# The New Economic Segmentation: Work, Inequality, and Market Power

The New Economic Segmentation: Work, Inequality, and Market Power

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## Abstract

This dissertation considers how changes in organizations and product markets contributed to rising earnings inequality and wage stagnation since the 1970s.

The first chapter shows how the spread of unequal bargaining relations between corporate buyers and their suppliers has slowed wage growth for workers. Since the 1970s, market restructuring has shifted many workers into workplaces heavily reliant on sales to outside corporate buyers. These outside buyers wield substantial power over working conditions among their suppliers. During the same period, wage growth for middle-income workers stagnated. By extending organizational theories of wage-setting to incorporate interactions between organizations, I predict that wage stagnation resulted in part from production workers' heightened exposure to buyer power. Panel data on publicly traded companies shows that dependence on large buyers lowers suppliers' wages and accounts for 10 percent of wage stagnation in nonfinancial firms since the 1970s. These results are robust to a series of supplementary measures of buyer power; instrumental variable analysis using mergers between buyers; corrections for selection and missing data; and controls for individual worker characteristics like education and

occupation. The results show how product market restructuring and new forms of economic segmentation affect workers' wages.

The second chapter assesses the contribution of job reorganization to rising within-firm earnings inequality. During the period of rising U.S. earnings inequality, many employers revived management practices in which complex and routine tasks are divided between higher- and lower-paid jobs. This article theorizes this process as job distillation and distinguishes it from other sources of increasing organization-level earnings inequality. To test the earnings effects of job distillation, panel models are fit using linked employer-employee data on employees working for U.S. labor unions. These administrative data include a rare direct measure of task content, which is validated via a survey of union representatives. Variance function regression shows that job distillation increases inequality within organizations. This effect is driven by separating routine and complex tasks across jobs and by lowering earnings as jobs are simplified with respect to tasks. These findings demonstrate that classic concerns in the sociology of work should be brought back into the study of inequality. The distribution of earnings hinges on the allocation of tasks into jobs.

The third chapter traces changing patterns of worker mobility across jobs. Since the 1970s, changing employment relations seem to have eroded the role played by organizations in shaping worker mobility and earnings. As internal labor markets have declined, organizations no longer buffer workers from competitive labor markets. Yet at the same time, worker mobility between firms, through the external labor market, has declined. I compare the earnings and occupational attainment effects of job mobility within- and between-organizations. Due to declining worker mobility between-employers, within-organization mobility makes up an increasing share of overall job-to-job mobility. However, contrary to predictions made by theories of internal labor markets, within-firm transitions decreasingly consist of moves associated with earnings increases or upward occupational mobility. Part of this decline in the pay-off to internal moves stems from a shifting mobility age structure, in which a within-firm job changers are older and less likely to benefit from within-firm mobility. These findings suggest that movement between jobs within organizations remains important, but that these moves provide less advantage for workers than they did previously.

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# Introduction

The US wage structure has undergone substantial changes since the 1970s. The post-World War II economic boom generated rapid and relatively egalitarian earnings growth for workers. Since the 1970s however, the rate of earnings growth fell by half (Bernstein 2016). Since the 1980s, earnings increases that did occur were unequally distributed, with only higher educated workers and top managers and professionals experiencing rapid gains (Goldin and Katz 2008; Mouw and Kalleberg 2010).

For more than 30 years, explaining these changes in wage structure has been a focus of empirical labor economists (Lee 1999; Lemieux 2006; Juhn and Murphy 1993). Sociologists have contributed some key findings about these changes (Western and Rosenfeld 2011; Mouw and Kalleberg 2010). However, formulating a theory of the changing wage structure has not been a core focus of the discipline. In part, sociologists of stratification have been focused on other topics, like occupational status attainment and identifying labor market discrimination based on race and gender (Morris and Western 1999).

Yet sociology's recent silence on the wage structure also reflects prior failures. The 1970s saw the rise of a trio of ambitious sociological theories addressing issues of wages, employment and work: dual labor market theory, internal labor markets and labor process theory. These approaches sought to place workers and wage setting in a broader context of work organizations, product markets and institutions. Where human capital theory established a methodologically individualist approach to explaining wage determination, these sociological alternatives depicted work and the employment relationship as riven with stubborn institutional rules and governed by overarching dynamics of market power and class struggle.

But just as these theories shared a reasonable diagnosis about the limits of prior accounts of the wage structure, they suffered common shortcomings. Each theory, in its own way, overfit models to the post-war US employment system at the very moment that system was deteriorating. Dual labor market theory split the economy into non-competing groups of workers, hypothesizing that a monopolistic industrial core of employers paid production workers generously (Tolbert, Horan, and Beck 1980; Kalleberg, Wallace, and Althauser 1981). This, just as foreign competition in auto and steel and reinvigorated attacks on labor unions were driving wage cuts in the industrial core (Moody 1988; Stein 2011). Labor process theorists argued that the continuing division of labor was deskilling work (Braverman 1974), just before the swelling supply of college graduates was outpaced by the growth of the college wage premium (Goldin and Katz 2008). The organizational sociologists and institutional labor economists developing theories of the internal labor market took as their model lifetime careers in large corporations (Althauser 1989; Doeringer and Piore 1971), even as those careers were roiled by outsourcing, downsizing and declining employment tenure (Hollister and Smith 2014; Hirsch and De Soucey 2006). Given this track record, it is no surprise that sociologists have hesitated to offer new explanations for changes in the wage structure.

The apparent failure of these sociological theories left the study of the wage structure to theories focused on skill-biased technological change and on policy changes, like declining minimum wages (Acemoglu and Autor 2011; Lee 1999). These studies provide a dominant view of the wage structure as ultimately given by skill supply and demand, but then modified by discrete policy changes.

However, recent research suggests limits to this austere view of the determinants of the wage structure. The college wage premium and the demand for cognitive skill have not risen since at least 2000 (Beaudry, Green, and Sand 2016), while inequality has continued increasing. Recent analysis of the minimum wage finds only small effects on reducing earnings inequality (Autor, Manning, and Smith 2016).

Moreover, a series of recent findings suggest that the broader organizational, institutional and product market context of labor markets could play an important role in wage determination. Increased earnings inequality since the 1980s has been primarily between-firms and between-workplaces, rather than among co-workers (Barth et al. 2016; Song et al. 2018). This rising between-firm inequality has come in part from outsourcing and other processes that sort more similar workers into the same workplaces (Handwerker 2018) and in part from higher paid workers matching with higher paying firms (Song et al. 2018). Organizational change has contributed to inequality (Caroli and Reenan 2001), while dwindling labor unions have exposed workers to market forces (Western and Rosenfeld 2011). Beyond organizations, broader changes in market structure could matter too. Recent research suggests a role for monopsony power in lowering wages in concentrated labor markets (Azar, Marinescu, and Steinbaum 2017; Benmelech, Bergman, and Kim 2018). On product markets, in my own previous research I found that the structure of consumer demand contributes to wage inequality (Wilmers 2017), and other research links financialization to earnings inequality (Lin and Tomaskovic-Devey 2013; Philippon and Reshef 2012).

Taken together, these patterns suggest value in returning to sociological theories of wage structure—theories that ask about the role of organizations, product markets and group conflict in shaping the wage structure. In this dissertation, I investigate recent changes in the wage structure and employment relations. In doing so, I excavate ideas from labor process theory (chapter 2), internal labor markets (chapter 3) and dual labor market or economic segmentation theory (chapter 1). I find insights in each of these theories that remain relevant for understanding wage setting. The dual labor market prediction that product market power can affect the availability of economic rents does not imply that the division between heavy and nondurable manufacturing is the key axis of inequality. The idea that the allocation of tasks across jobs affects earnings inequality does not presuppose a unidirectional process of deskilling. The diverse ways in which workers move through jobs within organizations can remain important for affecting workers' wage growth, notwithstanding declining worker job tenure.

In chapter 1, I investigate the product market context in which employers impose wage pressure on workers. I find that employers in industries that used to provide relatively high-paying jobs for workers without a college degree, like manufacturing, warehousing and transportation, have become increasingly reliant on sales to large corporate buyers. These buyers pressure their suppliers to lower their prices and wages. This effective pressure undermines the organizational pay premiums that workers in these sectors would otherwise enjoy. Increased buyer power explains around 10% of wage stagnation since the 1970s.

In chapter 2, I zoom in on within-organization processes to consider the effects of job reorganization on earnings inequality. To do so, I use data on the tasks and jobs of employees of labor unions. I find that when tasks are more divided across high- and low-paying positions, within-organization earnings inequality increases. This increased inequality results both from less mixing of higher and lower paid tasks across jobs and from lower earnings stemming from job homogeneity.

Finally, in chapter 3, I consider the effects of workers' earnings growth of the changing role played by organizations in worker mobility. Since at least the early 1990s, there has been a decline in the rate between-employer job switching among workers. Job mobility inside organizations has held steady. As such, a growing proportion of job mobility happens inside firms. I find that within-firm job mobility is decreasingly associated with earnings increases and occupational upgrading.

In the conclusion, I consider theoretical implications of the research as a whole, along with some future directions for empirical research on the wage structure.

# 1. Wage Stagnation and Buyer Power: How Buyer-Supplier Relations Affect U.S. Workers' Wages, 1978 to 2014

## Introduction

During the 1970s, U.S. workers' wages ended three decades of steady increase and have since stagnated. In the same period, market restructuring, lax anti-trust enforcement, and supply chain innovation left many supplier companies dependent on sales to large corporate buyers. Suppliers in the manufacturing, wholesale and transportation industries, former bastions of middle-income employment, were particularly exposed to rising buyer power: in 2014, the average publicly traded manufacturing firm received more than 25% of its revenue from large customers, up from 10% in the early 1980s. Case study research suggests that some buyer-supplier relations foster high-road employment practices (Whitford 2006:155), but that recent buyer ascendance left large buyers holding "the whip hand over manufacturers, while workers found their remuneration and working conditions subject to competitive pressures that often debased their status" (Lichtenstein 2012:8).

In this article, I develop the argument that rising buyer power has undermined workers' wages. More and more middle-skill workers, or workers without a college degree, work at intermediate employers that are substantially reliant for on a small number of dominant corporate buyers. To understand the wage effects of this mediated employment relationship, I extend organizational theories of wage setting beyond interactions between workers and their direct employers to incorporate interactions between workers, employers and outside buyers. Dominant buyers can use their power, unlike dispersed buyers, to reap concentrated benefits of labor cost cutting and, unlike direct managers, to renege on implicit pay norms that otherwise buoy wages for their suppliers' workers. I predict that since the 1970s, the rising share of employment relations structured by dominant buyers has eroded organizational bases of wage premiums for middle-skill workers and buyer power has contributed to wage stagnation.

I test this argument by modeling the effect of buyer power on suppliers' workers' wages. Company-level panel models estimate the wage effects of increases in the share of sales to dominant buyers; of selling to one compared to multiple dominant buyers; of increases in the duration of contracting relations between suppliers and buyers; and of mergers among buyers. Results are robust to alternative model specifications that address concerns about missing data and changing job composition. I draw on these estimates to quantify the contribution of rising buyer power to U.S. wage stagnation since the 1970s.

This study extends organizational theories of wage setting to include interactions between companies. By using research on organizational sources of pay premiums to rework an older theory of between-firm economic segmentation, I bring the analysis of imperfectly competitive product markets and market power back to the sociology of wage determination. These theoretical moves are crucial for studying stratification in an era of rising buyer power and workplace fissuring (Weil 2014). Understanding wage stagnation requires looking beyond both competitive labor market forces and changes inside organizations, to consider also shifts in product market structure. Instead of a return to the vertically integrated, monopolistic corporations of Fordist production, or the rise of an egalitarian, networked economy, middle-skill workers are increasingly subject to hierarchical markets—markets mediated by direct employers but ultimately governed by dominant buyers. These buyers undermine the organizational bases of workers' wage premiums.

## Wage Stagnation and Market Structure

From 1947 to 1979, earnings for the bottom 90% of U.S. workers increased on average by 2% each year. Since 1979, despite rapid growth in the late 1990s, annual earnings growth stagnated to 0.5% (Bernstein 2016). Part of this stagnation is due to a decline in productivity growth among workers without a college degree, or middle-skill workers: when employers automate routine work, middle-skill workers' wages suffer (Autor and Dorn 2013) and when manufacturing workers face import competition, the price of goods they produce falls (Autor, Dorn, and Hanson 2013).

Yet even as the market value of non-college workers' skills declined, organizational sources of pay premiums for these workers also dried up. Employers who seemed to overpay middle-earning workers (relative to workers' average fixed ability) have dwindled (Song et al. 2018). The decline in organizational pay premiums for non-college workers points to changes in employment relations irreducible to supply and demand in the competitive labor market. Explanations for this source of wage stagnation have focused on redistribution of resources within firms: the rise of shareholder value orientation undermined implicit contracts and wage norms among workers (Bidwell et al. 2013; Fligstein and Shin 2007); and the decline of collective bargaining made it harder for workers to capture a share of their employers' profits (Mishel 2015; Wilmers 2017).

These extant explanations for declining pay premiums focus on redistribution of economic resources *within* firms—from middle-skill workers to managers, shareholders and professionals—but wage stagnation since the 1970s has been defined by variation *between* firms. The growing gap between median and high earners is between high- and median-paying firms, rather than among coworkers at the same firm (Song et al. 2018) and the declining labor share is due to reallocation toward very profitable firms, rather than redistribution from workers to owners in the same firm over time (Autor et al. 2017).

Explaining the between-firm dynamics of declining pay premiums requires placing organizational wage setting in a broader context of strategic interaction and resource flows between firms. The classic statement of this approach comes from economic segmentation research: imperfect product market competition lets workers at protected firms receive a portion of their pay from rents, or extra profits their employer gains from reducing supply and raising prices above competitive rates (Dencker and Fang 2016; Tolbert, Horan, and Beck 1980; Kalleberg, Wallace, and Althauser 1981). More recent strategy and exchange theory research has characterized these market power-reliant companies as *value capture* firms (in contrast to *value creation* firms) (Reagans and Zuckerman 2008). Yet since at least the early 1980s, rising market concentration is not associated with rising firm-level pay (Autor et al. 2017) and the firm-size earnings premium has been steadily eroding for non-college workers (Cobb and Lin 2017). There seems to be less value around to capture and pay to middle-skill workers.

I argue that this decline in pay premiums can be traced to changes in market structure during this period. Specifically, more and more employers of middle-skill workers were brought into the ambit of dominant buyers. These buyers arose in part through reduced antitrust enforcement and resultant corporate mergers since the 1970s (Fligstein 1990:222-225): from 1981 to 2012 average industry concentration across all economic sectors increased (Autor et al. 2017). Yet, in the midst of declining antitrust enforcement, mergers between competitors did not expand corporate hierarchies back into supply chains. Technological change reduced coordination costs across companies and investors pushed businesses to focus on their core competencies (Davis, Diekmann, and Tinsley 1994; Zorn et al. 2004) and vertical integration declined steadily from the mid-1990s (Davis and Cobb 2010; Frésard, Hoberg, and Phillips 2014).[1] Instead industries with substantial corporate purchasing power ensconced keystone buyers to purchase myriad inputs from independent suppliers. Large retailers wrested control of supply chains and pitted factory against factory by commoditizing manufactured products (Lichtenstein 2010). Resource extraction companies contracted out less productive mines and split oil production from refining and distribution (Weil 2014). Manufacturers subcontracted production (Whitford and Zeitlin 2004), often shirking legacy union contracts and growing the wage gap between suppliers and final assembly (Sturgeon and Florida 2003). Together, these changes in antitrust enforcement, organizational norms and contracting costs left upstream producers—in middle-skill intensive industries like manufacturing, transportation, warehousing and primary resource extraction—increasingly reliant on dominant buyers.

## Buyer Power and Wage Determination

What are the wage effects of these increasingly prevalent employment relationships, defined by worker exposure to dominant outside buyers? Recent research on buyer power finds that large buyers can force reductions in profits among their dependent suppliers. Suppliers with concentrated buyers have lower returns on sales and assets (Kim 2016; Gosman and Kohlbeck 2009). Survey evidence on manufacturing suppliers finds that even when buyer-supplier relationships improve suppliers' operational productivity, buyers appropriate the resulting financial gains (Kim and Wemmerlöv 2015; Gulati and Sytch 2007). This research focuses on

[1] Prior to the 1990s, the trend in vertical integration is less clear. Initial analysis of mergers in the 1980s found that "virtually all avoided vertical integration" (Davis, Diekmann, and Tinsley 1994). Subsequent research showed vertical mergers were increasing in the 1980s and 1990s, albeit not at the same rate as horizontal mergers (Zorn et al. 2004; Fan and Goyal 2006). These analyses use industry-level measures of supply integration and focus only on mergers, not vertical integration overall. Recent research uses the text of corporate annual reports to measure vertical integration directly and finds a consistent decline since at least the mid-1990s (Frésard, Hoberg, and Phillips 2014).

profits rather than wages, but suggests that buyer pressure can undermine economic rents otherwise available to suppliers' workers.[2] For instance, Massey Coal Company contracts with "an abundance of small operators" to extract from its worst mines (Weil 2014:104). Massey purchases the coal mined by these suppliers at low fixed prices and pushes down suppliers' workers' wages (Weil 2014:101-107). Even some service industries face this pressure: automotive repair shops receive 75% of their revenue from insurance companies, which in turn push to lower mechanics' wages (Sacchetto 2009).

This asymmetrical dependency relation can undermine suppliers' wages and their profits indiscriminately. However, organizational theories of pay premiums suggest that buyer pressure can also specifically undermine workers' advantages, in ways accepted or welcomed by suppliers' owners and shareholders: dominant buyers can circumvent (1) within-firm wage norms (faced by managers) and (2) collective action problems in cost cutting (faced by dispersed buyers or shareholders).

First, outside buyers enjoy substantial social distance from their suppliers' workers. Dominant buyers are able to reduce labor costs when wage norms and the social proximity of workers to managers would otherwise elevate wages (Cobb and Stevens 2016; Weil 2014). As a result, when companies outsource janitorial or security workers, outsourced workers face slower wage growth than remaining, directly-employed workers (Goldschmidt and Schmieder 2017; Dube and Kaplan 2010). More broadly, buyers' social distance from suppliers' workers

[2] This prior research offers several definitions of buyer power. Monopsony consists of a single buyer, or a collusive group of buyers in oligopsony, that reduce demand to force suppliers to lower prices below a competitive rate. Transaction costs accounts define buyer power in terms of the hold-up problem and bargaining between buyer and supplier over a relationship-specific surplus. Finally, a weaker definition, relied on in this paper, treats buyer power as the capacity of a buyer to force lower prices from a supplier. These lower prices can be reduced below the market rate (as in classic monopsony); exacted out of an unequal distribution of relationship-specific surplus (as in transaction cost accounts); or arise from lowering inflated prices to a market rate. As the third definition encompasses the other two, and is sufficient to predict buyer effects on supplier wages, I focus on it throughout the analysis.

facilitates tough negotiating tactics on labor costs. Annual "five percent letters" from large buyers to their suppliers demanding immediate price reductions are "in widespread use across manufacturing" (Whitford 2006:86-88). The wage advantages for workers due to implicit wage norms are thus vulnerable to pressure from outside buyers. This vulnerability becomes apparent as a contracting relationship lasts longer and buyers impose additional real wage restraint: social distance keeps buyer-driven wage pressure sharp, notwithstanding longer term contracting.

Second, dominant buyers reap concentrated benefits from supplier cost cutting. In contrast, dispersed buyers face coordination problems and have insufficient incentive to gather and use production-cost information to push for labor cost reductions from suppliers (Kelly and Gosman 2000; Burt 1980): non-pressuring buyers can free-ride on price and cost reductions achieved by activist buyers. When a supplier receives the bulk of its revenue from a single dominant buyer, the supplier will be subject to cost cutting pressure more effective than would be mustered by multiple independent buyers. For example, when clothing manufacturer Farris Fashions began selling exclusively to Walmart, Walmart's "pressure for lower production costs never slackened" and the factory jobs became "poorly paid" (Lichtenstein 2012:18). When the benefits of cost pressure are concentrated, an outside buyer is more likely to push for lower wage costs at its supplier.

These dynamics of dependence, social distance and concentrated benefits show how several facets of buyer power can be expected to undermine organizational sources of wage premiums. But they presuppose a crucial scope condition: economic rents must be available for workers to demand and for suppliers and buyers to contest. In competitive product markets, suppliers' workers receive market-determined wages, and supplier price increases lead buyers to switch to a cheaper supplier, so organizational pay premiums are scarce. But when prices are imperfectly disciplined by competition with other suppliers, buyer pressure can become determinative. Studies of mergers among buyers find that suppliers in more concentrated industries experience larger declines in financial performance (Fee and Thomas 2004) and prices (Bhattacharyya and Nain 2011) following buyer mergers, relative to suppliers in less concentrated industries. Extending these findings to wages suggests that the wage effects of buyer power should be more negative when suppliers would otherwise receive economic rents. While the portion of wages due to rent is difficult to measure directly (Card, Devicienti, and Maida 2013), I expect a higher share of rents in companies with increased market share and profitability. The presence of this economic rent and value capture strategy is a scope condition for negative wage effects from buyer power.

In contrast, in some cases, large corporate buyers bring gains to suppliers (Patatoukas 2012; Chang, Hall, and Paz 2015), perhaps via supplier learning (Gulati and Sytch 2007; Uzzi 1997; Sabel 1994). Under what conditions will pay gains from improved performance outweigh suppliers' losses of economic rents? Research on supply chains finds that suppliers with distinctive capabilities oriented toward value creation can thrive even when contracting with large buyers (Gereffi, Humphrey, and Sturgeon 2005). Value creation can shield suppliers from the negative effects of buyer power and offers another test of the value capture scope condition. Following previous research (Mizik and Jacobson 2003), I proxy for value creation using the Research & Development (R&D) spending of each supplier.

## Data

**Table 1**. Descriptive Statistics on Firm-level Data

| | Obs. | Mean | Standard Deviation | Min. | Max. |
|---|---|---|---|---|---|
| log(Wages) | 33885 | 3.81 | 0.60 | 0.54 | 6.67 |
| Share of Revenue from Dominant Buyers | 33885 | 0.04 | 0.13 | 0.00 | 1.00 |
| Number of Buyers | 33885 | 0.25 | 0.61 | 0.00 | 2.00 |
| Years of Continuous Contracting | 33885 | 0.46 | 1.45 | 0.00 | 7.00 |
| Market Share | 33885 | 0.02 | 0.06 | 0.00 | 1.00 |
| Profit Margin | 33885 | 0.25 | 0.15 | 0.00 | 2.05 |
| R&D Intensity | 33885 | 0.00 | 0.02 | 0.00 | 0.80 |
| log(Revenue) | 33885 | 5.61 | 2.17 | -2.18 | 12.28 |
| log(Employees) | 33885 | 0.31 | 2.14 | -6.91 | 6.78 |
| log(Assets) | 33885 | 6.69 | 2.22 | -2.43 | 14.76 |
| log(Property Plant and Equipment) | 33885 | 4.29 | 2.68 | -6.76 | 12.38 |
| Share Financial Investments | 33885 | 0.27 | 0.20 | 0.00 | 1.00 |
| Share Dividends & Stock Buy-Backs | 33885 | 0.09 | 0.16 | 0.00 | 1.83 |
| Revenue Share from Foreign Sources | 33885 | 0.03 | 0.14 | -0.37 | 1.18 |
| Union density (industry) | 33885 | 0.11 | 0.16 | 0.00 | 1.00 |
| Buyer Merger | 33885 | 0.03 | 0.16 | 0.00 | 1.00 |
| Cancelled Buyer Merger | 33885 | 0.00 | 0.04 | 0.00 | 1.00 |

*Note*: Observations are firm-year pairs. All firm-years not reporting salary information are excluded from the sample.
*Source*: Compustat, Thompson-Reuters, CPS-ORG and May CPS.

I test this theory of buyer power over supplier wages by observing the effects of changing buyer-supplier relations on supplier pay. This research design requires firm-level longitudinal data with measures of supplier wages and reliance on buyers, along with controls that isolate the effect of buyer dependence from other determinants of wages. In this section I introduce the main data and variables used in the analysis before discussing the model in more detail in the next section. Table 1 presents descriptive statistics.

Administrative data used to study firm-level wages includes no information on buyer-supplier relations (Barth et al. 2014; Song et al. 2018). As such, I focus on publicly traded corporations, which are required to disclose sales to buyers that amount to at least 10% of their annual revenue. Compustat collects Securities and Exchange Commission (SEC) filings, generating the only data on buyer-supplier relations across multiple industries in the U.S.

economy. Publicly traded U.S. companies account for 37% and 30% of total U.S. employment in 1978 and 2014, respectively. These firms tend to be large and well-capitalized, and are relatively concentrated in finance and manufacturing industries. In robustness tests below I assess whether the results found for publicly traded firms are biased by this skewed industry composition.

The main outcome variable, firm-level wages, is derived from labor costs, which companies report as a supplementary item on income statements. Labor costs are defined broadly as staff expenses that include salaries, profit sharing and incentive compensation, payroll taxes and employee benefits (Compustat 2015).[3] A firm-level measure of average annual labor earnings per employee is calculated by dividing labor costs by the number of employees at a firm.[4] I refer to these deflated and logged average annual labor earnings estimates as firm-level wages, to avoid confusion with corporate earnings and profitability.[5] Not all firms report labor costs: firms that report do so because they consider the information material to shareholders. The share of reporters declined in the 1980s and 1990s, from 18% of total U.S. workers to 6% in 2014. In robustness tests, I consider the sensitivity of results to selection of firms into reporting labor cost information.

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[3] Employers do not distinguish between domestic and foreign employment, so changes in payroll and employment could reflect shifts between domestic and lower wage foreign employment. Only around 5% of publicly traded U.S. companies' employment is non-domestic (Lin 2016), so these composition effects are unlikely to drive results in the analysis. However, in a robustness check I control for whether a company experienced a Trade Adjustment Assistance (TAA) claim in a year. TAA qualifies workers for benefits if their job is offshored or outsourced due to foreign competition. Firm-level TAA certification data is available from the Department of Labor from 1997 to 2014. Controlling for offshoring via TAA certification does not change the wage effect of reliance on dominant buyers.

[4] An alternative model specification predicts logged total firm payroll conditional on firm-level employment, instead of predicting the ratio of payroll to employment. Results are robust to this alternative dependent variable, but I focus in the text on the payroll per employees ratio as it is more easily interpretable as average firm-level wages.

[5] I winsorize wages at the top and bottom 1% of the firm-level wage distribution to exclude unreliable extreme observations. The main results are robust to alternatively including wage outliers or dropping them entirely.

The main independent variable is the share of firm revenue from large private sector buyers, each of which amounts to at least 10% of a firm's total revenue. This reporting requirement has been in place since 1978, which defines the period for the main analysis. Throughout the analysis I refer to disclosed revenue as revenue from dominant buyers. The revenue share from dominant buyers serves as a firm-specific measure of exposure to buyer power. When suppliers make more of their sales to large buyers, they increase their revenue reliance on such buyers.

In addition to this main predictor, I consider two supplementary measures of buyer reliance. First, the duration of contracting relationship is measured as the running number of consecutive years that a supplier reports a given dominant buyer, averaged across buyers for each supplier. I expect longer contracting to facilitate buyers' intrusion into suppliers' wage setting practices, while the social distance of outside buyers from suppliers' workers frustrates any countervailing development of wage norms and commitments. Second, I construct an indicator variable for firms reporting zero, one or multiple dominant buyers. Suppliers reliant on a single dominant buyer are predicted to face more cost reduction pressure than suppliers with multiple buyers as revenue reliance increases, due to the concentrated benefits of cost savings captured by a single buyer.

I also construct three moderating variables to test the scope condition that firms relying on value capture rather than value creation strategies should be most vulnerable to buyer pressure. Supplier market power is measured as a firm's share of total revenue for each year and industry measured at the NAICS 3-digit level, following Grullon et. al. (2015). I supplement this measure with a direct measure of profit margins, as the ratio of Earnings Before Interest, Tax, Depreciation and Amortization (EBITDA) to revenue. With firm and year fixed effects (discussed further below), and conditional on total assets and fixed capital, increased profit margins suggest available surplus. Increases in either of these measures thus tend to increase the portion of suppliers' workers' workers wages that is comprised of rents. High portions of rents in wages makes workers more vulnerable to price pressures imposed by dominant buyers. Finally, I proxy for distinctive capabilities and value creation with the share of research and development spending out of total revenue.

Next, I control for other determinants of firm-level wages that could be associated with changes in buyer reliance. I condition on direct shareholder returns (dividends and share buy-backs over total operating expenses) and financial assets as a share of total assets, both constructed as in Lin (2016). These variables measure the rewards and leverage of firm owners against workers, respectively.[6] For worker power, I control for industry-level unionization, again at the NAICS 3-digit level, calculated using the Current Population Survey Outgoing Rotation Group (CPS ORG) (from 1984 to 2014) and the CPS May survey (1979-1981 and 1983), following Western and Rosenfeld (2011).

To control for changes in the value of worker skill, I use measures of technological change, globalization and productivity. Technological change is measured with logged assets, logged fixed capital and R&D spending intensity. Globalization is measured with the share of firm revenue from foreign sources. Productivity is measured as logged revenue conditional on logged number of employees (Barth et al. 2014). If revenue increases conditional on the number

[6] The type and structure of ownership could also influence worker-owner bargaining. Unfortunately, owner data, the Thompson-Reuters 13F institutional investors dataset, is only available from 1981, so I exclude these controls from the main models. In a robustness test, I add controls for ownership structure, using measures developed in previous research (Jung 2016; Cobb 2015): block ownership (an indicator for whether an investment manager holds at least 5% of a firm's equity); large owner share (the percentage of a firm's equity held by the largest 10% of all investment managers, by equity assets under management); and the portion of shares owned by quasi-indexers, dedicated owners and transient owners. All of these variables are calculated at the firm-year level using the Thompson-Reuters 13F institutional investors data. The latter three variables merge data drawn from Bushee's (1998) factor analysis of ownership styles. Adding these ownership controls does not affect the main results.

of employees and the amount of assets and fixed capital, this increase reflects an increase in output conditional on production inputs. This control is limited in several ways: it does not control for changes in non-labor inputs (like raw materials); it tracks revenue per worker, not product output per se; and it includes any compositional changes in worker skill. But, it provides a rough measure of labor productivity, which could affect both reliance on dominant buyers and workers' wages. In a robustness test below I condition instead on workers' education and occupation composition.

## Model

These data allow analysis of suppliers' firm-level wages as a function of dependence on dominant buyers. The full equation predicts logged wages $w_{it}$ at supplier firm *i* in year *t*:

$$\log(w_{it}) = \beta_1 x_{it} + \boldsymbol{\beta_2 c'_{it}} + \boldsymbol{\beta_3}(x_{it} * \boldsymbol{c'_{it}}) + \beta_4 s_{it} + \boldsymbol{\beta_5 v'_{it}} + \alpha_{1t} + \alpha_{2i} + e_{it},$$

where $\beta_1$ is the effect of an increasing share of suppliers' revenue coming from dominant buyers ($x_{it}$). As reliance on dominant buyers pushes down wages among workers at the supplier firm, $\beta_1$ will be negative. $\boldsymbol{c'_{it}}$ is a vector of mediators that includes the number of dominant buyers. To test the scope conditions discussed above, $x_{it}$ is interacted with the remaining variables in vector $\boldsymbol{c'_{it}}$ , market share, profit margins, and R&D intensity. The duration of contracting is measured as a series of dummy variables ($s_{it}$) indicating the number of years of consecutive contracting with the same dominant buyer for firm *i* as of year *t*.

Year fixed effects ($\alpha_{1t}$) remove common time-variant wage changes due to macro-economic changes like recessions and heightened unemployment (Bernstein 2016). Firm fixed effects ($\alpha_{2i}$) remove time-invariant characteristics of firms that could be related to both dominant buyer dependence and wage rates: firms using a particular production process or shaped by a stable firm culture might be more likely to have lower wages and be more reliant on dominant buyers.[7] $\boldsymbol{v}'_{it}$ is the vector of time variant control variables, discussed above, that could be associated with both wage changes and increasing reliance on dominant buyers. This model assumes that no common unobserved supplier business decisions drive both reliance on dominant buyers and changes to workers' wages. In a robustness test below, I test this assumption by examining the effect of buyer mergers, as an event exogenous to supplier business strategy.

## Trends in Buyer Power

[7] Prior research shows that a negative association between supplier profits and buyer reliance is sensitive to selection effects (Mottner and Smith 2009).

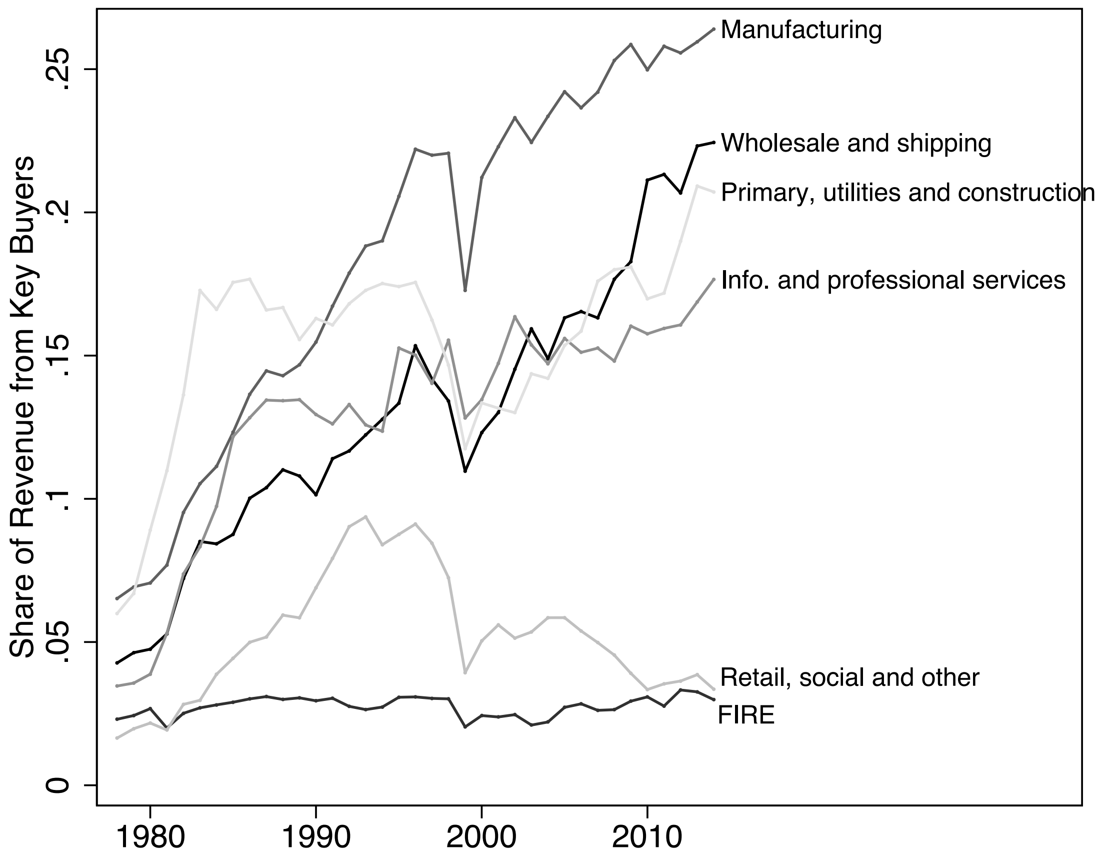


**Figure 1**. Reliance on Dominant Buyers among Publicly Traded Firms, by Sector, 1978-2014.
*Note*: Industry categories are aggregated from NAICS 2-digit codes as follows: Manufacturing (33, 31, 33); Wholesale and shipping (42, 48); Primary, utilities and construction (11, 21, 22, 23); Info. and professional services (51, 54, 56); Retail, social and other (44, 61, 62, 71, 72, 81, 99); FIRE (52, 53).
*Source*: Compustat.

Figure 1 breaks out changing dominant buyer reliance across broad industry groups. Finance, Insurance and Real Estate (FIRE) and Retail, Social and Other Services have a low share of companies dependent on dominant buyers, as these industries serve individual consumers along with a range of businesses. Manufacturing, wholesale and shipping have a high and rising reliance on dominant buyers.[8] Primary resource extraction, utilities and construction

[8] All trend lines show a large dip in 1999. Correspondence with Standard & Poors indicates that this dip is an artifact of data collection: "Our data collection team confirmed that the reason for gaps in the customer segment data in 1999 for some companies is that we changed to a different internal application

have experienced more fitful increases. In other descriptive analyses, not displayed here, I find that reliance in manufacturing, wholesale and shipping is growing at both intensive and extensive margins, with more suppliers becoming more reliant on dominant buyers. The upward trend persists within each decadal cohort of new public firms, suggesting it is not driven by cohort-specific changes in the selection of firms into public markets. The trend also persists in a sub-sample including only the S&P 500 companies, suggesting that it is not an artifact of improving coverage of smaller firms in the data. The trend in these industries also persists when companies are weighted by employment level. Industries employing many middle-skill workers are increasingly reliant on dominant buyers.

These supplier industry trends in revenue reliance do not reveal the type of buyer suppliers increasingly depend on. Supplier companies often disclose the names of their dominant buyers. These 185,000 buyer names are drawn by Compustat directly from supplier companies' annual reports. While it is not feasible to code all of these names, I identify the largest customers for descriptive purposes. I first clean the text by removing special characters and terms common in company names. I then manually code the data based on the largest counts of the first word in the text name. For first words that accurately describe a single company, I assign a unique identifying number, which I apply to any subsequent instance of the same company. For first words that describe multiple companies, I separate each company based on the full text entry. By following this procedure, I identify almost half of reports for the most common customer names from 1984 and 2014. Some companies with very different name variants could be undercounted in this process. Next, I identify the 100 buyers with the largest number of suppliers. I then categorize these buyers into groups by industry and purchasing strategy.

at that time and it caused a one-year gap in some data." Capital IQ Client Support, e-mail message to author, May 26, 2016.

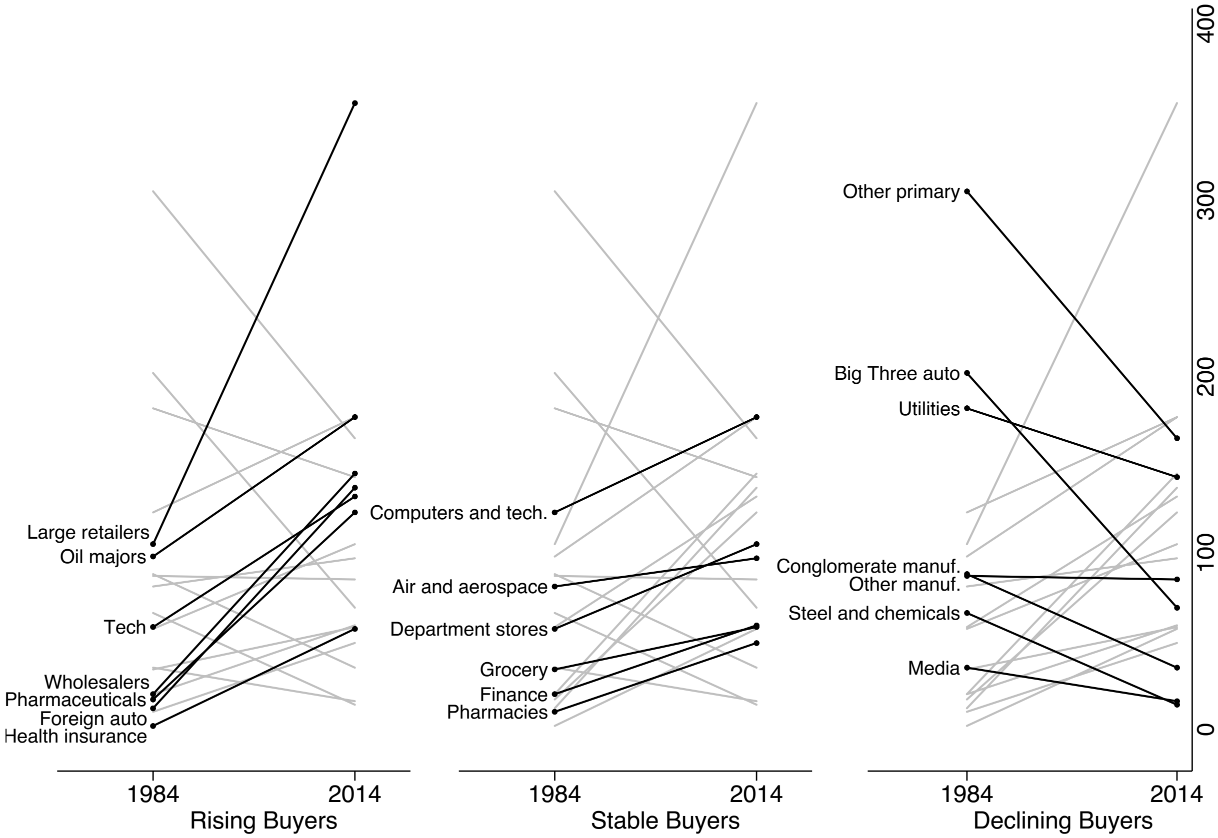


**Figure 2**: Changing Composition of Dominant Buyers in the U.S. Economy.
*Note*: Points indicate the total number of publicly traded companies reliant for at least 10% of their sales on companies in each buyer category for 1984 and 2014. Dominant buyer categories are drawn each year from the 100 most frequently reported buyers.
*Source*: Compustat.

Figure 2 summarizes changes in the composition of dominant buyers. These shifts reflect changes in the industry composition of the U.S. economy: the declining importance of steel, chemical and American automakers; the rise of oil and gas production with the discovery of new extraction methods. However, they provide a clearer image than shifting industry categories of the types of large buyers that direct substantial groups of suppliers. Intermediary entities like wholesalers, finance and health insurance, subject to substantial recent consolidation, play an increasingly prominent purchasing role, quantitatively rivaling large manufacturing firms by 2014. Most striking is the precipitous rise of large retailers, which more than tripled their number of U.S. suppliers, from 105 in 1984 to 355 in 2014. Over the same period, manufacturers like domestic auto, steel and chemicals, which once towered over largely domestic supply chains, declined in importance. Rising manufacturers like foreign auto companies and air and aerospace only partially counterbalance this trend. Research- and intellectual property-oriented producers, like pharmaceuticals, electronics and computer and technology companies have also increased their numbers of suppliers, perhaps reflecting outsourcing of lower value-added production. Overall, retailers, wholesalers and other intermediaries increased their number of large suppliers by 541 over this period, while manufacturers of all kinds had a net increase of 135.

These figures include only the largest corporate buyers and are reported by publicly traded suppliers, so they exclude the shifting dynamics of small suppliers. Nonetheless, the trends indicate a reshuffling of the buyers commanding U.S. suppliers. During the period of wage stagnation, more and firms became more dependent on dominant buyers, and the composition of these dominant buyers shifted toward large retailers and other intermediaries. Next, I test the firm-level wage effects of this restructuring of buyer-supplier relations.

## Main Results

Table 2 presents the main results of the analysis. The first models test whether increasing revenue reliance on dominant buyers is associated with decreasing wages among suppliers. Model 1 shows that conditional on firm and year fixed effects, a 10 percentage point increase in reliance on dominant buyers is associated with a 1.2% decrease in supplier wages. Model 2 incorporates the controls discussed above for worker and owner bargaining power, technological change, globalization and production changes. The negative association between increasing buyer reliance and wages is robust to controlling for these determinants of firm-level wages, with little change in the point estimate. Together, these models indicate that increasing revenue reliance on dominant buyers lowers suppliers' wages.

Model 3 assesses whether reliance on a single dominant buyer will lower wages more than reliance on multiple buyers, by interacting an indicator for number of buyers with revenue reliance. A single dominant buyer should concentrate the benefits of cost savings and therefore have more incentive to push down labor costs. At low levels of dominant buyer revenue reliance, there is little wage difference between selling to one dominant buyer or multiple buyers: both are associated with a 4% or 5% wage penalty. However, the negative wage effect of increasing revenue reliance on dominant buyers is twice as large when companies rely on a single dominant buyer than when they make sales to multiple large buyers. A 10% increase in revenue reliance is associated with a 1.1% wage decrease for suppliers reliant a single buyer and a 0.5% wage decrease for suppliers reporting multiple buyers. The difference between these coefficients is not statistically significant. Nonetheless, the point estimates suggest a substantively different effect of supplier reliance on one or multiple buyers: suppliers dependent on a single dominant buyer appear to face more effective downward real wage pressure than suppliers transacting with multiple large buyers.

Next, wages should continually decline as contracting relations last longer, insofar as buyers are buffered from suppliers' workers' appeals to wage norms. Models 4 and 5 add a series of indicators for the average number of years of consecutive contracting between a supplier and its buyers. Figure 3 plots the wages predicted at each of these years and shows that as firms rely on dominant buyers for longer periods, wages decline. From the first year of contracting to more than six years, holding productivity, profit margins and fixed capital at their means, wages

**Table 2**. Firm-level Wage Effects of Reliance on Dominant Buyers

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Share of Revenue from Dominant Buyers | -0.12*** | -0.13*** | | | |
| | (0.02) | (0.01) | | | |
| log(Revenue) | | 0.42*** | 0.42*** | 0.42*** | 0.42*** |
| | | (0.00) | (0.00) | (0.00) | (0.00) |
| log(Employees) | | -0.49*** | -0.49*** | -0.49*** | -0.49*** |
| | | (0.00) | (0.00) | (0.00) | (0.00) |
| log(Property Plant and Equipment) | | 0.06*** | 0.06*** | 0.06*** | 0.06*** |
| | | (0.00) | (0.00) | (0.00) | (0.00) |
| Profit Margin | | -0.10*** | -0.10*** | -0.11*** | -0.10*** |
| | | (0.01) | (0.01) | (0.01) | (0.01) |
| log(Assets) | | 0.01** | 0.01** | 0.01** | 0.01** |
| | | (0.00) | (0.00) | (0.00) | (0.00) |
| R&D Intensity | | 0.07 | 0.08 | 0.08 | -0.06 |
| | | (0.15) | (0.15) | (0.15) | (0.17) |
| Market Share | | -0.11** | -0.11** | -0.11** | -0.09* |
| | | (0.04) | (0.04) | (0.04) | (0.04) |
| Union density (industry) | | 0.11*** | 0.11*** | 0.11*** | 0.11*** |
| | | (0.02) | (0.02) | (0.02) | (0.02) |
| Share Financial Investments | | -0.05*** | -0.05*** | -0.05*** | -0.05*** |
| | | (0.01) | (0.01) | (0.01) | (0.01) |
| Share Dividends & Stock Buy-Backs | | 0.05*** | 0.05*** | 0.05*** | 0.05*** |
| | | (0.01) | (0.01) | (0.01) | (0.01) |
| Revenue Share from Foreign Sources | | -0.03*** | -0.03*** | -0.03*** | -0.03*** |
| | | (0.01) | (0.01) | (0.01) | (0.01) |
| Single Dominant Buyer * Share Rev. from Dom. Buyers | | | -0.11*** | -0.12*** | -0.03 |
| | | | (0.03) | (0.03) | (0.03) |
| Multiple Dominant Buyers * Share Rev. from Dom. Buyers | | | -0.05** | -0.06** | 0.03 |
| | | | (0.02) | (0.02) | (0.02) |
| Single Dominant Buyer | | | -0.04*** | -0.02** | -0.02** |
| | | | (0.01) | (0.01) | (0.01) |
| Multiple Dominant Buyers | | | -0.05*** | -0.03*** | -0.03*** |
| | | | (0.01) | (0.01) | (0.01) |
| Years of Cont. Contract w/Dom. Buyers: 1 | | | | -0.01 | -0.01 |
| | | | | (0.01) | (0.01) |
| 2 | | | | -0.01 | -0.00 |
| | | | | (0.01) | (0.01) |
| 3 | | | | -0.01 | -0.01 |
| | | | | (0.01) | (0.01) |
| 4 | | | | -0.03*** | -0.03*** |
| | | | | (0.01) | (0.01) |
| 5 | | | | -0.05*** | -0.05*** |
| | | | | (0.01) | (0.01) |
| 6 | | | | -0.04*** | -0.05*** |
| | | | | (0.01) | (0.01) |
| >6 | | | | -0.07*** | -0.07*** |
| | | | | (0.01) | (0.01) |
| Share of Revenue from Buyers * Market Share | | | | | -0.63*** |
| | | | | | (0.17) |
| Share of Revenue from Dominant Buyers * Profit | | | | | -0.40*** |

| | | | | | |
|---|---|---|---|---|---|
| Margin | | | | | (0.07) |
| Share of Revenue from Dominant Buyers * R&D | | | | | 1.11* |
| Intensity | | | | | (0.49) |
| Firm fixed effects | Yes | Yes | Yes | Yes | Yes |
| Year fixed effects | Yes | Yes | Yes | Yes | Yes |
| R2 | 0.91 | 0.94 | 0.94 | 0.94 | 0.94 |
| Within-R2 | 0.00 | 0.37 | 0.37 | 0.37 | 0.37 |
| Observations | 33424 | 33424 | 33424 | 33424 | 33424 |

*Note*: Standard errors are in parentheses. Observations are firm-year pairs. Total observations deviates from descriptive statistics due to 461 singleton firm-years dropped from fixed effects estimation.
*Source*: Compustat.
* $p < .05$; ** $p < .01$; *** $p <$

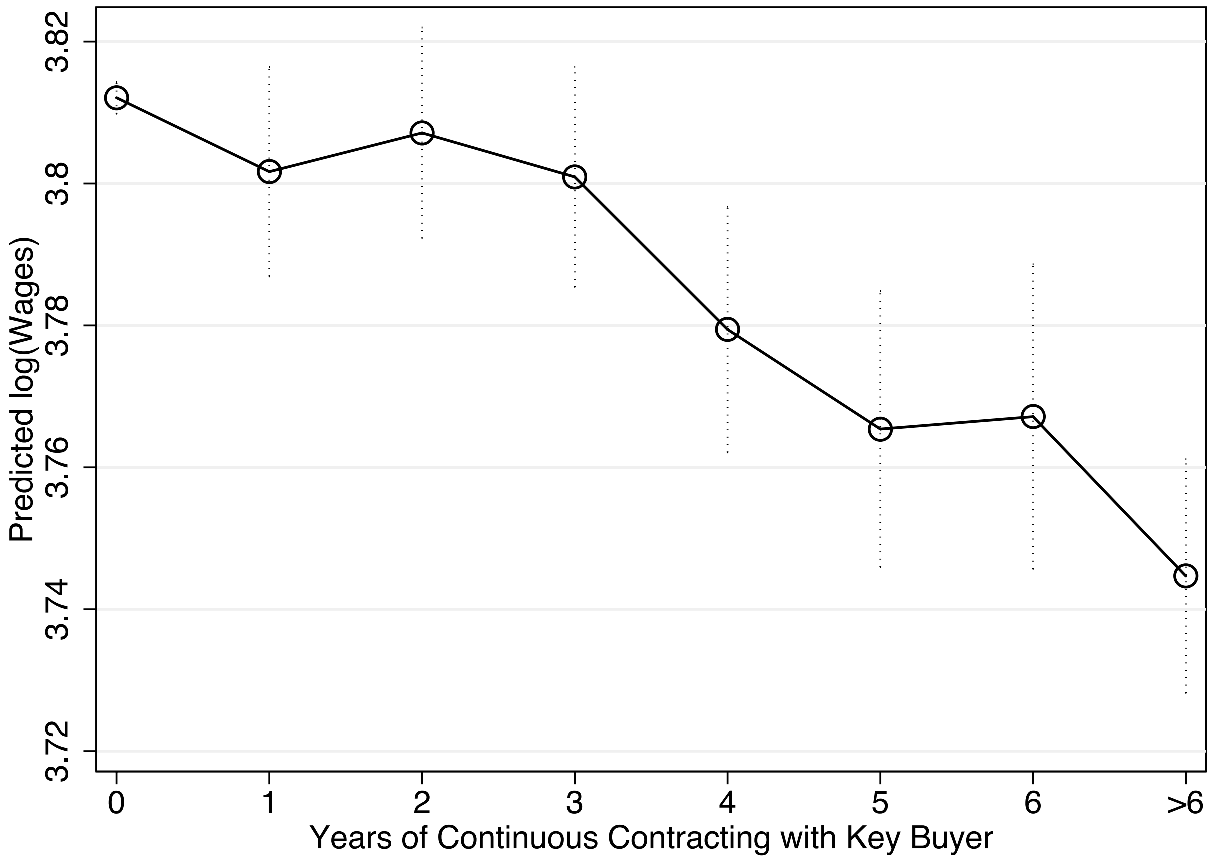


**Figure 3**. Mean Predicted Wages per Year of Continuous Relationship with Dominant Buyer.
*Note*: Predicted values are drawn from Model 5 in Table 2. The series calculates a predicted wage value by year of continuous contracting, holding other modeled variables at their means.
*Source*: Compustat.

decline 7%. This decline is consistent with social distance between buyers and suppliers' workers letting buyers maintain pressure on labor costs even in the presence of repeated contracting.

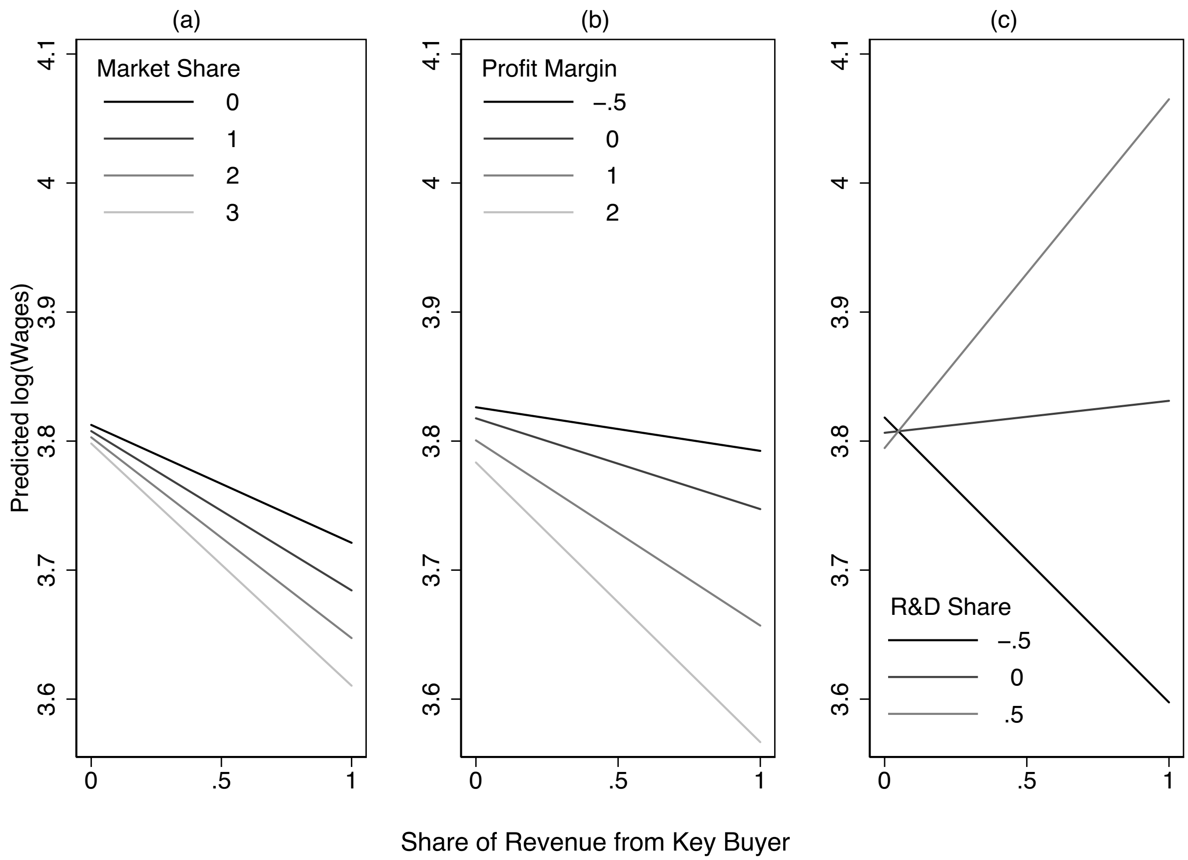


**Figure 4**. Effect of Reliance on Dominant Buyers on Wages, by Market Share and Profit Margin.
*Note*: Predicted wages were calculated using coefficients from Model 5 in Table 2, holding all other variables at their means. Slopes are drawn at number of standard deviations from the mean for each moderating variable, as indicated in the legends.
*Source*: Compustat.

To test the scope conditions of buyer power effects, Model 5 includes a series of interaction coefficients. Workers at suppliers with a higher portion of wages coming from shared rents risk more from buyer pressure. Model 5 shows negative interaction effects between dominant buyer revenue reliance and both supplier market share and supplier profit margins. Figure 4 charts predicted wages based on these models, with all other variables held at their means. Panel (a) of Figure 4 compares predicted wages across different market shares for different degrees of dominant buyer reliance. A firm with very little market share shifting from no buyer revenue reliance to 25% reliance faces 0.5% wage reductions, while one controlling around 15% of its industry faces a 1.3% wage decrease. Heterogeneity across different profit margins is charted in panel (b) of Figure 4. A firm with scant profit margins shifting from no dominant buyer reliance to 25% reliance faces a 1.7% wage decrease, while one with profit margins one standard deviation higher faces more than a 3.5% wage decrease. These patterns suggest that when a portion of workers' wages come from economic rents, workers are vulnerable to increased buyer power.

On the other hand, panel (c) charts the wage effects of buyer power by the intensity of R&D spending at supplier firms. Suppliers with little R&D spending have the familiar steeply negative wage effect of buyer dependence. But suppliers that spend 10% or more of their revenue on research are able to blunt the negative effects of buyer power. Among the most research-intensive supplier firms, substantial revenue reliance on dominant buyers is actually associated with higher wages. For research-intensive, value creating firms the benefits of selling to large buyers outweigh the negative wage effects of buyer power. However, benefiting in this way from large buyers is relatively rare: in 2014, only 20% of publicly traded firms had R&D spending intensity above 10% of revenue.

### Estimates from Buyer Mergers

The above estimates of the wage effects of buyer power are robust to potential bias from time invariant firm characteristics and to controls for worker and owner bargaining power, technological change, globalization and production changes. Nonetheless, some unobserved supplier change could drive both a reduction in wages and greater reliance on dominant buyers. To address this possibility I use buyer mergers as an instrument for dominant buyer reliance. Consolidation among buyers heightens their power over suppliers. If a supplier can easily switch

**Table 3**. Firm-level Wage Effects of Dominant Buyer Mergers

| | Successful Merger | | | Cancelled Merger (placebo) |
|---|---|---|---|---|
| | (6a) (First-stage) | (6b) (Reduced) | (6c) (IV) | (7) (IV) |
| Buyer Merger | 0.06*** | -0.03*** | | |
| | (0.00) | (0.01) | | |
| Share of Revenue from Dominant Buyers | | | -0.54*** | 0.28 |
| | | | (0.12) | (0.32) |
| Firm fixed effects | Yes | Yes | Yes | Yes |
| Year fixed effects | Yes | Yes | Yes | Yes |
| Firm-level controls | Yes | Yes | Yes | Yes |
| R2 | 0.76 | 0.94 | 0.94 | 0.94 |
| Within-R2 | 0.02 | 0.36 | 0.34 | 0.34 |
| F-statistic | 40.06 | 1425.83 | 1382.05 | 1649.21 |
| Wald F-stat. | | | 386.11 | 49.36 |
| Observations | 33424 | 33424 | 33424 | 33424 |

*Note*: Standard errors are in parentheses. Observations are firm-year pairs. Instrumental variable models are estimated using 2 Stage Least Squares. Placebo model 6 uses announced but cancelled mergers as an instrument. Firm-level controls are all financial controls included in Model 2.
*Source*: Compustat and Thompson-Reuters.
* p < .05; ** p < .01; *** p < .001 (two-tailed tests).

from one buyer to another, buyers would struggle to impose price restraint and wage reductions. However, unlike increased revenue reliance on buyers generally, supplier decisions (including wage-setting decisions) are unlikely to cause mergers among buyers. Mergers among buyers are thus plausibly exogenous to other facets of supplier-driven wage determination. Mergers are also unlikely to affect suppliers' wages through channels other than reliance on dominant buyers (the relevance of the merger is defined by a pre-existing contracting relation). In Appendix A I discuss the buyer merger instrument and the Two Stage Least Squares model.

Model 6 in Table 3 instruments for buyer reliance with an indicator for whether any of a supplier's buyers experienced a merger. Mergers between buyers increase suppliers' revenue reliance on dominant buyers by merging two current buyers or by increasing the size (and purchasing needs) of a single buyer already above the 10% reporting threshold. Model 6a reports on the first stage model and shows that experiencing a buyer merger increases dominant buyer revenue reliance by around 6%.[9] Next, Model 6b reports the reduced form effect of mergers directly on supplier firm-level wages. Experiencing a buyer merger is associated with a 3% decrease in supplier wages. Finally, Model 6c reports the full 2SLS model, which shows that the effect of a 10% increase in dominant buyer revenue reliance due to mergers is around a 5% reduction in wages. This point estimate is higher than the OLS result (-1.2%) above, which suggests that the OLS result could be biased toward zero by supplier selection into increased dominant buyer contracting. However, the confidence intervals for the IV estimate are large, and cannot rule out an effect as small as -3%.

While this model mitigates concerns about supplier selection into buyer reliance, it does not entirely resolve concerns about omitted variables. If common economic shocks affect both merger probability among buyers and supplier wage decisions, these should be addressed through year fixed-effects along with the revenue, profit margins and other financial controls described above. If a merger is initiated by buyers in response to high wages among suppliers, this should bias results positively (conservatively in relation to the expected effect), such that buyer selection into mergers happens when supplier wages are relatively high. However, if there is some other unobserved change driving buyer mergers and supplier wage reductions, this could still induce a spurious negative association. Model 7 uses announced but cancelled mergers as a placebo test (Blonigen and Pierce 2016): if some confounding variable leads to both wage decreases at supplier firms and prompts buyers to consider mergers, then cancelled mergers should have negative effects similar to completed mergers. Model 7 shows the placebo test point estimate is positive, suggesting that results in Model 6 are driven by actual merger completion,

[9] A key assumption in IV analysis is that the instrument is relevant, or sufficiently correlated with the endogenous variable to avoid bias. This assumption can be tested with an F-test (using the Wald F-statistic), which in the case of Model 6 is above the 10% critical value proposed by Stock and Yogo (Stock and Yogo 2005; Bound, Jaeger, and Baker 1995). Revenue reliance on dominant buyers is sufficiently responsive to buyer mergers that mergers isolate relevant variation in revenue reliance.

rather than a confounder driving both the consideration of buyer mergers and supplier wage reductions. But the estimate in Model 7 is very imprecisely estimated, so negative effects cannot be ruled out. Overall, buyer mergers provide further evidence for the negative wage effects of buyer power.

### Worker Composition and Industry-level Effects

These analyses include wage data only on publicly traded companies at the firm-level. Wage dynamics could be different among non-publicly traded firms. Moreover, firm-level wage data do not indicate whether wage changes result from changes in worker composition—perhaps by lowering education requirements or shifting toward a lower skilled mix of occupations—or from changes in wages at similar jobs before and after increased reliance on dominant buyers. To assess these two issues, I construct an industry-year-level measure of reliance on dominant buyers using the Compustat data, based on the employment-weighted mean of firms' buyer revenue reliance. I then merge this industry-level measure into worker-level data from the CPS from 1989 to 2014. Appendix C describes this merge and variable construction in the CPS.

**Table 4**. Individual-level Wage Effects of Reliance on Key Buyers

| | (8) | (9) | (10) | (11) | (12) | (13) | (14) |
|---|---|---|---|---|---|---|---|
| Share of Revenue from Dominant Buyers (industry-level) | -0.10** (0.03) | -0.11** (0.04) | -0.09** (0.03) | -0.03 (0.02) | | | |
| College * Share of Revenue from Dominant Buyers (industry-level) | | | | | -0.05 (0.03) | -0.08* (0.03) | -0.01 (0.02) |
| No College * Share of Revenue from Dominant Buyers (industry-level) | | | | | -0.21*** (0.06) | -0.11** (0.04) | -0.05* (0.03) |
| Industry fixed effects | Yes | Yes | No | Yes | Yes | No | Yes |
| Year fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Controls | No | Yes | Yes | Yes | Yes | Yes | Yes |
| Occupation dummies | No | Yes | No | Yes | Yes | No | Yes |
| State fixed effects | No | Yes | No | Yes | Yes | No | Yes |
| IndXOccXStateXEduc fixed effects | No | No | Yes | No | No | Yes | No |
| Individual fixed effects | No | No | No | Yes | No | No | Yes |
| R2 | 0.26 | 0.60 | 0.66 | 0.87 | 0.60 | 0.66 | 0.88 |
| Within-R2 | 0.00 | 0.28 | 0.21 | 0.03 | 0.28 | 0.21 | 0.03 |
| Observations | 3186158 | 3116654 | 2853089 | 1730564 | 3116654 | 2853089 | 1723122 |

*Note*: Standard errors are in parentheses and clustered at the 3-digit NAICS industry level. Observations are individual-year pairs. Sample size varies due to excluded singleton observations. Controls include 5 education categories, age, age squared, 4 race categories, gender, marital status, union membership, urban/non-urban indicator and part-time work status. Occupation dummies are 450 occupation categories from the IPUMS CPS occupations 2010 harmonized categories.
*Source*: Compustat and CPS ORG.
* p < .05; ** p < .01; *** p < .001 (two-tailed tests).

Table 4 presents results from this worker-level analysis. Model 8 includes industry and year fixed effects to show how individual workers' wages vary with changes in their industry's dependence on dominant buyers. A 10 percentage point increase in reliance on dominant buyers is associated with 1% lower wages. This coefficient is imprecise (its 95% confidence interval varies from -.04% to -1.6%), but its point estimate is very close to the firm-level coefficients presented above. Even when considering an entirely independent sample of private sector workers at both publicly traded and private firms, the wage effects of buyer reliance are similar to those estimated on the firm level.

Next, Model 9 adds individual-level control variables for 450 occupation categories, education, age, age squared, race, gender, marital status, union membership, state of residence, urban/non-urban residence and part-time/full-time work status. If buyer demands for lower wages are achieved by changes in the observable composition of suppliers' workers, then these controls would dampen negative wage effects. Model 9 shows that conditioning on these observable worker characteristics makes little difference to the negative wage effect of buyer power. Similarly, Model 10 models variation in pay within broadly defined jobs, by including interacted fixed effects for industry, occupation, state and education, This model assesses whether, even within the same industry-occupation-location-education requirement cells, increased industry dependence on large buyers lowers wages. Even in this quasi-within-job model, the negative wage effects of buyer power persist. The decline in wages associated with rising buyer power does not result from shifting education requirements or occupational downgrading, but instead reflects reductions in residual wages after controlling for observable worker characteristics and within broadly defined job cells.

Next, Model 11 adds individual worker fixed effects. The CPS ORG is a repeated survey, in which workers are asked about wages in the 6$^{th}$ and 18$^{th}$ months of the CPS survey. As such, it is possible to model the association between a one-year change in each worker's earnings and the change in reliance in their industry on dominant buyers. Model 11 shows that with individual fixed effects, the negative wage association with buyer power shrinks and loses statistical significance. After the individual fixed effects, the remaining variation in earnings with this short, two-period panel includes a high portion of measurement error. The confidence interval spans from -0.7% to 0.1%, so the apparent reduction in negative wage effects should be interpreted with caution. Nonetheless, this finding suggests that mean individual worker earnings mediates the effect of buyer reliance.

Finally, the CPS data allow effects to be broken out by education levels. Models 12, 13 and 14 present estimates of the wage effect of buyer reliance of workers with at least some college attendance and workers with no college education. The point estimates are consistently lower for non-college workers and remain negative and statistically significant even in the most stringent worker fixed effect model. The estimates are imprecise and the difference between effects for college and non-college workers is not statistically significant. However, these findings are suggestive that non-college workers face substantial wage penalties from increased buyer reliance in their industries.

### Additional Robustness Tests

The Compustat-based estimates of the wage effects of dominant buyer reliance could be biased due to several types of missing data and selection affecting the data: (1) some firms decide to report labor costs in some years and not other years; (2) some firms never report labor costs; and (3) firms select in and out of being publicly traded companies. In Appendix D, I discuss methods to test for selection issues of these different types. In brief: Heckman selection models address selection by firms that report labor costs in some years and not other years; models weighted by the inverse probability of reporting address selection by firms that never report labor costs; and models weighted by the industry composition of the overall economy test for selection by industry of firms into and out of public markets. Overall, these multiple methods of selection correction remain qualitatively consistent with the baseline model, suggesting that heterogeneity across sampled and out of sample firms is not so substantial as to reverse the predicted effects. I discuss these results further in Appendix D.

Next I check for heterogeneity in the wage effects of buyer reliance over time and across industry. Figure 5 plots coefficients of dominant buyer reliance over time. While dominant buyer reliance is generally associated with a negative wage effect, since the early 2000s negative wage effects have intensified, while in several years in the 1980s reliance on dominant buyers had negligible effects. During the period of wage stagnation and the restructuring of buyer-supplier relations, the wage effects of dominant buyer reliance turned increasingly negative.

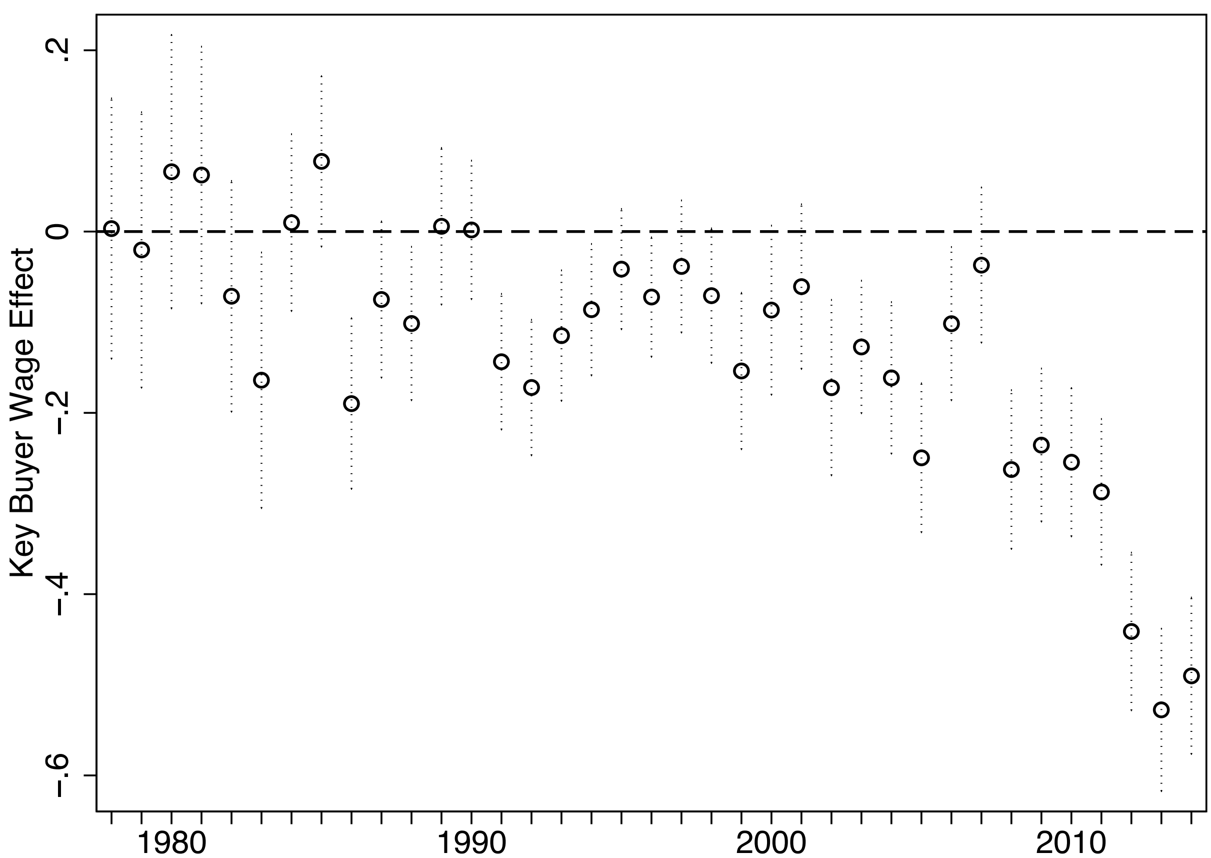


**Figure 5**. Effects of Dominant Buyer Reliance over Time.
*Note*: Coefficients are based on a model with the same controls as Model 2, along with year dummies interacted with the share of revenue from dominant buyers.
*Source*: Compustat.

Next, I test for heterogeneity across industries: perhaps the negative effects of dominant buyer reliance hold only for manufacturing, but not, for example, in the extractive industries. In Figure D1 in Appendix D, I interact dominant buyer reliance with each of six industry categories and the results are quite consistent, and not statistically distinguishable, for all industries except Finance, Insurance and Real Estate (FIRE). These results indicate a consistent buyer power effect on wages among non-financial companies. This finding provides further evidence of the potential effect of buyer power on wage stagnation: financial firms have been a prominent exception to overall wage stagnation.

Appendix D also outlines a series of additional robustness checks that test for reverse causality, firm-specific time trends, nonlinearities in buyer revenue reliance effects and a measure of buyer concentration.

## Adjusted Wage Trends

Finally, to quantify the contribution of increasing reliance on dominant buyers to the stagnation of wage growth since the 1970s, I compare real wage growth to an adjusted series that controls for the effect of buyer reliance on wages. In this analysis, I limit the sample to non-financial firms, as financial firms have not experienced wage stagnation.[10] I estimate firm-fixed effects models, as above, which removes large wage fluctuations due to the entry and exit of firms, and chart year effects in Figure 6. The solid black series shows that from 1955 to 1978, firm-level wages increased steadily, with an average growth of 1.8%. After 1978 however, earnings growth decelerated substantially, to a rate around one half of the previous trend. The

[10] The wage series for financial firms almost exactly continues the 1.8% growth trend enjoyed across firms in the pre-1978 series. Conducting the adjustment analysis with financial firms included in the sample yields the same difference between observed and real wage growth, but explains a bigger share of wage stagnation (9% compared to 18%), due to the smaller gap between wage growth before and after 1978 once finance firms are included.

difference between the dashed trend line, extrapolating linearly from the 1955 to 1978 data, and the observed series shows that had wage growth continued at the fast rates prior to 1979, wages would have been some 35% higher in 2014. This slowdown in earnings growth has been previously documented for the economy overall (Bivens et al. 2014). The Compustat series shows that this change in the rate of wage increases has also taken place in large, publicly traded companies and is not driven by selection of higher and lower wage companies in and out of the economy.[11]

[11] Note that this change could still be driven by changing selection across high and low wage *growth* companies.

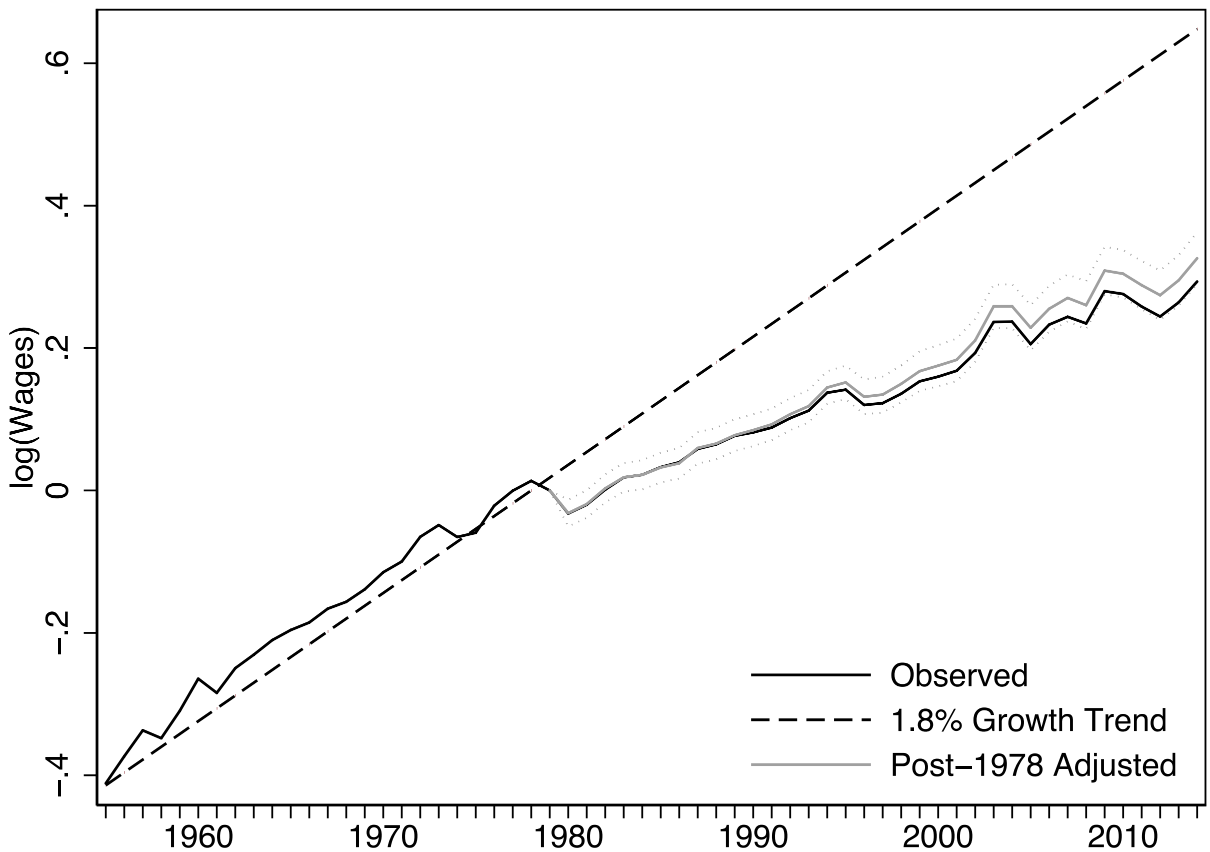


**Figure 6**. Firm Wage Trajectories in Non-Financial Companies; Adjusted Wages Control for Increase in Reliance on Dominant Buyers since 1978.
*Note*: Adjusted wages are based on year fixed effects, conditional on dominant buyer revenue reliance, years of continuous contracting and interactions with the number of buyers and a year trend.
*Source*: Compustat.

Next, I estimate adjusted post-1978 wages had increasing buyer power not had its depressing effect on wages. I do this by again estimating year fixed effects in the panel wage model and add controls for dominant buyer revenue reliance, length of continuous contracting and interactions of dominant buyer revenue reliance with the number of dominant buyers and a year trend. The remaining year fixed effects in this model indicate the wage levels that would have prevailed had both the incidence and negative wage effects of buyer reliance not increased over time. I chart this adjusted series in gray in Figure 6. The difference between the real and adjusted series can then be calculated as a share of wage stagnation relative to the earlier period, using the 1.8% growth trend line: growing buyer power from 1979 to 2014 accounts for around 10% of the decline in earnings growth relative to the 1955 to 1978 trend.

## Discussion

This article documents a restructuring of U.S. product markets from the 1970s to the present. This restructuring concentrated corporate purchasing power and left many workers employed at intermediate firms ultimately dependent on dominant buyers for a large share of their total revenue. As large retailers and other powerful buyers rapidly expanded their supplier base, suppliers in manufacturing, wholesale and transportation saw the share of their revenue coming from large buyers double since the mid-1980s.

This descriptive finding reconciles two lines of research on changing industry structure since the 1980s. Analysis of corporate consolidation has focused on the profitability boost afforded by rising monopoly power (Grullon, Larkin, and Michaely 2015; Furman and Orszag 2015), while research on buyer-supplier relations charted the rise of egalitarian, networked production (Sabel 1994; Podolny and Page 1998). These apparently contradictory accounts of a return of monopolistic capitalism alongside its post-Fordist fragmentation reflect the rising importance of buyer power. Corporate consolidation has been primarily horizontal, leaving vertical disintegration continuing apace and buyer-supplier relations ever more important. Yet horizontal consolidation among buyers has meant that rather than flat, collaborative networks, supplier companies rely on sales to powerful corporate buyers.

How does this restructuring of product market relations affect workers' wages? Increased dependence on large buyers lowers suppliers' wages. A 10% increase in revenue reliance on dominant buyers lowers suppliers' wages by 1.2%. This pattern holds even conditional on controls for bargaining power, productivity changes and other market determinants of workers' wages. Revenue reliance also appears to have stronger affects when the benefits of cost cutting are concentrated on a single buyer than when multiple large buyers are involved. The longer buyer-suppler relations last, the more wages diminish, consistent with social distance between outside buyers and suppliers' workers blunting wage norm effects. Mergers among buyers also lower suppliers' wages, suggesting that it is not supplier selection that drives the observed wage effects, but rather power exercised by dominant buyers.

These wage decreases seem to reflect suppliers losing rents that could otherwise be shared with their workers. Suppliers that are more profitable or have a larger market share face increased negative wage effects when they become dependent on dominant buyers. Worker-level analyses confirm that dominant buyer reliance lowers residual wages, conditional on observable worker characteristics, job types and, for less educated workers only, unobserved ability. While these findings suggest that wage reductions are due to losses in rents, they are far from determinative. It is also possible that in some labor markets large buyers function as a coordinating mechanism that allows their suppliers to lower workers' wages below a competitive rate. On the other hand, it is possible that some unobserved worker heterogeneity explains the decline in wages. Future research should do more to distinguish these different possible mechanisms.

Unlike value capture firms, value creation-oriented firms seem to be buffered from buyer power. Negative wage effects can be blunted when suppliers invest in distinctive capabilities through R&D. Overall, however, the negative wage effects of buyer power have been intensifying over time, and sector-level analysis shows that effects of buyer reliance are consistent across most industries. Among nonfinancial publicly traded U.S. companies, the recent rise in buyer reliance among suppliers has been associated with increased pressure and wage reductions.

Overall, rising buyer power could explain around 10% of wage stagnation among nonfinancial firms since the 1970s. These counterfactual results should be interpreted with some caution. The wage effects of buyer power are likely in part a proximate result of other economic changes. In particular, I cannot rule out the possibility that increased reliance on dominant buyers places domestic suppliers not only in stronger competition with one another, but also increases their direct exposure to foreign competition. Perhaps absent economic globalization, buyer power would be less effective at lowering wages. Future research, using industry case studies for which international data is available, should consider the relationship between wage stagnation, buyer consolidation, and the threat of offshoring. While the present study includes only domestic suppliers, its context is the ongoing internationalization of supply chains (Gereffi, Humphrey, and Sturgeon 2005). Yet, in the analysis, effects persist when conditioning on foreign revenue and when driven by mergers, which are unlikely to per se increase international sourcing. Moreover, negative wage effects are present in industries like wholesale and social services, which face little foreign competition. Growing buyer power cannot be analytically reduced to globalization; it plays its own independent role in wage stagnation.

This quantification stops short of a full decomposition of wage stagnation. Unfortunately, little previous research exists quantifying the contribution of other sources of wage stagnation, like technological change, declining unionization and economic globalization. Nonetheless, the effects of buyer power appear large enough to have made a substantive contribution to wage stagnation since the 1970s.

## Conclusion

This analysis reformulates and extends economic segmentation theory. When a portion of workers' pay comes from product market rents, it can be targeted for cost-cutting imposed by large buyers. During the period of U.S. wage stagnation, powerful outside buyers undermined organizational niches that previously benefited middle-skill workers: in industries from manufacturing to transportation to resource extraction, workers became vulnerable to buyers successfully demanding cost reductions.

This reformulation of economic segmentation theory contributes to sociological theories of wage determination. Sociological research on wages has bifurcated between tracing micro-level processes, like relational inequality in the workplace (Tomaskovic-Devey, Hällsten, and Avent-Holt 2015) and macro-level economic trends like globalization and technological change (Kristal 2013). Hierarchical relations between businesses—the inter-organizational meso-level—have been neglected. My analysis reveals the role of hierarchical relations between suppliers and buyers in determining the distribution of wages. Bargaining power does not just determine the distribution of resources within firms, but also affects the availability of resources across related and interacting firms.

On the other hand, studies of the wage effects of product market competition have tended to abstract away from organizational features that distribute advantages across owners and different groups of workers (Guadalupe 2007). Incorporating organizational features of pay setting—like social distance and concentrated benefits of cost cutting—into analysis of product market power clarifies how divergent interests within a given firm can make apparently constrained, dependent contracting relations into an acceptable strategy for suppliers. In the extreme case, suppliers' owners could use pressure from outside buyers to strengthen their bargaining credibility against workers' demands for higher wages. Building on organizational theories of pay setting shows how shifts in market power can impose specific pressures on suppliers' workers.

Beyond wage setting, this analysis implies a broader view of the set of actors shaping employment relationships. Often, employment relations are restricted conceptually to relations between groups within firms, such as interactions between workers and their managers. But recent research finds that outside entities, like financial firms, temporary employment agencies and independent contractor associations all intervene in employment conditions at firms with which they interact (Autor and Houseman 2010; Viscelli 2016; Lin 2016). Alongside these other outside, for-profit organizations, dominant buyers shape and constitute employment relations in putatively separate firms. Future research should examine how these outside entities affect other dimensions of the employment relationship, like worker voice, hours, job security and on-the-job training. Studying employment relations in a period of workplace fissuring requires placing employers in a broader network of between-firm relations.

Finally, the analysis broadens the scope of policy interventions to counter wage stagnation. Relations between firms should be considered in attempts to raise wages and reduce inequality. Policy discussion has focused on macroeconomic interventions (like reducing unemployment) or individual-level interventions like education and skill training. But a long history of interventions through buyer-supplier relations could be added to policy debates. In the 1920s, the U.S. textile workers union pressured large clothing purchasers to sign agreements imposing higher wages and longer-term contracts on their myriad small suppliers (Wolfson 1950). Under the National Recovery Act, New Deal regulators restructured steel purchasing contracts to improve working conditions among independent steel producers (Brand 1988:216-220). The 1931 Davis Bacon Act aimed to keep federal government buying power in the construction industry from undermining local wage rates. Activists and regulators laid the groundwork for the contracting relations that prevailed during the post-WWII period of wage compression. Renewed attention to buyer-supplier relations could contribute to contemporary debates on how to raise workers' wages.

Beyond attempts to regulate buyer-supplier relations, future research should consider the role of antitrust in rising buyer power. Since the 1980s, antitrust enforcement has weakened. Resultant rising corporate consolidation has been assessed primarily on its effects on consumers (Blonigen and Pierce 2016). Yet the analysis in this paper suggests another channel through which market power matters: buyer power reduces suppliers' workers' wages. The legacy of reduced antitrust enforcement should be assessed not only by its price effects on consumers, but also by its impact on workers (Atkinson 2015:126-127).

## Appendix A. Buyer Merger Instrumental Variables Analysis

An indicator for whether any of the dominant buyers listed by a supplier at year *t* experience a merger or acquisition is drawn from Thompson-Reuters Dealscan data available from 1979 to 2013. I merge the Thompson-Reuters Dealscan data to the cleaned version of the company names in the Compustat buyers data, using a fuzzy merge algorithm. This merge thus relies on a disclosed buyer accounting for at least 10% of a supplier's sales in the same year as a merger. I find that 1,185 supplier-year observations experienced at least one buyer merger (and 73 experienced an announced but cancelled merger). Mergers among buyers reduce suppliers' outside options and are predicted to increase buyer power.

In a two-stage least squares (2SLS) model, buyer reliance is regressed on an indicator of whether any of the suppliers' dominant buyers experienced a merger $z_{it}$, conditional on covariates included in Model 2 above:

$$x_{it} = \pi_1 z_{it} + \boldsymbol{\pi_2 v'_{it}} + \alpha_{1t} + \alpha_{2i} + e_{it} \qquad (2)$$

The predicted values $\widehat{x_{it}}$ from equation (2) allow wages, $\log(w_{it})$, to be modeled using the variation in dominant buyer revenue reliance predicted by mergers. The predicted values from this equation are used in the second-stage regression to predict wages:

$$\log(w_{it}) = \beta_{2SLS}\widehat{x_{it}} + \boldsymbol{\beta_2 v'_{it}} + \alpha_{1t} + \alpha_{2i} + e_{it}, \qquad (3)$$

where $\beta_{2SLS}$ is the unbiased estimate of the effect of dominant buyer reliance on supplier wages. I also report results in Table 3 from the reduced form equation, which shows the direct effect of mergers on supplier wages.

## Appendix B. Selection Correction Analyses

The estimates of the wage effects of dominant buyer reliance in the main models could be biased due to several types of selection affecting the data: (1) some firms decide to report labor costs in some years and not other years; (2) some firms never report labor costs; and (3) firms select in and out of being publicly traded companies. In the following section, I discuss methods used to test the robustness of the main findings to each of these potential selection problems.

First, within each firm panel, inconsistent reporting of labor costs could bias estimates of the wage effect of reliance on dominant buyers. Specifically, the fixed effects models are estimated off of variation from firms that report labor costs in at least two years. Many of these firms do not report labor costs in all years however: on average firms report labor costs in 60% of years and only 1 in 4 firms always report. Much of this variation reflects general firm accounting decisions, a secular decline in labor cost reporting and inconsistent data collection by Compustat. But some of the variation could be driven by unobserved changes affecting both the decision to report firm-level wages and decisions to change wage rates. If these changes are associated with reliance on dominant buyers, time-variant selection into reporting could bias the results in the main models. For example, if companies are less likely to report wages both when wages decrease and when reliance on dominant buyers decreases, then the apparent negative wage effect of dominant buyers could be an artifact of uneven reporting.

To test the robustness of the OLS results to this potential within-firm, time-variant reporting bias, I model selection into labor cost reporting using a Heckman selection correction. To identify idiosyncratic accounting decisions that are uncorrelated with firm wage rates, but are correlated with selection into wage reporting, I follow Shin's (2014) work on these same data and use binary variables for missing values on Selling, General and Administrative Expenses and Research and Development Expenditures. Year-to-year variation in these auxiliary reporting indicators results from changes in general accounting practices, rather than specific decisions regarding wage setting and labor cost reporting. The Heckman selection model is estimated with a first stage probit predicting selection, followed by a second stage OLS model of wages, conditional on the inverse Mills ratio of the predicted values from the probit model. I first estimate Heckman models with firm and year fixed effects, along with all control variables used in Model 2. While probit models with fixed effects face an incidental parameters problem and can thus be biased in the presence of small numbers of observations in each panel, simulations suggest that in 8 year panel data (the average for these data), bias should not be above 10% (Greene 2001). These Heckman models are all estimated on firms that report wages in at least one year and therefore make a time-variant selection decision into reporting.

Second, even if variation in reporting within panels does not bias the results, if there is heterogeneity across firms in the effect of reliance on dominant buyers, then both the OLS and Heckman estimates could still be misleading. While these estimates could be an unbiased local average treatment effect for reporting firms, they might not be representative of the magnitude of wage effects for non-reporting firms. Specifically, firms that report wages might face more negative effects from reliance on dominant buyers than firms that do not report wages. To address this possibility, I estimate models weighting by the inverse of the probability that a firm-year reports wages. To calculate these weights, I first fit a logit model of labor cost reporting on all firms in the sample, based on their NAICS 3-digit industry, year, log(Revenue), log(Employees), log(Property, plant and equipment) and log(EBITDA) (Pseudo-$R^2$=0.45). Together, these variables capture possible determinants of wage reporting like industry, time trend, firm size and firm employment. The predicted probabilities $p_{it}$ from this model indicate how likely each firm-year is to report wages. Weighting firms by $\frac{1}{p_{it}}$ downweights the types of firms more likely to report wages and upweights firms less likely to report. I then re-run the models using these weights. These models address selection on observable firm characteristics into reporting and not reporting wages, but cannot address effect heterogeneity along unobservable dimensions. Nonetheless, the rich set of variables available to model selection mitigate concerns about remaining unobserved characteristics.

Finally, even if time-variant reporting is unbiased and the firms that report wages face the same magnitude of effects as those that do not, it is unclear whether these estimates hold only for publicly traded companies, or whether wage effects of dominant buyers can be expected to affect non-publicly traded companies. While this external validity concern cannot be assessed directly, as buyer data from privately-held companies is unavailable, I use industry-level data from the

Bureau of Labor Statistics' Quarterly Census of Employment and Wages (QCEW), available from 1990, to provide some evidence on this concern. The QCEW is based on unemployment insurance reports by nearly all U.S. private sector establishments and provides establishment count data at the industry level. I weight reporting firms according to the number of establishments in their industry and year from the QCEW. This weights the sample to have a similar industry composition to that of the overall economy. This strategy cannot correct for potential differences between public and private firms within the same industry.

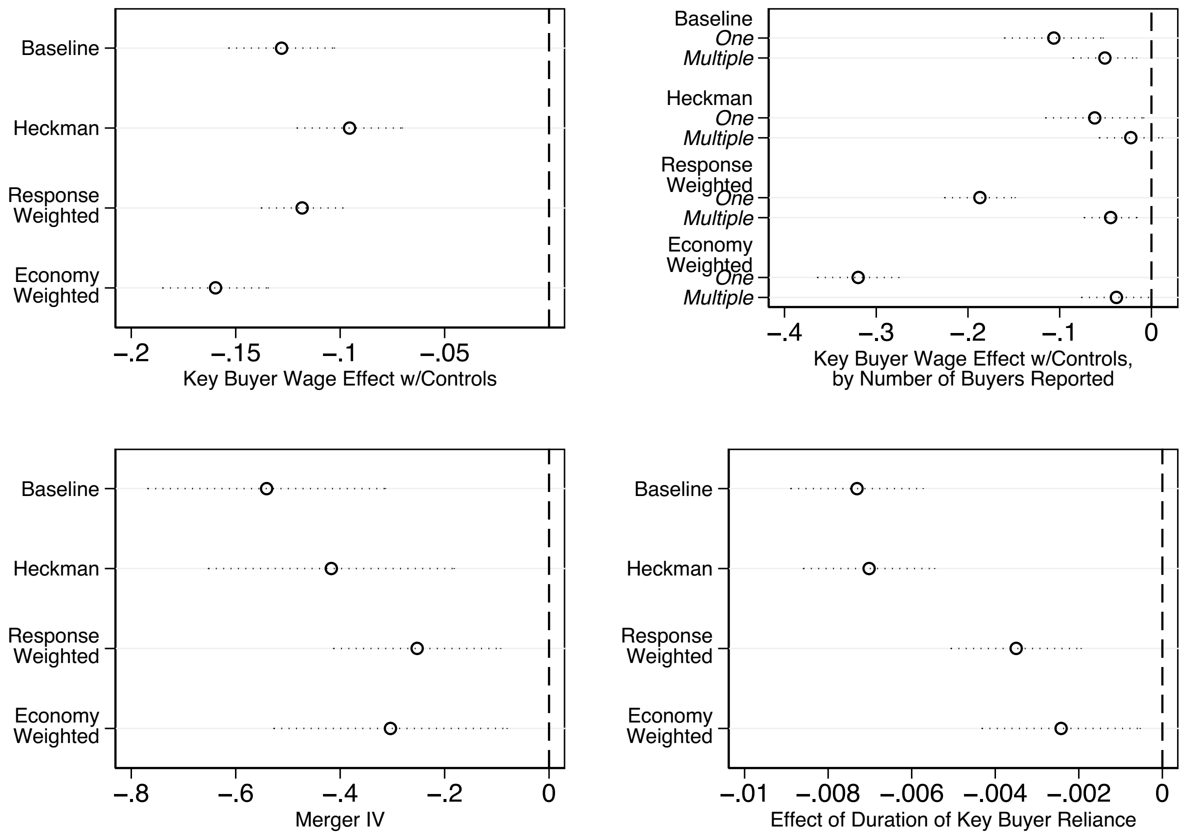


**Figure B1**. Robustness Tests on Effects of Reliance on Dominant Buyers (Main Results).
*Note*: Results are coefficients of interest from Models 2, 3, 4 and 5, with selection adjustments discussed in Appendix A.
*Source*: Compustat and QCEW.

Figure B1 summarizes the main effects across each of the tests for selection. The first column shows that the effect of dominant buyer revenue reliance in both the OLS and the 2SLS mergers models remains negative and statistically significant across the Heckman, response-weighted and industry-weighted models. The second chart in the first row shows that the difference in negative wage effects between suppliers dependent on a single dominant buyer and multiple buyers is also consistent across models, and gains statistical significance in the weighted models. Finally, the effect of buyer duration (a continuous variable version of the dummy model presented in Figure 3) remains negative and significant across all models. In the weighted models however, the effect size shrinks by around one half. Steady wage decreases from continuous contracting could thus be more pronounced for wage-reporting companies and for industries with disproportionately publicly traded companies than for companies overall. Nonetheless, the duration effect remains positive and statistically significant. These main results are robust to the selection tests presented here.

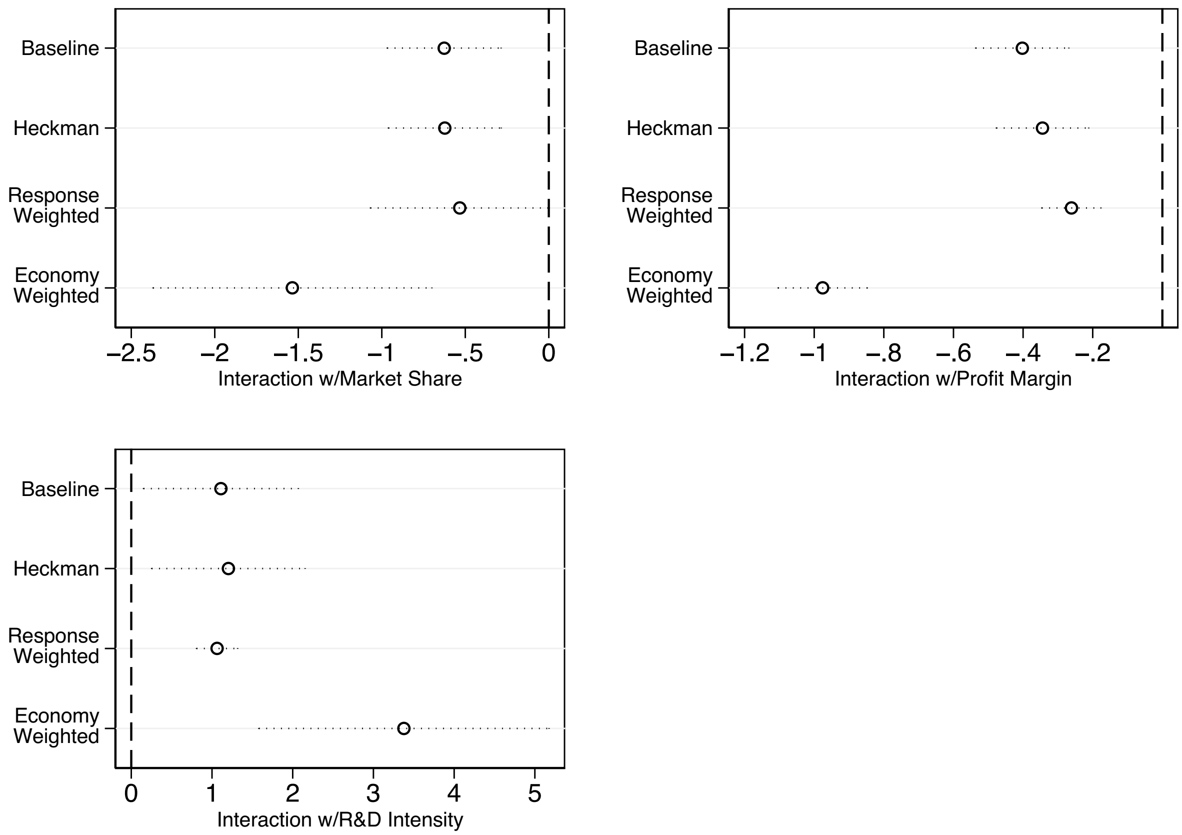


**Figure B2**. Robustness Tests on Effects of Reliance on Dominant Buyers (Interaction Results).
*Note*: Results are coefficients of interest from Models 2, 3, 4 and 5, with selection adjustments discussed in Appendix A.
*Source*: Compustat and QCEW.

In Figure B2 I present selection tests of the interaction results. These estimates are noisier, but remain qualitatively similarly to the baseline models. The point estimate of the market share interaction loses statistical significance under the weights correcting for consistently non-reporting firms. Results for the profit margin and R&D intensity interactions remain qualitatively similar, but become even more negative in the models weighted by economy-wide industry representativeness.

## Appendix C. Current Population Survey

The CPS ORG is drawn from the 1989-2014 data available through IPUMS CPS. I limit the time period to 1989 in order to use the panel respondent identifiers available in IPUMS from 1989 onward. To link these data to the Compustat buyer reliance data I first calculate the average revenue reliance on dominant buyers across NAICS 3-digit industry-years, weighted by firm-level employment. This calculation gives an industry-level, time variant measure of buyer reliance by industry. To merge this industry-level measure into the CPS ORG, I convert the Census Industry (CI) codes, present in the CPS, into NAICS 3-digit format, using the official Census crosswalk for the 1990, 2002, 2007 and 2012 CI and NAICS codes. In cases where multiple CI codes link to a single NAICS code, I treat workers with those codes as working in the same industry. In 11 cases where a single CI code links to multiple NAICS codes, I randomly divide workers in each of those CI codes into the multiple possible NAICS codes. This procedure prevents double-counting workers or leaving NAICS categories empty.

For the earnings outcome, I use the IPUMS CPS weekly earnings variable, which includes weekly pay for respondents who report being paid on a weekly basis and for hourly workers includes hourly wages multiplied by their usual number of hours worked per week. From 1990 to 1997, these earnings are top-coded at $1923, and from 1998 onward, at $2885. Following Autor, Manning and Smith (2016), I windsorize wages at and above these topcodes and multiply the topcode values by 1.5. I also restrict the sample, again following Autor, Manning and Smith (2016), to private sector workers aged 16 to 64.

I include a series of individual-level controls to measure worker composition. Education is categorized as less than high school, high school or finished 12$^{th}$ grade; some college or less than 4 years of college; a bachelors degree; or more than 4 years of college or a graduate degree. I control for 4 categories of race and ethnicity: non-Hispanic white, non-Hispanic black, Hispanic and other. The IPUMS CPS harmonized occupation categories are used at the detailed level (450 categories in total). Controls for state of residence and binary indicators for residence in a central metropolitan city, marital status, union membership or collective bargaining coverage and part-time work are also included.

In the models presented in Table 4, standard errors are clustered at the NAICS 3-digit industry level, as the dominant treatment of interest, reliance on dominant buyers, is only measured at the industry-year level. This clustering leads to less precision than is available with the firm-level version of the buyer reliance variable, but accurately reflects the reduced statistical power that results from using industry-level variation. I present unweighted regression results, but also run the models with ORG earnings weights. The results are not sensitive to weighting.

## Appendix D. Additional Robustness Tests

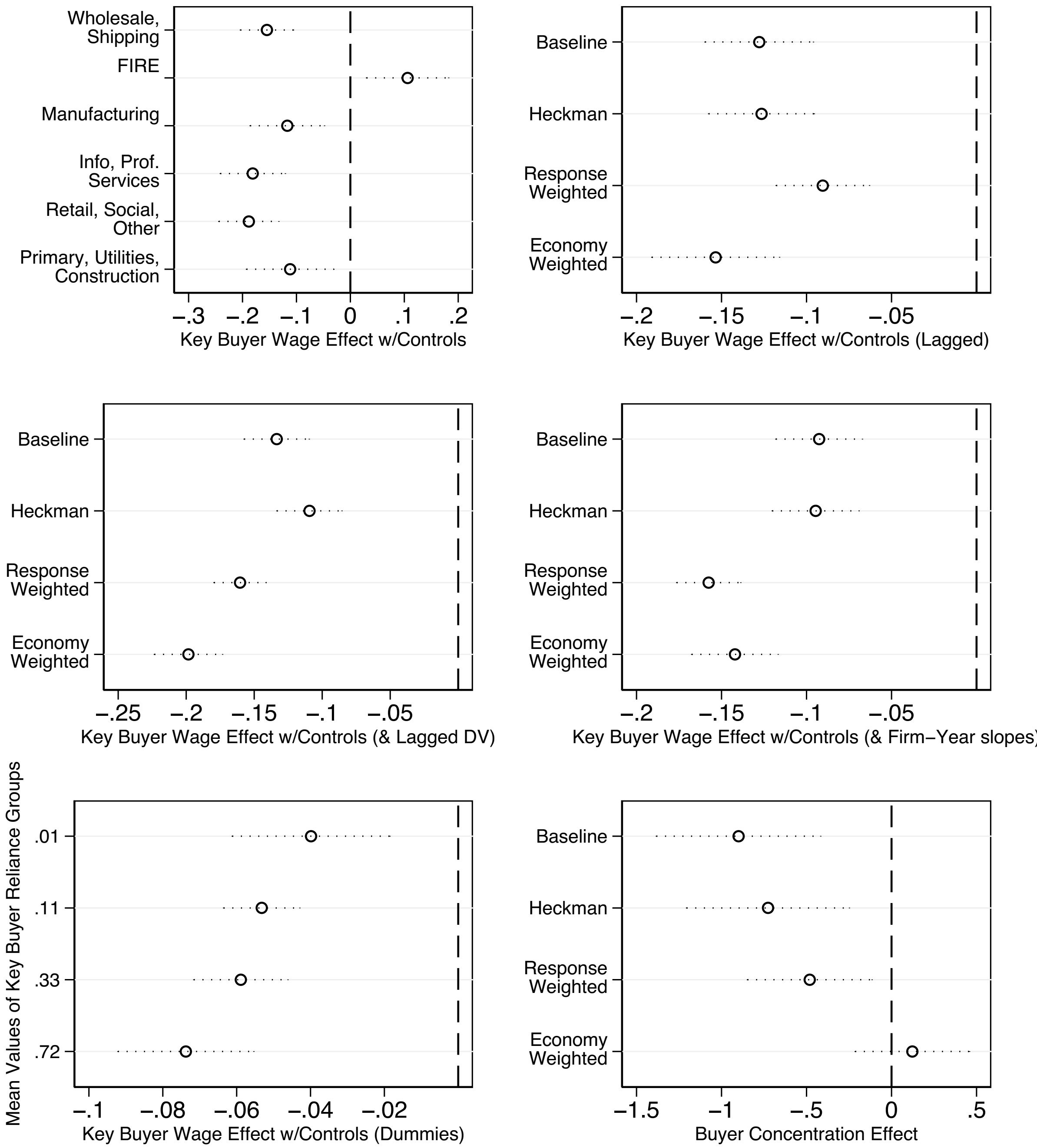


**Figure D1**. Robustness Tests on Effects of Reliance on Dominant Buyers (Supplementary Results).
*Note*: Results are coefficients of interest from tests described in the main text.
*Source*: Compustat and QCEW.

To test potential simultaneity between wages and dominant buyer revenue reliance, I estimate models lagging all independent variables. The second chart in Figure D1 shows results are consistently similar to the non-lagged models across all selection weighting models. In another model, I also add a control for a lagged dependent variable. Results again are consistent, providing evidence against reverse causality.

Next, I include firm-specific time trends as controls, in case some unobserved but linearly time-variant firm-level changes drive reliance on dominant buyers and decreased wages. The third chart in Figure D1 shows estimates with these additional trend controls are similar to the baseline models.

I also check the linearity of the dominant buyer reliance variable by dividing it into 5 dummy categories by the extent of revenue reliance (with means at 0%, 1%, 11%, 33% and 73%), and estimating the effect of firms switching across the categories. The results in Figure D1 show consistent wage decreases across the distribution of dominant buyer reliance: effects are not driven solely by firms switching from zero dominant buyer reliance to some reliance, nor from firms that become entirely captive to a dominant buyer. Rather, the wage effects become steadily more negative as dominant buyer reliance increases.

A final concern about the analysis is that while I test several dimensions of supplier dependence on buyers (share of revenue from dominant buyers; length of active contracting; number of dominant buyers), a supplier's outside option is unobserved. If a supplier faces many potential buyers, this will limit the power of even a single dominant buyer to intervene in the suppliers' employment relations. The Compustat buyer names data is messy and smaller buyers are not reported. Nonetheless, as a preliminary test, I calculated the revenue attributable to the top 4 dominant buyers per supplier industry as a share of total revenue for all firms in

Compustat. Figure D1 shows that increased buyer concentration is associated with decreased supplier wages (even conditional on all controls included in Model 2), across most weighting schemes (except the economy-wide industry reweighting). This measure includes substantial measurement error, but provides suggestive evidence of a fourth dimension of supplier dependence. Future research should do more to measure the outside option facing suppliers.

# 2. The Separation of Hand and Brain: Job Distillation and Earnings Inequality in U.S. Labor Unions

## Introduction

Since the 1970s, two thirds of workplace ethnographies report employers shifting tasks across jobs, in settings ranging from relocating a food processing plant (Fernandez 2001) to surgeons assigning menial tasks to medical students by seniority (Bosk 2003).[12] In a classic analysis, Harry Braverman uncovered, beneath this disorderly whirl of reassigned work tasks, a directional trend "separating the work of planning and the brain work as much as possible from the manual labor" (1974:88). The division of jobs by task content bifurcates jobs into the routine and the cognitively demanding. I call this process of dividing complex tasks out from low-paid, routine tasks *job distillation*.

Notwithstanding its prominence in case studies from the sociology of work, analysis of job distillation is missing from research on earnings inequality. Explanations for rising inequality have focused on the decline in routine manual tasks due to technological change (Autor and Dorn 2013), and on changes in the wage paid per task, perhaps due to union decline or decreased skill supply (Goldin and Katz 2008; Western and Rosenfeld 2011). Despite interest in the organizational context of inequality (Tomaskovic-Devey, Hällsten, and Avent-Holt 2015; Cobb 2016), the allocation of tasks into jobs has largely been neglected.

In this article, I systematize the concept of job distillation and theorize its consequences for organizational earnings inequality. Job distillation can increase earnings inequality by (1) separating routine from complex tasks across different jobs and by (2) unevenly lowering wages per task as jobs become more homogenous with respect to their constitutive tasks. I then outline

[12] Calculation by the author, using data from the Workplace Ethnography Project (Hodson 2004).

a task-based framework for decomposing sources of rising inequality into changes due to task price, task proportions, task quantity, and task mixing across jobs. Attention to job distillation shows that prior inequality research has neglected inequality produced by sorting tasks into jobs.

I test this theory of task-job allocation inequality by studying job distillation among the representatives, managers and clerical workers directly employed by U.S. labor unions from 2005 to 2015. Because unions are required to report on the activities performed by each of their employees, they offer a unique opportunity with linked employer-employee data to measure tasks. To validate unions' employee activity reports as measures of work tasks, I conduct an original survey of union representatives. Labor unions, like other white collar offices, employ staff for a bifurcated set of tasks: routine clerical office work and non-routine interactional and strategic tasks. Variation in the extent to which these different types of tasks are mixed or separated across jobs allows a test of the role of job distillation in driving organization-level earnings inequality.

Specifically, I estimate the earnings effects of doing more routine tasks and of doing a more homogenous job. I then use variance function regression to quantify the contribution job distillation makes to within-organization earnings inequality, relative to organization-wide changes in task price, proportions and quantity. Finally, I assess the mechanisms (task mix and task price changes) through which job distillation affects inequality.

This article identifies a key process of work reorganization connected to rising earnings inequality. Insights from the sociology of work bridge recent sociological research using linked employer-employee data, which emphasizes organizational processes but has not used data on actual work tasks (Tomaskovic-Devey, Hällsten, and Avent-Holt 2015; Dencker and Fang 2016) to the labor economics literature on tasks, which has neglected organizations (Autor and Handel

2013). In doing so, I demonstrate that a complete theory of organizational earnings inequality requires attention to the way tasks mix across jobs. More broadly, the analysis denaturalizes the job structure undergirding the earnings distribution. In place of a fixed occupational structure determined by economic and technological constraints, attention to job distillation reveals more modular scaffolding. Earnings inequality rests on processes that combine and fragment tasks across jobs.

## Job Distillation and Earnings Inequality

The sociology of work has documented a rich history of how the shifting organizational division of labor sorts tasks into jobs. In early twentieth century Taylorism, employers sought to split planning from manual labor in a "separation of hand and brain" (Braverman 1974:87). Managers and engineers took on organization and design decisions, narrowing workers' purview to the implementation of a production plan (Nelson 1975). To distinguish from other aspects of Taylorism (Littler 1978) I call this process of sorting routine from complex tasks across different jobs, *job distillation*.

Following a post-World War II lull, during which many jobs were stabilized through union rules and human relations policies (Edwards 1979; Guillen 1994), job distillation appears resurgent since the 1970s (Burris 1999). Beyond fin-de-siècle workshops, recent job distillation has roiled across the clerical and professional jobs of the white collar office (Attewell 1987; Glenn and Feldberg 1977; Vallas 1987; Rogers 1999). Large law firms have moved from a two-tier system of partners and associates to one including non-equity partners and project attorneys assigned routine legal tasks (Yoon 2014; Brooks 2011). The introduction of new imaging technology fragmented check deposit processing from a single job type into four separate job types, defined by different tasks (Autor, Levy, and Murnane 2002). More broadly, theorists of the knowledge economy find that companies shunt complex tasks up to more knowledgeable workers in the upper levels of a corporate hierarchy (Garicano and Rossi-Hansberg 2006). In all of these cases, routine and complex tasks are divided across different jobs.[13]

This division and sorting of task and labor has long been observed to boost productivity. But it also appears to drive workplace stratification. Until the imposition of scientific management, iron rollers and machinists worked under self-imposed work allocation rules "which made each group of workers average very similar earnings" (Montgomery 1979:13; Montgomery 1987:210). In place of egalitarian craftsmen, Taylorism left a bifurcated job structure of workers and managers (Nelson 1975). Corroborating quantitative research attributes rising postbellum inequality to the decline of craft production and the concentration of workers in large factories (Atack, Bateman, and Margo 2004). However, case studies show that job distillation has often been implemented amidst technological change, work intensification and changing skill supply (Cressey, Eldridge, and MacInnes 1985; Savage and Lombard 1986). To distinguish the effect of job distillation from these other sources of wage changes, I specify two channels through which job distillation directly contributes to organizational earnings inequality: (1) a task mixing channel by which lower paid and higher paid tasks are clumped into different jobs and; (2) a job simplification channel, by which the wage paid per task declines unevenly.

[13] Note that this process is distinct from workplace fissuring or outsourcing (Weil 2014). Fissuring can co-occur with job distillation: several case studies find that outsourcing shifts simpler tasks to contract workers, while increasing the proportion of complex tasks done by remaining in-house workers (Davis-Blake and Broschak 2009). However, in many cases, like that of outsourced janitors or security guards, outsourcing shifts a job from one employer to another without changing the tasks comprising that job (Dube and Kaplan 2010). As such, the focus in research on the wage effects of fissuring has been on employer avoidance of rent-sharing and fairness norms (Cobb and Stevens 2016; Goldschmidt and Schmieder 2017). Unlike workplace fissuring, job distillation requires a change in the task content of a job.

For channel (1), the key prerequisite is that when workers do a higher portion of routine tasks, they are paid less (Autor and Handel 2013). When a job is composed of more routine tasks, it requires less skill and delivers less productivity, so earnings decline:

*Hypothesis 1: Working on a higher portion of routine tasks decreases earnings.*

When job distillation *separates out* complex from routine tasks, it concentrates lower paid tasks into lower paid jobs. For example, when manufacturing firms add new layers of managers and professionals, pay decreases for lower-level workers, who do less managerial and design work than they did previously (Caliendo, Monte, and Rossi-Hansberg 2015). This process of separating hand from brain could keep the price paid per task constant, but increases earnings inequality by sorting lower paid and higher paid tasks into different jobs:

*Hypothesis 2: By decreasing task mixing across jobs, job distillation increases earnings inequality.*

Second, by separating tasks of different types, job distillation also *simplifies* jobs. Even if the component tasks in an organization are unchanged, any given job will contain a more homogenous set of tasks. The division of labor reduces costs by lowering a job's skill requirements, or what Braverman calls the Babbage principle (1974). Instead of paying two highly skilled programmers to spend half of each day coding and half of each day answering phones, job distillation would allow employing one dedicated programmer and one assistant. This change lowers the price paid per task, as the programming job will only require programming skills and the assistant job will only require clerical skills. For example, in the insurance industry, increasingly homogenous jobs drove down wages for clerical workers (de Kadt 1979):[14]

[14] Braverman expected that job distillation would lower the skill needed by production workers overall (Attewell 1987; Form 1987; Spenner 1983). The strong position on deskilling predicts that the level of

*Hypothesis 3: Working on a more homogenous set of tasks decreases earnings.*

Job simplification will increase inequality when the jobs being simplified are relatively lower paid or if job simplification has stronger effects on lower-paid jobs. The skill requirements for a job are often determined by its highest difficulty task, so separating routine from complex tasks is more likely to lower earnings in the lower-paid than in the higher-paid job. Non-productivity factors could also matter: wage equity norms often bind more among coworkers doing the same work than between those in different jobs (Weil 2014; Card et al. 2012), so fragmenting jobs by task could weaken norms. Job distillation can thus heighten earnings inequality when job simplification changes the wage an organization pays for each task:

*Hypothesis 4: By reducing the complexity of jobs, job distillation increases earnings inequality.*

Job distillation could increase inequality through sorting tasks across jobs or through an uneven task price change driven by the simplification of jobs. How do these predictions from the sociology of work fit with prior research on the causes of earnings inequality? Before testing these hypotheses, I contextualize them in a broader framework for decomposing earnings inequality. Approaching the causes of earnings inequality through the sociology of work suggests a work-task-based framework to organize rival explanations.

## Other Sources of Earnings Inequality

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skill required in the economy overall is decreasing (Attewell 1987; Form 1987; Spenner 1983), while other research asks whether particular jobs are being deskilled, even if compensated by upskilling in other occupations or industries (Ikeler 2016; Wallace and Kalleberg 1982; Keefe 1991). In both formulations however, deskilling does not require job distillation. Deskilling can occur through automation, when tasks are replaced by new technology (Acemoglu and Restrepo 2017), or even in work process improvements that persist independent of learning by particular workers (Levitt, List, and Syverson 2013). Unlike deskilling, job distillation is necessarily relational, such that when one job loses complex tasks, another gains them.

Prior explanations for rising inequality include changes in pay per task; changes in the proportion of task types; and changes in the quantity of tasks across jobs. The forgoing discussion suggests task mixing across jobs as a fourth, neglected determinant of the earnings distribution. Figure 1 schematizes each of these processes using a simple case of two jobs and two task types (routine and complex).

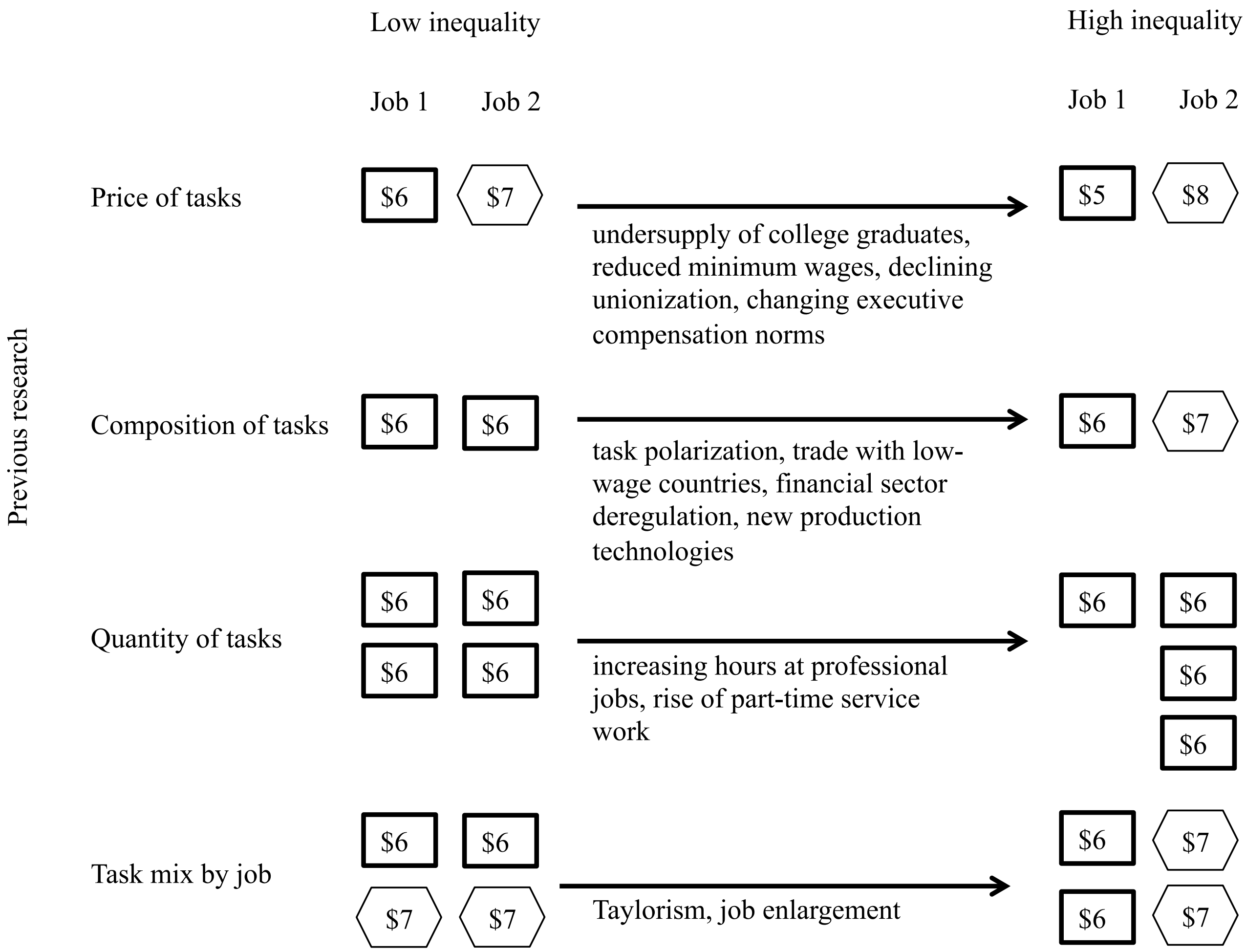


**Figure 1**. Task-related Processes Increasing Between-job Earnings Inequality.
*Note*: Routine tasks are denoted by squares; complex tasks by hexagons.

Line 1 of Figure 1 shows inequality increasing due to task prices diverging between routine and complex tasks. As noted above, if job simplification lowers earnings more for routine-task-intensive jobs, it does so through this wage per task channel. But earnings inequality also arises through the market price of labor, driven by factors like an undersupply of college graduates or reduced real minimum wages (Goldin and Katz 2008; Autor, Manning, and Smith 2016) and through local changes in organizations and bargaining institutions like declining unionization, changing executive compensation norms, the spread of variable compensation practices, and outsourcing (Piketty and Saez 2003; Weil 2014; Western and Rosenfeld 2011; Lemieux, MacLeod, and Parent 2009). To isolate the effect of job simplification, it is necessary to control for these organization- and labor market-wide sources of task price changes.

Earnings inequality can also increase due to changes in the proportions of tasks in an economy or organization. For example, new production technologies change the proportion of routine and complex tasks required, which is a second key confounding factor for measuring the effects of job distillation (Fernandez 2001). Causes of task proportion change also include task polarization, trade with low-wage countries and financial sector deregulation (Autor and Dorn 2013; Autor, Dorn, and Hanson 2013; Philippon and Reshef 2012). Line 2 of Figure 1 illustrates an inequality-increasing change in task proportions when the second of two jobs shifts to a more complex, higher paid task, while the first job remains unchanged.

Third, line 3 of Figure 1 shows how shifting a task from one job to another can generate increased inequality by changing the quantity of tasks or hours worked in each job. In this example, one job grows, while another shrinks, as in overwork by educated workers amidst involuntary part-time work for others (Kuhn and Lozano 2005; Cha and Weeden 2014). If task quantity decreases at the level of the economy overall, one job could shrink while another is unaffected. Either change could drive earnings inequality. In the context of work reorganization, the intensification and speed-up of work can occur alongside job distillation (Brannon 1994).

Task price, task proportion and task quantity changes together capture the diverse determinants of earnings inequality catalogued in prior research. But they miss the marquee inequality channel of job distillation: tasks associated with different levels of complexity, skill and pay can be more or less mixed across jobs, leading to more or less inequality. Line 4 of Figure 1 illustrates an inequality-increasing change, as in job distillation, in which higher- and lower-paid tasks are separated out across higher- and lower-paid jobs. In contrast, task mixing can increase due to team production, task rotation or job enlargement (Osterman 2006; Lindbeck and Snower 2000; Herzberg 1968). Integrating the sociology of work into analysis of inequality reveals this fourth pathway of inequality, through which tasks are mixed or separated across jobs.

Job distillation affects inequality through both task mixing effects (by separating complex from routine tasks) and task price changes (by job simplification). However, factors like work intensification (task quantity change), organizational change (task proportion change) and changing skill demand (task price change) could all confound apparent effects of job distillation. Untangling the association between job distillation on earnings inequality requires measuring the distribution of tasks alongside these confounding determinants of organizational earnings inequality.

## Measuring Tasks and the Case of Labor Unions as Employers

Data on work tasks is rarely collected. Previous research on tasks and earnings inequality has relied primarily on the Dictionary of Occupational Titles and O*NET, which includes only occupation-level data (Autor, Levy, and Murnane 2003; Autor and Dorn 2013). One survey asked individual respondents about work tasks, but was a one-time cross-sectional study (Autor and Handel 2013). Further, neither of these data sources nest employees in firms, which leaves job distillation indistinguishable from shifts in the overall proportions of tasks. Two case studies carefully measured work tasks, but were confined to single organizations (Fernandez 2001; Autor, Levy, and Murnane 2002).

The current article tackles these data limitations by drawing on administrative records reported by U.S. labor unions. The data cover individuals employed together in each labor union, which makes it possible to observe shifting configurations of tasks across jobs within unions. The data are also structured as panels on unions and individuals, which reduces the risk of unobserved, fixed attributes biasing estimates of earnings effects.

These improvements in data quality come at a cost of representativeness: unions are membership organizations with distinctive employment practices. But this distinctiveness can be exaggerated. 90% of national unions hired staff with no experience working in a union and 80% saw, like other employers, college degrees for staff as important (Clark, Gilbert, and Gray 1998; Clark, Gray, and Whitehead 2015). Unions have also been subject to economic pressures affecting other U.S. employers (Dunlop 1990:9-23). Like the manufacturing firms that were their longtime bastion, unions have faced secular decline, which has brought mergers (Moody 2009) and new management styles (Voss and Sherman 2000). Like other business and professional services providers, unions have seen their operations reshaped by information technology (Shostak 2001). Most importantly for this study, unions, like other white collar offices, face the challenge of allocating higher-paid interactional-strategic work and routine office tasks across jobs within a single workplace.

For unions, interactional work is mainly conducted by union representatives, organizers and business agents (Dunlop 1990). These employees interact with union members and shop stewards; negotiate collective bargaining agreements; and develop strategy for electoral and union recognition campaigns (Mcalevey 2012). They exemplify the social-interactional, cognitive and non-routine work that has been immune to technological replacement and offshoring (Frey and Osborne 2013; Deming 2016). In contrast, clerical tasks include bookkeeping, taking notes at meetings, processing membership applications, grievance forms and union expenditures, and performing other administrative work. These tasks are of the routine type vulnerable to technological change: information technology has substantially reduced the U.S. employment share of clerical occupations (Autor and Dorn 2013). I draw on these differences in task content to assess the contribution of job distillation to organization-level earnings inequality. When unions separate interactional and strategic tasks from routine clerical tasks across different jobs, they implement job distillation in context of the modern white-collar office.

## Data and Variables

**Table 1**. Descriptive Statistics on Unions and Union Employees

| | Mean | Standard Deviation | Observations |
|---|---|---|---|
| | | (a)Unions | |
| Within-union Var(log(Earnings)) | 0.26 | 0.14 | 25,494 |
| Job Distillation | 0.65 | 0.24 | 25,494 |
| Task Quantity Variance | 0.10 | 0.06 | 25,494 |
| Proportion of Tasks (Share Routine) | 0.41 | 0.23 | 25,494 |
| Wage Gap from Routine Tasks | 0.10 | 0.20 | 25,494 |
| log(Union members) | 10.47 | 2.39 | 25,494 |
| log(Revenue) | 17.04 | 1.87 | 25,494 |
| log(Assets) | 16.72 | 2.08 | 25,494 |
| | | (b) Employees | |
| log(Earnings) | 10.69 | 0.59 | 444,689 |
| Tasks: | | | |
| Complex: | 0.59 | 0.43 | 444,689 |
| Representational | 0.54 | 0.17 | 444,689 |
| Political | 0.05 | 0.43 | 444,689 |
| Union Contributions | 0.01 | 0.05 | 444,689 |
| Routine: | 0.41 | 0.43 | 444,689 |
| General Overhead | 0.25 | 0.39 | 444,689 |
| Administrative | 0.16 | 0.30 | 444,689 |
| Job Homogeneity | -0.29 | 0.35 | 444,689 |
| Task quantity | 0.38 | 0.39 | 444,689 |
| Tenure | 5.1 | 3.17 | 444,689 |
| Industry Experience | 6.3 | 3.34 | 444,689 |

*Note*: Values are based on analytical sample, with exclusions defined in the text. Observation counts indicate the number of union-years and number of individual employee-years in the sample. Values for union-level measures are weighted by union employee counts.
*Source*: OLMS.

Unions disclose financial and employment information to the Department of Labor's Office of Labor-Management Standards (OLMS) (Wilmers 2017). Starting in 2005, unions with over $250,000 in annual revenue were required to itemize the share of each of their employees' work time that goes to different activities. Analyzing data from 2005 to 2015, I consider the relationship between tasks and earnings for non-elected employees of labor unions. These data offer uniquely detailed linked employer-employee data that include worker-level salary and task information. The analytical sample is an unbalanced 11-year panel, including 3,500 labor unions and 105,000 employees of unions. Part A of the Appendix discusses the data and sample restrictions in detail. Table 1 provides descriptive statistics on the sample and variables discussed below.

The dependent variable in the analysis is logged employee earnings, defined as gross salary payments and adjusted for inflation. The variance of logged earnings in the sample is 0.35, around the mean within-industry variance of wages in the Current Population Survey Outgoing Rotation Group during the same time period.

Earnings are predicted by workers' share of routine tasks and by the homogeneity of their jobs with respect to tasks, following Hypotheses 1 and 3. To measure routine clerical tasks, I use the share of each employee's time going to administrative and general overhead activities, as opposed to representational, political, and contributions activities (Office of Labor-Management Standards 2014). I categorize representational and political activities as nonroutine interactional or strategic work.[15]

The OLMS instructions defining these categories are ambiguous, so there is likely some mix of routine and interactional tasks performed in each of the reported categories. Representational activities fit well with the definition of interactional work proposed above, including tasks "associated with preparation for, and participation in, the negotiation of collective bargaining agreements and the administration and enforcement of [these] agreements … [and] with efforts to become the exclusive bargaining representative for any unit of

[15] The final category of work, contributions, consists of union charitable and other donations. On average it accounts for only 0.5% of workers' tasks. I include this category with complex relational tasks, but in Part D of the Appendix I check the sensitivity of results to categorizing contributions activities as routine.

employees" (Office of Labor-Management Standards 2014:26). However, while general overhead activities include "support personnel at the labor organization's headquarters" elsewhere the instructions specify that "the salary of an assistant, whenever possible, should be allocated at the same ratio [to activity categories] as the person or persons to whom they provide supports" (Office of Labor-Management Standards 2014:29). These instructions suggest some mixing of tasks across categories—some representational activities are in fact routine clerical support performed for union representatives—which introduces measurement error into the independent variable. I expect this error to bias results toward zero, or conservatively in context of the hypotheses.

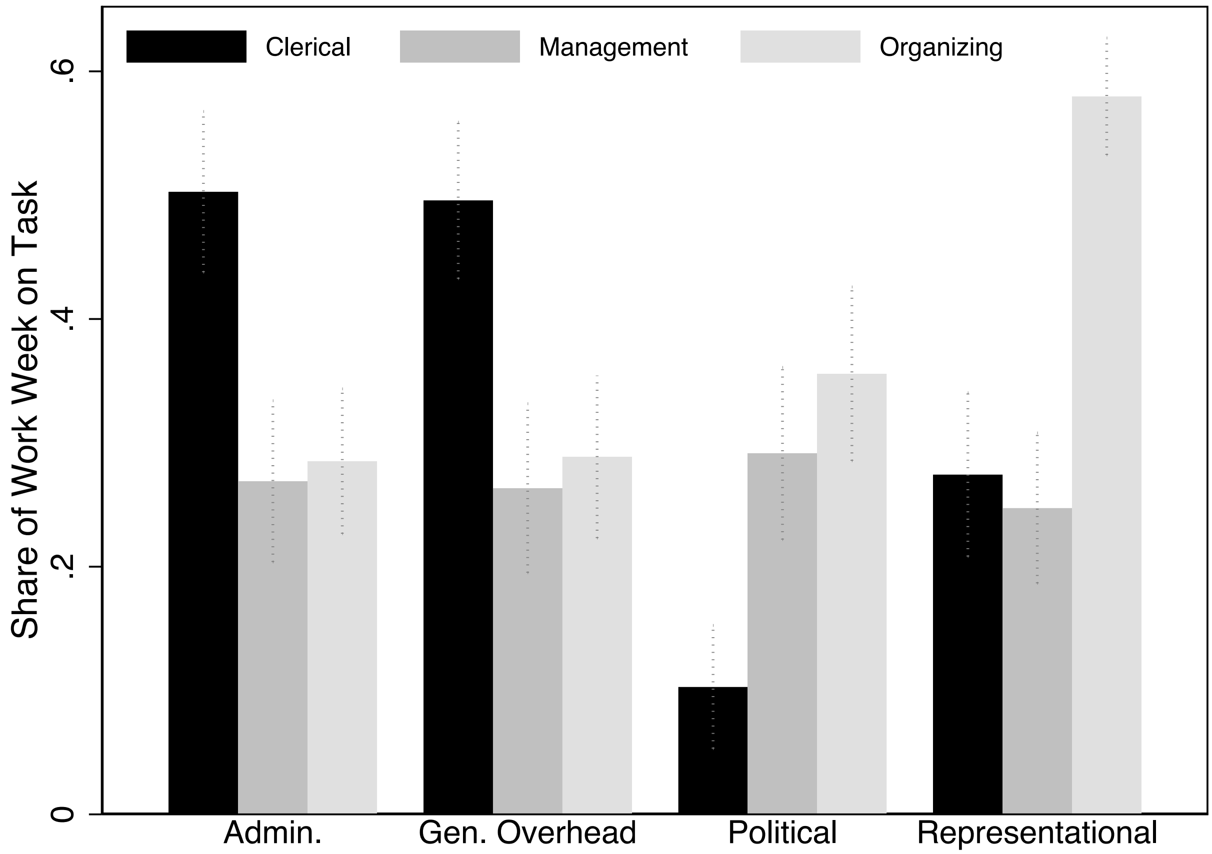


**Figure 2**. Distribution of Tasks by OLMS Activity Categories.
*Note*: Responses are coded as: Most of the week (0.7); Less than half of the week (0.3); Very little time (0). Responses need not sum to one. For more details on the survey, see Appendix Part B.
*Source*: Survey of union representatives (N=77).

Nonetheless, to validate this task measure, in March and April 2017, I surveyed union representatives (N=77), asking which OLMS categories consist of clerical, organizing and managerial tasks. Part B of the Appendix provides further details on the survey methods and sampling frame. Figure 2 displays the results. Respondents reported administration and general overhead jobs to be dominated by clerical work. In contrast, the bulk of representational activities were organizing and management task-related: 93% of respondents reported that organizing and management tasks accounted for most of the work week of representational employees. Political work was mainly organizing and management related as well.[16] Together, these survey responses validate interpreting routine clerical tasks as administrative and general overhead activities and non-routine interactional tasks as representational and political activities.

I use the task information in the OLMS data to construct measures of each job's share of routine tasks and the homogeneity of each job's tasks. I define $r_{iut}$ as the share of worker $i$'s tasks that are routine (administrative or general overhead) in union $u$ in year $t$. Hypothesis 1 predicts that as a worker does a higher portion of routine tasks, her wages will decline.

[16] Multiple respondents explained in written comments that their unions did not employ full time political employees Due to the ambiguity surrounding political work, I checked the robustness of models to categorizing political activities as routine tasks in Part D of the Appendix.

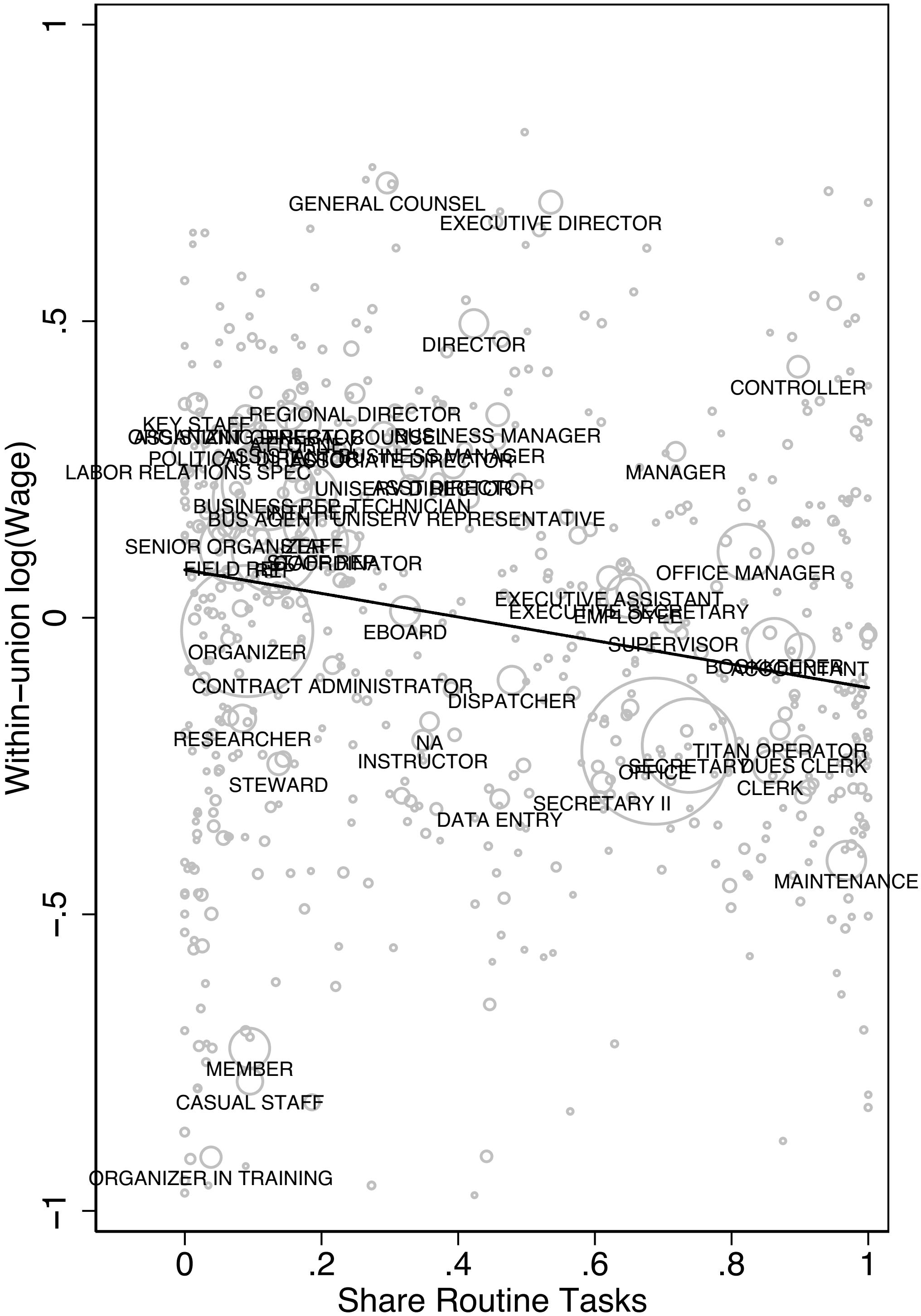


**Figure 3**. Earnings and Routine Tasks by Job Categories within U.S. Labor Unions.
*Note*: Routine tasks are defined as administrative and general overhead tasks. Bivariate regression coefficient is -0.20. Labeled job categories include all categories with at least 600 person-year observations (57% of total).
*Source*: OLMS.

Figure 3 charts workers' job titles by within-union relative earnings and by the share of routine tasks. Job titles of workers performing high shares of routine tasks show that this work is performed primarily by lower paid clerical employees: "Admin support;" "Office Admin;" "Office Manager;" "Maintenance." In contrast, representational and political work consists of the interactional work discussed above: "Business Agent;" "Organizer;" "Lawyer;" "Regional Director." These jobs are relatively highly paid. Exceptions include some highly paid job titles with high levels of administrative and general overhead tasks ("Controller", "Manager"), but overall there is a negative association between earnings and work on routine tasks.

Next, Hypothesis 3 predicts that as a worker does a more homogenous mix of tasks, her wages will decline. Earnings are determined not just by the simplicity or routineness of a given task, but by the simplicity or homogeneity of the job into which a set of work tasks are organized. I measure job homogeneity with Theil's entropy score (E), which has been used previously to measure racial diversity of schools and neighborhoods (Theil and Finizza 1971; Reardon and Firebaugh 2002). An entropy score ($E_{iut}$) is calculated for each worker-year using the share $\pi_{iutw}$ of each of the 5 task types $w$:

$$E_{iut} = \sum_w \pi_{iutw} * \log\left(\frac{1}{\pi_{iutw}}\right).$$ [17]

I multiply $E_{iut}$ by -1 so that jobs with a lower score are closer to an even mix of task types, while jobs with a higher score have a more homogenous set of work tasks. By using the 5 disaggregated task types to calculate job homogeneity, this measure includes both vertical task differences (as between routine and complex tasks) and horizontal task differences (as between administrative and general overhead activities or between representational and political

[17] As in previous work with the Theil index (Reardon and Firebaugh 2002), I define instances of no routine tasks as zeros, such that $0 * \log\left(\frac{1}{0}\right) = \lim_{\pi \to 0}\left(\pi * \ln\left(\frac{1}{\pi}\right)\right) = 0$.

activities). As job distillation proceeds, jobs become simpler or more homogenous with respect to their mix of tasks and $(E_{iut} * -1)$ increases toward zero.

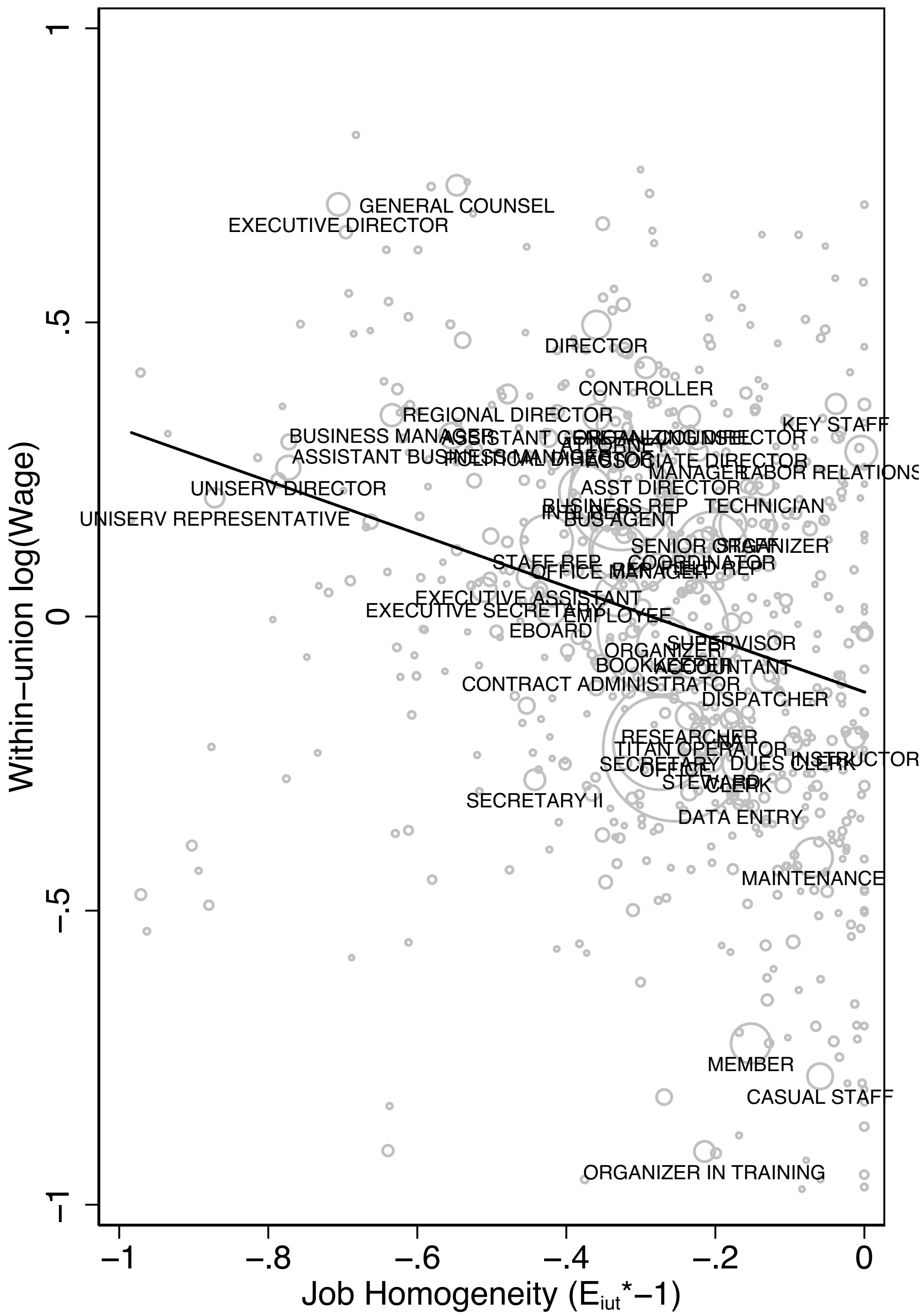


**Figure 4**. Earnings and Job Homogeneity by Job Categories within U.S. Labor Unions.
*Note*: Job homogeneity is defined as the homogeneity of a job's task distribution: jobs with mainly one type of task are considered more homogenous. Bivariate regression coefficient is -1.4. Labeled job categories include all categories with at least 600 person-year observations (57% of total).
*Source*: OLMS.

Figure 4 charts job titles and earnings by job homogeneity. Jobs with high homogeneity are can be entirely composed of general overhead tasks, like "Maintenance," administrative tasks like "Dues Clerk, or representational tasks, like "Senior Organizer." Other jobs, like "Business Manager" or "Regional Directors," do a mix of clerical and representational work. Although a number of highly paid jobs are homogenous, overall there is a negative association between job homogeneity and earnings. Just as more routine-task-intensive jobs tend to be lower paid, so too are jobs composed of a more homogenous set of tasks.

While the share of routine tasks and job homogeneity capture individual worker- and job-level characteristics, job distillation is a relational concept expected to contribute to organizational-level inequality. To predict organization-level earnings inequality, I calculate an organizational-level measure of the degree to which each union separates routine from complex tasks across jobs. I first calculate an entropy score ($E_{ut}$) for each union:

$$E_{ut} = \sum_{w} \pi_{utw} * \log\left(\frac{1}{\pi_{utw}}\right),$$

where $\pi_{ut}$ is the share of of union *u*'s tasks that are routine in year *t*. I then combine $E_{ut}$ with the worker-job level $E_{iut}$ score defined above, in order to derive a measure of segregation. This measure is the average deviation of individual-level task shares from the union-level task shares:

$$I_{ut} = \sum_{i=1}^{n} \frac{E_{ut} - E_{iut}}{E_{ut} * N_{ut}},$$

where $N_{ut}$ is the total employment of union *u* in year *t*. $I_{ut}$ is the Theil information index, which increases with increased job distillation. While the entropy score measures racial diversity in schools, the information index measures racial segregation across schools within a city (Reardon and Firebaugh 2002). In the job task context, the entropy score of a job measures its task homogeneity, while the information index measures the degree to which tasks are evenly mixed across jobs within an organization. For example, a union with only two employees, who each do

70% administrative tasks and 30% representational tasks would have a value of 0 on the $I_{ut}$ index, as each job in the union reflects precisely the overall organizational distribution of tasks. However, if that union implemented job distillation, it might maintain the same overall task proportions, but assign one worker entirely administrative tasks and concentrate the representational work into the other workers' job (60% of that workers' tasks). The union would then have a $I_{ut}$ index of 0.58. A high Theil information index indicates stronger job distillation and is predicted to be associated with higher inequality.

To use these task and job complexity variables to estimate the independent effect of job distillation, I control for other determinants of earnings inequality. I discuss the formulas and construction of each of these variables in Part C of the Appendix.

First, the union-specific earnings penalty for doing routine tasks captures general changes in local labor market conditions or in organization-specific pay setting practices. For example, a union with a larger supply of potential employees with the interpersonal and strategic skills necessary to organize might have a lower earnings penalty for routine tasks than a union facing weaker supply of skilled labor. The average gap in wages between routine and interactional tasks controls for the kind of local changes in labor demand and pay setting that impact the relative price of routine to complex tasks consistently, irrespective of task mix across jobs.

Next, task proportion is measured as the share of routine work out of all tasks performed by union employees. Some unions could be aggressively investing in information technology that reduces clerical, leaving employees to work on more complex tasks. Other unions could be campaigning and organizing new members, while others could be drifting along, only doing the minimum clerical work to continue as an organization. Controlling for task proportion adjusts for changes in the overall set of work tasks in a union, leaving job distillation-driven changes in the allocation of those tasks across jobs.

Finally, to measure the intensification of work, the data do not include direct measures of the number of tasks each worker performs. To proxy for task quantity, I use the inverse of the number of co-workers in each job title in each union. Fewer workers doing the same job indicates an intensification of work. The case studies cited above find that speed-up or work intensification often increase with job reorganization. If intensification is unevenly distributed across jobs, it could contribute to organizational earnings inequality. I control for the organization-level variance in task intensity by job title.

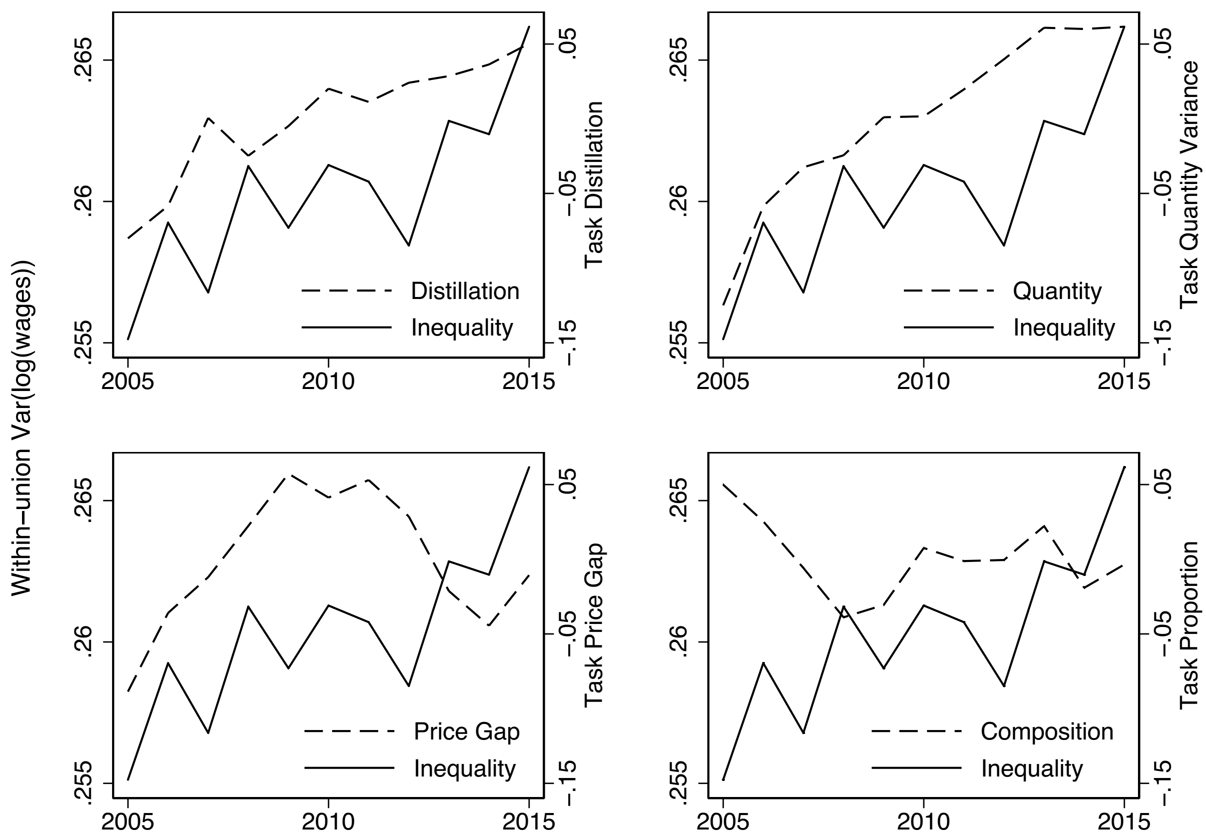


**Figure 5**. Trends in Wage Inequality and Task Measures.
*Note*: Task measures are those defined in the text, standardized for comparability.
*Source*: OLMS.

Figure 5 compares trends in within-union earnings inequality to standardized measures of job distillation, the routine task price gap, task proportion and task quantity variance. Earnings inequality within unions has been rising slowly, increasing around 5% since 2005. Job distillation and the variance of task quantity have also been rising. In contrast, task price and task proportion have followed mirror-image u-shaped patterns, in which the price of complex tasks initially increased, then declined. This initial descriptive evidence suggests that rising earnings inequality in labor unions could be associated with increased job distillation.

Methods

To test the inequality effects of job distillation I proceed in three steps. First, I establish the earnings effects through which job distillation can contribute to organizational inequality, by estimating the worker-level effect of doing more routine tasks and of working in a job with a more homogenous set of tasks. I then use variance function regression to model the effect of job distillation on within-union earnings variance, conditional on other determinants of organizational inequality. Finally, I add back in individual-level task measures to test the earnings mechanisms through which distillation affects inequality.

The first model predicts individual logged earnings *w* for worker *i* in union *u* and job title *j* at time *t*:

$$w_{iujt} = \beta_1 x_{iujt} + \beta_2 w_{iujt} + \boldsymbol{v}'_{\boldsymbol{iut}}\boldsymbol{\beta_4} + \boldsymbol{s}'_{\boldsymbol{ut}}\boldsymbol{\beta_5} + \alpha_{1t} + \alpha_{2d} + \alpha_{3g} + \sigma_{iujt}, \qquad (1)$$

where $x$ is a variable indicating the share of a worker's job accounted for by routine tasks, as discussed above as $R_{iut}$. $w$ is the homogeneity of a job, as defined above ($E_{iut} * -1$). I control for observable time-variant individual characteristics with a vector $\boldsymbol{v}'_{\boldsymbol{iut}}$ that includes task quantity ($R_{ujt}$, as above), years of tenure and years of union-industry experience. These latter two variables are calculated as the number of years each employee has worked for their current union employer since 2000 and the number of years each employee appears in the dataset working for any union since 2000, respectively.[18] I also control for union-year-level characteristics, $\boldsymbol{s}'_{\boldsymbol{ut}}$, with union-employer's number of members, logged assets and logged

[18] OLMS makes data available starting from 2000. Only data reported from 2005 and after includes task measures, but using the prior years of data allow a more accurate calculation of tenure and industry experience.

revenue, each of which could affect union employees' earnings. Unfortunately, the data do not include measures of skill, like years of education. To address the possibility that skill or other unobservable factors bias estimates, some models include fixed effects, $\boldsymbol{\alpha}'_{2d}$ for union-individual-job title triplets. The effects of shifting work tasks and job homogeneity on earnings are identified as long as no unobserved time-variant factors affect both task assignment and workers' earnings. Year effects $\alpha_{1t}$ are included in all models. Standard errors are clustered at the union level.

Next, I model the association between organization-level job distillation and within-union earnings inequality, controlling for union-year-level changes in task price gaps, task proportions and task quantity variance. I fit a variance function regression (Western and Bloome 2009) in which mean earnings are predicted with union-year fixed effects ($\alpha_{1ut}$):

$$w_{iut} = \boldsymbol{v}'_{\boldsymbol{iut}}\boldsymbol{\beta}_{\mathbf{1}} + \alpha_{1ut} + \sigma_{iut}, \qquad (2)$$

which isolates within-union-year earnings variance. I also include controls for experience and tenure in vector $\boldsymbol{v}'_{\boldsymbol{iut}}$, to control for observable variation in human capital.

Residual within-union earnings variance can then be modeled using the squared residuals $\sigma_{iut}$ from equation (2):

$$\log\,(\sigma^2_{iut}) = \lambda_1 d_{ut} + \boldsymbol{v}'_{\boldsymbol{ut}}\boldsymbol{\lambda}_{\mathbf{2}} + \zeta_{1u} + \zeta_{2t}, \qquad (3)$$

where $\log(\sigma^2_{iut})$ is the within-union variance of earnings of an employee *i* of union *u* in year *t*. Variance is predicted by job distillation ($d_{ut}$) and by a vector of union-year level controls $\boldsymbol{v}'_{\boldsymbol{ut}}$.

The primary controls are the price gap, proportions and variance of task quantity measures defined above. These controls distinguish the effects of job distillation from any change (like technological upgrading, increased labor supply or staff union organizing) that would operate by changing the organization-wide relative price of routine to interactional tasks or by changing the proportion or quantity of tasks. $\boldsymbol{v}'_{\boldsymbol{ut}}$ also includes controls for union size with the number of members, logged assets and logged revenue, which are included to control for differences in pay structure based on organizational size (Clark, Gray, and Solomon 1996). In all variance models year fixed effects are included ($\zeta_{2t}$), and union-level fixed effects are included ($\zeta_{1u}$), to control for time-invariant union heterogeneity.[19]

Finally, to test the mechanisms through which task distillation increases inequality, I add a additional predictors to equation (2). First, controlling for the share of routine tasks ($x$) removes any variance attributable to a changing mix of tasks across jobs. Second, controlling for job homogeneity ($w$) removes wage changes due to uneven task price changes stemming from job simplification. As these controls are added to the mean model, the earnings variance left for equation (3) will shrink. If the association between earnings variance and job distillation is indeed driven by these task mix and job complexity earnings channels, the $\lambda_1$ association will shrink when these controls are added.

These models first identify the individual-level connection between tasks and earnings. They then quantify the organization-level relationship between earnings inequality and job distillation. Finally, they connect the individual-level earnings mechanisms to organization-level

[19] Estimation for equations (2) and (3) proceeds iteratively to account for uncertainty in the mean regression in generating standard errors of the $\lambda$ coefficients (Western and Bloome 2009). First, OLS estimates of the mean regression (2) produce $\sigma_{iut}$. The variance equation is then estimated using a gamma regression with a log link function. This procedure is repeated until convergence, with each new gamma regression providing predicted variances to weight the next iteration of the OLS regression by its inverse variance.

inequality. This multi-step approach ties hypothesized micro-level pathways to organization-level inequality outcomes.

## Results

**Table 2**. Earnings Effects of Task Routineness and Job Homogeneity

| | (1) | (2) | (3) |
|---|---|---|---|
| Share Routine Tasks | -0.195*** | -0.042*** | -0.049*** |
| | (0.020) | (0.009) | (0.011) |
| Job Homogeneity | -0.207*** | -0.062*** | -0.075*** |
| | (0.022) | (0.010) | (0.013) |
| Task Quantity | 0.130*** | 0.183*** | 0.179*** |
| | (0.022) | (0.012) | (0.021) |
| Tenure | 0.043*** | 0.028*** | 0.032*** |
| | (0.003) | (0.002) | (0.003) |
| Industry Experience | 0.020*** | 0.013*** | 0.011*** |
| | (0.002) | (0.001) | (0.001) |
| log(Union members) | 0.005 | 0.080*** | |
| | (0.012) | (0.019) | |
| log(Union employees) | -0.111*** | 0.023* | |
| | (0.022) | (0.011) | |
| log(Revenue) | 0.133*** | 0.089*** | |
| | (0.027) | (0.016) | |
| log(Assets) | 0.037** | 0.011 | |
| | (0.014) | (0.010) | |
| Year fixed effects | Yes | Yes | Yes |
| Union-individual-jobtitle fixed effects | | Yes | Yes |
| City-job title-year fixed effects | | | Yes |
| Union-year fixed effects | | | Yes |
| R2 | 0.242 | 0.835 | 0.871 |
| Within-R2 | 0.240 | 0.025 | 0.012 |
| Observations | 444689 | 362259 | 284447 |

*Note*: Standard errors clustered at the union level. Sample size varies due to the exclusion of singleton observations from fixed-effects models. Union-year level variables are excluded from Model 3 due to union-year fixed effects.
*Source*: OLMS.
* p < .05; ** p < .01; *** p < .001 (two-tailed tests).

I first test Hypotheses 1 and 3, that workers' earnings decrease when they do a higher share of routine tasks or when their jobs become more homogenous with respect to tasks. The complexity of tasks and jobs could be associated with organization size or employee experience, so Model 1 includes controls for tenure, industry experience and time-varying union-level characteristics. The model also includes a control for task quantity, as the effect of job task changes could be accompanied by work intensification. Even conditional on these controls, Model 1 in Table 2 shows that workers doing a higher share of routine tasks receive lower pay. A 10 percentage point higher share of routine tasks is associated with a 2% decrease in earnings. Based on this estimate, doing a job like organizer (85% interactional tasks) rather than secretary (65% routine tasks) would correspond to 10% higher earnings. Likewise, workers doing a more homogenous set of tasks receive lower pay. A 10 percentage point more homogenous job is associated with a 2% decrease in earnings. Switching from a job with a mix of tasks like business manager (-.8) to a more homogenous job like data entry (-.2) would amount to around 12% lower earnings.

However, this variation in pay could result from differences in worker ability rather than from differences in tasks performed. Likewise, tasks are often assigned and determined by job titles, which themselves may affect earnings through non-task-channels (Baron and Bielby 1986; Strang and Baron 1990). For example, the association between tasks and earnings could be driven by earnings changes associated with workers being promoted. To address these issues, Model 3 adds fixed effects for union-individual-job title triplets. Effects here are estimated within the same worker with the same job title assigned a different mix of tasks over time. In Model 2, the magnitude of the effect of both task routineness and job homogeneity shrink. These reduced effects are consistent with either a strong correlation between task assignment and ability or with attenuation due to an increasing share of measurement error in the residual task variation. Nonetheless, the effects remain qualitatively consistent and statistically significant. Based on this stringent model, the same worker shifting from doing organizer work to secretarial work receives around 2% lower earnings. Likewise, a worker who shifts from a mixed task job like assistant business manager to more homogenous duties like data entry receives around a 3.5% drop in earnings.

Finally, Model 3 adds union-year and job title-city-year fixed effects to the model. The union-year effects remove any changes in tasks or wages that are common through a whole organization, like a shift in union strategy. The job title-city-year fixed effects remove variation due to shifting local labor market conditions, which might affect workers in different jobs differently. The ensuing model is estimated off of changes in a worker's tasks and earnings that are neither common across an organization nor across other workers in similar jobs in a given labor market. Results are consistent with Model 2, suggesting that the task and job changes themselves are driving earnings effects. These models of mean wages provide strong support for Hypotheses 1 and 3: workers doing more routine tasks or more homogenous jobs receive lower earnings.

**Table 3**. Within-union Earnings Inequality and Job Distillation

| | (4) | (5) | (6) | (7) |
|---|---|---|---|---|
| Task Distillation | 0.190*** | 0.149*** | 0.149*** | 0.195*** |
| | (0.027) | (0.028) | (0.027) | (0.027) |
| Task Quantity Variance | 0.395*** | 0.506*** | 0.376*** | 0.348** |
| | (0.111) | (0.115) | (0.111) | (0.111) |
| Wage Gap from Routine Tasks | 0.080** | 0.378*** | 0.046 | 0.046 |
| | (0.026) | (0.027) | (0.026) | (0.026) |
| Proportion of Routine Tasks | 0.101** | 0.074* | 0.038 | 0.078* |
| | (0.036) | (0.037) | (0.036) | (0.036) |
| log(Union members) | -0.037** | -0.034* | -0.041** | -0.039** |
| | (0.014) | (0.015) | (0.014) | (0.014) |
| log(Revenue) | -0.023 | -0.020 | -0.018 | -0.025 |
| | (0.013) | (0.014) | (0.013) | (0.013) |
| log(Assets) | -0.006 | -0.008 | -0.004 | -0.002 |
| | (0.011) | (0.011) | (0.011) | (0.010) |
| log(Union employees) | 0.461*** | 0.450*** | 0.448*** | 0.458*** |
| | (0.016) | (0.016) | (0.016) | (0.016) |
| Constant | -1.514*** | -1.535*** | -1.546*** | -1.519*** |
| | (0.009) | (0.009) | (0.009) | (0.009) |
| Year fixed effects | Yes | Yes | Yes | Yes |
| Union fixed effects | Yes | Yes | Yes | Yes |
| Mean equation controls: | | | | |
| Union-year fixed effects | Yes | Yes | Yes | Yes |
| Task routineness | | Yes | Yes | Yes |
| Job complexity | | | Yes | Yes |
| Task quantity | | | | Yes |
| Observations | 444689 | 444689 | 444689 | 444689 |

*Note*: All estimates are λ coefficients from equation (2) in the variance function regression, predicting variance of logged earnings. First-stage mean estimates are not presented here, but include controls as indicated. Standard errors are in parentheses, and were calculated using the iterated weighting procedure described in the text.
*Source*: OLMS.
* p < .05; ** p < .01; *** p < .001 (two-tailed tests).

Next, I test Hypotheses 2 and 4, which predict that these individual-level earnings effects contribute to organizational earnings inequality. Table 3 presents the variance coefficients $\lambda$ from equation (3). Model 4 shows that as job distillation increases, within-union earnings inequality increases, even conditional on changes in organization-wide task price gaps, task proportions and the variance of task quantity across job titles. A 10 percentage point increase in distillation is associated with a 2% increase in earnings variance.[20] In Part D of the Appendix, I present results across several models with coefficients standardized. The magnitude of the inequality effect of task distillation is quantitatively comparable to increases in the organization-wide task price gap between routine and interactional tasks.

Based on these variance coefficient estimates, the 3 percentage point increase in job distillation could account for around 25% of the small 4% increase in within-union earnings inequality since 2005.

These variance results show that job distillation at the union level is associated with increasing earnings inequality. But how much of the inequality effect of job distillation occurs through task mixing and job homogeneity price effects? In this last step of the analysis, I link individual-level earnings effects to organization-level inequality. Model 5 adds the share of routine tasks to the mean equation (2) of the variance function regression. This control removes any variation in earnings attributable to the distribution of routine tasks across jobs within a union: if job distillation raises inequality by changing the mix of tasks across jobs, then this control should reduce the association of union-level job distillation with earnings inequality. Model 5 shows that controlling for task mixing accounts for one quarter of the association between earnings inequality and job distillation.

Next, Model 6 adds job homogeneity to the mean equation, which removes variation in earnings due to the degree of job homogeneity. If inequality increases with job distillation due to job homogeneity lowering earnings in some jobs more than others, this control should again reduce the association of job distillation with earnings variance. Model 6 shows that controlling

[20] Due to the log link function in the variance regression, I exponentiate the raw coefficients in Table 3 to present percentage interpretations in the text. I express the change as a 10 percentage point increase (rather than a 100 percentage point increase), to use a scale relevant to observed variation in job distillation across unions (sd=26 percentage points) and within unions over time (sd=11 percentage points).

for worker-level job homogeneity further shrinks the association between earnings inequality and job distillation by another quarter.[21] Together, the direct mechanisms of a changing task mix and job homogeneity-related task price changes explain half of the association between increased job distillation and earnings inequality.

Finally, Model 7 adds task quantity to the mean equation. As tasks are shifted across jobs, the sociology of work suggests speed-up often follows. To check whether this intensification process biases the estimate of job distillation effects, I add the individual-level task quantity control to the mean model. While the association of task quantity variance and inequality decreases, the effect of job distillation is unchanged. Task quantity is measured indirectly here, so this result might reflect measurement error. But, it nonetheless suggests that work intensification does not drive the job distillation effect on inequality.

## Discussion

Recent sociological research on earnings inequality emphasizes the organizational context of inequality (Tomaskovic-Devey, Hällsten, and Avent-Holt 2015; Dencker and Fang 2016), but has proceeded without attention to the day-to-day stuff of work: job tasks. On the other hand, labor economists have shown that tasks are crucial determinants of wages and inequality, but have measured tasks at an aggregate level (Autor and Dorn 2013), decontextualized from organizational processes. I bridge these two streams of inequality research by drawing theory from the sociology of work. Beyond changes in task price, proportion and quantity, recent transformations of clerical and white collar work work harken

[21] Controlling for job homogeneity also decreases the point estimate of the task price gap by around 20%. This change is consistent with some component of the organization-wide price gap being driven by the homogeneity of jobs into which tasks are organized.

back to Taylorist strategies of scientific management. I theorize the process of distilling complex from routine tasks as a neglected determinant of rising earnings inequality.

Data from union employers provides a unique opportunity to test this prediction with linked employer-employee data. The analysis shows that when workers do a high share of routine tasks, their earnings decrease. Likewise, when workers do jobs with a more homogenous mix of tasks, their earnings decline. Even looking at year-to-year variation in task assignment for the same worker in the same job at the same union, doing more routine tasks and simpler jobs are associated with lower earnings. These findings hold conditional on the intensity of tasks and on organization-wide changes. These results suggest that effects are not driven by changes in task quantity like speed-up and work intensification, nor by patterns of sorting by worker ability or by broader changes in labor market supply and demand.

Variance function regression shows that job distillation increases within-organizational inequality, even after controlling for the overall organizational task wage gap, the variance of task quantity across job titles and organizational task proportions, along with union fixed effects and union size controls. When a union's job distillation increases by one standard deviation, its earnings variance increases by around 2%. Increased inequality due to a more unequal mix of routine tasks across jobs explains one quarter of this effect; changes in wages per task due to job simplification explain another quarter of this effect.

The analysis has several limitations, which suggest areas for future research. First, unions are a peculiar type of employer. Future studies should identify other industries in which task measures are available with linked employer-employee data. Beyond industry-level studies, economy-wide analysis of job distillation is needed. While some workplaces and industries have experienced increased job distillation during this period, in others the rise of teamwork, job rotation and job enlargement could have had opposite effects (Osterman 2006; Lindbeck and Snower 2000). Determining which workers and workplaces are exposed to job distillation would allow an overall assessment of the contribution of job distillation to rising wage inequality.

Second, the task data used in this study are recorded at a high level of generality. They were not designed to track specific tasks, but are rather proxies based on areas of union activity. While they improve on the occupation-level data used in many previous studies, future research would benefit from more detailed task measures.

Third, although the results presented here are robust across several modeling strategies, neither task assignment to individual workers nor the organization-level distillation of tasks across jobs are randomly assigned. As such, omitted variable bias remains a concern. Future research should explore potential field experiment sites in which tasks can be reallocated across jobs, without contamination from confounding influences like local labor market demand or changing business strategy.

Fourth, job distillation can operate through between-organization as well as within-organization processes. I focus on within-organization dynamics because there is little outsourcing in labor unions. Yet shifting organizational boundaries can be a means through which job distillation proceeds: if jobs are not lifted whole cloth from one organization to another, then an outsourcing event can fragment a previously mixed-task job. Only one third of rising inequality is within organizations (the inequality focused on in this paper); the remainder is due to worker sorting across employers (Song et al. 2018). Future research should consider how increasing distillation across jobs interacts with increasing skill segregation across employers.

## Conclusion

These findings bring into focus the malleability of the job structure, along with the earnings distribution it shapes. In place of stable occupational categories that invoke a deterministic connection between the tasks required by an economy and the jobs providing workers' pay, attention to job distillation highlights variable mixing of tasks across jobs. Some job distillation is given by technological and economic constraints. Changes in task price and task proportions, well-canvassed in research on wage inequality, can drive job distillation: a growing pay gap between complex and routine tasks raises the potential savings to employers of dividing tasks between jobs. But sociologists of work have also emphasized how the assembly of jobs is shaped by management ideology and employer strategies to control workers (Braverman 1974; Noble 1977), and how jurisdictional conflicts between professions allocate tasks across occupations (Abbott 1988). Organizations scholars have similarly found substantial contingency in the construction of jobs from tasks (Cohen 2013).

When viewed alongside these perspectives from the sociology of work, the findings in this article suggest unconventional channels for policy responses to rising inequality. Attempts to reduce inequality have focused on changing the price of labor per task, either by increasing the supply of educated workers or by encouraging unionization. Proposals to change the proportions of tasks include calls to revive American manufacturing with industrial policy or by reducing trade openness. Addressing inequality through job distillation suggests different strategies. Growing gaps in pay by task complexity lead to earnings inequality only if higher paid tasks are undiluted by lower paid ones. In some cases, dilution of high-paying tasks could pose serious productivity costs. In others, such dilution may be limited only by convention or management ideology.

This analysis also provides the rudiments of a broader theory of how inequality results from processes of fragmentation and separation. When tasks are distilled across jobs, inequality appears among workers previously doing interchangeable jobs for the same wages. The underlying pattern is of fragmenting otherwise pooled tasks, workers and jobs into separated types. In job distillation, as heterogeneity shifts from within-job to between-jobs, earnings inequality increases. This fragmentation is analogous to that found in the decline of the moral economy, in which unions can no longer enforce pay fairness norms within industries (Western and Rosenfeld 2011), and in the erosion of organization-wide pay compression (Cobb and Stevens 2016). These processes of fragmentation across multiple units of analysis define rising U.S. earnings inequality. Dominant theories of inequality emphasize workers' preferences and performances, as in economics, or relations of domination and exploitation, as in sociology (Tilly 1999). In contrast, processes of fragmentation need involve no new differences in ability or in relational power. They rest on differences that were previously suppressed becoming expressed and assigned differential rewards . Job distillation and other such processes translate otherwise latent heterogeneity—in ability, in productivity or in influence—into economic inequality.

## Appendix

### *Part A. OLMS data*

Unions are required to disclose financial and employment information in annual Labor-Management (LM) reporting forms, subject to audit by the Department of Labor's Office of Labor-Management Standards (OLMS) (OLMS 2017). The OLMS is an office tasked with monitoring internal union officer elections and enforcing anti-corruption regulations on labor unions. In addition to facilitating regulatory action by the OLMS, the data collected through the

LM forms provide publicly accessible information on labor union spending and employment practices. For example, a union member might be concerned about her union's political spending or how much her union representative is paid. The union member can pull the report for her specific union local and find this information. Due to the public transparency purpose of the reports, each union entity is identified by name (e.g. Communications Workers of America (CWA), Local Union 1037) and a permanent, uniquely identifying "file number" that consistently identifies unions across years. Employees are reported by first, middle and last names and by a job title. I construct an employee identifier based on first and last names of each employee. In a robustness test below, I check the validity of this approach for identifying union employees over time.

Figure S1 shows the form layout and questions covered in the employee information section of the LM form: unions must disclose the first and last name, title, gross salary and any other disbursements of each employee. In addition, the OLMS instructs unions to fill out "the percentage of time spent by each employee in the categories provided" to describe employees' activities (Office of Labor-Management Standards 2014:19-20). This reporting requirement is only present on the LM-2 forms, covering unions with at least $250,000 in revenue. Each employee's total working time is broken out across the activity categories of administrative, general overhead, representational, political, and (charitable) contributions activities. As mentioned in the main text, the instructions for applying these categories include some ambiguity. Nonetheless, these activity reports provide a rare source of employee-level data on work tasks.

**SCHEDULE 12 – DISBURSEMENTS TO EMPLOYEES**

FILE NUMBER:

| | (A) Name Last, First, MI | (B) Title | (C) Other Payer | (D) Gross Salary Disbursements (before any deductions) | (E) Allowances Disbursed | (F) Disbursements for Official Business | (G) Other Disbursements not reported in (D) through (F) | (H) TOTAL | | |
|---|---|---|---|---|---|---|---|---|---|---|
| 1 A | | | | | | | | | | |
| B | | | | | | | | | | |
| C | | | | | | | | | | |
| I | Schedule 15 Representational Activities | % | Schedule 16 Political Activities and Lobbying | % | Schedule 17 Contributions | % | Schedule 18 General Overhead | % | Schedule 19 Administration | % |
| 2 A | | | | | | | | | | |
| B | | | | | | | | | | |
| C | | | | | | | | | | |
| I | Schedule 15 Representational Activities | % | Schedule 16 Political Activities and Lobbying | % | Schedule 17 Contributions | % | Schedule 18 General Overhead | % | Schedule 19 Administration | % |
| 3 A | | | | | | | | | | |
| B | | | | | | | | | | |
| C | | | | | | | | | | |
| I | Schedule 15 Representational Activities | % | Schedule 16 Political Activities and Lobbying | % | Schedule 17 Contributions | % | Schedule 18 General Overhead | % | Schedule 19 Administration | % |
| 4 A | | | | | | | | | | |
| B | | | | | | | | | | |
| C | | | | | | | | | | |
| I | Schedule 15 Representational Activities | % | Schedule 16 Political Activities and Lobbying | % | Schedule 17 Contributions | % | Schedule 18 General Overhead | % | Schedule 19 Administration | % |
| 5 A | | | | | | | | | | |
| B | | | | | | | | | | |
| C | | | | | | | | | | |
| I | Schedule 15 Representational Activities | % | Schedule 16 Political Activities and Lobbying | % | Schedule 17 Contributions | % | Schedule 18 General Overhead | % | Schedule 19 Administration | % |
| TOTAL RECEIVED BY ALL OTHER EMPLOYEES MAKING $10,000 OR LESS | | | | | | | | | | |
| I | Schedule 15 Representational Activities | % | Schedule 16 Political Activities and Lobbying | % | Schedule 17 Contributions | % | Schedule 18 General Overhead | % | Schedule 19 Administration | % |

| | (D) | (E) | (F) | (G) | (H) |
|---|---|---|---|---|---|
| **TOTAL EMPLOYEE DISBURSEMENTS** | | | | | |
| LESS DEDUCTIONS | | | | | |
| NET DISBURSEMENTS | | | | | |

Form LM-2 (Revised 2010); (Tech. Rev. 2/2013) Page 16 of 26

**Figure S1**. Excerpt from Labor-Management Reporting Form.
*Note*: This page covers disclosures regarding employees of labor unions.
*Source*: OLMS.

I analyze LM data from 2005 to 2015 and include all non-elected employees in unions with at least 2 employees. The average union in the analytic sample has 17 employees, but they range from 2 to 1,200. This analytical sample reflects several restrictions. First, I exclude elected union officials because they are not comparable to employees in other settings. Union officials are reported in a separate part of the LM form from employees and are marked in the data. 40% of union officers make less than $10,000 per year from the union. Even restricting the set of union officers to those making more than $10,000, to make it more comparable to the employee data, the variance of logged earnings for officers is 40% higher than that of employees (0.49 compared to 0.35). Moreover, it is not even clear that these officers are employed to complete work tasks, rather than for political purposes. Due to these differences in the earnings patterns and roles for these two groups, I focus on employees in the analysis.

Second, I exclude single-employee unions as they have no within-union earnings inequality and several union-level predictors are undefined for these unions. This restriction removes around 1,000 of the smallest union entities from the sample. Finally, unions are not required to itemize salaries for employees who make less than $10,000 in a year. When unions do list these employees, I exclude them (around 11% of the sample) to ensure comparability across unions. In a robustness test below, I check whether these exclusions affect results.

*Part B. Survey methods*

I conducted the survey of union representatives to assess the validity of the activities reports in the OLMS data as measures of work tasks. Instead of interpreting the activities reports solely through the official instructions available through the OLMS, I asked the originators of the union reports how they interpreted the OLMS's categories in relation to their experience in their own unions.

To identify union employees, I used public names and email addresses published by the Federal Mediation and Conciliation Service (FMCS) in 2015 and 2016. Unions are legally required to report upcoming collective bargaining to the FMCS and to include a contact person in those reports. The titles of the contacts listed indicate they are primarily union representatives and business agents, along with a smattering of union presidents and other elected officials, full-time union negotiators and labor lawyers.

To draw a sampling frame from these bargaining reports, I first dropped any contacts with missing email addresses and dropped all duplicated names. The remaining sampling frame consisted of 5,579 unique union employees. I randomly selected an initial pilot of 50 union employees, followed by a full survey of another 500 employees. Of these participants, 59 emails bounced, leaving 491 that actually arrived in respondents' inboxes. 18 participants opened the survey link but did not complete any questions. 77 respondents completed the survey (no respondents started the short survey but did not complete it). The response rate was 14%, or the total 77 respondents out of the 550 sampled. The participation rate, excluding sampled participants with bounced email addresses, was 16%. The survey rollout consisted of an introductory email, a follow-up email 3 days later and a last chance for participation notice 6 days after that.

The introductory email for the survey explained that "you might have filled out LM forms at some point, so I want to ask which work tasks, in your experience, the Department of Labor's activity categories cover." The relatively low response rate (14%) likely reflects nonresponse from union representatives not involved in OLMS reporting.

For the survey instrument, I asked four separate questions covering representational, administrative, general overhead and political activities. I defined the clerical, organizing and managerial categories as similarly as possible to the discussion in the paper. The introductory text of the survey along with a sample question for the representational activity category is reproduced in Figure S2. The same questions were asked about administrative, general overhead and political activities (results are reported in Figure 2). I also included a free text entry question at the end of the survey to solicit feedback.

Every year, unions fill out "LM" forms for the Department of Labor. These forms ask how much each union employee works on *representational*, *administrative*, *general overhead* and *political* activities.

I want to understand the day-to-day work that usually fall under each of these activities. For example, representational activities might consist mostly of clerical work ("Most of the week"), but next to no management and strategy work ("Very little time").

First, think of someone working at your union who does primarily ***representational*** activities. In a typical week, how much time does this person spend on the following tasks:

| | Most of the week | Less than half the week | Very little time |
|---|---|---|---|
| **Clerical and Support**: for example, taking notes at meetings, keeping files, processing invoices, or other secretarial work | ○ | ○ | ○ |
| **Organizing and Working with Members**: Talking with stewards and members about grievances, conducting contract negotiations | ○ | ○ | ○ |
| **Management and Strategy**: Developing strategy around contract campaigns, electoral outreach or NLRB elections; making hiring and firing decisions | ○ | ○ | ○ |

**Figure S2**. Excerpt from Survey on Interpretation of LM forms.
*Note*: This excerpt includes the introduction of the survey and an excerpt coving representational activities.

*Part C. Key control variables*

While the analysis is focused on understanding the link between job distillation and earnings inequality, I construct a series of other task-related variables to control for other sources of earnings inequality.

First, I control for the organization-level price or wage difference between routine and interactional or strategic tasks. I do this by estimating a routine task earnings penalty for each union-year, using a hierarchical linear model with random slopes for each union-year. These union-specific random slopes can be interpreted as the decrease in logged dollars of earnings per each additional 1 percentage point of routine work. Note that the resulting measure captures task price in strictly relative terms: the ratio between representational and routine work does not capture common changes (like inflation) in the absolute price paid for these tasks. Following previous work on skill and task premiums (Goldin and Katz 2008), I consider this ratio the relevant determinant of earnings inequality.

Second, task proportion can be measured as the share of routine work out of all tasks performed by union employees:

$$C_{ut} = \pi_{ut},$$

where $\pi_{ut}$ is the share of of union *u*'s tasks that are routine in year *t*. This union-year level measure of the share of routine tasks captures organization-level variation in work task proportions. By controlling for this overall proportion of routine tasks, I isolate the reallocation of tasks across jobs described in job distillation.

Finally, to measure the quantity of tasks per job I calculate the number of co-workers in each job title in each union. When the number of workers with the same job title increases, tasks tend to be spread across more employees. Specifically, I use the inverse of the number of workers in union *u*, job title *j* and year *t*:

$$R_{ujt} = 1/N_{ujt},$$

where an increase in $R_{ujt}$ means a given worker in job title *j* does a larger share of the tasks associated with the job title. At the union-year level, I calculate the variance of $R_{ujt}$ within unions: when unions have more variation in the quantity of tasks across job titles, they are expected to have more earnings inequality stemming from an unequal distribution of tasks. Unfortunately, changes in the total set of tasks done by a job title are unobserved, which introduces measurement error into this control variable.

*Part D. Robustness tests*

To assess the robustness of the results in the paper to modeling decisions and sample and variable construction choices, I conduct a series of robustness checks, which I present below.

*i. Modeling*

I first discuss modeling decisions related to the mean wage models. I then consider the robustness of the variance function regression models, along with alternative approaches to modeling variance effects.

**Table S1**. Earnings Effects of Task Routineness and Job Homogeneity (Additional Models)

| | (8) | (9) | (10) |
|---|---|---|---|
| Share Routine Tasks | -0.225*** | -0.041*** | -0.050*** |
| | (0.017) | (0.007) | (0.008) |
| Job Homogeneity | -0.233*** | -0.071*** | -0.075*** |
| | (0.018) | (0.012) | (0.012) |
| Task Quantity | 0.187*** | 0.072*** | 0.110*** |
| | (0.015) | (0.007) | (0.009) |
| Tenure | 0.043*** | 0.032*** | 0.035*** |
| | (0.002) | (0.002) | (0.001) |
| Industry Experience | 0.017*** | 0.014*** | 0.009*** |
| | (0.002) | (0.001) | (0.001) |
| log(Union members) | 0.058*** | 0.062*** | 0.062*** |
| | (0.014) | (0.018) | (0.018) |
| log(Union employees) | -0.087*** | 0.028 | 0.012 |
| | (0.017) | (0.019) | (0.017) |
| log(Revenue) | 0.090*** | 0.093*** | 0.093*** |
| | (0.016) | (0.017) | (0.018) |
| log(Assets) | 0.009 | 0.006 | 0.010 |
| | (0.013) | (0.011) | (0.011) |
| Year fixed effects | Yes | Yes | Yes |
| Union fixed effects | Yes | | |
| Union-individual fixed effects | | Yes | |
| Union, individual, job title fixed effects | | | Yes |
| R2 | 0.383 | 0.804 | 0.806 |
| Within-R2 | 0.176 | 0.027 | 0.033 |
| Observations | 444689 | 407173 | 402485 |

*Note*: Standard errors clustered at the union level. Sample size varies due to the exclusion of singleton observations from fixed-effects models.
*Source*: OLMS.
* $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests).

First, in addition to the models discussed in the main text testing Hypotheses 1 and 3 and presented in Table 2, I also fit a series of additional fixed effects models, presented in Table S1. Each of these models estimates the effect of task routineness and job homogeneity by isolating slightly different sources of variation in pay and in task and job content. Model 8 includes union fixed effects and shows that comparing employees within the same union, employees doing a higher share of routine tasks and simpler jobs have lower earnings. Model 9 includes union-individual fixed effects, to compare the same employee, working at the same union, during years with more or less routine tasks and a more or less homogenous mix of tasks. In Model 9 again, both the share of routine tasks and job homogeneity is negatively associated with earnings.

Finally, Model 10 separately includes union, individual and job title fixed effects, to control for the average job title wage effect, conditional on union and individual fixed effects. In this model again, results are consistent with the findings presented in the text. However, the assumptions justifying this three-way fixed effect model requires more discussion. The model is estimated off of union employees who switch from one union to another (Guimaraes and Portugal 2009; Abowd, Kramarz, and Margolis 1999), and conditions out variation due to fixed worker ability along with fixed pay premiums associated with working for higher-paying unions. This model relies on workers and unions being connected by workers who switch across different union employers. However, because I use first and last names to identify employees over time, there is some measurement error when multiple workers in the data have the same name. One indicator of this issue is that around 6% of the name-years in the sample include more than one observations on a name in a given year. Part of this duplication comes from workers who are employed at one union for part of a year and another union for another part, and are thus reported by both unions. This category includes all workers who switch directly from one union to another (except those who switch at exactly the end of a fiscal year). As such, these repeated name-year observations are not surprising. However, some of these cases likely capture different workers with the same name who are employed at different unions. Depending on treatment of doubled person-year observations, there are 5,844 (if no person-year duplicates are included) or 11,435 switchers (if all observations are included). These switchers connect 91% or 95%, respectively, of the sample into the largest mobility group in the data. As such, the number of switchers is sufficient to estimate the two-way fixed effects model for the vast bulk of the data.

**Table S2**. Earnings Effects of Task Routineness and Job Homogeneity (No duplicate observations)

| | No duplicates: individual-year-union-job title | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Share Routine Tasks | -0.194*** | -0.042*** | -0.050*** |
| | (0.020) | (0.009) | (0.011) |
| Job Homogeneity | -0.206*** | -0.062*** | -0.074*** |
| | (0.022) | (0.010) | (0.013) |
| Year fixed effects | Yes | Yes | Yes |
| Union-individual-jobtitle fixed effects | | Yes | Yes |
| City-job title-year fixed effects | | | Yes |
| Union-year fixed effects | | | Yes |
| R2 | 0.242 | 0.836 | 0.872 |
| Within-R2 | 0.240 | 0.025 | 0.012 |
| Observations | 444273 | 361799 | 283979 |

*Note*: Models are defined as in Table 2.
*Source*: OLMS.
* p < .05; ** p < .01; *** p < .001 (two-tailed tests).

While, duplicated observations are of most concern for the AKM models, the individual-union-job title models presented in the text could also be biased by misidentification of individual workers over time. Even looking within union-individual pairs, there are 416 worker-years duplicated. These duplicates are either the result of clerical error on the part of unions or result from two coworkers having the same name. Table S2 reruns Models 1, 2 and 3 from Table 2, excluding these duplicated observations. Results are consistent with and without the duplicated observations.

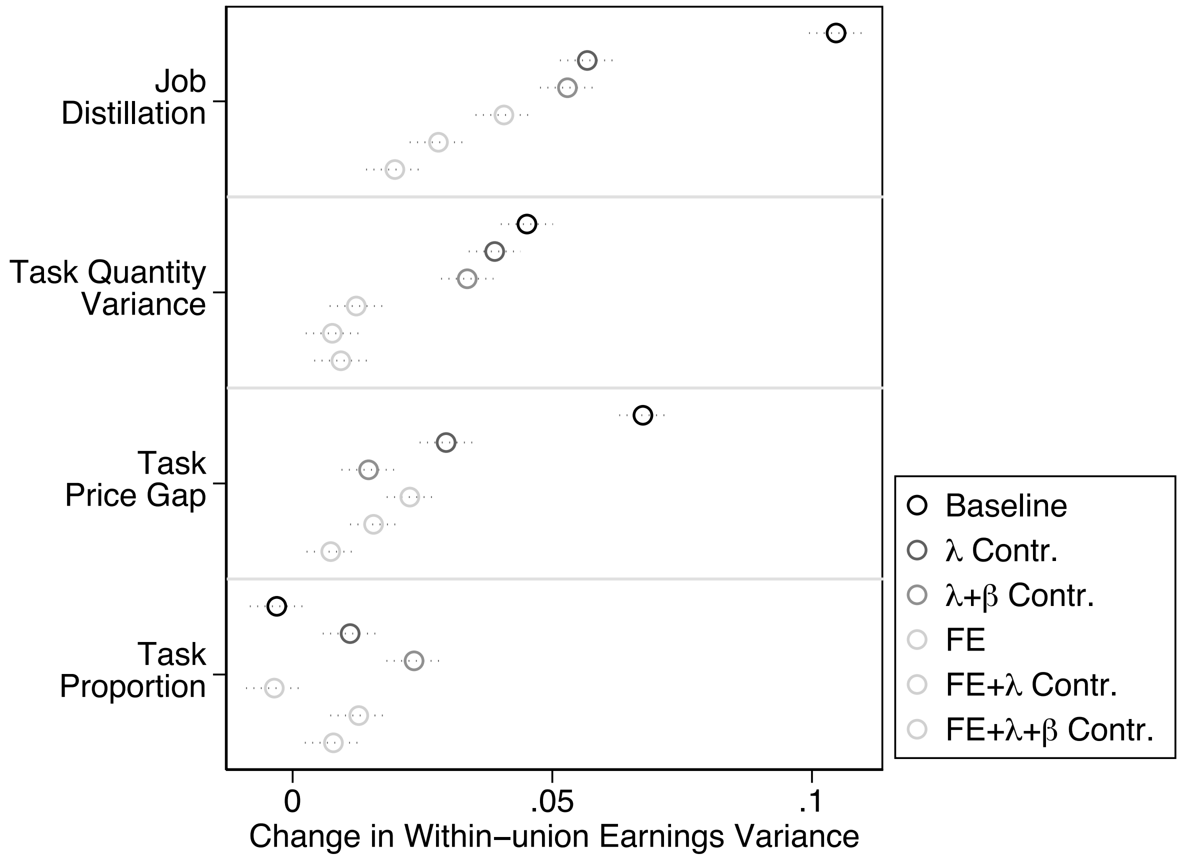


**Figure S3**. Comparing Inequality Effects Across Task Channels.
*Note*: Each variance function regression λ coefficient is interpretable as a percentage change in earnings inequality for a one standard deviation change in the predictor. All models include union-year fixed effects in the mean equation. λ controlled models include union size variables in the variance equation. β controlled models include experience and tenure in the mean equation. FE models include union fixed effects in the variance equation.
*Source*: OLMS.

Next, I consider robustness checks on the variance models. Figure S3 presents variance function regression coefficients derived from a series of different model specifications. Each coefficient is standardized for comparability. Specifically, I compare models with and without union fixed effects in the variance equation; with and without mean controls for tenure and experience; and with and without union size controls in the variance equation. Across each of these models, all of the task-related inequality channels are positively associated with organization-level earnings variance. The association of job distillation with earnings inequality is similar to that of task quantity variance and the task price gap.  Across multiple modeling specifications, job distillation has a quantitatively comparable association with earnings inequality as more conventional inequality channels explored in prior research.

**Table S3**. Within-union Earnings Inequality and Job Distillation (Union-level, Alternative Inequality Measures)

| | (1) | (2) | (3) |
|---|---|---|---|
| Task Distillation | 0.493*** | 0.050*** | 0.063*** |
| | (0.057) | (0.011) | (0.010) |
| Task Quantity Variance | 0.844*** | 0.006 | 0.054 |
| | (0.225) | (0.050) | (0.046) |
| Wage Gap from Routine Tasks | 0.034 | 0.060 | 0.042 |
| | (0.098) | (0.036) | (0.029) |
| Proportion of Routine Tasks | 0.115 | 0.054*** | 0.045** |
| | (0.081) | (0.015) | (0.015) |
| log(Union members) | -0.050 | -0.010 | -0.009 |
| | (0.044) | (0.008) | (0.009) |
| log(Revenue) | -0.076** | -0.003 | -0.008 |
| | (0.028) | (0.006) | (0.005) |
| log(Assets) | 0.024 | 0.005 | 0.004 |
| | (0.027) | (0.005) | (0.005) |
| log(Union employees) | 0.786*** | 0.111*** | 0.130*** |
| | (0.061) | (0.011) | (0.011) |
| Year fixed effects | Yes | Yes | Yes |
| Union fixed effects | Yes | Yes | Yes |
| R2 | 0.605 | 0.665 | 0.660 |
| Within-R2 | 0.079 | 0.069 | 0.087 |
| Observations | 24852 | 25059 | 25059 |

*Note*: All models are at the union-year level. Observations are union-year pairs, weighted by union employment. For Model 1, the outcome is logged variance of logged earnings. For Model 2, the outcome is variance of logged earnings. For Model 3, the outcome is the standard deviation of logged earnings.
*Source*: OLMS.
* p < .05; ** p < .01; *** p < .001 (two-tailed tests).

These variance function regression models provide a variety of advantages over an OLS approach (e.g. accurate standard errors and the inclusion of individual-level predictors). However, another approach to modeling within-union earnings inequality is to simply calculate union-year level variances and predict those variances using job distillation and the other organization-level determinants of inequality. Table S3 reports results from implementing this approach, by fitting OLS models predicting logged variance of earnings (most comparable to the log-link variance function regression approach); variance of earnings and standard deviation of earnings. Job distillation is consistently positively associated with earnings inequality; if anything the coefficients in the OLS approach are consistently larger than in the variance function regressions. Interestingly, several other inequality predictors (including the task price gap) are sensitive to this modeling decision.

A final option for modeling the effects of job distillation on earnings inequality is by simulating a counterfactual earnings distribution in which tasks are evenly distributed. This counterfactual requires simulating the earnings distribution that would hold if, when tasks are redistributed, unions retain the same characteristics and employees' other observable characteristics are unchanged. Employees with more routine tasks prior to the redistribution should benefit from redistribution, while those with more complex tasks should lose out. Likewise, employees with more homogenous jobs should benefit from increasing the mix of tasks across jobs. The counterfactual also requires attention to heterogeneity in the effects of task allocation across the earnings distribution: if routine tasks raise earnings at the bottom of the distribution and lower earnings at the top, then equalizing tasks could actually exacerbate inequality. To account for these two issues, I estimate quantile treatment effects by simulating a marginal distribution of earnings, following the approach proposed by Machado and Mata (2005) and generalized by Chernozhukov, Ferandez-Val and Melly (2013).

I first estimate quantile regressions across the earnings distribution, using *x* controls for observable characteristics described in equation (1), to generate a predicted distribution $F_{w_0|x_0}(w|x)$ of earnings *w*, based on the quantile-specific coefficients estimated from the observed (*0*) values of the *x* covariates across the earnings distribution. I then simulate an equalized distribution of tasks, $F_{x_1}$, by assigning each observation the mean values of share of task routineness for their union-year and assigning a low value of job homogeneity (-.3), but otherwise leaving worker and union characteristics unchanged. To estimate a counterfactual distribution, $F_{w\langle 0|1\rangle}$, if tasks were allocated equally as in $F_{x_1}$ but observed quantile associations were unchanged, the predicted conditional distribution is integrated with respect to the distribution of counterfactual characteristics:

$$F_{w\langle 0|1\rangle}(w) = \int F_{w_0|X_0}(w|x) dF_{x_1}(x).$$

This counterfactual unconditional distribution, $F_{w\langle 0|1\rangle}$, can then be compared to the fitted earnings distribution given in the observed distilled distribution of tasks. The differences between the distributions give quantile treatment effects that summarize the expected earnings changes if tasks were mixed evenly within unions instead of distilled between different jobs. Standard errors for the quantile effects are bootstrapped. I estimate this model using the procedure in Chernozhukov, Ferandez-Val and Melly (2013).

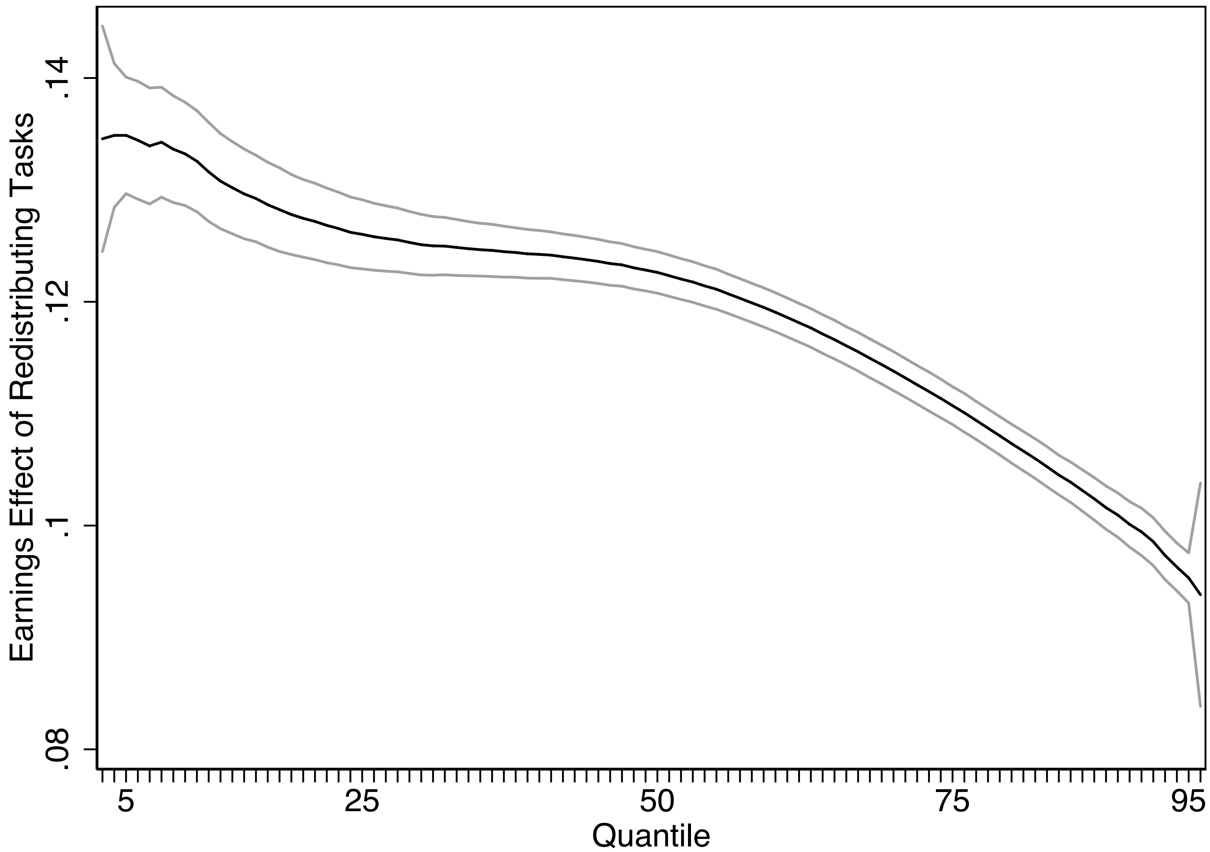


**Figure S4**. Quantile Treatment Effects of Equalizing Task Allocation.
*Note*: The counterfactual equal task allocation is defined as all employees doing their union-year's mean share of routine tasks and doing a mix of tasks equivalent to -0.30. Estimates are calculated through the procedure formulated in Chernozhukov, Ferandez-Val and Melly (2013), with confidence intervals drawn from bootstrapped standard errors. The top and bottom two percentiles are omitted due to unreliable estimates of the effects. Controls are those included in Model 1.
*Source*: OLMS.

Figure 5 presents quantile treatment effects, controlling for the same covariates as those included in Model 1 that simulate the effect of spreading routine tasks equally across jobs within each union and eliminating homogenous jobs. Figure 5 shows that under the egalitarian distribution of tasks, earnings would increase across the distribution, due to the increased pay associated with fewer homogenous jobs. However, earnings would increase most strongly at the bottom of the earnings distribution (around 13.5%) and more weakly at the top of the distribution (around 9%). This counterfactual exercise suggests the kinds of earnings changes that could result from reversing job distillation within organizations. Overall, it is consistent with the kind of decrease in earnings inequality predicted in the variance function regression models.

*ii. Sample and variable construction*

**Table S4**. Robustness of Results to Including Single-Employee Unions

| | (1) | (2) | (3) | (4) | (11) |
|---|---|---|---|---|---|
| Share Routine Tasks | -0.194*** | -0.040*** | -0.049*** | | |
| | (0.019) | (0.009) | (0.011) | | |
| Job Homogeneity | -0.204*** | -0.061*** | -0.075*** | | |
| | (0.022) | (0.010) | (0.013) | | |
| Job Distillation | | | | 0.107*** | 0.043 |
| | | | | (0.027) | (0.028) |
| Year fixed effects | Yes | Yes | Yes | Yes | Yes |
| Union-individual-jobtitle fixed effects | | Yes | Yes | | |
| City-job title-year fixed effects | | | Yes | | |
| Union-year fixed effects | | | Yes | Yes | Yes |
| Task routineness | | | | | Yes |
| Job complexity | | | | | Yes |
| R2 | 0.245 | 0.839 | 0.871 | | |
| Within-R2 | 0.244 | 0.026 | 0.012 | | |
| Observations | 457129 | 373548 | 284447 | 444689 | 444689 |

*Note*: Models (1), (2) and (3) replicate mean models in Table 2; models (4) and (7) replicate variance models in Table 3.

Beyond these modeling decisions, I also check the sensitivity of results to the sample exclusions imposed on the data. First, I try including unions with a single employee. For Models 1, 2 and 3, this can be done without changing the content of the model. For the variance function regression models, the variance of task quantity control needs to be excluded, as it is undefined for single-employee unions. Nonetheless, estimates for both task routineness and job distillation, shown in Table S4, are consistent with the other models.

**Table S5**. Robustness of Results to Including Employees Below $10,000

| | (1) | (2) | (3) | (4) | (11) |
|---|---|---|---|---|---|
| Share Routine Tasks | -0.308*** | -0.069*** | -0.102*** | | |
| | (0.029) | (0.016) | (0.027) | | |
| Job Homogeneity | -0.418*** | -0.100*** | -0.126*** | | |
| | (0.037) | (0.015) | (0.021) | | |
| Job Distillation | | | | 0.349*** | 0.320*** |
| | | | | (0.028) | (0.028) |
| Year fixed effects | Yes | Yes | Yes | Yes | Yes |
| Union-individual-jobtitle fixed effects | | Yes | Yes | | |
| City-job title-year fixed effects | | | Yes | | |
| Union-year fixed effects | | | Yes | Yes | Yes |
| Task routineness | | | | | Yes |
| Job complexity | | | | | Yes |
| R2 | 0.380 | 0.881 | 0.912 | | |
| Within-R2 | 0.379 | 0.024 | 0.011 | | |
| Observations | 500387 | 393842 | 310890 | 500387 | 500387 |

*Note*: Models (1), (2) and (3) replicate mean models in Table 2; models (4) and (7) replicate variance models in Table 3.

Second, I include workers making below $10,000. While not all unions report these workers, the reported workers below $10,000 are a substantial portion of the sample (11%). Table S5 shows results from the main models. All results for task complexity and job distillation are qualitatively similar, but the magnitude of the associations increases. This suggests that censoring of the bottom part of the wage distribution attenuates the estimated effects. The results presented in the paper may thus by conservative tests of the hypotheses, due to the left censoring in the data. On the other hand, adding the job homogeneity and routine task share controls reduces the magnitude of the variance association less than in the main models presented in the paper.

**Table S6**. Robustness of Results to Treatment of Uncertain Activity Categories

Treating contributions activities as routine

| | (1) | (2) | (3) | (4) | (11) |
|---|---|---|---|---|---|
| Share Routine Tasks | -0.196*** | -0.042*** | -0.051*** | | |
| | (0.021) | (0.009) | (0.011) | | |
| Job Homogeneity | -0.209*** | -0.063*** | -0.076*** | | |
| | (0.022) | (0.010) | (0.013) | | |
| Job Distillation | | | | 0.192*** | 0.123*** |
| | | | | (0.027) | (0.028) |

Treatment of political activities as routine

| | (1) | (2) | (3) | (4) | (11) |
|---|---|---|---|---|---|
| Share Routine Tasks | -0.191*** | -0.037*** | -0.040*** | | |
| | (0.020) | (0.008) | (0.011) | | |
| Job Homogeneity | -0.218*** | -0.064*** | -0.077*** | | |
| | (0.023) | (0.010) | (0.013) | | |
| Job Distillation | | | | 0.192*** | 0.125*** |
| | | | | (0.027) | (0.028) |
| Year fixed effects | Yes | Yes | Yes | Yes | Yes |
| Union-individual-jobtitle fixed effects | | Yes | Yes | | |
| City-job title-year fixed effects | | | Yes | | |
| Union-year fixed effects | | | Yes | Yes | Yes |
| Task routineness | | | | | Yes |
| Job complexity | | | | | Yes |
| Observations | 444689 | 362259 | 284447 | 444689 | 444689 |

*Note*: Models (1), (2) and (3) replicate mean models in Table 2; models (4) and (7) replicate variance models in Table 3.

Next, I check whether results are sensitive to different categorizations of the OLMS activities into routine and non-routine tasks. Specifically, both contributions and political activities, although a small share of overall employee tasks, were ambiguous. In Table S6, I show results in which I recategorize (1) Political activities and (2) Contributions as routine tasks. Results are qualitatively consistent with the main models, but coefficients shrink in the union-individual-job title mean fixed effects models when political activities are recategorized. While the coefficients remain within the confidence intervals in the main models, this shrinkage could represent increased measurement error from incorrectly categorizing political activities as routine tasks.

Finally, I conduct several checks assessing the separate identification of the job homogeneity and task routineness effects. Job homogeneity varies in part due to the share of vertical task components—routine and complex tasks—in a given job. Insofar as job homogeneity effects are estimated off this variation, they draw on the same underlying variation as the task routineness measure, but expressed through a different functional form.

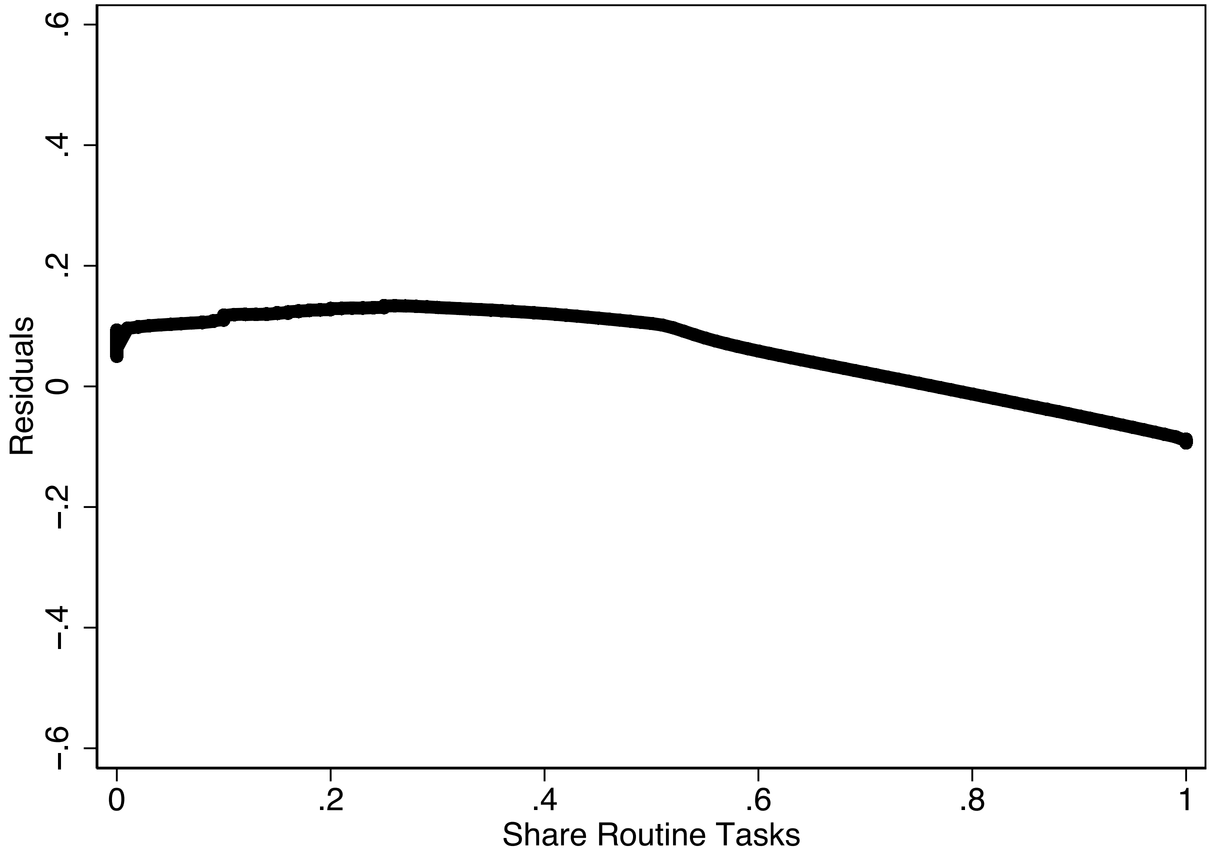


**Figure S5**. Lowess Curve of Residuals on Share Routine Tasks.
*Note*: Residuals are from a version of Model 1 without routine task share or job homogeneity controls.
*Source*: OLMS.

Figure S5 plots a lowess smoother of the residuals from a version of Model 1 without task measures against the share of routine tasks. The negative association between routine task share and earnings intensifies in the upper half of the distribution routine tasks distribution. Until 20-30% of tasks are routine, there is no negative association with a higher share of routine tasks. This pattern is consistent with a linear negative task routineness effect plus a job homogeneity effect that dampens the effect at low shares of routine tasks, but heightens it at higher shares. However, this pattern could also emerge from non-linearities in the earnings effect of routine tasks that are unrelated to the hypothesized job homogeneity mechanism.

**Table S7**. Earnings Effects of Task Routineness and Job Homogeneity

| | Only Workers with 100% Routine Tasks | | | Only Workers with 100% Complex Tasks | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Job Homogeneity | -0.177** | -0.064*** | -0.061* | -0.090 | -0.177*** | -0.173*** |
| | (0.056) | (0.017) | (0.029) | (0.084) | (0.025) | (0.023) |
| Task Quantity | 0.220*** | 0.238*** | 0.236*** | 0.107*** | 0.085*** | 0.042 |
| | (0.030) | (0.018) | (0.044) | (0.030) | (0.024) | (0.066) |
| Tenure | 0.039*** | 0.021*** | 0.020*** | 0.058*** | 0.032*** | 0.033*** |
| | (0.003) | (0.002) | (0.003) | (0.004) | (0.006) | (0.008) |
| Industry Experience | 0.008*** | 0.007*** | 0.006** | 0.022*** | 0.016*** | 0.013*** |
| | (0.002) | (0.002) | (0.002) | (0.003) | (0.002) | (0.002) |
| Year fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Union-individual-jobtitle fixed effects | | Yes | Yes | | Yes | Yes |
| City-job title-year fixed effects | | | Yes | | | Yes |
| Union-year fixed effects | | | Yes | | | Yes |
| Union-level controls | Yes | Yes | | Yes | Yes | |
| R2 | 0.265 | 0.870 | 0.913 | 0.251 | 0.827 | 0.869 |
| Within-R2 | 0.263 | 0.026 | 0.007 | 0.250 | 0.024 | 0.010 |
| Observations | 115044 | 91297 | 57058 | 128712 | 93899 | 77223 |

*Note*: Standard errors clustered at the union level. Sample size varies due to the exclusion of singleton observations from fixed-effects models. Union-year level variables are excluded from Model 3 due to union-year fixed effects.
*Source*: OLMS.
* p < .05; ** p < .01; *** p < .001 (two-tailed tests).

**Table S8**. Earnings Effects of Task Routineness and Job Homogeneity

| | Only Workers with 100% Routine Tasks | | | Only Workers with 100% Complex Tasks | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Job Homogeneity | -0.169** | -0.063*** | -0.060* | -0.093 | -0.194*** | -0.190*** |
| | (0.056) | (0.017) | (0.030) | (0.081) | (0.028) | (0.026) |
| Share Overhead Tasks | -0.019 | -0.008 | -0.002 | | | |
| | (0.025) | (0.008) | (0.019) | | | |
| Share Political Tasks | | | | -0.004 | -0.045 | -0.036 |
| | | | | (0.042) | (0.027) | (0.025) |
| Share Contributions Tasks | | | | -0.017 | -0.069 | -0.149 |
| | | | | (0.150) | (0.061) | (0.086) |
| Task Quantity | 0.221*** | 0.238*** | 0.236*** | 0.108*** | 0.085*** | 0.040 |
| | (0.030) | (0.018) | (0.044) | (0.032) | (0.024) | (0.066) |
| Tenure | 0.040*** | 0.021*** | 0.020*** | 0.058*** | 0.032*** | 0.033*** |
| | (0.003) | (0.002) | (0.003) | (0.004) | (0.006) | (0.008) |
| Industry Experience | 0.008*** | 0.007*** | 0.006** | 0.022*** | 0.016*** | 0.013*** |
| | (0.002) | (0.002) | (0.002) | (0.003) | (0.002) | (0.002) |
| Year fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Union-individual-jobtitle fixed effects | | Yes | Yes | | Yes | Yes |
| City-job title-year fixed effects | | | Yes | | | Yes |
| Union-year fixed effects | | | Yes | | | Yes |
| Union-level controls | Yes | Yes | | Yes | Yes | |
| R2 | 0.265 | 0.870 | 0.913 | 0.251 | 0.827 | 0.869 |
| Within-R2 | 0.263 | 0.026 | 0.007 | 0.250 | 0.024 | 0.010 |
| Observations | 115044 | 91297 | 57058 | 128712 | 93899 | 77223 |

*Note*: Standard errors clustered at the union level. Sample size varies due to the exclusion of singleton observations from fixed-effects models. Union-year level variables are excluded from Model 3 due to union-year fixed effects.
*Source*: OLMS.
* p < .05; ** p < .01; *** p < .001 (two-tailed tests).

To test for this issue, Tables S7 and S8 estimate effects of job homogeneity on two alternative samples: one that consists of workers who do entirely routine tasks and one that consists of workers who do entirely complex tasks. In the former sample, job homogeneity is estimated off of variation in the share of administrative relative to general overhead tasks. In the latter sample, job homogeneity effects are estimated off of variation in the share of representational, political and union contributions tasks. In Table S9, controls for these disaggregated task categories are also added. Focusing on jobs that are entirely routine or entirely complex excludes the vertical component of task variation—between routine and complex task types—from these models. In the cross-sectional model with only non-routine tasks jobs, standard errors are wide and the negative association of job homogeneity and earnings is not statistically significant. However, in all of the other models in Table S7 and Table S8, results are similar to the models estimated on the whole sample. This test provides evidence that job homogeneity has earnings effects separate from the share of routine tasks in a job.

# 3. Job Mobility Within and Between Firms: Earnings Effects of the Decline of Internal Labor Markets

## Introduction

Sociologists have long studied organizations as systems of stratification. Since the 1980s however, changing employment relations have seemed to erode the role played by organizations in shaping worker mobility and earnings. Formal career ladders and internal labor markets have been dismantled as firms rely more on external hiring than promotion from within (Cappelli 2001; Hirsch 1993) and employment tenure has declined since the 1980s (Hollister and Smith 2014). The decline of collective bargaining has loosened the legalistic rules that governed employment in unionized workplaces (Western and Rosenfeld 2011). Instead, variable compensation (Lemieux, MacLeod, and Parent 2009) and the growth of contract workers (Kalleberg 2009; Weil 2014) yokes pay more directly to labor market prices. Lower skill workers receive a declining wage premium from large employers (Cobb and Lin 2017) and middle-income workers are less likely to match with employers that pay above their fixed ability (Song et al. 2018). Perhaps in the new economy, organizations are no longer independent structures of employment stratification. They could instead be mere conduits of labor market supply and demand.

And yet, amidst this marketization and increased flexibility, firm boundaries appear increasingly impermeable. Both overall worker turnover and the rate at which employed workers switch firms have declined since in the early 1980s This decline is not explained by changing worker demographics or shifting industry composition (Molloy et al. 2016). At the same time, courts have endorsed exceptions to employment at will (Autor 2003) and equal opportunity compliance has created new processes governing employment within firms (Dobbin et al. 1993). As firm boundaries harden, rising earnings inequality has increased primarily between-firms, rather than among co-workers (Song et al. 2018). And when employers shift groups of workers across firm boundaries, as in outsourcing, workers' wages decline (Dube and Kaplan 2010). Lower paid workers are increasingly locked out of highly profitable firms, contributing to a decline in the labor share of national income (Autor et al. 2017). So external labor markets appear less active, even as the structures ordering mobility and pay in internal labor markets have eroded.

In this article, I argue that the result of these two trends has been a reconfiguration of the role of organizations in economic stratification. Employers increasingly use within-firm worker mobility not as a means of employee career advancement or human capital development, but as a source of cost flexibility. A growing share of worker mobility within organizations is likely involuntary and demanded by the employer rather than voluntary and sought after by the worker. As union work rules and career ladders have receded, employers have more freedom to restructure jobs and reallocate workers. Amidst a weakened external labor market, workers facing restructuring or demotion remain with the firm.

To test these predictions, I draw on little-used questions in the Current Population Survey, which allow a comparison of the effects of between- and within-firm job mobility. I show that while between-firm mobility has diminished, within-firm mobility has held steady. As a result, worker mobility within firms constitutes a growing share of the shrinking number of job-to-job transitions. Upward occupational mobility remains more likely in within organization moves than between organizations, but over time within organization moves have become less correlated with upward mobility, relative to between-organization moves. This decline is explained by changes in the compositional differences between workers who switch jobs within-firms and those who switch between-firms. Moreover, positive earnings effects associated with within-firm job mobility have declined. Workers still move around organizations, but a higher portion of moves appear uncompensated.

## Systems of Employment Stratification

A core contribution by organizational sociologists to the relationship between work and stratification lay in defining internal labor markets (Althauser 1989). Canonical work in institutional labor economics sought to explain why so much of worker mobility occurs within rather than between organizations (Doeringer and Piore 1971; Kerr 1977). A series of detailed empirical studies (DiPrete 1987; Osterman 1984; Femlee 1982) found that the logic governing within-firm mobility differs from between-firm job mobility. Internal labor markets provided reliable earnings increases and organized occupational advancement by logics of seniority and qualification (Dobbin et al. 1993). Studies of internal labor markets emphasize voluntary, and usually upward, mobility between jobs (Rosenfeld 1992). Indeed, much research on within-firm mobility drew on vacancy chain models, which assume that workers flow upward across some fixed set of job positions (White 1970; Chase 1991).

In contrast, research on worker mobility across firms studies both voluntary and involuntary job switches. Involuntary worker mobility, both transitioning out of employment and between employers, is associated with negative outcomes for workers (Brand 2015; Burgard, Brand, and House 2009). In the 1980s and 1990s, corporate downsizing became a popular strategy of increasing stock prices at the expense of displaced workers (Fligstein and Shin 2007; Hirsch and De Soucey 2006). Increased import competition spurred plant closures, lay-offs and reduced affected workers' earnings (Autor, Dorn, and Hanson 2013). Alongside downsizing and plant closures, many employers during this period refocused on their core competencies and outsourced other work tasks (Davis, Diekmann, and Tinsley 1994; Zorn et al. 2004). Comparisons of workers before and after outsourcing events find that outsourced workers experience slower wage growth (Dube and Kaplan 2010; Goldschmidt and Schmieder 2017).

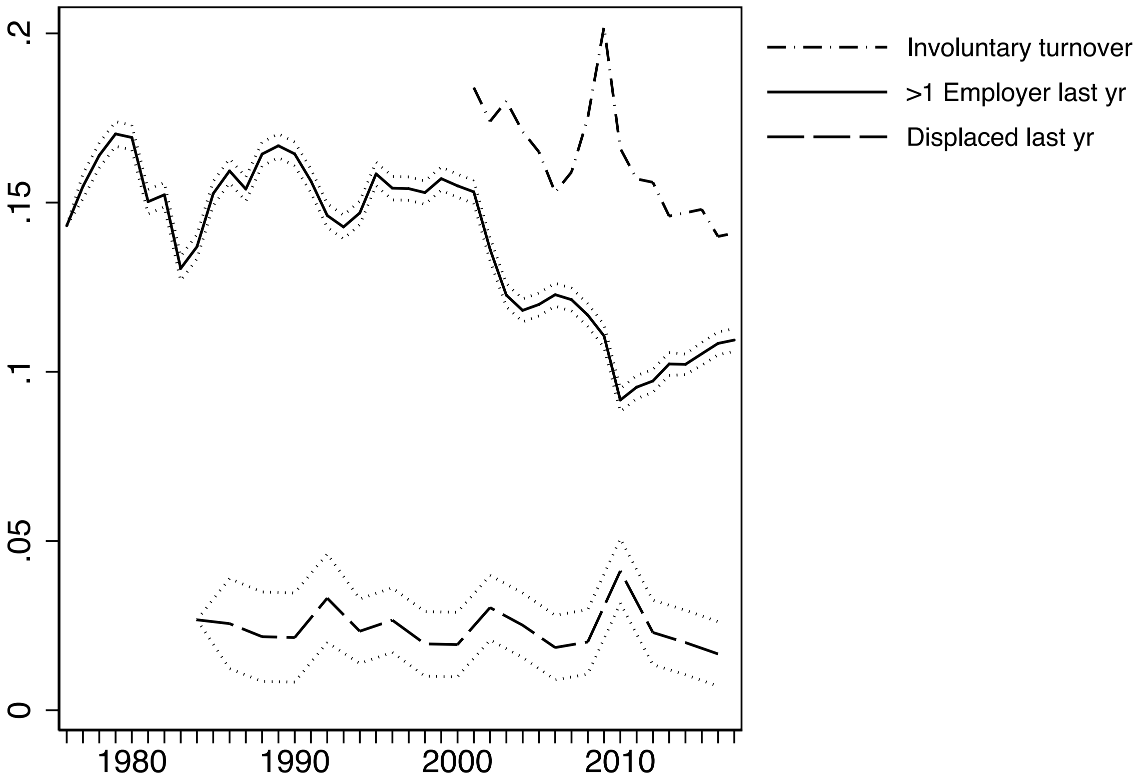


**Figure 1**. Declining Worker Mobility Across Employers.
Note: More than 1 non-simultaneous employer in previous year is from March CPS and is asked of all workers 15+ who worked in the previous year. Displaced worker status asks whether, in the last calendar year, a worker lost or left a job due to plant closure, position abolished, insufficient work or similar reason. It is asked of respondents 20+ in the CPS Displaced Worker Supplement. Involuntary turnover rate is the annualized rate of layoffs and discharges from the Job Openings and Labor Turnover Survey.
Source: March CPS; CPS Displaced Worker Supplement; Job Openings and Labor Turnover Survey.

However, as mentioned above, this period of attention to downsizing and lay-offs has coincided with overall diminished worker mobility across firms. Figure 1 shows that across several measures, between-firm job mobility is decreasing overall and involuntary between-firm job mobility appears to be either constant or decreasing. Nonetheless, workers perceive increased job instability during this period (Fullerton and Wallace 2007) and occupational mobility has risen steadily (Jarvis and Song 2017). Perhaps some of this mobility is due to a change in the nature of within-firm moves. Stable career ladders and other features of internal labor markets seem to have declined (Cappelli 2001; Hirsch 1993). Work rules that govern promotions and stabilize jobs have likely receded with the decline in collective bargaining coverage. This combination of declining worker mobility across firms and fewer structures guaranteeing upward mobility within firms suggests a new role of organizations in the stratification process. In the following section, I theorize this new role as one driving involuntary within-firm job mobility.

### Involuntary Within-firm Mobility

Despite attention to voluntary mobility within firms and the negative effects of involuntary mobility between firms, there has been less research on forced job mobility that happens within firms. Just as lay-offs displace workers across firms, workers can be exposed to involuntary job mobility within firms. I expect that demotions, job restructuring and real wage reductions increasingly characterize worker mobility within organizations. One study found that work reorganization in the context of corporate restructuring and downsizing does not increase workers' wages (Osterman 2000). In restructuring, job bumping, in which one eliminated job pushes an incumbent worker across different positions, rather than vacancy chains, governs worker mobility within the firm. More broadly, a line of research on firm flexibility characterizes lay-offs and worker flexibility as substitutes (Kalleberg 2001). Employers impose flexibility not just by varying their number of employees but also by reallocating workers across jobs within organizations. This margin of adjustment could become increasingly relevant as legal restrictions make terminating workers more difficult (Autor 2003; Bird and Knopf 2009) and as the decline of career ladders and union work rules make workers easier to shift across jobs.

Overall within-firm job mobility consists of a combination of upward and downward transitions job and, like job changing in the external labor market, it is some mix of voluntary and involuntary moves. I predict that with erosion of institutional structure within firms, within-firm job mobility will be increasingly composed of involuntary moves. I proxy for involuntary moves based on whether workers experience downward occupational mobility and whether job changes are associated with relative earnings increases or decreases:

Hypothesis 1: *Within-firm job mobility is less likely, over time, to be associated with upward occupational mobility.*

Hypothesis 2: *The earnings premium of within-firm job mobility has declined over time, relative to both within-firm job stability and between-firm mobility.*

Before testing these hypotheses, it is important to note a key puzzle that exists in hypothesizing involuntary mobility within firms that does not affect forced between-firms mobility. Workers are always free to quit their job, so in a setting where workers are paid no more than their outside option, shifting from a better or higher paid job to a worse and lower paid job will often lead a worker to quit. However, recent research provides several reasons to think that some workers would assent to involuntary within-firm movement. Across multiple industries, non-compete agreements are widespread and make it difficult for workers to switch employers (Marx 2011; Starr, Prescott, and Bishara 2017). Recent studies also provide suggestive evidence that employer concentration in local labor markets contributes to lower wages (Azar, Marinescu, and Steinbaum 2017; Benmelech, Bergman, and Kim 2018). In a labor market with substantial employer concentration, employed workers may concede to undesired job changes for lack of a better outside option. Research on job-lock and the employer-provided health insurance system provides more reason for workers not to leave their employers (Madrian 1994; Garthwaite, Gross, and Notowidigdo 2014). More generally, if workers accumulate seniority at a given firm, they may be unwilling to leave their current employer, even if it means taking a less desirable job.

## Data and variables

The Current Population Survey (CPS) is a large labor force survey conducted by the Census and the Bureau of Labor Statistics. I draw monthly CPS data from IPUMS-CPS, supplemented with additional variables from the NBER basic monthly CPS files.

The CPS is structured as a monthly panel in which a household is surveyed for each of 4 initial months; is given 1 year off; and is then sampled for a final 4 months. Questions about job mobility are not asked in the 1st or 5th months and are only asked of respondents employed in the prior month. In the following analysis, I limit the sample to workers' responses in months 2, 3, 4, 6, 7 and 8 and include only workers who are currently employed and were employed in the preceding month. In the earnings analysis, I restrict to months 4 and 8, limiting the sample to the outgoing rotation group that is asked earnings questions.

Starting in 1994, the CPS included questions about job mobility in an effort to tamp down on substantial month-to-month variation in industry and occupation categorization (Polivka and Rothgeb 1993). While these questions are structured around determining whether to ask occupation and industry questions, they allow a little-used opportunity to compare within- and between-firm job mobility (Moscarini and Thomsson 2007). The following three questions are asked in sequence and provide the core variables for the analysis:

(1) "Last month, it was reported that you worked for (Employer's name). Do you still work for (Employer's name)?"
YES→ Next question
NO → Skip to occupation and industry questions

(2) "Have the usual activities and duties of your job changed since last month?"
YES→ Skip to occupation and industry questions
NO → Next question

(3) "Last month you were reported as (a/an Occupation) and your usual activities were (Description). Is this an accurate description of your current job?"
YES→ Use last month's industry/occupation codes
NO → To occupation and industry questions

The first two questions are simple to code: (1)No indicates a between-employer job switch in the last month, while (2)Yes indicates a within-employer job switch in the last month.

However, the third question is more ambiguous (Kambourov and Manovskii 2013). In cases of (3)No, there are two possibilities: (a) the respondent initially forgot about a job change, but upon being reminded of his/her job activities in the previous month, remembered a change; (b) the respondent had no job change, but retrospectively disagrees with the job description recorded in the prior survey. Ideally, instances of (3)No(a) would be coded as job changes, alongside (2)Yes, but we would exclude (3)No(b). The most likely reason for (3)No(b) is that a different household member is responding to the survey than in the month immediately prior (Kambourov and Manovskii 2013). These month-to-month respondent-switchers account for 19% of overall months, but 28% of (3)No months. To avoid including these retrospective disagreements as job changes, I code as job switchers only instances of (3)No when the same respondent answers in the prior month. I expect this approach to reduce measurement error, although there could still be some (3)No(a) instances of respondents who regret an inaccurate prior description of their job.

While these questions were first asked in 1994, they were not asked in January of that year, and the first several months of questioning saw very high affirmative responses that declined within the first few months (see Figure A in Appendix). I interpret this rapid decay as an artifact of the new survey roll-out, so I begin the analysis in 1995.

**Table 1**. Sample Restrictions and Rates of Job Switching, 1995-2017

| | Yes | No | Blank/not asked | Refused | Remaining eligible |
|---|---|---|---|---|---|
| Employed? | 17,461,604 | 11,378,444 | 7,557,900 | | 17,461,604 |
| Not Months 1 or 5? | 13,154,464 | 4,307,140 | | | 13,154,464 |
| Employed T-1? | 11,775,492 | 498,516 | 880,456 | | 11,775,492 |
| Same Employer? | 10,979,679 | 270,025 | 520,589 | 5,199 | 10,979,679 |
| Switch Job? | 116,631 | 10,750,420 | 108,742 | 3,886 | 10,863,048 |
| Same Respondent as T-1? | 8,848,309 | 2,014,739 | | | 8,941,157 |
| T-1 Description accurate? | 8,658,586 | 85,751 | 195,310 | 1,564 | |
| Non-missing, if eligible, across all questions | | | | | 11,114,911 |

| | Switch Employer | Switch Job (1) | Switch Job (2) | Switch Job (1, 2) |
|---|---|---|---|---|
| Share of all answering "Same Employer?" | 0.024 | 0.010 | 0.008 | 0.018 |
| Share of non-missing, but eligible, across all questions | 0.024 | 0.010 | 0.008 | 0.018 |

*Note*: Remaining eligible indicates number of respondent-months remaining in the sample after each restriction, described in the text.
*Source*: CPS Basic Monthly.

Finally, around 5% of the sample are missing job change responses on at least one of the questions. I follow prior studies and delete cases with missing those missing values (Fallick and Fleischman 2004). Table 1 summarizes the exclusions that define the analytic sample and shows that monthly mobility rates are invariant to using a denominator of all workers asked the employer continuity question (Question 1 above) or a denominator that includes any workers with missing data on a question they would be eligible for based on the questionnaire guidelines.

I use two dependent variables, individual earnings and occupational rank, to measure the outcome of these different types of job mobility. For earnings, I use the IPUMS CPS usual weekly earnings variable from the Outgoing Rotation Group sample, which includes weekly pay for respondents who report being paid on a weekly basis and for hourly workers includes hourly wages multiplied by their usual number of hours worked per week. From 1995 to 1997, these earnings are top-coded at $1923, and from 1998 onward, at $2885. Following Autor, Manning and Smith (2016) I windsorize wages at and above these topcodes and multiply the topcode values by 1.5. I then deflate to 2000 dollars and log. To calculate occupational rank, I average earnings across each of 384 IPUMS 1990 occupational categories.

**Table 2**: Timing Sequence for Example Respondent

| Month in sample | 1 | 2 | 3 | 4 | (1 yr gap) | 5 | 6 | 7 | 8 |
|---|---|---|---|---|---|---|---|---|---|
| Weekly earnings | N/A | N/A | N/A | $640 | | N/A | N/A | N/A | $700 |
| Occupation? | Nurse | Manager | Manager | Manager | | Manager | Nurse | Nurse | Nurse |
| Employer switch? | N/A | Yes | No | No | | N/A | No | No | No |
| Within-firm switch? | N/A | No | No | No | | N/A | Yes | No | No |
| Post-employer switch? | | | | Yes | | | | | Yes |
| Post-within-firm switch? | | | | No | | | | | Yes |

*Note*: Only months 4 and 8 are included in earnings analysis. Months 2, 3, 4, 6, 7 and 8 are included in analysis of occupation rank.

Occupation is available in each month of data (although, as discussed above, it is only asked conditional on the job switching questions given above). However, earnings are only available in the outgoing rotation group, or respondents in the 4th and 8th interview months. Table 2 describes this timing sequence for an example respondent. For the occupational rank analysis, months 2, 3, 4, 6, 7 and 8 would be included. This respondent would provide examples of upward mobility during a between-firm switch (in Month 2) and downward mobility during a within-firm switch (in Month 6).

In contrast, for the earnings analysis, only months 4 and 8 can be used. Due to this restricted set of months, I code any months following a job switch as job switch months. In the Table 2 example, both months 4 and 8 are after the employer switch. So, this respondent does not have any variation in employer switching that can contribute to the earnings model. Only switches that take place after the initial earnings observation (in months 6, 7 or 8) can help predict earnings. Note that in this design, the 1 year gap between the first and second set of survey months introduces measurement error: job changes that happen during that period are unobserved. Based on the rates of workers who switch over a 3 month period, in one year around 8% of non-within-firm switchers are wrongly categorized and around 11% of non-between-firm switchers. This misclassification will tend to attenuate estimates of earnings changes associated with job changing. However, the key predictions given above relate to changes in earnings effects over time. Misclassification is likely to be invariant over time, as the survey design sequence does not change during this period.

I include several demographic controls for worker composition in the models. Education is categorized as less than high school, high school or finished 12$^{th}$ grade; some college or less than 4 years of college; a bachelors degree; or more than 4 years of college or a graduate degree. I control for 4 categories of race and ethnicity: non-Hispanic white, non-Hispanic black, Hispanic and other. Age is divided into 6 categories: less than 21; 21 to 29; 30 to 39; 40 to 49; 50 to 59; and above 60. I also include controls for part-time/full-time worker status. In the earnings regressions, I control for union membership or collective bargaining coverage, which is only available for the Outgoing Rotation Group surveys. I weight descriptive statistics and the occupational mobility analyses using the CPS Basic Monthly weights and the earnings models using the Outgoing Rotation Group weights.

## Trends in Within- and Between-firm Job Mobility

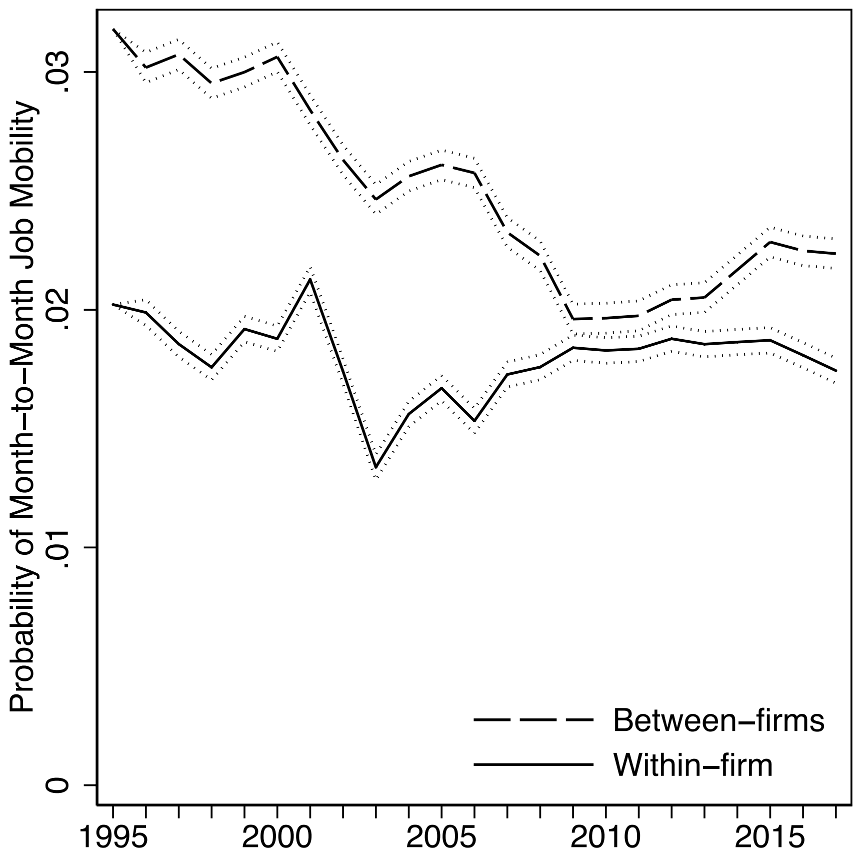


**Figure 2**: Trends in Monthly Job Mobility, Between- and Within-firms.
Note: Within-firm job mobility is defined as a worker either (1) answering "Yes" to "Have the usual activities and duties of your job changed since last month?" or (2) being the same respondent in the prior month and answering "No" to "Last month you were reported as (a/an Occupation) and your usual activities were (Description). Is this an accurate description of your current job?".
Source: CPS Basic Monthly.

Figure 2 shows changes in the monthly probability of job mobility within and between employers. Since at least the mid-1990s, between-firm job mobility has declined, consistent with findings from firm-level and linked worker-firm data (Davis and Haltiwanger 2014; Hyatt and Spletzer 2013). Initially, within-firm job mobility also declined. However, from 2003, within-firm job mobility slowly increased and then stabilized, while between-firm mobility continued to decline until 2009.[22] The most recent years indicate an uptick in between-firm mobility.

These trends in within-firm job mobility suggest that job moves are not shifting increasingly out of organizational bounds into the open labor market. On the contrary, since the mid-1990s, within-firm job mobility has only declined slightly, while between-firm mobility has declined by almost one third. The net result is that a higher share of job-to-job moves take place within the firm than at the beginning of the period.

[22] There is a sharp decline in within-firm job mobility from 2001 to 2003 (around 0.04). This decline holds within gender, age, education and month-in-sample groups and in most broad industries, occupations and geographical Census regions. It holds with and without sample weights. The drop is entirely driven by an increase in the number of "No" responses to question (2) (which asks whether work activities have changed) and decreases in the number of "Yes" and missing responses. (Taking the proportion of job switchers relative to missing and "No" responses reduces the decline to 0.033.) Responses to question (3) do not change. I can find no evidence that the question wording changed during that year, but I am worried that this drop is an artifact of data collection changes.

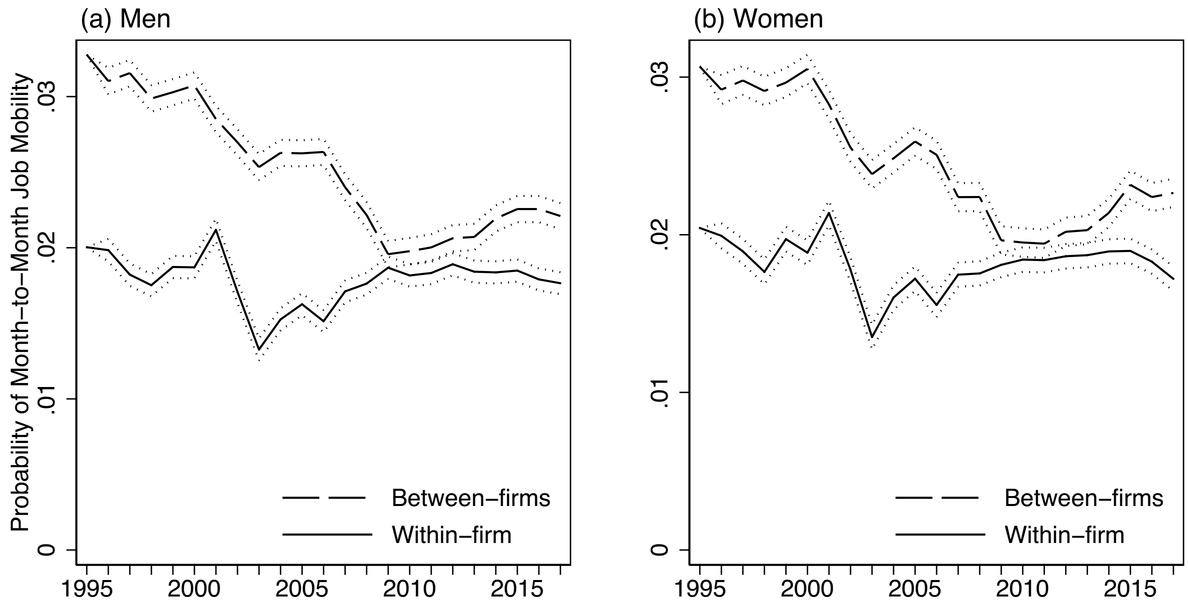


**Figure 3**: Trends in Monthly Job Mobility, Between- and Within-firms (by Gender).
Note: Within-firm job mobility is defined as in Figure 1.
Source: CPS Basic Monthly.

Figure 3 shows that this pattern has been consistent across male and female workers. Figure 4 shows between-firm mobility is highest for young workers and young workers have experienced the sharpest decline in between-firm mobility. However, lower rates of between-firm mobility appear across all age groups. On the other hand, within-firm job mobility has remained roughly constant across age groups. Figure 5 breaks these patterns out across education groups. Workers with less than a high school degree experience the most between-firm mobility, but again within-firm mobility is little changed across education groups or over time.

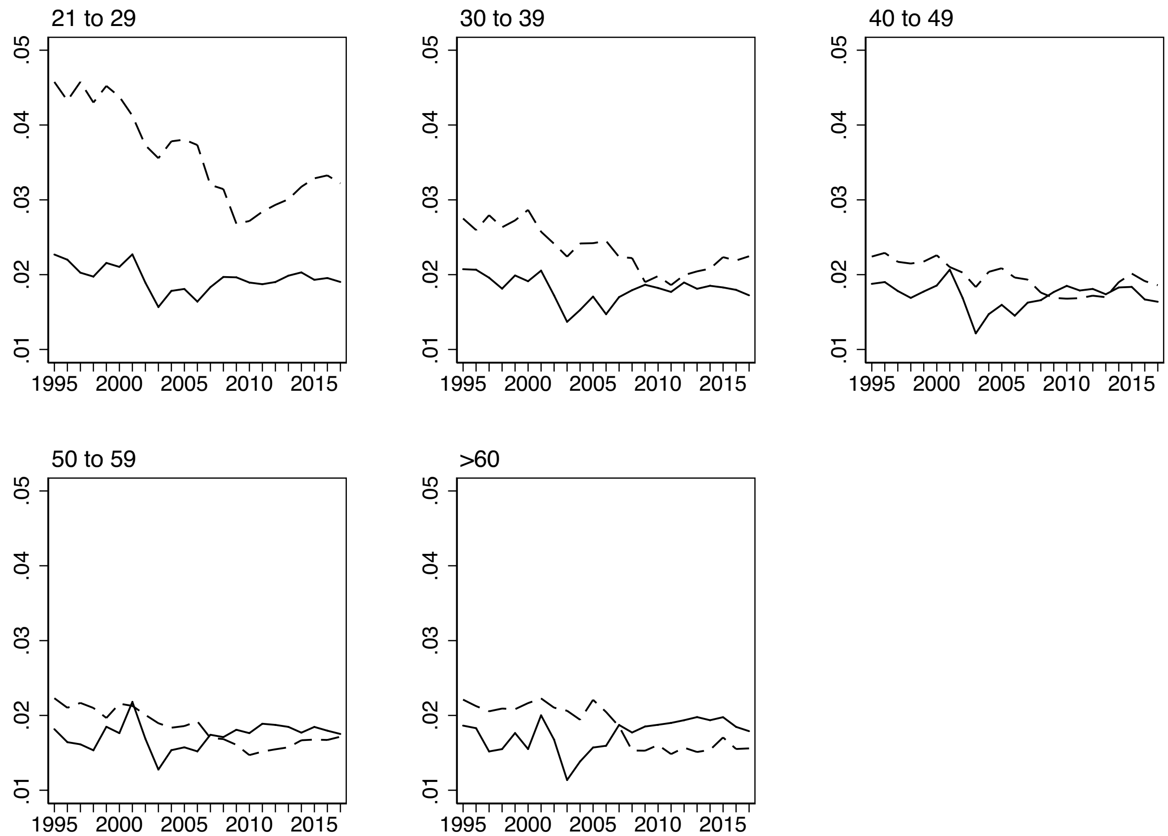


**Figure 4**: Trends in Monthly Job Mobility, Between- and Within-firms (by Age).
Note: Within-firm job mobility is defined as in Figure 1.
Source: CPS Basic Monthly.

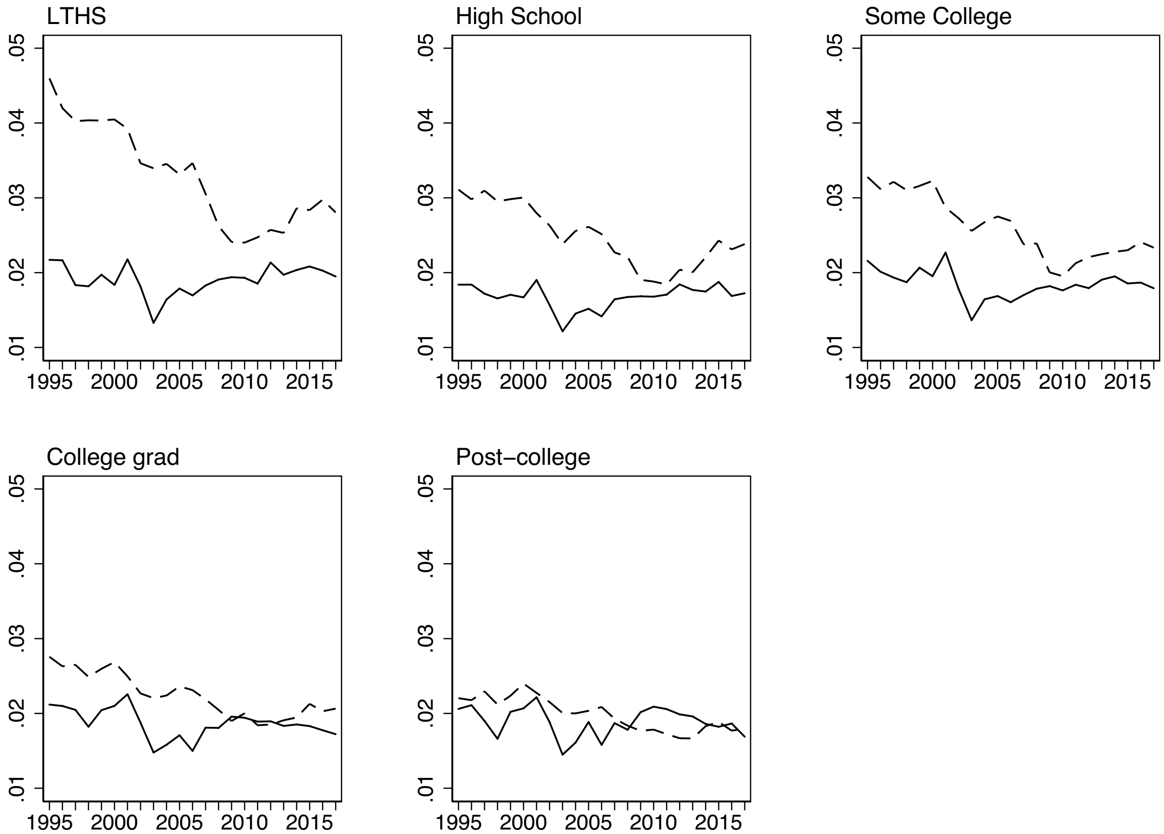


**Figure 5**: Trends in Monthly Job Mobility, Between- and Within-firms (by Education).
Note: Within-firm job mobility is defined as in Figure 1.
Source: CPS Basic Monthly.


**Figure 6**: Probability Job Change is Within-firm vs. Between-firms, by Origin and Destination Occupations Before and After 2006.
Note: In (a) occupation transitions are ranked by average earnings; in (b) they are ranked by size of share change across within- and between-firm job changes before and after 2006.
Occupational categories are defined using the task-based framework in as Managerial

(nonroutine cognitive: managerial and professional); Clerical (routine cognitive: clerical and sales); Production (routine manual: construction, manufacturing workers, drivers, farm workers, and miners); Services (nonroutine manual: services, private household employees and protective occupations).
Source: CPS Basic Monthly.

Next, I ask which occupational transitions have become relatively more common in within-firm transitions rather than between-firm transitions. Within-firm job mobility can be either voluntary—as in promotions up a job ladder—or involuntary—as in reassignment in the midst of corporate restructuring. The erosion of internal labor markets could be offset by an increase in involuntary within-firm job changes. Figure 6 plots the probability that each of 12 types of transitions between broad occupational groups occurs within the firm rather than between-firms. By comparing within- and between-firm mobility, this approach nets out changes due directly to occupation composition changes.

Figure 6 shows that transitions more likely to occur within firms tend to be higher-earning jobs, like promotions from production and clerical jobs into managerial positions. Lower paid transitions, like those in which workers shift from production or clerical positions into service occupations, are less likely to occur within firms. Comparing probabilities from the first half of the period (before 2006) to the second half, there has been a broad increase in the likelihood of transition occurring within firms rather than between firms (consistent with the trends in Figure 2).

The second chart in Figure 6 re-orders the occupational transitions based on which transitions experienced the biggest shift in shares toward within-firm job mobility. This increase in the share of within-firm job mobility has been spread across the occupational distribution, but transitions within occupational groups had the largest share increases. Moreover, apparent demotions, from managerial to clerical or production jobs, became increasingly the province of within-, rather than between-firm job switching. In contrast, transitions into managerial positions experienced lower increases in the within-firm mobility share. These descriptive patterns are consistent with a shift from upwardly oriented internal labor markets to job instability and involuntary worker mobility within firms.

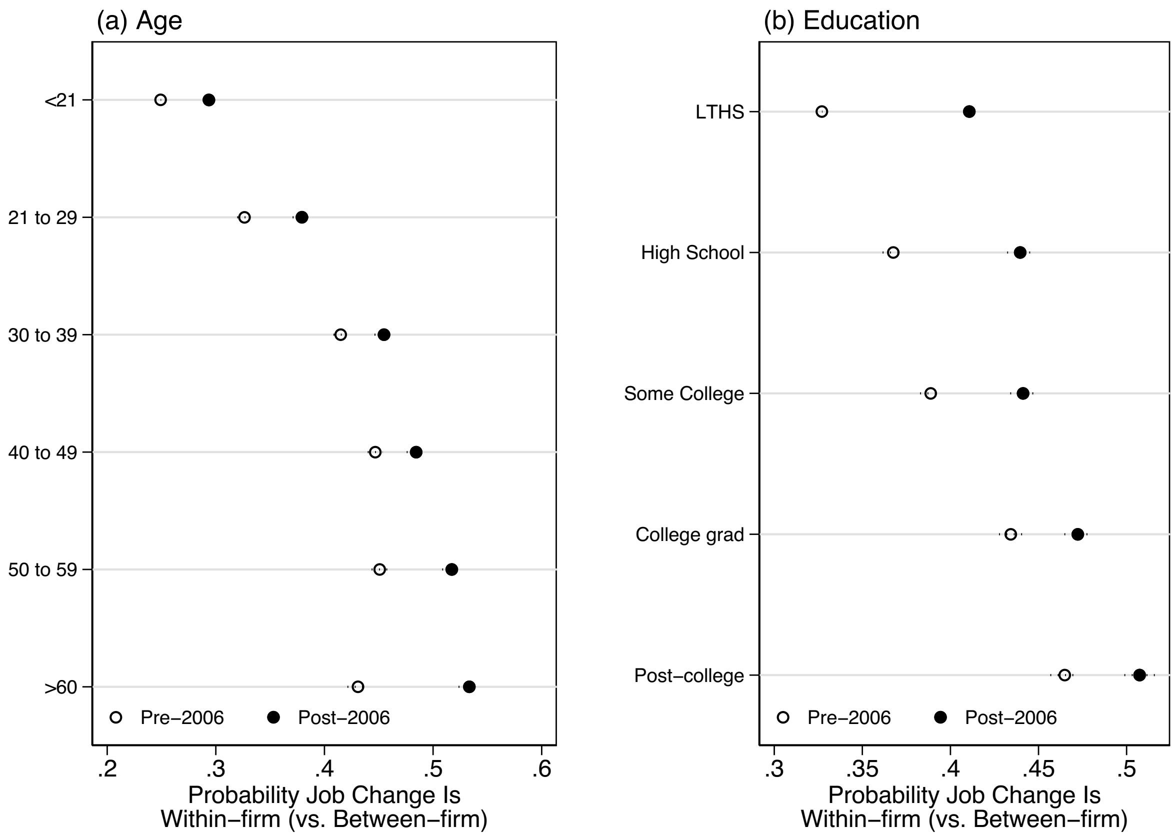


**Figure 7**: Likelihood Job Change is Within-firm vs. Between-firms, by Age and Education Before and After 2006.
Source: CPS Basic Monthly.

Figure 7 shows job mobility by education and age groups in terms of this proportion of moves occurring inside and outside of the firm. Notwithstanding the sharp decline in mobility among young workers, the largest increase in the share of mobility within-firms has been among older and less educated workers. Less educated workers have also experienced wage stagnation during this period, which could be consistent with decreased access to external job market opportunities.

## Methods

I proceed in two steps. First, I fit a model comparing the occupational achievement of workers who switch jobs and those who switch employers. I predict logged $s_{it}$ average occupational earnings for the occupation of worker i in time t:

$$\log(s_{it}) = \beta_1 x_{it} + \beta_2 w_{it} + \beta_3(x_{it} * d_t) + \beta_4(w_{it} * d_t) +$$
$$\beta_5 v'_{it} + \beta_6(x_{it} * v'_{it}) + \beta_7(w_{it} * v'_{it}) + s_{it-1} + \alpha_i + d_t + e_{it}.$$

where within-firm mobility $x_{it}$ and between-firm mobility $w_{it}$ are both interacted with a time trend $d_t$. I include a vector $v'_{it}$ of demographic controls, to account for shifting exposure over time to job mobility of groups that tend to experience more or less occupational mobility. I also interact these controls with within-firm and between-firm mobility, to account for some groups experiencing more occupational upgrading through between- or within-firm mobility. Finally, I include lagged occupational earnings, to account for the earnings level of the occupation from which a given worker is moving. Worker fixed effects $\alpha_i$ remove time-invariant earnings differences.

Next, I estimate a series of earnings regressions of a similar form, replacing occupational earnings with individual worker earnings and restricting the sample to months 4 and 8 (as described above). I add controls for union membership, available only in the outgoing rotation group sample, and for 18 broad occupation groups. I exclude the lagged control for this 2-period panel.

## Mobility Effects

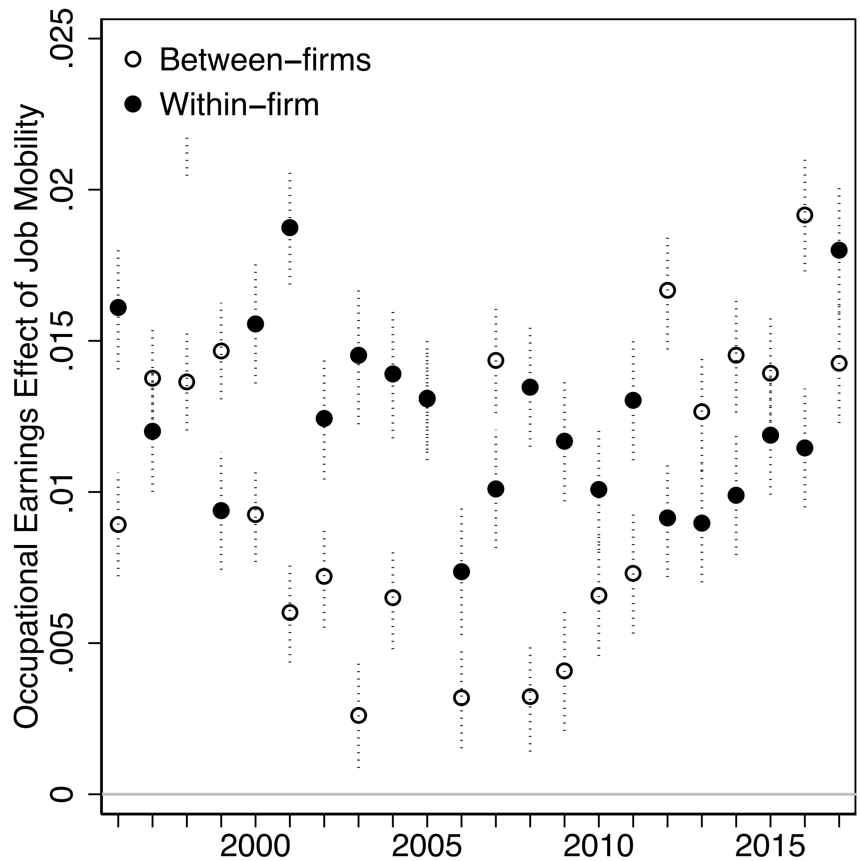


**Figure 8**. Change in Occupation Associated with Job Mobility, Between- and Within-firms. Source: CPS Basic Monthly.

Figure 8 shows the average occupational mobility, in logged average occupational earnings, for within- and between-firm job switchers over time, relative to workers who do not change jobs. There is little clear trend in between-firm mobility occupation effects. But, within-firm job occupational benefits of job mobility seem to have been declining until 2014. But, since 2014, the occupation effects of within-firm mobility seem to have increased again.

**Table 3**. Occupational Rank Effects of Between- and Within-firm Job Mobility Over Time

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Within-firm Mobility | 0.016*** | 0.016*** | 0.003*** | -0.004*** |
| | (0.000) | (0.000) | (0.001) | (0.001) |
| Within-firm Mobility * Time | -0.002*** | -0.002*** | -0.000 | 0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) |
| Between-firm Mobility | 0.010*** | 0.009*** | -0.002*** | -0.005*** |
| | (0.000) | (0.000) | (0.001) | (0.001) |
| Between-firm Mobility * Time | 0.002*** | 0.002*** | 0.003*** | 0.003*** |
| | (0.000) | (0.000) | (0.000) | (0.000) |
| Time | 0.005*** | 0.002** | 0.002*** | 0.002*** |
| | (0.000) | (0.001) | (0.001) | (0.001) |
| Individual fixed effects | Yes | Yes | Yes | Yes |
| Controls | | Yes | Yes | Yes |
| AgeXMobility | | | Yes | Yes |
| Education/Sex/RaceXMobility | | | | Yes |
| R2 | 0.980 | 0.980 | 0.980 | 0.980 |
| Observations | 11116063 | 11116063 | 11116063 | 11116063 |

*Note*: Standard errors are in parentheses. Models predict logged average earnings of occupation. Observations are individual-year pairs. Controls include age, education, race, part-time/full-time status and industry (18 categories). The main effects are at 1995 and the time trend is in decades (year*10) for interpretability.
*Source*: CPS Monthly and CPS ORG.
* p < .05; ** p < .01; *** p < .001 (two-tailed tests).

Table 3 models these patterns with a time trend. Model 1 shows that over time, the mobility within firms has become less positive, while mobility between firms has become more positive. Model 2 adds demographic controls, which do little to affect the trend. Model 3 interacts age indicators with mobility, to account for some demographic groups benefiting more from within-firm mobility than others. Adding these controls eliminates the downward time trend in upward mobility for within-firm job changers.

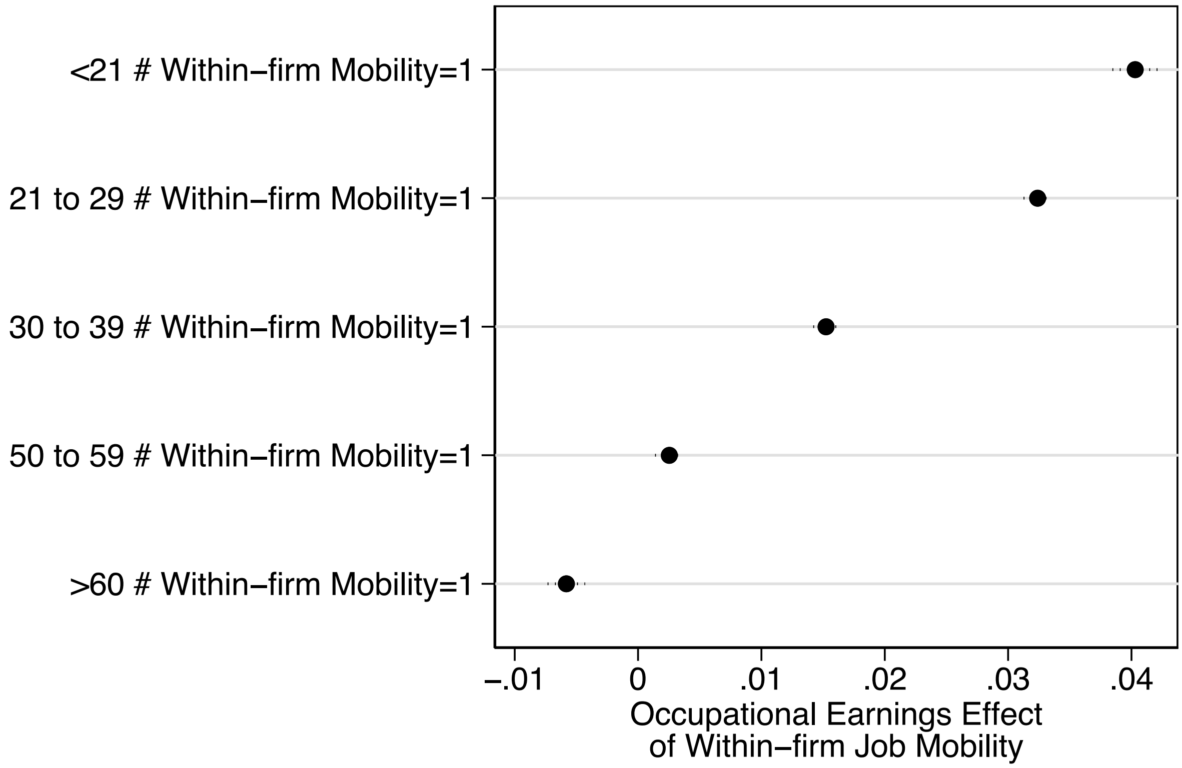


**Figure 9**. Change in Occupation with Job Mobility, by Age.
Note: Coefficients drawn from Model 2 in Table 3. Reference group is workers 40 to 49 years old.
Source: CPS Basic Monthly.

Figure 9 charts the age by within-firm mobility interactions from Model 3: within-firm job switching is more positive for younger workers than older workers. As such, the increased share of older workers experiencing job switching explains the decline in the positive occupational attainment effects of within-firm mobility. Model 4 adds interactions with education, sex and race. Results are similar, suggesting that shifting composition of education, sex and race among within-firm job changers does not explain shifting occupational rewards by mobility over time.

## Earnings Effects

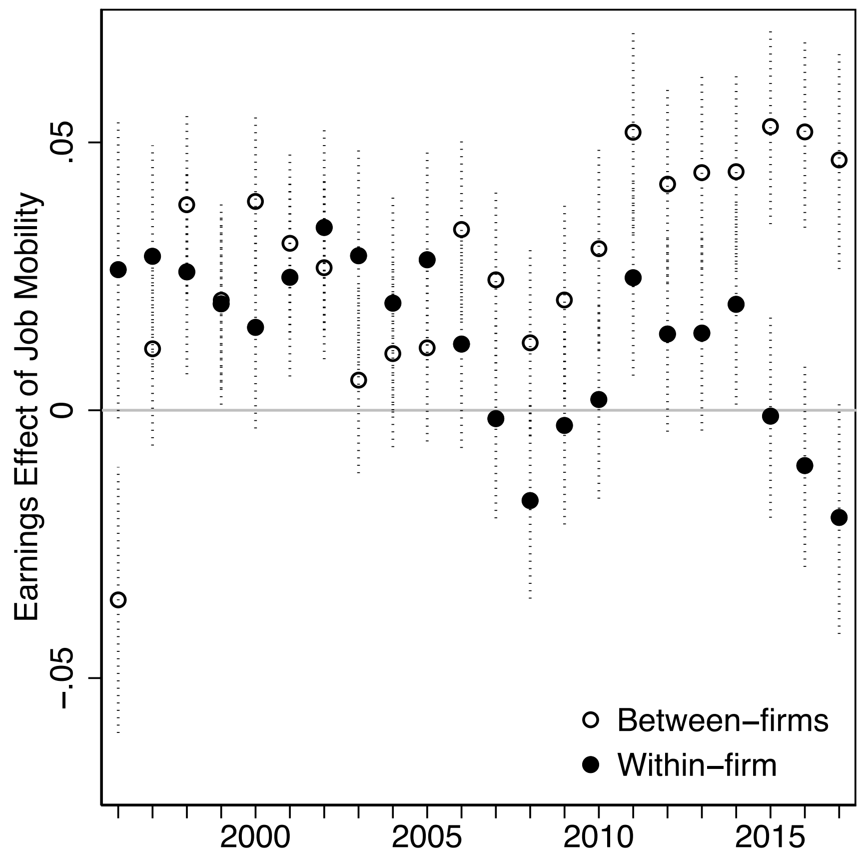


**Figure 10**: Earnings Effect of Job Mobility, Between- and Within-firms.
Note: Baseline model.
Source: CPS ORG.

Figure 10 shows the earnings change associated with between- and within-firm job changes for each year from 1995 to 2017. These changes are conditional on year dummies, so they show the deviation in earnings associated with switchers relative to average earnings changes for workers who remain in the same job. The earnings change associated with within-firm mobility was positive in the beginning of the period, but shrank steadily from 1995 to 2008. During the Great Recession, earnings changes appeared to be slightly more positive. But since the recovery, the downward trend has continued. In contrast, the earnings effects of between-firm moves have been consistently positive and even increased during this period. These patterns suggest that an increasing share of within-firm moves are associated with relative real wage penalties. Rather than climbing a career ladder within a protected internal labor market, within-firm moves are likely evidence of restructuring and the fragility of workers' jobs.

**Table 4**. Earnings Effects of Between- and Within-firm Job Mobility Over Time

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Within-firm Mobility | 0.040*** | 0.035*** | -0.022* | 0.008 | 0.016 |
| | (0.007) | (0.007) | (0.011) | (0.013) | (0.015) |
| Within-firm Mobility * Time | -0.023*** | -0.021*** | -0.015** | -0.015** | -0.014** |
| | (0.005) | (0.005) | (0.005) | (0.005) | (0.005) |
| Between-firm Mobility | 0.017** | 0.009 | -0.064*** | -0.029** | -0.003 |
| | (0.006) | (0.006) | (0.011) | (0.011) | (0.013) |
| Between-firm Mobility * Time | 0.012** | 0.008 | 0.015*** | 0.015*** | 0.015*** |
| | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) |
| Time | 0.315*** | 0.230*** | 0.230*** | 0.230*** | 0.229*** |
| | (0.006) | (0.006) | (0.006) | (0.006) | (0.006) |
| Individual fixed effects | Yes | Yes | Yes | Yes | Yes |
| Controls | | Yes | Yes | Yes | Yes |
| AgeXMobility | | | Yes | Yes | Yes |
| Education/Sex/RaceXMobility | | | | Yes | Yes |
| PT/Union/Occ.XMobility | | | | | Yes |
| R2 | 0.908 | 0.912 | 0.912 | 0.912 | 0.912 |
| Observations | 3860783 | 3860783 | 3860783 | 3860783 | 3860783 |

*Note*: Standard errors are in parentheses. Observations are individual-year pairs. Controls include age, education, race, part-time status, union membership or coverage status, industry (18 categories) and occupation (18 categories). The main effects are at 1995 and the time trend is in decades (year*10) for interpretability.
*Source*: CPS Monthly and CPS ORG.
* p < .05; ** p < .01; *** p < .001 (two-tailed tests).

However, these year-by-year earnings effects have wide confidence intervals and could be driven by compositional changes in workers switching jobs. Table 4 shows the main earnings findings, focusing on the trend in earnings associations for between- and within-firm switchers. Consistent with Figure 9, Model 1 shows that while in 1995 within-firm job changing was associated with a 4% increase in earnings, within 2 decades this earnings premium was erased. Model 2 includes controls for education, age, sex and race. There is little change in the rate at which the within-firm mobility premium shrinks. This suggests that the decline is not due to workers who experience lower earnings increases based on observed characteristics increasingly selecting into within-firm mobility. Model 3 adds interactions between age and within-firm mobility. This control begins to relax the assumption that all groups respond similarly to within-firm moves.  In Model 3, the rate of decrease in the within-firm mobility premium shrinks by around a quarter.

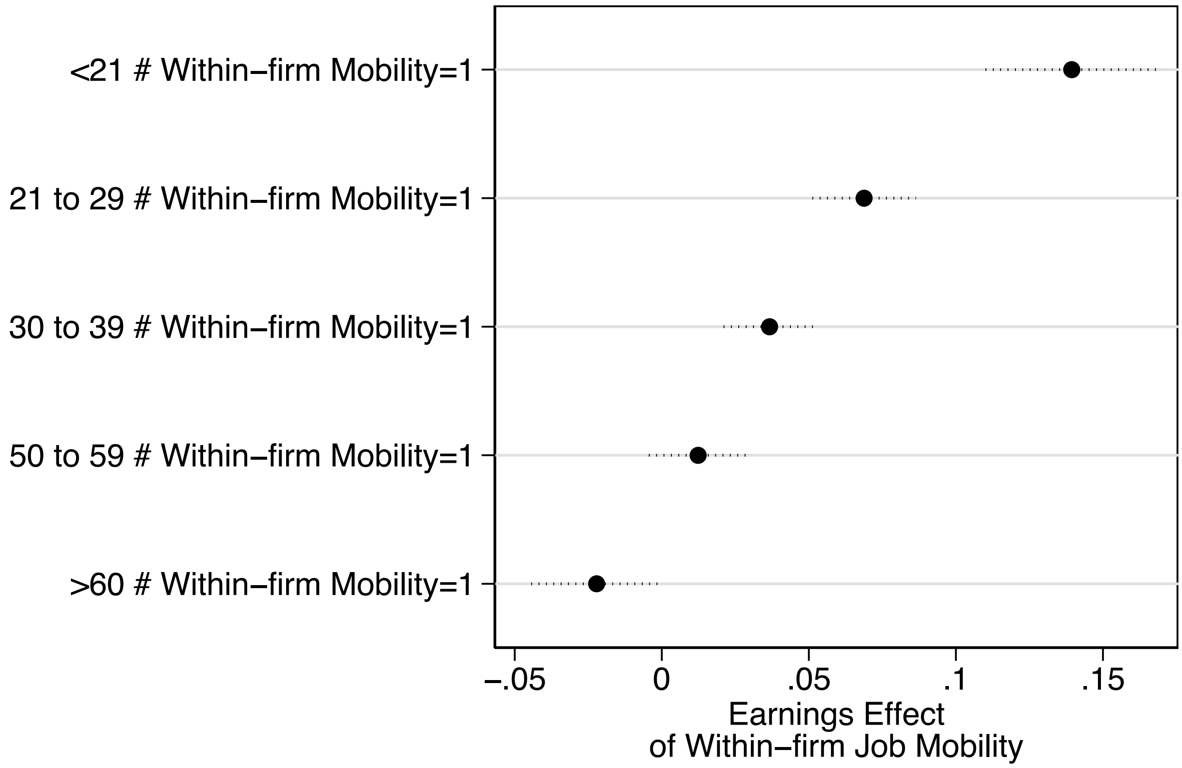


**Figure 11**: Change in Earnings with Job Mobility, by Age.
Note: Coefficients drawn from Model 3 in Table 4. Reference group is workers 40 to 49 years old.
Source: CPS ORG.

Figure 11 shows that younger workers experience a larger earnings premium for within-firm mobility, but during this period older workers become relatively more likely to experience within-firm mobility. This shifting age composition explains around one quarter of the earnings premium trend of within-firm mobility.

In Model 4, similar controls for mobility interactions with education, race and gender do not affect the rate of earnings premium decline. Likewise, in Model 5, adding part-time/full-time status and union membership interactions leave the rate of within-firm jobs earning premium decline essentially unchanged.

## Discussion

The restructuring of employment relations since the 1970s has not produced an economy of high turnover and casual attachment of workers to firms. Job changes within organizations have taken up a growing share overall job changes, as opportunities to switch jobs across firms have dwindled. However, rather than union rules and job ladders restricting within-firm moves into an upward career trajectory, job movement within firms appears increasingly haphazard. Over time, workers experience less increase in occupational status or earnings when they experience within-firm job changes. Part of this decline in earnings increases is due to a change in demographic composition of within-firm movers: increasingly it is workers less likely to benefit from job changes, those who are older, that experience job changes within a firm rather than between-firms.

Organizations remain important for structuring job mobility. However, rather than shielding workers from the labor market, the way they allocate jobs transmits flexibility for the firm into wages and work assignment. These findings suggest that our theories of declining job stability might look less to between-firm mobility, but instead at the haphazard way in which workers are pushed this way and that within firms. The results also provide indirect evidence in support of recent research suggesting some level of monopsony power in the labor market (Manning 2003): a growing portion of worker transitions within firms appear to be involuntary. An older tradition of social science research addressed issues of labor market power (Gaventa 1980; Reynolds 1951). Future sociological research should return to these themes.

In subsequent research, I plan to do more analysis exploring the shifting age structure of job mobility. The findings shows that changing age composition influences the changing effects of within-firm job mobility on both earnings and occupational standing. For both earnings and occupational standing, younger workers gain more than older workers from within-firm moves. But increasingly it is holder workers who experience such moves. Part of this shift could be due to the overall aging of the workforce since the early 1990s. Part of it could be increasing job insecurity among older workers, perhaps due to their concentration in particular industries and occupations.

Another area for future research lies in tying the wage and occupation mobility effects of within-firm moves more directly to potential sources of job lock. Several of these sources can be tested using other data collected through the CPS. The March Annual Social and Economic Supplement of the CPS includes questions about employer-provided health insurance and retirement benefit participation. I expect that workers receiving these benefits are more likely to remain with an employer notwithstanding wage stagnation or demotion. Another path forward is by bringing in data on tenure from the January and February job tenure supplements: high tenure workers should be less likely to exit upon experiencing undesired within-firm job mobility. Finally, prior research has used state-level variation in the enforcement of non-compete contracts to assess their effects (Samila and Sorenson 2011). While there is no individual-level data on non-competes on the CPS, this state-level design could be used.

Finally, I aim to tie these individual-level analyses more directly to broader changes in the wage structure during this period. While so far I have focused on mean changes in earnings and occupational attainment, job mobility could also be associated with increased variance over time. I could also quantify the contribution of a declining pay-off to within-firm job mobility to wage stagnation during this period.

Appendix A

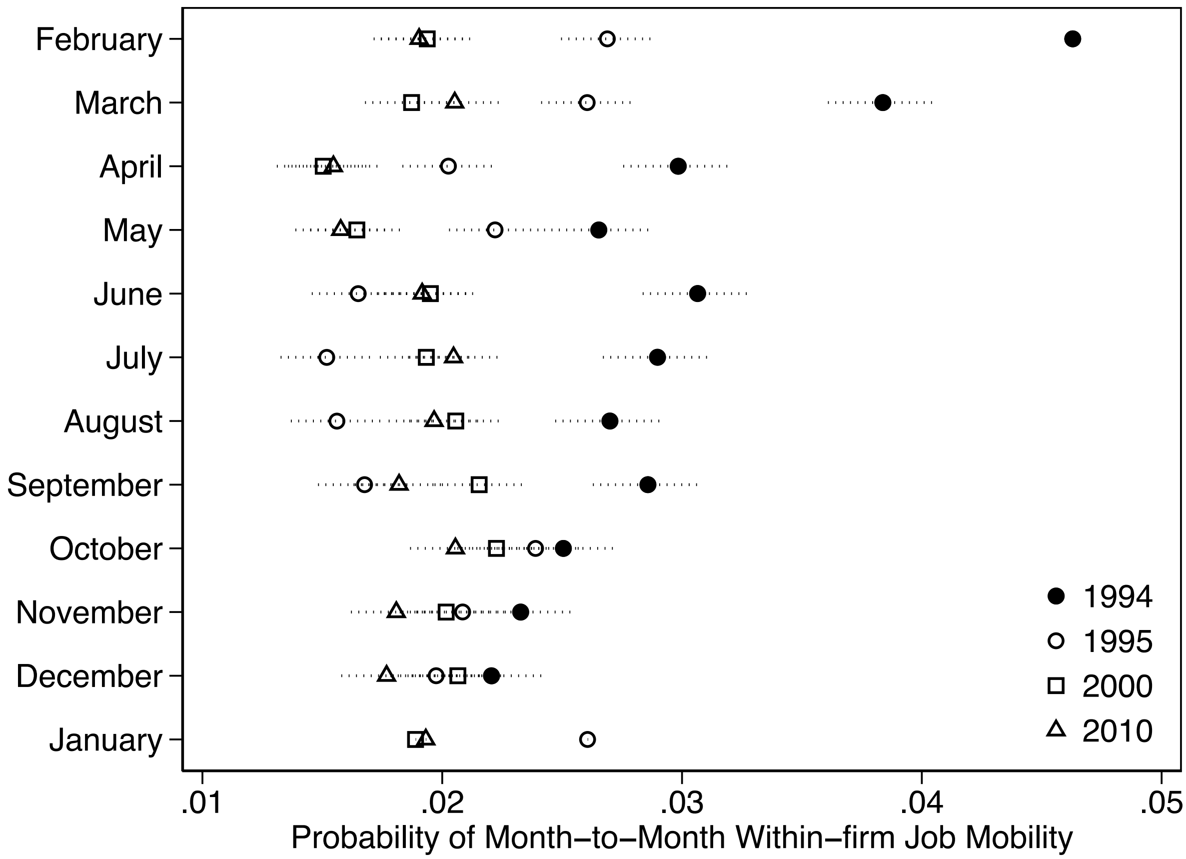


**Figure A1**. Monthly within-firm job mobility rates, 1994, 1995, 2000 and 2010.

## 4. Conclusion

Increased inequality and wage stagnation since the 1970s stem in part from a changing organizational and product market context. In this dissertation, I consider the effects on earnings of changes in the character of within-firm mobility; increased distillation of tasks across jobs; and workers' exposure to corporate buyer power. Each study examines wage setting in a context in which rules and norms no longer shield workers from dislocation and wage pressure as they once did. Insofar as deviations from competitive labor market wages still affect the wage structure, it is often in the direction of lower and less equal wages: jobs can be transformed, workers dislocated, and real wages lowered.

Despite this changed context, the dissertation demonstrates the continued relevance of sociological approaches to the labor market. By returning to dual labor market theory, internal labor markets and labor process theory—sociological approaches to labor markets that have been largely dormant since the 1980s—I develop predictions about the wage effects of market power, the organizational setting of worker mobility and the inequality implications of constructing jobs out of tasks. By testing these theories in a context of substantially different employment relations, several problems with these approaches are clarified: for example, the importance of buyer, not just seller, product market power in wage determination and the polarizing, rather than purely deskilling, effects of task distillation.

Beyond resuscitating and reworking these older approaches, the analysis also provides new perspective on wages and the employment relation. A key upshot of the analysis is a broadened theory of the role of power in shaping the wage distribution. Institutional labor economics and organizational sociologists emphasize the ways that political conflict between groups within firms (Osterman 2011; Stainback, Tomaskovic-Devey, and Skaggs 2010). But power can also be exercised between firms—as in captive suppliers—and group conflicts can extend beyond the bounds of a single organization—as in union attempts to coordinate collective bargaining across multiple employers. Insofar as rising inequality has occurred primarily between firms, rather than within firms, understanding the role of bargaining power in generating inequality requires looking beyond the bounds of individual organizations. Anti-trust enforcement affects interactions between companies, both by regulating competition and by restricting the exercise of market power in vertical contracting relationships. Collective bargaining agreements can impose wage standards across multiple employers or set rules regulating outsourcing (Slichter, Healy, and Livernash 1960). Changes in these institutions governing between-firm inequality are an important area for future research on the role of power in shaping the wage structure.

Another area for future research lies in understanding how holding different jobs in different market positions affects workers' skills. This is both a methodological and a substantive issue. In all of the chapters in this dissertation, I rely on individual worker fixed effects in some model or another, but often my data provide limited controls for changing worker characteristics over time. Yet, workers' exposure to different market positions or organizational resources often affects their human capital, which in turn is reflected in in heterogeneity in returns to tenure and experience. Substantively, these changes could contribute to changes in the wage structure. Buyer pressure might make it more difficult for employers to credibly commit to future employment standards. Haphazard within-firm mobility might deter workers from making training investments in their jobs. Further research on these issues requires better time-varying measures of human capital that go beyond measures of formal education and incorporate heterogeneity in the training content of different work experiences.

Finally, a core remaining question is whether increased pressure on employers has returned workers and wage setting to a competitive labor market, ending the institutional- and organization-driven barriers to pure market wage determination that prevailed for much of the twentieth century (Jacoby 2004). Around one third of rising earnings inequality is due to high ability workers increasingly sorting to high wage firms (Song et al. 2018). This pattern is consistent with the finding in chapter 1 that buyer power is destroying rents for middle-income workers. It is also consistent with job reorganization, from chapter 2, splitting out high- from low-paid tasks (perhaps between- as much as within-firms). The standard interpretation of these changes tells a story in which prior to the 1980s lower-skilled workers had been able to sponge off of the talents of their betters (either higher-skilled co-workers or fixed-capital investing owners). In the face of increased product market competition and technological change, the really lucrative positions became occupied by the really productive workers, with the lower skilled jobs either hived off into outsourcing firms or trapped in dying industries.

This might be the case. However, the increased sorting of highly paid workers into high paying firms could also indicate market reorganization that leaves high-paid workers and owners occupying market positions that allow them to organize the economy to their own benefit. The simplest version of this process relies on market winners using political channels to tilt the economy in their favor (Hacker 2011). But subtler processes can play out through economic channels. Prior research suggest that financialization facilitates a version of this: financial firms transmit polarized and flexible work practices into client firms in non-financial industries (Ho 2009; Lin 2016). Some findings from this dissertation harbor similar implications. Large corporate buyers impose wage restraint on their suppliers. The concentration of high paid tasks among high earners depends in part on the expropriation of tasks from lower paid jobs. Future research should explore links such as these: between the outsized profits of superstar firms (Autor et al. 2017) and productivity laggards among their competitors or suppliers; between the highly paid positions of managers, and the insecurity (Goldstein 2012) and task homogeneity of those they manage.

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