ESSAYS ON FINANCE AND LABOR

Wenting Ma

A dissertation submitted to the faculty of the University of North Carolina at Chapel Hill in partial fulfillment of the requirements for the degree of Doctor of Philosophy in the Department of Economics.

Chapel Hill
2019

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ABSTRACT

WENTING MA: ESSAYS ON FINANCE AND LABOR.
(Under the direction of Paige P. Ouimet and Neville Francis)

My dissertation applies different empirical methodologies with a variety of administrative datasets to investigate the interrelationship between firms and labor market outcomes. Chapter 1 examines *how* and *why* market power affect wages differently in financial industries. Increasing industry concentration has raised concerns that declining competition among firms for labor has led to slow wage growth. However, I find that finance wages have increased by almost three times the increase in non-finance wages, despite similar trends in market concentration. Using data from the U.S. Census, I construct measures of firm-specific market power and show that higher market power is associated with significantly higher wages in finance than in non-finance. I provide evidence that rent-sharing plays an essential role in driving the more pronounced effect of market power on finance wages for two reasons. First, financial firms with higher market power can extract relatively higher rents to share. Second, financial firms give a relatively higher share of rents to workers, especially high-skill workers, due to relatively higher worker bargaining power. As rents are disproportionally distributed to high-skill workers, financial firms with higher market power are associated with relatively higher within-firm inequality.

Chapter 2 is joint work with Paige Ouimet and Elena Simintzi. This chapter confronts the question of how mergers and acquisitions (M&As) contribute to important trends in job polarization and wage inequality. We document shifts in occupational composition following M&As along with increases in average wages and wage inequality. We propose M&As act as a catalyst for technological change. Due to an increase in scale, improved efficiency and lower financial constraints, M&As facilitate technology adoption, disproportionately increasing the productivity of high-skill workers and enabling the displacement of mid-income routine occupations. We document these

findings in M&A impacted establishments as compared to a matched sample of control establishments. These results generalize within industries, suggesting M&A activity is an important driver of economy-wide trends in job polarization and income inequality.

Chapter 3 is joint work with Tania Babina, Paige Ouimet and Rebecca Zarutskie. This chapter answers why young firms pay less. Using US Census employer-employee matched data, we show that lower wages at new firms are driven by the selection of lower quality workers into new firms. After including worker fixed effects, nearly three quarters of the new firm wage difference disappears. Once we control for firm fixed effects, absorbing time-invariant firm quality, the wage difference between new and established firms becomes economically unimportant. Overall, our findings indicate that, for a given worker who has job opportunities at similar quality new and established firms, the expected wage penalty of working at the new firm is, on average, economically insignificant. Moreover, young firms that can hire high-quality workers have higher future survival rates and total employment, suggesting that human capital is an important predictor of young firm success.

ACKNOWLEDGMENTS

I am deeply indebted to my tremendous advisors, Paige Ouimet and Neville Francis. Thank you for being supportive of my research, for encouraging me to pursue my career goals and for working actively to help me achieve those goals. Thank you!

I would like to thank Elena Simintzi, Patrick Conway and Valentin Verdier for serving as my committee members. I also want to recognize Rebecca Zarutskie, Tania Babina, Xingxing Wang, Yunzhi Hu, Jessie Davis, Andrei Concalves, Gill Segal, Andrew Yates, Lutz Hendricks, Fei Li, Peter Hanson, Dragana Cvijanovic, Stan Rabinovich, Jacob Sagi, and James Carlton Ingram for all their help.

I would like to express my gratitude to my dear family and friends. I can never thank my parents enough for all their unconditional love and all of the sacrifices that they've made on my behalf.

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CHAPTER 1

MARKET POWER, FINANCE WAGES AND INEQUALITY

Disclaimer

The research in this paper was conducted while the author was a Special Sworn researcher of the U.S. Census Bureau. Research results and conclusions expressed are those of the author and do not necessarily reflect the views of the Census Bureau. This paper has been screened to ensure that no confidential data are revealed.

1.1 Introduction

Recent literature documents an important trend in U.S. product markets: increasing concentration. Grullon et al. (2017) show that 75% of U.S. industries have become more concentrated since the 1990s, and the average firm is almost three times larger. One potential concern with this rise in industry concentration is that it reduces workers' employment options, and thus gives employers the ability to lower wages (Manning, 2011; Stiglitz, 2017; Benmelech et al., 2018).[1] The finance sector, however, has been an exception. I observe that from 1990 to 2008, real wages increased by 23.38% in finance but only 8.85% in non-finance. In the same period, the degrees of industry concentration as measured by the Herfindahl-Hirschman index (HHI) increased by roughly 40% in both finance and non-finance. These novel findings indicate that market concentration may not have the same wage dampening effect in the financial sector, suggesting a more nuanced understanding of the effects of concentration on wages and the finance wage premium.

Why does concentration impact finance wages differently? In this paper, I argue that rent-sharing plays an essential role in driving the difference. Industry concentration increases the market power of firms, especially larger firms within a given industry. An increase in firms' market power not only increases firms' labor market monopsony power to lower wages by decreasing competition for hiring workers, but also increases firms' product market monopoly power by decreasing competition for selling products or buying inputs. With higher product market monopoly power, firms can charge higher markup and thus extract higher rents. Wages rise when firms share

[1]This concern was raised in the Council of Economic Advisers Issue Brief (2016). The similar concern was addressed at the 2018 Jackson Hole Economic Symposium: "Within product markets, there has been a notable increase in economic activity associated with large multinational corporations along with the increased market concentration in many industries. These developments suggest that large firms today may have greater market power than in the past, and this shift may result in a decrease in competition within many industries. These shifts should concern central bankers since they likely have important linkages to observed structural changes in the global economy, including lower capital investment, a declining labor share, slow productivity growth, slow wage growth and declining dynamism." See more details at https://www.kansascityfed.org/~/media/files/publicat/newsroom/2018/pressrelease_jacksonhole18.pdf

these rents with their workers.[2] Therefore, higher market power is associated with two competing effects on wages: rent-sharing effect and labor monopsony effect. In finance, the rent-sharing effect appears to dominate the labor monopsony effect, and the net effect is relatively stronger. To support this argument, I propose and provide empirical evidence on two non-mutually exclusive mechanisms. First, financial firms with higher market power can extract relatively higher rents, and thus they have more to share with employees. Under this scenario, financial firms' profitability responds more positively to the increase in firm market power relative to non-financial firms. Second, financial firms may have to give a larger fraction of rents to employees, especially high-skilled workers, due to higher worker bargaining power. Under this scenario, the wages of high-skilled workers in finance respond more positively to an increase in firm market power as compared to those in non-finance.

Using administrative micro-level data from the U.S. Census, this paper first examines the relation between wages and industry concentration, and whether the relationship differs in finance. I find that concentration measured by HHI is negatively correlated with wages in non-finance industries. By contrast, HHI is positively correlated with wages in finance, and the positive correlation is statistically significant at 1% level. These findings are robust to various measures of industry concentration, indicating that industry concentration has different implications in finance.

While industry concentration means higher market power for firms remaining in the industry on average, previous studies argue that concentration disproportionally benefits larger players within industries. Relative to small players in a given industry, larger firms have higher market power to raise prices or lower input prices and thus make higher rents. In other words, market power should be firm-specific and dependent on the firm's market share, implying that rents increase with firm size within industries (Shepherd, 1972; Porter, 1979). With more rents, firms with higher market

[2]Previous literature provides evidence that firms share rents with their workers. For example, Blanchflower et al. (1996) use CPS data to show that changes in wages are explained by increases in industry profitability within the manufacturing sector. Card, Deficient and Maida (2014) use employer-employee matched data from Italy to show an increase in firm-specific profitability leads to significant increases in wages. Card et al. (2018) provides a summary of recent studies on rent sharing. A string of literature provides various reasons of rent-sharing: 1. making managers' lives easier (Bertrand and Mullainatha,2003; Cronqvist et al., 2009); 2. providing workers with incentives to keep working hard (Katz and Summer, 1989; Akerlof and Yellen,1990); 3. poor governance (Bebchuck and Fried, 2006).

power can afford higher wages on average as compared to firms with lower market power within a given industry. Using this within-industry variation, I next conduct firm-level cross-sectional analysis to examine how the relationship of firm-specific market power and wages in finance is different from that in non-finance.

As the baseline, a firm's market power is defined as its employment share in its industry.[3] The main result of this paper is that higher firm market power is associated with significantly higher wages in finance than that in non-finance. This result holds when controlling for a battery of factors that are likely to drive the heterogeneity of wage-setting behaviors across firms. Also, this result holds for defining firms' market by either two-digit or three-digit Standard Industry Classification (SIC). The difference between finance and non-finance is still significant if I instead look at median wages, average wages adjusted by cost of living, and average wages of male or female workers.

I next provide evidence for two non-mutually exclusive mechanisms that can explain why the positive relationship between firm market power and wages is stronger in finance. First, I argue that financial firms with higher market power can extract relatively higher rents as compared to non-financial firms. Market power may be particularly valuable in finance due to a stronger belief in "too big to fail, " or less geographical restrictions for allocating resources. For example, the "too big to fail " argument suggests that larger financial firms are more likely to be supported by the government when they face potential failure, and thus they can borrow at a much lower cost than smaller financial firms who must borrow based on their creditworthiness (Baker et al., 2009). Because financial firms can extract relatively higher rents with higher market power, they have more to share with workers. In support of this mechanism, I find that firms with higher market power exhibit higher profitability measured by return on assets (ROA) in finance relative to non-finance.[4]

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[3]This measure is used by the courts as a primary criterion to assess the existence of monopoly power in a given product market. See chapter 2 in "Competition and Monopoly: Single-Firm Conduct Under Section 2 of the Sherman Act" by Department of Justice for more details.

[4]Barber and Lyon(1996) show that ROA is a better measure than other profitability measures in detecting abnormal operating performance. Grullon et al. (2017) also uses this measure to investigate the relationship between market concentration and abnormal profits.

To further investigate the sources of the additional profitability created by firm market power in finance, I decompose ROA into two components: the Lerner Index and the Asset Utilization ratio. The Lerner Index (or price-cost margin) has been widely used in the literature as a proxy for the extent to which prices exceed marginal costs (e.g., Muller et al., 2017; Aghion, 2005; Grullon et al., 2017). The asset utilization ratio captures operational efficiency (Grullon et al., 2017). Results show that financial firms can generate relatively higher profitability with higher market power because they are able to charge significantly higher price-cost margins relative to non-financial firms.

Second, compared to non-financial firms, financial firms may have to give a higher proportion of their rents to workers due to higher worker bargaining power. This mechanism should apply especially to high-skilled workers in finance because they are more likely to be matched with larger scale tasks, or high external visibility of their performance increases the probability of being poached. I find that a one standard deviation increase in firm market power is associated with 1.7% higher average wages for high-skill workers in non-financial firms, whereas the effect is 6.37% in finance, which can be translated into $1468 per quarter. The results are robust to various definitions of high-skill workers.

As a consequence of rents being disproportionally distributed to high-skilled workers within firms, I find that financial firms with higher market power exhibit higher within-firm inequality, especially among male workers, as compared to non-financial firms.

The final part of my study conducts a comprehensive set of robustness analysis to eliminate alternative explanations. First, I conduct firm-local labor market level analysis to examine whether the difference in finance arises because non-financial firms are more likely to compete locally and measuring market power across national markets underestimates the firms ability to extract rents. Results at firm-local labor market-level are qualitatively similar to those at firm-level and do not support this alternative explanation. Second, I re-define market power using sales data to make sure market power is not more valuable in finance because market power measured by domestic employment captures characteristics that are more valuable in finance than in non-finance. Third, finance wages still have the highest sensitivity to market power when compared with wages in non-finance industries facing low import exposure. This result helps alleviate the concern that market power measured by domestic share is less valuable in non-finance because non-finance firms face higher import competition. Lastly, I conduct individual-level analysis while controlling for individual time-varying and time-unvarying quality. I continually find higher firm market power is associated with significantly higher wages in finance as compared to non-finance, which alleviates the concern that my results could be explained by sorting effect based on worker quality.

My paper builds on several bodies of literature. First, it builds on the literature on market concentration and its potential effects on wages. Much of this literature focuses on non-finance sectors, and a discussion on *how* and *why* concentration plays a differential role in finance wages has been missing. Weiss (1966) shows that wages are higher in more concentrated industries. However, the relationship is no longer significant and positive after controlling for personal characteristics, and the results only hold within manufacturing, transportation, and utility industries. Landon (1970) finds higher concentration in the newspaper industry is associated with lower wages. More recently, Autor et al. (2017) document higher industry concentration is associated with lower labor share. However, they do not find a significant relationship between concentration and wages in manufacturing. Benmelech et al. (2018) use U.S. manufacturing data to show that employer concentration increases firms' labor monopsony power, thus lowering wages. This study contributes to the literature by uncovering the differential role of market power on finance wages. I further provide evidence that rent-sharing can help explain the difference. Meanwhile, this study also contributes to the literature by showing that higher firm market power has different effects on the wages of different skill-level workers, thus contributing to higher within-firm inequality.

This study complements the literature that seeks to understand the substantial wage premium in finance industries (Philippon and Reshef, 2012; Boudtanifar et al., 2018; Bohm, Metzger and Stromberg, 2018; Axelson and Bond, 2015). Consistent with previous literature, I confirm finance wages are on average higher than non-finance wages using data from the U.S. Census. While previous literature shows that worker- and industry-level characteristics contribute to high wages in finance, these factors cannot fully explain the surge of finance wage premium (Philippon and Reshef, 2012; Bohm, Metzger and Stromberg, 2018). Building on previous literature, my paper provides a more nuanced understanding of finance wage premium through the lens of firms. Using U.S. employer-employee matched data, I show that higher market power is associated with higher wages in finance because market power is associated with a stronger rent-sharing effect in finance. My results suggest that firm market power can help explain the finance wage premium.

This paper also builds on the literature that investigates factors affecting within-firm inequality. Muller, Ouimet, and Simintzi (2017) examine UK data and find that, on average, larger firms have high pay inequality. Consistent with their finding, my results show firms with relatively larger sizes in a given industry exhibit higher pay inequality. My study further shows that firms that are relatively larger in finance industries are associated with even higher within-firm inequality because rents are disproportionally distributed to finance high-skilled workers who have relatively higher worker wage bargaining power. Ma, Ouimet, and Simintzi (2018) document that mergers and acquisitions (M&A) act as the catalyst for firm technology adoption, which in turn leads to an increase in inequality within target establishments. Instead of looking at how firm reorganization affects the wage distribution within the firm, my paper shows that the variation in firm market power can explain the heterogeneity of within-firm inequality.

1.2 Data

In this section, I start with reviewing multiple data sources used in this study and describing how I combine them to construct the baseline sample. I then describe how firms' industry and financial firms are defined. At the end of this section, I construct the measures of industry concentration and firm market power.

1.2.1 Datasources

The analysis in this paper combines data from three confidential databases maintained by the U.S. Census Bureau: 1) the Longitudinal Employment-Household Dynamics database (LEHD); 2) the Longitudinal Business Database (LBD); and 3) the Business Register(BR). I also link firm financial statement data from Compustat to firms in LBD through a Census internal bridge.

I use the LEHD to obtain information on firms' wage patterns and workforce composition. The LEHD is an employer-employee matched database which tracks employees and their wages with various employers on a quarterly basis.[5] Individual wages reported in the LEHD include all forms of compensation that are immediately taxable, including bonuses and exercised stock options which take a heavyweight in finance sector pay.[6] The LEHD also reports age, gender, and education level of each employee.[7] As workers in this program can be linked to their employers, it allows me to track wage distribution and workforce composition within each employer. The data start in 1990 for several states and coverage of states increases over time. The data coverage ends in 2008. This project has access to 31 states.[8] I map these states in Appendix Figure A1. While I do not observe data for all states, I observe almost 100% of private employment for any state in the program. I discuss the consequence of omitting states in section 1.7.

I supplement the information in the LEHD with firm-level information on employment and industry from the LBD.[9] This database tracks all US business establishments on an annual basis. An establishment is any separate physical location operated by a firm with at least one paid employee. The LBD includes information on industry, the number of employees and total payroll at each establishment. Also, the LBD contains a unique firm-level identifier which longitudinally links establishments that are part of the same firm. As the LBD tracks all establishments in the U.S., it allows me to measure total domestic employment for each firm and industry by aggregating employment across establishments. In section 1.2.3, I will discuss how I utilize establishment employment and industry to define firms' industry and identify financial firms.

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[5]See Abowd et al. (2006) for a more detailed description of the LEHD program and the underlying datasets that it generates.

[6]Axelson and Bond (2015) show that the financial sector is featured with high reliance on bonuses. Bell and Van Reenen (2013) use UK data to show the increase in top bankers' pay is entirely due to increased bonuses.

[7]Education is imputed for employees with missing education data (Abowd et al. 2006).

[8]31 states include: Arkansas, Colorado, Florida, Georgia, Hawaii, Iowa, Idaho, Illinois, Indiana, Louisiana, Maryland, Maine, Minnesota, Missouri, Montana, North Carolina, New Jersey, New Mexico, Nevada, Oklahoma, Oregon, Rhode Island, South Carolina, Tennessee, Texas, Utah, Virginia, Vermont, Washington, Pennsylvania and Wisconsin.

[9]See Jarmin and Miranda (2002) for more details.

To conduct mechanism tests in section 1.4, I collect firm financial information on earnings before interests, taxes, depreciation and amortization (EBITDA), total sales, and total assets from Compustat.

Lastly, I obtain sales data for firms from the Business Register (BR) database to conduct robustness checks in section 1.5.[10] This database collects business sales, including total revenue from selling products, interest income and gross rents, from the Internal Revenue Service (IRS). It tracks firms from all 50 states and the District of Columbia on an annual basis. A key advantage of the sales data from BR over standard firm-level databases such as Compustat is that they comprise both private and public listed firms.

1.2.2 Sample Construction

The baseline sample is at firm-year-quarter-level and spans from 1990 to 2008. To obtain firm wage patterns and workforce composition, I start with linking filtered employee-level data from the LEHD to firm identifiers in the LBD through federal employer identifier (EIN), and then I aggregate employee-level data to firm-level in each year-quarter.[11]

Specifically, I restrict my attention to full time workers in the LEHD by only including workers aged between 16 and 65 years old and by excluding employee-quarter that earned less than 80% of the 1990 federal minimum wage following Philippon and Reshef (2012), where wages are converted to constant 2001 dollars.[12] To ensure that I am not observing quarters in which an employee was only partially employed at a given firm, I only keep employee-firm quarters where I observe a full quarter of employment at the firm prior to and post that quarter.[13]

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[10]Before 2002, this database was referred as the Standard Statistical Establishment List(SSEL). See DeSalvo et al. (2016) for more information about the dataset.

[11]The matching process between establishments in LBD and units in LEHD is not perfect because the LBD infrastructure is based on physical establishments while the LEHD infrastructure uses reporting units (SEIN) in a given state for a given firm. SEINs may or may not match the physical establishments identified in the LBD. Therefore, I take firm as the unit of analysis.

[12]80% of the 1990 federal minimum wage in 2001 dollars is equal to \$1923/quarter (=$0.8\times$\$3.8/hour $\times 40$ hours/week $\times$ 12 weeks/quarter $\times 1.318$).

[13]To limit the probability of data errors in the sample, I also drop all observations for individuals where wages change by extreme values in one quarter.

I then link the filtered employee data to firms in the LBD to construct firm-year-quarter-level measures of wage patterns and workforce compositions. Since self-employment may have different wage setting behavior, I exclude firms who only have one paid employee to minimize the possibility of picking up self-employment. I define all the variables used in my analysis, in more detail, in the Appendix.

1.2.3 Define Firm Industry and Finance Firms

Throughout the study, I use the 1987 Standard Industry Classification (SIC) codes to define markets. To define a firm's market (termed "firm industry"), I use establishment-level information on SIC and employment from the LBD.[14] For a firm owning only one establishment, its industry is defined by its establishment's industry. For a firm owning multiple establishments spanned multiple industries, the firm is classified in an industry where it has more than 50 percent of its employment. I drop a negligible percentage of firms who span in multiple industries, but their employment share in each industry is less than 50 percent. Following these rules, a firm's industry is defined by a 2-digit or 3-digit SIC code.[15]

Following Philippon and Reshef (2012), finance industries include depository institutions (except central reserve depository institutions), nondepository institutions, security and commodity

[14]Census uses SIC to define industries until 2002. Starting 1997, Census defines industries using the North American Industry Classification System (NAICS). To get SIC codes for post-2001 observations, I follow Babina (2016) to build a crosswalk between SIC and NAICS codes using LBD data between 1997 and 2001. However, the mapping between SIC and NAICS may not be a one-to-one mapping. Under the case of one NAICS code being matched with multiple SIC codes, I assign the NAICS code with the SIC code used by the most number of establishments to create a one-to-one mapping between NAICS and SIC. For example, NAICS 0001 is matched with SIC 111 for 100 establishments, but it is only matched with SIC 112 for 10 establishments. Then I assign establishments in post-2001 classified into NAICS 0001 with SIC 111. If there is a tie in the number of establishments under different SIC's, then I assign the NAICS code with the SIC with the highest employment.

[15]The first two digits of SIC codes indicate the major group, and the first three digits indicate the industry group. While 3-digit SIC is a more granular way of defining product markets, it may be too narrow for some large firms whose activities span over closely related but separate markets. For example, for a large insurance company like State Farm or GEICO, they may have a similar proportion of activities within SIC 631(Life insurance), SIC 632(medical serve and health insurance), SIC 633 (fire, marine and causality insurance) and SIC 639(insurance carriers). By using a 2-digit SIC classification, I increase the probability that large corporations are grouped together as competing firms in the same industry. Grullon, Larkin, and Michaely (2017) use a similar idea when they define firm industry codes by NAICS.

brokers, insurance carriers, insurance agents, brokers and service and holding and other investment offices. Non-finance industries include all other nonfarm private industries. For a corporate to be a finance firm, its industry must be in one of the finance industries. Similarly, a firm is classified as a non-finance firm if its industry is in one of the non-finance industries.[16] Based on this classification rules, financial firms account for approximately 4.3% of my baseline sample, which is similar to the statistics reported in the Statistics of U.S. Businesses (SUSB): 4.1% in 2000 and 4.27% in 2008.[17]

1.2.4 Industry Concentration and Firm Market Power

In this subsection, I construct the measures of industry concentration and firm market power using employment data from the LBD.

To measure the degree of industry concentration, I construct the Herfindahl-Hirschman Index (HHI) as the sum of the squared firm employment shares in industry j in year y:

$$\mathrm{HHI}_{j,y} = \sum_{f} (\frac{emp_{f,j,y}}{emp_{j,y}})^2 \tag{1.1}$$

where $emp_{f,j,y}$ is employment of firm f in industry j in year t. $emp_{j,y}$ is the total employment in industry j in year t. I construct two variants of the HHI measures using two- or three-digit SIC industry codes.

As the HHI's are measured using employment data for almost all private and publicly listed firms in the U.S., they can capture the degree of industry concentration more accurately than the ones constructed using publicly listed firms. Moreover, my measures of HHI cover a wider range of industries and years than the publicly available statistics reported in the Economic Census, where HHI is limited to manufacturing industries in calendar years ending in 2 or 7.[18]

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[16]To reduce the probability of misclassifying workers into finance industries, I drop a small proportion of firms which have employment in both finance and non-finance industries. In the unreported results, I find baseline findings are robust to including these firms.

[17]In the SUSB, a firm is classified into 2-digit NAICS sector in which it paid the largest share of its payroll. See more details at https://www.census.gov/programs-surveys/susb/technical-documentation/methodology.html

[18]See more details about concentration ratios published by Census at

I next define the market power for a firm classified into industry j as the ratio of total employment in firm f in industry j to total employment in industry j:

$$\text{MarketPower}^{E}_{f,j,y} = \frac{emp_{f,j,y}}{emp_{j,y}} \times 100 \tag{1.2}$$

where $emp_{f,j,y}$ is employment of firm f in industry j in year t. $emp_{j,y}$ is the total employment in industry j in year t. E represents this market power measure is constructed using employment data. I construct two variants of the firm market power measures using two or three-digit SIC codes to define industries.

A firm's market power can be interpreted as its ability in extracting rents. The measure of firm market power is constructed following the argument of Shepherd (1972) that market power is firm-specific and it depends on the firm's market share. Relative to other firms within the same market, firms with higher market share are expected to extract higher rents because they have the higher bargaining power to lower input prices, they can take advantage of economies of scale, or they have higher monopoly power in raising prices. This measure has been used by courts as a primary criterion to assess the existence of monopoly power in a specific product market because measuring market power using firm-level markup is notoriously difficult.[19] [20]

Measures constructed in this section are at annual-level since the LBD reports employment as of March 12th in each year. To create a quarterly panel, I then link these measures in the year y to quarterly measures of firm workforce composition and wage patterns in the first three quarters of year y and the last quarter of year $y - 1$.

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https://www.census.gov/econ/concentration.html

[19]See chapter 2 in "Competition and Monopoly: Single-Firm Conduct Under Section 2 of the Sherman Act" by Department of Justice for more details. Other examples mentioned in this article are: U.S. Anchor Mfg., Inc. v. Rule Indus., Inc., 7 F.3d 986, 999 (11th Cir. 1993) ("The principal measure of actual monopoly power is market share..."); Weiss v. York Hosp., 745 F.2d 786, 827 (3d Cir. 1984) ("A primary criterion used to assess the existence of monopoly power is the defendant's market share.").

[20]Measuring firm-level markup requires detailed information on product prices, quantities produced, characteristics of products and marginal costs for producing each additional unit. However, such detailed data is often not available, especially for non-manufacturing industries.

1.2.5 Summary Statistics

1. Trends in Concentration and Wages: Finance vs. Non-finance. Figure 1a plots trends of the Herfindahl-Hirschman Index(HHI) constructed by Equation (1) using 3-digit SIC. The computed HHI is averaged across industry-year cells within each of the six-year periods (the last period includes seven years, 2002-2008) using the number of employees in each cell as the weight. The average HHI can be interpreted as the degree of employer concentration the average worker faces in the finance or non-finance industries. On average, the employer concentration keeps increasing since 1990 in both finance and non-finance industries. Specifically, the average HHI concentration measure has increased by approximately 39.56% in finance, and by about 40.16% in non-finance.[21]

Figure 1b plots trends of average real wages computed in finance and non-finance industries. The computed average wage is averaged across firm-year-quarter cells within each of the six-year periods (the last period includes seven years, 2002-2008) using the number of employees in each cell as the weight. Figure 1b shows stronger growth in real wages in finance over 1990-2008: real wages on average have increased by 23.38% in finance, whereas the increase is only 8.95% in non-financial industries within the sample. By looking at changes, figure 1a and 1b show that the increase in finance wages is around 2.6 times than the increase in non-finance wages while the changes in concentration are similar in the financial and non-financial industries. These results indicate that industry concentration creates less wage dampening effect on finance wages.

2. Cross-sectional Summary Statistics. Table 1.1 reports summary statistics of firm-level variables from the baseline sample. Column 1 reports mean values along with standard deviation in parentheses calculated across all firm-quarter within the sample. Column (2) and (3) report mean values calculated for non-finance and financial firms respectively. The last column reports the difference between columns (3) and (2) along with statistical significance-level. Panel A reports summary statistics of firm-level wage patterns, and Panel B reports other firm characteristics including measures of firm market powers and workforce composition respectively.

[21] All observation counts and estimates are rounded according to Census disclosure policies.

Panel A shows that the quarterly average wage in finance is \$10914 within the sample, which is 24% higher than the one in non-finance. Moreover, the average wage of high-skilled workers is 34.16% higher in finance. Compared to existing literature, I document a lower finance excess wage due to:1) the calculated wage is averaged across workers from the LEHD. However, the coverage of the LEHD data used in this study only extends to 31 states, and thus I may underestimate average finance wages by excluding workers working in excluded states such as New York and Connecticut where excess wages paid by financial firms are even higher (Philippon and Reshef, 2012). 2) The frequency of my baseline sample is quarterly. The excess wages paid by financial firms, which is mainly driven by bonuses (Bell and Van Reenen, 2013), may be smoothed out by taking averages across quarters. 3) Based on my firm classification rules described in section 1.2.3, I drop firms spanning in both financial and non-financial industries. I also drop firms which are too diversified to be classified into one single industry. Applying these filters exclude some diversified and large firms which pay higher wages (Oi and Idson,1999). I discuss the consequences of these restrictions in Section 1.7.

Panel B shows that, on average, firm market power constructed by equation (2) using two-digit (three-digit) SIC industries is 0.002 percentage points (0.011 percentage points) with a standard deviation of 0.05 percentage points (0.193 percentage points) for all firms within the sample. Among financial firms, the average of their market powers in two-digit (three-digit) SIC industries is 0.003 percentage points (0.014 percentage points) which is slightly higher than the average of non-financial firms by 0.001 percentage points (0.003 percentage points).

Panel B also reports summary statistics of variables measuring firms' workforce composition, including the average of workers' education level, the average of working experience, the share of college-educated workers and the share of male workers. Financial firms on average hire a higher share of college workers and more experienced workers as compared to non-financial firms. Consistent with findings in existing literature, these results indicate finance is a high-skill industry. Interestingly, the share of male workers in financial firms is 31.39%, which is 24.74% lower than the share in non-financial firms. The fact that financial firms hire relatively less male employees on average is consistent with the labor force statistics reported by the Bureau of Labor Statistics.[22]

1.3 Empirical Analysis

1.3.1 Concentration and Average Wages

Trends presented in Figure 1 indicate that industry concentration affects finance wages differently, but the difference can be driven by other factors varying across industries. This subsection conduct a cross-sectional analysis to estimate the difference between finance and non-finance in the correlation of concentration and average wages with controlling for other observed and unobserved factors. At one extreme, firms in more concentrated industries may extract higher rents by exercising product market monopoly power, and wages should rise when firms share these rents with workers (rent-sharing effect). Alternatively, workers in a more concentrated industry may face fewer outside options in the industry. In this case, firms have labor market monopsony power to lower wages (labor monopsony effect). The relationship between industry concentration and wages depends on the relative strength of rent-sharing effect and labor monopsony effect in a given industry.

To examine the relation between industry concentration and average wages, I estimate the following cross-sectional regression:

$$\text{logWage}_{f,j,t} = \alpha_t + \gamma_1 \text{HHI}_{j,t-4} + \gamma_2 \text{FIN}_f + \gamma_3 \text{FIN}_f \times \text{HHI}_{j,t-4} + \text{X}^{'}_{f,j,t-4}\beta + \epsilon_{f,j,t} \tag{1.3}$$

where $logWage_{f,j,t}$ is the log of average wages in firm f, operating within industry j, in year-quarter t. $HHI_{j,t-4}$ is the four-quarter lagged measure of concentration in industry j; FIN_f is equal to 1 if the firm is a finance firm. $X_{f,j,t-4}$ is a vector of firm-level control variables in four-quarter lags comprising the log of average worker education level, the share of male workers, the share of college workers, the log of average working experience and the log of firm age. All regressions include year-quarter fixed effect, α_t, to control for macro-level trends in affecting wages. To control for potential time-series dependence in the residuals, I cluster the standard errors at the

[22]See more at https://www.bls.gov/cps/cps_aa2002.htm

industry-level.

Table 1.2 presents the results from estimating Equation (3). Column (1) shows that, on average, wages in private sectors are negatively correlated with industry concentration measured by Herfindahl- Hirschman Index (HHI) using two-digit SIC industry codes. The negative relationship is still statistically significant at the 1% level after controlling for firm workforce composition and firm age which may affect firms' productivity and thus affect wages (column (2)). While there is a negative relationship between wages and industry concentration in non-finance, column (3) shows the relationship is positive in finance and significant at the 1% level after controlling for firm workforce composition and firm age. Precisely, a one standard deviation increase in HHI is associated with 13.78% (=0.011 $\times$ (-3.917+16.44) $\times$100) higher average wages in finance. Column (4) shows results are robust to HHI defined using three-digit SIC industry codes.

For robustness, I follow Grullon, et al (2018) and use total number of firms in a given two-digit SIC industry as a proxy of industry concentration. An industry is more concentrated when fewer firms remain in the industry. Consistently, column (5) shows that a decrease in the number of firms is associated with lower average wages in non-finance, whereas it is associated with significantly higher average wages in finance. In sum, these results indicate industry concentration is associated with stronger rent-sharing effect in finance.

1.3.2 Firm Market Power and Finance Wages

1.3.2.1 Main Result

While industry concentration means higher market power for firms remaining in the industry on average, previous studies show that concentration disproportionally benefits larger players within industries (Shepherd, 1972; Porter, 1979; Gale, 1972). Relative to small players in a given industry, larger ones are expected to yield higher rents for the following reasons. First, larger players' products have share-based product differentiation advantage in the sense that their products are widely advertised and recognized. Second, larger players can take advantage of economies of scale to achieve a cost advantage over rivals operating at a lower rate of output. Lastly, larger players have higher bargaining power to lower input prices or setting product prices to reflect their own interests. Therefore, market power should be firm-specific and dependent on the firm's market share. Firms with higher market power can extract higher wages as compared to firms with lower market power. Using this within-industry variation in this subsection, I examine the relationship between firm-specific market power and average wages to better identify the marginal difference in the treatment of firm market power on finance wages.

To visualize the relationship between firm market power and wages, I conduct a flexible estimation of the following equation for finance and non-financial firms separately:

$$\text{logWages}_{f,j,t} = \alpha_t + \gamma_1 \text{D}^{2nd}_{f,j,t-4} + \gamma_1 \text{D}^{3rd}_{f,j,t-4} + \gamma_1 \text{D}^{4th}_{f,j,t-4} + \text{X}'_{f,j,t-4}\beta + \epsilon_{f,j,t} \tag{1.4}$$

where $logWages_{f,j,t}$ represents the log of average wages at firm f, operating within industry j, in year-quarter t. $D^{2nd}_{f,j,t-4}$, $D^{3rd}_{f,j,t-4}$, or $D^{4th}_{f,j,t-4}$ are equal to 1 if the firm f's market power in year-quarter $t-4$ is respectively in the second, third or fourth quartile of firm market power distribution within the sample, where firm market power is constructed by equation (2) based on two-digit SIC industry classification. $X_{f,j,t-4}$ is a vector of workforce composition variables in 4-quarters lags comprising the log of average worker education level, the share of male workers, the share of college workers, and the log of average working experience.

I plot the coefficients of $D^{2nd}_{f,j,t-4}$, $D^{3rd}_{f,j,t-4}$, and $D^{4th}_{f,j,t-4}$ from estimating equation (4) in Figure 2a. Overall, the relationship between firm market power and wages appears to be convex and the slope is significantly steeper in finance. Specifically, workforce composition adjusted wages (wage premium) paid by financial firms with high market power (in the fourth quartile) is about 33% higher than financial firms with low market power (in the first quartile), whereas non-financial firms with high market power only pay about 3.69% higher. The high wage premium paid by financial firms with market power above the third quartile indicate that the firms with high market power drive up the average of finance wages.

I next measure the sensitivity of wages to firm market power in finance and non-finance by estimating the following equation:

$$\text{logWages}_{f,j,t} = \alpha_t + \gamma_1 \text{MarketPower}^E_{f,j,t-4} + \gamma_2 \text{FIN}_f + \gamma_3 \text{FIN}_f \cdot \text{MarketPower}^E_{f,j,t-4} + \text{X}'_{f,j,t-4}\beta + \epsilon_{f,j,t} \quad (1.5)$$

where $MarketPower^E_{f,j,t-4}$ is the market power of firm f, operating mainly in industry j (two- or three-digit SIC) in year-quarter $t-4$ and firm market power is defined by equation (2). The other variables are defined as the same as in equation (4). Standard errors are clustered at firm-level.

Table 1.3 reports results of estimating equation (5). Column (1) confirms that financial firms pay 19.8% higher on average than non-finance private sector when controlling for unobserved macro trends. I next add in my main variable of interest: $MarketPower^E$ constructed based on two-digit SIC codes, and I also controls for firm workforce compositions which may drive the heterogeneity in productivity and wages across firms. [23] Column (2) reports the results. Overall, there is a significant and positive relationship between firm market power and average wages: a one standard deviation(0.05 percentage points) increase in firm market power is associated with 0.62% $(= 0.05 \times 0.123 \times 100)$ higher wages on average. In column (3), I measure to what extent the sensitivity of average wage to the change in firm market power is different in finance by including the interaction of finance firm dummy and firm market power. The estimation results show that a one standard deviation increase in firm market power is associated with 2.64% $(= 0.05 \times (0.112 + 0.415) \times 100)$ higher average wages in finance, whereas the effect is only 0.56% $(= 0.05 \times 0.112 \times 100)$ in non-finance industries. The difference in the effect of firm market power on finance wage is significant at the 5% level. Column (4) shows that results also hold when I include finance-by-time fixed effects to control for time-varying differences across financial and non-financial industries.

To make sure that the positive relationship between firm market power and wages is not driven by the facts that more established firms possess higher market power and established firms pay higher wages (Dunne and Roberts, 1990a; Brown and Medoff, 2003), I add the log of firm age as

[23] Column (2) shows financial firms pay 21.9% higher on average than non-financial firms. Compared with the result reported in column (1), the excess wage paid by financial firms is even higher after controlling for $MarketPower^E$ and firm workforce composition. The increase in the excess wage paid by financial firms is mainly driven by the facts that financial firms have lower share of male workers and wages are positive correlated with share of male workers.

an additional control and report results in column (5).[24] While more established firms pay higher wages on average, the coefficients of $MarketPower^E$ and its interaction with finance firm dummy are similar the ones reported in column (3).[25]

In column (6), I replicate the specification as in column (3) but redefine $MarketPower^E$ using three-digit SIC codes to address the concern of the coarseness of defining product market using two-digit SIC codes. Results are consistent with earlier findings. Specifically, the sensitivity of wages in finance to the change in market power is about 2.4 times (=(0.0543+0.0381)/0.0381) higher than that in non-finance. This finding to some extent mirrors what I observe in Figure 1: while there is a similar increase in market power caused by concentration in finance and non-finance, the increase in finance wage is around 2.6 times higher than the increase in non-finance.

In sum, my results show that a higher firm market power is associated with significant higher wages in finance as compared to non-finance.

1.3.2.2 Other Measures of Wages

1. Median Wage. To make sure that the marginal difference in the treatment of firm market power for finance and non-finance wages is not solely driven by top earners at high market power financial firms being disproportionally benefited, I repeat the specification as in column (5) of Table 1.3 but take the logarithm of median wages as the dependent variable. Column (1) in Table 1.4 shows that a one standard deviation in firm market power is associated with 1.32%($= 0.05 \times (0.0686 + 0.195) \times 100$) higher median wages in finance, whereas the effect is only 0.34% ($= 0.05 \times 0.0686 \times 100$) in non-finance industries. The marginal difference in the treatment of firm market power for finance and non-finance median wages is lower than that for average wages (column (5) of Table 1.3), but it is still statistically and economically significant.

2. Wages Adjusted for Cost of Living. Financial firms, especially the ones with high market

[24]Firm age is defined as the oldest establishment that the firm owns in the first year the firm is observed in the LBD (Haltiwanger, Jarmin, and Miranda, 2012).

[25]One concern of measuring firm market power at national-level is that it may underestimate firms' labor monopsony power because job search is largely local (Moretti, 2011; Molloy, et al., 2014) and the lack of labor mobility lower firms' incentives in sharing rents. Firm-local labor market-level Results presented in Section 1.6.1 can alleviate this concern.

power, cluster in regions with high cost of living. This raises a question that whether these firms pay higher wages to compensate for high cost of living. To test this, I adjust worker wages for state-level price index using the methodology provided by the Bureau of Economic Analysis and then aggregate to firm-level by taking the average across adjusted individual wages.[26] Then I use the logarithm of the average wages adjusted for cost of living as the dependent variable and repeat the specification as in column (5) of Table 1.3. Column (2) of Table 1.4 reports the result. The result is qualitatively and quantatively similar to what reported in column (5) of Table 1.3. The consistency alleviates the concern that earlier findings are driven by the heterogeneity in cost of living across regions.

3. Male and Female Wages. In this section, I investigate whether a change in firm market power disproportionally affects average male or female wages in finance. If financial firms are able to discriminate against female workers by disproportionally sharing rents to male workers, the treatment of firm market power for male wages should be significantly higher than that for female wages in finance. To investigate this, I estimate equation (5) using the log of average wages of male or female at a given firm as the dependent variable. Results are reported in column (3) and (4) of Table 1.4. In finance, a one standard deviation increase in firm market power is associated with 1.99% ($= 0.05 \times (0.362 + 0.0354) \times 100$) increase in male wages and 2% ($= 0.05 \times (0.098 + 0.302) \times 100$) increase in female wages, and the difference is not statistically significant. Therefore, the main result of this paper is not unique to a specific gender in finance.

1.4 Exploring the Mechanism

This section explores mechanisms that may explain why market power is associated with relatively higher wages in finance. In this paper, I argue that market power is associated with two competing effects: labor monopsony effect and rent-sharing effect. In finance, the rent-sharing effect dominates labor monopsony effect and the net effect is relatively stronger. To support this argument, I provide evidence on two non-mutually exclusive mechanisms: 1) financial firms with

[26]To adjust wages in all sample years, I use state-level price index in 2008, which is the first year the index is available. See more details about the methodology of adjusting wages for state-level price index at https://www.bea.gov/sites/default/files/methodologies/RPP2016_methodology.pdf

higher market power extract relatively higher rents; 2) financial firms have to give a higher proportion of rents to their workers, especially high-skill ones, due to higher worker wage bargaining power.

1.4.1 Evidence on Higher Rents in Finance

With higher market power, financial firms may be able to make higher rents in several ways. First, it may be relatively easier for financial firms with higher market power to increase the complexity of their products, which is positively correlated with the hidden markup in the products (Celerier and Vallee, 2017). Second, the strong belief of "too big to fail" in finance suggests that firms with higher market power are more likely to be supported by the government when they face potential failure, and thus they may be able to borrow at a much lower cost than smaller financial firms who must borrow based on their creditworthiness (e.g., Baker et al., 2009; Ahmed et al, 2015). Lastly, it may be relatively easier for financial firms to improve operational efficiency and achieve economies of scale. For example, financial firms may be able to allocate resources or apply technology more efficiently because their production process is less geographically restricted, and thus they can generate higher revenue from each unit of assets. Under these scenarios, higher market power should be associated with relatively higher profitability in finance as compared to non-finance, and financial firms pay higher because they have more profit to share. To test this mechanism, I estimate the following equation:

$$\mathrm{ROA}_{f,j,t} = \alpha_t + \gamma_1 \mathrm{MarketPower}^E_{f,j,t-4} + \gamma_2 \mathrm{FIN}_f + \gamma_3 \mathrm{FIN}_f \cdot \mathrm{MarketPower}^E_{f,j,t-4} + \mathrm{X}'_{f,j,t-4}\beta + \epsilon_{f,j,t} \tag{1.6}$$

where $ROA_{f,j,t}$ is the return on assets of firm f operating in industry j in year-quarter t. I follow Grullon et al. (2017) to use ROA as a proxy for profitability because Barber and Lyon (1996) argue that ROA is superior to other measures of profitability in detecting abnormal operating performance. $X_{f,j,t-4}$ is a vector of firm-level control variables in 4-quarters lags comprising the log of average worker education level, the share of male workers, the share of college workers, the log of average working experience and the log of firm age. Standard errors are clustered at firm level.

To conduct the analysis, I extract a sample of publicly listed firms from the baseline sample as constructed in section 1.2.2, and match these firms with their financial statement data from

Compustat through a Census internal LBD-Compustat bridge.[27]

I next estimate equation (7) using the new sample to test whether a higher firm market power allows financial firms to create relatively higher profitability. Results are reported in Table 1.5. Consistent with my expectation, I find a positive relationship between firm profitability and firm market power in both finance and non-finance industries. This positive correlation is more pronounced in finance. Specifically, column (1) shows that a one standard deviation (0.893) increase in firm market power is associated with 0.029 ($= 0.893 \times (0.0087 + 0.0242)$) higher ROA within finance, but only 0.0078 higher ROA within non-finance and the difference between finance and non-finance is statistically significant at the 1% level. I add the log of firm age as a control in the specification as in column (2) because firms may have a better understanding of their production functions when they get more established. I continue to find 0.024 higher in ROA within finance, and the difference in the effects of market power on ROA between finance and non-finance is statistically significant at the 5% level.

To further investigate the sources of the additional profitability in finance, I follow Grullon et al. (2017) to decompose ROA into two components: the Lerner Index and the Asset Utilization ratio. Following Aghion et al. (2005), the Lerner Index is defined as operating income after depreciation scaled by total sales. Depreciation is excluded from operating income to take into account the cost of physical capital (Hall and Jorgenson,1967). This index approximates the extent to which prices exceed marginal costs (price-cost margins). The Asset Utilization ratio is defined as the ratio of total sales to total assets, which measures the firms' efficiency in utilizing assets to generate sales.

[27] Appendix Table A.1 reports the summary statistics of key variables from the sample. Interestingly, the average wage in finance is 3.29% lower than the average wage in other industries within this sample. This wage discount may be explained by lower share of male workers at financial firms and by excluding a big proportion of private hedge funds and private equity firms which pay significantly higher wage premium (Philippon and Reshef, 2012). In unreported results, within the sample, I find financial firms on average pay higher than non-financial firms do after controlling for the share of male worker. Moreover, as public listed firms are larger on average in terms of employment, firms in this sample possess higher market power on average within their industries. Lastly, the mean of ROA in finance is 0.04, which is lower than the ROA in non-finance by 0.058. The finding that the financial sector has lower profitability is consistent with Grullon et al. (2017). As the new sample is slightly different from the sample used in my baseline analysis, I start with replicating equation (5) using the new sample to validate my earlier findings. Results are reported in Appendix Table A.2. Consistent with earlier findings, I find a higher firm market power is associated with significantly higher wage in finance as compared to non-finance.

Then I re-estimate equation (6) using the Lerner Index and the Asset Utilization ratio as dependent variables. Table 1.6 reports results. Overall, I find firms with higher market power are associated with higher Lerner Index in non-finance, and the positive relationship is significantly stronger in finance. Meanwhile, I find a positive correlation between market power and Asset Utilization ratio on average. However, the positive effect of market power on the Asset Utilization ratio is not significantly higher in finance as compared to non-finance in all specifications.

In sum, results from this section support the hypothesis that financial firms with higher market power are associated with relatively higher profitability due to higher price-cost margins, and thus financial firms have more rents to share with their workers relative to non-financial firms.

1.4.2 Evidence on Higher Bargaining Power in Finance

Financial firms may have to give a relatively higher share of rents to employees because their employees have relatively higher wage bargaining power. This mechanism should apply primarily to high-skilled workers for the following reasons: first, a high-skilled worker in finance is more likely to be matched to a larger project than in other industry (Celerier and Vallee, 2017). The high scalability makes it crucial that financial workers take sufficient care of their work and get paid more.[28] It is plausible to expect financial firms to give a relatively higher share of their rents to high-skilled employees to compensate for the high scalability, and thus wages of high-skilled workers should be more sensitive to the change in market power within finance.

Second, relative to non-finance workers, finance workers are closer to the final products such that their performance are directly linked to employers' performance. Due to this reason, the costs for rivals to evaluate workers are relatively lower and the probability of workers being poached by rivals is relatively higher in finance. Financial firms may need to share a higher fraction of rents with their workers to retain workers, and thus wages in finance should be more sensitive to the change in market power. Finance high-skill workers have even higher external visibility relative to high-skill workers in non-finance. For example, *Institutional Investor* conducts an annual poll

[28]For example, Kaplan and Rauh (2010) estimate that the average partner in U.S. private equity firms oversaw about $430 millions of funds in 2004.

among financial firms and reports the ranking of top money managers, analyst leaders or other roles from different financial firms.[29] Due to relatively higher scalability and external visibility, wages of finance high-skill workers should be even more sensitive to firm market power changes.[30]

To examine this mechanism, I first re-estimate equation (4) with the log of average wages of high-skilled workers as the dependent variable. As the baseline measure, high-skilled workers are individuals whose earnings are above the 90th percentile of the wage distribution in the firm-year-quarter.[31]. Figure 2b plots estimation results. Compared to Figure 2a, wage premium at each quartile of firm market power over the first quartile is relatively higher in both finance and non-finance, indicating average wages of high-skilled workers are more sensitive to the change in market power. The steeper slope in finance suggests that the positive relationship between average wages of high-skill workers and firm market power is more pronounced in finance.

I then repeat the specifications as in Table 1.3 using the same sample with the log of average wages of high-skilled workers as the dependent variable to quantify the marginal difference in the effect of firm market power on high-skill workers' wages in finance. Results are reported in Table 1.7. Column (1) shows that, on average, a one standard deviation increase in firm market power is associated with 1.83% $(= 0.05 \times 0.365 \times 100)$ higher average wages of high-skill workers. Column (2) shows that this positive relationship is significantly stronger in finance. Specifically, a one standard deviation increase in firm market power is associated with 6.37% $(= 0.05 \times (0.34 + 0.933) \times 100)$ higher average wages of high-skill workers in finance, whereas the effect is only 1.7% $(= 0.05 \times 0.34 \times 100)$ in non-finance industries. Given the mean of wages of high-skill workers

[29]See more at ttps://www.institutionalinvestor.com/research.

[30]People may wonder whether financial firms give a higher share of rents to their workers due to higher unionization rates relative to non-financial firms. While data limitation does not allow me to provide firm-level evidence, I find unionization rates are substantially lower in finance based on statistics from the Union Membership and Coverage Database constructed using CPS. This database has been widely used by other literature on labor economics, such as Matsa (2010), Benmelech, et al. (2018). For example, 9.6% of workers in private non-farming sector are covered by labor unions in 2000, whereas only 1.8% in finance are covered.

[31]Although the education level can be a measure of skill level, it may have different meaning across different occupations or generations of workers (Phillippon and Reshef,2012). In unreported results of robustness checks, I find similar results when defining high-skill workers as workers with at least 16 or 18 years of education, or workers with wages above the 95th or 99th percentile of the within-firm wage distribution.

in finance is \$23050 per quarter within the sample, an increase of 6.37% can be translated into a \$1468.3 ($= 0.0637 \times 23050$) increase in the average quarterly wage of finance high-skill workers. I find similar results in column (3) and (4) where I define firm market power using three-digit SIC.

People may question whether financial firms share a higher share of rents to high-skill workers because they have relatively higher managerial power to extract higher rents without improving firm performance. Under this hypothesis, I should observe financial firms with higher market power perform worse than non-financial firms. However, results in section 1.4.1 do not support this prediction because firms with higher market power are associated with relatively higher profitability within finance.

1.5 Firm Market Power and Within-firm Inequality

Results from the last section indicate that rents are disproportionally distributed to high-skill workers. Thus, it is plausible to expect that higher market power is associated with higher within-firm inequality. In Table 1.8, I repeat the specifications as in Table 1.3 with the log difference of average top 90th percentile wages and average bottom 10th percentile wages as the dependent variable. Column (1) shows that a one standard deviation increase in market power is associated with 6.66% higher within-firm pay inequality in finance, whereas the effect is only 1.94% in non-finance. The difference between finance and non-finance is significant at the 5% level. Column (2) shows results are robust to defining industries using 3-digit SIC codes.[32]

Interestingly, the relationship between firm market power and inequality is not unique to a specific gender in both finance and non-finance. Column (3) (column (4)) in Table 1.8 shows the results where the 90th to 10th wage ratio of male (female) workers is the outcome variable. Higher market power is associated with higher inequality within a given gender on average, and the positive relationship is significantly stronger in finance. Even though higher inequality is found among both male and female workers at financial firms with higher market power, the effect is more pronounced for inequality among male workers than among female workers in finance. Specifically,

[32] In unreported results, I found qualitatively similar results when I measure within-firm pay inequality by the standard deviation of wages within a given firm-year-quarter.

in finance, a one standard deviation increase in firm market power is associated with 9.35% higher pay inequality among male workers, whereas it is associated with 5.63% higher pay inequality among female workers. This result suggests that rents in finance are disproportionally distributed to high-skill male workers.

1.6 Additional Robustness Checks

1.6.1 Local Market Power

One alternative explanation for the difference between finance and non-finance in the sensitive of wage patterns to firm market power measured at national-level is: some non-financial firms, such as restaurants and health care services, compete locally. These firms may be able to extract high rents without a high market share in national markets. Measuring market power within national-level markets may underestimate these firms' ability in extracting rents, and thus wages paid by these firms are not sensitive to firm market power measured across a national-level market.

To test this explanation, I examine the treatment of firms' market power in local markets for their local wages and how it is different for finance wages. To construct the sample, I select workers from LEHD following the same rules discussed in Section 1.2.2. I then aggregate worker-firm-commuting zone level data to get wage patterns and workforce compositions at firm-commuting zone-quarter level.[33] For a firm mainly operating in industry j (two-digit SIC), its market power in commuting zone (CZ) c is defined as its employment share in industry j-CZ c:

$$\text{MarketPower}^{L}_{f,j,y} = \frac{emp_{f,c,j,y}}{emp_{c,j,y}} \times 100 \tag{1.7}$$

where $emp_{f,c,j,y}$ is the total employment of firm f in CZ c-industry j in year y. $emp_{c,j,y}$ is the total employment in the CZ c-industry j in year y. I link this measure in year y to quarterly

[33]Commuting zones are clusters of U.S. counties that are characterized by strong within-cluster and weak between-cluster commuting ties. See details about commuting zones at David Dorn's website: https://www.ddorn.net/data/Dorn_Thesis_Appendix.pdf. LEHD provides county codes for workers' employers. LBD provides county codes for firms' establishments. Commuting zones are mapped to counties through the crosswalk constructed by Dorn(2009).

measures of firm-CZ workforce composition and wage patterns in the first three quarters of year y and the last quarter of year $y-1$.[34]

I implement the following specification to estimate the marginal difference in the effect of firm local market power on local wage patterns of financial and non-financial firms:

$$\mathrm{y}_{f,c,j,t} = \gamma_1 \mathrm{MarketPower}^L_{f,c,j,t-4} + \gamma_2 \mathrm{FIN}_f + \gamma_3 \mathrm{FIN}_f \cdot \mathrm{MarketPower}^L_{f,c,j,t-4} + \mathrm{X}^{'}_{f,c,j,t-4}\beta + \alpha_t + \tau_c + \epsilon_{f,c,j,t} \tag{1.8}$$

where $MarketPower^L_{f,c,j,t-4}$ is firm f's market power in CZ c-industry j in year-quarter $t-4$. FIN_f is equal to 1 if firm f is in finance. $X_{f,c,j,t-4}$ is a vector of firm-CZ workforce composition variables in 4-quarters lags comprising the log of average worker education level, the share of male workers, the share of college workers, and the log of average working experience. α_t represents year-by-quarter fixed effects. τ_c represents CZ fixed effects. Standard errors are double clustered at firm and CZ level.

Table 1.9 reports results. Panel A examines average wages as the outcome variable. Column (1) of Panel A shows a one standard deviation (4.81 percentage points) higher in local market power is associated with 1.95% higher average local wages in non-finance, whereas the effect of local market power is 0.16% higher in finance. In column (2) of Panel A, I control for finance-by-CZ-by-time fixed effects to absorb unobservable industry shocks in local markets. The coefficient of interaction of FIN and $MarketPower^L$ shows that the treatment of local market power for firms' local wages is 1.7% higher in finance than that in non-finance and the difference is significant at 1% level.

Panel B repeat specifications in Panel A, but examine the average wages of high-skilled workers at firm-CZ-level as the outcome variables respectively. I continue to find more pronounced effects of firm market power on wages of high-skilled workers within finance, and the differences between finance and non-finance are significant at 1% level.

In sum, finance average wages and average wages for high-skill workers are more sensitive to

[34] Appendix Table A.3 reports the summary statistics of key variables in the firm-CZ-level sample.

the change in local market power than non-finance ones. This result alleviates the concern that the high sensitivity of finance wages is driven by firm market power defined across national market underestimating the ability of non-financial firms who compete locally to extract rents.

An additional concern of measuring firm market power at national-level is that it may underestimate firms' labor monopsony power because job search is largely local (Moretti, 2011; Molloy, et al., 2014) and the lack of labor mobility lower firms' incentives in sharing rents. The significantly positive relationship between local market power and average wages alleviates concern.

1.6.2 Measuring Market Power using Sales

Using occupational data from 1990, 2000 and 2010 ACS and occupation offshorability score from Autor and Dorn (2013), I find that jobs in finance industry have the highest offshorability on average.[35] For this reason, financial firms may be able to extract higher rents by offshoring jobs and pay higher wages to domestic workers with a smaller increase in their employment share in the domestic market, which is the baseline measure of firm market power. To make sure that my results is not explained by the possibility that firm market power measured using domestic employment captures this feature in finance, I re-define firm market power as the ratio of total sales of firm f to total sales of firms in the sample classified in industry j:

$$\text{MarketPower}^{S}_{f,j,y} = \frac{sales_{f,y}}{sales_{j,y}} \times 100 \tag{1.9}$$

where $sales_{f,y}$ is total sales of firm f in the year y from the BR.[36] $sales_{j,y}$ is the summation of sales of firms in the sample classified in industry j in year y, where industry is defined by two- or three-digit SIC codes. S represents this market power measure is constructed using sales data. I then link this measure in the year y to quarterly measures of firm workforce composition and wage patterns in the first three quarters of year y and the last quarter of year $y - 1$.

[35]See the average offshorability of jobs in each industry in Appendix Table A.4

[36]The BR reports sales data at EIN-year level. To obtain firm-year-level sales, I match the BR with LBD on EIN-year. Some firms may have multiple EINs for tax filing purposes, and I sum up EIN-year-level sales to firm-year-level under this case.

I then estimate equation (5) but replace firm market power measured using employment by $MarketPower^S$. Table 1.10 reports the estimation results.[37] Market power used in Panel A is calculated using two-digit SIC industries while the one used in Panel B is calculated using three-digit SIC industries. Overall, I find results which are consistent with the earlier findings: firms with higher market power measured by firm sales exhibit higher average wages and higher wages of high-skill workers, and these positive linkages are significantly stronger in finance industries. The coefficients of the interaction between finance dummy and market power in Panel A show that, within finance, a one standard deviation (0.041 percentage points) higher in market power measured by sales is associated with additional 13.08% higher average wages and 28% higher average wages of high skill workers in finance.[38]

1.6.3 Import Competition

Some non-financial industries may face relatively higher import exposure than financial industries, for example manufacturing and wholesale trade which take a large proportion of non-finance firms. Measuring market power using only the domestic contributions to the market may understate the degree of competition faced by firms in these industries, and this may explain why higher market power is associated with a smaller increase in rents and wags in non-finance relative to finance.

One way to examine this alternative explanation is by adjusting firm market power for import

[37]It is worth noticing that large firms generally report sales data based on their fiscal calendar such that they may not have sales data ready by the time Census collect the data. Also, some multiunit firms do not file separate tax forms for each establishment location. For these reasons, sales data for a proportion of large firms are missing (DeSalvo et al., 2016), and I have to limit the sample to a subset of firm-year-quarters from my baseline sample. Appendix Table A.5 reports summary statistics of key variables from this new sample. Within this sample, I find average wages at financial firms on average are 26% higher the ones at non-financial firms between 1990-2008. The average of financial firms market powers calculated using two-digit (three-digit) SIC industries is 0.007 percentage points (0.03 percentage points) which is higher than the average of non-financial firms by 0.004 percentage points (0.008 percentage points). While this sample only includes about 60% of firm-year-quarters from the baseline sample, in unreported results, I find similar industry distributions and statistics of firm-level measures of workforce composition in these two samples.

[38]Compared to the measure of market power calculated using employment and 2-digit SIC codes, the average of market power measured using sales in finance is much higher (about 2 times) than the one in non-finance. And the standard deviation of market power in finance is smaller than the one in non-finance indicating that finance industries are more likely to be dominated by fewer firms who have high market shares. The low variation of finance market power is one potential reason to explain the large magnitudes of coefficients of the interaction between finance dummy and market power found in Panel A.

exposure. However, to my knowledge, the existing measures of import exposure are not available for non-manufacturing industries due to the complexity of measuring import value. Instead, I repeat the specification as in column (5) of Table 1.3 but omit firms in construction, transportation and public utilities, and services as the reference group. The intuition is that these industries should face lower import exposure as compared to other non-finance private sectors, including manufacturing, mining, and wholesale and retail trade. The alternative explanation would hold if the positive effect of firm market power on wages in finance is no longer higher than that in the new reference group. In contrast, results presented in Table 1.11 show that firm market power is associated with higher average wages and higher wages for high-skill workers in finance relative to those in construction, transportation and public utilities, and services. The magnitude of the difference is 2.78% ($= 0.05 \times 0.555 \times 100$) and significant at 1% level.

Table 1.11 also shows that, on average, average wages and average wages for high-skill have the highest sensitivity to firm market power in finance as compared to the ones in other one-digit SIC sectors. This result highlights the distinctive role of market power in finance.

1.6.4 Sorting Effect Based on Worker Characteristics

The difference between finance and non-finance wages reported in Table 1.3 could be explained by sorting based on individual characteristics. Financial firms employing workers with higher quality would be expected to have higher market power and also pay higher wages, leading to a potential upward bias in the measured effect of firm market power in finance in cross-sectional analysis that compare different firms at a given time. To address this concern, I construct a employee-employer matched panel data set, where I can track workers across firms over time.[39] This allows me to estimate the marginal difference in the treatment of firm market power for finance and non-finance wages with controlling for time-varying and time-unvarying worker quality. Specifically, I estimate:

[39] To construct the sample, I apply the same filters discussed in Section 1.2.2 to select workers from LEHD database, and also require each individual to be observed at least twice. To minimize the computing requirements of a large sample size, I only keep the wage paid in first quarter of each year for each worker.

$$\text{logWages\_ind}_{i,f,y} = \phi_i + \gamma_1 \text{MarketPower}^E_{f,j,y-1} + \gamma_2 \text{FIN}_f + \gamma_3 \text{FIN}_f \cdot \text{MarketPower}^E_{f,j,y-1} + \text{X}'_{i,f,y}\beta + \epsilon_{i,f,y} \tag{1.10}$$

where $logWages\_ind_{i,f,y}$ represents the log of individual i's quarterly wage at firm f in year y. ϕ_i represent individual fixed effects, which absorb the time-unvarying differences among workers in observed and unobserved characteristics. $MarketPower^E_{f,j,y-1}$ is the market power of firm f, operating mainly in industry j (two-digit SIC) in year $y-1$. FIN_f is equal to 1 if firm f is in finance. $X_{i,y}$ is a vector of time-varying controls, including year dummies interacted with education dummies, and function of worker age interacted with education dummies. Standard errors are clustered at firm level.

The results are reported in Table 1.12. In column (1), I only control for time fixed effect and time-variant worker characteristics. Similar to firm-level results, wages in finance are on average 18.2% higher than those in non-finance. Column (2) includes firm market power and its interaction with finance dummy as additional controls. Consistent with the firm-level cross-sectional analysis, I find higher firm market power is associated with significantly higher wages in finance than in non-finance within the individual-level sample. In column (3), I control for worker fixed effects to absorb unobserved variation in worker quality. The difference in the treatment of firm market power for individual wages stays statistically significant and positive. This result is robust to controlling for state-by-year fixed and industry-by-year fixed effects, which absorb changes in state-level policies and fluctuations in industries respectively. Documenting consistent results alleviates the concern that the marginal difference in the treatment of firm market power on finance and non-finance wages is driven by the sorting based on worker quality.

1.7 External Validity

In this section, I discuss the generalizability of the results presented in this paper. The LEHD data used in this study only cover 31 states. Given that financial firms are clustered in omitted states like New York and Connecticut in which financial firms pay even higher wage premium

(Philippon and Reshef, 2012), one might question whether my results can be generalized to represent the overall finance sector. I provide two sets of evidence to mitigate this concern. First, I find similar results when I look at average per worker pay ($Wage\_lbd$) calculated using firm total payroll and total employment data from LBD (Appendix Table A.6). Since the LBD covers payroll and employment of all establishments in the U.S., this measure of average wage does not suffer the problem of excluding workers who work for the same firm but located outside of the 31 states in my sample.

Second, in the unreported results, I also find higher market power is associated with significantly higher $Wage\_lbd$ within finance relative to non-finance using all firms from the LBD without controlling for firm workforce composition.[40] Moreover, I find the excess wage paid in finance within the sample of all firms from the LBD is much higher than the one I observed in my baseline sample (24%), and it is very comparable to the one documented in (Philippon and Reshef, 2012). These results suggest that excluding workers located outside of my 31 states underestimate the finance wage premium, but it does not drive the relation between firm market power and wages.

For a firm in multiple industries, I classify it to the industry in which the firm has at least 50% of its employment. This rule implicitly assumes a change in firm market power would have the same effect on the wages of workers at establishments in different industries but belonging to the same firm. To alleviate the concern that my earlier findings are driven by classifying some non-finance workers into finance industries, I replicate equation (5) using establishment-year data on average wages and industry from LBD. While I cannot control for workforce compositions due to data limitation at establishment-level, I control for firm fixed effects to absorb unobservable time-invariant firm quality. The identification is achieved using within-firm variation, comparing multiple establishments belonging to the same firm but different industries. In the untabulated results, I find the positive relationship between firm market power and average wages is still significantly stronger in finance within this establishment-year sample.

[40] Information on workforce composition are from LEHD, so they are not available for firms which are in the LBD but cannot be matched with LEHD.

1.8 Conclusion

Increasing industry concentration in the U.S. has raised concerns that declining competition in the labor market has led to slow wage growth. While this linkage holds generally, this paper shows the finance sector has been an exception. In this paper, I explore *how* and *why* market power affects financial firms' wage-setting behavior differently from non-financial firms. Given the size of the unexplained wage premium in finance and how it has contributed to income inequality, it is critical to understand why financial firms are unique in setting wages.

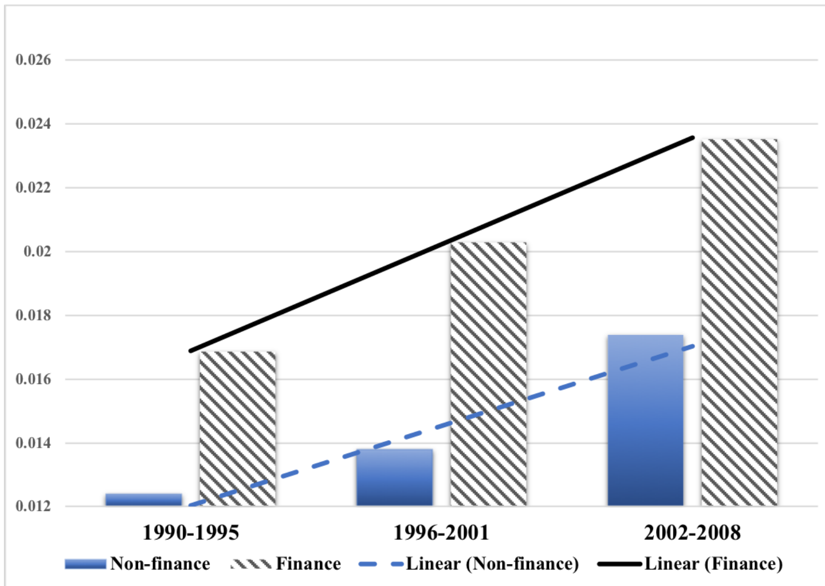
Using a large sample of private and publicly listed firm data from the U.S. Census Bureau, I construct proxies for firm-specific market power to examine how the variation in firm market power explains the heterogeneity of wages within finance and how the role of firm market power within finance differs from non-finance. Overall, this paper shows that higher firm market power is associated with relatively higher wages within finance as compared to non-finance.

In this paper, I argue that rent-sharing plays an essential role in driving the more pronounced effect of firm market power on finance wages. An increase in firms' market power not only increases firms' labor market monopsony power in lowering wages by decreasing competition for hiring workers, but also increases firms' product market monopoly power by decreasing competition for selling products or buying inputs. With higher market power, firms can extract higher rents to share with their workers. As compared to non-financial firms, financial firms with higher market power pay relatively higher wages because rent-sharing effect dominates the effect of labor monopsony, and the net effect is relatively higher in finance.

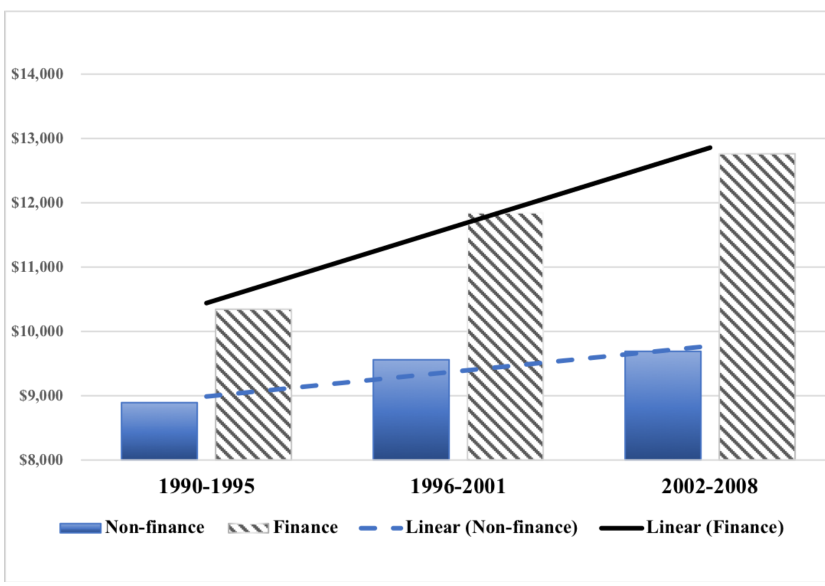
I provide evidence on two non-mutually exclusive mechanisms that explain why rent-sharing is more prevalent in finance. First, I show that with higher firm market power, financial firms can extract relatively higher profits to share with workers. Market power is particularly valuable in finance than in non-finance as it allows firms to charge relatively higher price-cost margins. Second, financial firms have to give a higher fraction of rents with workers as finance workers have relatively higher wage bargaining power. This mechanism is primarily applied to high-skill workers as they are more likely to be matched with larger scale jobs in finance. Indeed, I show that wages of high-skilled workers at financial firms respond more positively to the change in firm-specific market power. As financial firms disproportionally distribute rents to high-skilled workers, I also show higher market power is associated with higher within-firm pay inequality within finance relative to non-finance.

Figure 1.1: Trends in Industry Concentration and Wages, 1990-2008

Figure (a) plots trends in the employment-weighted average of the Herfindahl-Hirschman Index(HHI) constructed by Equation (1) at the 3-digit-SIC-year level by finance and non-finance sectors. Each bar represents the mean HHI in a given sector which is averaged across industry-year cells within each of the six-year periods(the last period includes seven years, 2002-2008) using the number of employees in each cell as the weight. The average HHI represents the degree of employer concentration the average worker faces in the finance or non-finance industries. Each straight line represent the linear trend of HHI between 1990 and 2008 in a given sector. Figure (b) plots trends in the employment-weighted average of real wages computed at the firm-year-quarter-level in finance and non-finance sectors. Each bar represents the average real wage which is averaged across firm-year-quarter cells within each of the six-year periods(the last period includes seven years, 2002-2008) using the number of employees in each cell as the weight. Each straight line represent the linear trend of average wage between 1990 and 2008 in a given sector.



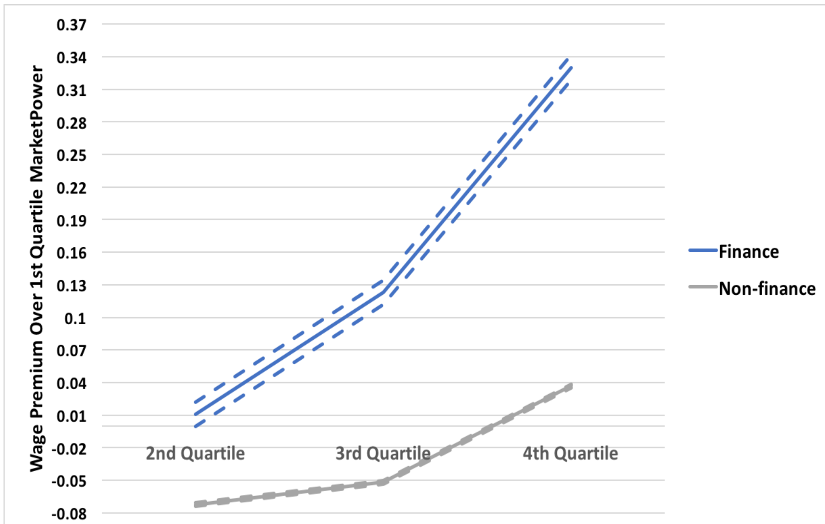
(a) Trends in Average Industry Concentration



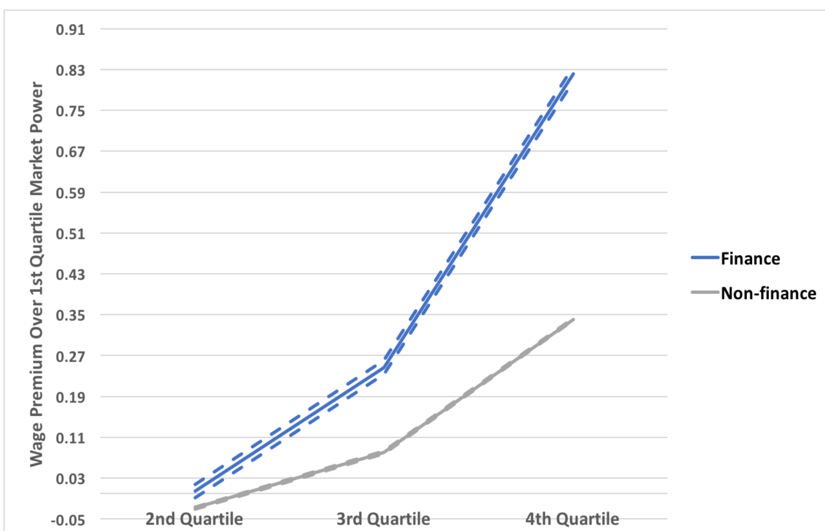
(b) Trends in Average Real Wage

Figure 1.2: Wage Patterns and Firm Market Power: Finance vs. Non-finance Industries

The figures show how wage patterns in finance and non-finance respond to a change in firm market power from the first quartile of firm market power distribution within the sample. A firm's market power is measured as the firm's employment share in its industry (defined using two-digit SIC). Each figure plots regression coefficients of $D^{2nd}_{f,j,t-4}$, $D^{3rd}_{f,j,t-4}$, and $D^{4th}_{f,j,t-4}$ from equation (4), where $D^{2nd}_{f,j,t-4}$, $D^{3rd}_{f,j,t-4}$, and $D^{4th}_{f,j,t-4}$ are equal to 1 if the firm f's market power in year-quarter $t-4$ is respectively in the second, third or fourth quartile of firm market power distribution within the sample. The depended s in plot (a) and (b) are the log of quarterly average wages ($logWages$) and the log of average wages of high-skill workers ($logWages\_hskill$) respectively. The solid line indicate point estimates and the dashed line indicate 95% confidence bounds based on standard errors clustered at the firm-level.



(a) Wage Premium and Market Power



(b) Wage Premium of High-skilled Workers and Market Power

Table 1.1: Summary Statistics: Firm Wage Pattern, Market Power and Other Characteristics

This table reports firm-level summary statistics. The sample consists of US public and private firms, and spans from Q2, 1990 through Q3, 2008. *All* refers to all observations in the sample. *Non-finance* refers to observations in finance industries. *Finance* refers to observations in non-finance industries. In columns (1) to (3) sample means (standard deviations) are computed across all-firm-quarter observations in each category. Column (4) provides differences between means in column (3) and column (2). Stars in the column (4) represent the level of p-values of testing the difference between columns 2 and 3: *** indicates p<0.01, ** indicates p<0.05, and * indicates p<0.1. All definitions are provided in Appendix I. The number of observations is rounded following the Census Bureau's disclosure rules.

	(1) All	(2) Non-finance	(3) Finance	(4) Difference [(3)-(2)]
		Panel A: Wage Pattern		
Average quarterly wage ($)	8864	8771	10910	2142***
	(8039)	(7786)	(12210)	
Average quarterly wage of high-skill ($)	17430	17180	23050	5869***
	(25650)	(24630)	(41880)	
Quarterly wage 90th/10th percentile ratio	4.277	4.247	4.957	0.71***
	(5.757)	(5.704)	(6.79)	
		Panel B: Firm Characteristics		
$MarketPower^E$ (2-digit SIC, %)	0.002	0.002	0.003	0.001***
	(0.05)	(0.051)	(0.04)	
$MarketPower^E$ (3-digit SIC, %)	0.011	0.011	0.014	0.002***
	(0.193)	(0.192)	(0.208)	
HHI (2-digit SIC)	0.004	0.004	0.007	0.003***
	(0.011)	(0.011)	(0.007)	
HHI (3-digit SIC)	0.008	0.008	0.01	0.003***
	(0.019)	(0.019)	(0.018)	
Average education level (year)	13.79	13.77	14.23	0.46***
	(1.355)	(1.354)	(1.294)	
Average working experience (year)	20.62	20.57	21.64	1.062***
	(6.888)	(6.886)	(6.865)	
CollegeShare (%)	35.05	34.72	42.27	7.546***
	(25.42)	(25.34)	(26.2)	
MaleShare (%)	55.07	56.14	31.39	-24.74***
	(33.42)	(33.34)	(25.57)	
Firm age	13.04	12.99	14.13	1.136***
	(8.545)	(8.519)	(9.02)	
Number of observations	64,790,000	62,000,000	2,795,000	

Table 1.2: Industry Concentration and Firm Wages

This table presents estimates of the relation between industry concentration and firm average wage. The sample consists of US public and private firms, and spans from Q1, 1990 through Q3, 2008. The dependent variable is the log-transformed average quarterly wages at the firm. Wages are in 2001 constant dollars. *HHI* represents the Herfindahl-Hirschman Index. *LogFirmN* represents the log of total number of firms in a given two-digit SIC industry. Column (2)-(5) control for the four-quarter-lag of log of firm age and firm-level measures of workforce composition, including share of male workers, log of average education level, share of college workers, and log of average worker experience. All controls are lagged by four quarters, except the indicator *FIN*. Standard Errors are clustered at industry level and reported in parentheses. *** indicates p<0.01, ** indicates p<0.05, and * indicates p<0.1. All definitions are provided in Appendix I. The number of observations is rounded following the Census Bureau's disclosure rules.

	(1) logWages	(2) logWages	(3) logWages	(4) logWages	(5) logWages
FIN			0.114***	0.186***	1.347**
			(0.002)	(0.002)	(0.642)
HHI (2-digit SIC)	-3.926***	-3.374***	-3.917***		
	(0.027)	(0.024)	(0.026)		
FINXHHI (2-digit SIC)			16.44***		
			(0.230)		
HHI (3-digit SIC)				-0.963***	
				(0.014)	
FINXHHI (3-digit SIC)				3.315***	
				(0.074)	
LogFirmN					0.0309
					(0.036)
FINXLogFirmN					-0.101*
					(0.057)
Number of observations	64,790,000	64,790,000	64,790,000	64,790,000	64,790,000
R-squared	0.014	0.152	0.161	0.155	0.157
YearxQuarterFE	YES	YES	YES	YES	YES
Workforce composition		YES	YES	YES	YES
Firm Age		YES	YES	YES	YES

Table 1.3: Firm Market Power and Wages in Finance

This table presents the estimates of the effects of firm market power measured by employment on the wages of finance and non-finance firms. The sample consists of US public and private firms, and spans from Q2, 1990 through Q3, 2008. The dependent variable is the log-transformed average quarterly wages at the firm. Wages are in 2001 constant dollars. Standard errors are clustered at firm-level and reported in parentheses. *** indicates p<0.01, ** indicates p<0.05, and * indicates p<0.1. All definitions are provided in Appendix I. The number of observations is rounded following the Census Bureau's disclosure rules.

VARIABLES	(1) logWages	(2) logWages	(3) logWages	(4) logWages	(5) logWages	(6) logWages
FIN	0.198***	0.219***	0.218***		0.217***	0.218***
	(0.0016)	(0.0015)	(0.0016)		(0.0016)	(0.0015)
$MarketPower^E$ (2-digit SIC)		0.123***	0.112***	0.112***	0.0971***	
		(0.0417)	(0.0391)	(0.039)	(0.0346)	
FINX$MarketPower^E$ (2-digit SIC)			0.415**	0.424**	0.4**	
			(0.172)	(0.175)	(0.163)	
$MarketPower^E$ (3-digit SIC)						0.0381***
						(0.0049)
FINX$MarketPower^E$ (3-digit SIC)						0.0543***
						(0.0161)
Number of observations	64,790,000	64,790,000	64,790,000	64,790,000	64,790,000	64,790,000
R-squared	0.014	0.151	0.151	0.151	0.154	0.151
YearxQuarterFE	YES	YES	YES	YES	YES	YES
Workforce Composition		YES	YES	YES	YES	YES
Firm Age					YES	
FIN×Year×Quarter FE				YES		

Table 1.4: Firm Market Power and Other Measures of Wages

This table presents the estimates of the effects of firm market power measured by employment on different measures of wages of finance and non-finance firms. The dependent variables are the log of median quarterly wages at the firm in column (1), the log of average quarterly wages adjusted for cost of living in column (2), the log of average quarteraly wages of male workers in column (3) and the log of average quarterly wages of female workers in column (4). Wages are in 2001 constant dollars. Standard Errors are clustered at firm-level and reported in parentheses. *** indicates p<0.01, ** indicates p<0.05, and * indicates p<0.1. All definitions are provided in Appendix I. The number of observations is rounded following the Census Bureau's disclosure rules.

	(1) logMedWages	(2) logWages_adj	(3) logWages_m	(4) logWages_f
FIN	0.213***	0.223***	0.268***	0.148***
	(0.001)	(0.0016)	(0.0025)	(0.0016)
$MarketPower^E$ (2-digit SIC)	0.0686***	0.0996***	0.0354**	0.098***
	(0.0253)	(0.0352)	(0.0157)	(0.035)
FINX$MarketPower^E$ (2-digit SIC)	0.195**	0.389**	0.362***	0.302**
	(0.0799)	(0.161)	(0.138)	(0.125)
Number of observations	64,790,000	64,790,000	39,990,000	39,990,000
R-squared	0.154	0.154	0.12	0.075
YearxQuarterFE	YES	YES	YES	YES
Workforce Composition	YES	YES	YES	YES
Firm Age	YES	YES	YES	YES

Table 1.5: Firm Market Power and Profitability in Finance

This table presents the estimates of the effects of firm market power measured by employment on the firms' profitability within finance and non-finance. The sample consists of US public firms, and spans from Q2, 1990 through Q4, 2005. The dependent variable is the return on asset(ROA) at the firm, where ROA is defined as the EBITDA scaled by total assets at given firm-year-quarter. Besides time fixed effects, all regressions control for the four-quarter-lag of firm-level measures of workforce composition, including the share of male workers, the log of average education level, the share of college workers, and the log of average worker experience. Column (2) and (4) also control for the four-quarter-lag of log firm age. Standard errors are clustered at firm-level and reported in parentheses. *** indicates $p<0.01$, ** indicates $p<0.05$, and * indicates $p<0.1$. All definitions are provided in Appendix I. The number of observations is rounded following the Census Bureau's disclosure rules.

	(1) ROA	(2) ROA	(3) ROA	(4) ROA
FIN	-0.0527***	-0.0591***	-0.0495***	-0.0564***
	(0.0054)	(0.0054)	(0.0054)	(0.0054)
$MarketPower^E$ (2-digit SIC)	0.0087**	0.0056**		
	(0.0035)	(0.0024)		
FIN$\times MarketPower^E$ (2-digit SIC)	0.0242***	0.021**		
	(0.008)	(0.0093)		
$MarketPower^E$ (3-digit SIC)			0.0047***	0.0035***
			(0.0012)	(0.001)
FIN$\times MarketPower^E$ (3-digit SIC)			0.0003	0.0007
			(0.002)	(0.002)
lgFirmAge		0.064***		0.063***
		(0.0052)		(0.0052)
Number of observations	91,000	91,000	91,000	91,000
R-squared	0.047	0.078	0.05	0.08
Year$\times$Quarter FE	YES	YES	YES	YES
Workforce Composition	YES	YES	YES	YES

Table 1.6: Firm Market Power, Price-cost Margins and Efficiency in Finance

This table presents the estimates of the effects of firm market power measured by employment on price-cost margins and efficiency of firms within finance and non-finance. The sample consists of US public firms, and spans from Q2, 1990 through Q4, 2005. The dependent in Panel A is the *Lener Index*, which is defined as the operating income after depreciation scaled by total sales at a given firm-year-quarter. The dependent in Panel B is the *Asset Utilization Ratio*, which is defined as total sales by total assets at a given firm-year-quarter. Besides time fixed effects, all regressions control for the four-quarter-lag of firm-level measures of workforce composition, including the share of male workers, the log of average education level, the share of college workers, and the log of average worker experience. Column (2) and (4) also control for the four-quarter-lag of log firm age. Standard errors are clustered at firm-level and reported in parentheses. *** indicates p<0.01, ** indicates p<0.05, and * indicates p<0.1. All definitions are provided in Appendix I. The number of observations is rounded following the Census Bureau's disclosure rules.

	Panel A				Panel B			
	(1) LernerIndex	(2) LernerIndex	(3) LernerIndex	(4) LernerIndex	(1) AssetUtilization	(2) AssetUtilization	(3) AssetUtilization	(4) AssetUtilization
FIN	0.250***	0.235***	0.247***	0.23***	-1.184***	-1.201***	-1.183***	-1.202***
	(0.0141)	(0.014)	(0.0136)	(0.0136)	(0.0345)	(0.0347)	(0.0339)	(0.0341)
$MarketPower^E$ (2-digit SIC)	0.0113**	0.0035			0.0347**	0.0262		
	(0.0051)	(0.0024)			(0.0165)	(0.0161)		
FIN$\times MarketPower^E$ (2-digit SIC)	0.0653*	0.0574*			0.0604	0.0519		
	(0.0398)	(0.0429)			(0.0454)	(0.0447)		
$MarketPower^E$ (3-digit SIC)			0.0061***	0.0029***			0.0037	0.0002
			(0.0015)	(0.0009)			(0.0053)	(0.0051)
FIN$\times MarketPower^E$ (3-digit SIC)			0.0261***	0.0273***			0.0043	0.0056
			(0.0049)	(0.0043)			(0.0087)	(0.0088)
lgFirmAge		0.158***		0.156***		0.170***		0.174***
		(0.0163)		(0.0163)		(0.0253)		(0.0255)
Number of observations	91,000	91,000	91,000	91,000	91,000	91,000	91,000	91,000
R-squared	0.065	0.097	0.068	0.1	0.204	0.211	0.203	0.21
Year$\times$Quarter FE	YES	YES	YES	YES	YES	YES	YES	YES
Workforce Composition	YES	YES	YES	YES	YES	YES	YES	YES

Table 1.7: Firm Market Power and Wages of High-Skill Workers in Finance

This table presents the estimates of the effects of firm market power measured by employment on the wages of high-skill workers at finance and non-finance firms. The sample consists of US public and private firms, and spans from Q2, 1990 through Q3, 2008. The dependent variable is the log-transformed average quarterly wages of high-skill workers at the firm. Wages are in 2001 constant dollars. Standard errors are clustered at firm-level and reported in parentheses. *** indicates p<0.01, ** indicates p<0.05, and * indicates p<0.1. All definitions are provided in Appendix I. The number of observations is rounded following the Census Bureau's disclosure rules.

	(1) logWages_hskil	(2) logWages_hskil	(3) logWages_hskil	(4) logWages_hskil
FIN	0.242***	0.239***	0.242***	0.241***
	(0.0023)	(0.0026)	(0.0023)	(0.0023)
$MarketPower^E$ (2-digit SIC)	0.365***	0.34***		
	(0.116)	(0.111)		
FINX$MarketPower^E$ (2-digit SIC)		0.933**		
		(0.428)		
$MarketPower^E$ (3-digit SIC)			0.124***	0.119***
			(0.0156)	(0.0158)
FINX$MarketPower^E$ (3-digit SIC)				0.0919**
				(0.0401)
Number of observations	64,790,000	64,790,000	64,790,000	64,790,000
R-squared	0.093	0.093	0.093	0.093
Year × QuarterFE	YES	YES	YES	YES
Workforce composition	YES	YES	YES	YES

Table 1.8: Firm Market Power and Within-firm Inequality in Finance

This table presents the estimates of the effects of firm market power measured by employment on the wage disparities within finance and non-finance firms. The sample consists of US public and private firms, and spans from Q2, 1990 through Q3, 2008. The dependent in column (1) and (2) is the log difference of the average quarterly wages above the 90th percentile and below the 10th percentile of the quarterly wage distribution in that firm-year-quarter. The dependent s in column (3) (column (4))are the log difference of the average male (female) wages above the 90th percentile and below the 10th percentile of the male (female) wage distribution in that firm-year-quarter. Standard errors are clustered at firm-level and reported in parentheses. *** indicates p<0.01, ** indicates p<0.05, and * indicates p<0.1. All definitions are provided in Appendix I. The number of observations is rounded following the Census Bureau's disclosure rules.

	(1)	(2)	(3)	(4)
	logWages90th_10th	logWages90th_10th	logWages90th_10th_m	logWages90th_10th_f
FIN	0.0586***	0.0604***	0.116***	0.00292
	(0.0026)	(0.0024)	(0.004)	(0.0024)
$MarketPower^E$ (2-digit SIC)	0.388***		0.424***	0.391***
	(0.124)		(0.134)	(0.128)
FINX$MarketPower^E$ (2-digit SIC)	0.944**		1.323**	0.647*
	(0.458)		(0.588)	(0.351)
$MarketPower^E$ (3-digit SIC)		0.132***		
		(0.018)		
FINX$MarketPower^E$ (3-digit SIC)		0.075*		
		(0.0433)		
Number of observations	64,790,000	64,790,000	39,990,000	39,990,000
R-squared	0.024	0.024	0.132	0.162
Year × QuarterFE	YES	YES	YES	YES
Workforce composition	YES	YES	YES	YES

Table 1.9: Firm Local Market Power and Wage Patterns in Finance

This table presents the estimates of the effects of firm local market power on local wage patterns at finance and non-finance firms. The sample consists of US public and private firms, and spans from Q2, 1990 through Q3, 2008. The dependent variables are the log-transformed average quarterly wages at firm-commuting zone level in Panel A, and the log-transformed average quarterly wages of high-skill workers at firm-commuting zone level in Panel B. Wages are in 2001 constant dollars. All regressions control for the four-quarter-lag of firm-commuting zone level measures of workforce composition, including the share of male workers, the log of average education level, the share of college workers, and the log of average worker experience. Standard errors are double clustered at firm and commuting zone and reported in parentheses. *** indicates p<0.01, ** indicates p<0.05, and * indicates p<0.1. All definitions are provided in Appendix I. The number of observations is rounded following the Census Bureau's disclosure rules.

	Panel A		Panel B	
	(1)	(2)	(1)	(2)
	logWages_cz	logWages_cz	logWages_hskill_cz	logWages_hskill_cz
FIN	0.219***		0.230***	
	(0.0014)		(0.0022)	
$MarketPower^L$	0.00406***	0.00398***	0.0114***	0.0114***
	(0.000)	(0.000)	(0.0001)	(0.0001)
FINX$MarketPower^L$	0.00034	0.00353***	0.00476***	0.0091***
	(0.0002)	(0.0003)	(0.0005)	(0.0007)
Observations	69,270,000	69,270,000	69,270,000	69,270,000
R-squared	0.194	0.197	0.136	0.138
Year×QuarterFE	YES		YES	
CZ FE	YES		YES	
Workforce Composition	YES	YES	YES	YES
FIN×CZ×Year×Quarter		YES		YES

Table 1.10: Firm Market Power Measured by Sales and Wage Patterns in Finance

This table presents the estimates of the effects of firm market power measured by sales on wage patterns at finance and non-finance firms. The sample consists of US public and private firms, and spans from Q2, 1990 through Q3, 2008. Industries are defined by two- and three-digit SIC in Panel A and Panel B respectively. In each panel, The dependent variable is the log-transformed average quarterly wages in column (1), the log-transformed average quarterly wages of high-skill workers in column (2), and the log difference of the average quarterly wages above the 90th percentile and below the 10th percentile of the quarterly wage distribution in column (3). Wages are in 2001 constant dollars. Besides time fixed effects, all regressions control for the four-quarter-lag of firm-level measures of workforce composition, including the share of male workers, the log of average education level, the share of college workers, and the log of average worker experience. Standard errors are clustered at firm-level and reported in parentheses. *** indicates p<0.01, ** indicates p<0.05, and * indicates p<0.1. All definitions are provided in Appendix I. The number of observations is rounded following the Census Bureau's disclosure rules.

	Panel A		Panel B	
	(1)	(2)	(1)	(2)
	logWages	logWages_hskil	logWages	logWages_hskil
FIN	0.193***	0.16***	0.215***	0.208***
	(0.0021)	(0.0033)	(0.0021)	(0.0034)
$MarketPower^S$ (2-digit SIC)	0.337***	0.82***		
	(0.0568)	(0.143)		
FINX$MarketPower^S$ (2-digit SIC)	3.19***	6.819***		
	(0.146)	(0.295)		
$MarketPower^S$ (3-digit SIC)			0.0388***	0.0967***
			(0.0031)	(0.0077)
FINX$MarketPower^S$ (3-digit SIC)			0.0554*	0.113*
			(0.0286)	(0.0617)
Number of observations	39,090,000	39,090,000	39,090,000	39,090,000
R-squared	0.149	0.091	0.148	0.089
Year × Quarter FE	YES	YES	YES	YES
Workforce composition	YES	YES	YES	YES

Table 1.11: Finance vs. construction, transportation and public utilities, and services

This table presents results of the effect of firm market power on average wages and high-sklll wages. The sample consists of US public and private firms, and spans from Q1, 1990 through Q3, 2008. In both columns, the reference group includes firms in Construction, Transportation and Public Utilities, and Services. Standard Errors are clustered by firm and reported in parentheses. *** indicates p<0.01, ** indicates p<0.05, and * indicates p<0.1. All definitions are provided in Appendix I. The number of observations is rounded following the Census Bureau's disclosure rules.

	(1)	(2)
	logWages	logWages_hskill
Construction, Transportation, Public Utilities and Services	Omitted	Omitted
Mining	0.123***	0.139***
	(0.004)	(0.006)
Manufacturing	-0.0232***	0.0861***
	(0.001)	(0.002)
Wholesale	0.0776***	0.159***
	(0.0011)	(0.0016)
Retail	-0.323***	-0.332***
	(0.001)	(0.001)
FIN	0.0655***	0.0625***
	(0.001)	(0.002)
$MarketPower^E$ (2-digit SIC)	0.0858***	0.331***
	(0.013)	(0.059)
MiningX$MarketPower^E$	0.0262	-0.131
	(0.031)	(0.084)
ManufacturingX$MarketPower^E$	0.163***	0.388***
	(0.042)	(0.135)
WholesaleX$MarketPower^E$	0.093	0.369
	(0.088)	(0.354)
RetailX$MarketPower^E$	-0.0372	-0.189*
	(0.037)	(0.109)
FINX$MarketPower^E$	0.555***	1.137**
	(0.199)	(0.472)
Observations	64,790,000	64,790,000
R-squared	0.21	0.143
Year×Quarter FE	YES	YES
Workforce Composition	YES	YES
Firm Age	YES	YES

Table 1.12: Individual Level Regressions: Firm Market Power and Worker Wages

This table presents the estimates of the effects of firm market power measured by employment on the wages of finance and non-finance firms using individual-level panel data. This sample only includes the wage paid in first quarter of each year for each worker. The dependent variable is the log of real quarterly wages. Real wages are in 2001 constant dollars. FIN is equal to 1 if the worker' employer is classified as a finance firm. $MarketPower^E$ is a measure of a firm's market power in a given 2-digit SIC industry. All regressions control for year fixed effects, year fixed effects by education and function of worker age interacted with education dummies. Standard Errors are clustered by firm and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

	(1)	(2)	(3)	(4)
	LogWages_wk	LogWages_wk	LogWages_wk	LogWages_wk
FIN	0.182***	0.156***	0.0823***	
	(0.0127)	(0.0072)	(0.0021)	
$MarketPower^E$ (2-digit SIC)		-0.00791***	-0.00171***	0.00258***
		(0.0026)	(0.0006)	(0.0008)
FINX$MarketPower^E$ (2-digit SIC)		0.0945***	0.0486***	0.0255***
		(0.0208)	(0.0081)	(0.007)
Observations	466,600,000	466,600,000	466,600,000	466,600,000
R-squared	0.168	0.854	0.854	0.874
Year FE	YES	YES	YES	YES
Worker FE	NO	NO	YES	YES
Edu×Year	YES	YES	YES	YES
Age×Edu	YES	YES	YES	YES
State×Year				YES
Industry×Year				YES

CHAPTER 2

MERGERS AND ACQUISITIONS, TECHNOLOGICAL CHANGE AND INEQUALITY (WITH PAIGE OUIMET AND ELENA SIMINTZI)

2.1 Introduction

The structure of job opportunities in the United States has sharply polarized over the last forty years. Automation technologies and robotic machines have replaced workers with moderate skills performing routine tasks (Autor, Levy, and Murnane, 2003; Acemoglu and Autor, 2011; Autor and Dorn, 2013). At the same time, these technologies have increased the productivity of high-skilled workers and, hence, the skill premium (Katz and Autor, 1999). Both trends have led to increasing wage inequality. While technology adoption has long been recognized as a key factor in the observed labor market changes, less is known about when firms decide to invest in these technologies.[1]

In this paper, we provide micro foundations for these economy-wide labor market trends. We argue that mergers and acquisitions (M&As) act as catalysts for technology adoption associated with important occupational and wage changes. First, M&As are unambiguously economically important events that significantly impact the target firm with potentially economy-wide implications. Second, M&As arguably lower the opportunity costs of investing in labor-saving technologies. Specifically, it may be economically efficient to adopt a given technology following an M&A if one investment can now replace more employees or if the acquirer has specific skills in implementing such technologies. Alternatively, an M&A may alleviate frictions to the implementation of such technologies, such as financial constraints or a reluctance by the target's manager to invest in technology that requires firing specific employees.

[1]Exceptions include Jaimovich and Siu (2015), Zhang (2016), and Hershbein and Kahn (2016) who show that technology adoption is accelerated in recessions, when opportunity cost of investing in technology is lower.

We use establishment level data from the Occupational Employment Survey (OES), conducted by the Bureau of Labor Statistics (BLS), to study the occupational employment and wage changes following M&As. We focus on horizontal M&A deals over the 2001-2007 period and identify a set of 2,141 establishments belonging to 348 M&A target firms covered by the OES survey. We form a control sample of similar establishments in terms of industry, year of observation in the OES survey, pre-treatment employment and share of routine occupations and perform a difference-in-differences (DiD) identification strategy.

We find that establishments that are M&A targets become less routine task intensive, as compared to a matched sample of establishments. Specifically, routine share intensity, namely the percent of employees in routine occupations, is reduced by 4.4% in treated establishments, consistent with technological adoption disproportionately displacing workers performing routine, easily codifiable tasks, a process often referred to as routine-biased technological change. Routine-intensive occupations have been shown to be over-represented in the middle of the income distribution and, as such, displacing those occupations with technology has been linked to the polarization of job opportunities in the U.S. labor market (Autor and Dorn, 2013). We also find that there is an occupational shift towards relatively more high skilled workers following M&As in treated establishments. The occupational share of high skill jobs increases by 2pp (or by 17% relative to the pre-treatment mean), which can be explained by complementary technology increasing demand for high skill workers, a process often referred to as skill-biased technological change. These occupational shifts away from middle- and towards high-skill workers suggest that employment in M&A establishments tends to become more polarized.

These shifts in the employment distribution in M&A targets have implications on wages. Mean wages may increase following M&As as the relative fraction and productivity of high-skill workers increase. Indeed, we find a 4% increase in the average wage at treated establishments following the acquisition, as compared to the matched sample of control establishments. Moreover, wages are likely to become more polarized as the labor shares are increasingly represented by both the high and low tails of the skill distribution. Consistent with the notion that M&As are associated with more unequal pay, we find that the standard deviation in wages relatively increases by 8.9%

We provide evidence for three non-mutually exclusive mechanisms that can help explain how M&As act as a catalyst for labor-saving technology adoption. First, we argue that the increased scale within occupations following an M&A can better offset the fixed costs of investing in new technology. To fix ideas, if an investment in computer software can more efficiently perform a specific function in accounting, then it can displace one worker in a small firm but possibly several workers in a larger firm. In support of this mechanism, we find that labor market outcomes are more pronounced in target establishments that have greater occupational overlap with their acquirer. Second, M&As often target underperforming firms leading to ex-post efficiency gains (Maksimovic and Phillips, 2001). Acquirers may have already invested in technology and as such, may already own rights to technology which can be efficiently utilized at the target or may know how best to integrate automation at the target. We identify such acquirers by low shares of routine intensive occupations before the acquisition, indicating that these acquirers have already invested in labor-saving technologies. Consistent with this mechanism, we show greater effects on the target's labor market outcomes when more technologically advanced acquirers are involved. Third, M&As may resolve financial constraints at the target firm (Erel, Jang, and Weisbach, 2015). This may induce automation if financially constrained targets were unable to finance the initial fixed costs necessary to invest in new technologies. We show overall greater effects for private targets, namely those targets more likely to be financially constrained (Officer, 2007).

Besides documenting labor market changes consistent with technology adoption, we provide direct evidence that IT intensity increases following M&As. Using granular data on IT investment, we find that investments in IT significantly increase at target establishments following the M&A, as compared to a matched set of control establishments. We find that overall IT budgets, as well as specific budgets for software, hardware and services, increase by about 5% post-M&As. These results further support our argument that M&As act as a catalyst of technology adoption. These results cannot be explained by an alternative hypothesis of simply cost-cutting following M&As, accompanied by labor restructuring unrelated to the adoption of technology.

Our estimates are consistent with both firms pursuing M&As with the objective of implementing labor-saving technology ex-post as well as with an explanation where firms pursue M&As for reasons orthogonal to technology and ex-post learn of the benefits to greater technological adoption. Irrespective of their motivation, it is important to consider all M&As to document a mechanism through which technology adoption and the accompanied labor changes feed into the real economy. Nevertheless, we still need to rule out the possibility that an omitted variable, such as industry or technology shocks (Harford, 2005), may lead to both M&As and changes in labor demand. In our baseline analysis, we use a sample of matched control establishments to control for trends that would equally affect similar firms in the economy. We also control for time-invariant establishment characteristics by including establishment fixed effects, for time-varying industry characteristics by including interacted industry and year fixed effects, and for time-varying local characteristics by including interacted state and year fixed effects.

We provide additional analyses that further support a causal interpretation. We first consider a sample of M&As that get cancelled due to an exogenous reason to labor demand. Specifically, we look at deals that are cancelled either because of regulatory intervention or due to the bidder being acquired by a third party following the acquisition announcement. We follow the same matching procedure used for our baseline analysis and create a control sample of matched establishments. We repeat our analysis using the set of the cancelled M&A targets ('pseudo-treated') and the matched set of non-M&A establishments (controls). We cannot replicate the same pattern of results in our baseline analysis; if anything, the estimated coefficients have now the opposite sign. Assuming that both completed and exogenously cancelled M&A deals should equally reflect changes in demand for M&As driven by an omitted variable, these results help mitigate such concerns.

Second, we present estimations *within* establishments, further alleviating concerns that time-varying differences between treated and control establishments are driving our findings. Specifically, we separate occupations in a given establishment into routine and non-routine and estimate whether there are differential effects on employment and wages specific to routine occupations following the M&A. Consistent with technology adoption disproportionately displacing employees performing routine tasks and thereby dampening their wages (Autor, Levy, and Murnane, 2003), we find a greater reduction in both wages and employment for those employees performing routine tasks in the establishment, relative to their peers in non-routine occupations. Importantly, this analysis allows us to control for interacted establishment and year fixed effects absorbing any time-varying shocks at the establishment level that may be correlated with changes in establishment labor demand.

The labor market changes we identify within establishments do not seem short-lived, or specific to our sample of OES establishments. In a sample of establishments we are able to track over time, we observe the same patterns hold in the long-run. Most importantly, we show that occupational and wage changes post-M&A are generalizable industry-wide. Using data since 1980s, we measure M&A intensity as the count of horizontal deals in an industry-decade normalized by the count of total horizontal deals in the decade. We collect data on occupational employment and wages from the Integrated Public Use Microdata Service (IPUMS) available every decade. We are able to replicate the same patterns at the industry level: routine share intensity decreases within industries when past M&A activity increases; at the same time, the share of workers with college education, an alternative measure of employee skill, increases when past M&A intensity increases. Similar to our establishment level results, these shifts in occupational employment following M&As have implications on industry inequality. We find that high M&A activity within industries is related to higher average wages and higher wage disparity.

Our paper contributes to the finance literature on mergers and employment outcomes. This literature argues that human capital considerations are important determinants of M&As. Pontiff, Shleifer, and Weisbach (1990) show evidence of pension asset reversions following takeovers consistent with the view that hostile takeovers breach contracts between firms and employees. Dessaint, Gobulov, and Volpin (2015) and John, Knyazeva, and Knyazeva (2015) find that labor restructuring (in the form of layoffs) is a primary source of synergies and value creation in corporate takeovers. Olsson and Tåg (2017) find that following an acquisition by a PE firm, workers in more routine or easily offshorable occupations are more likely to lose their jobs. Ouimet and Zarutskie (2016) show that some firms use takeover markets to acquire the workforce at the target. Tate and Yang (2016) show that diversifying acquisitions occur more frequently among industry pairs with higher human capital transferability. Babenko, Du, and Tserlukevich (2017) show that employment stock option compensation may be modified by acquirers in a way that is not beneficial to employees and this in turn can affect the offered premium for the acquisition. Our paper delves deeper into the heterogeneity of employment outcomes post M&A. Workers engaged in routine activities are most likely to be replaced as a result of investment in technology. At the same time, high skill workers may gain following an M&A.

The paper also builds on the literature on skill-biased technological change (Katz and Autor, 1999; Goldin and Katz, 2008, 2009; Acemoglu and Autor, 2011) and routine-biased technological change (Autor, Levy, and Murnane, 2003; Autor and Dorn, 2013; Goos, Manning, and Salomons, 2014). Rapid technological progress is viewed as the primary cause of the pattern of increasing wage inequality in the U.S. We contribute to the literature by showing that M&A activity acts as catalyst for job polarization leading to occupational shifts and wage trends which assimilate the aggregate patterns.

2.2 Data and methodology

2.2.1 Data

We use confidential micro-data from the Occupational Employment Survey (OES), conducted by the Bureau of Labor Statistics (BLS). This data comes from an annual or biannual survey of individual establishments in the U.S. No establishment is surveyed twice within three years, however, it is common for larger establishments to appear in the data exactly once every three years. The surveyed establishments are selected in a manner to allow for optimal inferences about the US economy as a whole. Aggregated versions of this data are released publicly and used to measure national occupational employment.

For each establishment-year, we observe employment in 800 different occupational categories (represented by 6-digit SOC codes). Within each of these occupations at a given establishment-year, we then observe the count of employment within twelve separate wage bins, where the exact cutoff points for each wage bin changes over time to best reflect changing wage distributions. Furthermore, for each surveyed establishment, we also observe its location (by county), EIN, name, legal name (ultimate owner), industry and a time invariant establishment-identifier which we can use to track establishments which have switched owners over time.

We identify horizontal M&A deals, namely M&As in the same 4-digit NAICS industry, from SDC Platinum. We match those deals to the OES survey over the 2001-2007 period. We start in 2001 as the identifier which we need to link establishments over time is unavailable in earlier years. We end in 2007 to avoid any overlap with the financial crisis which affected both the intensity of M&A activity and firms' labor market outcomes. We identify a total of 348 horizontal M&A deals in the OES survey that cover 2,141 establishments that had an M&A occurring during the time period the establishment is sampled by OES.[2] We create a set of possible control establishments after excluding all establishments identified to be involved in M&As during our sample period from this group. For each target establishment, we find two control establishments satisfying the following matching criteria:[3] i) they operate in the same 4-digit NAICS industry as the target establishment and appear for the first time in the same year in the OES survey, ii) they are sampled for the second time within one year of the treated establishment's second sampling, iii) they have similar size with the target as measured by number of employees (within 100% of employment distance), iv) they are similar with the target in terms of pre-treatment routine share intensity (within 100% of routine share intensity distance).[4] We end up with a sample of 3,081 control

[2]We use a two-step procedure to match M&A deals to the OES survey. First, we match using EIN and the target firm's Compustat provided EIN. However, since firms often report multiple EINs, we also use a name matching procedure. We start with a fuzzy logic algorithm to identify possible candidates, then hand match all likely candidates. A match is only retained if we observe the target establishment strictly before and after the M&A is completed.

[3]We allow matched establishments to repeat.

[4]In cases where more than two control establishments satisfy the matching criteria, we keep those establishments with the closest value of ex-ante employment.

establishments. Both treated and control establishments are observed exactly twice in the 2001-2007 period.[5]

We define routine share intensity following Autor and Dorn (2013).[6] Routine share intensity (RSH) of an establishment is defined as the ratio of total employment of routine task intensive occupations over total employment in the establishment. We use a log transformation of one plus the average value of RSH at the establishment level to avoid dropping cases where an establishment has no routine occupations. In the Internet Appendix, we will also present results using instead RTI as our measure.[7]

We define the share of high-skill employment as total employment identified as high-skill at the establishment-year level as a percent of total employment. We define high-skill employment as managerial occupations in the baseline analysis. We also present robustness using three different definitions of high-skill employment. First, we use data from the 2000 American Commuting Survey (ACS). An occupation is high skill if the percent of workers who have completed some college education is above the 75th percentile of the distribution across all occupations in the ACS sample. Second, to show our results are robust to alternative cutoffs, we again use the ACS survey but instead define an occupation to be high skill if the percent of workers who have completed some college education is above the 66th percentile of the distribution across all occupations in the ACS sample. Third, we define high skill occupations following Hecker (2005). These occupations are scientific, engineering and technician occupations.

We define offshorability of a given occupation following Autor and Dorn (2013) and compute

[5] OES data are imputed when missing. To confirm that our results are not driven by imputation we drop cases where establishment data is imputed for either one or for both years and re-estimate our baseline regressions. Results are robust.

[6] Autor and Dorn (2013) define the frequency of "routine" tasks typically performed by employees assigned to a given occupation. Since occupations involve multiple tasks (routine, abstract, manual) at different frequencies, Autor and Dorn (2013) create an indicator which measures the routine task intensity (RTI) by occupation and define an occupation as routine task intensive if in the top employment-weighted third of routine task-intensity. We merge RTI to occupations in OES by SOC codes using crosswalks from David Dorn's website. http://www.ddorn.net/data.htm.

[7] 8% of establishment-years in our sample have no routine occupations. We find qualitatively similar results if we drop those from the estimation.

an employment weighted average of offshorability at the establishment level.[8] We measure wages by taking the occupation-wage bin employment-weighted median within each establishment. All wages are adjusted for inflation and reported in 2001 dollars. We define all variables used in our analysis in the Appendix.

Table 2.1 reports summary statistics for our sample establishments. The average establishment in our sample employs 199 employees. As described earlier, the OES survey over-samples larger establishments. This limits our ability to reach conclusions about the smallest of establishments but ensures that our results are based on a sample of economically important entities. Fifty-three percent of employment at the average establishment is identified as routine occupations and twelve percent as high-skill occupations. Given occupations are coded as routine if they have a routine intensive measure in the top one third of the data, these results suggest that target establishments (and mechanically the matched control establishments) tend to have a disproportionate share of routine employment. Our sample firms have an average (median) wage of \$16.5 (\$14.2) per hour. This is comparable to the mean (median) hourly US wage in 2001 of \$16.35 (\$13.0).[9] Finally, we report an average standard deviation of hourly wages equal to 8.4.

We require treated and control establishments to match in terms of pre-treatment employment size and routine share intensity and we report summary statistics pre-treatment for the two groups in columns 4-9, Table 2.1. Our control and treated establishments show economically similar characteristics with the exception of routine share intensity. This may indicate that firms with high routine-share intensity ex-ante are more likely to be M&A targets as the acquirers are aware of the benefits of replacing routine occupations with technology. In untabulated results, we run predictive logit regressions and confirm this to be the case. Our sample of target establishments covers a wide range of industries. About a quarter of our sample M&As take place in the manufacturing sector and 70% in services. The industry distribution is similar across treated and control samples by

[8]We use SOC codes to merge with the OES sample using crosswalks from David Dorn's website. http://www.ddorn.net/data.htm.

[9]See https://www.bls.gov/oes/bulletin_2001.pdf for more information.

definition.

2.2.2 Methodology

To identify the effect of M&As on labor outcomes, we estimate the following difference-in-differences specification at the establishment-year level:

$$\mathrm{y}_{i,t} = \alpha_t + \alpha_i + \gamma_1 \cdot \mathrm{Post}_t + \gamma_2 \cdot \mathrm{Post}_t \cdot \mathrm{M\&A}_i + \beta \cdot X_{i,t} + \epsilon_{i,t} \tag{2.1}$$

where i denotes establishments and t denotes years. $Post_t$ is an indicator set equal to one for years following M&As—zero otherwise. $M\&A_i$ is an indicator equal to one for establishments targeted by M&As (treated) and zero for the matched set of control establishments.[10] Both treated and control establishments are observed exactly twice in our sample, once prior to the year of the M&A and once after. $X_{i,t}$ controls for offshorability to alleviate concerns that changes in establishments' offshoring potential could affect both the probability of M&As and our measured outcomes. α_i is an establishment fixed effect which controls for establishment characteristics that do not vary over our sample period; and α_t is a year fixed effect which absorbs aggregate shocks affecting all establishments. In all specifications, we report robust standard errors clustered at the firm level.

2.3 Results

2.3.1 Baseline results

We first examine how M&As affect the occupational composition in target firms. We hypothesize that technology adoption following M&As will result in the displacement of employees performing routine occupations. The labor economics literature (Autor, Levy and Murnane, 2003; Autor and Dorn, 2013 among others) has established that technology is "routine-biased because it is best at replacing workers performing routine, easily codifiable tasks that are typically tasks

[10]Note $M\&A_i$ is absorbed by the establishment fixed effects.

requiring middle level skills. To test this, we examine how the share of routine-intensive occupations (RSH) changes at the target following the M&A, as compared to a matched group of control establishments. Table 2.2 presents the results.

Column 1 shows that M&As are associated with a 4.4% average decrease in the routine share intensity of the establishment, as compared to the matched control sample, in a specification with establishment and year fixed effects. This result is statistically significant at the 1% level. In column 2, we control for the potential of establishments to offshore their production and continue to find a 3.1% decrease in routine share intensity, also significant at the 1% level. Note we report a positive correlation between the percent of offshorable jobs and the change in routine share intensity. This is consistent with findings in the literature that more offshorable tasks tend to be also more routine intensive. Goos, Manning, and Salomons (2014) report a correlation of 0.46.[11] We next repeat the estimation additionally controlling for interacted (4-digit NAICS) industry and year fixed effects (column 3), interacted state and year fixed effects (column 4), and both industry-year and state-year fixed effects (column 5) to control for industry shocks and local economic shocks that might be contemporaneous with the timing of the merger, respectively. Across specifications, the coefficients are similar in terms of magnitudes and statistical significance suggesting that industry or local shocks are not driving our findings.[12]

These results indicate that, on average, employment in highly routine occupations declines by over 3% in a short window following an M&A. This is an economically important change similar to employment losses documented following private equity acquisitions. Davis et al (2014) find a decline in total employment at private equity acquisition targets in the three years post-deal of 3% as well. Note we do not suggest that the observed decline in routine jobs post M&A

[11]In our data, we also confirm a positive univariate correlation between routine intensity and offshorability equal to 0.54 and significant at the 1% level.

[12]In Internet Appendix Table B.1, we show these results are robust to an alternative measure of routine task intensity. This alternative measure, RTI, is an employee-weighted average of the continuous variable measure of routine task intensity at the occupational level, as used in Autor and Dorn (2013). The benefit of RTI is that we now use all the variation as opposed to RSH, which is a dummy variable if RTI is in the top 1/3rd of the distribution. The downside of this alternative measure is that it does not allow for a straight-forward interpretation of the economic magnitudes. As such, we follow Autor and Dorn (2013) and use RSH as the primary measure but replicate results with RTI.

suggests a total decline in employment of 3% in our setting. In fact, our economic intuition has no explicit prediction regarding changes in total employment as it is possible that a greater reliance on automation ex-post may lead to an increase in employment in non-routine jobs, offsetting the job losses in routine jobs. Specifically, technological adoption should increase demand for high skill workers as new technology disproportionally increases productivity of high skill employees (skill-biased technological change). In other words, technology is complementary to human capital (Krueger, 1993; Autor, Katz, and Krueger, 1998).

In support of skill-biased technological change, we find an increase in the share of high-skill employees in treated establishments following the M&A as compared to the group of control establishments. Table 2.3 repeats the specifications in Table 2.2 and shows a 2 percentage point increase in the share of high-skill employees, or a 17% increase relative to the pre-treatment mean (column 1). The coefficients are also statistically significant at the 1% level even after controlling for industry and local economic shocks.[13]

Technology adoption associated with lower demand for workers performing routine tasks, disproportionately represented in the middle of the wage distribution, and higher demand for high skill employees at the right tail of the wage distribution should shift mean wages higher and increase within-establishment wage inequality. Indeed, Table 2.4 shows a significant 4% increase in treated establishments' average hourly wage as compared to the control sample. This is similar to the 4% wage increase for white collar workers following anti-takeover legislation that insulates managers from hostile takeovers documented in Bertrand and Mullainathan (2003)—although the wage increases in their setting captures managerial inertia and is associated with a fall in productivity as opposed to upskilling following technological adoption.[14] It is important to note that by focusing on hourly wages, we avoid any contamination in our results from changes in hours worked around the M&A event.

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[13]In Internet Appendix Table B.2, we instead define high-skill in three different ways, as detailed in Section 2.2.1. Our estimates are similar in both magnitudes and significance no matter which one of the three alternative skill definitions we look at.

[14]In Internet Appendix Table B.3, we find similar results if we consider establishment median hourly wages instead.

Moreover, we show M&As increase the within establishment wage inequality. We measure wage inequality using the establishment standard deviation of wages, as in Barth, Bryson, Davis, and Freeman (2016). Table 2.5 shows a 9% increase in establishment standard deviation of wages (column 1), significant at 1% level, as compared to matched control establishments. This is a rapid increase in inequality, compared to historical trends.[15]

In sum, the results are consistent with the notion that M&As act as a catalyst for labor-saving technology adoption. We document reduced employment in high routine occupations and an increase in within-establishment wage inequality. We also find evidence suggesting that the adoption of this technology increases the productivity of high skill workers, as shown by the increase in relative employment of high skill occupations and the increase in mean wages. Thus, these results suggest a more nuanced impact of M&As on workers as compared to earlier work which focused on total employment changes and suggest that post-M&A changes involves a complex restructuring of the labor force which may reflect cost-cutting, as evidenced in the decline of routine employment, but also reflects significant reallocation of labor towards more skilled occupations that accompany technology investments.

2.3.2 Evidence concerning mechanisms

We next explore potential channels driving the relationship between M&As and technological change. We propose three non-mutually exclusive mechanisms: 1) an increase in occupational scale; 2) tech-savvy acquirers being better equipped to adopt technology, or possibly directly implement their existing technology at the target; and 3) lower financial constraints.

Our first mechanism is motivated by the fact that horizontal M&As involve the integration of two firms engaged in the same industry. Assuming a technology is able to replace workers in a given occupation, the fixed cost of investing in the technology is reduced as M&As increase the number of employees in a given occupation that can be replaced by the technology. To test this

[15]In Internet Appendix Table B.4, we also consider the 90$^{\text{th}}$ to 10$^{\text{th}}$ percentile hourly wage ratio (in logs) within establishments, following the labor literature studying aggregate inequality. However, such an approach is particularly noisy in our setting given the modest distribution of employees within an establishment and the fact that our wages are reported in bins. We report results that are qualititatively similar.

mechanism, we measure the extent to which occupations at the target are also observed at the acquirer before the acquisition. To this end, we measure ex-ante occupations at the acquirer using all establishments which can be linked to the acquirer and observed within a two year window prior to the acquisition. We drop treated observations for which the acquirer cannot be matched to the OES data. Specifically, we define a dummy variable which takes the value of 1 if the percent of target's occupational employment overlapping with the acquirer is greater than the sample median, 0 otherwise ($Overlap\_Occup_i$). We augment our baseline specification by including an interaction between $\text{Post}_t \cdot \text{M\&A}_i$ and $Overlap\_Occup_i$. We repeat the key baseline measures, namely routine share intensity, share of high skill occupations, average wages, and standard deviation of wages. In all specifications, we include establishment fixed effects to control for time-invariant establishment characteristics, interacted industry and year fixed effects to control for time-varying industry shocks, and region times year fixed effects to control for time-varying local shocks. We use region times year fixed effects, as opposed to the state times year fixed effects used in the baseline to avoid over-saturating the model with fixed effects, due to the smaller sample. However, results are qualititavely similar, albeit overall slightly weaker, when we use state times year fixed effects instead.

We report the results in Table 2.6, Panel A. When looking at the proportion of routine workers, we find consistent results with greater reduction on the labor force—a 2.2% greater decline— when there is a greater overlap between occupations in the acquirer and the target. We find no significant difference in the treatment effect on high skill-employment by occupational overlap. However, we do find consistent results with an upskilling in the labor force when we look at average wages and at within-establishment inequality. Mean wages increase by 2.7% more and standard deviation of wages by 6.6% more when there is a greater overlap between occupations in the acquirer and the target pre-treatment, and these effects are statistically significant.

Second, we propose that following an M&A, more technologically advanced acquirers may be better equipped to implement labor-saving technology at targets. These acquiring firms may have already invested in a certain technology that can be directly implemented at the target at competitive prices. Or, these firms may just generally be better able to identify or implement value-added automation technologies. To identify these tech-savvy acquirers, we measure ex-ante acquirers' routine share intensity and take the employee-weighted average over the three years prior to the acquisition. The idea is that acquirers with low routine share intensity pre-treatment should have already invested in technology that has displaced workers performing routine tasks. We define a dummy variable which takes the value of 1 if the pre-treatment employee-weighted average routine share intensity for the acquirer is below the sample median, 0 otherwise ($Acq\_Low\_RSH_i$). We thus augment our baseline specification by including an interaction between $\text{Post}_t \cdot \text{M\&A}_i$ and $Acq\_Low\_RSH_i$.

We repeat the same specifications as in Panel A, and report the results in Table 2.6, Panel B. We find that routine share intensity decreases by 3.9% more for targets acquired by tech-savvy acquirers although the results are just outside conventional levels of statistical significance. Note, in unreported results, we find that the coefficient becomes significant at the 5% level once we control for state times year (instead of region times year) fixed effects. Similarly, we find positive but insignificant effects when we consider the share of high skill employment as our dependent variable. Although noisy, these results are suggestive that technology is more likely to be introduced following acquisitions by tech-savvy acquirers. Our intuition is strengthened when we instead consider wages: both mean wages and standard deviation of wages increase by 9.13% and 15.6% more, respectively, when the acquirer can better implement labor-saving technologies.

Third, we consider the role of financing constraints. Some M&A deals are motivated with the goal of easing financial constraints at the target, as in Erel, Jang, and Weisbach (2015). We assume private targets are most likely to be financially constrained, consistent with the finding in Officer (2007) that private firms are bought at lower multiples. We identify the status of the M&A target from SDC Platinum and create a dummy which is 1 for private targets, 0 otherwise ($Private_i$). We augment our baseline specification by including an interaction between $\text{Post}_t \cdot \text{M\&A}_i$ and $Private_i$ and estimate our analysis using consistent samples with the previous analysis (requiring that the acquirer is observed in the OES data).[16] We report the results in Table 2.6, Panel C. We find stronger treatment effects for private targets when we consider changes in share of high-skill employment, mean wages and wage inequality, but we are unable to find support for the financial constraints channel when we instead consider changes in targets routine share intensity.

The greater treatment at private targets could also be explained by an agency mechanism. Ellul, Pagano and Schivardi (2017) find that family firms, which represent a large fraction of private firms, are less likely to fire workers even in the presence of a permanent shock. To the extent that adopting labor saving technology is only value-enhancing if the technology replaces workers, these same firms may be especially reluctant to pursue such investments. As such, acquirers of private targets may have greater untapped opportunities to adopt labor-saving technologies, predicting greater treatment effects.

In sum, these results suggest multiple mechanisms at play that can plausibly explain the observed labor market changes post acquisitions. A caveat of this analysis, however, is that we cannot assess the importance of each channel due to differences in the power of our empirical proxies.

2.3.3 Investment in IT

So far, we have argued that firms increase investment in labor-saving technology post M&A by documenting changes in the labor force and compensation thereof. We next document changes in IT spending. While we cannot directly observe investments specifically meant to reduce labor costs, we can observe investments specific to technology using information from the Computer Intelligence Technology Database (CiTDB), a proprietary database that provides information on computers and telecommunication technologies at establishments across the U.S. CiTDB is a key resource for data on IT investments at US firms and has been used in a number of papers exploring technology spending, including Brynjolfsson and Hitt (2003), Bloom, Garicano, Sadun, and Van Reenen (2014), Tuzel and Zhang (2017). CiTDB generates their data using annual surveys of establishments. The data contain detailed information on IT investment and use, including budgets

[16]One exception is that we drop the top 10% of private targets by size, as these large private targets are unlikely to be financially constrained.

for new investments. This data is used by the sales and marketing teams at large US IT firms, such as IBM and Dell, thereby assuring high data quality, as errors would be quickly picked up by clients in their sales calls.

In this analysis, we compare outcomes at target establishments beginning two years before the M&A effective date to two years after the M&A effective date. We follow a standard dif-in-dif approach and compare these changes at target establishments with changes at a matched control sample over the 2010-2015 period. CiTDB started collecting and tracking IT spending in 2007. However, CiTDB data greatly increases in scope in 2010 with the 2010 survey covering 12 times more establishments as compared to the 2007 or the 2009 survey. As such, we start our analysis in 2010 although in unreported analysis we confirm our results are robust to starting our sample in 2007.[17] Over our CiTDB sample period, the data covers over 19 million establishment-years. We focus on IT budget, the main spending item related to technology adoption, and its three largest components: i) hardware budget, ii) software budget, and iii) services budget.

We match target firm names from SDC using firm names available in the CiTDB data and we include in the treated sample all establishments linked to the target and observed in the pre- to post- M&A period. To create the matched sample, we start with the set of establishments which are observed in the pre- to post- periods and are not identified as a target firm during our sample period. We match based on establishment attributes in the pre-treatment year and require control firms to match on (4-digit NAICS) industry, pre-treatment year and type of establishment.[18] To identify one unique control establishment out of this set of possible control establishments (all matched by industry, year and type), we select the establishment which is closest to the treated firm in terms of IT budget in the pre-M&A year. We end up with a sample of 4,707 unique firms covering 230 (4-digit NAICS) industries and all states. The average (median) establishment in our

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[17]We run this analysis on all available observations in the CiTDB data matched to an M&A and we dont limit the sample to the establishments in our OES sample. Requiring an establishment to be observed in both data sets would result in a small sample size.

[18]CiTDB identifies four different types of establishments: branch, headquarters, stand-alone and ultimate headquarters. The majority of our matched establishments are branches (80%) and our results are robust to limiting the sample to just branches.

sample spends \$333 (92) thousand in IT, \$57 (17) thousand in hardware, \$95 (\$26) thousand in software and \$ 105 (\$29) thousand in services, respectively.

Table 2.7 presents the results. We control for establishment fixed effects, interacted (4-digit NAICS) industry and year fixed effects, and interacted state and year fixed effects in all columns. In column 1, we document IT spending increases by 4.8% post-M&A as compared to a matched set of control establishments, and this increase is statistically significant at the 1% level. We document similar increases that are both economically and statistically significant when we instead consider hardware, software and sevices budget, in columns 2-4, all consistent with our argument that targets invest in technology. Our results also hold when we normalize our dependent variables by the number of employees in the establishment, reported in columns 5-8, suggesting these establishments become more capital intensive after they get acquired.

Overall, these results are consistent with our hypothesis that M&As act as a catalyst for technology adoption. Moreover, these results argue against an alternative cost-cutting interpretation of our baseline findings. Firms may be reducing employment in some areas, specifically employees engaged in routine occupations, however, they are also expanding IT budgets and increasing the relative share of high skill employees post M&A. Moreover, the results are large given that we conjecture that some of the technology applied at the target post M&A may be technology already owned by the acquirer. Thus, we interpret our estimated magnitudes with caution as they may be downwards biased.

2.3.4 Identification concerns

To study how M&A activity relates to macro labor trends, we need to consider all M&As irrespective of whether technology adoption was part of the ex-ante incentive to pursue the M&A or not. However, to conclude that M&As act as a catalyst for the adoption of labor saving technology, we need to address the alternative interpretation that an omitted variable (e.g. industry shock) may be driving both M&A activity and the associated occupational changes we document in the data. Our analysis allows us to absorb variation in industry and local conditions by controlling for time-varying industry and state fixed effects. In this section, we provide further evidence to mitigate an omitted variable concern.

First, following the approach in Seru (2014) and Malmendier, Opp, and Saidi (2016), we consider a sample of M&A deals that were announced but subsequently cancelled for reasons exogenous to the targets labor needs. To identify this sample, we start with all M&A deals announced over the 2001-2007 period that were subsequently withdrawn. We then read Factiva news articles explaining the reasons for the cancellation and retain a sample of deals where the M&A was either blocked by regulators, typically for anti-trust concerns, or because the acquirer was acquired ex-post and had to withdraw the deal. This leaves us with a small sample of deals cancelled for reasons exogenous to the target's labor demand.[19] We are able to identify 33 establishments in the OES survey data with cancelled M&A deals and this forms our 'pseudo treated' group. Following the same matching procedure as described in Section 2.2, we create a control sample which excludes establishments involved in M&As over our sample period.

Table 2.8 repeats the specification in column 3, Table 2.2 controlling for establishment and industry times year fixed effects.[20] We consider all our main dependent variables using this sample of 'pseudo-treated' deals and their matched control establishments. Across all our measures, we cannot replicate the same pattern as in our baseline results. In fact, all coefficients always take the opposite sign from what our hypotheses predict. These findings thus reinforce the notion that our difference-in-difference results capture the effect of M&As and not of some other confounding variables, as such omitted variables should impact target firms associated with completed M&As and the cancelled M&As in our sample equally.

Second, we perform estimations within establishments, absorbing any time-varying shocks at the establishment level that could be driving our results. To include establishment-year fixed

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[19]The other most common reasons stated for why deals get cancelled include: the management of the acquirer or the target rejecting the deal; disagreement on the price; changes in market or industry conditions; or bad news being revealed for the target. However, these reasons are arguably not exogenous to the target's labor demand and therefore we choose not to consider them.

[20]We do not show results where we also account for local shocks due to the small sample size in this analysis.

effects, we need variation within establishment-year. To this end, we separate routine and non-routine employment. We then test our predictions regarding employment and wage outcomes post-M&A looking specifically at routine occupations, occupations that are known to be disproportionately impacted by labor-saving technology, while controlling for changes at non-routine occupations at the same establishment. Specifically, we define $Routine$ to take a value of one for routine employment in a given establishment and 0 for non-routine employment. We then interact $Routine$ with $\text{Post}_t \cdot \text{M\&A}_i$ and estimate the effect of the M&A on routine occupations within establishments in a triple differences specification.

In Table 2.9, we show a greater reduction in the share of employment (column 1) for routine (as opposed to non-routine) occupations in treated establishments following the acquisitions as compared to control establishments. These results suggest lower demand for tasks substitutable by technology in M&A targets— a prediction unique to our technology adoption hypothesis— which is estimated after fully controlling for any contamporaneous shocks at the establishment level that could be driving changes in employment or wages. We estimate a decline in employment in routine workers, relative to non-routine workers of 8.5%. Comparing this estimate to the estimated decline of 4.4% in Table 2.2, column 1, using all employees, suggests that non-routine workers gain in employment post-M&A adding to our earlier argument that the impact of an M&A on the target firms employees is conditional on the type of worker.

Likewise, we reported in Table 2.4 that wages, on average, increase. In column 2, we examine what is the effect on routine workers' wages following the acquisition. We repeat the specification in column 1, except we additionally control for the share of employment by occupation type to control for the concurrent occupation employment changes that take place at the establishment. We show that wages for routine workers falls by -3.9%, suggesting differential wage results conditional on the occupational type.

2.3.5 External validity

In the previous results, we focus on changes in a short window following the M&A, in order to best identify changes that can be directly attributed to the M&A itself. However, these findings raise the question whether the labor market effects we capture are short-lived or whether, instead, M&As create permanent shifts within target firms. Although the structure of our data does not allow for a comprehensive assessment of long-term outcomes, we present suggestive evidence that our findings are unlikely to be reversed in the longer term. We are able to extend a subset of our sample to include later years.[21] Specifically, we use a subset of establishments in our baseline analysis that were also observed twice after 2007—the last year in our baseline analysis. Since all observations in our base sample are observed twice in the baseline years, observations in this sample are observed four times, with the second two observations occurring between the years 2008 and 2013 (inclusive).

Due to the survey nature of the data, this analysis includes one third of the original establishments. We create dummies $Post_1$, $Post_2$ and $Post_3$ that take a value of 1 for the first, second and third observation of the establishment post M&A, and interact those dummies with $M\&A_i$. We consider all our baseline measures in a specification with establishment fixed effects, interacted industry and year fixed effects and state and year fixed effects.[22]

We present results in Table 2.10. We find roughly similar point estimates of the changes in key outcomes right around the merger, although these results are not always statistically significant, as compared to earlier results using the full sample. Notably, there is a pattern of increasing point estimates of the change in routine intensity post-M&A over time suggesting that the M&A may change not just the stock of routine employment but also the rate at which routine employment is replaced at the target. Moreover, given the statistically significant findings on $Post_2 \cdot M\&A_i$ and $Post_3 \cdot M\&A_i$, it is clear that the results we document in the baseline tests are not immediately reversed. We also find significant effects in the long run for the rest of our measures ($Post_3 \cdot M\&A_i$ is always significant).

[21]We are unable to extend our sample backwards in time as the identifier which we need to link establishments over time is unavailable in earlier years.

[22]In unreported results, we repeat our baseline analysis limiting the sample to those establishments only to address a potential concern that these establishments would behave differently than the average firm in our baseline sample. Results are qualitatively similar.

2.4 Industry-level evidence

So far, we have presented evidence documenting the micro-fundamentals of the labor market changes associated with the adoption of automation technologies. In this section, we demonstrate that the firm-specific evidence that we have documented aggregates to the industry level. If our intuition that M&As are an important driver of technology adoption is correct, we should also observe M&As to be associated with occupational and wage changes industry-wide.

2.4.1 Industry Analysis: Data and summary statistics

To create our industry sample, we combine databases from three key sources: Thompson's SDC; IPUMs; and datasets on routine intensity and offshorability of occupations from Autor and Dorn (2013).

As in our baseline analysis, we collect data on horizontal mergers and acquisitions from Thomson's SDC. We use all deals, announced between 1980 and 2010, of a US target and US acquirer, for which we can confirm the acquirer completed a purchase of a majority stake.[23] We define the variable, merger intensity, as the count of horizontal deals in a given decade, for a given industry, normalized by all horizontal deals in that decade. We normalize by all deals in the decade to control for changes in the scope of coverage of SDC over time. This variable is log transformed (adding one to account for industries with no mergers) to address skewness.

We collect data on occupational employment from the Integrated Public Use Microdata Service (IPUMs) 5 percent extract for 1980, 1990, 2000 and the 2010 American Community Survey (ACS).[24,25] IPUMs provides detailed surveys of the American population drawn from federal censuses and the American Community Surveys. IPUMs was created to facilitate time series analysis and, as such, has unique industry (IND1990) and occupational identifiers (OCC1990), which are defined as to minimize changes in industry and occupation definitions over time. We use the crosswalk defined by Autor and Dorn (2013), which is a slightly modified version of occupational

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[23]Our sample begins in 1980 due to availability of M&A activity in SDC.

[24]ACS is the continuation of the decennial Census surveys post-2000.

[25]For more information, see Ruggles, Genadek, Goeken, Grover, and Sobek (2015).

identifiers (OCC1990) provided by IPUMs, to ensure time-consistent occupation categories.

We map NAICS industries from SDC to IPUMs industries, using the cross-walk provided by IPUMs, as detailed in the Internet Appendix. Following this approach, we end up with 132 industries and more than 300 occupations in each Census-year. Our IPUMs sample consists of individuals who are between 18 and 64 years old and who were employed in the prior survey. We apply the same sample criteria as in Autor and Dorn (2013) and drop military and farming occupations, residents of institutional group quarters (e.g., prisons) and unpaid family workers. We follow Autor and Dorn (2013) and calculate a labor supply weight equal to the number of weeks worked times the usual number of hours per week. Each individual is weighted by their employment weight which is equal to the Census sampling weight times the labor supply weight.

IPUMs also provides data on yearly wage and salary income ($incwage$), from which we exclude self-employed workers and observations with missing wages, weeks, or hours worked. We define hourly wages as yearly wages and salary divided by the product of weeks worked ($wkswork$) and usual weekly hours ($uhrswork$). Wages are adjusted to year 2001 dollars using the Consumer Price Index of all urban consumers in order to be comparable to the establishment level analysis. IPUMs also provides data on workers' education allowing us to define workers with graduate education (at least 5 years of post-secondary education). We aggregate all variables at the industry-Census year level by computing employment weighted averages.

We measure RSH as in the baseline analysis, using data in Autor and Dorn (2013). We merge these data with IPUMs using the occupation crosswalks detailed above. Following these steps, we can characterize occupations in a given industry-year in terms of their routine intensity and construct the share of these routine intensive occupations by industry-year.[26] We define all variables used in our analysis in the Appendix.

Table 2.11 reports summary statistics of several key variables used in the analysis. We report

[26]Appendix Table B.5 provides some examples of our sample industries with high and low routine employment shares. Industries with a high share of routine intensive occupations include accounting and legal services. On the other hand, industries with a low share of routine intensive occupations include taxicab services and vending machines operators.

the mean value across all industries for a given year along with the standard deviation in brackets. On average, a given industry reflects between 0.46-0.65% of the overall merger activity. Similar to Autor and Dorn (2013), we document that around one third of all occupations are routine-intensive. We find that over 5% of workers in our average industry had a graduate degree in 1980. This fraction increases over time and is about 8% in 2010. The average hourly wage is $16.8 in 1980. Moreover, we show an increase in the standard deviation of wages within a given industry, consistent with the fact that inequality has increased over time.

2.4.2 Industry Analysis: Results

To parallel our establishment-level results, we examine how shares of routine intensive occupations and shares of high-skill employees evolve following M&A activity. Moreover, we explore the wage implications of such technology adoption following M&As. We estimate the following specification:

$$\mathrm{y}_{j,t} = \alpha_t + \alpha_j + \gamma \cdot \log(\text{merger intensity})_{j,(t-10,t-1)} + \beta \cdot X_{j,t} + \epsilon_{j,t} \tag{2.2}$$

where t indexes years and j indexes industries. $X_{j,t}$ controls for average offshorability of tasks, time-varying at the industry level. *Merger intensity* is our proxy of M&A activity defined as the count of horizontal deals in a given decade, for a given industry, normalized by all horizontal deals in the decade and log-transformed.[27] α_j is an industry fixed effect to control for industry time-invariant characteristics; α_t is a year fixed effect to control for differences across time. The IPUMs data is only available every 10 years for the period between 1980 and 2000. As such, M&A activity is measured over three decades in our sample: 1980-1989; 1990-1999; and, 2000-2009.[28] Our outcome measures y are measured every decade in 1990, 2000, and 2010. Standard errors are

[27] Internet Appendix Table B.6 shows the key results are robust to using M&A transaction values to define our M&A measure. Specifically, we define M&A activity as the logarithm of one plus the total transaction values of horizontal deals in a given (4-digit NAICS) industry-decade normalized by total transaction values of all horizontal deals in the decade. We use the M&A count as opposed to transaction values in our baseline analysis due to the high number of observations with missing data on transaction values.

[28] Internet Appendix Table B.7 shows that the key results are robust to defining M&A activity over the first six year of each decade.

clustered at the industry level to take into account correlation in industries over time.

Column 1, Table 2.12, examines routine share intensity as our outcome variable. An increase in industry M&A intensity by 1% is associated with a 2.8% decrease in routine share intensity in the industry. These results suggest that high industry M&A intensity is associated with a subsequent decline in occupational shares of routine tasks, consistent with our hypothesis. At the same time, this process of automation can also increase relative demand for high-skill employees as technology tends to be complementary to skilled labor, leading to an "upskilling" of affected industries. Thus, column 2, Table 2.12, looks at the share of high-skill workers within a given industry, following mergers and acquisitions. The results are economically important: an increase in M&A intensity by 1% is associated with an increase in the share of highly-educated employees by nearly 1 percentage point within industries.

Similar to the establishment-level evidence, these results show that M&A activity is followed by a decrease in routine-intensive labor and a simultaneous increase in the share of high-skilled workers in a given industry. Next, we test whether these occupational changes have important implications for wages. In column 3, we explore predictions related to hourly wages. We use the log of the industry average hourly wage as the dependent variable and find an increase in the average wage in affected industries. Note these results do not necessarily translate into an increase in wages for the same employed workers but, instead, likely reflect a change in the composition of jobs as indicated in the previous two columns. To test the effect on wage polarization following M&A activity, we examine the standard deviation of hourly wages in column 4. Within industries, an increase in M&A activity by 1% increases wage disparity by 2.1%. Consistent with our establishment level findings, we report increases in wage dispersion with an industry following higher M&A activity.

Overall, the industry-level results parallel the trends we documented at the establishment-level. These results indicate that establishment-level changes in labor demand and compensation appear to aggregate to the industry level. These results are not consistent with an argument that changes at a given M&A firm are offset by counter-balancing changes at non-M&A peer firms absorbing the redundant labor from the M&A firms. These results also confirm that our within establishment evidence are not unique to our establishment-level sample, but they have industry-wide implications for labor outcomes and inequality.

2.5 Conclusion

Given the importance of trends in job polarization and wage inequality for workers, firms, and society, understanding their causes and consequences has been at the epicenter of an important literature in economics and finance. We provide micro foundations for these economy-wide labor market trends by exploring the impact of mergers and acquisitions on changes in job polarization and wage inequality.

We argue that M&As may accelerate technology adoption due to an increase in scale, improved efficiency, or lower financial constraints. Automation should in turn lead to occupational and wage changes consistent with changes predicted by skill-biased and routine-biased technological change. We find that M&As within establishments are followed by a reduction in the share of routine intensive occupations. This is often described as "hollowing-out" of the occupational distribution as routine-intensive occupations, those most easily replaced by computers, disproportionately comprise middle-skill occupations. At the same time, we also observe an ex-post increase in the demand for high-skill workers following M&As. This "upskilling" is consistent with the argument that technology is complementary to skilled human capital and, as such, increases demand for high-skill employees. The changes observed in occupational distributions are mirrored in wages: we observe an increase in the average wage and, most importantly, in overall wage inequality within establishments. We are able to generalize those findings at the macro level, where we find that industries impacted by high M&A activity exhibit similar changes in labor outcomes and wages as those identified within establishments.

A key conclusion of our results, is that the impact of M&As on target firm workers is heterogeneous. Workers engaged in highly routine activities fare the worst, while high-skill non-routine workers may seem expanded opportunities following the M&A. However, we need to emphasize a caveat: Our results are unique to the sample of employed workers. As such, they are consistent with patterns of increasing skill premia and increasing income inequality documented in the macro economy. However, our results do not take into account unemployed or under-employed workers. In particular, while we show an increase in wages following M&A activity, this is only for the employees who remain employed in the firm or industry.

Table 2.1: Summary statistics of establishment-level variables

This table reports the mean and standard deviation of key variables from the Occupational Employment Statistics (OES) by Bureau of Labor Statistics. Each observation is measured at the establishment-level. Columns 1-3 present summary statistics for the full sample; columns 4-6 present summary statistics for establishments without M&A (controls) and 7-9 for establishments with M&A (treated) the years in the sample before an M&A. All variable definitions are provided in the Appendix.

				Before M&A					
	All Establishments			Establishments without M&A			Establishments with M&A		
	N	Mean	Std. Dev.	N	Mean	Std. Dev.	N	Mean	Std. Dev.
Establishment employment	10,444	199.3	387.4	3,081	198.3	384.4	2,141	212.6	397.8
Routine share intensity (RSH) (%)	10,444	52.9	33.2	3,081	45.8	33.4	2,141	60.5	31.4
Share high-skill (%)	10,444	11.5	9.30	3,081	11.9	9.66	2,141	10.4	8.56
Average hourly wage ($)	10,444	16.5	8.66	3,081	16.8	8.65	2,141	15.60	7.96
Standard deviation of hourly wages	10,389	8.44	5.93	3,074	8.45	5.73	2,137	7.51	5.28
Offshorability	10,444	0.39	0.69	3,081	0.34	0.74	2,141	0.45	0.60

Table 2.2: Effects of M&A on establishment routine share intensity

This table presents estimates of changes in routine share intensity at establishments of M&A targets as compared to control establishments. The dependent variable is the logarithm of one plus routine share intensity (RSH) defined at the establishment-level. $Post_t$ is estimated but not reported for brevity. The sample consists of establishments targeted in M&As between 2001 and 2007 and those of matched control establishments. All variables are defined in the Appendix. Robust standard errors are clustered at the firm level. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	(1) RSH	(2) RSH	(3) RSH	(4) RSH	(5) RSH
$Post_t \cdot M\&A_i$	-0.0443***	-0.0314***	-0.0320***	-0.0317***	-0.0329***
	(0.0058)	(0.0055)	(0.0052)	(0.0058)	(0.0053)
$Offshorability$		0.131***	0.132***	0.128***	0.126***
		(0.0096)	(0.0102)	(0.0083)	(0.0084)
Establishment FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes			
Industry · Year FE			Yes		Yes
State · Year FE				Yes	Yes
Observations	10,444	10,444	10,370	10,426	10,352
R-squared	0.863	0.889	0.905	0.897	0.913

Table 2.3: Effects of M&A on establishment high-skill employment

This table presents estimates of changes in high-skill employment share at establishments of M&A targets as compared to control establishments. The dependent variable is the share of high-skill employment defined at the establishment-level. $Post_t$ is estimated but not reported for brevity. The sample consists of establishments of firms targeted in M&As between 2001 and 2007 and those of matched control establishments. All variables are defined in the Appendix. Robust standard errors are clustered at the firm level. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	(1) Share high-skill	(2) Share high-skill	(3) Share high-skill	(4) Share high-skill	(5) Share high-skill
$Post_t \cdot M\&A_i$	0.0201***	0.0172**	0.0146***	0.0169**	0.0152***
	(0.0071)	(0.0069)	(0.0047)	(0.0072)	(0.0047)
$Offshorability$		-0.0294***	-0.0272***	-0.0283***	-0.0260***
		(0.0044)	(0.0044)	(0.0044)	(0.0044)
Establishment FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes			
Industry · Year FE			Yes		Yes
State · Year FE				Yes	Yes
Observations	10,444	10,444	10,370	10,426	10,352
R-squared	0.665	0.673	0.721	0.699	0.744

Table 2.4: Effects of M&A on establishment average wages

This table presents estimates of changes in average wages at establishments of M&A targets as compared to control establishments. The dependent variable is the log-transformed average hourly wage at the establishment-level. $Post_t$ is estimated but not reported for brevity. The sample consists of establishments targeted in M&As between 2001 and 2007 and those of matched control establishments. All variables are defined in the Appendix. Robust standard errors are clustered at the firm level. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	(1) Wage	(2) Wage	(3) Wage	(4) Wage	(5) Wage
$Post_t \cdot M\&A_i$	0.0398***	0.0423***	0.0375***	0.0394***	0.0396***
	(0.0123)	(0.0123)	(0.0104)	(0.0107)	(0.0097)
$Offshorability$		0.0253**	0.0262**	0.0243**	0.0292***
		(0.0112)	(0.0115)	(0.0110)	(0.0113)
Establishment FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes			
Industry · Year FE			Yes		Yes
State · Year FE				Yes	Yes
Observations	10,444	10,444	10,370	10,426	10,352
R-squared	0.908	0.908	0.920	0.914	0.927

Table 2.5: Effects of M&A on establishment wage dispersion

This table presents estimates of changes in standard deviation of hourly wages at establishments of M&A targets as compared to control establishments. The dependent variable is the log-transformed standard deviation of hourly wages at the establishment-level. $Post_t$ is estimated but not reported for brevity. The sample consists of establishments targeted in M&As between 2001 and 2007 and those of matched control establishments. All variables are defined in the Appendix. Robust standard errors are clustered at the firm level. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	(1) StdWages	(2) StdWages	(3) StdWages	(4) StdWages	(5) StdWages
$Post_t \cdot M\&A_i$	0.0893***	0.0926***	0.0729***	0.0806***	0.0699***
	(0.0298)	(0.0295)	(0.0242)	(0.0263)	(0.0226)
$Offshorability$		0.0377	0.0321	0.0412*	0.0397
		(0.0252)	(0.0264)	(0.0235)	(0.0246)
Establishment FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes			
Industry · Year FE			Yes		Yes
State · Year FE				Yes	Yes
Observations	10,222	10,222	10,138	10,202	10,118
R-squared	0.812	0.812	0.832	0.827	0.846

Table 2.6: Mechanisms

This table presents estimates of occupational and wage changes at establishments of M&A targets as compared to control establishments, further interacting $Post_t \cdot M\&A_i$ with characteristics of the target. In Panel A, $Overlap\_Occup_i$ is an indicator variable that measures the share of employment in overlapping occupations between the target and the acquirer. In Panel B, $Acq\_Low\_RSH_i$ is an indicator variable that measures if the acquirer had high routine share intensity prior to the acquisition. In Panel C, $Private_i$ is an indicator variable that measures if the target is a private firm. In column 1, the dependent variable is the logarithm of one plus routine share intensity (RSH); in column 2, the dependent variable is the share of high-skill employment; in column 3, the dependent variable is the log-transformed average hourly wage; in column 4, the dependent variable is the log-transformed standard deviation of hourly wages. $Post_t$ is estimated but not reported for brevity. The sample consists of establishments targeted in M&As between 2001 and 2007 and those of matched control establishments. All variables are defined in the Appendix. Robust standard errors are clustered at the firm level. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

Panel A

	(1) RSH	(2) Share high-skill	(3) Wage	(4) StdWages
$Post_t \cdot M\&A_i$	-0.0106	0.0293***	0.0237	0.0795**
	(0.0071)	(0.0077)	(0.0144)	(0.0313)
$Post_t \cdot M\&A_i \cdot Overlap\_Occup_i$	-0.0222**	-0.0095	0.0266*	0.0657**
	(0.0111)	(0.0075)	(0.0159)	(0.0324)
$Offshorability$	0.161***	-0.0364***	-0.0007	-0.0243
	(0.0145)	(0.0072)	(0.0156)	(0.0428)
Establishment FE	Yes	Yes	Yes	Yes
Industry · Year FE	Yes	Yes	Yes	Yes
Region · Year FE	Yes	Yes	Yes	Yes
Observations	5,658	5,658	5,658	5,542
R-squared	0.915	0.739	0.928	0.830

Panel B

	(1) RSH	(2) Share high-skill	(3) Wage	(4) StdWages
$Post_t \cdot M\&A_i$	-0.0406***	0.0141	-0.0007	0.0441
	(0.0145)	(0.0130)	(0.0240)	(0.0495)
$Post_t \cdot M\&A_i \cdot Acq\_Low\_RSH_i$	-0.0386	0.0172	0.0913**	0.156*
	(0.0341)	(0.0197)	(0.0386)	(0.0796)
$Offshorability$	0.158***	-0.0413***	0.0125	-0.0132
	(0.0132)	(0.0085)	(0.0189)	(0.0430)
Establishment FE	Yes	Yes	Yes	Yes
Industry · Year FE	Yes	Yes	Yes	Yes
Region · Year FE	Yes	Yes	Yes	Yes
Observations	3,656	3,656	3,656	3,574
R-squared	0.914	0.734	0.922	0.841

Panel C				
	(1)	(2)	(3)	(4)
	RSH	Share high-skill	Wage	StdWages
$Post_t \cdot M\&A_i$	-0.0560**	-0.0159	-0.0386	-0.152*
	(0.0262)	(0.0216)	(0.0464)	(0.0886)
$Post_t \cdot M\&A_i \cdot Private_i$	0.0386	0.0469**	0.0781*	0.271***
	(0.0268)	(0.0226)	(0.0474)	(0.0914)
$Offshorability$	0.160***	-0.0361***	-0.0007	-0.0298
	(0.0148)	(0.0073)	(0.0159)	(0.0441)
Establishment FE	Yes	Yes	Yes	Yes
Industry · Year FE	Yes	Yes	Yes	Yes
Region · Year FE	Yes	Yes	Yes	Yes
Observations	5,430	5,430	5,430	5,314
R-squared	0.915	0.737	0.928	0.828

Table 2.7: M&As and investment in IT

This table presents estimates of changes in IT investment at establishments of M&A targets as compared to control establishments. In column 1, the dependent variable is the logarithm of one plus the $ budget for IT; in column 2, the dependent variable is the logarithm of one plus the $ budget for hardware; in column 3, the dependent variable is the logarithm of one plus the $ budget for software; in column 4, the dependent variable is the logarithm of one plus the $ budget for services. In columns 5-8, we normalize all variables by the number of employees in the establishment before taking the log and adding one. $Post_t$ is estimated but not reported for brevity. The sample consists of establishments in the CiTDB data that are targeted in M&As between 2010 and 2015 and those of matched control establishments. All variables are defined in the Appendix. Robust standard errors are clustered at the firm level. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	(1) IT budget	(2) Hardware budget	(3) Software budget	(4) Services budget	(5) IT budget/Emp	(6) Hardware budget/Emp	(7) Software budget/Emp	(8) Services budget/Emp
$Post_t \cdot M\&A_{it}$	0.0479***	0.0575***	0.0428**	0.0508***	0.0393**	0.0490***	0.0343*	0.0423**
	(0.0183)	(0.0168)	(0.0181)	(0.0187)	(0.0186)	(0.0169)	(0.0185)	(0.0189)
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry · Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State · Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	35,406	35,406	35,406	35,406	35,406	35,406	35,406	35,406
R-squared	0.948	0.950	0.952	0.952	0.916	0.927	0.930	0.924

Table 2.8: Cancelled M&As

This table presents estimates of occupational and wage changes at establishments of M&A targets that were announced and subsequently withdrawn as compared to control establishments. Cancelled M&A deals are included in the sample if they were blocked by regulators or the bidder was acquired ex-post by a third party. In column 1, the dependent variable is the logarithm of one plus routine share intensity (RSH); in column 2, the dependent variable is the share of high-skill employment; in column 3, the dependent variable is the log-transformed average hourly wage; in column 4, the dependent variable is the log-transformed standard deviation of hourly wages. $Post_t$ is estimated but not reported for brevity. The sample consists of establishments targeted in cancelled M&As between 2001 and 2007 and those of matched control establishments. All variables are defined in the Appendix. Robust standard errors are clustered at the firm level. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	(1) RSH	(2) Share high-skill	(3) Wage	(4) StdWages
$Post_t \cdot pseudo\ M\&A_i$	0.0120	-0.0349	-0.132*	-0.222
	(0.0249)	(0.0292)	(0.0742)	(0.200)
$Offshorability$	0.182***	-0.0393	-0.0458	-0.129
	(0.0257)	(0.0346)	(0.0582)	(0.160)
Establishment FE	Yes	Yes	Yes	Yes
Industry · Year FE	Yes	Yes	Yes	Yes
Observations	180	180	180	170
R-squared	0.914	0.616	0.762	0.784

Table 2.9: Within-establishment labor outcomes

This table presents estimates of changes in employment and wages within establishments of M&A targets as compared to control establishments. In column 1, the dependent variable is the log-transformed employment at the establishment-level; in column 2, the dependent variable is the log-transformed establishment average wage. $Routine$ takes a value of one if an occupation is routine, and 0 if it is non-routine. The sample consists of establishments targeted in M&As between 2001 and 2007 and those of matched control establishments. All variables are defined in the Appendix. Robust standard errors are clustered at the firm level. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	(1) Employment	(2) Wage
$Routine$	-0.0576***	-0.304***
	(0.0153)	(0.0119)
$Post_t \cdot Routine$	0.0631***	0.0049
	(0.0064)	(0.0097)
$M\&A_i \cdot Routine$	0.202***	0.0347
	(0.0436)	(0.0311)
$Post_t \cdot M\&A_i \cdot Routine$	-0.0848***	-0.0392*
	(0.0118)	(0.0218)
$OccupationalEmployment$		-0.0739***
		(0.0049)
Establishment · Year FE	Yes	Yes
Observations	20,888	18,314
R-squared	0.047	0.877

Table 2.10: M&A long-term effects

This table presents estimates of occupational and wage changes at establishments of M&A targets as compared to control establishments in an extended sample. The sample includes establishments that are observed twice over 2001-2007 (as in our main analysis) and have been also surveyd twice in our post-sample period (2008-2013). $Post_1$, $Post_2$, and $Post_3$ take the value of 1 for the first, second and third observation of the establishment post-M&A. In column 1, the dependent variable is the logarithm of one plus routine share intensity (RSH); in column 2, the dependent variable is the share of high-skill employment; in column 3, the dependent variable is the log-transformed average hourly wage; in column 4, the dependent variable is the log-transformed standard deviation of hourly wages. $Post_1$, $Post_2$, and $Post_3$ are estimated but not reported for brevity. All variables are defined in the Appendix. Robust standard errors are clustered at the firm level. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	(1)	(2)	(3)	(4)
	RSH	Share high-skill	Wage	StdWages
$Post_1 \cdot M\&A_i$	-0.0134	0.0252***	0.0273	0.0659*
	(0.0098)	(0.0082)	(0.0179)	(0.0350)
$Post_2 \cdot M\&A_i$	-0.0268***	0.0236***	0.0155	0.0193
	(0.0098)	(0.0064)	(0.0190)	(0.0428)
$Post_3 \cdot M\&A_i$	-0.0425***	0.0137*	0.0322*	0.0713*
	(0.0101)	(0.0070)	(0.0191)	(0.0418)
$Offshorability$	0.156***	-0.0166***	0.0048	0.0136
	(0.0106)	(0.0042)	(0.0133)	(0.0235)
Establishment FE	Yes	Yes	Yes	Yes
Industry · Year FE	Yes	Yes	Yes	Yes
State · Year FE	Yes	Yes	Yes	Yes
Observations	6,091	6,091	6,091	6,057
R-squared	0.907	0.685	0.918	0.828

Table 2.11: Industry-level analysis: Summary statistics

This table reports the mean and standard deviation of key variables from SDC and IPUMs for the years identified in the column header for the industry sample. Each observation is an industry-year, measured once per decade, with the exception of merger intensity, which is measured over years t-10 to t-1. All variable definitions are provided in the Appendix.

	1980	1990	2000	2010
Merger intensity (%)		0.46	0.54	0.65
		[.0075]	[.0087]	[.0132]
Routine share intensity (RSH) (%)	34.75	32.75	33.28	33.82
	[.164]	[.156]	[.155]	[.161]
High-skill employment share (%)	6.72	5.91	7.21	8.62
	[.0805]	[.0735]	[.0801]	[.0977]
Average hourly wage ($)	16.80	17.11	18.46	18.89
	[3.53]	[3.81]	[4.42]	[5.52]
Standard deviation of hourly wages	11.27	12.95	16.74	15.16
	[2.01]	[3.07]	[4.23]	[4.83]
Offshorability	0.12	0.12	0.13	0.16
	[0.43]	[0.44]	[0.45]	[0.45]

Table 2.12: Industry-level analysis: Baseline results

The table presents estimates of occupational and wage changes at the industry j and time t following M&As. In column 1, the dependent variable is the logarithm of routine share intensity (RSH); in column 2, the dependent variable is the share of high-skill employment; in column 3, the dependent variable is the log-transformed average hourly wage; in column 4, the dependent variable is the log-transformed standard deviation of hourly wages. The timeline starts in 1980 and ends in 2010 with one observation per decade for each industry. All variables are defined in the Appendix. Robust standard errors are clustered at the industry-level. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	(1) RSH	(2) Share high-skill	(3) Wage	(4) StdWages
$Merger\ Intensity_{j,(t-10,t-1)}$	-2.820	0.975	2.759	2.124
	(0.866)***	(0.241)***	(0.895)***	(1.237)*
$Offshorability$	0.365	0.012	-0.023	0.007
	(0.313)	(0.023)	(0.081)	(0.152)
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Observations	396	396	396	396
R-squared	0.956	0.965	0.960	0.97

CHAPTER 3

ENTREPRENEURIAL WAGES (WITH TANIA BABINA, PAIGE OUIMET AND REBECCA ZARUTSKIE)

Disclaimer

The research in this paper was conducted while the authors were Special Sworn researchers of the U.S. Census Bureau. Research results and conclusions expressed are those of the authors and do not necessarily reflect the views of the Census Bureau. This paper has been screened to ensure that no confidential data are revealed.

3.1 Introduction

Young firms account for 11% of US employment and are credited with a disproportionate share of total job creation (Haltiwanger, Jarmin, and Miranda, 2013). Given the importance of young firms in generating jobs, an extensive literature has explored the drivers of these new firms. However, the question of why workers join new firms remains controversial. On average, employees earn lower wages at young firms (Brown and Medoff, 2003), small firms (Oi and Idson, 1999), and when self-employed (Hamilton, 2000; Moskowitz and Vissing-Jorgensen, 2002). One literature has interpreted this fact as evidence of a willingness of employees at new firms to accept below-market wages due to offsetting attributes from working at new firms. A second, mostly theoretical, literature has instead argued that lower wages at new firms reflect selection: Young firms employ disproportionately more lower quality workers, either because new firms are lower quality or financially constrained. In this paper, we revisit this debate to separate between the wage penalty and selection interpretations of lower wages at new firms.

Using US Census employer-employee matched data over almost two decades, we confirm that new firms, defined as three years of age or younger, pay 31% lower wages, on average. However, we disprove the assumption that these workers are accepting lower wages, i.e. a wage penalty, as compared to the wages they would have earned at established firms, i.e. market wages. New firms pay economically identical wages after controlling for differences in worker quality and time invariant firm quality. Our findings suggest that a given worker considering joining either a new or established firm of equivalent quality would receive equivalent wages at both employment opportunities, supporting the selection interpretation of the new firm wage discount.

To reach these conclusions, we start by including worker fixed effects. Previous studies do not usually include worker fixed effects either because of the cross-sectional nature of the data (Brown and Medoff, 2003) or due to a different focus (Burton, Dahl and Sorensen, 2017). With these controls for time invariant differences in worker quality, we find the new firm wage differential declines by almost three fourths. Adding controls for time varying observable worker characteristics further reduces the magnitude of the new firm wage discount. These results indicate that new firms, on average, employ workers who receive lower market wages due to differences in skills or talent. A disproportionate matching of low-skill workers to new firms is consistent with positive assortative matching. New firms in our data include a large representation of low quality firms, which are unlikely to succeed over the long run, as well as higher quality new firms with greater survival potential. Alternatively, new firms may not have the financial resources necessary to employ high-wage workers.

Moreover, once we add firm fixed effects, absorbing time invariant firm quality, the wage difference between new and mature firms becomes positive, although economically small. In our setting, firm fixed effects reflect any time-invariant wage premium or discount paid to all employees of a given firm above and beyond the person-specific component of pay, captured by the worker fixed effects. Abowd, Kramarz and Margolis (1999) shows a positive correlation between this firm-specific component of pay and firm-level productivity. Equivalent workers will, on average, have relatively higher individual output in more productive firms. Our finding of a reduction in the new firm wage discount with firm fixed effects support this argument assuming new firms have lower initial quality, as compared to the set of established firms which have successfully survived to maturity.

After controlling for differences in employee and firm quality, the expected wage penalty of working at a new firm is, on average, economically insignificant. Earlier conclusions that new firms pay lower wages still hold. However, this fact is explained by the types of workers new firms employ and by the variety of firm quality represented by new firms. Assumptions regarding preferences or biased beliefs are not required to understand why workers join new firms. Instead, the difference in wages is explained by the lower mean quality of new firms or a higher likelihood of financial constraints which limit the ability to hire high skill workers.

We find similar results if we instead use a sample of only college educated workers or a set of college educated workers employed in the technology sectors. Documenting equivalent results in these samples indicates that new firms hire relatively more lower-skill workers, as compared to established firms, even within sets of high-skill workers.

To further support our argument that employee quality reflects firm quality, we do two additional tests. First, we show that firms that survive for at least 10 (5) years (firms that are likely born more productive) have higher quality initial workforces, as compared to firms which exit prior to year 10 (5). Second, we document that firms which employ higher quality workers at birth have higher 5-year total employment after their creation. These results show that the human capital of young firms is an economically important predictor of the new firms' performance. Hence, a new firm's ability to hire a high quality team matters: If firms are financially constrained or otherwise unable to secure talent, they are less likely to survive and grow.

Firm size has also been used as a proxy for firm quality, and given that young firms also tend to be small, it is important to document that our effect is distinct from the firm-size wage premium documented in Oi and Idson (1999). As expected, controlling for firm size reduces the new firm wage differential, even in the absence of worker and firm fixed effects, as size proxies for firm quality. However, we continue to observe a significant coefficient on firm age, indicating firm age is a distinct firm characteristic from firm size. Moreover, as in the baseline results, we observe a decline in the magnitude of the new firm wage differential with the addition of worker fixed effects, worker time varying controls and firm fixed effects. In addition, with both sets of fixed effects and controls for firm size, we now document a significant new firm wage premium of nearly 2% . These results show that firm age is unique and not fully captured by firm size.

One important caveat to our analysis is that we do not observe exogenous movement between firms. While this is a common feature in papers that include worker fixed effects, it potentially limits the generalizability of our results. Our conclusions apply to the real world setting where employees who chose to match to new firms presumably do so in anticipation of productive matches. However, we also find economically similar results when estimated using only exogenous job switchers, workers who had to change jobs following establishment closure.

Our paper is the first to use a large sample of employee-employer matched data for US firms over nearly two decades to examine the underlying drivers of new firm wages. A handful of prior studies have also examined the new firm wage penalty, primarily using employee-employer matched data in Europe. However, there are inconsistent findings across these European studies, likely driven by differences in empirical specifications or in country-level factors. For example, looking at young establishments in Germany, Brixy, Kohaut, and Schnabel (2007) find an 8% wage penalty, while Schmieder (2013) instead finds a 10% wage premium. The closest paper to ours is Burton, Dahl and Sorensen (2017), which uses Danish data and differs from our work in other substantial ways. While Burton, Dahl and Sorensen (2017) focus primarily on disentangling the effects of firm age on wages from the effects of firm size on wages, our paper focuses on the question of whether a given worker will receive a wage penalty when joining a new firm. Our main contribution to this literature is two-fold. First, by showing the impact of controlling for time invariant worker characteristics, time varying worker characteristics and time invariant firm characteristics - separately, we provide strong evidence that selection mechanisms explain the difference in mean wages at new and established firms. Second, we provide new evidence using a large sample of US employer-employee matched data.

We reach our conclusions using the AKM method, an approach that includes worker and firm fixed effects to model wages. It was developed by Abowd, Kramarz, and Margolis (1999) and used in Card, Heining and Kline (2013). The method was also used to explore CEOs (Bertrand and Schoar, 2003; Graham et al, 2011), investment bankers (Chemmanur, Ertugrul, and Krishnan, 2017), venture capitalists (Chemmanur, Loutskina, and Tian, 2014; Ewens and Rhodes-Kropf, 2015), and loan officers (Gao, Martin, and Pacelli, 2017). This approach uses workers who change jobs to simultaneously isolate employer and employee fixed effects. We contribute by focusing on employees at young firms and show that young firms' ability to attract high quality workers is an important predictor of the future firm performance. Understanding wages and potential employment frictions present at young firms is important. If young firms cannot hire desirable workers, they cannot grow.

Our paper also adds to the literature's understanding of why people found or join new firms, given they provide lower earnings than incumbent firms (Hamilton, 2000; Brown and Medoff, 2003). Prior studies have argued that people select into entrepreneurship due to non-pecuniary benefits (Moskowitz and Vissing-Jorgensen, 2002; Hurst and Pugsley 2011), a preference for skewness (Kraus and Litzenberger, 1976), preferences for attributes of entrepreneurial firms, such as autonomy and tolerance of risk (Roach and Sauermann, 2015), overconfidence in expected benefits (Bernardo and Welch, 2001), learning about one's own abilities through experimentation (Manso, 2016; Dillon and Stanton, 2018), measurement issues (Hurst, Li, and Pugsley 2014; Levine and Rubinstein, 2017), and sorting based on personal assets (Dinlersoz, Hyatt, and Janicki, 2016). We contribute by providing a better estimate of the wage consequences of joining a new firm, as compared to an established firm, after controlling for a given worker's opportunity set.

3.2 Why New Firms Pay Lower Wages?

In this section, we briefly describe the key theoretical arguments pertaining to new firms and wages which have previously been made in the existing literature. We group these arguments into two broad categories. The first group argues that workers voluntarily accept below market wages at new firms due to offsetting benefits associated with employment at a new firm. In effect, these papers argue the new firm wage discount is driven by a supply of workers willing to accept a wage penalty to be employed at a new firm. The second group argues that the wage differential is driven by selection. In effect, these papers argue the new firm wage discount is driven by selection of low-skill, low-wage workers into new firms.

A number of papers have argued that employees at entrepreneurial firms accept lower wages due to the presence for offsetting attributes from working at new firms. Evans and Leighton (1989) find greater autonomy to be a benefit to entrepreneurial work, and Blanchflower and Oswald (1992) find higher self-reported satisfaction among these workers. Hamilton (2000) suggests non-pecuniary benefits explain the wage difference among self-employed. Although most of these early papers focus on the self-employed, the same arguments can be applied among all workers at new firms. Alternatively, employees may join new firms due to greater tolerance of risk, as in Roach and Sauermann, (2015), preferences for skewness (Kraus and Litzenberger, 1976), or due to overconfidence in the expected benefits, as in Bernardo and Welch, (2001). Finally, workers may accept lower earnings at young firms because they are willing to learn about their own abilities in entrepreneurship (Manso, 2016; Dillon and Stanton, 2018).

The second strand of literature focuses on sorting and argues that the wage differential between young and established firms is in fact a proxy for worker ability or differences in quality across firms. Young firms are born with a given time-invariant draw of productivity, driven by differences in initial ideas, technology or resources. Failure rates among young firms are high. Better firms survive, as in Baker and Kennedy (2002) and, older firms (i.e., surviving firms) are more productive as in Pakes and Erickson (1998), Hopenhayn (1992), and Oi and Idson (1999a, 1999b). Assortative matching on firm and worker productivity would then suggest that low productivity, and hence low wage workers, would disproportionately match to young firms.

Young firms are also more likely to face financial constraints as in Evans and Jovanovic (1989), Petersen and Rajan (1994), and Hadlock and Pierce (2010). These financial constraints can be driven by higher opacity at young firms, making access to external finance more costly (Berger and Udell, 1998). As such, young firms may not be able to afford high-wage workers and disproportionately employ low-skill, low-wage workers. Moreover, low productivity firms are relatively more likely to be financially constrained, further reinforcing the correlation between low wages and low productivity young firms (Evans and Jovanovic, 1989).

3.3 Data

We combine confidential databases from the US Census Bureau to form our estimation sample. Our primary database is the Longitudinal Employer-Household Dynamics data (LEHD) maintained by the US Census Bureau. This employer-employee matched database tracks employees and their wages with various employers on a quarterly basis. LEHD data are collected from the unemployment insurance records of states participating in the program.[1] The data start in 1990 for several states and coverage of states increases over time. The data coverage ends in 2008. While our project has access to 31 states, we observe nearly 100% of private employment for these

[1] See Abowd et al. (2006) for a more detailed description of the program and the underlying data sets that it generates.

states. This comprehensive data coverage means that we cannot include all of the available states in our estimation sample due to computational constraints: The 31 states cover over 60% of the US private sector employment, which translates into billions of observations over the data sample period – an infeasible sample for a regression analysis.[2] As we explain later, the estimation strategy requires the inclusion of firms that are connected through worker mobility across firms. For that reason, instead of randomly selecting workers across 31 states, for our main analysis, we chose Maryland, Colorado and Vermont with high, average, and low population of young firms respectively. A random sample generates selection towards large firms, as explained and shown in Woodcock (2005).[3] Selecting all workers within a state ensures that almost all observations within a state are included. Including small firms is crucial for our analysis since most firms are born small. For each individual we observe total quarterly wages at the current place of employment. Although the LEHD does not contain equity ownership, wage data include all forms of compensation that are immediately taxable. Stock options are typically not taxed until exercised and, as such, are unlikely to be counted in wages at the time of the grant, but are counted at the time of the exercise. Because our data does not have information on equity ownership, we do not separate between founders and non-founders. Both are included in our data, although most employees are non-founders: On average, a new firm has 15 employees in our sample, and an average firm has two founders (Parker, 2009).[4] The LEHD also allows us to observe the age, gender, race, place of birth, and education of each employee.[5]

To construct our baseline sample, we start with all workers ever observed in the state. For these workers, we retrieve their entire work history and wages in the LEHD from 1990 through 2006.

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[2]The map of the 31 states available in the data are shown in Appendix, Figure C.1.

[3]In untabulated results, we find qualitatively similar results if we draw a random 10% sample of all workers or if we use a subsample of employees across all states who were ever employed at US public firms.

[4]One approach used in the existing literature, such as Azoulay, Jones, Kim, and Miranda (2018), is to identify founders as the highest wage earner at the time of founding. They show that in 60 to 70 percent of cases the top three earners capture the founders. However, sorting on wages at the new firm is problematic in our empirical setting, given our dependent variable is wages.

[5]Education is imputed for employees with missing education data (Abowd et al. 2006).

We stop in 2006 to have enough time passed to obtain future performance outcomes for new firms. Wages are normalized to year 2014 constant dollars and measured at the quarterly level. Following Card, Heining and Kline (2013), we also minimize part-time jobs in our sample by keeping only the observations with the highest paid wage when a given worker reports wages at multiple firms in a given quarter. To limit the probability of data errors in our sample, we drop all observations for individuals where wages change by 5,000% in one year. We use log wages in the regressions to address the skewed distribution of wages as well as to minimize the role of outliers.

In the LEHD data, we observe wages over a full quarter with no information on weeks worked. We follow the literature and drop observations for workers with incomplete quarters of employment, defined as employee-firm quarters where we do not observe both a previous and subsequent quarter of employment at the same firm. This step is acutely important in our setting as worker transitions between jobs are unlikely to occur at the exact start of a new quarter, leading to a downwards bias in wages around a job change. The implications of such a step is that we under-sample workers with especially high turnover rates. Furthermore, to minimize the computing requirements of a large sample size, we retain only the first quarterly wage estimate for each employee.

We supplement the information in the LEHD with firm-level information from the Census's Longitudinal Business Database (LBD). The LBD is a panel dataset that tracks all US business establishments and described in Jarmin and Miranda (2002). An establishment is any separate physical location operated by a firm with at least one paid employee. The LBD contains information on the number of employees working for an establishment and total establishment payroll. In addition, the LBD contains a unique firm-level identifier, *firmid*, which longitudinally links establishments that are part of the same firm. We observe the LBD for all 50 states and the District of Columbia, which allows us to measure firms total employment across all 50 states.

We also use the LBD to measure firm age. Firm age is equal to the age of the oldest establishment that the firm owns in the first year the firm is observed in the LBD (Haltiwanger, Jarmin, and Miranda, 2013). This definition of firm age will not misclassify an establishment that changes ownership through M&As as a firm birth, since a firm is defined as a new firm only when all the firm establishments are new establishments. Given that the LBD covers employer firms with at least one physical establishment, a representative new firm in our sample will be an incorporated business with a few employees and a physical office. This is a distinction from the self-employed definition of entrepreneurship who Hurst and Pugsley (2011) and Levine and Rubinstein (2017) argue have little desire to grow and are unlikely to create economic benefits beyond the self-employed. We link the LEHD to firm identifiers in the LBD using the employer identification numbers (EIN). We then track whether an individual stays at the firm or moves to work for another firm.

3.4 Empirical Strategy

To identify wage patterns specific to new firms, we adapt the AKM method as developed by Abowd, Kramarz, and Margolis (1999). We use the following specification:

$$\mathrm{y}_{it} = \alpha_i + \delta_{J(i,t)} + \eta t + \mathrm{X'}_{it}\beta + \gamma \mathrm{newfirm}_{Jt} + \epsilon_{it} \tag{3.1}$$

where y_{it} are log quarterly real wages of individual i in year t and α_i are employee fixed effects. $\delta_{J(i,t)}$ are firm fixed effects where $J(i,t)$ gives the identity of the unique firm that employs employee i in year t. η_t are year fixed effects and X'_{it} is a vector of time-varying observable individual characteristics. $newfirm_{Jt}$ is an indicator variable which assumes the value of one if in year t the worker is employed in a firm J that is three years of age or younger in that year $t.\epsilon_{it}$ is an error term.

Employee fixed effects capture the time-invariant fraction of individual pay driven by innate skill and other individual and time-invariant attributes which are rewarded equally across employers. Firm fixed effect reflects any time-invariant wage premium or discount paid to all employees of a given firm. Abowd, Kramarz, and Margolis (1999) and Song et al (2017) find significant inter-firm wage differentials. These firm-specific premiums or discounts may be explained by differences in intrinsic productivity or rent-sharing across firms. We add year fixed effects to control for time varying changes in wages across the economy. Finally, we include the set of time-varying worker controls, age and squared and cubed terms of age (to allow for a non-linear trend in wages over an employee's lifetime) and education interacted with employee age and all nonlinear terms of age (to allow for variation in the returns to skill over an employee's lifetime). This is the same specification as used in Card, Heining and Kline (2013).

For this model to be estimated, the analysis must be run on a connected set, a subset of the full data. To be in the connected set, a firm must be linked to at least one other firm in the connected set by worker mobility. We use the largest connected set available. Consistent with other studies that use a universe of all workers within a state (Woodcock 2005), our connected set contains nearly all observations and appears otherwise similar to the full set of firms.

3.5 Summary Statistics

Our baseline sample is a panel of 48.4 million worker-year observations over 1990-2006, which includes 7.1 million unique workers and 345 thousands unique firms. All observation counts and estimates are rounded according to the US Census disclosure policies. To motivate our analysis, in Figure 3.1(a), we plot average wages of employees in our sample by firm age for each two-year firm age cohort from firm birth to firms 18-19 years old and for firms 20 years or older. As in Brown and Medoff (2003), employees at young firms receive lower wages as compared to employees at older firms. Specifically, employees at firms aged 0-1 receive quarterly wages which are, on average, almost $ 2,500 lower as compared to employees at firms with age 20 years or older. This is a wage difference of 29% , as compared to the sample mean. In Figure 3.1(b), we plot a one-year wage growth of employees by employer age. Wage growth is measured for all employees at the firm, including new joiners and employees who were employed at the same firm in the last period. In contrast to Figure 3.1(a), we observe no clear pattern in wage growth across firm age. The fact that employees at firms aged 0-1 (a group disproportionately composed of employees who recently switched to a young firm) do not realize wage declines is inconsistent with theories of workers at new firms accepting a wage discount due to their offsetting attributes. Instead, this group experiences an average wage growth of 5.5% , a year on year wage growth rate that is above the sample average of 4.6% .

In Table 3.1, we report summary statistics for firms (in Panel A) and workers (in Panel B) in our sample. In Panel A, column 1, we report mean values and standard deviations, in parentheses, calculated across all firm-year observations in our sample. In column 2, we report statistics for established firms, defined as firms four years of age or greater. In column 3, we report statistics for new firms, defined as firms less than four years of age. As expected, Panel A shows that new firms are significantly smaller, in terms of employee counts. New firms in our sample have an average of 15 employees, as compared to nearly 210 employees at established firms.[6] However, in terms of percent of male employees and percent of college educated workers, both samples are similar economically. In Panel B, column 1, we report summary statistics calculated across all worker-year observations in our sample. Column 2 contains all employees at established firms and column 3 samples all employees at new firms. As in Figure 3.1, wages at young firms are lower and wage growth is similar, as compared to established firms. As expected, employees at older firms have longer tenures, but economically similar representations of males and college educated workers.[7]

In Table 3.2, we report summary statistics for the employees who switch and do not switch employers. Given that our estimation strategy depends on the assumption that employees who switch jobs are representative of the overall sample, we report these summary statistics for the set of employees who never switch employers during our sample (column 1) and employees who switch employers (column 2). We find workers are economically similar in the two groups in terms of education and gender. However, job switchers are younger, have lower tenure, earn lower wages and have higher wage growth. These results are consistent with a finding that younger and shorter tenure workers switch jobs more frequently as in Topel and Ward (1992). The table also reveals that jobs switchers are more common that non-switchers and account for almost three quarters of all observations, further mitigating any representatives concerns of the switchers.

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[6]The median new (established) firm has an employment of 6 (13). Due to the US Census confidentiality rules, the medians are calculated as an average of observations within an interquartile range.

[7]In Panel B, tenure is measured as maximum numbers of years the worker is employed by the current firm. We report that the average tenure of workers at new firms is 3.2 years. This is longer than the reported average firm age at new firms in Panel A of 1.8 years. The difference reflects the fact that some employees stay with a young firm after the employer matures and becomes an established firms.

3.6 Baseline Results

We report our baseline estimations in Table 3.3. All standard errors are double clustered at the firm and at the worker level. To facilitate interpretation with previous work, we first estimate the new firm wage penalty using a simple OLS with year fixed effects. We then add individual fixed effects to control for time invariant worker quality. We next add controls for employee characteristics to control for time-varying observable employee characteristics. Finally, we add firm fixed effects to control for time invariant firm quality. We discuss the interpretation of each regression next.

3.6.1 OLS Estimation

As reported in column 1, new firms pay wages that are 31% lower as compared to established firms, after controlling for year fixed effects. This is consistent with results in Brown and Medoff (2003) and Ouimet and Zarutskie (2014). This wage gap may be due to employees accepting a wage penalty in return for compensating differentials or due to differences in the types of employees hired at new firms.

3.6.2 Worker Fixed Effects

In column 2, we include worker fixed effects. By controlling for time invariant worker quality, the coefficients on new firms is cut by almost three fourths. In this specification, a worker who switches between an established and new firms will earn, on average, an 8.7% lower wage at the new firms. The difference in the magnitudes of the coefficient on new firm between columns 1 and 2 tells us that young firms employ, on average, workers who earn less – workers who presumably have lower time-invariant skill. There is also a dramatic increase in the R-squared of this regression, suggesting that time invariant worker traits explain most of the wage variation.

Young firms may disproportionately hire low-wage workers because less productive workers match to new firms or because they are financially constrained and cannot afford to pay the high wages necessary to attract high skill workers. The set of new firms in our sample includes a mix of both low quality new firms – that are unlikely to survive beyond four years – as well as high quality young firms with strong growth potential. Under an assumption of positive assortative matching, lower quality employees will match to lower quality firms and receive lower wages. Financial constraints, common at new firms, will likely reinforce this relationship as lower quality firms will have more difficulty raising capital.

By adding worker fixed effects, we can identify the new firm wage penalty which is not driven by employing workers of lower intrinsic quality. However, by adding the worker fixed effect, we now estimate the new firm dummy variable using only the sample of workers who switch jobs. We argue that this limitation does not skew the results given the generally similar summary statistics reported for job switchers and non-job switchers in Table 3.2.

In column 3, we add controls for observable time-varying employee characteristics associated with wages. We control for age, age squared and age cubed to control for typical non-linear patterns in wages over the career of a typical employee. We also interact the age terms with the employee education level to allow for the fact that more educated workers can have different wage patterns across time (Card, Heining and Kline, 2013). Given new firms disproportionately employ time invariant lower quality workers, it is reasonable to expect that young firms may also disproportionately employ workers at points in their career where they would expect lower wages (Ouimet and Zarutskie, 2014). Indeed, after controlling for time-invariant worker characteristics, the coefficient on the new firm wage penalty is further reduced in magnitude to -0.077, consistent with new firms hiring workers at points in time in their career where they would command lower wages.

3.6.3 Individual and Firm Fixed Effects (AKM)

In Table 3.3, column 4, we add firm fixed effects, thereby estimating an AKM regression. The firm fixed effects capture the firm-specific and time-invariant component of compensation above and beyond the person-specific component of pay, as captured by the worker fixed effects. The coefficient on new firm is now positive and statistically significant, equals to 0.7% , and economically small. The time-invariant and firm-specific component of compensation captured by the firm fixed effects is correlated with firm productivity (Abowd, Kramarz and Margolis, 1999). As such, the change in the coefficient on new firm with the addition of firm fixed effects is consistent with new firms, on average, being of lower quality. This result suggests that for a given worker who has job opportunities from a similar quality new and established firm, the expected wage differences of going to work at the new firm are, on average, economically insignificant.

The worker fixed effects from this specification are an estimate of time invariant worker quality after controlling for time-varying worker characteristics and time-invariant firm differences. We plot the mean worker fixed effect (as estimated in this specification) by firm age in Figure 3.2(a). By construction, the average worker fixed effect across the whole sample is zero. We observe that the mean worker fixed effect is negative for firms aged 0-1, increases with firm age and turns positive for firms aged 12-13, peaks for firms aged 16-17, then goes down, becoming slightly negative for firms aged 20 years and older. These novel statistics are consistent with the argument that young firms employ lower quality workers. These statistics also are inconsistent with another set of theories that explain lower wages in new firms due to learning and skill accumulation. They suggest that workers might accept lower wages at new firms because they learn skills that allow them to experience faster wage growth down the road.

Adding firm fixed effects changes the sample used to estimate the coefficient on new firm in a manner similar to adding person fixed effects. With firm fixed effects, the coefficient on new firm is only estimated for the set of firms which survive for four or more years. To ensure that this is not introducing a significant bias, we estimate the same regression but define new firms as ages zero to one. The results are reported in Appendix Table C.1. We find qualitatively similar results.

Our estimates are based on a sample of employees who endogenously match to firms. While the presence of this type of endogenous mobility does not invalidate AKM assumptions, it does impact the interpretation of our findings.[8] Our sample of employees who move to new firms is likely biased towards employees who specifically anticipate relatively higher productivity at these new firms. While it is true that employees who match to established firms are likely to exhibit a similar bias, this is unlikely to fully offset the effect at new firms. Wage gains associated with this type of endogenous mobility should be impounded into any new hire wage bump. Given new hires

[8] Section 9 discusses in more detail the type of endogenous mobility which can invalidate AKM assumptions.

are relatively more common at new firms, this could impact the estimate of the new firm coefficient. However, the effect, if any, appears to be modest. First, as discussed earlier, in Appendix Table C.1, we define new firms as firms aged zero to one. In this specification, new hires should be an even larger part of total firm employment, yet, we observe no meaningful change in the estimate of the new firm coefficient.

Second, we find no difference in the new firm wage estimate when controlling for the one form of endogenous mobility we can directly observe in the data, the decision to leave the current employer. Workers who leave an established firm voluntarily to join a new firm presumably anticipate especially higher productivity gains, as revealed by the preference for employment at the new firm compared to the set of other new employment opportunities and the existing job. On the other hand, workers who are required to leave their existing position at an established firm due to a firm closure and then select to join a new firm are choosing from a smaller set of options. If employees facing a relatively more constrained choice set experience significantly lower new firm wage differentials, then this would suggest that endogenous mobility has important implications for our results. To test this prediction, we separately estimate the new firm wage differential for two sets of employees, employees departing a continuing establishment and employees departing a closing establishment. As reported in Appendix Table C.2, columns 1 and 2, workers leaving a closed establishment experience a similar new firm wage differential.

Likewise, our results are not driven by workers moving between new and young firms. In Table 3.4, we expand Equation 1 to include ten dummy variables capturing firms in each two-year age cohort from birth to firms 18-19 years old. Firms aged 20 or greater are the excluded set. We then repeat the same specifications as in Table 3.3, starting with an OLS estimation and then adding worker fixed effects, controls for time-varying characteristics and firm fixed effects. The pattern which emerges is consistent with the earlier table. In an OLS framework, young firms pay lower wages. However, as we add controls which absorb differences in worker and firm quality, the wage discount at younger firms becomes a wage premium.

In conclusion, on average new firms pay lower wages. However, the large wage difference observed when just looking at simple averages is driven by the fact that new firms hire disproportionately more workers who command lower wages due to lower intrinsic quality as well as more workers at a point in time when they are commanding relatively lower wages due to youth or inexperience. Moreover, some new firms are of time invariant lower quality. These firms are likely to always pay lower wages, even if they are able to survive to maturity. Controlling for individual time invariant and observable time varying characteristics as well as firm time invariant characteristics largely explains the difference in wages between new and established firms.

3.7 Alternative Samples

In the previous section, we show that new firms disproportionately hire low-skill, low-wage workers when using the full sample of employees. In this section, we re-estimate the same specifications but limit the sample to sets of high-skill workers, namely college educated workers or college educated workers employed in the high tech sector. If our results hold in these two alternative samples, then we can conclude that new firms hire relatively more low quality workers, as compared to established firms, across different distributions of skill.

3.7.1 College Educated Employees

We start by looking at the subsample of college-educated workers, defined as employees with sixteen or more years of education. A large literature in economics shows that highly educated workers are also relatively more skilled, compared to the general population. In Table 3.5, we repeat the same empirical specifications as used in the baseline sample but applied to the sample of college educated workers. We find similar results to the baseline sample with all workers. In a univariate setting, we still observe a significantly lower wage at new firms, as reported in column 1. As in Table 3.3, employee fixed effects (column 2) and worker time varying characteristics (column 3) continue to be important explanatory variables of wages in new firms, even within the more homogenous set of college-educated workers. Moreover, adding firm fixed effects (column 4) lowers the new firm wage difference to be statistically zero.

3.7.2 High Technology Firms

In Table 3.6, we further restrict the sample to just college educated workers at high technology firms. We define the high technology sector to include firms in computers, biotech, electronics and telecom, and use the first industry observed for a given worker.[9] We focus on these industries given the concentration of high value startups in these industries. Overall, the pattern of wages is similar for college educated workers in high technology areas as compared to college educated workers in the full sample.

3.8 Controlling for Firm Size

In the previous analysis, we do not control for firm size. Firm size is positively correlated with firm age and negatively correlated with wages. As such, the exclusion of this variable is biasing our coefficient on “new firm” downwards, or making the wage penalty for working at new firms appear more negative. We chose not to include firm size in the baseline estimation to capture the typical wage implications for a given employee joining a new firm, which in almost all likelihood will also be a small firm. However, there is value in understanding how much of the wage penalty associated with new firms is driven by firm size. Hence, in Table 3.7, we add firm size to the baseline regressions. Specifically, we measure firm size as log employment and the second and third order transformations of log employment.

In column 1, after controlling for firm size and year fixed effects, we find a negative and significant coefficient on new firms. However, the coefficient on new firms is significantly smaller, as compared in the baseline estimation suggesting that the relation between firm size and wages can explain some of the baseline finding. Moreover, as in Oi and Idson (1999), firm size is a significant predictor of wages. This result is inconsistent with Burton, Dahl and Sorenson (2017) which finds that firm age has no bearing on wages, after controlling for firm size in a sample of Danish firms,

[9]Specifically, we define a firm as being in the ”Computer” industry if its primary SIC code is 3570-5379, 5044, 5045, 5734, or 7370-7379. A firm is in the ”Biotech/Medical” industry if its primary SIC code is 2830-2839, 3826, 3841-3851, 5047, 5048, 5122, 6324, 7352, 800-8099, or 8730-8739 excluding 8732. A firm is in the ”Electronics” industry if its primary SIC code is 3600- 3629, 3643, 3644, 3670-3699, 3825, 5065, or 5063. A firm is in the ”Telecom” industry if its primary SIC code is 3660-3669 or 4810-4899.

likely reflecting differences in the samples of new firms across the two countries.

After controlling for individual fixed effects in column 2, the coefficient on new firm is further reduced, but remains negative and significant. Furthermore, with the addition of time-varying controls for observable worker characteristics in column 3, the magnitude of the new firm wage differential is again further reduced.

In column 4, with the addition of firm fixed effects, we report a positive and significant coefficient on new firms. The coefficient is now an economically modest but not insignificant 1.7%. These results suggest that employees at larger new firms realize a wage premium. Likewise, adding controls for firm size increases the coefficient on new firm if we use just the sample of college educated workers (column 5) or college educated workers in the tech sectors (column 6).

3.9 Tenure Wage Relationship

Another common belief is that young firms offer higher wage gains for retained employees. Such a pattern would be consistent with financial constraints, where firms essentially borrow from workers in the early years and then repay them with higher wage growth as the firm becomes less financially constrained (Guiso, Pistaferri and Shivardi, 2013). To test this, we create a set of dummy variables for employees with 0, 1, 2, 3 or 4 or more years of tenure with their given firm and interact these variables with our new firm dummy variable. We then repeat the same specifications as in the baseline and report the results in Table 3.8.

We show a dramatic increase in wages at established firms with greater tenure. The omitted group is workers with 0 tenure at established firms. These regressions are estimated on all workers and, as such, the sample of workers used to estimate wages with 0 tenure is different from the sample of workers used to estimate wages with 1 year of tenure. In column 1, the large difference in wages between the 0 tenure and 1 year tenure is primarily driven by a selection effect. Workers who leave within a year are, on average, less productive workers. Alternatively, by including worker fixed effects in column 2, we can now observe the impacts of tenure on wages after controlling for differences in worker quality across the different tenure groups. As predicted, the wage increases with tenure are now much more gradual at established firms. Adding time varying controls or firm fixed effects has relatively little impact on the tenure wage gradient at established firms.

In column 1, with just year fixed effects, there is a significantly less positive tenure-wage relation at new firms. This is consistent with results in Brown and Medoff (2003) who show that in a cross-sectional analysis wages grow faster with tenure at more established firms. However, this appears to be driven by the higher proportion of lower quality firms in the set of new firms. These firms may not be able to increase wages over time as they are less productive and already starting to fail. Once we include firm fixed effects, in column 4, there is now a more positive tenure-wage relation at new firms, consistent with a financial constraints mechanism.

3.10 Does Worker Quality Matter for New Firm Performance?

In the previous sections, we document that young firms disproportionally hire low quality workers. We argue that this selection can be attributed to a matching of less productive workers to less productive new firms or by the fact that new firms are financially constrained and less able to pay for high skill workers. The two explanations are inter-related: financial constraints will likely reinforce assortative matching as lower quality firms will have more difficulty raising capital. Because the two explanations are difficult to separate empirically, we do not try to distinguish between them, leaving this for future research. Instead, as a means to further support both selection mechanisms, we explore the relation between worker quality and new firm future performance, which should correlate with intrinsic firm quality. We explore two tests. First, we plot mean worker fixed effects using a two by two sort on firm age and whether or not the firm survives to year 10. Then, in a regression setting, we investigate whether firms with higher quality workers at birth perform better ex-post. Results from both test a mechanism where low quality workers disproportionately match to low quality firms and financially constrained firms cannot hire high skill workers and underperform (Evans and Jovanovic, 1989).

In Figure 3.2(b), we plot mean worker fixed effects from the baseline AKM wage regression specification in Table 3.3, column 4. We measure the quality of a firm's workforce in a given year as the firm's mean worker fixed effect, using all workers employed by the firm in that year. In Figure 3.2(b), we look at the variation within firm age group by whether or not the firm survived to year 10. Firms that survive to year 10 (in green) have above average worker quality at birth: Among surviving firms, firms with age 0 to 1 have worker fixed effects equal to 4.7% higher than the average worker's fixed effect (which includes workers from all firms: new and established). It also shows, that firms that exit within 9 years of birth (in red) have well below average worker quality at birth: among these firms, the firms with age 0 to 1 have worker fixed effects 9.4% lower than average. The difference between the mean fixed effect for surviving and failing firms aged zero to one is 21% of the standard deviation of the worker fixed effect across the full sample (the standard deviation is 0.66). These univariate results support our argument that young firms which disproportionately hire low-wage workers are themselves worse quality, on average. The figure also shows that the difference in worker fixed effects between surviving and failing firms shrinks as firms age: the differences goes down from 14.1% to 6.5% for firms with age 8 to 9, however, this could also be driven by the fact that the surviving to year 10 or not is less informative of firm quality at older firms.

In a second set of tests, we examine whether the ability of young firms to attract a high quality workforce at birth predicts young firm performance post-birth. In these tests, we use the initial workforce quality of a firm, or the average worker fixed effects using only workers employed by that firm in its first year of existence. Then, in OLS regressions, we predict a new firm's 5-year exit rate and 5-year employment as a function of these worker fixed effects. Five-year employment is set to zero if the firm fails before year 5.[10] In all these regressions, we also control for initial firm employment, birth year, and state and industry fixed effects. We report the results in Table 3.9. The workforce quality at firm birth is positively associated with the firm survival and future employment. Moreover, the relationship is economically significant. When all controls are included (columns 2 and 4), one standard deviation increase in the worker fixed effect predicts an 8% higher survival rate (from the mean survival rate of 41.5%) and a 9% higher employment. The caveat is that these estimates do not present a causal relation between the worker intrinsic quality

[10]We find similar results if we use a logit specification to predict startup exit or if we measure startup exit in 6 or 4 years instead of 5 years. We also find similar results if we predict firm future employment, conditional on survival.

and the new firm future performance (which should correlate with firm quality). However, they do contribute to the literature that used similar methodology to estimate an effect of CEOs (Bertrand and Schoar, 2003) on firm performance. Our results show that the human capital of young firms is an economically important predictor of the new firms' performance. Hence, a new firm's ability to hire a high quality team matters: If firms are financially constrained or otherwise unable to secure talent, they are less likely to survive and grow.

3.11 Validating AKM Assumptions and Endogenous Mobility

In order to interpret the regression coefficients from an AKM specification in an unqualified manner, certain conditions must be met. In this section, we show the validity of our empirical approach by repeating the diagnostic tests used in Card, Heining and Kline (2013). In the AKM model, the error term consists of three separate random effects: 1) a firm-employee match component; 2) a unit root component; and 3) a transitory error. All three terms must be uncorrelated with the firm fixed effects and three types of endogenous mobility can violate this assumption. We discuss each in turn.

One problematic type of endogenous employee mobility would occur if employees sort into firms based on a firm-employee match component. An example of this type of mobility follows when employee job transitions are motivated by an expectation that employee-specific traits will be specifically valued and compensated by the new employer, but not by other employers. It is possible to test for such sorting in two ways.

First, if employees tend to move to jobs based on the match component, then people who exchange workplaces will not necessarily experience systematic wage changes. Alternatively, in the absence of worker-firm specific matches, the wage gains will be symmetric to the losses. More precisely, if an average worker gains when moving from firm A to firm B, then an average worker moving from firm B to firm A should realize symmetric loses. This symmetry is due to differences in wage premiums across firms. An individual who joins a workplace where other employees are highly paid will, on average, experience a wage gain, whereas an individual who joins a workplace where others are poorly paid will experience a wage loss.

To test for this symmetry, we present event-study analyses that examines the wage effects of switching employers, as in Card, Heining and Kline (2013). Specifically, we begin by calculating the distribution of mean co-worker wages across all person-year observations. For each job change, we classify the origin and destination firms into quartiles, based on the mean wages of co-workers in the firm at that point in time. We then assign job changes to one of 16 groups based on the quartiles of coworker wages at the origin and destination workplaces. Finally, we calculate mean wages in the two years before and after the job change event for each group and plot in Figure 3.3.

For clarity, Figure 3.3 only shows the wage profiles for workers leaving quartile 1 and quartile 4 employers (i.e., those with the lowest- and highest-paid coworkers). The figure provides strong evidence that moving to a job with higher paid coworkers raises pay and *vice versa*. Most importantly, the figure shows the approximate symmetry of the wage losses and gains for those who move between quartile 1 and quartile 4 firms. Namely, workers who move from the 4^{th} to the 1^{st} quartile realize wage losses that are similar in magnitude to the wage gains of workers who move from the 1^{st} to the 4^{th} quartile. The gains and losses for other mover categories exhibit a similar degree of symmetry, particularly after adjusting for trend growth in wages (see Online Appendix Figure C.2). This symmetry suggests that a simple model with additive worker and firm effects may provide a reasonable characterization of the mean wages resulting from different pairings of workers to firms.

Second, if wages tend to be set at the worker-firm match level, then the implication of such a wage setting mechanism is that neither worker nor firm fixed effects would explain much variation in wages. However, across all our AKM specifications, the R-squared ranges from 80 to 82 percent, suggesting that firm and worker fixed effects explain large fraction of variation in wages.

We find no evidence that the unit root component of the error term violates the AKM assumptions. If a unit root error component were correlated with the firm fixed effects, then job transitions would systematically occur following a pattern of either increasing or decreasing wages at the prior employment. Such a pattern is best motivated by a mechanism where worker ability is revealed slowly over time. Under this scenario, a high ability worker could realize wage increases at her current employer before making the transition to a firm with a relatively greater density of high-ability workers, a firm which is likely to also be a high wage firm. If true, the individual fixed effect would be biased low due to the years before the high quality was revealed. Moreover, this would lead to an over-estimation of the firm fixed effect for high quality worker/high wage firms due to the bias in the individual fixed effects.[11] However, we find that the data does not support the existence of such a pattern. In Figure 3.3, we find no evidence of trends in the wages of workers pre-transition based on the future transition (e.g. low to high wage firm or high to low wage firm). Most importantly, even if the estimates of the fixed effects themselves were biased, (which would change the interpretation of the Rˆ 2 of regressions), the interpretation of the estimate on the new firm dummy should not be affected. For example, with the inclusion of worker fixed effects, the estimate on the new firm dummy is measuring relative wage growth for a given person who switches between young and old employers.

Finally, our results would be biased if fluctuations in the transitory error term were correlated with mobility patterns between higher and lower wage firms and, potentially, with new firm status. In other words, workers who have recently received a positive (negative) transitory wage shock will be more likely to move to higher (lower) wage firms, leading to attenuation of the estimated employment effects. Essentially, this would predict that transitory shocks are followed by a systematic pattern of job changes to one specific type of firm: (1) high vs. low wage; or (2) new vs. established firm. To mitigate this concern we explore a highly relevant shock in our setting, unemployment rates. It would be concerning if workers are more likely to transition to young firms during periods of high unemployment and receive lower wages. However, we find no such evidence, as reported in Appendix Table C.1, columns 3 and 4.

3.12 Conclusion

In this paper, we use US Census administrative data to report important facts regarding wages at young firms. As in earlier studies, we confirm a lower average wage at new firms. We document

[11] Likewise, this same pattern would lead to an under-estimation of the firm fixed effect for low wage firms if low ability is revealed slowly over time. For reference, please see Card, Heining and Kline (2013).

that nearly three quarters of this wage difference can be attributed to differences in worker quality at new firms. These results mitigate the common perception that employees joining new firms accept a wage penalty. Instead, most of the observed wage difference is due to the fact that these new firms are employing relatively more workers who command lower wages on the market due to differences in inherent skills or experience.

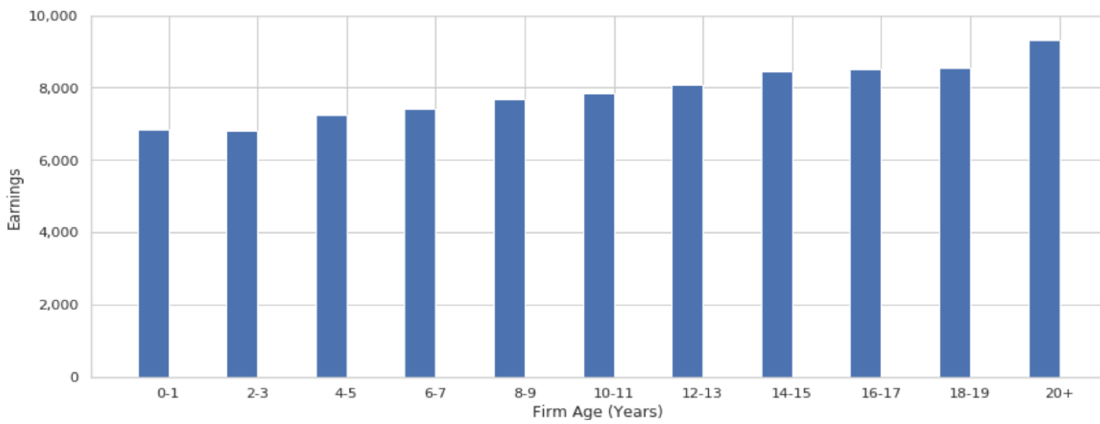
Moreover, once we also control for time varying observable worker characteristics and firm fixed effects, the wage penalty disappears and is instead replaced by a statistically significant but economically small wage premium at new firms of 0.7% . In this saturated specification, we control for time invariant differences in firm quality. This is important as new firms in our data will include a varied group of both low quality new firms, which are unlikely to succeed over the long run, as well as high quality new firms with tremendous potential. As such, our results can be interpreted as saying that a given worker with job opportunities at a new and established firm of equivalent quality will expect to earn equivalent wages at both.

These results contradict the earlier assumptions that workers had to accept a wage penalty, on average, when joining a new firm and add to our understanding of why individuals chose to join young businesses. These results no longer require that employees of new firms offset a wage penalty with a compensating differential associated with working at a new firm. Instead, the new firm wage difference appears to be driven by selection. New firms disproportionately hire lower wage workers due to either positive assortative matching or higher likelihood of financial constraints at new firms which limit the ability to hire high wage workers.

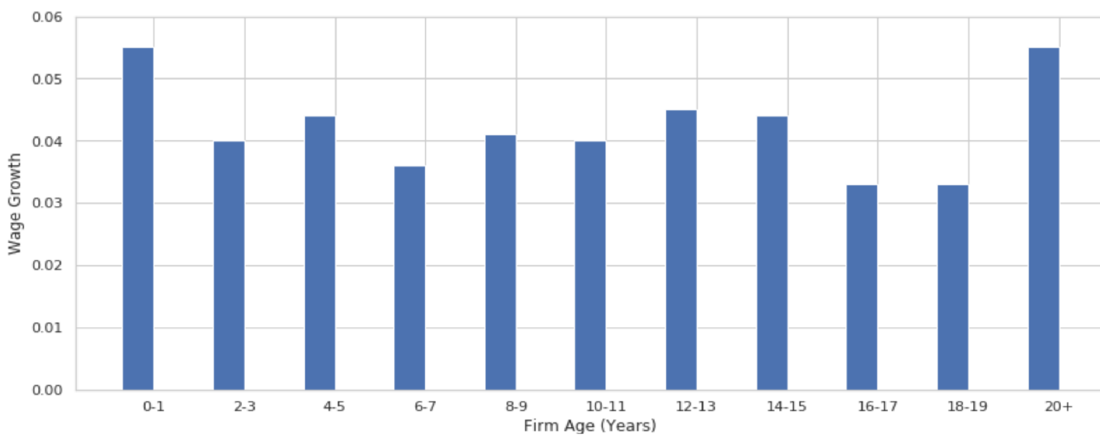
One important implication of these results is that initial worker quality at a new firm is a proxy for firm quality and an economically important predictor of future firm performance. We show that initial worker quality at a new firm can predict 5- and 10-year survival rates and future employment. Hence, a new firms ability to hire a high quality team matters: If firms are financially constrained or otherwise unable to secure talent, they are less likely to survive and grow.

Figure 3.1: Average Wages and Average Wage Growth by Firm Age

Figure shows mean worker wages (Figure (a)) and mean worker wage growth (Figure (b)) by employer age of all worker-years in the baseline sample. The baseline sample is a worker-year panel from 1990 through 2006. In Panel A, wages are quarterly and normalized to real 2014 dollars. In Panel B, wages growth is the log differences between the current and the previous year quarterly wages.



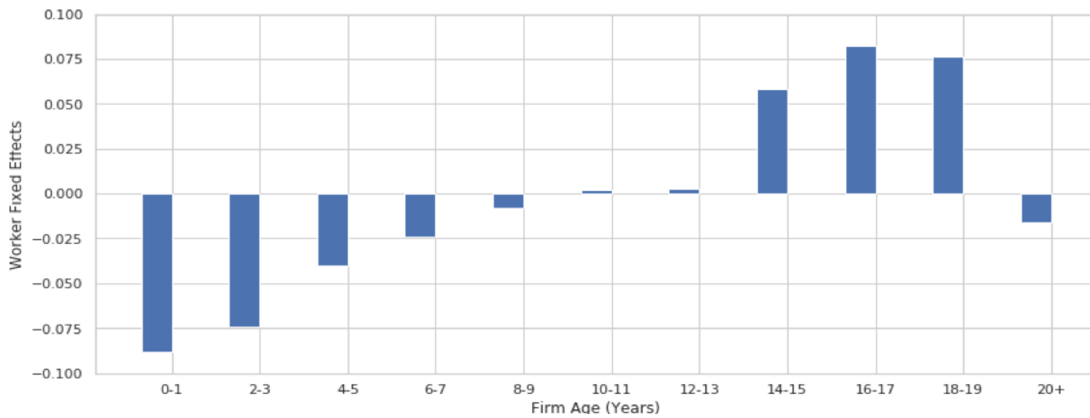
(a) Average Wages



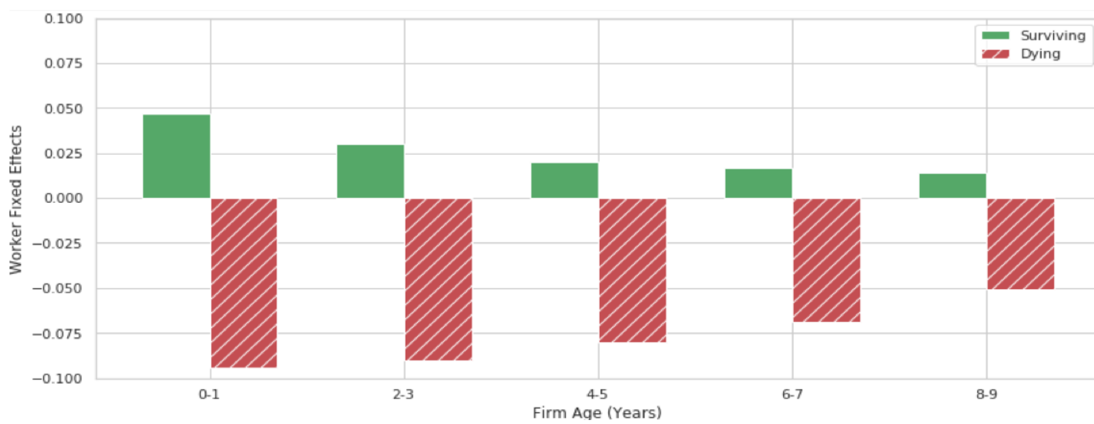
(b) Average Wage Growth

Figure 3.2: Worker Fixed Effects by Firm Age

Figure shows mean of worker wage fixed effects by employer age for workers in the baseline sample. The baseline sample is a worker-year panel from 1990 through 2006. Wages are log normalized to real 2014 dollars. Worker wage fixed effects are estimated from the baseline wage regression in Table 3.3, column 4. Figure (a) reports the statistics for all worker-years from the base sample.Figure (b) shows the statistics for a sub-sample of worker-years at firms that survive for at least ten years (in green) and for a sub-sample of worker-years at firms that exit within nine years of the firm birth (in red).



(a) All Firms



(b) Surviving and Dying Firms

Figure 3.3: Mean Wages of Job Changers Classified by Quartile of Mean Wages of Coworkers at Origin and Destination Firm

Figure shows mean wages of workers from the baseline sample who change jobs (i.e., employers) in the year zero, and held the preceding job for two or more years (years -2 and -1), and the new job for two or more years (years 1 and 2). The baseline sample is a worker-year panel from 1990 through 2006. Each job is classified into quartiles based on mean wage of coworkers. Wages are log normalized to real 2014 dollars.

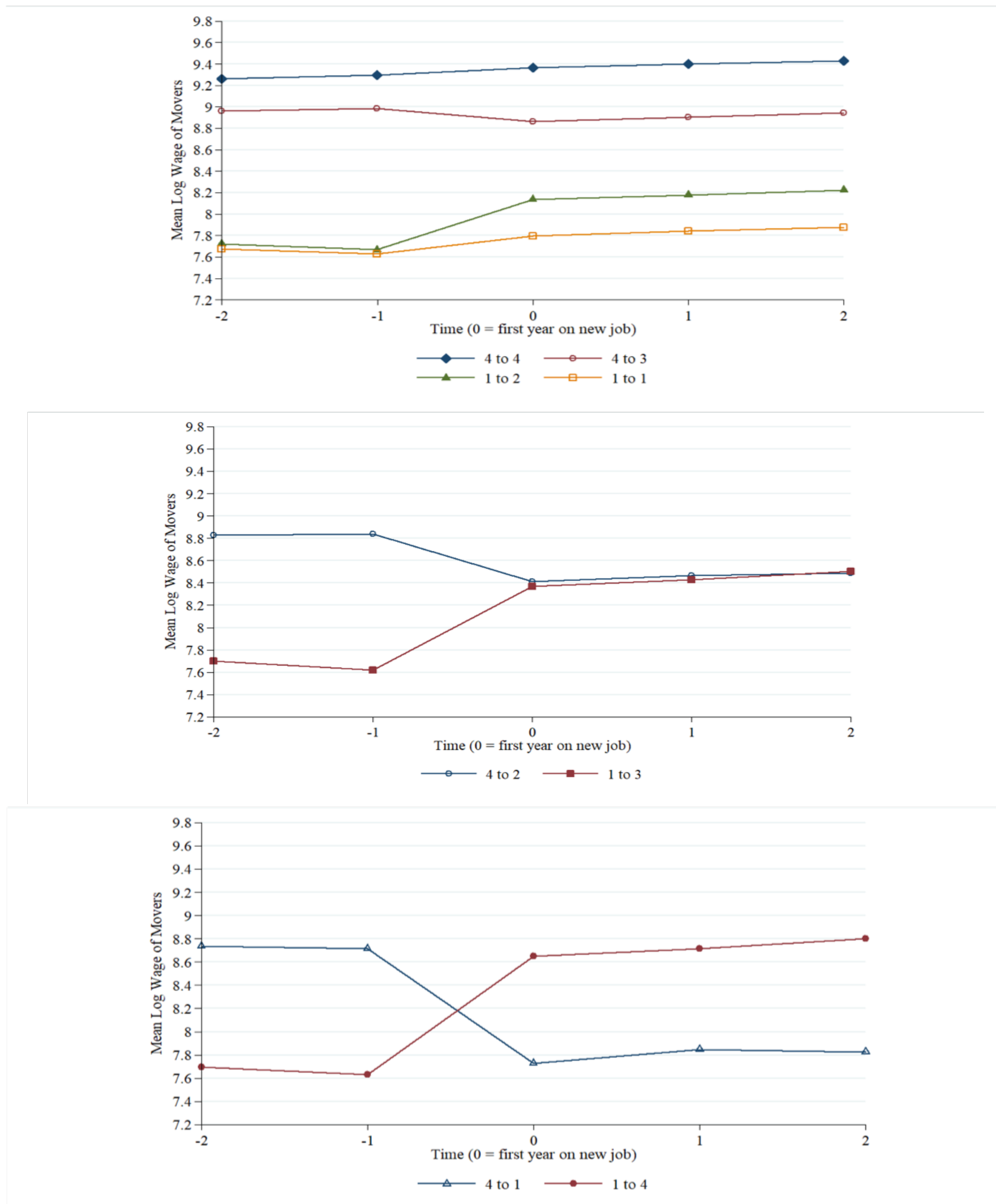


Table 3.1: Summary Statistics for New and Established Firms

Panel A shows mean (standard deviation) statistics at the firm-year level, and Panel B at the worker-year level for the baseline sample. The baseline sample is a worker-year panel from 1990 through 2006. Column 1 reports statistics using the sample of all firms. Column 2 (3) reports statistics for established firms (new firms). Established firm is a firm aged four or older; new firm is aged three years or less. In Panel A, workforce statistics are calculated at a unique firm-year level in a following way: first, for a given variable the average is calculated for each firm-year across all workers employed by that firm-year; second, reported means and standard deviations are calculated across firm-years.

Panel A. Firm-year level variables

	(1) All Firms	(2) Established Firms	(3) New Firms
Firm Age	11.1	13.7	1.8
	(8.1)	(7.3)	(1.0)
Firm Employment	167	210	14.6
	(965)	(1,084)	(131)
Percent Male Employees	0.532	0.528	0.545
	(0.330)	(0.325)	(0.349)
Percent College Educated Employees	0.355	0.364	0.323
	(0.254)	(0.246)	(0.276)
Number of Observations (millions)	2.1	1.6	0.45

Panel B. Worker-year level variables

	(1) All Firms	(2) Established Firms	(3) New Firms
Quarterly Earnings (2014$)	8,536	8,673	6,818
	(7,602)	(7,643)	(6,839)
Wage Growth	0.046	0.046	0.046
	(0.485)	(0.476)	(0.582)
Tenure (years)	5.7	5.9	3.2
	(4.4)	(4.5)	(2.7)
Age	38.7	38.9	36.0
	(12.8)	(12.7)	(12.7)
Male	0.524	0.523	0.535
	(0.499)	(0.499)	(0.498)
Education (years)	13.9	13.9	13.6
	(2.6)	(2.6)	(2.5)
Number of Observations (millions)	48.4	44.8	3.6

Table 3.2: Summary Statistics for Workers Who Change and Do Not Change Employers

Table shows summary statistics for workers who never change employers in the sample (Column 1) and change employers at least once (Column 2) for the workers in the baseline sample. The base sample is a worker-year panel from 1990 through 2006. Statistics are means and standard deviations (in parenthesis).

	(1) Do Not Move	(2) Move
Quarterly Earnings (2014$)	10,020	8,002
	(8,905)	(6,999)
Wage Growth	0.018	0.056
	(0.372)	(0.519)
Tenure (years)	8.3	4.7
	(5.3)	(3.6)
Age	41.9	37.5
	(13.4)	(12.3)
Male	0.558	0.512
	(0.496)	(0.500)
Education (years)	14.2	13.8
	(2.5)	(2.6)
Number of Observations (millions)	12.8	35.6

Table 3.3: New Firm Wages for All Workers

Table reports baseline results of wages at new firms. The baseline sample is a worker-year panel from 1990 through 2006. In all columns, the dependent variable is the log of worker total quarterly wages. Wages are in real 2014 dollars. New firm is defined as a firm of three years of age or less. Time-varying worker controls include worker age squared, worker age cubed, worker age times education, worker age squared times education, worker age cubed times education. Worker age is log transformed. Education is measured in years of schooling and log transformed. Note, worker age and education are not included as linear controls in regressions with worker fixed effect since they are collinear with the fixed effect. Standard errors are clustered at the firm and the worker level, and reported in parentheses. ***, **, * indicate statistical significance as the 1%, 5%, and 10% level, respectively.

	(1) Log(Wage)	(2) Log(Wage)	(3) Log(Wage)	(4) Log(Wage)
New Firm	-0.307***	-0.087***	-0.077***	0.007***
	(0.017)	(0.003)	(0.002)	(0.002)
Observations (millions)	48.4	48.4	48.4	48.4
R-squared	0.009	0.748	0.772	0.810
Time-Varying Worker Controls	No	No	Yes	Yes
Worker FE	No	Yes	Yes	Yes
Firm FE	No	No	No	Yes
Year FE	Yes	Yes	Yes	Yes

Table 3.4: New Firm Wages by Firm Age

Table reports baseline results of wages at firms of different age. The sample is a worker-year panel from 1990 through 2006. In all columns, the dependent variable is the log of worker total quarterly wages. Wages are in real 2014 dollars. Worker controls include worker age squared and age cubed, and their interactions with worker education. Time-varying worker controls include worker age squared, worker age cubed, worker age times education, worker age squared times education, worker age cubed times education. Worker age is log transformed. Education is measured in years of schooling and log transformed. Note, worker age and education are not included as linear controls in regressions with worker fixed effect since they are collinear with the fixed effect. Standard errors are clustered at the firm and the worker level, and reported in parentheses. ***, **, * indicate statistical significance as the 1%, 5%, and 10% level, respectively.

	(1) Log(Wage)	(2) Log(Wage)	(3) Log(Wage)	(4) Log(Wage)
Firm Age 0-1	-0.382***	-0.127***	-0.126***	0.063***
	(0.025)	(0.005)	(0.004)	(0.015)
Firm Age 2-3	-0.387***	-0.138***	-0.139***	0.041***
	(0.025)	(0.005)	(0.004)	(0.014)
Firm Age 4-5	-0.303***	-0.105***	-0.117***	0.038***
	(0.032)	(0.009)	(0.006)	(0.014)
Firm Age 6-7	-0.258***	-0.089***	-0.105***	0.031**
	(0.030)	(0.006)	(0.005)	(0.012)
Firm Age 8-9	-0.208***	-0.072***	-0.090***	0.028**
	(0.033)	(0.007)	(0.005)	(0.011)
Firm Age 10-11	-0.184***	-0.064***	-0.079***	0.027***
	(0.036)	(0.008)	(0.005)	(0.010)
Firm Age 12-13	-0.157***	-0.052***	-0.064***	0.028***
	(0.039)	(0.008)	(0.006)	(0.009)
Firm Age 14-15	-0.128***	-0.035***	-0.048***	0.025***
	(0.035)	(0.008)	(0.008)	(0.007)
Firm Age 16-17	-0.081***	-0.016**	-0.034***	0.018***
	(0.029)	(0.007)	(0.006)	(0.005)
Firm Age 18-19	-0.049*	-0.011***	-0.025***	0.009**
	(0.025)	(0.004)	(0.004)	(0.004)
Firm Age 20+	(omit)	(omit)	(omit)	(omit)
Observations (millions)	48.4	48.4	48.4	48.4
R-squared	0.018	0.748	0.772	0.810
Time-Varying Worker Controls	No	No	Yes	Yes
Worker FE	No	Yes	Yes	Yes
Firm FE	No	No	No	Yes
Year FE	Yes	Yes	Yes	Yes

Table 3.5: New Firm Wages for College Educated Workers

Table shows results from regressions of worker wages on new firm indicator variable for college educated workers from our baseline sample. The baseline sample is a worker-year panel from 1990 through 2006. In all columns, the dependent variable is the log of worker total quarterly wages. Wages are in real 2014 dollars. New firm is defined as a firm of three years of age or less. Time-varying worker controls include worker age squared, worker age cubed, worker age times education, worker age squared times education, worker age cubed times education. Worker age is log transformed. Education is measured in years of schooling and log transformed. Note, worker age and education are not included as linear controls in regressions with worker fixed effect since they are collinear with the fixed effect. Standard errors are clustered at the firm and the worker level, and reported in parentheses. ***, **, * indicate statistical significance as the 1%, 5%, and 10% level, respectively.

	(1) Log(Wage)	(2) Log(Wage)	(3) Log(Wage)	(4) Log(Wage)
New Firm	-0.275***	-0.092***	-0.088***	0.002
	(0.016)	(0.002)	(0.002)	(0.003)
Observations (millions)	18.3	18.3	18.3	18.3
R-squared	0.008	0.751	0.763	0.809
Time-Varying Worker Controls	No	No	Yes	Yes
Worker FE	No	Yes	Yes	Yes
Firm FE	No	No	No	Yes
Year FE	Yes	Yes	Yes	Yes

Table 3.6: New Firm Wages for College Educated Workers at Technology Firms

Table shows results from regressions of worker wages on new firm indicator variable for college educated workers in technology sector from our baseline sample. The baseline sample is a worker-year panel from 1990 through 2006. In all columns, the dependent variable is the log of worker total quarterly wages. Wages are in real 2014 dollars. New firm is defined as a firm of three years of age or less. Time-varying worker controls include worker age squared, worker age cubed, worker age times education, worker age squared times education, worker age cubed times education. Worker age is log transformed. Education is measured in years of schooling and log transformed. Note, worker age and education are not included as linear controls in regressions with worker fixed effect since they are collinear with the fixed effect. Standard errors are clustered at the firm and the worker level, and reported in parentheses. ***, **, * indicate statistical significance as the 1%, 5%, and 10% level, respectively.

	(1) Log(Wage)	(2) Log(Wage)	(3) Log(Wage)	(4) Log(Wage)
New Firm	-0.206***	-0.095***	-0.090***	0.005
	(0.027)	(0.004)	(0.004)	(0.005)
Observations (millions)	6.29	6.29	6.29	6.29
R-squared	0.014	0.724	0.739	0.804
Time-Varying Worker Controls	No	No	Yes	Yes
Worker FE	No	Yes	Yes	Yes
Firm FE	No	No	No	Yes
Year FE	Yes	Yes	Yes	Yes

Table 3.7: New Firm Wages After Controlling for Firm Size

Table reports baseline results of wages at new firms after controlling for firm size. Columns 1-4 use the baseline sample of workers which consists of a worker-year panel from 1990 through 2006. Column 5 uses the sub-sample of workers from the baseline sample who are college educated. Column 6 uses the sub-sample of workers from the baseline sample who are college educated and are in tech sector. In all columns, the dependent variable is the log of worker total quarterly wages. Wages are in real 2014 dollars. New firm is defined as a firm of three years of age or less. Time-varying worker controls include worker age squared, worker age cubed, worker age times education, worker age squared times education, worker age cubed times education. Worker age is log transformed. Education is measured as years of schooling and is log transformed. Firm employment is log transformed. Note, worker age and education are not included as linear controls in regressions with worker fixed effect since they are collinear with the fixed effect. Estimates marked "Included" are not reported due to the US Census disclosure limits on the number of estimates that can be cleared. Standard errors are clustered at the firm and the worker level, and reported in parentheses. ***, **, * indicate statistical significance as the 1%, 5%, and 10% level, respectively.

	All				College	College & Tech
	(1)	(2)	(3)	(4)	(5)	(6)
	Log(Wage)	Log(Wage)	Log(Wage)	Log(Wage)	Log(Wage)	Log(Wage)
New Firm	-0.131***	-0.014***	-0.008***	0.017***	0.015***	0.017***
	(0.007)	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)
Firm Employment	-0.010	0.104***	0.119***	0.087***	Included	Included
	(0.032)	(0.006)	(0.005)	(0.008)		
Firm Employment ^2	0.018***	-0.006***	-0.009***	-0.005**	Included	Included
	(0.006)	(0.001)	(0.001)	(0.002)		
Firm Employment ^3	-0.001***	-0.000	0.0002***	0.000	Included	Included
	(0.000)	(0.000)	(0.0001)	(0.000)		
Observations (millions)	48.4	48.4	48.4	48.4	18.3	6.29
R-squared	0.035	0.751	0.775	0.811	0.809	0.804
Time-Varying Worker Controls	No	No	Yes	Yes	Yes	Yes
Worker FE	No	Yes	Yes	Yes	Yes	Yes
Firm FE	No	No	No	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 3.8: Wages by Worker Tenure and Firm Age

Table reports results of wages by worker tenure at an employer and by the employer's age. The sample is a worker-year panel from 1990 through 2006. In all columns, the dependent variable is the log of worker total quarterly wages. Wages are in real 2014 dollars. Tenure 1 (2) (3) (4+) equals one for workers who were at the employer for one (two) (three) (four or more) years. New firm is defined as a firm of three years of age or less. Worker controls include worker age squared and age cubed, and their interactions with worker education. Worker age is log transformed. Education is measured in years of schooling and log transformed. Note, worker age and education are not included as linear controls in regressions with worker fixed effect since they are collinear with the fixed effect. Standard errors are clustered at the firm and the worker level, and reported in parentheses. ***, **, * indicate statistical significance as the 1%, 5%, and 10% level, respectively.

	(1) Log(Wage)	(2) Log(Wage)	(3) Log(Wage)	(4) Log(Wage)
Tenure 0	(omit)	(omit)	(omit)	(omit)
Tenure 1	0.165***	0.041***	0.010***	0.007***
	(0.005)	(0.002)	(0.002)	(0.002)
Tenure 2	0.370***	0.109***	0.063***	0.061***
	(0.007)	(0.003)	(0.002)	(0.002)
Tenure 3	0.477***	0.128***	0.082***	0.085***
	(0.009)	(0.003)	(0.003)	(0.003)
Tenure 4+	0.686***	0.117***	0.119***	0.133***
	(0.014)	(0.004)	(0.004)	(0.003)
Tenure 0 * New Firm	-0.086***	-0.050***	-0.047***	0.033***
	(0.014)	(0.003)	(0.003)	(0.002)
Tenure 1 * New Firm	-0.087***	-0.058***	-0.048***	0.027***
	(0.016)	(0.003)	(0.003)	(0.002)
Tenure 2 * New Firm	-0.071***	-0.059***	-0.043***	0.027***
	(0.016)	(0.003)	(0.002)	(0.002)
Tenure 3 * New Firm	-0.0263	-0.064***	-0.038***	0.015***
	(0.019)	(0.004)	(0.003)	(0.003)
Observations (millions)	48.4	48.4	48.4	48.4
R-squared	0.079	0.749	0.773	0.812
Time-Varying Worker Controls	No	No	Yes	Yes
Worker FE	No	Yes	Yes	Yes
Firm FE	No	No	No	Yes
Year FE	Yes	Yes	Yes	Yes

Table 3.9: New Firm Outcomes as a Function of Worker Fixed Effects from Wage Regressions

Table shows cross-sectional OLS results from predicting new firm exit (Columns 1-2) and future employment (Columns 3-4) as a function of worker fixed effects estimated from the wage regression in Table 3, column 4. The sample is a cross-section of new firms from the base sample. The base sample is a worker-year panel from 1990 through 2006. In Columns 1-2, dependent variable, New Firm Exits in 5 Years, equals one for new firms that exit by year five since founding. In Columns 3-4, dependent variable, New Firm 5-year Employment, is the log of a new firm's employment at age five. Mean Worker Fixed Effects is the mean of worker fixed effects of workers at the new firm in its first year of existence. Estimates for control variables (Log New Firm Employment in First Year, Log Mean Worker Education in First Year, and Log Mean Worker Age in First Year) are not reported due to the US Census disclosure limits on the number of estimates that can be cleared. State FE and Industry FE refer to the industry of the new firm. Standard errors are clustered at the firm level, and reported in parentheses. ***, **, * indicate statistical significance as the 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)	(4)
	New Firm Exits in 5 Years		New Firm 5-year Employment	
Mean Worker Fixed Effects	-0.077***	-0.065***	0.177***	0.180***
	(0.006)	(0.005)	(0.013)	(0.013)
Observations (thousands)	205	205	205	205
R-squared	0.035	0.036	0.099	0.1
Log New Firm Employment in First Year	Yes	Yes	Yes	Yes
Log Mean Worker Education in First Year	No	Yes	No	Yes
Log Mean Worker Age in First Year	No	Yes	No	Yes
Year of Firm Birth FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes

APPENDIX A

CHAPTER 1 APPENDIX

A.1 Variable Definitions

Average quarterly wage is the average quarterly wage in each firm-year-quarter. Wages are adjusted to 2001 dollars. *Source: LEHD*

Average quarterly wage of high-skill is the average quarterly wage of high-skilled workers in each firm-year-quarter. High-skilled workers are defined as workers whose earnings are above the 90th percentile of the firm wage distribution in that year-quarter. Wages are adjusted to 2001 dollars. *Source: LEHD*

Quarterly wage 90th/10th percentile ratio is the ratio of the average of quarterly wages above the 90th percentile to the average of quarterly wages below the 10th percentile of the quarterly wage distribution in that firm-year-quarter. Wages are adjusted to 2001 dollars. *Source: LEHD*

logWages is the logarithm of the average quarterly wage in each firm and year-quarter. Wages are adjusted to 2001 dollars. *Source: LEHD*

logMedWages is the logarithm of the median quarterly wage in each firm and year-quarter. Wages are adjusted to 2001 dollars. *Source: LEHD*

logWages_adj is the logarithm of the average quarterly wage in each firm and year-quarter. Wages are adjusted to 2001 dollars and adjusted for state-level cost of living. *Source: LEHD*

logWages_m (logWages_f) is the logarithm of the average quarterly wage of male (female) workers in each firm and year-quarter. Wages are adjusted to 2001 dollars. *Source: LEHD*

logWages_wk is the logarithm of a worker's wage in the first quarter of a given year. Wages are adjusted to 2001 dollars. *Source: LEHD*

logWages_cz is the logarithm of the average quarterly wage in each firm-commuting zone-year-quarter. Wages are adjusted to 2001 dollars. *Source: LEHD*

logWages_hskill is the logarithm of the average quarterly wage of high-skilled workers. High-skilled workers are defined as workers whose earnings are above the 90th percentile of the firm wage distribution in that year-quarter. Wages are adjusted to 2001 dollars. *Source: LEHD* *logWages_hskill_cz* is the logarithm of the average quarterly wage of high-skilled workers. High-skilled workers are defined as workers whose earnings are above the 90th percentile of the wage distribution in that firm-commuting zone-year-quarter. Wages are adjusted to 2001 dollars. *Source: LEHD*

logWages90th_10th is the log difference of the average quarterly wages above the 90th percentile and below the 10th percentile of the quarterly wage distribution in that firm-year-quarter. *Source: LEHD*

logWages90th_10th_m (logWages90th_10th_f) is the log difference of the average male wages above the 90th percentile and below the 10th percentile of the male wage distribution in that firm-year-quarter. *Source: LEHD*

logWages90th_10th_cz is the log difference of the average male wages above the 90th percentile and below the 10th percentile of the male wage distribution in that firm-commuting zone-year-quarter. *Source: LEHD*

logWages_lbd is the logarithm of the average per worker pay in each firm and year-quarter. Average per worker pay is calculated as the total payroll divided by the total employment as of March 12th in each firm-year. To create a quarterly panel, the annual measure in the year y is linked to the first three quarters of year y and the last quarter of year $y - 1$. Wages are adjusted to 2001 dollars. *Source: LBD*

Average working experience is the average of workers' working experiences in each firm-year-quarter, where working experience is defined as worker age minus year of education minus six. *Source: LEHD*
Average education level is the average of workers' education levels in a given firm-year-quarter. *Source: LEHD*

MaleShare is the share of male workers (in percentage) in each firm-year-quarter. *Source: LEHD*

CollegeShare is the share of workers (in percentage) who have at least 4-year college education in each firm-year-quarter. *Source: LEHD*

firm age is defined as the oldest establishment that the firm owns in the first year the firm is observed in the LBD (Haltiwanger, Jarmin, and Miranda, 2012). *Source: LBD*

lgFirmAge is the logarithm of firm age, where firm age is defined as the oldest establishment that the firm owns in the first year the firm is observed in the LBD (Haltiwanger, Jarmin, and Miranda, 2012). *Source: LBD*

Firm employment is the total number of workers in a given firm-year-quarter. *Source: LBD*

FIN is equal to 1 if a firm is classified as a finance firm, and equal to 0 if a firm is operating in other private non-farming industries. A firm is classified as a finance firm if 1) more than 50 percent of its employees working in establishments belonging to one of the finance industries, and 2) all establishments belonging to the firm are in finance industries. A firm is classified as a non-finance firm if 1) more than 50 percent of its employees working in establishments belonging to a private non-finance non-farming industry, and 2) none of its establishments is in finance industries. *Source: LBD*

HHI (2-digit SIC) is a measure of concentration for 2-digit SIC industry in a given year-quarter. It is the summation of the square of firm employment shares in the industry as defined by equation (1). *Source: LBD*

HHI (3-digit SIC) is a measure of concentration for 3-digit SIC industry in a given year-quarter. It is the summation of the square of firm employment shares in the industry as defined by equation (1). *Source: LBD*

LogFirmN is a measure of concentration for 2-digit SIC industry in a given year-quarter. It is the logarithm of total number of firms in a given industry-year. *Source: LBD*

$MarketPower^E$ *(2-digit SIC)* is a firm's employment share in its main industry in a given year-quarter where industries are defined using 2-digit SIC codes. textitSource: LBD

$MarketPower^E$ *(3-digit SIC)* is a firm's employment share in its main industry in a given year-quarter where industries are defined using 3-digit SIC codes. *Source: LBD*

$MarketPower^S$ *(2-digit SIC)* is a firm's sales share in its main industry in a given year-quarter where industries are defined using 2-digit SIC codes. *Source: BR*

$MarketPower^S$ *(3-digit SIC)* is a firm's sales share in its main industry in a given year-quarter where industries is defined using 3-digit SIC codes. *Source: BR*

$MarketPower^L$ is the firm's employment share in a given industry-commuting zone-year-quarter. *Source: LBD*

ROA is the ratio of earnings before tax, interest, depreciation, and amortization (EBTIDA) to total assets in a given firm-year-quarter. The is winsorized at the 1st and 99th percentiles of its empirical distribution. *Source: Compustat*

Lerner Index is the ratio of operating income after depreciation to total sales in a given firm-year-quarter. The is winsorized at the 1st and 99th percentiles of its empirical distribution. *Source: Compustat*

Asset utilization ratio is the ratio of total sales to total assets in a given firm-year-quarter. The is winsorized at the 1st and 99th percentiles of its empirical distribution. *Source: Compustat*

Manufacturing is equal to 1 if the firm is in SIC 20-39 and is equal to 0 for firms in other industries. *Source: LBD*

Mining is equal to 1 if the firm is in SIC 10-14 and is equal to 0 for firms in other industries. *Source: LBD*

Wholesale Trade is equal to 1 if the firm is in SIC 50-51 and is equal to 0 for firms in other industries. *Source: LBD*

Retail Trade is equal to 1 if the firm is in SIC 52-59 and is equal to 0 for firms in other industries. *Source: LBD*

A.2 Appendix Graphs and Tables

Figure A.1: Accessible States in LEHD

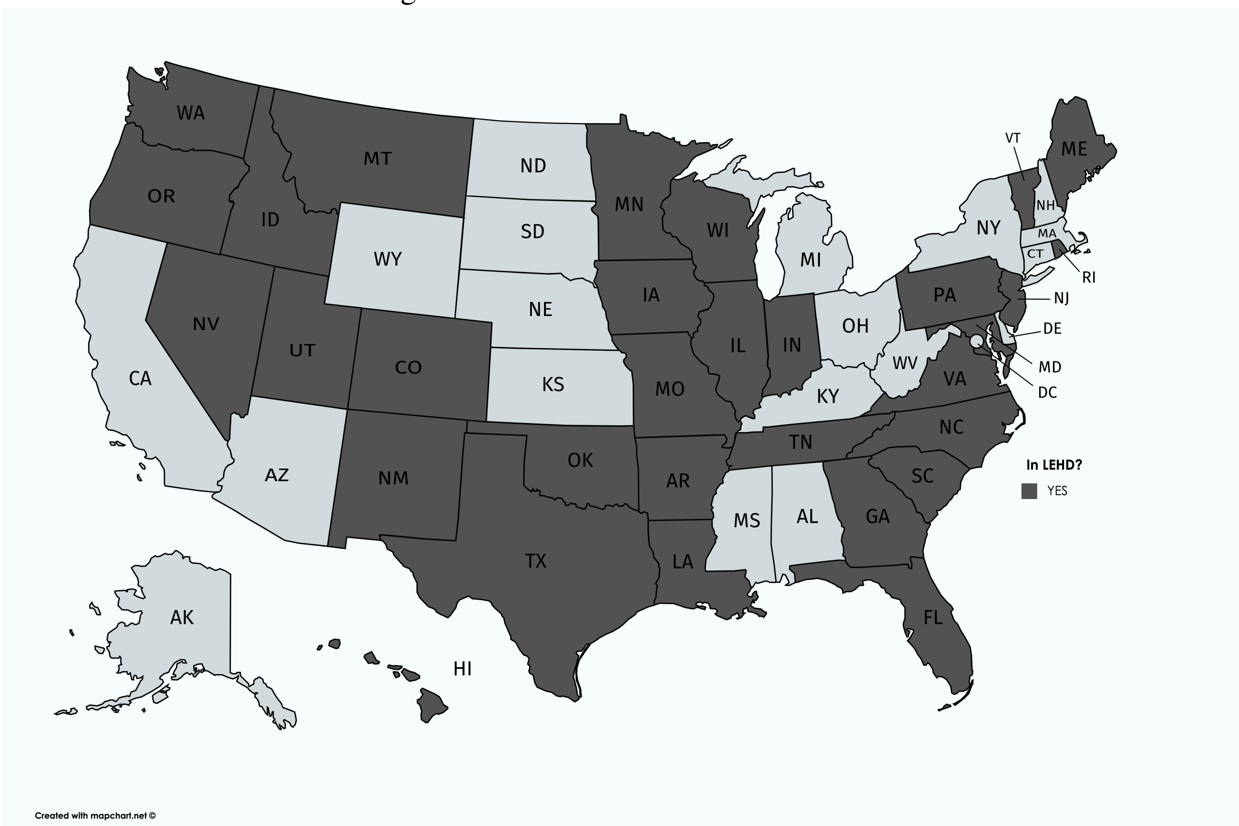


Table A.1: Summary Statistics: Public Firm Wage Pattern, Market Power and Other Characteristics

This table reports firm-level summary statistics. The sample consists of publicly listed firms only, and spans from Q2, 1990 through Q4, 2005. *All* refers to all observations in the sample. *Non-finance* refers to observations in finance industries. *Finance* refers to observations in non-finance industries. In columns (1) to (3) sample means (standard deviations) are computed across all-firm-quarter observations in each category. Column (4) provides differences between means in column (3) and column (2). Stars in the column (4) represent the level of p-values of testing the difference between columns 2 and 3: *** indicates p¡0.01, ** indicates p¡0.05, and * indicates p¡0.1. All definitions are provided in Appendix I. The number of observations is rounded following the Census Bureau's disclosure rules.

	(1) All	(2) Non-finance	(3) Finance	(4) Difference [(3)-(2)]
		Panel A: Wage Pattern		
Average quarterly wages ($)	12780	12840	12410	-423***
	(8,542)	(8,451)	(9,077)	
Average quarterly wages of high-skill($)	34450	33610	39600	5994***
	(35,460)	(34,510)	(40,450)	
Quarterly wage 90th/10th percentile ratio	7.358	6.953	9.848	2.895***
	(5.772)	(5.514)	(6.63)	
		Panel B: Firm Characteristics		
$MarketPower^E$ (2-digit SIC)	0.219	0.239	0.093	-0.146***
	(0.893)	(0.956)	(0.258)	
$MarketPower^E$ (3-digit SIC)	0.874	0.952	0.393	-0.558***
	(2.673)	(2.788)	(1.737)	
ROA	0.089	0.097	0.04	-0.058***
	(0.173)	(0.181)	(0.093)	
Lerner Index	0.039	0.005	0.253	0.248***
	(0.420)	(0.429)	(0.278)	
Asset utilization ratio	1.241	1.408	0.217	-1.191***
	(1.026)	(0.99)	(0.525)	
Average Worker Age	38.9	38.78	39.67	0.9***
	(4.393)	(4.522)	(3.397)	
lgAvgEdu	14.24	14.2	14.44	0.237***
	(0.784)	(0.804)	(0.604)	
CollegeShare	42.46	41.9	45.89	3.989***
	(14.88)	(15.25)	(11.82)	
MaleShare	56.4	60.65	30.32	-30.32***
	(22.77)	(20.88)	(15.37)	
Firm age	18.53	18.23	20.41	2.185***
	(6.572)	(6.513)	(6.617)	
Number of observations	91,000	78000	13000	

Table A.2: Firm Market Power and Wages in Finance (Public Firms Only)

This table presents the estimates of the effects of firm market power measured by employment on the wages of finance and non-finance firms. The sample consists of US public firms only, and spans from Q2, 1990 through Q4, 2005. The dependent variable is the log-transformed average quarterly wages at the firm. Wages are in 2001 constant dollars. Besides time fixed effects, all regressions control for the four-quarter-lag of firm-level measures of workforce composition, including the share of male workers, the log of average education level, the share of college workers, and the log of average worker experience. Standard errors are clustered at firm-level and reported in parentheses. *** indicates p¡0.01, ** indicates p¡0.05, and * indicates p¡0.1. All definitions are provided in Appendix I. The number of observations is rounded following the Census Bureau's disclosure rules.

	(1) logWages	(2) logWages	(3) logWages
FIN	0.0615***	0.0394***	0.0502***
	(0.0147)	(0.015)	(0.015)
$MarketPower^E$ (2-digit SIC)		-0.0182***	
		(0.0046)	
FIN$\times MarketPower^E$ (2-digit SIC)		0.193***	
		(0.0353)	
$MarketPower^E$ (3-digit SIC)			-0.0049***
			(0.0015)
FIN$\times MarketPower^E$ (3-digit SIC)			0.0219***
			(0.0081)
Number of observations	91,000	91,000	91,000
R-squared	0.513	0.515	0.514
Year$\times$Quarter FE	YES	YES	YES
Workforce composition	YES	YES	YES

Table A.3: Summary Statistics: Local Market Power and Local Wage Patterns

This table reports firm-commuting zone-level summary statistics. The sample consists of US public and private firms, and spans from Q2, 1990 through Q3, 2008. *All* refers to all observations in the sample. *Non-finance* refers to observations in finance industries. *Finance* refers to observations in non-finance industries. In columns (1) to (3) sample means (standard deviations) are computed across all-firm-commuting zone-quarter observations in each category. Column (4) provides differences between means in column (3) and column (2). Stars in the column (4) represent the level of p-values of testing the difference between columns 2 and 3: *** indicates p¡0.01, ** indicates p¡0.05, and * indicates p¡0.1. All definitions are provided in Appendix I. The number of observations is rounded following the Census Bureau's disclosure rules.

	(1) ALL	(2) Non-finance	(3) Finance	(4) Difference [(3)-(2)]
Average quarterly wages (CZ, $)	8839	8742	10930	2189***
	(8075)	(7788)	(12610)	
Average quarterly wages of high-skill(CZ, $)	17340	17080	23000	5929***
	(26340)	(25330)	(42180)	
Quarterly wage 90th/10th percentile ratio (CZ)	4.233	4.202	4.897	0.695***
	(5.683)	(5.629)	(6.726)	
LocalMarketPower	0.932	0.911	1.392	0.481***
	(4.810)	(4.790)	(5.193)	
Number of observations	69,270,000	66,210,000	3,061,000	

Table A.4: Offshorability

This table reports employment weighted average of offshorability by industries. Higher offshorability score means jobs in the industry are more offshorable.

SIC Code	Non-farming Private Industry	Offshorability
1000-1499	Mining	-0.481
2000-3999	Manufacturing	0.227
4000-4999	Transportation and Utilities	-0.771
5000-5199	Wholesale Trade	0.395
5200-5999	Retail Trade	-0.009
6000-6799	Finance and Insurance	0.976
7000-8999	Services	0.059

Table A.5: Summary Statistics: Firm Market Power Measured by Sales and Wage Patterns

This table reports firm-level summary statistics. The sample consists of US public and private firms, and spans from Q2, 1990 through Q3, 2008. *All* refers to all observations in the sample. *Non-finance* refers to observations in finance industries. *Finance* refers to observations in non-finance industries. In columns (1) to (3) sample means (standard deviations) are computed across all-firm-quarter observations in each category. Column (4) provides differences between means in column (3) and column (2). Stars in the column (4) represent the level of p-values of testing the difference between columns 2 and 3: *** indicates p¡0.01, ** indicates p¡0.05, and * indicates p¡0.1. All definitions are provided in Appendix I. The number of observations is rounded following the Census Bureau's disclosure rules.

	(1) ALL	(2) Non-finance	(3) Finance	(4) Difference [(3)-(2)]
Average quarterly wages ($)	9036	8948	11270	2326***
	(8,196)	(7,993)	(12,080)	
Average quarterly wages of high-skill($)	17920	17700	23630	5938***
	(26,060)	(25,260)	(41,150)	
Quarterly wage 90th/10th percentile ratio	4.399	4.375	5.012	0.638***
	(5.988)	(5.941)	(7.076)	
$MarketPower^S$ (2-digit SIC)	0.004	0.004	0.007	0.004***
	(0.041)	(0.041)	(0.023)	
$MarketPower^S$ (3-digit SIC)	0.023	0.022	0.03	0.008***
	(0.306)	(0.307)	(0.276)	
Number of observations	39,090,000	37,620,000	1,471,000	

Table A.6: External Validity

This table presents the estimates of the effects of firm market power measured by employment on the wages of finance and non-finance firms. The sample consists of US public and private firms, and spans from Q2, 1990 through Q3, 2008. The dependent variable is the logarithm of per worker wage in the firm. Per worker wage is calculated using total pay roll divided by total firm employment, adjusted inflation to 2001 constant dollars and winsorized at 1%. Wages are in 2001 constant dollars. Standard errors are clustered at firm-level and reported in parentheses. *** indicates p¡0.01, ** indicates p¡0.05, and * indicates p¡0.1. All definitions are provided in Appendix I. The number of observations is rounded following the Census Bureau's disclosure rules.

	(1)	(2)	(3)	(4)
	logWages_lbd	logWages_lbd	logWages_lbd	logWages_lbd
FIN	0.280***	0.316***	0.314***	0.315***
	(0.0018)	(0.0017)	(0.0018)	(0.0017)
$MarketPower^E$ (2-digit SIC)			0.104***	
			(0.0359)	
FIN$\times MarketPower^E$ (2-digit SIC)			0.461**	
			(0.182)	
$MarketPower^E$ (3-digit SIC)				0.039***
				(0.0049)
FIN$\times MarketPower^E$ (3-digit SIC)				0.0669***
				(0.0169)
Number of observations	64,790,000	64,790,000	64,790,000	64,790,000
R-squared	0.009	0.135	0.135	0.135
Year×Quarter FE	YES	YES	YES	YES
Workforce composition		YES	YES	YES

APPENDIX B

CHAPTER 2 APPENDIX

B.1 Variable Definitions

Establishment-level analysis

$M\&A_i$ is an indicator equal to one if the establishment belongs to a firm acquired in an M&A and zero otherwise.

$Post_t$ is an indicator equal to one in the year in our sample post the M&A and zero otherwise.

Routine employment share (RSH) measures the employment share of routine occupations in an establishment. It is defined as the logarithm of one plus the total employment of routine occupations in establishment i and year t divided by the total employment in the same establishment-year. We define occupations as routine following Autor and Dorn (2013) and merge their data to OES data by SOC codes. See routine occupation data at http://economics.mit.edu/faculty/dautor/data/autor-dorn-p.

High-skill workers labor share (Share high-skill) is the share of employment of managerial occupations in the establishment, as defined in the SOC titles.

Average hourly wage ($Wage$) is the logarithm of the average hourly wage in each establishment and year. OES data reports twelve hourly wage bins for each occupation and employment in each wage bin-occupation. We take the average of the lower and upper bounds of each wage bin to proxy for hourly wage of workers in that wage bin. Then we take employment-weighted mean of hourly wages of all workers in the establishment as a proxy of establishment-level hourly wages.

Standard deviation of hourly wages ($StdWages$) is the logarithm of the employment-weighted standard deviation of hourly wages in each establishment and year.

$Offshorability$ captures the degree to which the tasks performed by occupations in an establishment are offshorable. It is defined as the employment-weighted average of occupational offshorability, which is available by Autor and Dorn (2013) at the occupation level and merged to OES data using SOC occupation codes.

$Overlap\_Occup_i$ is an indicator equal to one if the target has share of overlapping employment with the acquirer greater than the sample median, 0 otherwise. For each target establishment, we first identify occupations in the target which are overlapping with occupations in the acquirer and we compute the share of overlapping employment in the target.

$Acq\_Low\_RSH_i$ is an indicator equal to one if the employee weighted average RSH for the acquirer is below the sample median, 0 otherwise. We identify all establishments associated with the acquirer in the same M&A deal observed in the year the M&A deal became effective or the two prior years. For each establishment, we measure RSH. Then we take the employee weighted average RSH over the three years.

$Private_i$ is an indicator equal to one if the target firm is private, 0 otherwise.

pseudo $M\&A_i$ is an indicator equal to one if the establishment belongs to a firm which was the target of a withdrawn deal. We only include deals which were withdrawn either because they were blocked by regulators or because the acquirer was acquired ex-post and had to withdraw the deal.

$Routine$ is an indicator equal to one if an occupation in the establishment is identified as a routine occupation, 0 if it is a non-routine occupation. We define occupations as routine following Autor and Dorn (2013) and merge their data to OES data by SOC codes. The routine occupation data are available at: http://economics.mit.edu/faculty/dautor/data/autor-dorn-p.

IT budget is the logarithm of one plus the modeled budget for IT in the establishment.

Hardware budget is the logarithm of one plus the modeled budget for hardware in the establishment.

Software budget is the logarithm of one plus the modeled budget for software in the establishment.

Services budget is the logarithm of one plus the modeled budget for services in the establishment.

Industry-level analysis

Merger intensity captures the intensity of M&A activity in an industry-decade. It is the logarithm of one plus the count of horizontal deals in a given (4-digit NAICS) industry-decade normalized by all horizontal deals in the decade.

Routine employment share (RSH) measures the employment share of routine occupations in an industry-year. It is defined as the logarithm of total employment of routine occupations in industry j and year t divided by the total employment in the same industry-year. We define occupations as routine following Autor and Dorn (2013).

High-skill workers labor share (Share high-skill) is defined as the employment share of high skill workers in each industry and year. Those are workers with graduate degrees (5+ years of post-secondary education).

Average hourly wage ($Wage$) is the logarithm of the average hourly wage in each industry and year. It is employment-weighted average of hourly wages of workers in that industry. Each worker's hourly wage is calculated as annual income and salary income divided by the product of weeks worked per year and hours worked per week. All wages are inflated to year 2001 following the instruction provided by IPUMs, https://cps.ipums.org/cps/cpi99.shtml.

Standard deviation of hourly wages ($StdWages$) is the logarithm of the employment-weighted standard deviation of hourly wages in each industry and year.

$Offshorability$ captures the degree to which the tasks performed by an industry are offshorable. It is defined as the employment-weighted average of occupational offshorability, which is available by Autor and Dorn (2013) at the occupation level and merged to IPUMs data using the available occupation crosswalks.

B.2 Appendix Tables

Table B.1: Robustness: Effects of M&A on establishment routine task intensity

This table repeats specifications in Table 2.2, except the dependent variable is now the logarithm of one plus routine task intensity. RTI characterizes the routine intensive occupations in each establishment. It is the occupation employment weighted average of occupation routine task indices, collected from Autor and Dorn (2013) and merged to OES data using available occupation crosswalks from David Dorn's website. $Post_t$ is estimated but not reported for brevity. Robust standard errors are clustered at the firm level. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	(1)	(2)	(3)	(4)	(5)
	RTI	RTI	RTI	RTI	RTI
$Post_t \cdot M\&A_i$	-0.1150***	-0.0893***	-0.0841***	-0.0921***	-0.0881***
	(0.0123)	(0.0117)	(0.0120)	(0.0123)	(0.0123)
$Offshorability$		0.268***	0.265***	0.268***	0.262***
		(0.0183)	(0.0189)	(0.0176)	(0.0185)
Establishment FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes			
Industry · Year FE			Yes		Yes
State · Year FE				Yes	Yes
Observations	10,408	10,408	10,332	10,390	10,314
R-squared	0.856	0.877	0.894	0.886	0.902

Table B.2: Robustness: Effects of M&A on establishment high-skill employment

This table repeats specifications in Table 2.3, except the dependent variable is now using an alternative definition of a high-skill occupation. Share high-skill is the employment share of high-skill workers in a given establishment-year. In Panel A, high-skill employment is employment in high-technology occupations as defined by Hecker (2005). In Panel B, high-skill employment is based on occupations that have a share of employees with some college education in the top quartile of the sample distribution. We obtain data on education by occupation from ACS 2000. In Panel C, we follow the same definition as in Panel B except high-skill employment is based on occupations that have a share of employees with some college education in the top tercile of the sample distribution. $Post_t$ is estimated but not reported for brevity. Robust standard errors are clustered at the firm level. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	Panel A				
	(1)	(2)	(3)	(4)	(5)
	Share high-skill	Share high-skill	Share high-skill	Share high-skill	Share high-skill
$Post_t \cdot M\&A_i$	0.0108***	0.0162***	0.0166***	0.0162***	0.0177***
	(0.0035)	(0.0035)	(0.0035)	(0.0038)	(0.0038)
$Offshorability$		0.0548***	0.0568***	0.0550***	0.0576***
		(0.0063)	(0.0066)	(0.0060)	(0.0062)
Establishment FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes			
Industry · Year FE			Yes		Yes
State · Year FE				Yes	Yes
Observations	10,444	10,444	10,370	10,426	10,352
R-squared	0.845	0.854	0.874	0.863	0.884

Panel B					
	(1)	(2)	(3)	(4)	(5)
	Share high-skill	Share high-skill	Share high-skill	Share high-skill	Share high-skill
$Post_t \cdot M\&A_i$	0.0133**	0.0177***	0.0160***	0.0183***	0.0176***
	(0.0053)	(0.0054)	(0.0046)	(0.0058)	(0.0051)
$Offshorability$		0.0448***	0.0452***	0.0448***	0.0464***
		(0.0072)	(0.0075)	(0.0070)	(0.0071)
Establishment FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes			
Industry · Year FE			Yes		Yes
State · Year FE				Yes	Yes
Observations	10,444	10,444	10,370	10,426	10,352
R-squared	0.832	0.837	0.859	0.847	0.869

Panel C					
	(1)	(2)	(3)	(4)	(5)
	Share high-skill	Share high-skill	Share high-skill	Share high-skill	Share high-skill
$Post_t \cdot M\&A_i$	0.0123*	0.0179***	0.0144**	0.0161**	0.0142**
	(0.0071)	(0.0069)	(0.0063)	(0.0069)	(0.0065)
$Offshorability$		0.0570***	0.0548***	0.0561***	0.0559***
		(0.0087)	(0.0090)	(0.0082)	(0.0084)
Establishment FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes			
Industry · Year FE			Yes		Yes
State · Year FE				Yes	Yes
Observations	10,444	10,444	10,370	10,426	10,352
R-squared	0.846	0.850	0.869	0.859	0.878

Table B.3: Robustness: Effects of M&A on establishment median wages

This table repeats specifications in Table 2.4, except the dependent variable is now median (instead of average) hourly wage. $Post_t$ is estimated but not reported for brevity. Robust standard errors are clustered at the firm level. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	(1)	(2)	(3)	(4)	(5)
	Wage	Wage	Wage	Wage	Wage
$Post_t \cdot M\&A_i$	0.0261**	0.0283**	0.0270**	0.0314***	0.0345***
	(0.0129)	(0.0129)	(0.0108)	(0.0120)	(0.0106)
$Offshorability$		0.0220*	0.0229*	0.0211*	0.0275**
		(0.0120)	(0.0124)	(0.0117)	(0.0120)
Establishment FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes			
Industry · Year FE			Yes		Yes
State · Year FE				Yes	Yes
Observations	10,444	10,444	10,370	10,426	10,352
R-squared	0.895	0.895	0.909	0.903	0.916

Table B.4: Robustness: Effects of M&A on establishment wage dispersion

This table repeats specifications in Table 2.5, except the dependent variable is now the log-transformed ratio of the 90th percentile of wages to the 10th percentile of wages at the establishment-level. $Post_t$ is estimated but not reported for brevity. Robust standard errors are clustered at the firm level. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	(1)	(2)	(3)	(4)	(5)
	Wages 90/10	Wages 90/10	Wages 90/10	Wages 90/10	Wages 90/10
$Post_t \cdot M\&A_i$	0.0353*	0.0343*	0.0310*	0.0182	0.0184
	(0.0203)	(0.0202)	(0.0167)	(0.0168)	(0.0151)
$Offshorability$		-0.0101	-0.0107	-0.0108	-0.0131
		(0.0156)	(0.0160)	(0.0158)	(0.0160)
Establishment FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes			
Industry · Year FE			Yes		Yes
State · Year FE				Yes	Yes
Observations	10,444	10,444	10,370	10,426	10,352
R-squared	0.753	0.753	0.780	0.770	0.796

Table B.5: Industries ranked by level of routine share intensity

Panel A of the table ranks the industries with the highest RSH by decade (in descending order). Panel B of the table ranks the industries with the lowest RSH by decade (in ascending order). 4-digit NAICS are included in parentheses.

1980	1990	2000	2010
Panel A. Industries with highest RSH			
legal services(5411)	legal services(5411)	legal services(5411)	legal services(5411)
veterinary services_miscellaneous personal services_beauty shops_barber shops(5419_8121_8129)	accounting, auditing, and bookkeeping services(5412)	accounting, auditing, and bookkeeping services(5412)	accounting, auditing, and bookkeeping services(5412)
newspaper publishing and printing_printing, publishing, and allied industries, except newspapers(5111_3231)	newspaper publishing and printing_printing, publishing, and allied industries, except newspapers(5111_3231)	grocery stores(4451)	drug stores(4461)
advertising (5418)	metalworking machinery(3335)	liquor stores(4453)	grocery stores(4451)
metalworking machinery (3335)	advertising(5418)	newspaper publishing and printing_printing, publishing, and allied industries, except newspapers(5111_3231)	metalworking machinery(3335)
Panel B. Industries with lowest RSH			
taxicab service (4853)	retail florists (4531)	retail florists(4531)	taxicab service (4853)
logging (1133)	logging (1133)	taxicab service (4853)	nonmetallic mining and quarrying, except fuels(2123)
metal mining (2122)	taxicab service (4853)	logging (1133)	metal mining(2122)
nonmetallic mining and quarrying, except fuels (2123)	metal mining (2122)	metal mining (2122)	shoe stores(4482)
vending machine operators (4542)	miscellaneous vehicle dealers (4412)	auto and home supply stores (4413)	retail florists (4531)

Table B.6: Robustness: Defining M&A intensity using transaction values

This table repeats specifications in Table 2.12, except $Merger\ Intensity_{j,(t-10,t-1)}$ is based on M&A transaction values (instead of M&A counts). Robust standard errors are clustered at the industry level. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	(1)	(2)	(3)	(4)
	RSH	Share high-skill	Wage	StdWages
$Merger\ Intensity_{j,(t-10,t-1)}$	-0.985	0.369	1.088	1.407
	(0.622)	(0.147)**	(0.497)**	(0.406)**
$Offshorability$	0.364	0.0127	-0.0217	0.0098
	(0.316)	(0.0223)	(0.0822)	(0.151)
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Observations	396	396	396	396
R-squared	0.956	0.963	0.959	0.885

Table B.7: Robustness: Defining M&A counts using first six years of each decade

This table repeats specifications in Table 2.12, except $Merger\ Intensity_{j,(t-10,t-4)}$ is based on M&A counts over the first six years of each decade. Robust standard errors are clustered at the industry level. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	(1) RSH	(2) Share high-skill	(3) Wage	(4) StdWages
$Merger\ Intensity_{j,(t-10,t-4)}$	-3.943	1.169	3.265	2.510
	(0.989)***	(0.249)***	(0.763)***	(1.115)**
$Offshorability$	0.370	0.0108	-0.0217	0.0041
	(0.310)	(0.0232)	(0.0824)	(0.153)
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Observations	396	396	396	396
R-squared	0.957	0.966	0.961	0.884

B.3 Industry Mapping Between IPUMs and SDC Data

IPUMs was created to facilitate time series analysis and, as such, has unique industry identifiers (IND1990), which offer consistent industry definitions over time. There are 224 unique industries defined in IND1990. IPUMs also provides a different definition of industry, INDNAICS, and a crosswalk between INDNAICS and 2007 NAICS. SDC includes information on the target and acquirer 2007 NAICS. To map IND1990 to 2007 NAICS, we take the following steps.

In the first step, we map the variable INDNAICS from ACS 2008-2014 samples to NAICS 2007 using a crosswalk provided by IPUMs.[1] About 4% percentage of the unique IND1990 industry classifications are not mapped to an INDNAICS. We drop these IND1990 classifications. We also standardize NAICS codes by limiting all NAICS to four digits. This crosswalk provides a one-to-one mapping between INDNAICS and IND1990.

In the second step, we map IND1990/INDNAICS to NAICS 2007. This step is more complicated as one IND1990/INDNAICS may match to more than one NAICS and one NAICS may match to more than one IND1990/INDNAICS. We start by saving all unique combinations of IND1990 and NAICS 2007 codes. To identify only the set of industries for which we can cleanly match between IND1990 and NAICS 2007 and avoid noise associated with ambiguous industry mapping, we consider only cases (after possibly aggregating IND1990 industries to one meta-industry) of industries (or meta-industries) that map to one and only one NAICS 2007, or aggregation of NAICS 2007 codes.

For example, IND1990 industry 0190 maps to NAICS 2213 and to NAICS 2212. NAICS 2213 and NAICS 2212 only map to IND1990 industry 0190. In this case, we combine NAICS 2213 and NAICS 2212 into one meta-industry and identify a clean link between IND1990 industry 0190 and NAICS industry 2213-2212. We follow an iterative approach to identify all possible such matches. Industries which cannot be assigned to a clean match are dropped.

Upon completion, we have a mapping from IND1990 to INDNAICS to NAICS 2007. It is

[1]The crosswalk is available at the following website: https://usa.ipums.org/usa/volii/indcross03.shtml

useful to think of the industry definitions in the paper as meta-industries as they may include more than one unique IND1990 and more than one unique 4-digit NAICS 2007. We have 132 unique meta-industries. Of the 224 unique industries in IND1990, we are able to successfully map 178 industries into our meta-industries or 79.5% of the unique IND1990 industries in IPUMs. Our mapping includes 209 unique 4-digit NAICS 2007.

APPENDIX C

CHAPTER 3 APPENDIX

C.1 Appendix Graphs and Tables

Figure C.1: Map of the US states available in the LEHD

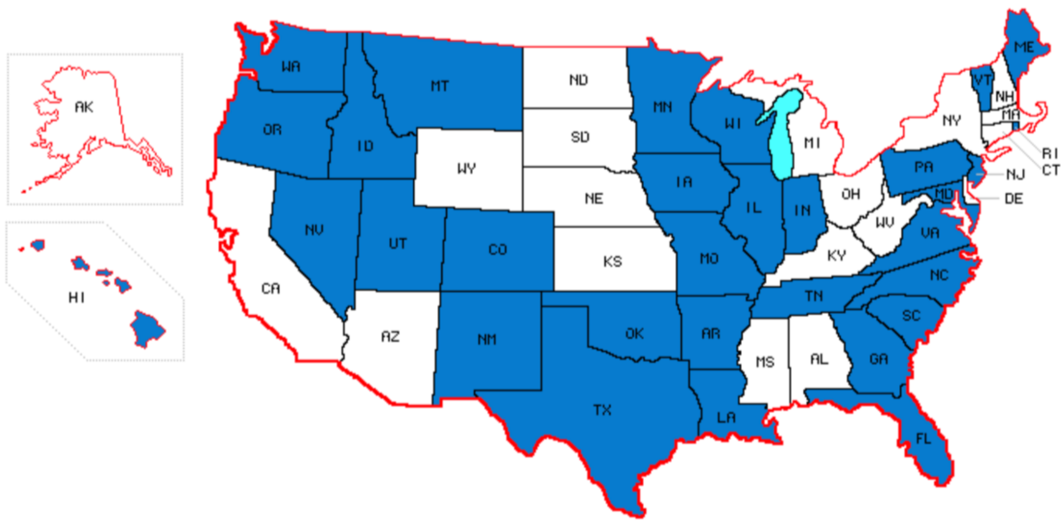


Figure C.2: Mean Adjusted Wages of Job Changers Classified by Quartile of Mean Wages of Coworkers at Origin and Destination Firm

Figure shows mean adjusted wages of workers from the baseline sample who change jobs (i.e., employers) in the year zero, and held the preceding job for two or more years (years -2 and -1), and the new job for two or more years (years 1 and 2). The baseline sample is a worker-year panel from 1990 through 2006. Each job is classified into quartiles based on mean wage of coworkers. Wages are log normalized to real 2014 dollars. Wages are adjusted by employee age squared and cubed and employee age*education, employee age squared*education and employee age cubed*education. Age, education and wages are log normalized.

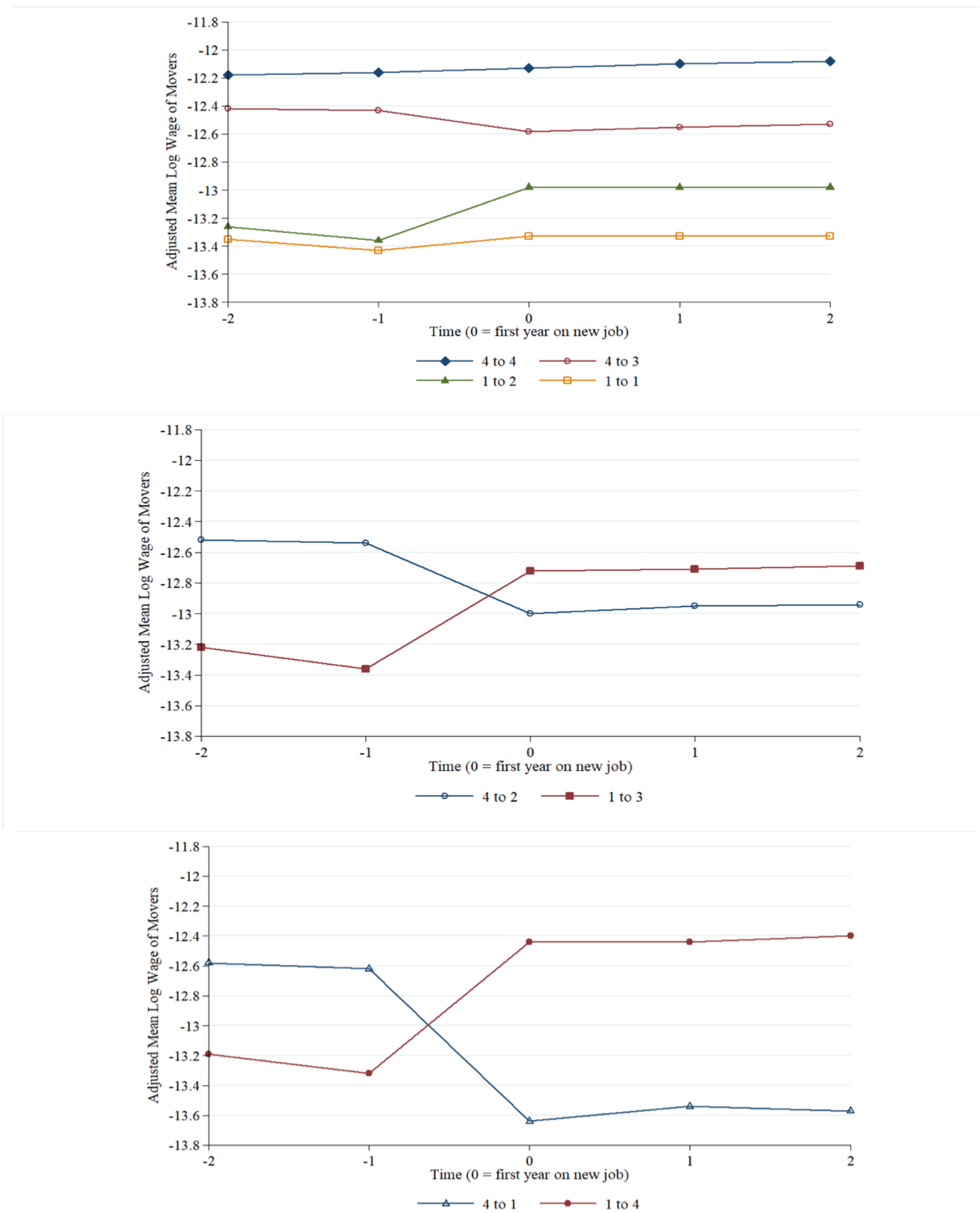


Table C.1: New Firm Wages: Define New Firm as Aged Zero or One

Table reports baseline results of wages at new firms, where new firm is defined as aged zero or one. The sample is a worker-year panel from 1990 through 2006. In all columns, the dependent variable is the log of worker total quarterly wages. Wages are in real 2014 dollars. New firm is defined as a firm of three years of one or less. Time-varying worker controls include worker age squared, worker age cubed, worker age times education, worker age squared times education, worker age cubed times education. Worker age is log transformed. Education is measured in years of schooling and log transformed. Note, worker age and education are not included as linear controls in regressions with worker fixed effect since they are collinear with the fixed effect. Standard errors are clustered at the firm and the worker level, and reported in parentheses. ***, **, * indicate statistical significance as the 1%, 5%, and 10% level, respectively.

	(1) Log(Wage)	(2) Log(Wage)	(3) Log(Wage)	(4) Log(Wage)
New Firm (Age 0-1)	-0.290***	-0.065***	-0.056***	0.021***
	(0.017)	(0.003)	(0.003)	(0.002)
Observations (millions)	48.4	48.4	48.4	48.4
R-squared	0.005	0.748	0.772	0.810
Time-Varying Worker Controls	No	No	Yes	Yes
Worker FE	No	Yes	Yes	Yes
Firm FE	No	No	No	Yes
Year FE	Yes	Yes	Yes	Yes

Table C.2: Robustness

Table reports results of robustness tests for the baseline sample. The baseline sample is a worker-year panel from 1990 through 2006. In all columns, the dependent variable is the log of worker total quarterly wages. Wages are in real 2014 dollars. New firm is defined as a firm of three years of age or less. Columns 1-2 show results for workers who have moved after the plant closure. Move to New Firm from Closed Plant (Open Plant) show results for workers who move to new firms from closed plants (open plant). Plant is defined as closed when the employment is zero either in the year of the move to a new employer or the year prior to the move. Columns 3-4 show results for workers who are at new firms during low unemployment (New Firm and Low Unemployment) and high state-level unemployment (New Firm and High Unemployment). High Unemployment is one for firms in states with unemployment rate above the national unemployment rate-year. Worker age is log transformed. Education is measured as years of schooling and is log transformed. Estimates marked "Included" are not reported due to the US Census disclosure limits on the number of estimates that can be cleared. Standard errors are clustered at the firm and the worker level, and reported in parentheses. ***, **, * indicate statistical significance as the 1%, 5%, and 10% level, respectively.

	Moves From Closed vs. Open Establishment		Moves During Low vs. High Unemployment	
	(1)	(2)	(3)	(4)
	Log(Wage)	Log(Wage)	Log(Wage)	Log(Wage)
Move to New Firm from Closed Establishment	-0.056***	0.026***		
	(0.002)	(0.003)		
Move to New Firm from Open Establishment	-0.076***	0.017***		
	(0.003)	(0.002)		
Worker Starts Employment in New Firm	-0.100***	-0.013***		
	(0.003)	(0.002)		
New Firm and Low Unemployment			-0.077***	0.007***
			(0.002)	(0.002)
New Firm and High Unemployment			Included	Included
Observations (millions)	48.4	48.4	48.4	48.4
R-squared	0.77	0.81	0.77	0.81
Time-Varying Worker Controls	Yes	Yes	Yes	Yes
Worker FE	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes
P-value from t-test	0.00	0.00	NA	NA

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