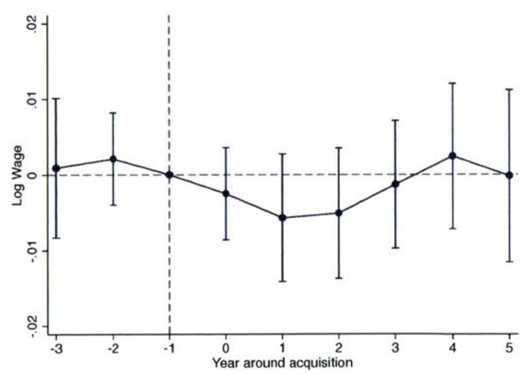


Notes: The figure shows regression coefficients and associated confidence intervals for the difference between treatment and comparison group in a given year τ relative to the year of acquisition in the treatment group establishments, i.e., the δ_τ from the difference-in-differences model in (9). The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

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



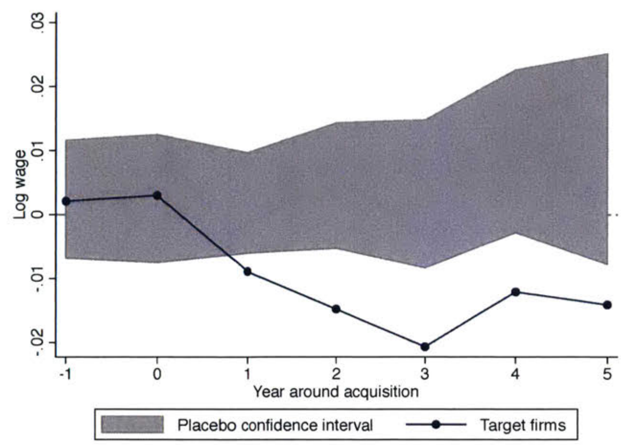
(a) Staying workers



(b) All initially employed workers

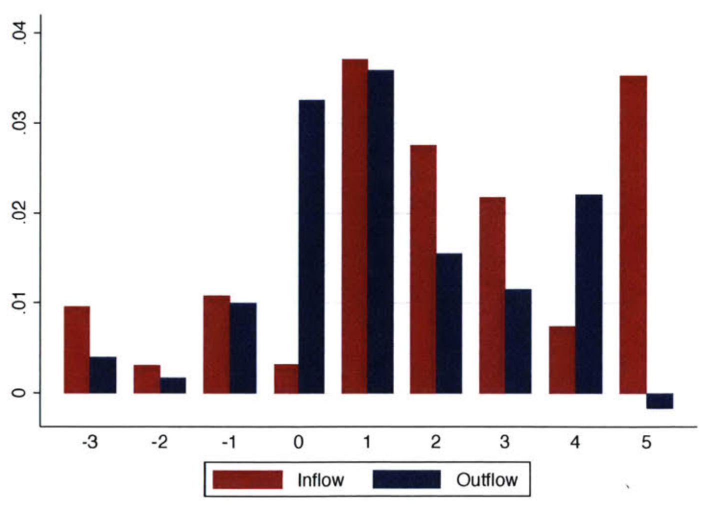
Notes: This figure shows the effect of mergers on workers' annual wages in acquiring firms. The left figure shows the effects on wages of all workers staying in acquiring establishments, and right figure shows the effects on wages of all workers employed in acquiring firms in the year before the merger.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

Figure A5: Synthetic control



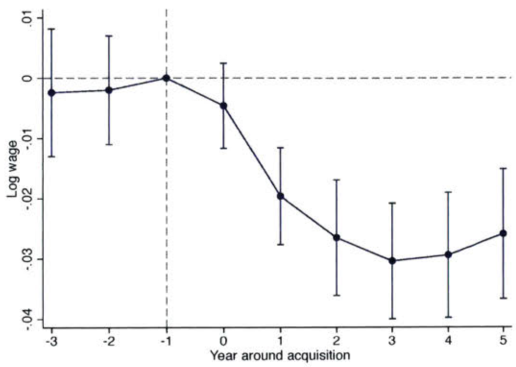
Notes: This figure shows the estimate of the effects of M&As on target establishments' earnings per worker using synthetic control. The shaded area is the [2.5, 97.5] confidence interval constructed using placebo treatment groups. See Appendix A.1.2 for details.

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

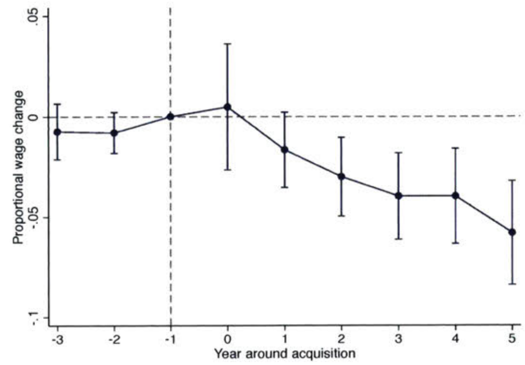


Notes: This figure shows differences in the inflow and outflow of workers between target establishments and control establishments. The regression controls for industry-year fixed effects. Inflow (outflow) at year τ is calculated as the number of entrants (leavers) between year $\tau - 1$ and year τ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

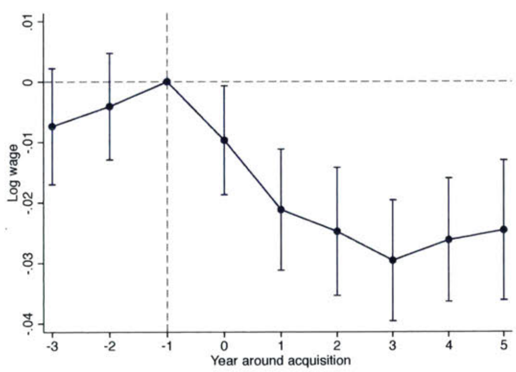
Figure A7: Changes in all initially employed workers' wages after M&As



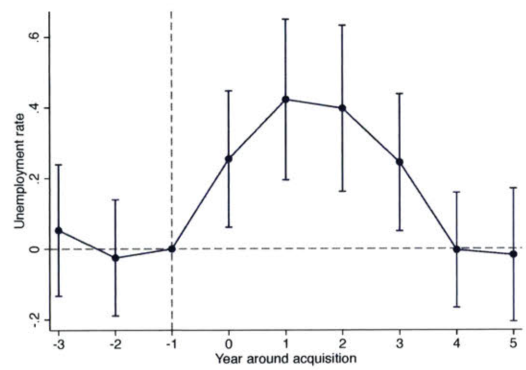
(a) Log annual wage



(b) Proportional wage change



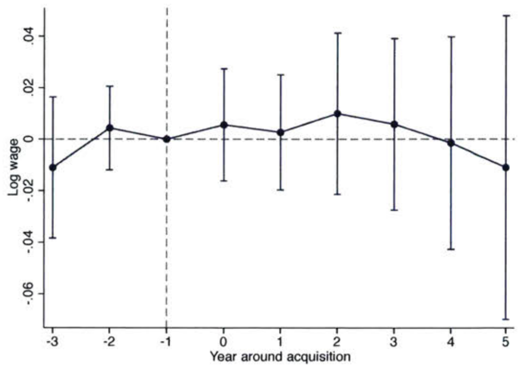
(c) Log hourly wage



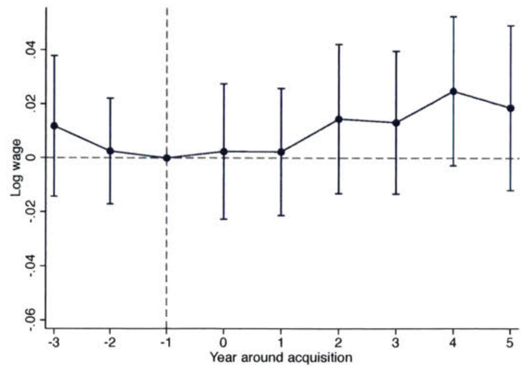
(d) Unemployment

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

Figure A8: Wage effects of failed mergers



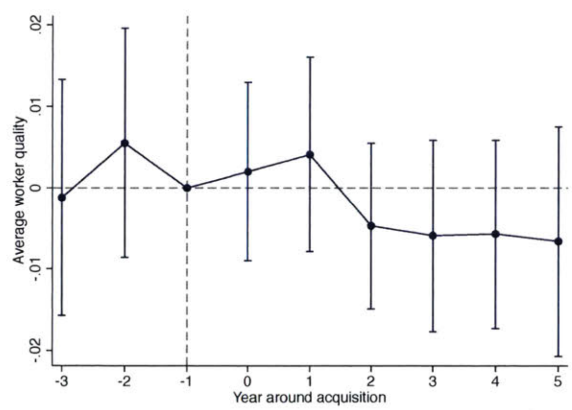
(a) Staying workers



(b) All initially employed workers

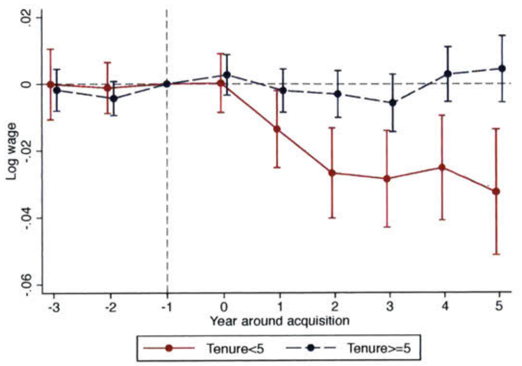
Notes: The figure shows regression coefficients and 95% confidence intervals for the difference between wages at failed target and control establishments. We match 365 targets of failed mergers from SDC Platinum to the administrative register data. We then match each failed target establishment to a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

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

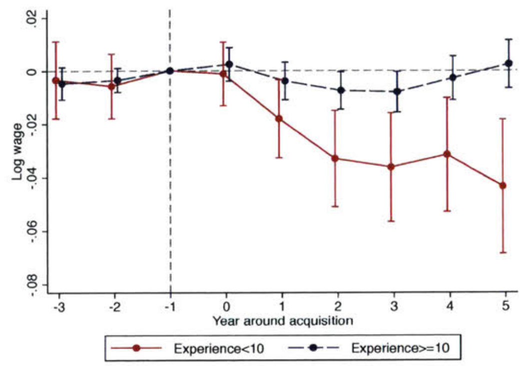


Notes: The figure shows regression coefficients and associated confidence intervals for the difference between treatment and comparison group in a given year τ relative to the year of acquisition in the treatment group establishments, i.e., the δ_τ from the difference-in-differences model in (9). The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

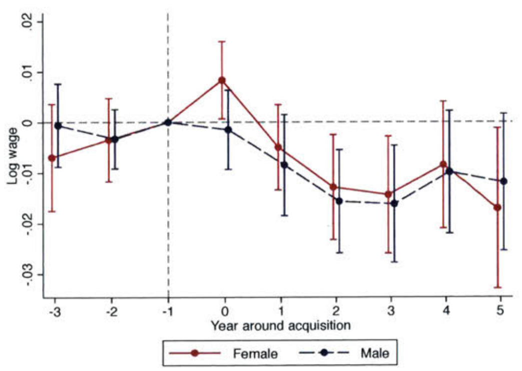
Figure A10: Heterogeneity by worker covariates



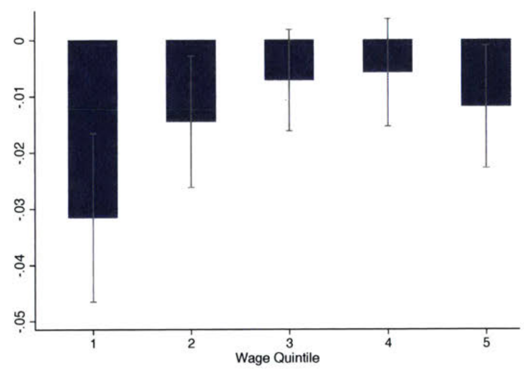
(a) By tenure



(b) By experience



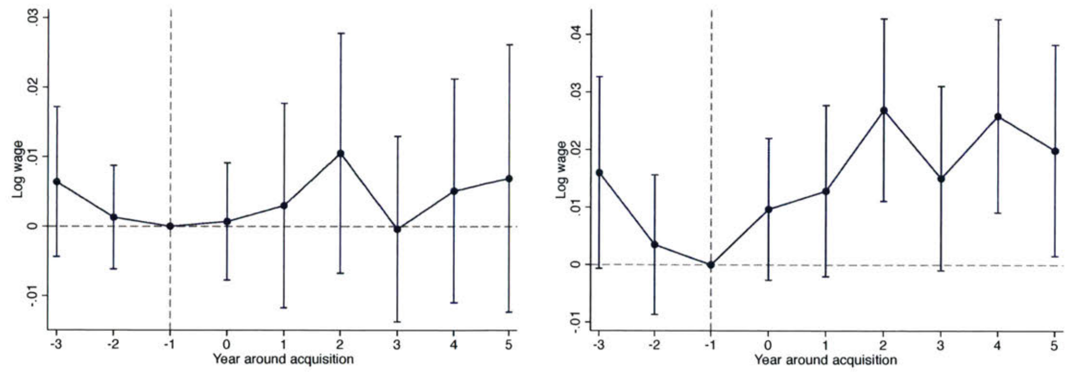
(c) By gender



(d) By pre-merger wage quintile

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

Figure A11: Wage effects of public sector mergers

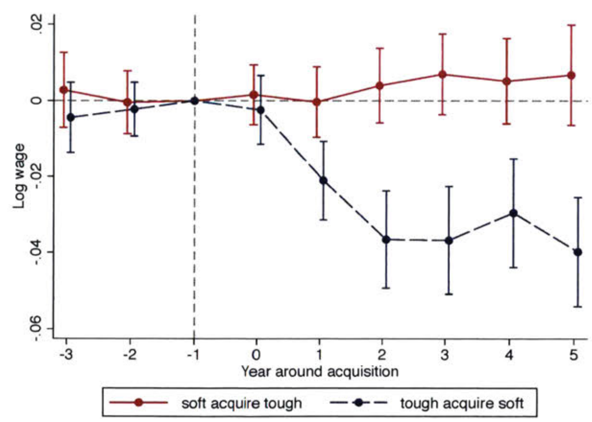


(a) Staying workers

(b) All initially employed workers

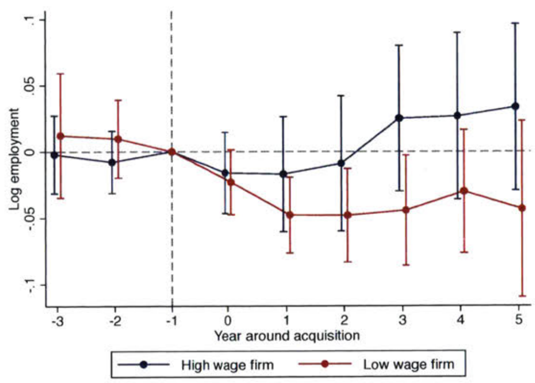
Notes: The figure shows regression coefficients and 95% confidence intervals for the difference between wages at target and control establishments in the public sector. Public sector industries are defined as industries comprising of firms owned by the government, including education, public administration,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

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



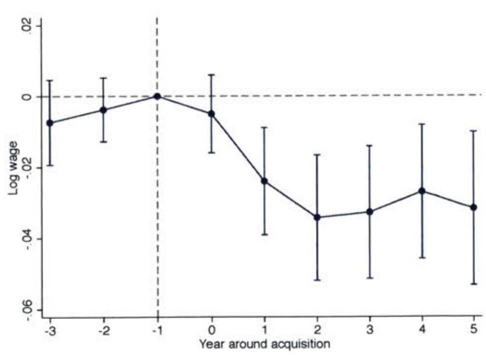
Notes: The figure shows regression coefficients and associated 95% confidence intervals for the difference between staying workers at target and corresponding control establishments. The treatment establishments are re-matched to control establishments such that they are in the same quartile of manager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

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

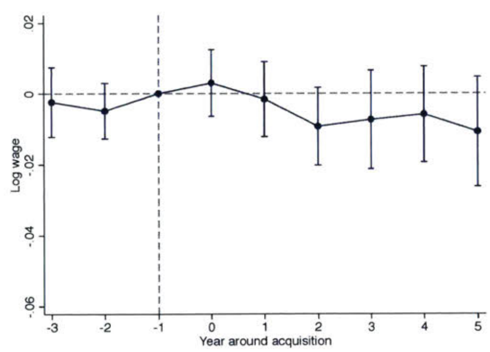


Notes: The figure shows regression coefficients and associated 95% confidence intervals for the difference between log employment at target and corresponding control establishments separately by target establishments' pre-merger wage residual. Inflow (outflow) at year τ is calculated as the number of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

Figure A14: Heterogeneity by manager turnover



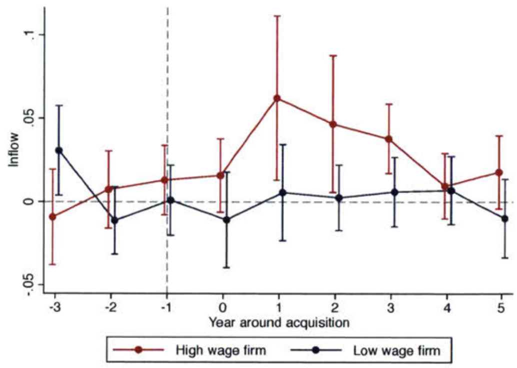
(a) Manager leaves



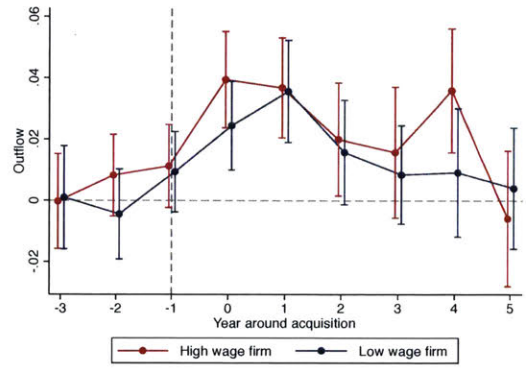
(b) Manager stays

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

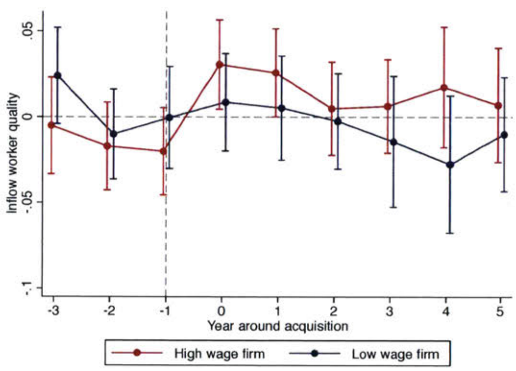
Figure A15: Inflow and outflow at high wage and low wage target establishments



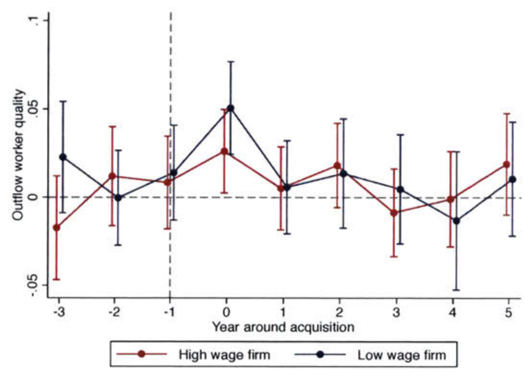
(a) Inflow



(b) Outflow



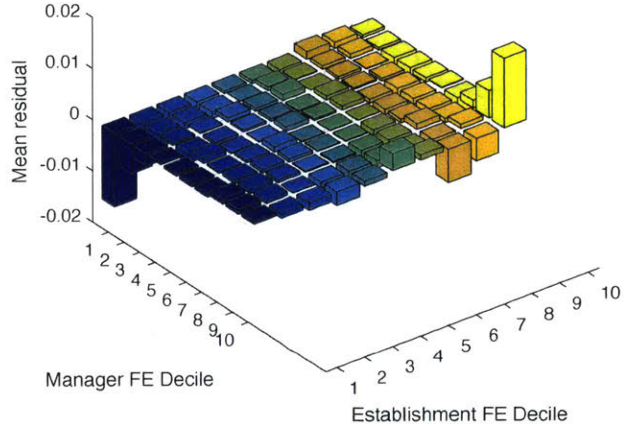
(c) Inflow Worker Quality



(d) Outflow Worker Quality

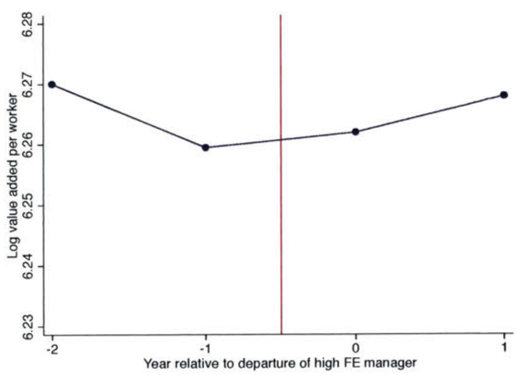
Notes: The figure shows regression coefficients and associated 95% confidence intervals for the difference between job inflows and outflows at target and corresponding control establishments separately by target establishments' pre-merger wage residual. Inflow (outflow) at year τ is calculated as the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

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

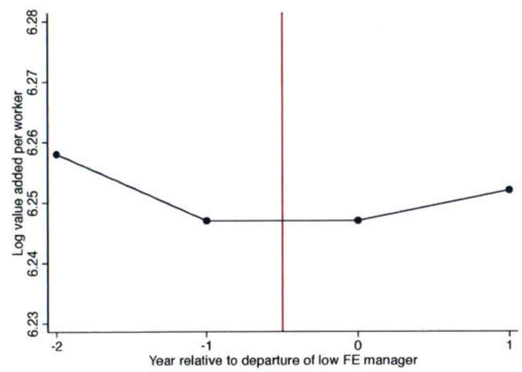


Notes: Figure shows mean residuals from estimating manager FE (equation 7) with cells defined by decile of estimated establishment effect, interacted with decile of estimated manager effect.

Figure A17: Event study of exogenous manager departures on productivity



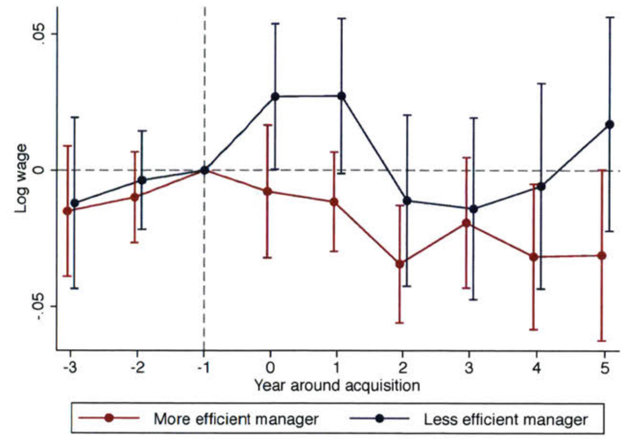
(a) High FE manager departure



(b) Low FE manager departure

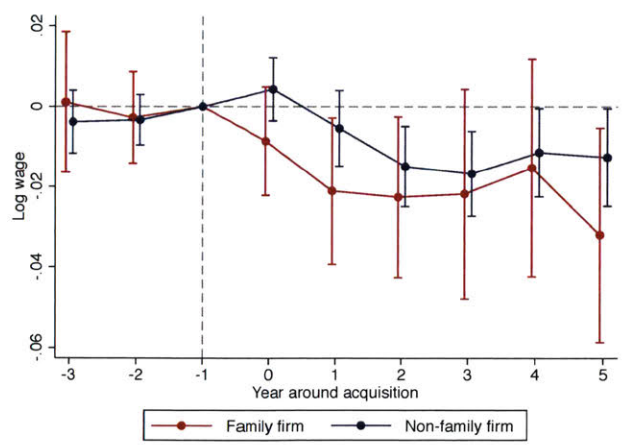
Notes: This figure shows the changed in log value-added per worker around the departure of managers that are at least 62 years old. Year 0 is the year when the manager leaves, and we only include managers that had stayed in the same firm for at least three years before they retire and had never been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

Figure A18: Manager productivity and effects of mergers on wages



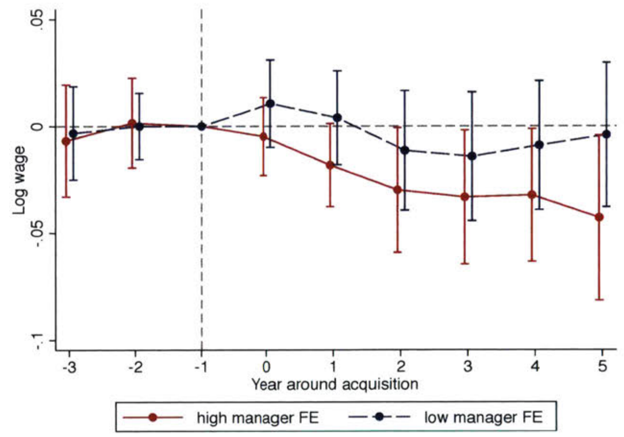
Notes: The figure shows regression coefficients and associated 95% confidence intervals for the difference between staying workers at target and corresponding control establishments separately by target establishments' pre-merger manager productivity. Manager productivity is estimated using equation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

Figure A19: Family firms and effects of mergers on wages



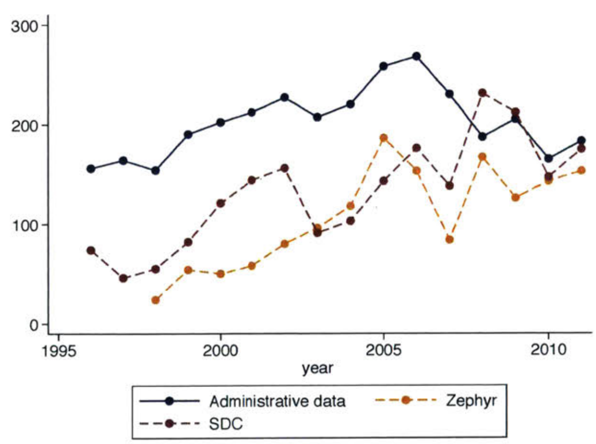
Notes: The figure shows regression coefficients and associated 95% confidence intervals for the difference between staying workers at target and corresponding control establishments for family firms and non-family firms respectively. We define family firms following Bennedsen et al. (2007) and a firm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

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



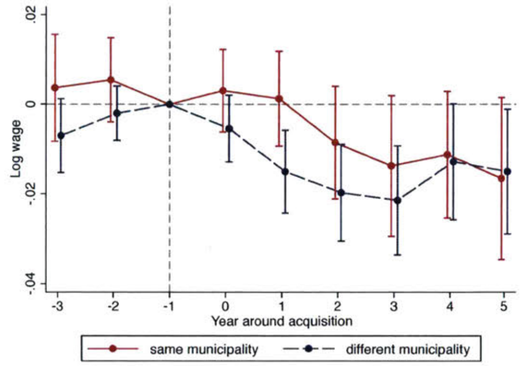
Notes: The figure shows regression coefficients and associated 95% confidence intervals for the difference between staying workers at target and control establishments separately by target establishments' manager FE. The sample is divided evenly by odd and even years and manager FE is estimated for each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

Figure A21: Compare Mergers in Administrative Datasets and External Datasets

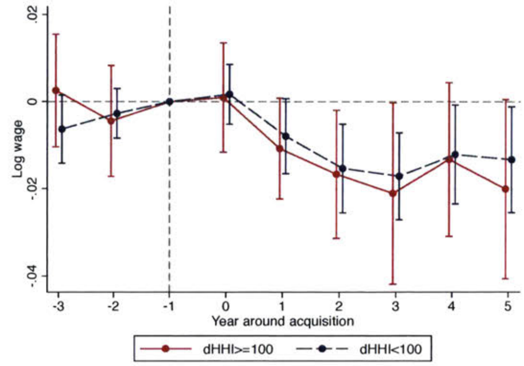


Notes: This figure shows the number of merger transactions in Denmark by year from 1996 to 2011. The solid line is the number of merger transactions in our data, the red dashed line is the number of transactions from the SDC Platinum data, and the orange dashed line is the number of transactions from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

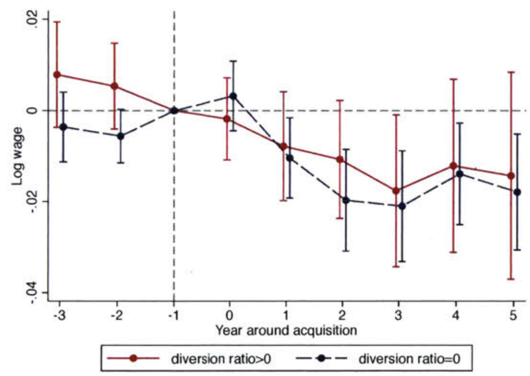
Figure A22: Testing Monopsony



(a) Local labor market by municipality



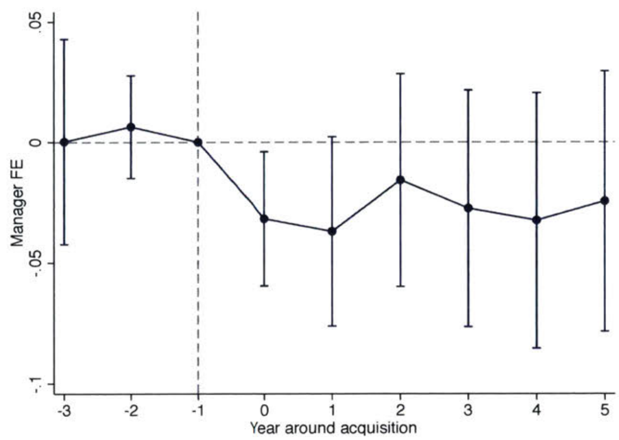
(b) Labor market by occupation and region



(c) Diversion ratio

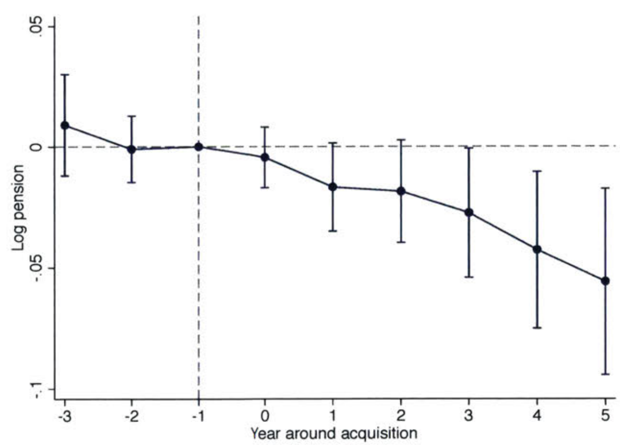
Notes: This figure tests whether negative wage effects of mergers are due to increased monopsony power in the labor market. Each figure plots the regression coefficients and associated 95% confidence intervals for the difference between staying workers at target and corresponding control establishments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

Figure A23: Effects of mergers on manager fixed effects



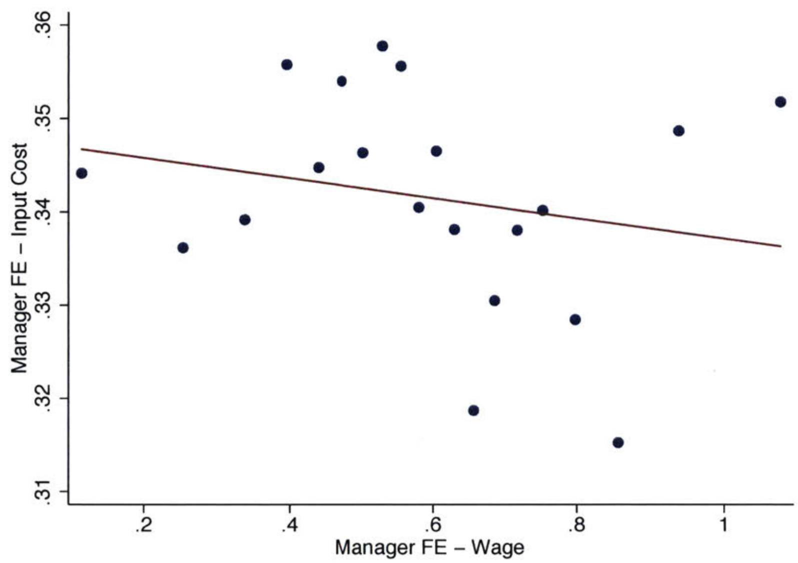
Notes: The figure shows regression coefficients and associated 95% confidence intervals for the difference between manager fixed effects at target and corresponding control establishments. The manager fixed effects measure managers' generosity in wage setting and the estimation is detailed in Section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

Figure A24: Effects of mergers on pensions of target employees



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

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



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

Table A1 Characteristics of Treated and Control Establishments

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

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

Table A2 Determinants of Being Target Firm

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

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

Table A3 Measuring Impact of Rent Extraction on Profitability

Dependent variable	Mean	Standard deviation	Q1	Median	Q3
	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 5.5). Wage bill and total assets are calculated in the year before merger. Manager fixed effects are estimated in Section 4.1. The cumulative abnormal return is calculated over an 11-day event window around the merger announcement. The data on stock prices of the merging firms are from SDC Platinum. The portfolio CAR refers to the cumulative abnormal return to a value-weighted portfolio of the target and acquirer. The medians and quantiles are calculated as the average value of 5 observations around the median/quantile.

Table A4 Wage Effects on Stayers: Alternative Matching Strategies

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

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

Chapter 2

How Does Liquidity Constraint Affect Employment and Wages? Evidence from Danish Mortgage Reform

2.1 Introduction

A large fraction of households are severely liquidity constrained. In the United States, for example, approximately a quarter of households are unable to come up with $2,000 to cope with an unexpected need (Lusardi, Schneiderm, and Tufano 2011).[1] This makes them very fragile to unexpected income shocks. The view that liquidity constraints are particularly severe during a recession has important implications for the design of stabilization policies (Eberly and Krishnamurthy 2014).[2] While the impact of liquidity constraints on consumption is well known (Gross and Souleles 2002;

[1] An additional 19 percent of households could only come up with $2,000 by pawning or selling possessions or taking out a payday loan (Lusardi, Schneider, and Tufano 2011).

[2] For example, there are debates around whether policies that replenish the liquid balances of households, such as reductions in mortgage payments that are concentrated in the periods of the crisis, would be more effective than debt write-downs that reduce mortgage payments over the entire duration of the mortgage contract (Ganong and Noel 2017; Dobbie and Song 2018). It is also argued that policies that prevent households from refinancing their debt during times of economic distress can significantly inhibit efforts aimed at curtailing the costs of recessions (DeFusco and Mondragon 2018).

Agarwal, Liu and Souleles 2007; Leth-Petersen 2010), much less is known about how liquidity constraints affect labor supply and the types of jobs that individuals are willing to take. Recent works show that liquidity constraints can affect individuals' job search behavior (Herkenhoff, Phillips and Cohen-Cole 2016a; Kaplan 2012; Ji 2018) and mobility across occupations and locations (Hawkins and Mustre-del-Rio 2016; Brown and Matsa 2017). In this paper, we exploit a unique mortgage reform in Denmark to provide causal estimates of the effects of liquidity constraints on employment and earnings.

Estimating the effects of liquidity constraints is challenging since the assets and earnings are both endogenously determined. Even studies using exogenous variations often have modest effects on the amount of credit access, or have confounding effects that makes it hard to isolate the effects of liquidity constraints. For example, credit reports also affect the credit checks and therefore employment opportunities (Dobbie et al. 2016; Herkenhoff, Phillips and Cohen-Cole 2016b). Debt relief programs and changes in housing prices affect both short-run liquidity constraints and long-run debt overhang. Therefore many studies rely on structural models to quantify the effects of liquidity constraints (Kaplan 2012; Herkenhoff, Phillips and Cohen-Cole 2016a; Ji 2018).

In this paper, we overcome these challenges using the Danish mortgage reform in 1992 as a natural experiment. The reform allowed homeowners in Denmark, for the first time, to borrow against their housing equity for purposes other than financing the underlying property. The resulting increase in available home equity was large, equivalent to over one year's disposable income for the median treated individual in our sample. Since the notion of home equity finance did not exist prior to this reform and the reform itself was passed within three months, the reform was unexpected for individuals and therefore unrelated to house purchase decisions before 1992. We document that differences in the timing of individuals' home purchase relative to the reform led to systematic cross-sectional variation in the intensity of the reform's treatment across home owners, even after controlling for detailed life-cycle and demographic characteristics. That is, home owners who bought their homes shortly before 1992 had paid down less of their mortgage and hence had less home equity available to borrow against compared to home owners who bought their homes well before the reform. We then combine the household balance sheets data with detailed matched employer-employee data to study the impact of the expanded credit access on employment and earnings.

We find that the reform led to more housing equity extraction and higher debt levels for individuals with more housing equity, and individuals with more housing equity experienced faster wage and earnings growth after 1992. Individuals with equity to value ratio (ETV) higher than 0.2[3] in 1991 experienced an increase in debt of 6% of annual income and a 0.7% increase in earnings after the reform compared to individuals with ETV lower than 0.2 in 1991.

To isolate the reform's effects of relaxing liquidity constraints, we compare the effects on individuals with liquid assets[4] less than one month's disposable income in 1991, and individuals with more liquid assets in 1991. While liquidity-constrained individuals with ETVs higher than 0.2 experienced an increase in debt levels by 13% of annual income and an increase in earnings by 1.6% following the reform, non-liquidity-constrained individuals with ETVs higher than 0.2 only experienced an increase in debt levels by 4% of annual income and an increase in earnings by 0.1%. Furthermore, among individuals affected by the reform, the employment rate of liquidity-constrained individuals declined after the reform, while the employment rate of non-liquidity-constrained increased slightly after the reform. These results suggest that relaxation of liquidity constraint allowed liquidity-constrained individuals to seek jobs that offer higher wages but also higher unemployment risks. On the other hand, the reform had little impact on the wages of non-liquidity-constrained households since they mostly substituted other forms of debt with housing debt.

Our identification relies on the assumption that individuals with more housing equity and individuals with less housing equity would have followed parallel wage trends absent the reform conditional on observed characteristics, including demographics, total wealth, industry and location. We show that individuals with more housing equity and less housing equity had similar wage

[3] Since the maximum loan-to-value ratio allowed is 80%, only individuals with ETVs higher than 0.2 can extract housing equity after the reform.

[4] Liquid assets are non-housing assets like bank deposits, cash, stocks and bonds.

trends before 1991, both for liquidity-constrained and non-liquidity constrained groups. We also conduct a placebo test using data before the reform, and show that individuals with more housing equity in 1989 and less housing equity in 1989 had similar wage growth rates during the period 1990-1992 when controlling for the observed characteristics in 1989.

Why does more credit access lead to higher earnings? We show that the ability to borrow against housing equity can increases wage through two channels. First, risk-averse workers prefer jobs that pay higher wages but have higher unemployment risks when they are able to borrow to insure against unemployment risks (Acemoglu and Shimer 1999; Kaplan 2012). Second, additional credit access increases the value of unemployment, which allows workers to bargain for higher wages.

We first examine how housing equity extractions interact with unemployment risks. We find that workers who recently become unemployed and experience negative earnings shocks are more likely to borrow against housing equity, suggesting that the extra credit from housing equity indeed allows workers to insure against negative labor market shocks. Unemployed workers who have access to housing equity stay in unemployment for longer, and get higher wages when re-entering employment.

We then test the bargaining and sorting channels directly and find evidence supporting both channels. We find that after the reform liquidity-constrained individuals with positive housing equity are more likely to switch jobs and switch cities. The AKM firm fixed effect and average wage of coworkers increase, as well as the probability of being in a top position, suggesting that workers are moving to better firms and better job positions. On the other hand, consistent with the bargaining channel, workers with access to housing equity also experienced higher wage growth within job spells after the reform.

Our paper is closest to Herkenhoff et al. (2016a), who shows that more consumer credit access leads to longer unemployment durations and higher reemployment wages. We find similar effects for unemployed workers, but we also find that the option to borrow from housing equity allows employed workers to switch to more highly-paid jobs and bargain for higher wages. We also highlight the heterogeneity by the level of liquid assets – for individuals with little liquid assets, the additional credit leads to higher wages, but for individuals with a lot of liquid assets, the additional credit has little impact on wages and even reduced reemployment wages for unemployed workers. Compared to consumer loans, the mortgage reform also has much larger impact on the amount of credit access – the option to borrow against housing equity provided an increase in access to credit comparable to at least one year of disposable income for more than 50 percent of the households in our sample.

Our paper is also related to previous literature on how unemployment benefits and payday loans affect employment and wages. The unexpected credit access provided by the mortgage reform combined with the amount of liquid assets before the reform allow us to isolate the effect of relaxing liquidity constraint on wages and employment. However, changes in unemployment benefits also have moral hazard effects in addition to liquidity effects (Chetty 2008). Similar to home equity loans, payday loans also offer insurance against negative shocks (Morse 2011). However, in contrast to our results, payday loans with high interest rates often have high default rates and lead to increased difficulty in paying debts (Melzer 2011; Carrell and Zinman 2014). This is because the interest rate on home equity loan in Denmark is lower than bank loans, and the default rate is very low due to full recourse and a loan-to-value ceiling. The contrast between home equity loan and high-interest payday loans highlights the importance of the contractual form of credit policies intended to alleviate liquidity constraints (Zingales 2015).

Finally, our paper relates to two previous papers that study the impact of the 1992 Danish mortgage reform on labor market. Jensen, Leth-Petersen and Nanda (2015) finds that access to housing equity increases entrepreneurship. We replicated the positive effect on entrepreneurship rates, but we show that the effect is too small to explain our wage effects. We also find similar results for employment and wages when excluding all self-employed workers. Markwardt et al. (2014) finds that the home equity loans partially substitute for unemployment benefits. We show that even though the level of unemployment benefit is high in Denmark, the additional credit offered by the mortgage reform still has large positive effect on wages, which implies that the effect may be even larger in countries with less generous unemployment benefits.

The rest of the paper is organized as follows. Section 2 describes the institutional details of the mortgage reform. Section 3 presents a simple conceptual framework to illustrate how liquidity constraints affect earnings. Section 4 describes the data used and the empirical strategy. Section 5 and Section 6 present the results. Section 7 concludes.

2.2 The 1992 Mortgage Reform in Denmark

The Danish mortgage reform took effect on 21 May 1992. The most important element of the reform is that it enabled home owners, for the first time, to borrow against their home for purposes other than financing the underlying property. Until 2007, mortgage debt in Denmark was provided exclusively through mortgage banks, which are financial intermediaries specialized in the provision of mortgage loans. The May 1992 bill introduced a limit of 60% of the house value for loans for non-housing purposes. This limit was extended to 80% in December 1992. The granting of loans is solely on the basis of the value of housing collateral, which is not true for loans from commercial banks.[5] In other words, the reform allowed individuals with housing collateral who could not previously obtain loans through commercial banks to now get access to credit through mortgage banks. Another feature of the reform is that the maximum maturity of mortgage loans was expanded from 20 to 30 years. For people who were already mortgaged to the limit prior

[5]When granting a mortgage loan for a home in Denmark, the mortgage bank issued bonds that directly matched the repayment profile and maturity of the loan granted. The bonds were sold on the stock exchange to investors and the proceeds from the sale are paid out to the borrower. Once the bank had screened potential borrowers based on the valuation of their property and on their ability to service the loan, all borrowers who were granted a loan at a given point in time faced the same interest rate. This was feasible because of the detailed regulation of the mortgage market. First, mortgage banks were subject to solvency ratio requirements monitored by the Financial Supervision Authority, and there was a legally defined threshold of limiting lending to 80% of the house value at loan origination. In addition, each plot of land in Denmark has a unique identification number, the title number, to which all relevant information about owners and collateralized debt is recorded in a public title number registration system. Mortgage loans have priority over any other loan and the system therefore secures optimum coverage for the mortgage bank in case of default and enforced sale. Creditors can enforce their rights and demand a sale if debtors cannot pay. Furthermore, mortgage banks accumulate a buffer through contributions from all borrowers, and they use this buffer to cover loans defaults. The combination of the regulation around mortgage lending and protection afforded by the title registration system and the buffer to cover loan defaults implied that the loans offered by mortgage banks were very safe, justifying lending based solely on the value of collateral.

to the reform, and who therefore could not establish additional mortgage loans for non-housing consumption, this option provided the possibility of acquiring more liquidity.

The reform was implemented with short notice and passed through parliament in three months. The short period from its introduction to implementation is useful for our identification strategy since individuals have little time to strategically take advantage of the reform. The reform was introduced during the 1992 recession and implemented was right before the Danish economy started to grow rapidly, so the lessons from this reform may shed light on other similar policies during recoveries.

Another element of the reform is the option to refinance. Refinancing makes it possible for borrowers to lower the cost of the loan when the market interest rate falls. This enables the borrower to exploit changes in the market rate of interest in order to reduce the costs of funding. While the other two parts of the reform influence the access to credit, this part of the reform provides house owners with the option to lock in low market interest rates in order to obtain lower monthly payments on their mortgages and an overall gain in wealth.

In this paper we focus on the the first two elements of the reform which provided home owners access to extra credit. The option to borrow against housing equity provided an increase in access to credit comparable to at least one year of disposable income for more than 50 percent of the households in the sample (Leth-Petersen 2010). To isolate the credit access effect of the reform, we will focus on households with high level of equity-to-value ratios and credit-constrained households, who are most likely to be affected by the expanded credit access of the reform. We will discuss the detailed empirical design in Section 4.4.

Mortgage loan delinquencies and defaults have traditionally been low in Denmark. The LTV ceiling of 80 percent on new mortgage loans limits lender losses in the event of a default. In addition, mortgage loans are full recourse in Denmark and borrowers remain personally liable for any shortfall between the sale value of a repossessed property and the outstanding amount of the loan.[6] Therefore borrowers have strong incentives to keep payments and avoid forced sales.

[6] A mortgage loan is declared in default after 3.5 months of non-payment, and forced sale procedures are initiated unless alternative workout procedures are agreed with the borrower. It typically takes no more than nine months from

2.3 Conceptual Framework

We consider a simple theoretical framework similar to Acemoglu and Shimer (2000). For risk averse agents, a relaxation of credit access allows them to smooth consumption over time and increases the utility when unemployed. For simplicity, we consider a static model and study how increases in the utility of unemployment affect wages and employment.

Suppose there are a large continuum of jobs, indexed by their "specificity" $\alpha \in [0, 1]$. Each job produces $y(\alpha)$ when filled. A job with higher α produces more output, so g is an increasing function. However, a high α job is also harder to fill. Workers do not know before applying for the job whether they will be a good fit. High α jobs require a better match between the firm and its employee, so the probability that a random worker possesses the skills and abilities required for a job of specificity α is given by the decreasing function $M(\alpha)$.

A worker consumes her wage w when employed and b when unemployed. Workers and firms get together via search. Jobs are posted at the beginning of each period. Each worker then decides where to apply for a job. After the matching stage, the pair learns whether the worker has the requisite skills. If she does not, both remain unmatched. If she does, the pair produce $y(\alpha)$, and wages are determined by bargaining.

In equilibrium, the worker maximizes her expected utility:

$$\max_{\alpha,w} M(\alpha)u(w(\alpha;b)) + (1 - M(\alpha))u(b) \tag{2.1}$$

where wage $w(\alpha; b)$ is determined by Nash bargaining:

$$\max(u(w(\alpha;b)) - u(b))^{\beta}(y(\alpha) - w(\alpha;b))^{1-\beta} \tag{2.2}$$

A higher b has two effects on wages. First, it increases wages by raising workers' outside options. Given job type α, $w(\alpha; b)$ is increasing in b. This is because workers have a higher value of unemployment due to better consumption smoothing. Second, it increases wages by increasing the

the declaration of default until a forced sale is finalized.

specificity of jobs α that workers search for. Since workers are better insured against unemployment, they are more willing to search for jobs that pay high wages but have lower probability of employment.

In a dynamic setting, these two forces still exist. When workers are credit constrained, an increase in credit access due to the mortgage reform allows them to smooth consumption across time and therefore increase their value of unemployment. As a result, they are able to bargain for higher wages and switch to jobs that have higher earnings and earnings growth. As the same time, they also face greater unemployment risks. We will test these predictions in the following sections.

In a general equilibrium, an increase in workers' access to credit could also change the equilibrium job composition, e.g. by creating more high-wage jobs (Acemoglu and Shimer 1999; Acemoglu 2001). While we are not exploring the general equilibrium effects of the mortgage reform in this paper, this implies that comparing workers affected by the reform and workers not affected by the reform might understate the overall positive wage effects of the reform.

2.4 Data and Research Design

2.4.1 Data

We combine several registers from Statistics Denmark to create a matched employer-employee panel dataset covering all population in Denmark from 1988 to 2000.

The first part of the dataset is regarding wealth and income of the households. The income and wealth information exists because Denmark had a wealth tax during this period. The data on assets and liabilities can be divided into a number of categories.[7] Assets are divided into six different categories: housing assets, shares, deposited mortgage deeds, cash holdings, bonds, and other assets. Housing assets are de ned as the cash value of property as set by the tax authorities. Tax assessed house values are a bit different from market values, and we scaled them with the aggregate ratio of actual house prices to tax assessed values. We define liquid assets as the total

[7]The definitions of these categories are not stable across the observation period, and the level of detail decreases after 1992.

value of non-housing assets. Liabilities are available under four categories: mortgage debt, bank debt, secured debt, and other debt. Mortgage debt is recorded as the market value of the underlying bonds at the last day of the year. House value, cash holdings, mortgage debt, and bank debt are reported automatically by banks and other financial intermediaries to the tax authorities for all Danish taxpayers and are therefore considered to be very reliable. The remaining components are self-reported, but subject to being audited by the tax authorities.

The second part of the dataset is individuals' labor market history. The data are collected from government registers in the last week of November each year, providing detailed data on the labor market status of individuals, including the unemployed and those who do not participate in the labor force. The data contains detailed information on annual wage income, hourly wage, occupation, and unemployment benefits and durations. Each employed worker is matched to her establishment. Establishments are unique physical work locations, such as an office, store, or factory, and each establishment has a unique identifier that is consistent over time. The database links an individual's ID with a range of other demographic characteristics such as their age, gender, educational qualifications, marital status and number of children.

Since we are exploiting a mortgage reform for our analysis, we focus on individuals who are homeowners in 1991 (the year before the reform). Among home owners, we focus on those who are between the age of 25 and 55 in 1991, to avoid interference from retirement decisions. Individuals who are living with their parents and those living in a communal or common household are omitted from the sample. To make sure results are not driven by sample attrition during the sample period, we keep individuals who are observed in every year from 1988 to 1996.[8] This leaves a balanced sample of 826,062 individuals.

2.4.2 Summary Statistics

Table 1 summarizes the statistics of variables on demographics, earnings, and balance sheets for all home owners in 1991. Housing equity constitutes the majority of assets for most of the home

[8] Since we observe people who are unemployed and out of labor force, the sample attrition is very small. Only less than 3% of the observations are dropped.

owners. The median individual has very little liquid assets: the median level liquid asset is about one tenth of average annual earnings. Most people in Denmark are paid their December salary a few days before the end of the year, and asset holdings are summarized for tax purposes at the end of the year. For many households liquid asset holdings corresponding to one month's disposable household income thus amount to having virtually no liquid assets as a buffer.

On the right panel of Table 1 we compare households with equity to value (ETV) above 0.2 and households with ETV below 0.2 in 1991. The reform allowed individuals to borrow up to a maximum of 80% of the home value. Therefore individuals with ETV lower than 0.2 won't be able to extract any housing equity for other purposes. The high-ETV group is older than the low-ETV group since older people are more likely to buy houses at an earlier time. Nevertheless, the other demographic characteristics (gender, marital status, children, education) of high-ETV group is very similar to the low-ETV group, and both groups also have similar wages and unemployment.

At the bottom part of Table 1 we calculate the potential amount of housing equity that was unlocked by the reform as housing equity in 1991 minus 20 percent of the housing value (it takes the value of zero if ETV is less than 0.2). It shows that the amount of equity unlocked was substantial. The reform unlocked an average value of 79,000 DKK (about 13,000 USD) in housing equity. The average amount of housing equity unlocked for people with ETV below 0.2 is very little, while the average amount of housing equity unlocked for people with ETV above 0.2 is 164,000 DKK, which is close to one year's earning.

2.4.3 Identify Housing Equity Extraction

We follow Bhutta and Keys (2016) to identify housing equity extractions in the data. We define equity extractions as instances when a borrower's outstanding mortgage debt increases by more than 5 percent over a one year period, with a minimum increase of 5,000 DKK. Since we do not observe the trade line information for each mortgage held, we further require that the borrower do not move over the one year period to exclude second mortgages and new mortgages. This increase in mortgage debt can come from borrowing against housing collateral, or changes in the maturity of the mortgage.

Figure 1 shows the fraction of home owners in each year that have positive equity extractions. Before 1992 the fraction is around 1%, and these may be false positives of new mortgages (e.g. summer houses). After 1992, the fraction of borrowers with an increase of at least 5 percent in total mortgage balance has risen sharply to over 5% per year. Between 1993 and 1996, the average fraction of home owners extracting equity is 11.8%, which is close to the fraction in Bhutta and Keys (2016). In 1994, almost 23% of homeowners borrowed against their housing equity.

How does ETV affect equity extraction? Figure 2 (a) shows that the probability of extracting housing equity between 1992 and 1996 is monotonically increasing in the ETV in 1991. Borrowers with ETV higher than 0.6 in 1991 are twice more likely to extract their housing equity than households with ETV lower than 0.2 in 1991. Note that the probability of extracting equity is not zero even for households with ETV lower than 0.2 in 1991, since housing prices grew rapidly since 1991 and higher housing prices led to higher ETVs for home owners. Figure 2 (b) (b) plots the total share of housing equity extracted by the borrower against ETV in 1991. The share of housing equity extracted is the amount of increase in outstanding mortgage debt normalized by the average housing price over the one year period, and we sum up all the shares for years 1992-1996. Borrowers with low ETV in 1991 extracted little equity, while borrowers with ETV higher than 0.6 extracted about 20% of their housing equity.

2.4.4 Empirical Strategy

The reform allowed individuals, for the first time, to borrow against their housing equity for non-housing purposes. Our research design exploits cross-section variation in the exposure to the reform's treatment across individuals. As shown in Figure 2, individuals with higher ETV at 1991 are more likely to borrow against housing equity and are able to extract more housing equity after the reform. We therefore divide all individuals into two groups based on whether their ETV in 1991 is higher than 0.2. We then use a difference-in-differences approach to compare the differential responses of the liabilities, income and employment of the two ETV groups to the reform. Given that the reform was first introduced in May of 1992 and data are recorded as of November, we include 1992 in our post-reform period and measure individual attributes as of 1991.

Our baseline specification is as follows:

$$y_{it} = \beta Post_t \times \mathbf{1}(ETV_{91} > 0.2)_i + \theta X_i^{1991} \times \phi_t + \alpha_i + \varepsilon_{it} \tag{2.3}$$

where y_{it} is the outcome for person i at year t, $Post_t \times \mathbf{1}(ETV_{91} > 0.2)$ equals one if person i had ETV greater than 0.2 in year 1991 and year t is 1992 or later. The key coefficient is β, which measures the high-ETV group's response to the reform relative to the low-ETV group, who were affected little by the reform by construction.

We include person fixed effects in all regressions. Standard errors are clustered at the individual level. We also account for the differential response of individuals at different points in the life cycle, wealth, and working in different industries and living in different municipalities by including an interaction between these individual covariates measured in 1991 and year fixed effects. Specifically, we include in X_i^{1991} indicators for the individuals' gender, education level, marital status, children, age, decile of total household wealth,[9] the municipality of residence, and the industry the person works in. We interact each of these characteristics with year dummies, ϕ_t, to control for different trends in debt accumulation and earnings across people with different observable characteristics. Thus we are comparing two "identical" individuals (in terms of their age, gender, educational background, wealth, marital status and children) who work in the same industry and live in the same municipality, but one who bought the home some years before the other.

The identifying assumption is that, conditional on the observed covariates in 1991, the timing of the housing purchase is uncorrelated with changes in employment and wages after 1992. The fact that the mortgage reform was unexpected indicates that the reform did not directly impact the decision to purchase houses before 1992. Table 1 shows that individuals with high ETV are older and have less debt, but have similar marital status, children, education, and income as individuals

[9] The asset levels would affect workers' attitude towards risk. For example, with constant relative risk aversion, richer workers have lower absolute risk aversion. As a result, they are more willing to accept riskier jobs, compared to poorer workers.

with low ETV. Although age is an important determinant of the timing of housing purchase, even for people with the same age there is a lot of variations in the timing of housing purchase.[10] Potential threats to identification would be unobserved shocks that affect both the timing of housing purchase and the changes in employment and wages after 1992. For example, individuals who purchased houses more recently may have experienced a recent divorce, which may also affect their income. In such case, the incomes of different ETV groups would have started to diverge before the 1992 reform , and we can use the pre-trend to assess the validity of the identifying assumption.

Since there is an almost linear relationship between ETV in 1991 and housing extraction (Figure 2), in an alternative specification, we also interact the post-reform dummy with the level of ETV in 1991:

$$y_{it} = \beta Post_t \times ETV_{91,i} + \theta X_i^{1991} \times \phi_t + \alpha_i + \varepsilon_{it} \tag{2.4}$$

To isolate the effects of the reform on individuals' liquidity constraints, we compare the effects of the reform on individuals with high level of liquidity assets and low level of liquidity assets. Since the key element of the reform is to relax individuals' liquidity constraints by allowing them to borrow against housing equity, it should have little effect on individuals who already have a large buffer of liquid assets. We define an individual as having low liquidity if her average level of liquid assets is less than her average monthly income between 1988 and 1991.[11] By this definition, almost 40% of all the individuals in our sample have low liquidity before the reform.

To estimate the differential effect of the reform on high-liquidity and low-liquidity households, we estimate the following triple-differences specification:

$$\begin{aligned} y_{it} = {} & \beta Post_t \times \mathbf{1}(ETV_{91} > 0.2)_i + \gamma Post_t \times \mathbf{1}(ETV_{91} > 0.2)_i \times \text{LowLiquidity}_i + \\ & \delta \text{LowLiquidity}_i + \theta X_i^{1991} \times \phi_t + \alpha_i + \varepsilon_{it} \end{aligned} \tag{2.5}$$

[10] For example, housing purchases can be driven by life events (Bernstein and Struyven 2017) or beliefs about future changes in housing prices (Bailey et al. 2018).

[11] We also use an alternative measure of the maximum liquid asset to income ratio before 1992, and get similar results. Liquid asset holding is not a perfect indicator of constrained status (Jappelli 1990). For the test implemented here a sufficient requirement is that the high liquid asset group is not constrained. It is not required that households with low liquid assets are all restricted, only that some households in the low liquid asset group are affected by constraints.

where Lowliquidity_i is an indicator for having less liquid assets than one month's disposable income in 1991. β is the effect of the reform on high-ETV group relative to low-ETV group among high-liquidity individuals, and $\beta + \gamma$ is the effect of the reform high-ETV group relative to low-ETV group among low-liquidity individuals. The difference γ measures the differential response of credit-constrained individuals relative to unconstrained individuals to the increased credit access.

To further test whether individuals with high ETVs would have parallel trends in wages and employment as individuals with low ETVs, we conduct a placebo test in Section 5.4 using only years before the mortgage reform. We estimate the following specification:

$$y_{it} = \beta Post89_t \times ETV_{89,i} + \theta X_i^{1989} \times \phi_t + \alpha_i + \varepsilon_{it} \tag{2.6}$$

where $Post89_t$ is an indicator for years after 1989, and $ETV_{89,i}$ is the ETV in 1989. If high-ETV individuals and low-ETV individuals differ in systematic ways in their unobserved characteristics, we would expect to see different trends in this pre-period even when borrowing against housing equity was not possible.

2.5 Results

2.5.1 Effects of the Reform on Borrowing

To verify that the mortgage reform impacted the homeowners, we first look at the effects of the reform on equity extraction and the overall liabilities. Columns (1) to (3) of Panel A in Table 2 show results from difference-in-differences regressions of measures of borrowing on indicators for high- and low- ETV groups after 1992 (Equation 2.3). The unit observation is person-year. Following the mortgage reform, individuals with high ETVs are more likely to extract housing equity and extract a larger share of their housing equity, confirming the findings in Figure 2. In Column (3), we use total liabilities divided by average annual income as the dependent variable. Total liabilities include mortgage, bank debt and other secured and unsecured debt, and average income is the average annual income during the period 1988-1996. High-ETV individuals increased their debt level substantially after the reform: individuals with ETVs higher than 0.2 in 1991 increased their total debt level by 7.6% of their annual earnings than individuals with ETVs lower than 0.2 in 1991.

Next, we study how the effects differ by whether the individual is liquidity constrained or not. If the reform increased the level of debt because it relaxes the credit constraint, it should have little impact on the borrowing for individuals who have a lot of liquid assets and are not credit constrained. Columns (4) to (6) of Table 2 show the triple-differences estimates. First, the triple-interaction terms of low liquidity, high ETV and post 1991 have positive and significant effects for all three measures, indicating that individuals with little liquid assets borrow more against housing equity and increase their debt more after the reform. Second, among individuals with a lot of liquid assets and thus not liquidity constrained, those with high ETVs also borrow more against housing equity, but the change in total debt level is very little. For example, households with high liquidity and ETV higher than 0.2 only increased their total debt by 4% of annual earnings, while households with low liquidity and ETV higher than 0.2 increased their total debt by 13% of annual earnings. This suggests that equity extractions crowd out other sources of debt such as bank loans for non-liquidity-constrained households.

In Panel B of Table 2, we use the continuous measure of ETV in 1991 as the treatment variable (Equation 2.5) and get similar results. A one-standard-deviation increase in ETV of 1991 increases debt level by 8% of a annual salary. The effect on borrowing is twice larger for liquidity-constrained individuals than non-constrained individuals.

These results indicate that the reform indeed relaxed credit constraint for individuals' with high ETVs. For credit-constrained individuals, this increased the borrowing significantly; for non-credit-constrained individuals, the additional borrowing from housing equity crowds out other sources of borrowing and has small impact on the amount of total debt.

2.5.2 Effects of the Reform on Wages and Employment

How does the relaxation of credit constraint affect wages and employment? Table 3 shows results from our baseline regressions using measures of wages and employment as dependent variables.

In Column 1, we use normalized earnings as dependent variable where we divide annual earnings by the average annual earnings from 1988 to 1996.[12] This measure takes into account individuals with zero earnings. Following the reform, individuals with ETVs higher than 0.2 experienced a 0.7% gain in earnings, and individuals with ETVs between 0.4 and 0.6 experienced a 0.6% gain in earnings. In Column 2, we use log annual wage as dependent variable and thus excludes individuals with zero earnings and get similar results: earnings increased by 0.4% for individuals with ETVs higher than 0.2 in 1991. In Column 3, the dependent variable is employment rate, which equals to one if the individual has positive earnings and zero otherwise. The employment rate of high-ETV groups increased by 0.8%, but the difference is not statistically significant.

Column 4 to 6 Table 3 present results for triple-differences specification (equation 2.5). For credit-constrained individuals, an ETV of greater than 0.2 leads to an 1.6% increase in earnings . On the other hand, for non-constrained individuals, a higher ETV is not associated with significantly higher earnings after the reform. Nevertheless, the employment rates of liquidity-constrained individuals fell after the reform, while employment rates of non-liquidity-constrained individuals increased after the reform. This suggests that the higher earnings experienced by the individuals with high ETVs are due to the relaxation of borrowing constraint for liquidity-constrained individuals.

In Panel B of Table 3, we use continuous ETV as the treatment variable. A one-standard-deviation increase in ETV in 1991 increases earnings by 0.4 percent on average, and increases earnings by 0.8 percent for liquidity-constrained individuals.

How big is this effect? The estimates in Column 4 indicates that the earnings of liquidity-constrained individuals with ETVs higher than 0.2 increase by 1.6% after the reform. Assuming that the earnings growth remain the same afterwards, and that careers last 20 years and discount rate is 5 percent, an 1.6% earnings increase implies a loss in present discounted value equal to 20% of annual earning, which is larger than the increase in amount of borrowing by these individuals (13% of annual earning from Column 6 of Table 2).

[12]The normalized earnings are winsorized at 1st and 99th percentile. Results are similar when normalizing earnings by the average earnings before the reform (1988-1991).

To test whether the wages of high ETV groups and low ETV groups would have followed parallel trends without the reform, we estimate the treatment effects on wages over time as follows:

$$y_{it} = \alpha_i + \sum_{\tau=1988}^{1996} \beta_\tau \mathbf{1}(ETV_{91} > 0.2)_i \times D_t(\tau) + \theta X_i^{1991} \times \phi_t + \varepsilon_{it} \tag{2.7}$$

where $D_t(\tau)$ is equal to one if $t = \tau$. β_τ is the effect of high ETV on wages in year τ, and year 1991 is chosen as the base year. Figure 3 plots the coefficients β_τ. The effects are insignificant from zero before 1991, and are increasing over time after 1991. We estimate the same regression separately for low-liquidity individuals and high-liquidity individuals and plot the coefficients in the bottom figure of Figure 3. For both groups, individuals with high ETVs have similar wage trends as individuals with low ETVs before 1991, which suggests that conditional on controls individuals with different levels of ETVs follow similar counterfactual wage trends. Following the reform, having higher ETV has no effect on wages for the individuals with a lot of liquid assets, while higher ETV leads to higher wage growth for individuals with little liquid assets, suggesting that being able to borrow against housing equity leads to higher wage growth for liquidity-constrained individuals.

2.5.3 Heterogeneity

We examine the heterogeneity of treatment effects on wages by demographic characteristics in Table 4. Each column is a separate regression for all individuals in a demographic group, and the dependent variable is normalized earnings.

Column (1) to Column (3) show that workers with basic education benefited the most from the reform. Workers with only basic education who were liquidity-constrained and had high ETVs in 1991 experienced a wage increase of over 3%, while workers with vocational education and higher education have much smaller wage gains. This might be due to the fact that less skilled workers have higher income volatility.

Column (4) and Column (5) show that women have larger wage responses to the reform than men. The last two columns show that older workers experienced larger increases in earnings following the reform than younger workers. Bhutta and Keys (2016) find that the equity extraction of young homeowners are more responsive to house price growth since they are more likely to be collateral constrained. However, in our setting older homeowners are more likely to have higher ETVs and more expensive houses, and therefore are more likely to benefit from the reform.

2.5.4 Robustness

Are the results driven by entrepreneurs?

One alternative explanation for our findings is that the option to borrow against housing equity encourages workers to start up their own businesses and earn more. Schmalz, Sraer and Thesmar (2012) shows that increase in the value of housing collateral leads to higher probability of becoming an entrepreneur. Jensen, Leth-Petersen and Nanda (2015) studied the same mortgage reform as our paper, and found that homeowners with high ETVs in 1991 are more likely to become entrepreneurs.

Consistent with Jensen, Leth-Petersen and Nanda (2015), we find that individuals with high ETVs in 1991 have a 0.1% higher probability of becoming self-employed, and the effect is more pronounced for liquidity-constrained individuals. However, the effect on entrepreneurship rate is much smaller than the effect on earnings – for entrepreneurship to explain all of the increase in earnings, the earnings of the entrepreneurs would have to be 7 times higher than the earnings in other jobs.

To further investigate how much of the earnings increase is due to entrepreneurship, we re-ran our baseline regressions excluding individuals who were self-employed between 1992 and 1996. Table 5 shows that after excluding entrepreneurs, we still find a similar earnings increase among individuals who had high ETVs in 1991 and were liquidity-constrained. Therefore increase in entrepreneurship cannot explain the positive effect of credit access on earnings.

Placebo Test Using Pre-Reform Years

The key identifying assumption of our empirical strategy is that individuals with high ETVs follow the same wage trends as individuals with low ETVs conditional on observable characteristics. We have shown that individuals with high ETVs in 1991 and individuals with low ETVs in 1991 have parallel wage trends before 1991. Nevertheless, it is still possible that individuals with higher ETVs in 1991 have different wage trends after 1991 for reasons other than the mortgage reform. For instance, individuals with higher ETVs have less debt and lower leverage, and previous studies have shown that debt overhang may affect labor supply and job search behavior (Bernstein 2018; Ji 2018).

To test this we perform a placebo test using data before 1992. We divide the period into a pre-period (1988-1989), and a post-period (1990-1991), and test whether individuals with higher ETVs in 1989 had higher wage growth in 1990 and 1991. Since the placebo sample is before the mortgage reform took place, we would not expect to see differential wage trends for individuals with high ETVs in 1989 since they wouldn't be able to extract their housing equity to finance their other needs. We apply the same difference-in-differences specification as Equation 2.3, and measure all observable characteristics at 1989.

Table 6 presents the results of the placebo test. Individuals with ETV higher than 0.2 in 1989 have similar trends in normalized earnings and log wages from 1990 to 1991 as individuals with ETV lower than 0.2 in 1989.[13] In Column (4) to Column (6), we compare the wage responses for liquidity-constrained and non-liquidity-constrained individuals based on their level of liquid assets in 1988 and 1989. The coefficient of the interaction term between low liquid assets and high ETV is statistically insignificant from zero in all of these regressions, indicating that liquidity-constrained and non-liquidity-constrained individuals have nearly identical wage and employment responses to different levels of ETV in 1989. This suggests that liquidity-constrained individuals with high ETVs had faster wage growth after 1992 precisely because the reform relaxed their credit

[13]The positive effect of high ETV on subsequent earnings is consistent with the debt overhang effects (Ji 2018; Bernstein 2018). For example, individuals with more debt may have less incentive to work due to implicit taxes. However, such effects are small in our setting.

constraints.

2.6 Mechanisms

In this section we investigate the mechanisms of how expanding credit access leads to higher earnings. As shown in our conceptual framework, a relaxation of credit constraint increases the value of unemployment and allows individuals to choose jobs that are risker and have higher earnings, as well as bargain for higher wages at current jobs. We first start by describing which individuals borrow from housing equity, and show that access to housing equity are indeed used to insure against negative labor income shocks. Then we look at the job search behavior of unemployed individuals, and show that individuals with more housing equity stay in unemployment for longer and get higher reemployment wages. Finally, we look at job switching behaviors and within-job-spell wage changes of all employed workers to examine the job search channel and the bargaining channel separately.

2.6.1 Who Borrows Against Housing Equity?

We start our analysis by looking at the determinants of equity extraction. If the additional borrowing from housing equity provides insurance against negative labor market shocks, we would expect to see more borrowing when individuals experience negative labor market shocks. For example, Kaplan (2012) find that workers are more likely to move back home to live with their parents when they lose their jobs.

We estimate a linear probability model of the propensity to extract housing equity[14]:

$$\text{Extract}_{ict} = \beta_1(\text{IncomeGrowth}_{it}) + \beta_2(\text{IncomeGrowth}_{it} \times \text{LowLiquidity}_i) + \gamma \mathbf{X}_i + \alpha_{ct} + \epsilon_{ict} \quad (2.8)$$

where Extract_{it} is an indicator variable for housing equity extraction, IncomeGrowth_{it} represents

[14]The large dataset and large number of FEs raise challenges for a probit specification related to computation and interpretation. Furthermore, comparing the main results from our estimated linear probability model with the appropriate marginal effects (including accounting for the interaction term) from a probit model yielded virtually identical estimates.

the average income growth rate over the past three years. We interact the income growth with an indicator variable for having low level of liquid assets in 1991 to study the different responses of high-liquidity and low-liquidity individuals. The vector $\mathbf{X}_i$ includes individual-level covariates including ETV in 1991, the level of liquid assets in 1991, and decile of total wealth in 1991. We also include municipality-year fixed effects to account for different housing price trends at the municipality level. The unit of observation is person-year, and we only include observations for homeowners after 1991.

Column (1) of Table 7 shows that individuals that experienced a negative earnings shock are more likely to borrow against housing equity. A standard deviation decrease in income growth leads to a 1.1 percentage point rise in equity extraction: a 10 percent increase relative to the 11 percent average extraction rate across all years after 1991. Households with little liquid assets have on average 3.4 percentage points higher extraction rate. Column (2) shows that liquidity-constrained individuals are also more likely to extract equity in response to negative earnings shocks: a standard deviation decrease in income growth leads to a 0.7 percentage points increase in equity extraction for individuals with sufficient liquid assets, and a 1.4 percentage points increase in equity extraction for individuals with little liquid assets.

In Columns (3) and (4) we examine how labor market shocks affect equity extraction. Workers who lost their jobs are more likely to extract equity. Workers are also more likely to extract equity when their employers have negative employment growth. To account for unobserved heterogeneity across homeowners in their propensity to extract equity that may be correlated with labor market outcomes, we include person fixed effects in Columns (5) and (6). For the same person, the timing of equity extraction is positively correlated with unemployment and negatively correlated with shocks to earnings. These evidence indicate that homeowners borrow against their housing equity to insure against negative labor market shocks.

2.6.2 Effects on Unemployed Workers

Extra credit from housing wealth allows unemployed households to augment today's liquid asset position by borrowing against future income. Chetty (2008) shows that increases in unemployment benefits or severance payments lead to longer unemployment durations, especially for liquidity constrained households. Herkenhoff et al. (2016a) finds that better access to consumer credit increases unemployment durations and wages conditional on finding a job.

To examine how the borrowing against housing equity affect the job search behavior of unemployed workers, we compare unemployment durations and reemployment wages of workers who are unemployed at year 1991 and have different levels of housing equity. In particular, we estimate the following equation:

$$D_i = \gamma \mathbf{1}(ETV_{91} > 0.2)_i + \pi \mathbf{1}(ETV_{91} > 0.2)_i \times \text{LowLiquidity}_i + \beta \mathbf{X_i} + \varepsilon_i \tag{2.9}$$

where D_i is the unemployment duration of individual i, control $\mathbf{X_i}$ include age dummies, municipality fixed effects and dummies for year entering unemployment. The coefficients of interest are γ, which is the effect of having positive housing equity on unemployment duration, and π, which is the differential effect of having positive housing equity of liquidity-constrained individuals relative to non-liquidity-constrained individuals.

Table 8 shows that having positive housing equity on average increases unemployment durations by 0.07 years, or 3.7 weeks. Liquidity-constrained households increased unemployment durations by 0.18 years, or 9.1 weeks, while non-liquidity-constrained households increased their unemployment durations by 0.04 years, or 2.1 weeks.

Column 3 and Column 4 looks at how access to housing equity affects reemployment wages. The dependent variable is replacement rate, defined as reemployment wage divided by average wage in three years before the unemployment spell. On average the access to housing equity has insignificant positive effect on reemployment wages. However the effect is opposite for liquidity-constrained and non-liquidity-constrained individuals: liquidity-constrained households with positive housing equity experienced a 5% higher replacement rate, whereas non-constrained households with positive housing equity experienced a 2% lower replacement rate.

Our results are similar to Herkenhoff et al. (2016a), who finds that an increase in unused revolving debt of one year's income leads to an increase in unemployment durations by 0.11 years and an increase in replacement rate by 6%. In addition, we show that the effect is heterogeneous by individuals' liquidity constraint: while more credit access allows liquidity-constrained individuals to search for jobs with higher wages, more credit access make non-liquidity-constrained individuals stay in unemployment for too long and hurt their reemployment wages.

2.6.3 Sources of Wage Growth Following the Reform

We investigate the mechanisms through which access to housing equity affects wages. As shown in the conceptual framework there are two channels. The first channel is sorting. Workers with access to housing assets are more selective and therefore are more likely to move to firms and jobs with higher wages. The second channel is bargaining. The ability to borrow in unemployment improves workers' outside options and allows them to bargain for higher wages given the job type. Both mechanisms are widely discussed in the theoretical literature, but direct empirical evidence to distinguish between the bargaining and sorting mechanisms is rare.

We first look at whether workers are more likely to switch jobs after the reform allowed them to borrow against housing equity. Table 9 shows the difference-in-differences estimates as in Equation 2.5. The dependent variable in Column 1 is an indicator variable that equals one if the worker switches employer. Liquidity-constrained individuals with ETV higher than 0.2 are 1.5% more likely to switch jobs after the reform, while non-constrained individuals with ETV higher than 0.2 are only 0.2% more likely to switch jobs. Individuals are also more likely to move to new cities when they can borrow against their housing equity. The additional credit helps individuals cover their moving costs or finance down payments for their new homes. This is consistent with the view that liquidity constraints lock in people at their current locations (Brown and Matsa 2016).

The access to housing credit also allows people to move to better firms. We estimate two-way fixed effects model as in Abowd, Kramarz and Margolis (1999) for the period 1988-1996, and use the estimated firm fixed effects as dependent variables to measure the firm-specific wage premium. Column (3) shows that workers with high ETVs move to firms that pay higher wages after the mortgage reform. Individuals with ETVs higher than 0.2 in 1991 are employed in firms that pay 0.2% higher wages. In Column (4), we use the average wage of coworkers as an alternative measure of firms' wage premium, and find similar results.

Having credit access also affects workers' job positions within firms. We define a job position as a top position in a firm if it is a managerial position or in the highest hierarchy. About 10% of the workers are in top positions. Column (5) shows that individuals with high ETVs are more likely to work in a top position following the reform.

Finally, we test the bargaining channel by looking at wage changes within job spells. In Column 6, we include establishment-year fixed effects i.e. job fixed effects. Workers with ETVs higher than 0.2 experience an increase in wages of 0.4% in their existing jobs, which accounts for approximately 20% of the positive wage effect.

Taken together the results suggest that both sorting and bargaining channels are important. Bargaining explains about 20% of the positive wage effect, and the rest is explained by moving to firms paying higher wages and better job positions.[15]

2.7 Conclusion

Housing assets constitute the majority of wealth for most households, but they are highly illiquid, and many individuals are liquidity constrained despite owning a large amount of housing wealth (Gorea and Midrigan 2018). In this paper we exploit a natural experiment in Denmark which allowed homeowners to borrow against housing equity, and find that the expanded credit access

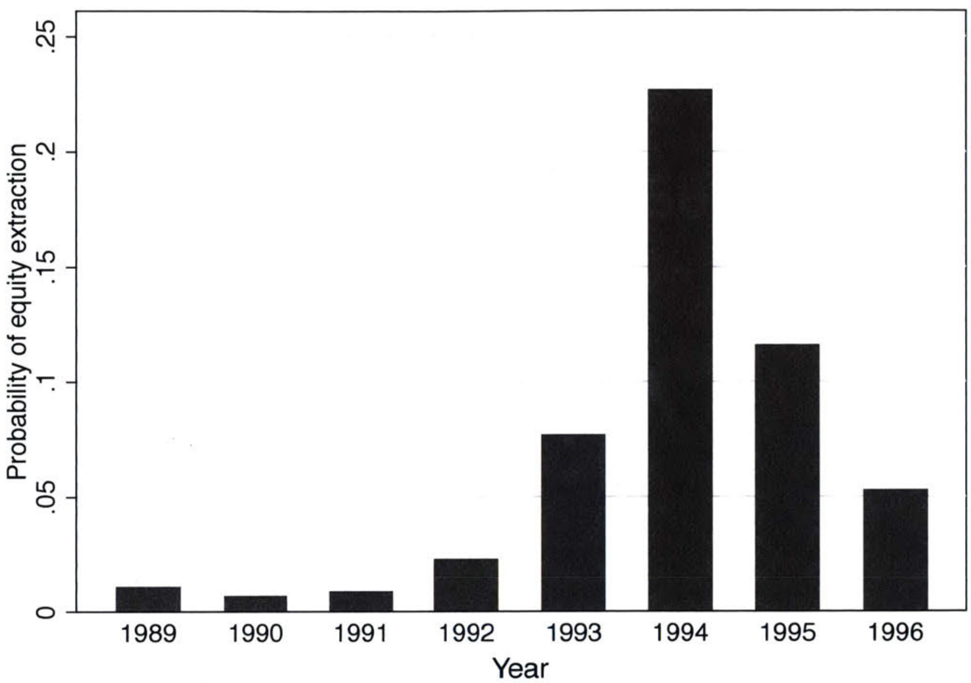
[15] Another explanation is that relaxation of credit constraint increases workers' productivity by encouraging more human capital accumulation. Similar to firms cutting investment when financially constrained (Bolton, Chen and Wang 2011), individuals may also invest less in human capital when credit constrained (Sun and Yannelis 2016; Fos, Liberman and Yannelis 2017). In Appendix Table A3 we show that the probability of training increases for individuals with high ETVs after the reform, but the effect on duration of training is insignificant.

increased earnings and job quality for liquidity-constrained individuals.

Our results suggest that access to housing collateral plays an important role in insuring against negative income shocks even in a country like Denmark, which has one of the most generous UI benefit systems among OECD countries. Markwardt et al. (2014) find that people with more housing equity are less likely to take up UI, suggesting that borrowing in credit markets is substitute for public insurance like UI benefits. However, contrary to UI benefits, we do not find negative employment effects of more credit access, perhaps because borrowers have more incentives to work to pay up the debt. How to optimally combine credit markets with public insurance to relax liquidity constraints is an interesting direction for future research.

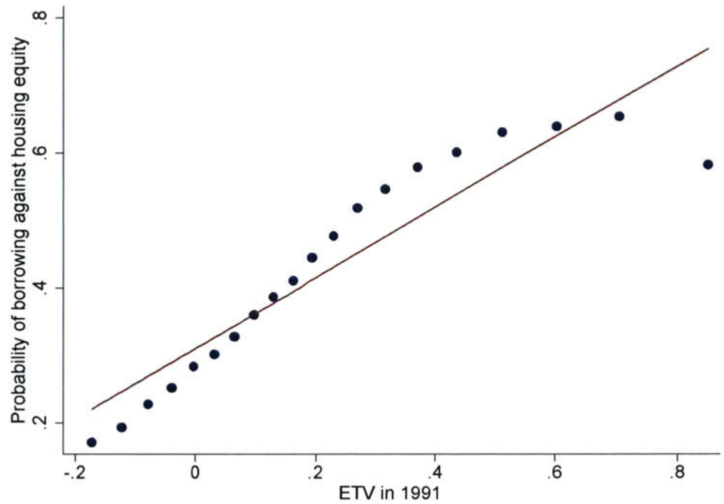
2.8 Figures

Figure 1: Share of homeowners extracting equity by year

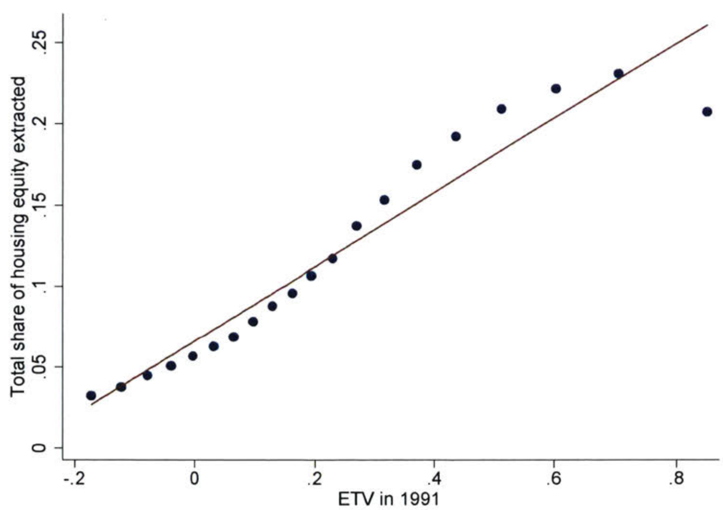


Notes: This figure shows the share of home owners extracting housing equity in Denmark by year. Following Bhutta and Keys (2016), we define extraction of housing equity as instances when a borrower's outstanding mortgage debt increases by more than 5 percent over a one year period, with a minimum increase of 5,000 DKK. Since we do not observe the trade line information for each mortgage held, we further require that the borrower do not move over the one year period to exclude second mortgages and new mortgages.

Figure 2: Equity extraction by ETV in 1991



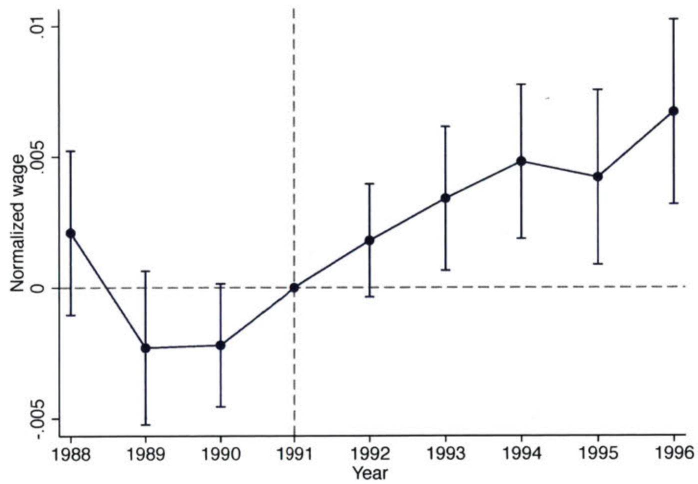
(a) Probability of equity extraction



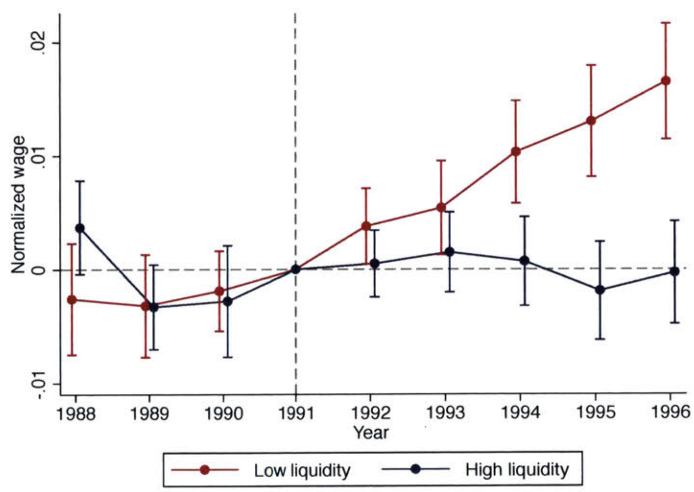
(b) Fraction of housing equity extracted

Notes: This figure shows the binscatter of the probability of equity extraction and the share of housing equity extracted over the five-year period of 1992-1996 against the equity-to-value (ETV) ratio in 1991. Each dot contains the same number of individuals. The share of housing equity extracted is calculated as the amount of increase in outstanding mortgage debt normalized by the average housing price over the one year period, and we sum up all the shares for years 1992-1996.

Figure 3: Effects of reform on wages over time



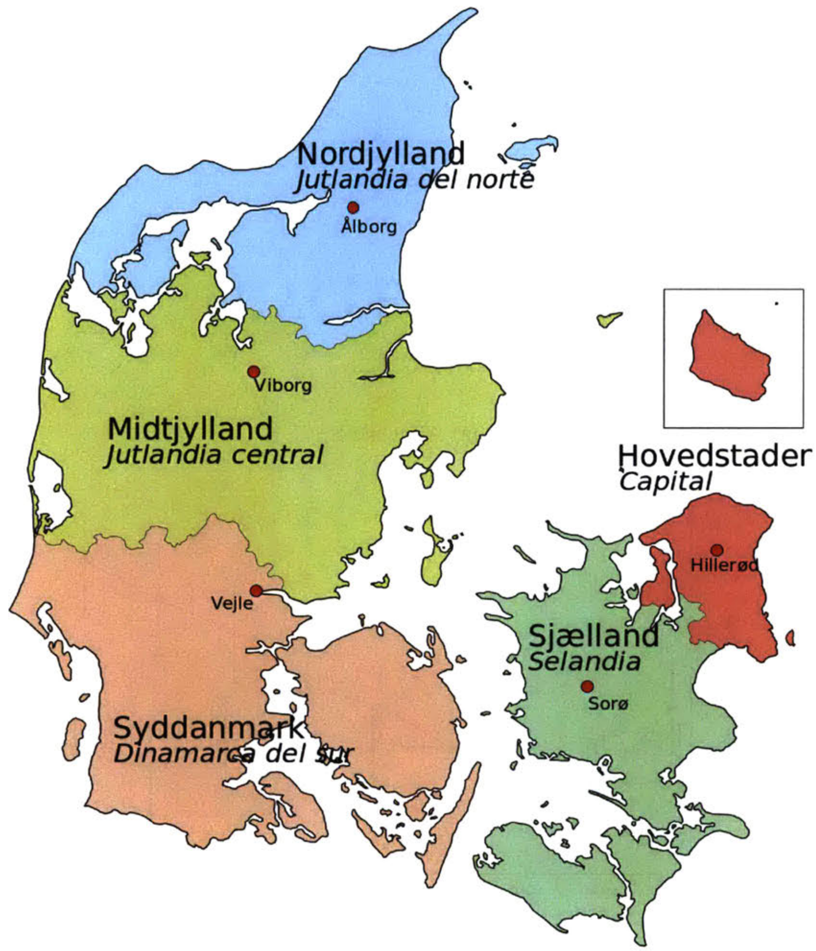
(a) All workers



(b) By level of liquid assets

Notes: This figure shows the dynamic treatment effects of the mortgage reform on earnings of individuals with ETVs higher than 0.2 in 1991 over time, i.e. coefficients β_τ in equation (7). The dependent variable is annual wage earnings normalized by the average annual wage earnings during the sample period. Control variables include year fixed effects interacted with fixed effects for birth-cohort, decile of wealth, educational level, partner, gender and having children, each measured in 1991, as well as person fixed effects and municipality-year fixed effects. Standard errors are clustered at the individual level. The bottom figure plots the treatment effects for low-liquidity individuals (individuals with liquid assets less than one month's disposable income in 1991) and high-liquidity individuals respectively.

Figure 4: Regions of Denmark



Notes: The five Regions of Denmark were created as part of the 2007 Danish Municipal Reform, when the counties were abolished. Each region is close to a commuting zone in the United States: it mostly takes less than two hours to travel between places within a region.

Table 1 Summary Statistics

	All home owners			ETV<0.2	ETV>0.2
	Mean	Median	Std. Dev.		
Age	40.6	41	8.52	38.2	44
Female	0.35	0	0.48	0.37	0.34
Kids	0.27	0	0.45	0.29	0.25
Partner	0.84	1	0.37	0.84	0.84
Basic education	0.3	0	0.46	0.29	0.31
Vocational training	0.44	0	0.5	0.43	0.44
College education	0.26	0	0.44	0.26	0.24
Experience	15.6	15.1	7.69	14.4	17.3
Annual earning (1000 DKK)	212.5	199.6	181.9	217.7	207.7
Annual wage (1000 DKK)	192.3	197.1	131.8	198.8	186.4
Hourly wage	152.2	137	79.2	150.6	151.9
Unemployment in 1991	0.06	0	0.24	0.06	0.06
AKM Firm FE	0.33	0.35	0.2	0.33	0.32
Job tenure	4.7	3	4.3	4.2	5.3
Housing price in 1991 (1000 DKK)	411.1	356	230.6	367.9	434.9
Total asset in 1991 (1000 DKK)	525.4	410.4	1425	455.9	590.6
Liquid asset in 1991 (1000 DKK)	114.3	22.8	1374	88	140.7
Total liability in 1991 (1000 DKK)	380.7	312.7	743.7	452.6	302.4
Mortgage debt in 1991 (1000 DKK)	263.7	228.9	188	331.6	189
Bank debt in 1991 (1000 DKK)	79.4	37.4	611.7	78.9	79.8
Maximum housing equity unlocked in 1991 (1000 DKK)	78.7	9.5	127.8	1.2	163.5
ETV IN 1991	0.34	0.27	0.31	0.05	0.62
Number of observations	7,434,558			3,564,324	3,870,234
Number of person	826,062			396,036	430,026

Notes: This table reports the summary statistics for 826,062 in our balanced-panel sample of home owners. Worker level information are from income register and is available for the entire sample period (1988-1996). All monetary values are normalized to real 2010 Danish krones. 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 than that are classified as "basic education". Housing assets refer to the tax assessed valuation of the individual's property scaled with the ratio of market prices to tax assessed house values for house that have been traded in that municipality and year. Non housing assets include the individual's other assets including stocks, bonds and bank deposits. All medians are calculated as the average value of 10 observations around the median.

Table 2 Effects of Mortgage Reform on Borrowing

Dependent variable	(1) Equity Extraction		(2) Fraction of equity extracted		(3) Liability/ Income		(4) Equity Extraction		(5) Fraction of equity extracted		(6) Liability/ Income	
	A. Treatment: Dummy for (ETV91>0.2)											
Post*1(ETV91>0.2)	0.0620	***	0.0211	***	0.0757	***	0.0491	***	0.0178	***	0.0427	***
	(0.0004)		(0.0001)		(0.0026)		(0.0004)		(0.0001)		(0.0032)	
Post*1(ETV91>0.2)* Low Liquidity							0.0374	***	0.0097	***	0.0906	***
							(0.0007)		(0.0002)		(0.0046)	
	B. Treatment: ETV91											
Post*ETV91	0.1045	***	0.0385	***	0.2384	***	0.0788	***	0.0313	***	0.1799	***
	(0.0006)		(0.0002)		(0.0044)		(0.0007)		(0.0002)		(0.0053)	
Post*ETV91* Low Liquidity							0.0863	***	0.0241	***	0.1788	***
							(0.0012)		(0.0004)		(0.0076)	
Person FE	✔		✔		✔		✔		✔		✔	
Municipality*year FE	✔		✔		✔		✔		✔		✔	
Observables*year FE	✔		✔		✔		✔		✔		✔	
Number of observations	6,819,246		6,819,246		6,819,246		6,819,246		6,819,246		6,819,246	

Notes: (* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$) This table reports estimates from OLS regressions (equation (4) and (5)). Equity extraction is defined as in Bhutta and Keys (2016). The share of housing equity extracted is calculated as the amount of increase in outstanding mortgage debt normalized by the average housing price over the one year period. Liabilities include mortgage debt, bank debt, secured debt, and other debt. The main right-hand-side variables are equity to value ratio in 1991, ETV interacted with an indicator for the post mortgage reform period, and interactions of ETV, post-reform-period dummy, and an indicator variable which equals one if the individual has liquid assets less than one month's disposable income in 1991. Control variables include year fixed effects interacted with fixed effects for birth-cohort, decile of wealth, educational level, partner, gender and having children, each measured in 1991, as well as person fixed effects and municipality-year fixed effects. Standard errors are clustered at the individual level and are reported in parentheses.

Table 3 Effects of Mortgage Reform on Wages and Employment

Dependent variable	(1) Normalized earnings		(2) Log wage		(3) Employment rate		(4) Normalized earnings		(5) Log wage		(6) Employment rate	
	A. Treatment: Dummy for (ETV91>0.2)											
Post*1(ETV91>0.2)	0.0072	***	0.0044	***	0.0008		0.0009		-0.0046	***	0.0018	***
	(0.0023)		(0.0017)		(0.0007)		(0.0022)		(0.0016)		(0.0007)	
Post*1(ETV91>0.2)* Low Liquidity							0.0155	***	0.0209	***	-0.0030	***
							(0.0052)		(0.0022)		(0.0009)	
	B. Treatment: ETV91											
Post*ETV91	0.0130	***	0.0107	***	0.0019	**	0.0030		-0.0074	***	0.0033	***
	(0.0031)		(0.0021)		(0.0009)		(0.0037)		(0.0027)		(0.0011)	
Post*ETV91* Low Liquidity							0.0243	***	0.0445	***	-0.0041	***
							(0.0065)		(0.0036)		(0.0016)	
Person FE	✔		✔		✔		✔		✔		✔	
Municipality*year FE	✔		✔		✔		✔		✔		✔	
Observables*year FE	✔		✔		✔		✔		✔		✔	
Number of observations	6,819,246		6,178,846		6,819,246		6,819,246		6,178,846		6,819,246	

Notes: (* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$) This table reports estimates from OLS regressions (equation (4) and (5)). Normalized earnings are annual earnings divided by the average annual earnings from 1988 to 1996, which takes into account individuals with zero earnings. Employment rate is an indicator variable which equals one if the wage income is positive. The main right-hand-side variables are equity to value ratio in 1991, ETV interacted with an indicator for the post mortgage reform period, and interactions of ETV, post-reform-period dummy, and an indicator variable which equals one if the individual has liquid assets less than one month's disposable income in 1991. Control variables include year fixed effects interacted with fixed effects for birth-cohort, decile of wealth, educational level, partner, gender and having children, each measured in 1991, as well as person fixed effects and municipality-year fixed effects. Standard errors are clustered at the individual level and are reported in parentheses.

Table 4 Heterogeneity of Wage Effects by Individual Covariates

	(1)		(2)		(3)		(4)		(5)		(6)		(7)	
	Dependent variable: earnings normalized by average annual earnings 1988-1996													
	Basic Education		Vocational Education		Higher Education		Male		Female		Age<40		Age>=40	
Post*1(ETV91>0.2)	0.0077		0.0020		-0.0045		-0.0033		0.0040		-0.0046	*	0.0033	
	(0.0055)		(0.0029)		(0.0033)		(0.0024)		(0.0043)		(0.0028)		(0.0039)	
Post*1(ETV91>0.2)* Low Liquidity	0.0300	**	0.0069		0.0164	**	0.0100	*	0.0515	***	0.0144	*	0.0353	***
	(0.0118)		(0.0070)		(0.0075)		(0.0056)		(0.0123)		(0.0080)		(0.0065)	
Person FE	✔		✔		✔		✔		✔		✔		✔	
Municipality*year FE	✔		✔		✔		✔		✔		✔		✔	
Observables*year FE	✔		✔		✔		✔		✔		✔		✔	
Number of observations	2,008,718		3,021,472		1,704,596		4,276,165		2,281,393		3,118,932		3,438,639	

Notes: (* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$) This table reports estimates from OLS regressions (equation (5)) for each demographic group. Normalized earnings are annual earnings divided by the average annual earnings from 1988 to 1996, which takes into account individuals with zero earnings. All demographic characteristics are measured in 1991. The main right-hand-side variables are equity to value ratio in 1991, ETV interacted with an indicator for the post mortgage reform period, and interactions of ETV, post-reform-period dummy, and an indicator variable which equals one if the individual has liquid assets less than one month's disposable income in 1991. Control variables include year fixed effects interacted with fixed effects for birth-cohort, decile of wealth, educational level, partner, gender and having children, each measured in 1991, as well as person fixed effects and municipality-year fixed effects. Standard errors are clustered at the individual level and are reported in parentheses.

Table 5 Robustness to Self Employment

	(1)		(2)	(3)		(4)		(5)	
				(Excluding self-employed workers)					
Dependent variable	Self employment		Self employment	Normalized earnings		Log wage		Employment rate	
Post*1(ETV91>0.2)	0.0009	**	0.0008	-0.0026		-0.0060	***	0.0014	**
	(0.0004)		(0.0005)	(0.0020)		(0.0014)		(0.0006)	
Post*1(ETV91>0.2)* Low Liquidity			0.0009	0.0288	***	0.0440	***	-0.0008	
			(0.0010)	(0.0049)		(0.0032)		(0.0014)	
Person FE	✔		✔	✔		✔		✔	
Municipality*year FE	✔		✔	✔		✔		✔	
Observables*year FE	✔		✔	✔		✔		✔	
Number of observations	6,819,246		6,819,246	6,143,229		5,786,635		6,143,229	

Notes: (* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$) This table reports estimates from OLS regressions (equation (4) and (5)). In Column 1 and Column 2, the dependent variable is an indicator variable which takes the value of 1 if the individual is an entrepreneur in a given year. In Column 3 to 5 we exclude all individuals who were entrepreneurs at any time during 1988-1996. The main right-hand-side variables are equity to value ratio in 1991, ETV interacted with an indicator for the post mortgage reform period, and interactions of ETV, post-reform-period dummy, and an indicator variable which equals one if the individual has liquid assets less than one month's disposable income in 1991. Control variables include year fixed effects interacted with fixed effects for birth-cohort, decile of wealth, educational level, partner, gender and having children, each measured in 1991, as well as person fixed effects and municipality-year fixed effects. Standard errors are clustered at the individual level and are reported in parentheses.

Table 6 Placebo Test: Year 1988 to 1991

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	Normalized earnings	Log wage	Employment rate	Normalized earnings	Log wage	Employment rate
Post*1(ETV89>0.2)	0.0039	-0.0017	0.0010 **	0.0058 *	-0.0012	0.0013 *
	(0.0024)	(0.0013)	(0.0005)	(0.0030)	(0.0021)	(0.0007)
Post*1(ETV89>0.2)* Low Liquidity				-0.0029	0.0005	-0.0015
				(0.0050)	(0.0024)	(0.0010)
Person FE	✔	✔	✔	✔	✔	✔
Municipality*year FE	✔	✔	✔	✔	✔	✔
Observables*year FE	✔	✔	✔	✔	✔	✔
Number of observations	2,452,892	2,219,735	2,452,892	2,452,892	2,219,735	2,452,892

Notes: (* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$) This table reports estimates from placebo OLS regressions for the pre-reform period (1988-1991). Normalized earnings are annual earnings divided by the average annual earnings from 1988 to 1991, which takes into account individuals with zero earnings. Employment rate is an indicator variable which equals one if the wage income is positive. The main right-hand-side variables are equity to value ratio in 1989, ETV interacted with an indicator for the post-1990 period, and interactions of ETV, post-1990 dummy, and an indicator variable which equals one if the individual has liquid assets less than one month's disposable income in 1989. Control variables include year fixed effects interacted with fixed effects for birth-cohort, decile of wealth, educational level, partner, gender and having children, each measured in 1989, as well as person fixed effects and municipality-year fixed effects. Standard errors are clustered at the individual level and are reported in parentheses.

Table 7 Determinates of Equity Extraction

	Outcome variable is Extract={0,1}											
	(1)		(2)		(3)		(4)		(5)		(6)	
Income growth	-0.0347	***	-0.0227	***								
	(0.0017)		(0.0018)									
Income growth × Low liquidity in 1991			-0.0227	***								
			(0.0045)									
Unemployment					0.0028	***			0.0023	*		
					(0.0007)				(0.0014)			
Firm employment growth							-0.0032	***				
							(0.0008)					
Log earnings											-0.0037	***
											(0.0012)	
Low liquidity in 1991	0.0341	**	0.0346	**	0.0333	**	0.0335	**				
	(0.0005)		(0.0005)		(0.0005)		(0.0005)					
Municipality*Year FE	✔		✔		✔		✔		✔		✔	
Person FE									✔		✔	
No. of observations	2,196,158		2,196,158		2,196,158		2,196,158		2,196,158		2,196,158	

Notes: (* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$) This table reports estimates from regressions on propensity to borrow against housing equity (equation (8)). The dependent variable is an indicator variable for extracting housing equity. Individuals with low liquidity in 1991 are individuals who had liquid assets less than one month's disposable income in 1991. The regressions control for municipality-year fixed effects, and in Column 5 and 6 also control for individual fixed effects. Standard errors are clustered at the individual level and are reported in parentheses.

Table 8 Effects of Mortgage Reform on Unemployed Workers

	(1)		(2)		(3)		(4)	
Dependent variable	Unemployment duration		Unemployment duration		Replacement rate		Replacement rate	
1(ETV91>0.2)	0.0703	**	0.0409		0.0080		-0.0177	
	(0.0300)		(0.0348)		(0.0122)		(0.0146)	
1(ETV91>0.2)* Low Liquidity			0.1347	**			0.0674	***
			(0.0650)				(0.0249)	
Log wage before unemployment	-0.0523	***	-0.0623	***	-0.3243	***	-0.3326	***
	(0.0086)		(0.0087)		(0.0034)		(0.0035)	
Age dummies	✔		✔		✔		✔	
Municipality FE	✔		✔		✔		✔	
Cohort FE	✔		✔		✔		✔	
Number of observations	42,952		42,952		42,952		42,952	

Notes: (* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$) This table reports estimates from cross-sectional regressions on unemployed workers in 1991 (equation (9)). Unemployment duration is measured in years. The replacement rate is calculated as the reemployment wage divided by the average annual wage during three years before unemployment. The main right-hand-side variables are equity to value ratio in 1991 and ETV interacted an indicator for having liquid assets less than one month's disposable income in 1991. All regressions control for fixed effects of age, municipality and year of beginning unemployment, as well as the log wage before unemployment.

Table 9 Mechanisms of Wage Growth

Dependent variable	(1) Switch firm		(2) Switch city		(3) AKM Firm FE		(4) Coworker wage		(5) Top position		(6) Log wage	
Post*1(ETV91>0.2)	0.0024	*	0.0223	***	0.0008	**	0.0010	*	-0.0033		-0.0010	
	(0.0013)		(0.0006)		(0.0004)		(0.0006)		(0.0024)		(0.0007)	
Post*1(ETV91>0.2)* Low Liquidity	0.0121	***	0.0083	***	0.0012	**	0.0017	**	0.0100	*	0.0049	***
	(0.0020)		(0.0009)		(0.0006)		(0.0009)		(0.0056)		(0.0009)	
Person FE	✔		✔		✔		✔		✔		✔	
Municipality*year FE	✔		✔		✔		✔		✔		✔	
Observables*year FE	✔		✔		✔		✔		✔		✔	
Person*firm (job) FE											✔	
Mean of Dep. Var.	0.164		0.055		0.496		11.71		0.093		11.71	
Number of observations	5,445,480		5,445,480		5,445,480		5,445,480		5,445,480		5,445,480	

Notes: (* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$) This table reports estimates from OLS regressions (equation (5)). In Column 1 dependent variable is an indicator variable for changing employer. In Column 2 dependent variable is an indicator variable for changing municipality. In Column 3 dependent variable is the AKM firm fixed effect of the employer, which is estimated from two-way fixed effect regressions with worker FE and firm FE. In Column 4 dependent variable is average wage of coworkers. In Column 5 dependent variable is an indicator variable for working in a top position, which is identified by the job hierarchy code. The main right-hand-side variables are equity to value ratio in 1991, ETV interacted with an indicator for the post mortgage reform period, and interactions of ETV, post-reform-period dummy, and an indicator variable which equals one if the individual has liquid assets less than one month's disposable income in 1991. Control variables include year fixed effects interacted with fixed effects for birth-cohort, decile of wealth, educational level, partner, gender and having children, each measured in 1991, as well as person fixed effects and municipality-year fixed effects. In Column 6 the regression also includes person-establishment fixed effects. Standard errors are clustered at the individual level and are reported in parentheses.

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Chapter 3

Corporate R&D Spillovers and Investment in the Innovation Network

3.1 Introduction

Knowledge spillover from corporate investment on research and development (R&D) is important for understanding innovation and technological change, but spillovers are hard to measure empirically. Moreover, new innovations often build on past achievements in other areas, as the descriptive phrase "standing on the shoulders of giants" suggests. Firms that innovate in more "upstream" and fundamental technological areas would create larger knowledge spillovers to other firms in the society than firms in "downstream" technological areas. Nevertheless, the knowledge spillovers across technology areas has been largely ignored in previous studies. This paper delivers a framework in which it is possible to measure how important technology spillovers from upstream to downstream technological fields are for firm outcomes including patenting, productivity, investment and market value, and study which firms create the largest knowledge spillovers.

We build on the recent work by Bloom, Schankerman, and Van Reenen (2013, BSV in short), which develops a general framework incorporating two kinds of spillovers: a positive technology spillover and a negative business stealing effect from product market rivals. They use a Jaffe metric and a more general Mahalanobis metric to measure the closeness between firms in technology space and product space based on technology distribution of firms' patents and industry composition of firm sales. An important insight from the paper is that the distances in the technology space and product space is uncorrelated, which creates enough variation to identify the two spillover effects separately.

Both the Jaffe metric and Mahalanobis metric do not consider an important aspect of knowledge spillovers: innovation is a cumulative process and new progress builds on past achievements. Acemoglu, Akcigit and Kerr (2014, AAK in short) shows that the knowledge spillovers in the form of "standing on the shoulders of giants" is huge: about half of the citations in patents are from outside the technology class, and number of patents in upstream technology classes has strong predictive power of the future innovation in downstream technology classes.

In this paper we develop a new "network measure" to measure the technology spillovers between firms, and the distance between different technology classes is based on citation patterns in AAK. We show that when using the network measure to calculate R&D spillovers, the spillovers from firms that are technologically connected has a positive and significant impact on firm's *R&D* and *patenting*, while spillovers have no significant impact on R&D and smaller impact on patenting when using other measures. This confirms our hypothesis that knowledge spillovers from upstream areas have a significant impact on downstream innovations. However, the R&D spillovers from firms close in technology space has the largest impact on firm's *productivity* and *market value* when we use the Mahalanobis distance measure to calculate spillovers. One explanation is that the network measure measures the pool of knowledge generating "innovation" spillovers which leads to higher innovation output due to a larger knowledge stock to build on, and the Mahalanobis measure measures the pool of knowledge generating "production" spillovers which leads to higher productivity due to technology adoption.

To examine the robustness of our finding, we first separately measure the impacts of spillovers within and across technology fields and show that spillovers across technology fields have a significant impact on firm outcomes. Second, we use patent stock rather than R&D stock as an alternative measure of firms' knowledge stock. We also study the effects of R&D spillovers over time and show that effects on patenting, market value and productivity persist over a long period.

Finally, we use our measure to study which firms generate the largest spillovers. The results are completely the opposite to BSV: while they find small firms operate in technological niches, we find they are more likely to operate in more upstreamĂİ technology fields. This result suggests that the social return to R&D may be a lot higher than private R&D for small and young firms, and justifies providing R&D subsidies to these firms. In general, since the network measure takes into account the fundamental and upstream research done by firms which provides basis for later on innovations, it would allow us to better characterize the knowledge spillovers that leads to new innovations, and compare private vs. social returns to R&D.

The paper is organized as follows. Section 2 describes the network measure of technological distance between firms. Section 3 outlines a simple model of innovation network and the main empirical specifications. Section 4 describes the data we use. Section 5 and Section 6 presents the empirical results and some specification tests. In Section 7 we use the measure to identify which firms generate and receive the largest spillovers, and we conclude in Section 8.

3.2 The Network Measure of Technological Proximity

The conceptual framework for technology spillovers was first introduced by Griliches in 1979. In his seminal work, technological spillovers arise through a common pool of technological knowledge. Firms make use of the pool of knowledge as an additional input in their production functions. The R&D of one firm both affects the firm's own productivity and indirectly affects the productivity of other firms through the pool of knowledge. Jaffe (1986) introduced a technological distance between firms in order to capture the likelihood that technological spillovers would arise between firms. Two firms are more likely to benefit from R&D of each other if they are technologically close. Specifically, the spillover from firm j to firm i is:

$$SPILLOVER^{J}_{ij} = TECH^{J}_{ij} n_j$$

$$\mathit{TECH}_{ij}^{J} = n_i F_i F_j' = \sum_{\tau} \sum_{\tau'} n_i F_{i\tau} F_{j\tau'}$$

where n_i is number of patents by firm i. The $1 \times Y$ vector $F_j = (F_{j1}, ..., F_{j\tau})$ is defined as in BSV, where $F_{j\tau} = \frac{n_{i\tau}}{n_i}$ is the share of patents in technology class τ of all firm i's patents. If we think of n_i as a proxy for the number of scientists in firm i, the measure of spillover is exactly the number of encounters between scientists from firm i and j [1].

The BSV also extends the Jaffe measure to Mahalanobis measure, which considers technological spillovers between closely related fields.

$$\mathit{TECH}_{ij}^{M} = n_i F_i \Omega F_j' = \sum_{\tau} \sum_{\tau'} n_i F_{i\tau} \omega_{\tau\tau'} F_{j\tau'}$$

where $\Omega = [\omega_{\tau\tau'}]$ denotes the $Y \times Y$ matrix that describes the probability of knowledge transfer when two scientists from fields τ and τ' meet, and is based on the extent of co-location of patenting across technology fields.

Both the Jaffe measure and Mahalanobis measure neglects the technology spillovers in the form of "standing on the shoulders of giants", meaning that firms innovating in more upstream and more fundamental technology fields creates larger knowledge spillovers to the rest of the firms in the economy. By considering spillovers from more upstream fields to more downstream fields, my methodology also captures asymmetries in spillovers, which are ruled out by construction in distance measures, like Jaffe and Mahalanobis. Allowing for asymmetric spillover effects is important since, as stated in Syverson (2011) page 349: "Firms are likely to attempt to emulate productivity leaders in their own and closely related industries". That is, spillovers are likely to originate in high productive firms and cascade down to less productive firms.

In particular, we develop a measure of spillover from firm j to firm i based on patent citations

[1] The measure of spillover we consider in this paper is the "exposure" measure in BSV, while the traditional Jaffe measure normalizes the uncentered covariance on the standard deviation of the share vectors F. Both measures lead to very similar results as shown in BSV.

network:

$$TECH_{ij}^{N} = n_i F_i C F_j' = \sum_{\tau} \sum_{\tau'} n_i F_{i\tau} C_{\tau\tau'} F_{j\tau'} \tag{3.1}$$

The $Y \times Y$ matrix C represents the patent citation network in AAK. The element $C_{\tau\tau'}$ is the share of patents in class τ that cites from a patent in class τ', which is the average number of times a patent in technology class τ' is cited by patents in technology class τ within 10 years of being granted divided by number of patents in technology class τ:

$$C_{\tau\tau'} = \sum_{a=1}^{10} \frac{\text{Citations}_{\tau\to\tau',a}}{\text{Patent}_{\tau'}\text{Patent}_{\tau}}$$

The measure $TECH_{ij}^{N}$ has an intuitive interpretation: it is the average number of times each patent in firm j gets cited by patents from firm i given the technology class distribution of the two firms. In particular, $F_{j\tau'}$ is the probability of firm j patenting in a particular technology class τ', and $C_{\tau\tau'}$ is the average share of patents in class τ that cites from this patent given its class τ', and $n_i F_{i\tau}$ is the number of patents in class τ from firm i.

We can then compute the pool of technology spillover to firm i in year t:

$$SPILLTECH_{it}^{N} = \sum_{j\neq i} TECH_{ij} G_{jt}, \tag{3.2}$$

where G_{jt} is the stock of R&D. The number of patent citations from firm i to firm j thus approximates the knowledge spillovers from firm j's R&D to firm i. This network measure will be the baseline measure for technology spillovers in our research. Intuitively, let's say firm j invests 1 million in R&D and produces 100 patents, and these 100 patents are cited by 1 patent from firm i on average, then our measure says that the 1 million R&D investment of firm j generates a technology spillover of 1 patent to firm i.

We can also decompose within-class and cross-class spillovers by writing $C = C^D + C^H$, where C^D is a diagonal matrix, and C^H is a matrix with all diagonal elements equal to zero. Then the network measure can be written as: $TECH_{ij}^{N} = n_i F_i C^D F_j' + n_i F_i C^H F_j'$.

There are several caveats with our construction of the network measure of proximity between firms. First, patent citations may not reflect actual knowledge flows. Sometimes citations are added by patent examinees after the patent has been applied, and our measure would overestimate cross-technology field spillovers if citing other fields are more likely to be added afterwards. Second, the network measure does not deal with measurement errors in assigning technology fields. If patent office examiners erroneously allocate patents in closely related fields, the Mahalanobis measure corrects it by recognizing the fields as close to each other, but the network measure won't recognize them if there are no patent citations between those fields. Finally, this measure does not differentiate between long-term and short-term spillovers. In particular, the R&D stock G_t comprises mostly of recent R&D investments when we choose a 15% depreciation rate, but some spillovers may take effect after more than 5 years, especially if across technology fields, and our measure of $SPILLTECH_{it}$ may overweight the R&D investments in more recent years.

3.3 Empirical Strategy

3.3.1 A simple model of innovation network

Suppose firm i has a product market competitor firm m_i and a technology neighbor τ_i . Firm i's profit is given by $\pi(x_i, x_m, k_i)$, where x_i is output or quantity, x_m is the output of quantity chosen by the firm's competitor firm m_i, and k_i is knowledge stock (or innovation output). Since in the last stage of each period, each firm chooses price and quantities given each other's knowledge stock, we can write profit as $\Pi(k_i, k_m, k_\tau)$. We generalize the function to also include the innovation output of firm τ to incorporate knowledge spillovers to firm i's productivity without affecting firm i's innovation, for example in the case of firm i's technology and firm τ's technology being complements in production.

At each period t, firm i produces its innovation with its own R&D, and spillovers from the

R&D of firms to which it is close in the technology space:

$$k_i^t = \phi(r_i^t, k_i^{t-1}, k_\tau^{t-1}) \tag{3.3}$$

The k_τ^{t-1} in this function reflects the pool of knowledge that the firm builds its innovation on. The knowledge production function $\phi(.)$ is non-decreasing and concave in each argument. Firm i solves the following maximization problem:

$$\max_{r_i^t} V_i^t = \tilde{\Pi}(\phi(r_i^t, k_i^{t-1}, k_\tau^{t-1}), k_m^{t-1}, k_\tau^{t-1}) - c(r_i^t) \tag{3.4}$$

The function $\tilde{\Pi}$ is the expectation of firm's profits given the knowledge stock of product market rivals and technology space neighbors at time $t-1$.

In this stylized model there are only three firms, but we can easily generalize to the case with many firms. Suppose T is a matrix representing the network of interactions of all the firms in the technology space, and S is a matrix representing the network of interactions in the product space (both matrix has all diagonal elements equal to zero), then we can write:

$$k_\tau^t = T_i \mathbf{k}^t = \sum_{j \neq i} T_{ij} k_j^t, \quad k_m^t = S_i \mathbf{k}^t = \sum_{j \neq i} S_{ij} k_j^t$$

and

$$r_\tau^t = T_i' \mathbf{r}^t = \sum_{j \neq i} T_{ij}' r_j^t$$

T' represents the innovation network, which may be different from T. As we've discussed in Section 2.2, T can be approximated by the Mahalanobis measure, and T' can be approximated by the network measure.

The first order condition of the maximization problem in (4) is: $\tilde{\Pi}_1 \phi_1 = c'(r_0^t)$, and the solution is:

$$r_i^{t*} = R(k_i^{t-1}, k_\tau^{t-1}, k_m^{t-1}) \tag{3.5}$$

Plug this into (3) and we get:

$$k_i^t = \phi(R(k_i^{t-1}, k_\tau^{t-1}, k_m^{t-1}), k_i^{t-1}, k_\tau^{t-1}) \tag{3.6}$$

The first k_τ^{t-1} in the above equation reflects endogenous effect of the innovation of technologically close firms on firm i's R&D: it can arise from the non-linearity of the profit function, or from strategic complementarity (or substitutability)[2] between R&D of firms with similar technologies. The sign of this effect is ambiguous. The second k_τ^{t-1} reflects technology spillover, which has a positive impact on firm i's innovation output. The k_m^t in the equation is the strategic effect of innovation output of product rivals: it has a positive effect if firm i's innovation and its competitor's innovation is strategic complements, and vice versa.

To be able to estimate the model, we assume both $\phi(.)$ and $\Pi(.)$ are linear. As shown in some theoretical literature on networks, many conclusions in the linear case can be generalized to nonlinear cases as well. In particular, suppose that the knowledge production function is:

$$k_i^t = \phi(r_i^t, k_i^{t-1}, k_\tau^{t-1}) = (1-\delta)k_i^{t-1} + \alpha r_i^t + \sigma_i k_\tau^{t-1} + \gamma r_i^t k_\tau^{t-1} + \epsilon_i^t \tag{3.7}$$

where δ is the depreciation of knowledge stock, α is the return to R&D, σ_i is the direct spillover from R&D of technology neighbors, and γ reflects the strategic complementarity or substitutability of R&D investments between firms that are close in technology space; $\gamma > 0$ if firms can innovate more productively when building on a larger pool of knowledge. The payoff function is:

$$\max_{r_i} V_i^t = \tilde{\Pi}(k_i^t, k_m^{t-1}, k_\tau^{t-1}) - c(r_i) = (\beta_i k_i^t + \mu k_i^t k_m^{t-1} + \eta k_m^{t-1} + \psi k_\tau^{t-1} + \xi_i^t r_i) - \frac{1}{2}\theta r_i^2 \tag{3.8}$$

where $\mu > 0$ in the case of strategic complements, and η is the direct effect of knowledge stock of product market rivals on firm's profits. ψ reflects that firms produce more efficiently when the

[2]Examples of strategic complements include patent races and complementarity between closely related technologies, and examples of strategic substitutability include decreasing returns from innovating in a field and substitutability between similar technologies.

knowledge stock of their technology neighbors is larger. θ is the R&D cost parameter and may be affected by R&D tax credits. Solving the FOC and the optimal R&D is:

$$r_i^t = \frac{\alpha}{\theta}\beta_i + \frac{1}{\theta}\beta_i\gamma k_\tau^{t-1} + \frac{1}{\theta}(\alpha + \gamma k_\tau^{t-1})\mu k_m^t + \frac{1}{\theta}\xi_i^t \tag{3.9}$$

For simplicity we assume $\mu \approx 0$, so there is no strategic effects of R&D by product market rivals. We shall verify this in the empirical section. Plug into (7) and we get:

$$\begin{aligned} k_i^t - (1-\delta)k_i^{t-1} &= \alpha r_i^t + \sigma_i k_\tau^{t-1} + \gamma r_i^t k_\tau^{t-1} + \epsilon_i^t \\ &= \frac{\theta}{\beta} r_i^{t2} + \sigma_i k_\tau^{t-1} + \epsilon_i^t \qquad (3.10) \\ &= \frac{\alpha^2}{\theta}\beta_i + (\frac{2\alpha}{\theta}\beta_i\gamma + \sigma_i)k_\tau^{t-1} + \frac{1}{\theta}\beta_i\gamma^2 k_\tau^{t-1\,2} + \tilde{\epsilon}_i^t \qquad (3.11) \end{aligned}$$

Finally, the profit is:

$$\pi_i^t = \theta r_i^{t2} + \beta_i(1-\delta)k_i^{t-1} + (\sigma_i\beta_i + \psi)k_\tau^{t-1} + \eta k_m^{t-1} + \varepsilon_i^t \tag{3.12}$$

One difficulty of empirically estimating these equations is how to measure knowledge R&D r^t and stock k^t. In practice R&D may take several years to take effect, so instead of current-period R&D, a better approximation would be R&D stock, which is the sum of R&D in recent years. For k^t, BSV approximates knowledge stock using the stock of R&D which is the sum of R&D investments and depreciates over time. This may lead to measurement errors especially for k_m^t: for example, the product market competitor firm m may invest little in R&D but has huge innovation outputs due to spillovers from his technology neighbors, then using R&D stock would underestimate k_m^t. As an alternative I also used the stock of patents as the knowledge stock of a firm. Using patent stock can be problematic too: there are many innovations that are not patented and cannot be captured by patents.

3.3.2 Empirical specifications

The generic equation we are estimating is:

$$\ln Q_{it} = \beta_1 G_{it-1} + \beta_2 \ln \mathit{SPILLTECH}_{it-1} + \beta_3 \ln \mathit{SPILLSIC}_{it-1} + \beta_4 X_{it} + u_{it} \tag{3.13}$$

where G_t is the stock of R&D, and is a proxy for the stock of knowledge k_t. We also use firms' patent stock to proxy for knowledge stock in robustness checks. The spillovers from technology neighbors *SPILLTECH* is defined in equation (2), and spillovers from product rivals is defined similarly using the Jaffe measure:

$$\mathit{SPILLSIC}_{it} = \sum_{j \neq i} \mathit{SIC}_{ij} G_{jt} = \sum_{j \neq i} n_i S_i S_j' G_{jt} \tag{3.14}$$

X_{it} is a vector of controls. To deal with unobserved heterogeneity, we include firm fixed effects (η_i) and year fixed effects (τ_t) in all regressions except patent equations, and allow the error term to be heteroskedastic and serially correlated.

The R&D on the right hand side may be endogenous due to transitory shocks. To address this concern, we follow BSV and use tax-induced changes to the user cost of R&D capital as instrument. The user cost of R&D differs across firms for two reasons: first, different states have different levels of R&D tax credits and corporation tax, which will differentially affect firms depending on their cross-state distribution of R&D activity; second, it also has a firm-specific component, in part because the definition of what qualifies as allowable R&D for tax purposes depends on a firm-specific "base". We use these tax policy instruments to predict R&D, and then use these predicted values to calculate predicted spillovers according to equations (2) and (14). The spillover terms are being instrumented by the values of other firms' tax prices, weighted by their distance in technology and product market space.[3]

[3]The Appendix of BSV provides details of construction of the instrument, and shows that the R&D tax credits are exogenous to changes in economic conditions.

First, we consider the R&D equation where the left hand side variable is R&D intensity:

$$\ln\left(\frac{R}{Y}\right)_{it} = \alpha_2 \ln \mathit{SPILLTECH}_{it-1} + \alpha_3 \ln \mathit{SPILLSIC}_{it-1} + \alpha_4 X_{it}^R + \eta_i^R + \tau_t^R + \nu_{it}^R \quad (3.15)$$

This corresponds to equation (9) in our model. Coefficient α_2 (α_3) is positive when R&D of firms close in technology (product) space is strategic complements, and negative when they are strategic substitutes. The user cost of R&D capital is absorbed in the fixed effects and time dummies. To mitigate endogeneity, we lag the key right hand side variables by one year. We also examine specifications that relax the constant returns assumption, using $\ln R$ as the dependent variable and including $\ln Y$ on the right hand side of the equation.

We then estimate the patent equation using a Negative Binomia Model:

$$P_{it} = \exp(\lambda_1 G_{it-1} + \lambda_2 \ln \mathit{SPILLTECH}_{it-1} + \lambda_3 \ln \mathit{SPILLSIC}_{it-1} + \lambda_4 X_{it}^P + \tau_t^P + \nu_{it}^P) \quad (3.16)$$

This corresponds to equation (10) in our model. Conditional on own R&D, the coefficient λ_2 captures the spillover effects, and λ_3 should be close to zero. To control for firm fixed effects, we include industry fixed effects and "pre-sample mean scaling" to control for fixed effects (it's computationally demanding to estimate a negative binomial model with firm dummies). We used a long pre-sample history (from 1970 to at least 1980) of patenting behavior to construct the pre-sample average. This can then be used as an initial condition to proxy for unobserved heterogeneity under the assumption that the first moments of all the observables are stationary.

The market value equation is a linearization of the value function introduced by Griliches (1981) augmented with our spillover terms:

$$\ln\left(\frac{V}{A}\right)_{it} = \ln\left(1 + \gamma_1 \left(\frac{G}{A}\right)_{it-1}\right) + \gamma_2 \ln \mathit{SPILLTECH}_{it-1} + \gamma_3 \ln \mathit{SPILLSIC}_{it-1} + \gamma_4 X_{it}^V + \eta_i^V + \tau_t^V + \nu_{it}^V \quad (3.17)$$

where V is the market value of a firm, A is the stock of non-R&D assets, G is the R&D stock. The term $\ln\left(1 + \gamma_1 \left(\frac{G}{A}\right)_{it-1}\right)$ is approximated by a sixth order series expansion.

Finally, the productivity equation is:

$$\ln Y_{it} = \varphi_1 \ln G_{it-1} + \varphi_2 \ln \mathit{SPILLTECH}_{it-1} + \varphi_3 \ln \mathit{SPILLSIC}_{it-1} + \varphi_4 X_{it}^{Y} + \eta_i^{Y} + \tau_t^{Y} + \nu_{it}^{Y} \tag{3.18}$$

This roughly corresponds to equation (12) in our model. The coefficient φ_2 captures two spillover effects: a firm builds on the knowledge of other firms to innovate more and expand its knowledge stock; and it also produces more efficiently using the pool of knowledge as an additional input in their production functions. As in patent equation, conditional on own R&D, R&D of product rivals should have no impact on productivity. However, in practice, we measure output as "real sales" – firm sales divided by an industry price index. Because we do not have information on firm-specific prices, this induces measurement error. If R&D by product market rivals depresses own revenues, the coefficient on *SPILLSIC* may be negative.

3.4 Data

We use firm-level data from North America CompuStat from 1980 to 2001. We then use the matching built by Hall et al. to match patents from USPTO to CompuStat firms (See NBER data archive and Hall, Jaffe, and Trajtenberg (2001)). They contain all the patents granted between January 1963 and December 1999, and all citations made to these patents between 1975 and 1999. This matching from USPTO patents to CompuStat is the best so far, but it still contains many type 1 and type 2 error given the difficulty of cleaning and disambiguating assignee names in patent applications.

The book value of capital is the net stock of property, plant, and equipment. R&D is used to create R&D capital stocks calculated using a perpetual inventory method with a 15% depreciation rate. So the R&D stock, G, in year t is $G_t = R_t + (1 - \delta)G_{t-1}$,where R is the R&D flow expenditure in year t and $\delta = 0.15$. We use deflated sales as our output measure, and industry price deflators were taken from Bartelsman, Becker, and Gray (2000) until 1996 and then the BEA four digit NAICS Shipment Price Deflators thereafter. For Tobin's Q, firm value is the sum of the values of common stock, preferred stock, and total debt net of current assets. The book value of capital includes net plant, property and equipment, inventories, investments in unconsolidated subsidiaries, and intangibles other than R&D.

Since our technological distance requires information on patenting, we exclude firms that have no patents between 1963 and 1999, leaving an unbalanced panel of 715 firms. I also exclude patents in three technology classes (1, 395 and 520) that do not exist in the innovation network. The share vectors F is based on the share of patents of each firm over the period 1970 to 1999 in 423 technology classes.[4]

The patent citation matrix (C in equation (1)) is constructed using the citations data used in AAK, which consists of all the patents granted between 1975 and 1984. Since it's different from the time period which we use to construct distance measures, it will induce some measurement errors. Nevertheless, the measurement errors tend to be small as AAK found that patent citation networks are quite stable over time.

Figure 1 compares the network measure with the Jaffe and Mahalanobis measures for $TECH_{ij}$. For each pair of firm, the left figure plots the network measure along with the Jaffe exposure measure, and the right figure plots the network measure along with the Mahalanobis exposure measure. The correlation of network measure with Jaffe measure is 0.79, and with Mahalanobis measure is 0.82. (The correlation between Jaffe and Mahalanobis is 0.90.) This suggests that our network measure is very different from the two measures, and the knowledge spillovers across technology fields though the innovation network is nontrivial.

3.5 Empirical results

3.5.1 R&D equation

Table 1 presents the results for the R&D equation (15). In all specifications we control for log total sales weighted by by SIC matrix and its one-period lag. In Column 1, when we do not include

[4]BSV rounded the percent shares to the nearest integer to reduce memory size, but I didn't do the approximation.

firm fixed effects, both technology spillover *SPILLTECH* and product market spillover *SPILLSIC* have large and significant positive impact on R&D. In Column 2 we include firm fixed effects, and both coefficients get smaller, but still positive and significant. The Hausman test rejects the null of random effects versus fixed effects (p-value<0.001). The positive effects indicate that R&D among product rivals and technology neighbors are strategic complements. In Column 3 we use the Mahalanobis measure to calculate technological distance, and in Column 4 we use the network measure defined in Section 2 to calculate technological distance. In both columns *SPILLSIC* remains positive, and a 10 percent increase in R&D of product rivals is associated with 0.6 to 1 percent increase in the firm's R&D intensity; the coefficient on *SPILLTECH* is mildly negative for Mahalanobis measure, and positive for network measure. To relax the constant returns assumption, we use ln(R&D) as the dependent variable and added ln(Sales) to the right hand side, and get very similar results.[5]

In Columns 5 to 7, we treat *SPILLTECH* and *SPILLSIC* as endogenous and use R&D tax credits as instruments. The first stage for both variables are strong. In all three columns, the coefficients on *SPILLTECH* is positive, and is significant for Jaffe and network measures, suggesting that own and technology neighbor's R&D are strategic complements. OLS are biased toward zero; one reason is measurement errors, and mobility of scientists and engineers across firms may result in negative correlation between R&D of firms that innovate in similar technological fields. In the last column, when we are using the network measure, R&D of technology neighbors has very large positive impact on own R&D: a 10 percent increase in *SPILLTECH* leads to 3.7% increase in own R&D. This suggests that own R&D responds strongly to the knowledge stock of "upstream" firms, and a firm invests more on R&D when there is a larger pool of knowledge to build on.

Throughout the last three columns, the coefficient on *SPILLSIC* is negative and insignificant, suggesting that there is very weak strategic effects between own and product market rival's R&D, which supports the assumption that $\mu \approx 0$ in our model. OLS estimates are biased upwards due to common shocks that affect firms in the same industry.

[5]For example, when using Jaffe measure, the coefficient (standard error) of *SPILLTECH* is 0.099(0.066), and of *SPILLSIC* is 0.088(0.035).

3.5.2 Patent equation

Table 2 presents the results for the citation-weighted patents equation. The first column shows that spillovers from technology neighbors have a positive and significant impact on patenting, and spillovers from product rivals, which theoretically have zero impact on patenting, has a positive but much smaller coefficient.

In Columns 2 to 4, we control for firm fixed effects by using the Blundell, Griffith, and Van Reenen (1999) method of conditioning on the pre-sample, citation-weighted patents. This method relaxes the strict exogeneity assumption underlying the approach of Hausman, Hall, and Griliches (1984). We also used the approach of Hausman, Hall, and Griliches (1984) to control for firm fixed effects in negative binomial regressions, and the results are similar.[6] For all three measures of technological proximity, the coefficient on *SPILLTECH* is positive and significant, and the coefficient on *SPILLSIC* is close to zero, which is consistent with the theory.

In the last three columns, we treat R&D spillovers as endogenous, and the coefficients don't change much. In all specifications we used citation-weighted patents as the dependent variable, and the results are roughly similar if we use unweighted patent counts: for Jaffe measure, the coefficient on *SPILLTECH* is 0.488 (standard error=0.040), and on *SPILLSIC* is 0.069 (standard error=0.018), which is close to the results in Column 2.

In Column 4 and Column 7, when we use network measure of technological proximity between firms, we get larger effects of *SPILLTECH* than using the other two measures. This is most likely due to smaller measurement errors, since by incorporating the R&D spillovers from "upstream" technological fields, we get a more precise measure of R&D spillovers from technology neighbor firms. In an unreported regression, when we include all three measures as regressors, only the coefficient on *SPILLTECH* using the network measure is positive and significant.[7]

[6]The coefficient (standard error) on *SPILLTECH* is 0.259(0.021), and on *SPILLSIC* is -0.004(0.012).

[7]The coefficient (standard error) of *SPILLTECH* constructed using Jaffe, Mahalanobis, and network measure is 0.154(0.128), 0.160(0.170), and 0.519(0.092).

3.5.3 Market value equation

Table 3 shows the results of the market value equation. In Column 1, without the firm fixed effects, both *SPILLTECH* and *SPILLSIC* have a positive effect on firm's market value. The sign of coefficient on *SPILLSIC* turned negative but insignificant once we control for firm fixed effects in Column 2. The Hausman test rejects the null of random effects against fixed effects with p-value equal to 0.058. The R&D spillovers from technologically related firms is positive and significant: a 10% increase in *SPILLTECH* is associated with about 1.7% increase in market value. In Column 3, we estimated fixed effects regression using the Mahalanobis distance measure, and the coefficient on *SPILLTECH* rises substantially. This suggests that the Mahalanobis measure reduces attenuation bias by more accurately weighting the distance between technology fields. In Column 4 we used the network distance measure, and results are similar to the Jaffe distance measure. Columns 5 to 7 present the results of the 2SLS regressions using R&D tax credits as instruments. The coefficients on *SPILLTECH* are positive and significant, and have larger magnitudes than OLS regressions. Coefficients are similar for all three distance measures, and largest for the Mahalanobis measure.

Throughout the regressions, the coefficient on *SPILLSIC* is negative, but the product market rivalry effects of R&D on market value is not statistically significant. The third row shows that higher R&D investments increases a firm's market value. A 10 percent increase in own R&D stock increases market value by 3 percent, as compared to a 5 to 7 percent increase in market value caused by a 10 percent increase in R&D spillovers from technologically close firms.

3.5.4 Productivity equation

Finally, Table 4 summarizes the results for the productivity equation. In Column 1, without firm fixed effects, both *SPILLTECH* and *SPILLSIC* have negative coefficients. In Column 2, when we control for firm fixed effects, *SPILLTECH* has a positive effect on productivity, and *SPILLSIC* has nearly zero effect on productivity, which is consistent with the theory. The Hausman test again rejects random effects with p-value less than 0.001.

In Column 3 we use the Mahalanobis distance measure, and in Column 4 we use the network distance measure. As in market value equations, the coefficient on *SPILLTECH* is the largest when we use the Mahalanobis measure.

In the last three columns, we treat the R&D spillovers as endogenous, and the results don't change much. Notably, the coefficient on *SPILLTECH* is no longer significant when we use the network distance measure and instrument for the R&D spillovers.

3.5.5 Summary of empirical results

In Table 1 to Table 4 we redo the empirical analysis of BSV using the Jaffe and Mahalanobis exposure distance measure and the network distance measure constructed in Section 2. The results using Jaffe and Mahalanobis exposure measures are similar to the results in BSV which uses Jaffe and Mahalanobis correlation measures. The results are consistent with our model: higher R&D by firms close in technology space is associated with higher patenting, market value and productivity, while higher R&D by firms close in product space (weakly) diminishes market value and has no effect on patenting or productivity. Own R&D is strategic complements with R&D by firms operating in similar or "upstream" technology fields, and is neither strategic substitutes nor strategic complements with R&D by product market rivals.

In R&D and patent equations, the coefficient on *SPILLTECH* is the largest when using the network distance measure; in market value and productivity equations, the coefficient on *SPILLTECH* is the largest when using the Mahalanobis measure. Higher R&D of firms operating in upstream technological fields increases a firm's own R&D and patenting, but has much smaller effects on productivity or market value.

These can be interpreted using our model. In the model there are two kinds of spillovers from technologically connected firms: one is "innovation" spillover (σ_i and γ in equation (7)), which leads to higher innovation output due to knowledge accumulation; the other is "production" spillover (ψ in equation (8)), which leads to higher productivity due to technology adoption (note that the "innovation spillover" γ does not appear directly in productivity equation (12)). The pool of knowledge generating the "innovation" spillover is more precisely measured by network distance measure, while the pool of knowledge generating the "production" spillover is better characterized by the Mahalanobis distance measure. For example, communications is an upstream technology field of computer technology, and R&D by AT&T would lead to higher patenting of a firm producing computers, but may not have direct effects on productivity of these firms. On the other hand, suppose technology A and B are complements in production, R&D of firm 1 in technology A leads to higher productivity of firm 2 which innovates in the technology B. When calculating *SPILLTECH* for firm 2, Mahalanobis measure would take into account firm 1's R&D, which would be ignored by network measure if there is no patent citations from technology B to technology A.

3.6 Robustness and Extensions

3.6.1 Decomposing within-class and cross-class technology spillovers

Among the three distance measures, Jaffe measure only considers technology spillovers within technology classes, while Mahalanobis and network measure also considers technology spillovers across technology classes: Mahalanobis considers spillovers from closely related fields, and network measure considers "upstream" fields that patents in the technology field cite from. One concern is that the effects of *SPILLTECH* in regressions using the Malahanobis and network distance measures are driven by within-class technology spillovers.

To assess the importance of technology spillovers across technology fields we decompose the Mahalanobis measure into within-class (diagonal) and cross-class (non-diagonal) components:

$$TECH_{ij}^{M} = n_i F_i \Omega F_j' = n_i F_i F_j' + n_i F_i(\Omega - I)F_j' = TECH_{ij}^{J} + TECH_{ij}^{M-Cross}$$

Note that the within-class component is exactly the Jaffe distance measure. We can also do the same decomposition for the network distance measure.

Table 5 presents the results for within and cross class technology spillovers separately. We report OLS results, and using tax credits as instruments also yields similar results. Column 1 repeats the results for Jaffe distance measure. Column 2 uses only the cross class component of network measure as the measure of *SPILLTECH*. The cross class technology spillovers alone still have a positive and significant impact on R&D, patenting, market value and productivity, which is of similar magnitude as the impact of within-class technology spillovers. For instance, in panel A, a 10 percent increase in within class R&D spillovers is associated with a 1 percent increase in own R&D intensity, while a 10 percent increase in cross class R&D spillovers is associated with a 1.5 percent increase in own R&D intensity.

In Column 3 we include both within-class (same as Jaffe) and cross class component of R&D spillovers measured using network measure. In R&D equation, the coefficient on within-class *SPILLTECH* is small and insignificant, and the coefficient on cross-class *SPILLTECH* is positive and significant, suggesting that R&D responds strongly to R&D by firms operating in "upstream" technology fields. This is consistent with the finding in AAK that about half of the patent citations are from outside the technology class. Cross-class spillovers also has a larger impact on market value and patenting than within-class spillovers. However, cross-class spillovers has a smaller impact than within-class spillovers on productivity, since the cross-class citation patterns fail to capture the "production" spillovers.

In the last column, we include cross-class component of both network and Mahalanobis distance measure to see which is a better characterization of the spillovers across technology fields. Not surprisingly, the network measure has the largest coefficient in R&D and patent equations, and the Mahalanobis measure has the largest coefficient in market value and productivity equations. In R&D and patent equations, within-class and cross-class spillovers both have positive and significant impact, and are of similar magnitudes. However, the coefficient on within-class spillovers is negative and significant in market value equation, and is insignificant in productivity equation. One reason is that firms that are close in technology space using Jaffe measure have a lot of overlap with firms that are close in technology space using Mahalanobis measure (correlation between Jaffe and Mahalanobis measure is 0.9), so *SPILLTECH* using Mahalanobis measure is highly correlated with *SPILLTECH* using Jaffe measure even after subtracting the within-class component.

3.6.2 Using patent stock as knowledge stock

So far we have followed BSV and use the R&D stock to approximate knowledge stock k_t. R&D may not measure knowledge stock accurately, for example, some firms may possess a big knowledge stock despite little investment in R&D if they are very productive in research or they benefit from a lot of spillovers from other firms. A more direct measure of knowledge stock would be to use the stock of patents owned by a firm, and spillovers can be calculated as:

$$SPILLTECH_{it}^{N} = \sum_{j \neq i} TECH_{ij} P_{jt}$$

$$SPILLSIC_{it}^{N} = \sum_{j \neq i} SIC_{ij} P_{jt}$$

where P_{jt} is the accumulated unweighted number of patents of firm j at year t, and is calculated using perpetual inventory method with a 15% depreciation rate. The patent stock may not accurately measure the knowledge stock either, as part of knowledge stock (like scientists, management practices) are not captured by number of patents.

The results are presented in Table 6. In Column 1 to Column 3, we calculate *SPILLTECH* using all the three distance measures respectively, and in the last column all three measures are included as regressors. In R&D equation, only *SPILLTECH* using the network distance measure has positive and significant coefficient, suggesting that firms' R&D responds positively to patent stock of firms operating in "upstream" technological fields but not patent stock of firms operating in similar technological fields.

The overall results are very similar to using R&D stock to measure knowledge stock. All three measures of *SPILLTECH* has a positive and significant effect on patenting, market value and productivity. The network measure performs the best in patent equation, while the Mahalanobis measure performs the best in market value and productivity equations.

3.6.3 Persistence of technology spillovers over time

In this subsection we study the effects of technology spillovers over time. Technology diffusion happens gradually, especially if across technology fields. AAK shows that in the first year after invention, 62% of the downstream citations come from the same patent class, and 81% are from the same patent category; after ten years, only 51% of citations are from the same patent class, and 75% from the same category.

In Table 7, we use the measure of R&D spillovers *SPILLTECH* lagged by 1 year, 5 years and 10 years as regressors separately. Each element in the table is a separate OLS regression (regressions using IV yield qualitatively similar results). The effect of *SPILLTECH* on own R&D becomes insignificant after five or ten years. Nevertheless, the impact of *SPILLTECH* on patenting, market value and productivity tend to persist after five or ten years. In patent equations, a one percent increase in R&D spillovers from firms in upstream and similar technology fields is associated with a nearly one percent increase in patenting even after 10 years. This implies that technology diffusion is slow and R&D has a long-lasting impact on future innovation.

In market value equation, the effect of *SPILLTECH* more than doubled after 10 years, suggesting that market value reacts slowly to changes in innovation. In productivity equations, *SPILLTECH* using the network measure has a smaller coefficient than the other two measures when lagged by one year, but has a larger coefficient than other measures when lagged by ten years. This is consistent with the view that "innovation" spillovers across technology fields takes a longer time to have effects on productivity than "production" spillovers where new technologies can be adopted immediately.

In the last row of each panel, I used the R&D spillovers of five years later as a falsification exercise. In R&D, market value and productivity equations, all the coefficients on *SPILLTECH* become statistically insignificant except for one case (Mahalanobis measure in productivity equation). *SPILLTECH* still has a positive and significant impact on patenting, but the magnitude is less than one third of the coefficients on lags of R&D spillovers. This confirms that the effect of R&D spillovers from technologically close firms on firm outcomes that we found is credible and not purely driven by common shocks.

3.7 Which Firms Generate and Receive the Largest Spillovers?

The network measure gives us a simple measure of the spillover of a firm's knowledge stock to other firms: the predicted number of times the firm's patents get cited by other patents. From equation (1) we can write it as:

$$CITE_i = \sum_{j \neq i} TECH^N_{ji} = \sum_{j \neq i} n_j F_j C F_i' \approx \sum n_j F_j C F_i' = \tilde{C} F_i' \tag{3.19}$$

where $\tilde{C}_{\tau\tau'} = N_\tau C_{\tau\tau'}$. The predicted average citations of a firm's patents is determined only by a firm's distribution of technology classes, and it is larger when the firm innovates in more "upstream" technology classes.

Figure 2 plots the actual average citations per patent along with the predicted citations per patent of each firm. The left panel contains all 796 firms in the sample. The trending line shows that the actual citations per patent increase by 1.5 when the predicted citations per patent increase by one. This specification explains about 34% of the variation in actual citations per patent. The actual citations per patent is below the 45 degree line because the number of citations is truncated by the sample period, and the distribution of number of citations is right skewed. Since the average citations is more volatile for small firms, the right panel of Figure 2 contains 277 firms that have more than 100 patents. The actual average citations is more aligned with predicted citations, and the technology class distribution of a firm accounts for 56% of the variation in average citations per patent.

BSV claims that smaller firms generate less spillovers because they innovate in technological niches where few other firms innovate in. In Table 8 we also divide firms into quartiles by employment size and calculate the mean measures for spillovers within each quartile. In line with BSV's conclusion, the average Jaffe and Mahalanobis measure of *TECH* increases with firm size. Nevertheless, both the network measure and the cross-class component of network measure are largest for the smallest quartile. This suggests that although smaller firms operate in less technological fields and have less overlap with other firms, they are also more likely to operate in more "upstream" technological fields, which generates large knowledge spillovers to the other firms. The largest firms also generate more spillovers than medium-sized firms. This is consistent with some literature on reallocation which finds that smaller (and younger) firms are more innovative and an R&D subsidy to entrants and small firms is welfare-improving.

There are some other interesting insights from using the network measure. Using the citation network matrix C, we can rank all the technology classes from "upstream" to "downstream" using the average number of citations each patent in that class gets. For example, the most upstream technology field is "Data Processing: Artificial Intelligence": each patent in this field are cited 5.3 times within 10 years, half from within the field and half from outside the field. Nearly all the top 10 upstream technology fields are in the computer software and data processing category. We can also rank the firms from technologically upstream to downstream based on their technology field distributions. Among the top 100 upstream firms that generates the largest spillovers, more than 70% are from five three-digit (SIC) industries: Surgical, Medical, and Dental Instruments and Supplies (384), Electronic Components and Accessories (367), Computer and Office Equipment (357), Drugs (283), and Computer Programming, Data Processing, and other Computer Related Services (737).

In Table 9 we divided firms into quantiles based on innovation intensity. The innovation intensity is calculated as the total number of patents from from 1970 to 1999 divided by the number of employees. Firms with higher innovation intensity are on average smaller, and generated larger spillovers to other firms. Comparing Column 3 and Column 4, the firms in the highest quantile and second highest quantile have similar nearly the same *TECH* on average when using Jaffe and Mahalanobis measure, but the highest quantile have much larger *TECH* measured by the network measure. This suggests that firms that are most intensive in research and innovation tend to also research more in upstream technological fields.

On the other hand, to study the spillovers a firm receives, consider the knowledge production function from Section 3:

$$k_i^t = \phi(r_i^t, k_i^{t-1}, k_\tau^{t-1}) = \phi(r_i^t, k_i^{t-1}, T_i\mathbf{k}^{t-1})$$

The spillover term is:

$$T_i k^{t-1} = \sum_{j \neq i} TECH_{ij} k_j^{t-1} \propto \sum_{j \neq i} F_i C F_j' k_j^{t-1} \approx F_i C (\sum_j F_j' k_j^{t-1}) = (\sum_j k_j^{t-1}) F_i C F_{all}'$$

where F_{all} is the technology class distribution of all the firms.

The term $F_i C F_{all}'$ is the measure of the technology spillovers of all other firms' R&D on firm i. We calculated this measure for all firms in the CompuStat sample, and Table 8 and Table 9 presents the results for firm quantiles of employment and innovation intensity respectively . Loosely speaking, the predicted citations per patent measures how "upstream" in the technology space the firm is, and the spillovers from other firms measures how technologically "downstream" the firm is. Since spillovers within technology classes are hard to interpret, we also included measures for only cross-class spillovers.

Last two rows of Table 8 shows that smaller firms receive more spillovers from other firms than large firms. Table 9 suggests that firms with lower innovation intensity receive more spillovers from other firms. This may be explained by endogenous technology choices. If firms could direct their research and choose the technology class distribution F_i, then for small firms and firms with low R&D intensity, spillovers from other firms are more important in the knowledge production, and they benefit more from choosing more "downstream" technology fields.

Table 8 and Table 9 show that small firms are the largest sources and the largest receivers of technology spillovers, since they populate in the most upstream and downstream technological fields. This comes from the asymmetry of the network measure. In contrast, in BSV small firms operate in technological niches, so they would generate and receive the smallest spillovers. Our result is similar to the findings of Manresa (2013), who estimated unknown network relationships between firms using the same data, and found that sources of spillovers are on average small firms with low employment and market value, and have higher R&D intensity and patent citations.

8. Conclusion

In this paper we develop a new measure of technological distance between firms based on the patent citations network in AAK. Unlike Jaffe and Mahalanobis measure, the network measure is asymmetric: firms doing research in upstream technology classes generate knowledge spillovers to firms doing research in downstream technology classes because new innovations builds on past achievements, but not vice versa.

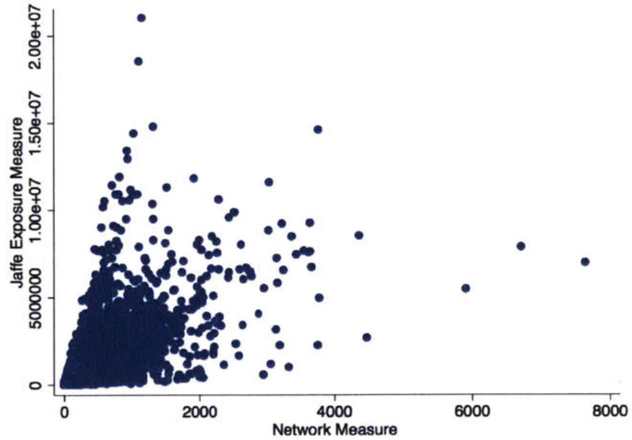
We then use this measure to estimate the effects of R&D of firms close in technology space or product space on firm's R&D, patenting, market value and productivity. We do not find significant product rivalry effect, but R&D spillovers from firms close in technology space have a positive and significant impact on market value, productivity and patenting. When using the network distance measure we constructed, the R&D spillovers from upstream firms also has a positive and significant impact on R&D. The spillovers using the network measure also have the largest impact on patenting, though the spillovers using the Mahalanobis measure have the largest impact on market value and productivity. This together with our model suggests that the network measure is a more accurate measure of the technology spillovers that future innovations builds on, while Mahalanobis measure better captures technology adoption in production.

We view this as a first step in understanding the micro-foundations of the innovation network. While AAK established the network relationships between technology classes, little is known of how firms innovating in different technology classes interact with each other and how this affects the aggregate innovations in the economy. By using the network measure instead of Jaffe or Mahalanobis measure, we put firms on a directed innovation network rather than a technology space. Firms' R&D decisions form a game over the innovation network, and we already presents some evidence that firm's R&D responds positively to R&D of upstream firms.

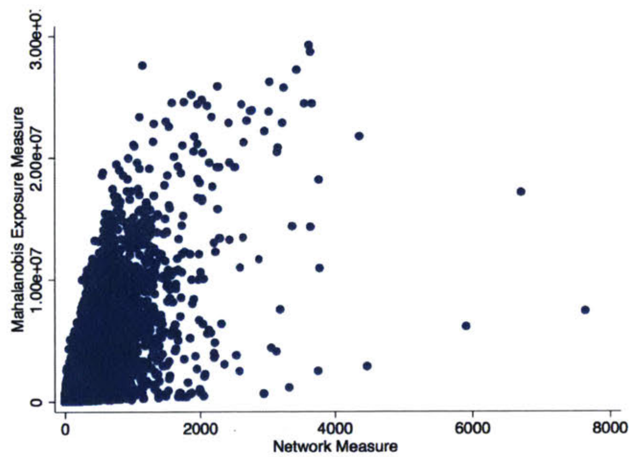
Studying firm interactions over the innovation network is important in many ways. First, we can identify which firms generate the largest spillovers to other firms and how large the spillovers are, which is crucial for designing R&D tax policies and is the main motivation of most studies on R&D spillovers. Second, it can help us understand better the aggregate effect of shocks on innovation by looking at how shocks are transmitted through the innovation network to upstream and downstream firms. For example, if some shocks (for example, Chinese imports competition or financial crisis) affects the R&D and innovation of a subset of firms adversely, then the R&D and innovation of firms operating in downstream technology fields of the affected firms will suffer too.

One future direction would be to refine the measure by backing out the unknown network relationships between firms, like Manresa (2013), since some of the knowledge flows are not captured by patent citations. It would also be interesting to compare the network relationship identified using the firm outcomes and the network measure constructed in this paper and other measures of technological distance.

3.8 Figures and Tables

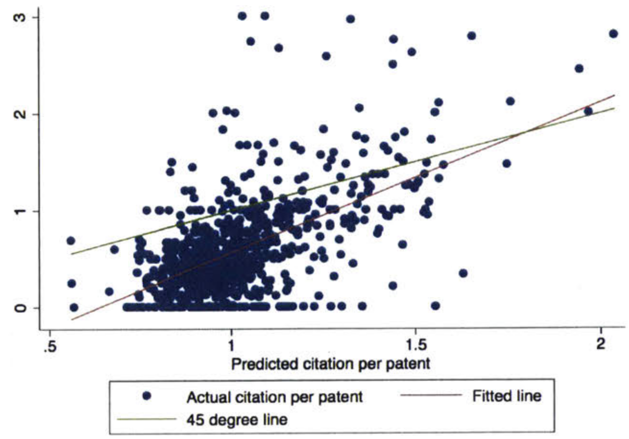


(a) Network and Jaffe

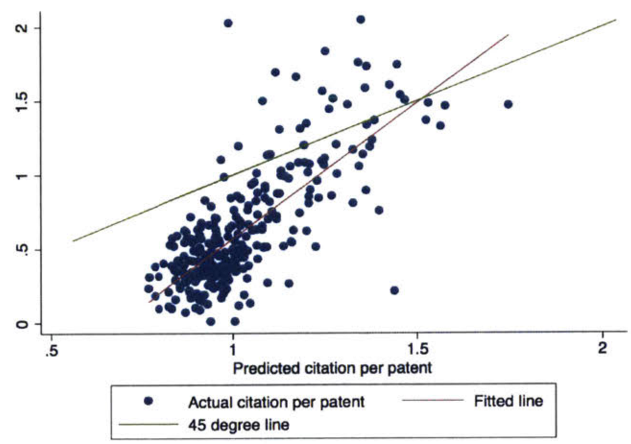


(b) Network and Mahalanobis

Figure 1: Comparing Jaffe, Mahalanobis and Network Measures



(a) All firms



(b) Firms with more than 100 patents

Figure 2: Actual and Predicted Citations Per Patent

Table 1
R&D Equation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Specification	OLS	OLS	OLS	OLS	2SLS	2SLS	2SLS
Distance measure	Jaffe	Jaffe	Malahanobis	network	Jaffe	Malahanobis	network
ln(*SPILLTECH*)$_{t-1}$	0.438	0.099	-0.008	0.187	0.192	0.085	0.370
	(0.029)	(0.068)	(0.087)	(0.076)	(0.105)	(0.113)	(0.104)
ln(*SPILLSIC*)$_{t-1}$	0.369	0.084	0.106	0.068	-0.055	-0.008	-0.104
	(0.013)	(0.035)	(0.034)	(0.034)	(0.073)	(0.070)	(0.069)
					IV 1st stage F-tests		
ln(*SPILLTECH*)$_{t-1}$					220.6	795.6	383.4
ln(*SPILLSIC*)$_{t-1}$					30.6	40.5	24.4
Firm fixed effects	no	yes	yes	yes	yes	yes	yes
Number of obs	8,579	8,579	8,579	8,579	8,579	8,579	8,579

Table 2
Patent Equation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Specification	Neg. Bin.	Neg. Bin.	Neg. Bin.	Neg. Bin.	NB IV	NB IV	NB IV
Distance measure	Jaffe	Jaffe	Malahanobis	network	Jaffe	Malahanobis	network
ln(*SPILLTECH*)$_{t-1}$	0.548	0.621	0.709	0.841	0.622	0.718	0.848
	(0.037)	(0.040)	(0.045)	(0.049)	(0.042)	(0.046)	(0.050)
ln(*SPILLSIC*)$_{t-1}$	0.069	0.085	0.064	0.037	0.083	0.057	0.029
	(0.023)	(0.023)	(0.022)	(0.023)	(0.025)	(0.023)	(0.024)
ln(R&D Stock)$_{t-1}$	0.048	0.040	0.068	0.050	0.041	0.044	0.072
	(0.032)	(0.035)	(0.033)	(0.034)	(0.035)	(0.035)	(0.034)
					IV 1st stage F-tests		
ln(*SPILLTECH*)$_{t-1}$					229.7	490.2	205.5
ln(*SPILLSIC*)$_{t-1}$					58.9	13.8	14.0
Pre-sample FE	no	yes	yes	yes	yes	yes	yes
Number of obs	9,046	9,046	9,046	9,046	9,046	9,046	9,046

Table 3
Market Value Equation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Specification	OLS	OLS	OLS	OLS	2SLS	2SLS	2SLS
Distance measure	Jaffe	Jaffe	Malahanobis	network	Jaffe	Malahanobis	network
$\ln(SPILLTECH)_{t-1}$	0.061	0.171	0.510	0.204	0.547	0.722	0.523
	(0.017)	(0.082)	(0.105)	(0.090)	(0.129)	(0.128)	(0.132)
$\ln(SPILLSIC)_{t-1}$	0.062	-0.024	-0.036	-0.025	-0.039	-0.028	-0.030
	(0.006)	(0.026)	(0.025)	(0.026)	(0.071)	(0.068)	(0.071)
$\ln(\text{R\&D Stock/Capital})_{t-1}$	0.644	0.323	0.330	0.312	0.316	0.336	0.294
	(0.138)	(0.174)	(0.174)	(0.174)	(0.174)	(0.174)	(0.174)
					IV 1st stage F-tests		
$\ln(SPILLTECH)_{t-1}$					195.3	946.2	411.6
$\ln(SPILLSIC)_{t-1}$					40.9	41.3	35.9
Firm fixed effects	no	yes	yes	yes	yes	yes	yes
Number of obs	12,561	12,561	12,561	12,561	12,561	12,561	12,561

Table 4
Productivity Equation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Specification	OLS	OLS	OLS	OLS	2SLS	2SLS	2SLS
Distance measure	Jaffe	Jaffe	Malahanobis	network	Jaffe	Malahanobis	network
$\ln(SPILLTECH)_{t-1}$	-0.026	0.141	0.237	0.116	0.128	0.185	0.031
	(0.009)	(0.041)	(0.053)	(0.045)	(0.074)	(0.072)	(0.066)
$\ln(SPILLSIC)_{t-1}$	-0.015	-0.006	-0.006	-0.002	0.025	0.019	0.064
	(0.004)	(0.012)	(0.011)	(0.012)	(0.056)	(0.055)	(0.054)
$\ln(\text{R\&D Stock})_{t-1}$	0.061	0.043	0.042	0.044	0.042	0.042	0.043
	(0.005)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
					IV 1st stage F-tests		
$\ln(SPILLTECH)_{t-1}$					84.8	1082.9	440.9
$\ln(SPILLSIC)_{t-1}$					17.2	61.9	54.5
Firm fixed effects	no	yes	yes	yes	yes	yes	yes
Number of obs	9,949	9,949	9,949	9,949	9,949	9,949	9,949

Table 5
Comparing Within and Cross Class Technology Spillovers

	(1)	(2)	(3)	(4)
A. R&D Equation				
$\ln(SPILLTECH)_{t-1}$-Jaffe	0.099		0.017	0.278
	(0.068)		(0.116)	(0.126)
$\ln(SPILLTECH)_{t-1}$-network cross		0.152	0.135	0.358
		(0.087)	(0.147)	(0.155)
$\ln(SPILLTECH)_{t-1}$-Mahalanobis cross				-0.661
				(0.154)
$\ln(SPILLSIC)_{t-1}$	0.084	0.081	0.080	0.071
	(0.035)	(0.034)	(0.035)	(0.035)
B. Patent Equation				
$\ln(SPILLTECH)_{t-1}$-Jaffe	0.621		0.370	0.269
	(0.040)		(0.052)	(0.101)
$\ln(SPILLTECH)_{t-1}$-network cross		0.757	0.426	0.363
		(0.048)	(0.064)	(0.082)
$\ln(SPILLTECH)_{t-1}$-Mahalanobis cross				0.163
				(0.144)
$\ln(SPILLSIC)_{t-1}$	0.085	0.078	0.048	0.047
	(0.023)	(0.024)	(0.022)	(0.022)
C. Market Value Equation				
$\ln(SPILLTECH)_{t-1}$-Jaffe	0.171		-0.063	-0.422
	(0.082)		(0.125)	(0.161)
$\ln(SPILLTECH)_{t-1}$-network cross		0.356	0.411	0.080
		(0.106)	(0.160)	(0.167)
$\ln(SPILLTECH)_{t-1}$-Mahalanobis cross				0.990
				(0.209)
$\ln(SPILLSIC)_{t-1}$	-0.024	-0.029	-0.026	-0.013
	(0.026)	(0.025)	(0.026)	(0.027)
D. Productivity Equation				
$\ln(SPILLTECH)_{t-1}$-Jaffe	0.141		0.132	0.027
	(0.041)		(0.057)	(0.076)
$\ln(SPILLTECH)_{t-1}$-network cross		0.135	0.015	-0.140
		(0.052)	(0.072)	(0.076)
$\ln(SPILLTECH)_{t-1}$-Mahalanobis cross				0.354
				(0.108)
$\ln(SPILLSIC)_{t-1}$	-0.006	0.0004	-0.006	-0.003
	(0.012)	(0.012)	(0.012)	(0.012)

Table 6
Using Patent Stock as Knowledge Stock

	(1)	(2)	(3)	(4)
A. R&D Equation				
ln(*SPILLTECH*)$_{t-1}$-Jaffe	-0.001			-0.194
	(0.038)			(0.089)
ln(*SPILLTECH*)$_{t-1}$-Mahalanobis		0.017		-0.005
		(0.051)		(0.112)
ln(*SPILLTECH*)$_{t-1}$-network			0.065	0.252
			(0.041)	(0.071)
ln(*SPILLSIC*)$_{t-1}$	0.026	0.023	0.014	0.020
	(0.018)	(0.017)	(0.017)	(0.017)
B. Patent Equation				
ln(*SPILLTECH*)$_{t-1}$-Jaffe	0.599			0.247
	(0.041)			(0.124)
ln(*SPILLTECH*)$_{t-1}$-Mahalanobis		0.663		-0.013
		(0.045)		(0.159)
ln(*SPILLTECH*)$_{t-1}$-network			0.836	0.599
			(0.050)	(0.092)
ln(*SPILLSIC*)$_{t-1}$	0.111	0.089	0.043	0.035
	(0.025)	(0.024)	(0.024)	(0.023)
C. Market Value Equation				
ln(*SPILLTECH*)$_{t-1}$-Jaffe	0.077			-0.549
	(0.071)			(0.189)
ln(*SPILLTECH*)$_{t-1}$-Mahalanobis		0.251		0.630
		(0.105)		(0.242)
ln(*SPILLTECH*)$_{t-1}$-network			0.175	0.280
			(0.082)	(0.182)
ln(*SPILLSIC*)$_{t-1}$	0.017	0.009	0.008	0.018
	(0.022)	(0.021)	(0.022)	(0.022)
D. Productivity Equation				
ln(*SPILLTECH*)$_{t-1}$-Jaffe	0.129			0.030
	(0.037)			(0.099)
ln(*SPILLTECH*)$_{t-1}$-Mahalanobis		0.181		0.118
		(0.050)		(0.119)
ln(*SPILLTECH*)$_{t-1}$-network			0.138	0.026
			(0.041)	(0.073)
ln(*SPILLSIC*)$_{t-1}$	0.014	0.016	0.015	0.014
	(0.013)	(0.013)	(0.013)	(0.013)

Table 7
Effects of Spillovers Over Time

	R&D	Patent	Market Value	Productivity
A. Network				
$\ln(SPILLTECH)_{t-1}$	0.187	0.841	0.204	0.116
	(0.076)	(0.049)	(0.090)	(0.045)
$\ln(SPILLTECH)_{t-5}$	0.093	0.980	0.331	0.069
	(0.083)	(0.059)	(0.111)	(0.054)
$\ln(SPILLTECH)_{t-10}$	0.147	0.945	0.696	0.171
	(0.142)	(0.075)	(0.282)	(0.098)
$\ln(SPILLTECH)_{t+5}$	0.169	0.237	-0.022	-0.120
	(0.159)	(0.025)	(0.088)	(0.175)
B. Jaffe				
$\ln(SPILLTECH)_{t-1}$	0.099	0.621	0.171	0.141
	(0.068)	(0.040)	(0.082)	(0.041)
$\ln(SPILLTECH)_{t-5}$	0.039	0.724	0.424	0.110
	(0.074)	(0.051)	(0.101)	(0.050)
$\ln(SPILLTECH)_{t-10}$	0.107	0.662	0.753	0.166
	(0.127)	(0.068)	(0.270)	(0.097)
$\ln(SPILLTECH)_{t+5}$	0.202	0.251	-0.082	0.090
	(0.181)	(0.024)	(0.080)	(0.187)
C. Mahalanobis				
$\ln(SPILLTECH)_{t-1}$	-0.008	0.709	0.510	0.237
	(0.087)	(0.045)	(0.105)	(0.053)
$\ln(SPILLTECH)_{t-5}$	-0.018	0.825	0.735	0.109
	(0.090)	(0.055)	(0.123)	(0.062)
$\ln(SPILLTECH)_{t-10}$	0.050	0.760	1.036	0.124
	(0.161)	(0.074)	(0.230)	(0.119)
$\ln(SPILLTECH)_{t+5}$	0.123	0.252	-0.061	0.387
	(0.219)	(0.024)	(0.127)	(0.198)

Table 8

Average Measures of Spillovers in Firm Size Quartiles

Quartile	1	2	3	4
Median number of employees	685	3,398	10,442	50,000
Avg. Jaffe *TECH*	0.027	0.031	0.035	0.050
Avg. Malahanobis *TECH*	0.105	0.121	0.143	0.213
Avg. Predicted average citations per patent (Network TECH)	1.054	1.000	0.994	1.010
Avg. Cross-class network TECH	5.393	5.104	5.143	5.269
Avg. spillovers from all other firms	1.183	1.110	1.107	1.088
Avg. cross-class spillovers from all other firms	0.577	0.534	0.54	0.515

Note: the number of employees is measured at the year with the maximum number of employees.

Table 9

Average Measures of Spillovers in Quartiles of Innovation Intensity

Quartile (by innovation intensity)	1	2	3	4
Median number of patents	5	25	110	191
Median number of employees	11,029	4,791	5,700	2,727
Avg. Jaffe *TECH*	0.026	0.033	0.042	0.042
Avg. Malahanobis *TECH*	0.096	0.131	0.174	0.180
Avg. Predicted average citations per patent (Network *TECH* normalized)	0.97	0.993	1.010	1.085
Avg. Cross-class network *TECH*	4.993	5.044	5.201	5.674
Avg. spillovers from all other firms	1.153	1.111	1.099	1.126
Avg. cross-class spillovers from all other firms	0.597	0.537	0.520	0.512

Note: innovation intensity is measured by the number of patents divided by the number of employees.

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