

## ABSTRACT

Title of dissertation:      ESSAYS ON WAGES AND EMPLOYMENT  
OVER THE BUSINESS CYCLE

Seth Murray  
Doctor of Philosophy, 2020

Dissertation directed by:  Professor John Haltiwanger  
Department of Economics

I present three studies on wages and employment over the business cycle. In Chapter 1, I provide quasi-experimental evidence that downward nominal wage rigidity causes firms to destroy jobs and that this effect is empirically relevant for the macroeconomy. Given the unanticipated nature of the financial collapse in Q3 of 2008, differences across firms in their patterns of seasonal nominal wage adjustment generated heterogeneity in firms' exposure to downward nominal wage rigidity in Q4 of 2008. I find that exposure to downward nominal wage rigidity generated by firms' seasonal wage adjustment patterns accounts for 23% of the spike in aggregate job destruction that occurred in Q4 of 2008.

In Chapter 2, I present descriptive work with Leland Crane and Henry Hyatt regarding variation over the business cycle in both: i) the composition of employment by worker and firm productivity, and ii) the degree of assortative matching between workers and firms. Using employer-employee linked data for the U.S., we implement a battery of methods for ranking workers and firms by their productivity.

Across all these methods, we find three consistent patterns. First, the changes in the composition of employment by worker and firm productivity types move in opposite directions over the business cycle. During and immediately after recessions, low-productivity workers are less likely to work, whereas the employment share of low-productivity firms increases. Second, we find evidence of positive assortative matching between workers and firms (high-productivity workers are more likely to work at high-productivity firms). And third, the degree of positive assortative matching between workers and firms strengthens during the early stages of labor market downturns as low-productivity workers are disproportionately laid off from high-productivity firms.

In Chapter 3, I present a methodological advancement in the measurement of workers' base wages, variable compensation, and weeks worked in large administrative employer-employee linked data sets that only report workers' earnings. I develop a set of machine learning methods that identify each worker's unobserved persistent base wages, paydays weeks, and annual bonuses from the worker's observed quarterly earnings. I then implement and evaluate the quality of these methods using quarterly earnings data in the U.S. Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) dataset, an employer-employee linked dataset for the United States. Using the estimated nominal wages of workers in 30 U.S. states, I document three patterns of nominal wage adjustment: i) estimated persistent wage changes exhibit downward nominal wage rigidity, ii) optimal real wage cuts are suppressed by downward nominal wage rigidity, and iii) workers' nominal raises follow a Taylor-like pattern, with the probability of a wage raise spiking every four quarters.

ESSAYS ON WAGES AND  
EMPLOYMENT OVER THE BUSINESS CYCLE

by

Seth Murray

Dissertation submitted to the Faculty of the Graduate School of the  
University of Maryland, College Park in partial fulfillment  
of the requirements for the degree of  
Doctor of Philosophy  
2020

Advisory Committee:  
Professor John Haltiwanger, Chair  
Professor Borağan Aruoba  
Professor Judith Hellerstein  
Dr. Henry Hyatt  
Professor John Shea  
Professor Liu Yang, Dean's Representative

## Disclaimer

The opinions expressed herein are those of the author alone and do not reflect the view of the U.S. Census Bureau. All results have been reviewed to ensure that no confidential data are disclosed. See U.S. Census Bureau Disclosure Review Board bypass numbers: DRB-B0073-CED-20190910, DRB-B0069-CED-20190725, DRB-B0037-CED-20190327, CBDRB-2018-CDAR-061, CBDRB-2018-CDAR-042, and DRB-B0070-CED-20190829.

© Copyright by  
Seth Murray  
2020

## Preface

Recessions are characterized by surges in unemployment early in the recession, as job destruction spikes, and then protracted periods of high unemployment, as hiring slowly recovers. This dissertation addresses two long-standing debates regarding these cyclical fluctuations in employment.

First, macroeconomists have long debated whether downward nominal wage rigidity plays an important role in these employment fluctuations over the business cycle. This debate is still ongoing in part because of the dearth of empirical evidence determining whether there is a causal link between downward nominal wage rigidity and firm-level employment. In Chapter 1, I present quasi-experimental evidence that downward nominal wage rigidity causes firms to destroy jobs and that this effect is large enough in magnitude to be empirically relevant for explaining the spikes in job loss that occur at the beginnings of recessions.

The quasi-experiment exploits exogenous variation in firms' exposure to downward nominal wage rigidity that results from the timing of the unanticipated financial collapse in Q3 of 2008 relative to the calendar quarter in which firms historically tended to raise their workers' nominal wages. In 2008:Q4, immediately after the financial collapse, firms that historically tended to raise their workers' wages in the fourth calendar quarter ("Q4-raising firms") could choose to freeze workers' nominal wages, thereby lowering the firms' real wage bills. Conversely, firms that typically raised workers' wages in the second calendar quarter ("Q2-raising firms") would have raised their workers' wages in 2008:Q2, not anticipating the financial collapse

in 2008:Q3. As a result, the Q2-raising firms would have had to cut their workers' nominal wages to achieve a decrease in the firms' real wage bills similar to that of the Q4-raising firms. If exposure to downward nominal wage rigidity has a causal effect on job destruction, we should expect larger increases in the job destruction rate at Q2-raising firms relative to the Q4-raising firms.

I find that the job destruction rate at Q2-raising firms increased 36% in 2008:Q4, nearly double the 19% increase in the job destruction rate at Q4-raising firms. I further find that if all firms has been Q4-raisers, then U.S. firms' real wage bills at the start of 2008:Q4 would have been 1.1% lower and the spike in the job destruction rate would have been 23% smaller in 2008:Q4.

Second, macroeconomists have debated the implications of cyclical employment fluctuations for the overall efficiency of the economy. On one side, are those such as Caballero and Hammour (1994), who argue that these cyclical employment fluctuations can represent a "cleansing" of the economy. Such a cleansing effect could occur if: i) the spike in job destruction reflects workers separating from low-productivity firms, and ii) the subsequent recovery of employment reflects these unemployed workers being hired by higher productivity firms. On the other side of the debate are those, such as Barlevy (2002), who note that recessions could alternatively have a "sullyng effect" if workers find new employment at lower quality job matches or experience scarring effects during their unemployment spells.

In Chapter 2, I present descriptive work with Leland Crane and Henry Hyatt regarding both i) how the composition of employment by worker and firm productivity varies over the business cycle, and ii) whether the degree of assortative matching

between workers and firms strengthens or weakens during and following a recession. Using employer-employee linked data for the U.S., we implement a battery of methods for ranking workers and firms by their productivity. Across all these methods, we find three consistent patterns. First, the changes in the composition of employment by worker and firm productivity types move in opposite directions over the business cycle. During and immediately after recessions, low-productivity workers are less likely to work, whereas the employment share of low-productivity firms increases. The finding that low-productivity firms actually gain employment share following a recession is consistent with a sullyng effect of recessions. Second, we find evidence of positive assortative matching between workers and firms (high-productivity workers are more likely to work at high-productivity firms). And third, the degree of positive assortative matching between workers and firms strengthens during the early stages of labor market downturns as low-productivity workers are disproportionately laid off from high-productivity firms.

This dissertation also makes a methodological advancement in the measurement of workers' base wages in administrative data sets. In Chapter 3, I present a set of machine learning methods that enable the measurement of workers' base wages, variable compensation, and weeks worked from the workers' observed quarterly earnings (which are reported in many large administrative employer-employee linked data sets). I then implement and evaluate the quality of these methods using quarterly earnings data in the U.S. Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) dataset, an employer-employee linked dataset for the United States. Using the estimated nominal wages of workers in 30 U.S. states, I

document three patterns of nominal wage adjustment: i) estimated persistent wage changes exhibit downward nominal wage rigidity, ii) optimal real wage cuts are suppressed by downward nominal wage rigidity, and iii) workers' nominal raises follow a Taylor-like pattern, with the probability of a wage raise spiking every four quarters.

The rich data contained in employer-employee linked data sets have enabled researchers to explore a broad range of questions examining policy changes, worker and firm decision-making, and labor market dynamics. The wage measurement methods presented in Chapter 3 further expand the set of economic questions that can be addressed. For instance, the measurement of workers' base wages in an employer-employee linked dataset is a necessary innovation for the quasi-experiment presented in Chapter 1. These methods similarly allow the study of many other important economic questions where the analysis requires decomposing workers' observed quarterly earnings into their underlying base wages, variable compensation, and weeks worked.

## Dedication

*To my wife Joanne; my children Katherine, Thomas, and Nielsen; and  
my parents Rosanne and Michael - with all my love and gratitude.*

## Acknowledgments

I am extremely grateful to my advisor, John Haltiwanger, for introducing me to the study of labor markets, demonstrating how large-scale administrative data can provide insight to macroeconomic questions, and guiding my investigation of new research ideas. I am similarly appreciative of the guidance and support I have received from Henry Hyatt, my mentor at the U.S. Census Bureau. Whether it is through suggesting new approaches, referencing related literature, or reviewing my analyses, Henry has been instrumental in shaping my approach to framing and exploring research questions. I am also indebted to Judy Hellerstein. Judy's critical eye both pushed my thinking to new depths and served as a useful check on my enthusiasm for the next shiny research idea. I thank Borağan Aruoba for teaching an empiricist about what questions a structural model can address (and how to build one). I benefited greatly from our many discussions of what makes research good, as well as from his practical advice on how to approach my PhD and subsequent career. I am deeply appreciative of John Shea's willingness to review early drafts of my research proposals and papers - and the detailed comments that he always provided. More generally, I've benefited from John's ability to distill the essence of an argument into a pithy and intuitive synopsis (both in my own research and in many, many macro seminars).

In addition to my committee, I thank faculty members of the Economics Department, especially Katharine Abraham, Pierre De Leo, Thomas Drechsel, Sebnem Kalemli-Ozcan, Ethan Kaplan, Guido Kuersteiner, Ingmar Prucha, Felipe Saffie,

and Luminita Stevens, for our many conversations about both my research and career. I am especially grateful to Jessica Goldberg, Lesley Turner, and Ethan Kaplan for organizing a pre-708 in my first and second years in the PhD program - this was my first exposure to self-directed economic research (and where I learned what causal identification meant). I also thank the Economics Department staff members, especially Terry Davis, Vickie Fletcher, Jessica Gray, Amanda Statland, Mark Wilkerson for helping me throughout my six years at the University of Maryland. My fellow PhD students at the University of Maryland have been of immense help throughout my PhD. I thank Jake Blackwood, Joonkyu Choi, Alejandro Graziano, Gonzalo Garcia-Trujillo, Bryan Hardy, Rodrigo Heresi, Karam Jo, Donggyu Lee, Sai Luo, Scott Ohlmacher, Veronika Penciakova, Matthew Staiger, Cody Tuttle, Mateo Uribe-Castro, and Yi Zhao. Your friendship and our extended research conversations have enriched my last six years.

I am deeply indebted to the LEHD team at the U.S. Census Bureau's Center for Economic Studies for welcoming me into such a dynamic and exciting research group. I have learned so much from the projects I've worked on, the seminars I've attended (and given), and our conversations over Thursday bagels. I specifically want to thank Kevin McKinney and Stephen Tibbets for introducing me to SAS programming and imputation methods; Kristin Sandusky for teaching a former entrepreneur all about self-employment data; Larry Warren for providing copious amounts of feedback and guidance on labor search-and-matching models; Hubert Janicki and Lee Tucker for being great research partners; and Erika McEntarfer for being the type of research manager who creates such a wonderful environment for an inquisitive intern.

My development as an economist has also benefited from the generous advice and feedback of many others, most notably Leland Crane, Rosanne Ducey, Joanne Gaskell, Michael Murray, Stephen O'Connell, and Pawel Krolkowski.

I am grateful to both the Center for Retirement Research at Boston College and the funders of the Betancourt Fellowship for the financial support that has allowed me to focus on my research.

Lastly, I am eternally grateful to my family for their love, support, and ribbing.

## Table of Contents

Preface	ii
Dedication	vi
Acknowledgements	vii
Table of Contents	x
List of Tables	xiii
List of Figures	xiv
List of Abbreviations	xv
1 Downward Nominal Wage Rigidity and Job Destruction	1
1.1 Introduction	1
1.2 Data	6
1.3 Patterns of nominal wage adjustment	8
1.4 Quasi-experimental evidence that DNWR causes job destruction	10
1.4.1 Identification from the onset of the Great Recession	12
1.4.2 DiD: 2008:Q4 job destruction by typical raise quarter	15
1.4.3 A further test of the parallel trends assumption	19
1.4.4 IV: Raise schedules and the real wage bill - exploring the mechanism	23
1.4.5 Job destruction: More layoffs, not less hiring	29
1.4.6 Variation in worker layoff risk from DNWR exposure	30
1.5 Summary	34
2 Cyclical Labor Market Sorting	37
2.1 Introduction	37
2.2 Data	42
2.2.1 Source data	42
2.2.2 Ranking workers and firms	43
2.3 Empirical evidence on composition and sorting	46
2.3.1 Overview and notation	46
2.3.2 Worker and firm composition	48
2.3.2.1 Worker composition	48

2.3.2.2	Firm composition . . . . .	54
2.3.3	Cyclical worker-firm rank agreement . . . . .	63
2.3.3.1	Employment shares of worker-firm rank combinations	64
2.3.4	The poaching and nonemployment margins of sorting . . . . .	67
2.3.4.1	Worker-firm rank correlation and unemployment . . . . .	73
2.3.5	Summary . . . . .	75
3	Measurement of Nominal Wages in Administrative Earnings Data	77
3.1	Introduction . . . . .	77
3.2	Data . . . . .	80
3.3	Estimation of payday weeks . . . . .	81
3.3.1	Payroll schedules . . . . .	83
3.4	Post-Lasso estimation of persistent nominal wage changes . . . . .	85
3.4.1	Quality evaluation of post-Lasso estimated nominal wage changes	89
3.5	Comparison of estimated persistent nominal wage changes to literature	93
3.6	Patterns of nominal wage adjustment . . . . .	95
3.6.1	Wages exhibit downward nominal rigidity . . . . .	95
3.6.2	Nominal wage rigidity suppresses real wage changes . . . . .	97
3.6.3	Annual schedules of nominal raises . . . . .	101
3.7	Summary . . . . .	104
A	Appendix to Chapter 1	105
A.1	DiD: Endogeneity of revenue change . . . . .	105
A.2	Biases in OLS regression of job destruction on real wage bill ratio . . . . .	109
A.3	Alternative IV results . . . . .	110
B	Appendix to Chapter 2	113
B.1	Employment and transition definitions . . . . .	113
B.1.1	Employment concepts . . . . .	114
B.1.2	Transition concepts . . . . .	115
B.1.3	Aggregation . . . . .	117
B.2	Worker ranking implementation details . . . . .	118
B.2.1	Method 1: Worker nonemployment duration and firm poaching hire share . . . . .	118
B.2.1.1	Ranking firms by poaching share of hires . . . . .	119
B.2.1.2	Ranking workers by prime-age employment rates . . . . .	120
B.2.2	Method 2: Average earnings and labor productivity . . . . .	121
B.2.2.1	Ranking workers based on average earnings . . . . .	121
B.2.2.2	Ranking firms based on revenue productivity . . . . .	121
B.2.3	Method 3: Additive worker and firm effects . . . . .	123
B.2.4	Method 4: Worker reranking and surplus . . . . .	126
B.2.4.1	Worker residuals for ranking . . . . .	126
B.2.4.2	Reranking workers to minimize disagreement . . . . .	127
B.2.4.3	Surplus-based firm ranking . . . . .	131
B.3	Supplemental tables and figures . . . . .	135

B.3.1	Poaching vs. nonemployment margins . . . . .	135
B.3.2	Worker-firm rank shares . . . . .	140
B.3.3	Correlations among worker and firm ranks . . . . .	142
B.4	Reranking production function inversion estimation . . . . .	146
B.4.1	Value of unemployment by worker type . . . . .	146
B.4.2	Value of employment by worker-firm combination . . . . .	147
B.4.3	Match surplus by worker-firm combination . . . . .	147
B.4.4	Vacancy value by firm type . . . . .	147
C	Appendix to Chapter 3 . . . . .	149
C.1	RD tests for downward real and nominal rigidity . . . . .	149
C.2	Permanent versus transitory earnings changes . . . . .	154
C.3	Measures of wage compensation . . . . .	157
C.3.1	Measurement error: Rounding in hours paid . . . . .	159
C.3.2	Overtime compensation . . . . .	160
C.3.3	Variable compensation: Annual bonuses . . . . .	161

## List of Tables

1.1	Probability of nominal wage change & typical raise quarter . . . . .	10
1.2	Firm characteristics by typical raise quarter . . . . .	14
1.3	Difference-in-differences job destruction by typical raise quarter . . .	18
1.4	Differential Q2 vs Q4-raiser job destruction rate & annual revenue change . . . . .	22
1.5	First-stage: Start-of-quarter / 4-Q lag moving average real wage bill	27
1.6	Second-stage: Job destruction rate . . . . .	28
1.7	Differential Q2 vs Q4-raiser employment outcomes in 2008:Q4 . . . .	31
1.8	Differential layoff rate in 2008:Q4 by worker characteristic . . . . .	33
2.1	Changes in worker rank shares and the unemployment rate . . . . .	51
2.2	Net nonemployment hiring by worker rank and unemployment . . . .	52
2.3	Changes in firm rank shares and the unemployment rate . . . . .	56
2.4	Change in net hiring by firm rank and unemployment . . . . .	59
2.5	Changes in worker-firm rank shares and unemployment . . . . .	66
2.6	Net nonemployment hires for worker-firm rank shares and unemploy- ment . . . . .	68
2.7	Net poaching hires for worker-firm rank shares and unemployment . .	71
2.8	Relationship between worker-firm correlations and the unemployment rate . . . . .	75
3.1	Comparison of measures of nominal compensation changes . . . . .	94
3.2	Suppression of wage changes due to downward nominal wage rigidity	102
A.1	Second-stage: Job destruction rate . . . . .	112
B.1	Change in share and unemployment (HP) . . . . .	140
B.2	Change in share and unemployment (FD) . . . . .	141
B.3	Correlation of worker and firm ranks across methods . . . . .	144
B.4	Correlation of additive model worker and firm ranks across imple- mentation methods . . . . .	145
C.1	Regression discontinuity test at zero nominal wage change . . . . .	152
C.2	Regression discontinuity test at zero real wage change . . . . .	153
C.3	Hourly earnings change comparison . . . . .	162

## List of Figures

1.1	Period-specific Q2 vs. Q4-raiser differential job destruction coefficient estimates . . . . .	16
2.1	Changes in worker rank shares . . . . .	49
2.2	Change in firm rank shares . . . . .	55
3.1	Cumulative 4-quarter change in persistent log nominal wage . . . . .	96
3.2	Histogram of persistent nominal wage changes at annual frequency near zero . . . . .	97
3.3	Probability of wage change by quarters since last wage change . . . . .	103
B.1	Percent change in worker employment . . . . .	137
B.2	Percent change in firm employment: Nonemployment . . . . .	138
B.3	Percent change in firm employment: poaching . . . . .	139
B.4	Production Function from Worker Reranking & Surplus Method . . . . .	148
C.1	Histogram of persistent real wage changes at quarterly frequency near zero . . . . .	151

## List of Abbreviations

2SLS	Two Stage Least Squares
Ag	Agriculture
CPS	Current Population Survey
DHS	Davis Haltiwanger Schuh
DiD	Difference-in-Differences
DNWR	Downward Nominal Wage Rigidity
DSGE	Dynamic Stochastic General Equilibrium
EE	Employer-to-Employer
EIN	Federal Employer Identification Number
EN	Employment-to-Nonemployment
FE	Fixed Effect
FIRE	Finance Insurance Real Estate
GDP	Gross Domestic Product
GHY	Grigsby Hurst Yildirmaz
HP	Hodrick Prescott
iid	Independent and Identically Distributed
IV	Instrumental Variables
JD	Job Destruction
Lasso	Least absolute shrinkage and selection operator
LEHD	Longitudinal Employer-Household Dynamics
NAICS	North American Industry Classification System
NBER	National Bureau of Economic Research
NE	Nonemployment-to-Employment
OLS	Ordinary Least Squares
PSID	Panel Study of Income Dynamics
RD	Regression Discontinuity
rLBD	Revenue-enhanced Longitudinal Business Database
SEIN	State Employer Identification Number
SIPP	Survey on Income and Program Participation
Std Dev	Standard Deviation
UI	Unemployment Insurance

# Chapter 1: Downward Nominal Wage Rigidity and Job Destruction

## 1.1 Introduction

A prominent question in macroeconomics is the role of downward nominal wage rigidity (DNWR) in explaining employment fluctuations over the business cycle. Whether and how DNWR affects employment has important implications for a wide range of macroeconomic questions, including: why does the effectiveness of monetary policy differ for contractionary versus expansionary monetary policy shocks; what is the Federal Reserve's optimal inflation target; and why do employment and output exhibit asymmetric fluctuations over the business cycle. Despite the importance of these questions and the central role of downward nominal wage rigidity in many New Keynesian DSGE models<sup>1</sup> and labor search-and-matching models,<sup>2</sup> there is remarkably little empirical evidence whether DNWR causes firms to destroy jobs

---

<sup>1</sup>Kim and Ruge-Murcia (2009), Fagan and Messina (2009), and Schmitt-Grohé and Uribe (2013) use DSGE models with DNWR to explore the optimal inflation rate. Benigno and Ricci (2011) and Daly and Hobijn (2014) examine the importance of DNWR for the Phillips Curve. Abbritti and Fahr (2013) and Schmitt-Grohé and Uribe (2016) argue that DNWR may be responsible for the asymmetric distribution of unemployment and output over the business cycle. Schmitt-Grohé and Uribe (2017) examine the role of DNWR in explaining jobless recoveries. Shen and Yang (2018) demonstrate the DNWR causes the fiscal multiplier to vary over the business cycle. Eggertsson, Mehrotra and Robbins (2019) include DNWR in a New Keynesian DSGE model exhibiting secular stagnation.

<sup>2</sup>Elsby (2009); Carlsson and Westermarck (2016); Chodorow-Reich and Wieland (forthcoming); and Dupraz, Nakamura and Steinsson (2019) embed DNWR into search-and-matching models of the labor market to examine employment fluctuations over the business cycle.

(or change their employment allocations more generally).

This chapter presents quasi-experimental evidence that DNWR plays an important causal role in employment fluctuations through the job destruction margin. I begin by establishing that within a firm, employees' nominal wage raises tend to be synchronized to occur in the same calendar quarter year-over-year, but the calendar timing of these synchronized wage raises differs across firms. The quasi-experiment then exploits exogenous variation in firms' exposure to DNWR generated by the timing of an unanticipated negative aggregate shock relative to the calendar quarter in which the firms tend to raise their workers' nominal wages.

The financial collapse precipitated by the failure of Lehman Brothers in the last month of 2008:Q3 was a large, unanticipated negative aggregate shock. In 2008:Q4, immediately after the collapse, firms that historically tended to raise their workers' wages in the fourth calendar quarter ("Q4-raising firms") could choose to freeze workers' nominal wages, thereby lowering the firms' real wage bills. Conversely, firms that typically raised workers' wages in the second calendar quarter ("Q2-raising firms") would have raised their workers' wages in 2008:Q2, not anticipating the financial collapse in 2008:Q3. As a result, the Q2-raising firms would have had to cut their workers' nominal wages to achieve a decrease in the firms' real wage bills similar to that of the Q4-raising firms. If exposure to DNWR has a causal effect on job destruction, we should expect larger increases in the rate of job destruction at Q2-raising firms relative to otherwise similar Q4-raising firms.

For the quasi-experiment, I use a 10% random sample of firms from 30 states in the U.S. Census Bureau's LEHD data set, an employer-employee linked admin-

istrative data set covering approximately 96% of employment in each state. I begin by employing the set of machine learning methods described in Chapter 3 to extract estimates of the timing and magnitude of each worker’s unobserved persistent base wage changes from the worker’s observed quarterly earnings. I then use these estimated persistent wage changes to identify the firm-specific seasonal adjustment patterns in workers’ nominal wages.

I find that the job destruction rate at Q2-raising firms increased by 36% in 2008:Q4, nearly double that of the 19% increase in the job destruction rate of Q4-raising firms (who had less exposure to DNWR). I further find that if all firms had been Q4-raisers, then U.S. firms’ real wage bills at the start of 2008:Q4 would have been 1.1% lower and the increase in the job destruction rate would have been 23% less in 2008:Q4. Since this estimate does not take into account the fact that the Q4-raising firms had less, but still some exposure to DNWR, this estimate is a lower-bound of DNWR’s role in the spike in job destruction that immediately followed the financial collapse in 2008:Q3.

The largest contribution of this chapter is to the empirical literature examining causal links between DNWR and firm-level employment allocation.<sup>3</sup> Establishing a causal link between downward nominal wage rigidity and job destruction specifically,

---

<sup>3</sup>A number of other studies have explored DNWR and its effect on employment at a more aggregate level. Bernanke and Carey (1996) examine the role of DNWR in explaining countries’ differential response during the Great Depression to monetary policy shocks transmitted through the gold standard. Fehr and Goette (2005) and Ridder and Pfajfar (2017) use variation in localities’ exposure to wage rigidity in a monetary union (Switzerland and the United States respectively) to identify the effect of DNWR on local employment. Pischke (2018) finds that occupations with greater wage flexibility (real estate agents) had smaller employment declines in response to the post-2006 U.S. housing market collapse relative to occupations with more rigid wages (architects and construction workers). Kaur (2019) examines agricultural employment in Indian villages - finding that wages exhibit downward nominal rigidity and that this rigidity causes agricultural employment to fall if a positive rainfall shock is followed by a negative shock.

or employment in general, is notoriously hard. The challenge lies in identifying variation in a firm's exposure to downward nominal wage rigidity that is exogenous with respect to the unobserved factors affecting its employment decisions. This is difficult because firms' wage setting and employment decisions are forward looking and almost inextricably intertwined.

I am aware of only three studies that examine whether nominal wage rigidity affects firm-level employment. Card (1990) examines unionized Canadian manufacturing firms. He finds that when unionized manufacturing firms are bound by fixed nominal wage contracts, firm employment moves in the opposite direction of unexpected real wage changes. While Card focuses on firms' overall employment, the studies by Kurmann and McEntarfer (2019) and Ehrlich and Montes (2019) examine the distinct effects of DNWR on the job creation / hiring and job destruction / layoff margins. Both studies use employer-employee linked administrative data to establish a measure of individual firms' historical incidence of DNWR. They then examine whether a firm's job destruction or layoffs respond more strongly to a negative shock if the firm historically exhibited more DNWR.<sup>4</sup> Both studies find substantial effects on layoffs or job destruction from greater exposure to downward nominal wage rigidity.

---

<sup>4</sup>In the case of Kurmann and McEntarfer (2019), they use LEHD data for Washington state to construct various firm-level measures of wage rigidity in the years prior to 2008, and then they examine the differential response of job destruction and other employment outcomes during the Great Recession at firms with more historical nominal wage rigidity. Ehrlich and Montes (2019) use administrative data from Germany to construct similar firm-specific measures of historical DNWR. Since the persistence of productivity shocks may confound the relationship between firms' historical incidence of DNWR and current productivity shocks, Ehrlich and Montes (2019) use collectively bargained wage floors that are set at a region-industry level as an instrumental variable for the firms' DNWR measure. These region-industry specific wage floors are valid instruments if, as Ehrlich and Montes (2019) argue, the business conditions of firms in a given region and industry do not affect the bargaining of the wage floor for that industry and region.

The quasi-experimental evidence presented in this chapter contributes to this sparse empirical literature in two ways. First, this chapter’s identification strategy applies to nearly all firms, not just firms in a particular industry or those firms with high historical levels of nominal wage rigidity. As a result, my empirical results can be interpreted as the average treatment effect of downward nominal wage rigidity on job destruction for the population of U.S. firms. Second, this chapter demonstrates a causal effect of exposure to DNWR on job destruction by relying on a different set of identifying assumptions than Kurmann and McEntarfer (2019) and Ehrlich and Montes (2019). Identification in these earlier studies requires that no confounding variables affect both the historical incidence of DNWR at the firm (or the industry-region for Ehrlich and Montes) and the firm’s current-period employment decisions. Thus, persistent negative shocks affecting both the historical incidence of wage rigidity and current-period business conditions threaten the validity of these studies’ identifying assumptions. On the other hand, the identifying assumption of the quasi-experiment presented in this chapter is robust to persistent negative shocks. Specifically, my quasi-experiment assumes that no confounding variables affect both: i) the calendar quarter in which a firm has historically tended to raise its workers’ nominal wages, and ii) the firm’s employment decisions when it experiences an unanticipated negative aggregate shock.

My quasi-experimental empirical finding that exposure to DNWR causes a substantial increase in firms’ job destruction rates is inconsistent with many common macro models of employment with wage rigidity.<sup>5</sup> The literature has mostly

---

<sup>5</sup>When search-and-matching models of the labor market incorporate wage rigidity, the models

dismissed the effect of DNWR on job destruction, instead focusing on how DNWR can affect employment through either the intensive margin of hours worked or the extensive margin of job creation. My results show that the causal effect of DNWR on job destruction is sufficiently large that our models should account for DNWR's effect on employment through the job destruction margin.

## 1.2 Data

My proposed quasi-experiment requires panel data on both firms' employment levels and workers' nominal wages at a sub-annual frequency. This chapter uses the U.S. Census Bureau's LEHD data set - an employer-employee linked data set with quarterly earnings for approximately 96% of all employment in a state.

Individuals in the LEHD are uniquely identified by a Protected Identification Key (PIK) that allows each individual to be tracked across different employers and locations. Each individual's quarterly earnings data in the LEHD is derived from employers' mandatory unemployment insurance filings. The LEHD identifies employers at the level of a state employer identification number (SEIN). For simplicity, I refer to each SEIN as a firm.

I construct a 10% random sample of SEINs from thirty states covering the pe-

---

tend to assume that the wage rigidity affects job creation, but not job destruction. New Keynesian DSGE models where wage adjustment costs generate nominal wage rigidity (a la Rotemberg (1982)) oftentimes only find small effects of nominal wage rigidity on employment. DSGE models that generate nominal wage rigidity using staggered wage adjustment (Taylor (1980)) or random arrival of wage adjustment opportunities (Calvo (1983)) find larger effects of wage rigidity on employment, but violate the Barro critique. Barro (1977) argues that models should not generate an effect of wage rigidity on employment through an assumption that workers and firms ignore mutually beneficial nominal wage cuts.

riod from 1998:Q1 to 2017:Q1.<sup>6</sup> I chose these thirty states because there are no gaps in reported quarterly earnings for any of these states over the sample period. When I examine labor market outcomes for workers, such as employment-to-nonemployment (EN) or employer-to-employer (EE) transitions, I identify these transitions using 100% of the firms in the 30 states. I then restrict my subsequent analysis to workers at the 10% random sample of firms. Using 100% of firms ensures that these labor market transitions are accurately identified (otherwise some EE transitions would incorrectly be categorized as EN transitions).

The earnings data in the LEHD is complemented with both worker characteristics (age, sex, race, and education) and firm characteristics (industry, firm age, and firm size) from other data sources. Firm age and firm size are derived from aggregating one or more SEINs (potentially across states) to the level of the federal employer identification number (EIN). Although revenue data is not available in the LEHD data set, each SEIN is linked to an EIN for which I obtain real revenue and real revenue per worker at an annual frequency from the Census Bureau's revenue-enhanced Longitudinal Business Database (Haltiwanger, Jarmin, Kulick and Penciakova (2019)). Last, I use the methods described in Chapter 3 to estimate the timing and magnitude of each worker's unobserved persistent base wage changes from the worker's observed quarterly earnings.

---

<sup>6</sup>The states included in the primary sample are: CA, CO, CT, FL, GA, HI, ID, IL, IN, KS, LA, MD, ME, MT, NC, ND, NJ, NM, NV, OR, PA, RI, SC, SD, TN, TX, VA, WA, WI, and WV.

### 1.3 Patterns of nominal wage adjustment

Chapter 3 presents evidence that: i) the post-Lasso estimated persistent wage changes exhibit downward nominal wage rigidity, and ii) workers' nominal raises follow a Taylor-like pattern, with the probability of a wage raise spiking every four quarters. This section complements these findings by showing that the timing of workers' annual raises are synchronized within each firm. I find that a worker is twice as likely to receive a nominal wage raise in the firm's "typical raise quarter" - the calendar quarter in which coworkers have historically tended to receive nominal wage raises at the firm.

I classify a firm as having a particular calendar quarter as its "typical raise quarter" if two criteria are met. First, at least 33% of raises in previous years occurred in the given calendar quarter. Second, given the observed number of raises in this calendar quarter and all calendar quarters, I reject the null hypothesis that raises are randomly distributed with equal probability across the four calendar quarters (I use a one-sided hypothesis test at the 5% significance level for a binomial distribution with  $p = 0.25$ ). I identify typical raise quarters for the firms of 79.6% of workers. This is likely to be an underestimate of the prevalence of within-firm synchronization of annual raises because the procedure for identifying typical raise quarters is underpowered for firms with relatively few observed nominal wage changes. These typical raise quarters exhibit some seasonality: the plurality of workers have Q3 as their typical raise quarter (26%), the fewest have Q2 (10%), approximately the same share have Q1 and Q4 (16% and 15% respectively), and 12.6% of workers have two

typical raise quarters.<sup>7</sup>

To determine whether workers' annual schedules of wage raises are coordinated within firms, I test whether the probability that a worker receives a nominal raise can be predicted using the historical typical raise quarter of coworkers. I focus only on a typical raise quarter measure based on data from previous years so as to avoid the concern that firm-wide shocks generate contemporaneous correlation in coworkers' raise frequencies. Thus, I estimate the following relationship using OLS:

$$\mathbb{1} [\Delta_{ikt}^w > 0] = \alpha d_{ikt}^{RQ} + \mathbf{X}_{ikt}\beta + \epsilon_{ikt} \quad (1.1)$$

where  $\mathbb{1} [\Delta_{ikt}^w > 0]$  is an indicator variable equal to one if worker  $i$  at firm  $k$  receives a nominal wage raise in quarter  $t$ ;  $d_{ikt}^{RQ}$  is an indicator variable equal to one if the calendar quarter in  $t$  corresponds to the firm's typical raise quarter for coworkers in previous years; and  $\mathbf{X}_{ikt}$  is a set of control variables that includes the worker's age, tenure, quarters since their last wage change, and earnings quintile dummy variables.

The regression results shown in Table 1.1 indicate that the probability that a worker receives a nominal raise increases 9.3 percentage points in the firm's typical raise quarter, more than doubling the baseline 7.3% probability that a worker

---

<sup>7</sup>That nominal wage raises are more likely to occur in the second half of the year aligns with the hypothesis of Olivei and Tenreyro (2007), which used this fact to show that the effectiveness of monetary policy differs over the calendar year. Olivei and Tenreyro found when monetary policy shocks occur in the first half of the year, wages are slower to adjust and output responds more strongly to the shock. A more recent study by Björklund, Carlsson and Nordström Skans (2019) examines the implications of seasonal nominal wage rigidity in Sweden and finds that monetary policy was more effective in periods when the staggered timing of union contracts meant that a larger share of workers had rigid nominal wages.

receives a nominal raise in any given quarter. This implies that workers' annual nominal raise schedules are strongly coordinated within the firm.

Table 1.1: Probability of nominal wage change & typical raise quarter

	Raise Probability			Cut Probability	
	(1)	(2)	(3)	(4)	(5)
Typical Raise Quarter	<b>8.2***</b> (0.9)		<b>9.3***</b> (0.9)		<b>-0.3***</b> (0.03)
Baseline (Q1)		<b>9.3***</b> (0.6)	<b>7.3***</b> (0.8)	<b>1.2***</b> (0.07)	<b>1.1***</b> (0.10)
Q2		-0.24 (0.37)	-0.09 (0.32)	<b>-0.18***</b> (0.04)	<b>-0.18***</b> (0.04)
Q3		1.94 ( 1.14)	1.15 (0.81)	<b>-0.28***</b> (0.03)	<b>-0.20**</b> (0.05)
Q4		0.55 (0.32)	0.59 (0.28)	0.14 (0.07)	-0.09 (0.10)
Observations	17.4M	10.6M	10.6M	10.6M	10.6M

*Note:* Outcome variables are indicator variables equal to one if a worker has a nominal wage raise (1-3) or cut (4-5) in the quarter. Models 1, 3, and 5 include an indicator if the quarter qualifies as the firm's typical raise quarter. Models 2-3 and 4-5 include a set of calendar quarter dummy variables (with the intercept representing the calendar quarter I). All models include as control variables: worker age, tenure, quarters since last wage change, and earnings quintile dummy variables. Robust standard errors clustered at the firm level. \*\*\*, \*\*, \* indicate statistical significance at the 0.1%, 1.0%, and 5.0% levels, respectively. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0069-CED-20190725.

#### 1.4 Quasi-experimental evidence that DNWR causes job destruction

To determine whether greater exposure to downward nominal wage rigidity (DNWR) causes firms to increase their rate of job destruction when faced with a

negative shock, I require variation in firms' exposure to DNWR that is exogenous with respect to the unobserved factors affecting their employment decisions. For this quasi-experiment, this exogenous variation is generated by the timing of firms' annual schedule of nominal wage raises relative to an unanticipated negative aggregate shock.

In any given quarter, firms with more recent typical raise quarters tend to have real wage bills above their annual average real wage bill. This follows from the finding that, within a firm, employees' annual nominal raises are synchronized to occur in the same calendar quarter year-over-year. This synchronization of annual raises generates a stair-step pattern in the firm's quarterly nominal wage bill. Given positive inflation, the stair-step pattern of nominal wages implies that, over any given four-quarter period, the firm's real wage bill spikes in the typical raise quarter, declines over the next three quarters (as inflation eats away at the fixed nominal wage), and reaches a nadir immediately before the firm's next typical raise quarter.

When a large, unanticipated negative aggregate shock occurs, firms with upcoming typical raise quarters can choose to freeze their workers' nominal wages, resulting in quarterly real wage bills that remain below their recent annual average real wage bills. On the other hand, firms that just experienced their typical raise quarter would have to cut workers' nominal wages to achieve a similar decrease in their quarterly real wage bill. Thus, when an unanticipated negative aggregate shock occurs, firms will have differential exposure to downward nominal wage rigidity simply because of differences in their typical raise quarters. If exposure to DNWR has a causal effect on job destruction, we should expect larger increases in job destruction

at firms that had their typical raise quarter immediately prior to the negative shock.

The identification strategy of the quasi-experiment relies on two assumptions. First, it requires an unanticipated negative shock. If firms anticipate the negative shock, then those firms with typical raise quarters immediately before and after the negative shock will similarly freeze their workers' nominal wages. As a result, when the negative shock is anticipated, firms' exposure to DNWR is unrelated to the timing of the shock relative to firms' typical raise quarters.

Second, the identification requires that, absent these differences in typical raise quarters, the job destruction rates at firms would have been similar (parallel trends assumption). Most critically, this requires that the magnitude of the negative shock is independent of the firms' typical raise quarters. If the negative shock is stronger for firms with a particular typical raise quarter, then this differential magnitude of the shock confounds the effect of exposure to DNWR by typical raise quarter. Any such confounding invalidates the characterization of treatment versus control groups based on their typical raise quarters.

#### 1.4.1 Identification from the onset of the Great Recession

To answer whether exposure to DNWR causes firms to destroy jobs at higher rates, I use the identification strategy described above, focusing on job destruction in 2008:Q4, immediately following Lehman Brothers' bankruptcy in September 2008 and the ensuing financial collapse. I look to the Survey of Professional Forecasters by the Federal Reserve Bank of Philadelphia (2008:Q4) for evidence that the financial

collapse in September 2008 qualifies as a large, unanticipated negative aggregate shock. Between August 8th and November 10th of 2008, professional economic forecasters' predictions for the annualized real GDP growth rate in 2008:Q4 fell by 3.8 percentage points, from +0.7% to -2.9%. Their newfound pessimism also extended into longer-term forecasts, as their predictions for real GDP growth in 2009 fell from +1.5% to -0.2%. Given the dramatic downward revisions of professional forecasters' predictions, the financial collapse in 2008:Q3 arguably qualifies as a large, unanticipated negative aggregate shock.

Although it is impossible to test the assumption that firms' job destruction rates at the onset of the Great Recession would have been similar absent their differences in typical raise quarter, I improve the plausibility of this assumption by including both firm-specific seasonal dummy variables and industry-by-time fixed effects as explanatory variables for firm-level job destruction. Table 1.2 reports the share of start-of-quarter employment in 2008:Q4 for Q2 and Q4-raising firms, broken down by two-digit NAICS industry, firm age, and firm size in 2008:Q4. Along all three dimensions, there are significant differences in the share of Q2 versus Q4-raisers by industry, firm size and firm age. These differences, particularly by industry, are to be expected since industry-specific seasonal demand patterns may affect the optimal timing of firms' annual nominal raise schedules. Given these differences in industry composition, I control for both firm-specific seasonality (using firm-specific calendar-quarter fixed effects) and industry-by-time fixed effects (which help absorb industry-specific shocks in 2008:Q4).

Table 1.2: Firm characteristics by typical raise quarter

Employment Share at Start of 2008:Q4				
	QWI	Typical Raise Quarter		
		Any Quarter	Q2 Only	Q4 Only
<b>Industry (NAICS)</b>				
Ag & Mining & Utilities	1.9%	1.6%	1.3%	2.1%
Construction	6.5%	10.2%	18.9%	4.0%
Manufacturing	12.3%	15.2%	11.6%	18.7%
Wholesale & Retail Trade	19.0%	13.9%	16.9%	12.3%
Transportation	3.9%	3.3%	3.4%	4.6%
Information	2.4%	2.5%	0.7%	2.7%
FIRE	6.8%	7.5%	2.8%	9.0%
Professional Services	6.9%	10.8%	6.1%	14.5%
Management	1.9%	2.0%	0.7%	2.4%
Waste Management	7.0%	6.3%	9.5%	3.4%
Education	2.0%	4.0%	0.5%	5.1%
Health Care	13.5%	7.4%	4.0%	10.1%
Arts & Entertainment	1.7%	3.5%	5.8%	2.4%
Accommodation	10.5%	9.3%	15.0%	6.3%
Other Services	3.7%	2.5%	2.8%	2.4%
<b>Firm Age</b>				
0-1	4.0%	2.1%	2.6%	1.6%
2-3	4.6%	4.2%	5.3%	3.2%
4-5	4.1%	4.5%	6.0%	3.4%
6-10	9.5%	10.5%	13.8%	8.8%
11+	77.8%	78.7%	72.3%	83.0%
<b>Firm Size</b>				
0-19	19.8%	24.6%	26.8%	24.7%
20-49	10.1%	18.1%	24.4%	16.1%
50-249	15.8%	25.8%	30.8%	24.5%
250-499	5.7%	8.34%	7.0%	9.7%
500+	48.6%	23.2%	11.0%	24.9%

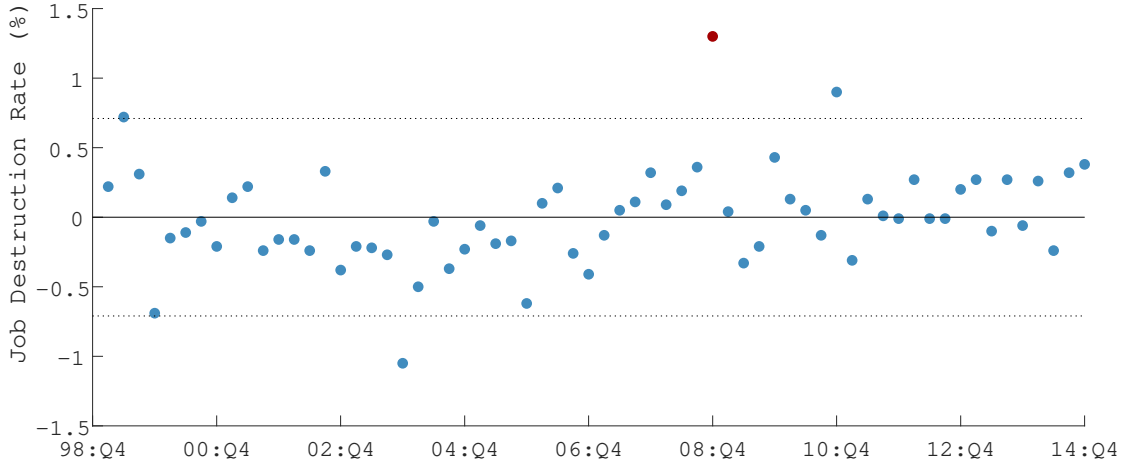
*Note:* The “QWI” column reports the share of start-of-quarter employment by industry from the U.S. Census Bureau’s Quarterly Workforce Indicators data product for the same 30 states included in 10% random sample from LEHD. The “Any Quarter” column reports the share of employment in the given industries at firms for which I identify typical raise quarters. The “Q2 Only” and “Q4 Only” columns report the share of employment in the given industries with typical raise quarters in Q2 or Q4. U.S. Census Bureau Disclosure Review Board bypass number CBDRB-2018-CDAR-061.

Since the parallel trends assumption is untestable, I follow the common practice of examining both the trend in the differences between Q2 and Q4-raising firms prior to 2008:Q4 and the relative magnitude of these differences across periods. I use the same difference-in-differences specification that I use for the quasi-experiment to estimate the difference in job destruction rates between Q2-raising firms versus Q4-raising firms in every other sample period. Figure 1.1 plots these coefficient estimates. The two key takeaways from this figure are that: i) there are no economically or statistically significant pre-trends in the differences between Q2 and Q4 raising firms, and ii) the magnitude of the difference between Q2 and Q4-raising firms in 2008:Q4 is an extreme outlier relative to the typical magnitude of differences between these two firm types. (The magnitude of the 2008:Q4 coefficient estimate is 30% larger in absolute value than the next largest coefficient estimate from any other period and 3.6 times larger than the standard deviation of the coefficient estimates from all periods.)

#### 1.4.2 DiD: 2008:Q4 job destruction by typical raise quarter

To implement this identification strategy, I begin with a difference-in-differences estimation of the job destruction rate in 2008:Q4. Firms that typically raise wages in Q4 comprise the control group. Firms that typically raise wages in Q1, Q2, and Q3 comprise the three distinct treatment groups. Q1 and Q2-raising firms should have greater exposure to DNWR since, not anticipating the financial collapse, they are more likely to have raised their workers' nominal wages earlier in the year. Of

Figure 1.1: Period-specific Q2 vs. Q4-raiser differential job destruction coefficient estimates



*Notes:* Coefficient estimates for the differential job destruction rate at firms with typical raise quarters in Q2 versus Q4 after controlling for firm-specific seasonality, as well as fixed effects for 2-digit NAICS sector by time, firm age, and firm size. The red dot indicates the coefficient estimate for 2008:Q4. The dashed grey lines represent  $\pm 2$ -standard deviations from the zero mean difference. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0069-CED-20190725.

these treatment groups, the Q2-raising firms should have the greatest exposure, since inflation would have had more time to eat away at the nominal wage bill of Q1-raising firms. The relative degree of exposure to DNWR of the treatment group composed of Q3-raising firms is more ambiguous, since some Q3-raising firms may have observed the start of the financial collapse (which began in late 2008:Q3) before deciding whether to raise their workers' nominal wages - thus giving them a degree of nominal wage flexibility similar to that of the Q4-raising control group.

The difference-in-difference specification is:

$$JD_{kt} = D_{kt}^{2008:Q4} \theta^{Q4} + \sum_{q=1}^3 D_{kt}^{2008:Q4} D_{kt}^{Qq \text{ Raiser}} \theta^{Qq} + \mathbf{X}_{kt} \beta_t + \epsilon_{kt} \quad (1.2)$$

where  $JD_{kt}$  is firm  $k$ 's DHS job destruction rate in period  $t$ ,<sup>8</sup>  $D_{kt}^{2008:Q4}$  is an indicator variable equal to one if  $t = 2008:Q4$ ,  $D_{kt}^{Qq \text{ Raiser}}$  is a set of indicator variables equal to one if firm  $k$ 's typical raise quarter equals calendar quarter  $q$ , and  $\mathbf{X}_{kt}$  is a set of control variables that includes firm-specific calendar quarter fixed effects, as well as fixed effects for industry by time, firm age, and firm size. The coefficients of interest are  $\theta^{Q1}$  and  $\theta^{Q2}$ . These coefficient estimates indicate whether firms with greater exposure to DNWR destroyed jobs at a higher rate when confronted with a negative aggregate shock.

I estimate this difference-in-differences specification on all firms in the primary sample for which I identify the firm as having a typical raise quarter. Table 1.3 shows the results of this difference-in-differences estimation without industry-by-time fixed effects (Model 1), with industry-by-time fixed effects (Model 2), and with industry-by-time fixed effects and employment weighting (Model 3). First, as expected given the severity of the financial crisis, Q4-raising firms increased their rates of job destruction by 1.4 percentage points in 2008:Q4. Second, across all specifications, I find strong evidence that exposure to DNWR significantly increased firms' job destruction. Specifically, Q2-raising firms, which had the greatest exposure to DNWR because of their nominal raise schedule, increased their job destruction rates by an additional 1.3 percentage points (or 2.0 percentage points when weighting by em-

---

<sup>8</sup>I compute the DHS rate of change measure proposed by Davis, Haltiwanger, Schuh et al. (1998) for a firm's quarterly job destruction rate, as defined by the Census Bureau's Quarterly Workforce Indicators. Specifically:

$$JD_{kt} = \max \left[ 0, \frac{\text{start of quarter employment} - \text{end of quarter employment}}{0.5 \text{ start of quarter employment} + 0.5 \text{ end of quarter employment}} \right]$$

The DHS rate of change has the advantages of both being symmetric (unlike percent changes) and capable of handling zero values (unlike logs). It is also bounded between -2.0 and 2.0.

ployment). This implies that the firms most constrained by DNWR because of their typical raise quarter increased their job destruction rates by nearly twice as much as the least-constrained Q4-raising firms. The results for Q1 and Q3-raising firms are more ambiguous, with statistical significance depending on the model’s control variables and employment-weighting.

Table 1.3: Difference-in-differences job destruction by typical raise quarter

Dependent Variable	Job Destruction Rate		
	(1)	(2)	(3)
2008:IV * Q4-Raiser	<b>1.48***</b> (0.16)		
	Relative to Q4-Raiser		
2008:IV * Q1-Raiser	0.34 (0.28)	0.24 (0.25)	<b>1.02***</b> (0.18)
2008:IV * Q2-Raiser	<b>2.11***</b> (0.21)	<b>1.30***</b> (0.26)	<b>2.01***</b> (0.20)
2008:IV * Q3-Raiser	<b>0.70**</b> (0.26)	0.26 (0.24)	-0.22 (0.14)
Industry*Time FE	No	Yes	Yes
Employment Weighted	No	No	Yes
Mean JD Rate	8.6%	7.7%	4.9%
Observations	5.7M	5.6M	5.6M
R-Squared	0.28	0.29	0.38

*Note:* The outcome variable is the SEIN-level DHS job destruction rate. This is regressed on a set of control variables, a 2008:Q4 dummy variable, and this dummy variable interacted with a set of dummy variables indicating firms with typical raise quarters in Q1, Q2, or Q3. The control variables include firm-specific seasonal fixed effects as well as dummy variables for firm age and firm size. Models (2) and (3) also include fixed effects for two-digit industry by time. Quarterly LEHD data from 1999:Q1 to 2014:Q4. Sample includes only firms with one (and no more) typical raise quarter. Robust standard errors clustered at the SEIN-level. \*\*\*, \*\*, \* indicate statistical significance at the 0.1%, 1.0%, and 5.0% levels, respectively. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0069-CED-20190725.

### 1.4.3 A further test of the parallel trends assumption

A key concern for the parallel trends assumption is the possibility that seasonal factors in firms' business conditions could be correlated with both the firm's historical typical raise quarter and the firm's exposure to the financial collapse. This concern is reinforced by the fact that nominal wage raises in certain industries appear to be clustered in particular calendar quarters (e.g. firms in professional services and FIRE tend to raise workers' wages in Q4, whereas construction and accommodation firms cluster their typical raise quarters in Q2; see Table 1.2 for the full breakdown). Even though the difference-in-differences specification controls for both industry-specific shocks in 2008:Q4 and firm-specific seasonality, it is still possible that unobserved factors that determine a firm's historical raise patterns also affect the magnitude of the shock that the firm experienced in 2008:Q4.

To address this concern, I extend the difference-in-difference estimation model to determine whether the job destruction rate at Q2-raising firms responded more strongly than at Q4-raising firms to a given-sized change in revenue. If exposure to DNWR is the root cause of Q2-raising firms choosing to destroy more jobs, then I should expect Q2-raising firms to respond more strongly than Q4-raising firms to similar magnitude negative revenue changes in 2008:Q4. The regression model I estimate is:

$$JD_{kt} = \mathbf{D}_{kt}^{2008:Q4} \left( \theta^{Q4} + \mathbf{R}_{kt} \gamma^{Q4} + \mathbf{D}_{kt}^{Q2 \text{ raiser}} \left( \theta^{Q2} + \mathbf{R}_{kt} \gamma^{Q2} \right) \right) + \mathbf{X}_{kt} \beta_t + \epsilon_{kt} \quad (1.3)$$

where the outcome variable,  $JD_{kt}$ , is the firm's DHS job destruction rate;  $\mathbf{D}_{kt}^{2008:Q4}$  is an  $n \times n$  diagonal matrix where  $n$  is the number of observations and each diagonal element equals one if  $t=2008:Q4$ ;  $\mathbf{R}_{kt}$  is an  $n \times 2$  matrix where the first column corresponds to the firm's year-over-year DHS real revenue change if it is positive (and zero otherwise), and the second column is the absolute value of the DHS revenue change if it is negative (and zero otherwise);  $\mathbf{D}_{kt}^{Q2 \text{ raiser}}$  is an  $n \times n$  diagonal matrix with each diagonal element equal to one if firm  $k$ 's typical raise quarter is the second calendar quarter; and  $\mathbf{X}_{kt}$  is a set of control variables that includes  $\mathbf{R}_{kt}$ ,  $\mathbf{R}_{kt} \mathbf{D}_{kt}^{Q2 \text{ raiser}}$ , firm-specific seasonal fixed effects, industry-by-time fixed effects, and dummy variables for firm age and firm size.

The coefficient of interest is the  $\gamma^{Q2}$  coefficient for negative revenue change, since this indicates whether Q2-raising firms had stronger responses than Q4-raising firms to a given-sized negative revenue shock. My coefficient estimate from this difference-in-differences framework is likely to be biased due to a combination of: i) measurement error resulting from the annual measure of revenue change spanning a longer time range than the quarterly measure of job destruction, and ii) simultaneity in the relationship between changes in employment levels and changes in revenue (since, even absent any demand or productivity shocks, it is standard to assume that a firm's labor inputs contemporaneously affect its revenue). In Appendix A.1 I argue that despite these sources of bias, the coefficient estimate is still informative as to whether Q2-raising firms responded more strongly than Q4-raising firms to negative revenue changes in 2008:Q4. First, I discuss how the similarity of Q4-raising firms' response to negative revenue changes in 2008:Q4 relative to other

periods indicates that the bias from measurement error was similar across periods (and thus is compensated for by the difference-in-differences estimation strategy). Second, I show that the difference-in-difference framework implies that the  $\gamma^{Q2}$  coefficient estimate is only biased if the Q2-raising firms had a differential response to negative revenue shocks in 2008:Q4. Furthermore, I demonstrate that, given standard assumptions about the relationship between revenue and employment, the simultaneity bias attenuates the coefficient estimate, resulting in an under-estimate of the true differential response of Q2-raising firms to negative revenue shocks.

There are two significant changes in the sample used for this estimation. First, annual revenue data is only available in the U.S. Census Bureau's Revenue-Enhanced Longitudinal Business Database (rLBD) from 2002. Second, a large fraction of SEINs are not linked to EINs with revenue data in the rLBD. The mean job destruction rates of the original and more restricted samples are similar (7.7% and 7.0%, respectively).

Model (2) in Table 1.4 reports the results of this difference-in-differences estimation with the additional revenue change variables and industry-by-time fixed effects (I include column (2) of Table 1.3 as Model (1) for comparison purposes, since it uses the same specification with industry-by-time fixed effects and no employment weighting, but without the revenue change variables).

There are three important takeaways from this table. First, in periods other than 2008:Q4, the job destruction rate at both Q2 and Q4-raising firms responded similarly and strongly to negative year-over-year revenue changes. For every 1% fall in annual revenue, job destruction rose approximately 0.165 percentage points at

Table 1.4: Differential Q2 vs Q4-raiser job destruction rate & annual revenue change

	Job Destruction Rate	
	(1)	(2)
Q4-Raiser * $\Delta^+$ Revenue		<b>2.47***</b> (0.13)
Q4-Raiser * $\Delta^-$ Revenue		<b>16.48***</b> (0.12)
2008:IV * Q4-Raiser * $\Delta^+$ Revenue		-0.52 (0.82)
2008:IV * Q4-Raiser * $\Delta^-$ Revenue		1.02 (0.76)
	Relative to Q4-Raiser	
2008:IV * Q2-Raiser	<b>1.30***</b> (0.26)	0.06 (0.36)
Q2-Raiser * $\Delta^+$ Revenue		<b>-0.66***</b> (0.17)
Q2-Raiser * $\Delta^-$ Revenue		<b>-0.68***</b> (0.16)
2008:IV * Q2-Raiser * $\Delta^+$ Revenue		1.15 (1.89)
2008:IV * Q2-Raiser * $\Delta^-$ Revenue		<b>7.68***</b> (1.70)
Industry*Time FE	Yes	Yes
Employment Weighted	No	No
Mean JD Rate	7.7%	7.0%
Observations	5.6M	1.6M
R-Squared	.29	.36

*Note:* The outcome variable is the SEIN-level DHS job destruction rate. Controls include fixed effects for firm-specific seasonality, firm age, firm size, and industry-by-time. Specification (4) also includes measures of positive ( $\Delta^+$ Revenue) and negative ( $\Delta^-$ Revenue) annual revenue changes, interacted with the firm's typical raise quarter. Quarterly LEHD data from 2002:Q1 to 2014:Q4. Robust standard errors clustered at the SEIN-level. \*\*\*, \*\*, \* indicate statistical significance at the 0.1%, 1.0%, and 5.0% levels, respectively. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0069-CED-20190725.

Q4-raising firms and 0.158 percentage points at Q2-raising firms.

Second, in 2008:Q4, there was no statistically significant change in the response

of job destruction to either positive or negative year-over-year revenue changes at Q4-raising firms. Similarly, in 2008:Q4, the response of the job destruction rate at Q2-raising firms did not change for firms experiencing positive year-over-year revenue changes (which is what I would expect if DNWR is what drives the differential response of Q2-raising firms).

Last, and most importantly, the responsiveness of the job destruction rate at Q2-raising firms to negative year-over-year revenue changes increased by 48%, rising from a 0.158 percentage point increase in the job destruction rate for every 1% decline in revenue to a 0.235 percentage point increase in 2008:Q4. These results are consistent with the hypothesis that differences in the calendar quarter in which firms historically tended to raise their workers' wages caused firms' job destruction rates to respond differently to similar magnitude shocks in 2008:Q4.

#### 1.4.4 IV: Raise schedules and the real wage bill - exploring the mechanism

According to the reasoning laid out in Section 1.4.1, if downward nominal wage rigidity causally affects a firm's rate of job destruction by constraining the firm's ability to cut workers' nominal wages, then the firm will have a higher job destruction rate in response to an unanticipated negative shock if the firm's real wage bill is exogenously above its optimal level. Although it is impossible to observe a firm's optimal real wage bill, the firm's average four-quarter real wage bill is a reasonable benchmark, since the annual staggering of nominal wage raises implies

that firms should set the nominal wage such that the expected average real wage over the year equals its optimal level. Thus, for each firm-quarter observation, I measure the ratio  $W_{kt}$  of the firm's real wage bill in the previous period relative to its four-quarter moving average as follows:

$$W_{kt} = \frac{\sum_{i \in E_{kt}^{FY}} w_{ikt-1}^r}{\sum_{i \in E_{kt}^{FY}} \sum_{s=1}^4 w_{ikt-s}^r / 4} \quad (1.4)$$

where  $E_{kt}^{FY}$  is the set of full-year workers (workers who have been full-quarter employees at the firm for each of the last four quarters)<sup>9</sup> and  $w_{ikt-s}^r$  is the real wage of the worker in period  $t - s$  (in 2015:Q1 dollars, computed using the Employment Cost Index (ECI) from the Bureau of Labor Statistics).

The reduced form relationship of interest explores whether a firm's job destruction rate in 2008:Q4 is higher when its start-of-quarter real wage bill ratio is higher. Specifically,

$$JD_{kt} = \gamma W_{kt} + \gamma^{2008:Q4} d_{kt}^{2008:Q4} W_{kt} + \mathbf{X}_{kt} \beta_t + \epsilon_{kt} \quad (1.5)$$

where  $JD_{kt}$  is the firm's DHS job destruction rate,  $d_{kt}^{2008:Q4}$  is an indicator variable equal to one in 2008:Q4, and  $\mathbf{X}_{kt}$  is a set of control variables that includes firm fixed-effects, as well as sets of dummy variables for firm age, firm size, and two-digit industry fixed effects for 2008:Q4.

A simple OLS regression of the model in Equation 1.5 is unlikely to yield a

---

<sup>9</sup>Restricting the real wage ratio to include only workers who have been employed for the entire previous year ensures a consistent measure of relative wages but biases the measure to represent the real wage ratio of longer-tenure workers.

causal estimate of the effect of having a higher real wage bill in 2008:Q4 due to a combination of measurement error in real wages and persistent unobserved confounders affecting both past wages and current-period job destruction (see Appendix A.2 for a detailed discussion of these endogeneity issues and the expected direction of the bias). To estimate the causal effect of DNWR on a firm’s rate of job destruction, I employ an instrumental variables strategy using the firm’s historical pattern of nominal wage raises. As instrumental variables, I construct the employment-weighted share of a firm’s nominal wage raises that occurred in each of the last three calendar quarters in previous years (prior to 2007:Q4). For instance, in 2008:Q4, I construct three instrumental variables for each firm:  $r_{kt-1}^Q$ ,  $r_{kt-2}^Q$ , and  $r_{kt-3}^Q$ , which correspond to the historical employment-weighted share of nominal raises occurring in calendar quarters Q3, Q2, and Q1 respectively (as a share of all raises).

To construct these raise share variables, I calculate the probability that a full-quarter employee receives a nominal raise at the firm for each calendar quarter (Q1 to Q4) in the period before 2007:Q4 ( $p_k^q$  where  $q$  indicates the calendar quarter). Then I calculate the historical employment-weighted share of nominal raises for each calendar quarter  $q$  as:

$$s_k^q = \frac{p_k^q}{\sum_{a=1}^4 p_k^a} \quad (1.6)$$

These historical raise share measures for the four calendar quarters sum to one for every firm.<sup>10</sup> Finally, I convert these employment-weighted historical raise shares

---

<sup>10</sup>I use the historical share of raises instead of the historical probability of a raise in a given calendar quarter. This is because firms that were historically doing well would also have been more likely to raise workers’ nominal wages. Given that business conditions persist over time, the historical probability of a raise in a given calendar quarter is likely to be correlated with the current business conditions and thus would not address the omitted variable problem.

into three firm-quarter specific measures of the historical raise shares in quarters  $t - 1$ ,  $t - 2$ , and  $t - 3$ . For instance, if  $t$  falls in the fourth calendar quarter, then I define the instrumental variables:  $r_{kt-1}^Q = s_k^3$ ,  $r_{kt-2}^Q = s_k^2$ ,  $r_{kt-3}^Q = s_k^1$ .

Using these instrumental variables, I estimate the following first-stage relationships for the endogenous explanatory variables  $W_{kt}$  and  $d_{kt}^{2008:Q4}W_{kt}$ :

$$W_{kt} = \sum_{a=1}^3 \alpha_a r_{kt-a}^Q + \sum_{a=1}^3 \alpha_a^{2008:Q4} d_{kt}^{2008:Q4} r_{kt-a}^Q + \mathbf{X}_{kt} \beta_t + \nu_{kt} \quad (1.7)$$

and similarly for  $d_{kt}^{2008:Q4}W_{kt}$ . Table 1.5 reports the results of these first-stage regressions for the set of firms with at least ten observed nominal raises prior to 2007:Q4. These results are consistent with the theory that a firm's real wage bill spikes in the quarter in which it historically tends to raise workers' nominal wages and steadily declines over the next three quarters.<sup>11</sup> There does not appear to be a weak instruments problem for either of the endogenous explanatory variables.

Table 1.6 shows the results of estimating the reduced form relationship of interest from Equation 1.5 with both OLS and 2SLS. The 2SLS estimation finds that firms' rate of job destruction rose 3.6 percentage points in 2008:Q4 for every 1% that firms' start-of-quarter real wage bill was above their four-quarter average real wage bill due to the firms' historical raise schedules.<sup>12</sup>

<sup>11</sup>To give a sense of the magnitude of variation in firms' real wage bill over the calendar year, it is easiest to consider the special cases where 100% of a firm's historical raises occurred in the calendar quarter in  $t - 1$ ,  $t - 2$ , or  $t - 3$ . In such cases, the firm's real wage bill at the start of period  $t$  is higher by 4.1%, 3.2%, and 1.6% in the first, second, and third quarters after the firm's historical raise quarter. In 2008:Q4, however, the real wage bill increase was more muted, with the firm's real wage bill only being 2.1%, 1.7%, and 0.7% higher in the first, second, and third quarters after the firm's historical raise quarter. This more muted relationship in 2008:Q4 is consistent with the fact that the recession began in January 2008.

<sup>12</sup>This is most likely an underestimate of the effect of DNWR on job destruction since it includes

Table 1.5: First-stage: Start-of-quarter / 4-Q lag moving average real wage bill

Dep Var: $W_{kt}$ = Start-of-Quarter / 4Q Lag Moving Average Real Wage Bill				
Model:	$W_{kt}$ (2a)	$d_{kt}^{2008:Q4}W_{kt}$ (2b)	$W_{kt}$ (4a)	$d_{kt}^{2008:Q4}W_{kt}$ (4b)
Raise Share ( $r_{kt-x}^Q$ )				
1Q Lag	<b>4.06***</b> (0.03)	$2 \times 10^{-4}$ ( $1 \times 10^{-4}$ )	<b>3.91***</b> (0.07)	<b><math>2.2 \times 10^{-3}</math>**</b> ( $7 \times 10^{-4}$ )
2Q Lag	<b>3.19***</b> (0.03)	$-3 \times 10^{-4}$ ( $2 \times 10^{-4}$ )	<b>3.09***</b> (0.09)	$2.1 \times 10^{-3}$ ( $1.4 \times 10^{-3}$ )
3Q Lag	<b>1.56***</b> (0.02)	$2 \times 10^{-4}$ ( $2 \times 10^{-4}$ )	<b>1.60***</b> (0.07)	$4 \times 10^{-4}$ ( $1.2 \times 10^{-3}$ )
2008:Q4 * 1Q Lag	<b>-2.14***</b> (0.17)	<b>2.14***</b> (0.18)	-0.81 (0.35)	<b>3.08***</b> (0.39)
2008:Q4 * 2Q Lag	<b>-1.58***</b> (0.17)	<b>1.66***</b> (0.17)	<b>-1.05***</b> (0.31)	<b>1.94***</b> (0.30)
2008:Q4 * 3Q Lag	<b>-0.86***</b> (0.17)	<b>0.68***</b> (0.17)	-0.43 (0.33)	<b>1.02***</b> (0.31)
Employment Weighted	N	N	Y	Y
Firm FE	Y	Y	Y	Y
F-Test (clustered SE)	4444	33.45	596	15.2
Kleibergen-Paap rk LM		33.4		14.2
Andersen-Rubin Wald Test		342		18.6
Observations	7.07 million			
Clusters	161,000 firm clusters			

*Note:* The outcome variable is the SEIN-level real wage ratio. Control variables include firm fixed-effects, as well as fixed effects for two-digit NAICS industry in 2008:Q4, firm age, and firm size. Quarterly LEHD data from 1999:Q1 to 2014:Q4. Sample includes only firms with at least ten raises observed prior to 2007:Q4. Robust standard errors clustered at the SEIN-level. \*\*\*, \*\*, \* indicate statistical significance at the 0.1%, 1.0%, and 5.0% levels, respectively. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0073-CED-20190910.

To give a sense of the aggregate magnitude of this estimate, I consider the counterfactual scenario in which all firms had the nominal wage flexibility of Q4- as an instrumental variable the share of raises that historically occurred in Q3. Since some of these Q3-raising firms would have observed the Lehman Brothers' bankruptcy that occurred on September 15th, 2008, some Q3-raising firms may have endogenously frozen more of their workers' wages and thus been even less exposed to DNWR than the Q4-raising firms. If, instead, I use only Q1 and Q2 raise shares as instrumental variables and include the Q3 raise share as a control variable in 2008:Q4, then a 1% increase in the real wage bill ratio is estimated to increase the firm's job destruction rate by 7.8 percentage points. See Appendix A.3 for the alternative second-stage regression results.

Table 1.6: Second-stage: Job destruction rate

Dependent Variable:	Firm DHS Job Destruction Rate			
Estimator:	OLS	IV	OLS	IV
Model:	(1)	(2)	(3)	(4)
Real Wage Bill Ratio				
$W_{kt}$	<b>-0.19***</b> (0.004)	<b>1.26***</b> (0.04)	<b>-0.13***</b> (0.02)	<b>-0.67**</b> (0.22)
2008:Q4 * $W_{kt}$	<b>-0.15***</b> (0.03)	<b>3.56***</b> (0.55)	0.04 (0.07)	<b>3.20***</b> (0.83)
Employment Weight	N	N	Y	Y
Firm FE	Y	Y	Y	Y
R-Squared	0.050		0.054	
Observations	7.07 million			
Clusters	161,000 firm clusters			

*Note:* The outcome variable is the SEIN-level DHS job destruction rate. This is regressed on the predicted real wage bill ratio and the predicted real wage bill ratio in 2008:Q4, as well as a set of control variables. The control variables include firm fixed effects as well as dummy variables for firm age, firm size, and two-digit industry-specific shocks in 2008:Q4. Quarterly LEHD data from 1999:Q1 to 2014:Q4. Sample only includes firms with at least ten nominal raises prior to 2007:Q4. Robust standard errors clustered at the SEIN-level. \*\*\*, \*\*, \* indicate statistical significance at the 0.1%, 1.0%, and 5.0% levels, respectively. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0073-CED-20190910.

raising firms in 2008:Q4. This is equivalent to using the coefficient estimates from the second-stage of the IV regression to calculate the job destruction rate at firms if all historical raise share values are set to zero (and thus the firm is a Q4 raiser). When I do this simple adjustment to eliminate the exposure to DNWR generated by the annual staggering of firm's nominal raise schedules, I find that the job destruction rate would have risen 23% less in 2008:Q4 (which was the quarter with the most job destruction since job destruction became available in the Quarterly Workforce Indicators in 1993). I should note that this estimate serves as a lower bound of the effect of DNWR on job destruction in 2008:Q4, since even the less-exposed

Q4-raising firms (which serve as the baseline comparison group) had exposure to DNWR.<sup>13</sup>

An important caveat regarding the external validity of my causal estimate of the effect of DNWR on job destruction is that the quasi-experiment examines the largest unanticipated negative aggregate shock experienced by the United States in at least the last 35 years. The aggregate implications of the estimated effect of DNWR on job destruction may be very different in other recessions for which the negative shocks are likely to be smaller in magnitude and short-term financing may be more readily available. Similarly, the aggregate dynamics resulting from DNWR may be very different in more stable periods, when only a small set of firms may be without the financial resources necessary to smooth large unanticipated negative shocks.

#### 1.4.5 Job destruction: More layoffs, not less hiring

There are two potential channels by which exposure to DNWR could generate job destruction. Namely, the decline in employment between the start and end of the quarter could result from some combination of more layoffs and less hiring of replacements for retiring and quitting workers. I now evaluate which of these channels drove the large increase in job destruction at firms with greater exposure to DNWR in 2008:Q4. I do this by re-estimating the difference-in-differences specification described in Section 1.4.2, changing the outcome variable to be either the

---

<sup>13</sup>Two additional concerns are: one, this is a partial equilibrium estimate of the aggregate effect of DNWR since my firm-level IV specification does not take into account any general equilibrium effects. And two, this aggregate estimate does not fully capture the effect of DNWR on job destruction since my instruments only capture one-dimension of firms' exposure to DNWR.

firms' hiring rate or layoff rate.<sup>14</sup> The results in Table 1.7 show that the layoff rate at Q2-raising firms rose by 1.44 percentage points more than at Q4-raising firms. This estimate is statistically significant and similar in magnitude to the 1.30 percentage point rise in the job destruction rate at Q2-raising firms relative to Q4-raising firms. The estimate for the change in the hiring rate at Q2-raising firms, however, is not statistically different from that of Q4-raising firms; while it is large in magnitude, it is of the wrong sign to explain a rise in job destruction. Thus, the greater increase in job destruction rates at Q2-raising firms relative to Q4-raising firms in 2008:Q4 comes from more workers being laid off at Q2-raising firms, and not from a decline in replacement hiring.

#### 1.4.6 Variation in worker layoff risk from DNWR exposure

Given the substantial effect that DNWR has on firms' rates of job destruction, it is also informative to explore whether particular worker characteristics expose employees to higher layoff risk when their employer is constrained by DNWR. I use a similar difference-in-differences framework as described in Section 1.4.2 but now at the level of worker-firm pairs. The outcome of interest is whether a worker is laid off, defined as any instance when the worker either: i) transitions from employment in the current quarter to non-employment in the next quarter, or ii) switches from one employer in this quarter to a new employer either in this quarter or the next, but where the earnings gap between the two jobs exceeds one month of earnings.

---

<sup>14</sup>Since the LEHD data set does not contain the reason for separation, I label a job separation as a "layoff" in any case where either: i) the worker experienced an employment-to-nonemployment transition (so has at least one full quarter of non-employment post separation), or ii) experienced a same-quarter or adjacent-quarter employer-to-employer transition where the earnings gap between jobs was at least one month (see Haltiwanger, Hyatt, Kahn and McEntarfer (2018a)).

Table 1.7: Differential Q2 vs Q4-raiser employment outcomes in 2008:Q4

Dep Var:	Job Destruction (1)	Layoffs (2)	Job Creation (3)	Hiring (4)
	Relative to Q4-Raiser			
2008:Q4 * Q2-Raiser	<b>1.30***</b> (0.26)	<b>1.44*</b> (0.66)	0.22 (0.12)	2.04 (2.79)
Industry*Time FE	Yes	Yes	Yes	Yes
Employment Weighted	No	No	No	No
Mean Rate	7.7%	9.6%	5.8%	25.3%
Observations	5.6M	5.6M	5.6M	5.6M
R-Squared	0.29	0.24	0.34	0.18

*Note:* The outcome variable is the SEIN-level DHS job destruction, layoff, job creation, and hiring rates. These are regressed on a set of control variables, a 2008:Q4 dummy variable, and this dummy variable interacted with a set of dummy variables indicating firms with typical raise quarters in Q1, Q2, or Q3. The control variables include firm-specific calendar quarter fixed effects as well as dummy variables for firm age, firm size, and two-digit industry by time. Quarterly LEHD data from 1999:Q1 to 2014:Q4. Sample only includes firms with one (and no more) typical raise quarter. Robust standard errors clustered at the SEIN-level. Robust standard errors clustered at the SEIN-level. \*\*\*, \*\*, \* indicate statistical significance at the 0.1%, 1.0%, and 5.0% levels, respectively. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0069-CED-20190725.

(This measure of job transitions is derived from Haltiwanger, Hyatt, Kahn and McEntarfer (2018a)). I augment the difference-in-difference estimation with a large set of worker characteristics that include the worker’s sex, race, education, age group, tenure group, and log earnings. These worker characteristics are fully interacted with an indicator variable for 2008:Q4 and a set of dummy variables for the employer’s typical raise quarter. Thus, I estimate:

$$L_{ikt} = \mathbf{C}_{ikt} \left[ \alpha^4 + \mathbf{D}_{kt}^{2008:Q4} \theta^4 + \sum_{q=1}^3 \left( \mathbf{D}_{kt}^{\text{Qq-raiser}} \alpha^q + \mathbf{D}_{kt}^{2008:Q4} \mathbf{D}_{kt}^{\text{Qq-raiser}} \theta^q \right) \right] + \mathbf{X}_{kt} \beta_t + \epsilon_{kt}$$

where  $L_{ikt}$  is an indicator variable equal to one if worker  $i$  was laid off from firm  $k$  in period  $t$ ;  $\mathbf{C}_{ikt}$  is the matrix of worker characteristics;  $\mathbf{D}_{kt}^{2008:Q4}$  is a diagonal matrix with values equal to one if  $t=2008:Q4$ ;  $\mathbf{D}_{kt}^{\text{Qq-raiser}}$  is a diagonal matrix with values equal to one if firm  $k$  typically raises wages in quarter  $q$ ; and  $\mathbf{X}_{kt}$  is a set of firm-level control variables that includes dummy variables for firm age, firm size, and industry-by-time fixed effects, as well as firm-by-calendar-quarter fixed effects (for firm-specific seasonality).

Table 1.8 reports the results of this regression. This table shows that less-educated workers and workers hired prior to the start of the recession (i.e. before 2008:Q1) but within the last three years had higher layoff risk because of DNWR. These results are consistent with firms differentially laying off lower productivity workers when the firms are constrained by DNWR, since these workers have either fewer years of education or less time to accumulate firm-specific human capital. Conditional on observables, higher-paid workers are more likely to be laid off, which is consistent with firms choosing to lay off workers with lower firm surplus.

That older workers (age 61-70) and black and multi-racial workers are disproportionately exposed to layoff risk is not surprising given the greater cyclical volatility of these groups' employment rates. It is surprising, however, to find that younger workers (under age 35) have slightly lower layoff risk relative to middle-aged workers (age 36-60).

Table 1.8: Differential layoff rate in 2008:Q4 by worker characteristic

Worker Layoff Rate at Q2-Raising Firms in 2008:Q4 (Relative to Q4-Raising Firms)		
	Coefficient	Standard Error
Log Earnings	<b>0.76***</b>	(0.05)
Sex		
Male	Baseline	
Female	<b>-1.67***</b>	(0.10)
Education		
Less than High School	<b>1.76***</b>	(0.34)
High School	Baseline	
Some College	<b>-1.03***</b>	(0.26)
College	<b>-1.47***</b>	(0.30)
Race		
White	Baseline	
Black	<b>1.10***</b>	(0.17)
American Indian	0.22	(0.47)
Asian	<b>-0.7***</b>	(0.22)
Native American	0.6	(0.89)
Two or More Races	<b>1.6***</b>	(0.37)
Age		
18 to 20	<b>-0.98***</b>	(0.25)
21 to 25	<b>-0.55**</b>	(0.20)
26 to 30	<b>-0.75***</b>	(0.20)
31 to 35	<b>-0.88***</b>	(0.20)
36 to 40	<b>-0.06**</b>	(0.20)
41 to 45	Baseline	
46 to 50	0.21	(0.20)
51 to 55	0.14	(0.21)
56 to 60	-0.02	(0.22)
61 to 65	<b>0.92**</b>	(0.25)
66 to 70	<b>2.80***</b>	(0.33)
Tenure		
2 quarters	<b>-5.2***</b>	(0.20)
3 quarters	<b>0.4*</b>	(0.20)
1 year	<b>0.65**</b>	(0.22)
2 years	Baseline	
3 years	<b>-0.65***</b>	(0.16)
4 years	<b>-0.96***</b>	(0.19)
5 years	<b>-0.92***</b>	(0.21)
6-10 years	<b>-1.59***</b>	(0.16)
11+ years	<b>-3.07***</b>	(0.19)
R-Squared	0.068	
Observations	49.7M	

Outcome Variable: Worker level layoff rate (mean quarterly layoff rate among start-of-quarter workers is 5.5%), where layoff is defined as either an employment-to-nonemployment transition or an employment-to-employment transition with an earnings gap of at least one month. The set of control variables includes dummy variables for firm age, firm size, and industry-by-time fixed effects, as well as firm-by-calendar-quarter fixed effects. Standard errors clustered at the SEIN-level. Quarterly LEHD data from 1999:Q1 to 2014:Q4. \*\*\*, \*\*, \* indicate statistical significance at the 0.1%, 1.0%, and 5.0% levels, respectively. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0069-CED-20190725.

## 1.5 Summary

This chapter argues that downward nominal wage rigidity plays an important causal role in explaining employment fluctuations through the job destruction margin. First, the chapter presents quasi-experimental evidence that in 2008:Q4, the job destruction rate increased twice as much at firms with greater exposure to DNWR due to the timing of their historical raise schedules relative to the unanticipated financial collapse in September 2008. I find that the increase in the aggregate job destruction rate in 2008:Q4 would have been 23% smaller if all firms had had the wage flexibility of firms whose annual raise schedules occurred in the fourth calendar quarter. Since this estimate does not account for the fact that the Q4-raising firms may also have had exposure to downward nominal wage rigidity, it is a lower-bound estimate of the effect of downward nominal wage rigidity on job destruction in 2008:Q4.

This chapter leaves unanswered several important questions about the relationship between downward nominal wage rigidity and job destruction. First, although I show that greater exposure to downward nominal wage rigidity causally increased firms' rates of job destruction during the Great Recession, the instrument I use (variation in the historical seasonality of firms' wage raises) does not capture a firm's full exposure to DNWR. Thus, while the effect of DNWR on job destruction that I identify is large, this estimate serves as only a lower bound for the true causal effect of DNWR on job destruction. Second, the quasi-experiment in this chapter focuses on the large unanticipated negative aggregate shock at the onset

of the Great Recession, a period in which firms faced unusual financial exigencies. This brings into question the external validity of the magnitude of the aggregate effect. While firms experiencing large negative shocks in more “normal” recessions are likely to respond in a similar fashion to the firms in my instrumental variables estimation, there will be fewer such firms if the recession is less severe. As a result, I expect downward nominal wage rigidity to generate less aggregate job destruction when the recession is less severe.

There are two important implications of this chapter for monetary policy. First, regarding the Federal Reserve’s target inflation rate, studies examining the optimal inflation rate have largely ignored the potential for downward nominal wage rigidity to inefficiently destroy jobs.<sup>15</sup> Thus, it would be informative to examine whether and how much the optimal rate of inflation changes once the inefficient job destruction caused by downward nominal wage rigidity is incorporated into a model designed to identify the optimal rate of inflation. One complexity in modeling the effect of downward nominal wage rigidity in a high-inflation environment is that the variation in firms’ exposure to downward nominal wage rigidity that I use in the quasi-experiment could actually be exacerbated in a high-inflation environment since the real wage change over the calendar year would be greater.

Second, the finding that exposure to downward nominal wage rigidity causes firms to destroy positive-surplus jobs has implications for the asymmetric response of employment and output to contractionary versus expansionary aggregate shocks.

---

<sup>15</sup>Kim and Ruge-Murcia (2009); Coibion, Gorodnichenko and Wieland (2012); Mineyama (2018); Dupraz, Nakamura and Steinsson (2019)

Many of the DSGE and labor search-and-matching models that explore these asymmetric responses tend to ignore the possibility that downward nominal wage rigidity affects employment through the job destruction margin. It would be informative to explore how the results change when the models include the effect of downward nominal wage rigidity on the job destruction margin. Importantly for monetary policy, asymmetric responses of employment and output to aggregate shocks due to the effect of downward nominal rigidity on job destruction also have implications for the effectiveness of contractionary versus expansionary monetary policy shocks.

## Chapter 2: Cyclical Labor Market Sorting<sup>1</sup>

### 2.1 Introduction

It is commonly said that during and after recessions, overqualified workers get stuck in low-paying jobs. Studies by Kahn (2010) and Oreopoulos, Von Wachter and Heisz (2012) have shown that college graduates obtain relatively low-skill jobs during labor market downturns. This disconnect between workers and their best job matches is what Barlevy (2002) called the “sullyng” effect of recessions. Barlevy (2002) also emphasized that, during labor market downturns, a lower rate of voluntary quits for better employment can cause workers to spend more time in worse matches.

Such a sullyng effect of recessions contrasts with the more conventional “cleansing” effect.<sup>2</sup> This mechanism suggests that, during economic downturns, the least productive jobs are destroyed. This cleansing mechanism implies that the remaining jobs will be (at least relatively) more productive. There are thus two plausible competing channels for how economic downturns might affect job match quality. However, little is known empirically about how economic downturns affect the qual-

---

<sup>1</sup>This chapter contains the empirical analysis from my working paper titled *Cyclical Labor Market Sorting*, coauthored with Leland Crane and Henry Hyatt.

<sup>2</sup>See Caballero and Hammour (1994). Note that Barlevy (2002) considered both the cleansing and sullyng effects of recessions.

ity distributions of workers and firms, and the sorting of workers between firms.

In this paper, we provide evidence on the cleansing and sullyng effects of recessions on workers, firms, and sorting in the labor market. We use matched employer-employee data to implement several methods of ranking workers and firms to establish how labor market sorting (i.e., the degree to which low- vs. high-rank workers work at low vs. high-rank firms) varies over the business cycle. We find that, regardless of the ranking method, recessions are times when the employment distribution shifts towards high-rank workers. This cleansing effect on the worker distribution is fairly intuitive. Somewhat more surprising are the firm quality dynamics. We find evidence of a sullyng effect on the firm quality distribution. The firm quality distribution shifts down in recessions, as low-rank firms take a larger share of employment. Although several mechanisms are at work, positive sorting strengthens during recessions.

We present evidence on how labor market sorting varies over the business cycle. To do so, we make use of the insights of many contributions on sorting in the labor market that exploit the unique properties of universe-level linked employer-employee data. We implement four methods of ranking workers and firms using quarterly linked employer-employee data for 11 U.S. states for 1994-2014. Each of these methods involves ordering workers and firms along a univariate, time-invariant ranking.<sup>3</sup> In other words, we assume that workers and firms are of high or low intrinsic rank along a single dimension. We start with methods that rank workers

---

<sup>3</sup>See Lindenlaub and Postel-Vinay (2016) for a multi-dimensional model of worker and firm sorting.

and firms independently from each other. We rank workers based on the time spent in employment vs. nonemployment, as well as by their average earnings when working. Following Bagger and Lentz (2019), we rank firms based on their share of hires from poaching. Motivated by the recent work of Bartolucci, Devicienti and Monzón (2018) and Haltiwanger, Hyatt and McEntarfer (2018b), we also rank firms by labor productivity (revenue per worker, with industry adjustments to capture differences in value added). We also rank workers and firms by assuming that earnings are an additive function of a worker effect and a firm effect as in Abowd et al. (1999). Finally, we implement a ranking algorithm that follows Hagedorn, Law and Manovskii (2017) and Lopes de Melo (2018), whose methods are motivated by labor market search models. In total, we implement four methods of ranking workers and firms on our linked employer-employee data. We focus on cyclical changes in composition and sorting using employment-weighted terciles (i.e., low, middle, and high) of the worker and firm rank distribution.

All four methods of ranking workers and firms yield qualitatively similar results on worker composition, firm composition, and sorting. Low-rank workers are most affected by labor market downturns. Although both low-rank and high-rank workers have fewer net flows from nonemployment in worse labor markets, changes in the nonemployment transition rate are more severe for low-rank workers. Thus, recessions are times when the composition of the workforce shifts away from low-rank workers. Every percentage point increase in the unemployment rate is associated with a 0.114 to 0.449 percentage point decline in the employment share of work-

ers ranked in the lowest tercile.<sup>4</sup> This result can be characterized as a cleansing effect. During labor market downturns, the most productive workers are most able to compete for scarce jobs. Many potential job opportunities for low-rank workers are no longer profitable in worse states of the economy. This result echoes work by Oi (1962) and Van Ours, Ridder et al. (1995) on the relative cyclicity of worker employment by skill and education.

During labor market downturns, sullyng effects drive changes in firm composition. Especially in times of high unemployment that follow recessions, the employment share of low-rank firms increases. Every additional percentage point of the unemployment rate above its HP trend is associated with a 0.054 to 0.063 percentage point increase in the employment share of firms ranked in the lowest tercile. We show the central importance of the cyclical job ladder, which drives the observed countercyclical increase in employment at low-rank firms through the differential poaching margin response of low versus high-rank firms.<sup>5</sup> We find that the net nonemployment hiring of low-rank and high-rank firms adjust similarly in times of high unemployment. Each additional percentage point of the unemployment rate above its HP trend is associated with a decline in net hiring from nonemployment of 0.102 to 0.122 percentage points for low-rank firms, and 0.091 to 0.109 percentage points for high-rank firms. This differential nonemployment response of 0.010 to 0.028 percentage points favors high-rank firms. However, this difference is small

---

<sup>4</sup>All results in this paper are quarterly unless otherwise noted.

<sup>5</sup>The cyclical job ladder references the procyclical rate at which workers voluntarily quit their jobs for better matches. This process by which workers obtain a series of better matches is often called “climbing the job ladder.” For a review of the literature on the cyclical job ladder, see Moscarini and Postel-Vinay (2018).

compared to the difference between the net poaching responses of low-rank and high-rank firms. For low-rank firms, net hires from poaching *increase* by 0.060 to 0.076 percentage points, while those of high-rank firms *decrease* by 0.035 to 0.065 percentage points. This differential poaching response of 0.111 to 0.130 percentage points strongly favors low-rank firms. Therefore, the increase in the employment share of low-rank firms in times of high unemployment can be attributed to changes in net poaching flows. Our paper links the cyclical job ladder, considered by Haltiwanger, Hyatt, Kahn and McEntarfer (2018a), Haltiwanger, Hyatt and McEntarfer (2018b), and Moscarini and Postel-Vinay (2018), to employment composition by firm rank.<sup>6</sup> During labor market downturns, workers spend more time in worse jobs.

Cyclical changes in labor market sorting naturally follow from these composition changes. We explore the frequency with which workers of different ranks are employed at firms of different ranks. Labor market downturns are times when low-rank workers are less likely to work at high-rank firms. Specifically, a one percentage point increase in the unemployment rate is associated with a 0.041 to 0.181 percentage point decrease in the share of low-rank workers at high-rank firms. This change for low-ranked workers is driven by a slowdown in the job ladder: differential changes in net poaching flows into low- vs. high-ranked firms are larger than differential changes in net nonemployment flows. This decline in the share of low-rank workers at high-rank firms strengthens the agreement between worker rank

---

<sup>6</sup>Of papers on the cyclical job ladder, ours is most similar to Haltiwanger, Hyatt and McEntarfer (2018b) who rank workers based on education and firms by within-industry productivity in order to study transitions in the 2007-2009 recession, along with the expansions that precede and follow it. Haltiwanger, Hyatt, Kahn and McEntarfer (2018a) focus on the cyclical transition rates, and do not consider aggregate composition or directly measure the degree of agreement between worker and firm ranks.

and firm rank. By contrast, high-rank workers are more likely to work at low-rank firms during labor market downturns. A one percentage point increase in the unemployment rate is associated with a 0.023 to 0.098 percentage point increase in the share of high-rank workers at low-rank firms. The slowdown in the job ladder drives this change. The countercyclical increase in the share of high-rank workers at low-rank firms weakens the agreement between worker rank and firm rank. Overall, the mechanisms that strengthen labor market sorting dominate, and the agreement between worker rank and firm rank increases slightly. This increase in rank agreement is more apparent during recessions than in the times of high unemployment that follow recessions. Overall, our paper provides evidence of small, countercyclical increases in positive sorting between workers and firms.

## 2.2 Data

### 2.2.1 Source data

The Longitudinal Employer-Household Dynamics (LEHD) linked employer-employee data allows us to explore the cyclical behavior of labor market composition and sorting. These are records of earnings disbursements collected as part of unemployment insurance reporting that cover nearly all private sector employment as well as state and local (but not federal) government workers; see Abowd, Stephens, Vilhuber, Andersson, McKinney, Roemer and Woodcock (2009). We use these data to link workers and firms over time. Because different states enter the LEHD microdata at different times, we use a consistent set of eleven states with

data available from 1994-2014.<sup>7</sup>

Recent enhancements to the LEHD data have facilitated the measurement of employer-to-employer transitions. We follow the approach to measuring employer-to-employer transitions in Hyatt et al. (2014).<sup>8</sup> This involves considering the set of jobs (i.e., distinct employer-employee combinations) that span two consecutive quarters. A worker’s “dominant job” is the employer at which that worker earns the most among all such consecutive quarter jobs. Following those definitions, when a worker’s dominant employer changes without a gap in earnings, the worker undergoes an employer-to-employer transition. If the worker has one or more quarters without earnings, then any flows into or from employment are considered flows from and into nonemployment, respectively.

## 2.2.2 Ranking workers and firms

We rank workers and firms in four different ways, roughly following different strands of the literature on labor market sorting. We provide here a brief overview of each of the methods of measuring the extent and cyclicity of sorting.<sup>9</sup> All ranks are calculated on an employment-weighted basis, and use real 2014 dollars.

We start by ranking workers and firms based on simple summary statistics. Our first method ranks workers and firms in ways that do not rely directly on observed earnings. We rank firms based on the share of new hires that come from

---

<sup>7</sup>These states are California, Colorado, Idaho, Illinois, Kansas, Maryland, Montana, North Carolina, Oregon, Washington, and Wisconsin.

<sup>8</sup>For exact definitions, see Appendix A.

<sup>9</sup>For additional details on our ranking methods, see Appendix B.

other firms vs. from nonemployment, following Bagger and Lentz (2019).<sup>10</sup> A firm’s poaching (i.e., employer-to-employer transition) hires as a share of all hires is a rough metric for how desirable a firm is as an employer. This measure, in principle, reflects wage and salary compensation, as well as nonwage amenities. To rank workers, we use the fraction of their careers that they spend in employment vs. nonemployment. We count workers who are more frequently employed as being more productive.<sup>11</sup> Specifically, we regress employment on a set of year of birth by quarter dummies, separately by gender, and then rank workers based on the average of the residuals from that regression. This method also yields ranks that have a straightforward interpretation in the model of Lise and Robin (2017). These methods identify both the workers who are more likely to encounter a productive match with the firms operating in the economy, and the firms who are likely to offer workers a more productive job.

Our second method ranks workers by their earnings and firms by labor productivity. To rank workers, we use the average residual from regressing earnings on year of birth by quarter dummies. Note that this measure of average regression-adjusted worker earnings also provides the initial guess of a worker’s rank in our third and fourth ranking methods. For firms, we use revenue from the U.S. Census Bureau’s Business Register, in the spirit of the recent work by Haltiwanger, Jarmin,

---

<sup>10</sup>Appropriate caution in interpreting our results is warranted because Bagger and Lentz (2019) do not consider aggregate uncertainty.

<sup>11</sup>This method serves as a measure for worker quality because workers with less to gain from working might spend less time doing so. Bagger and Lentz (2019) use unemployment duration as a method of ranking workers in Section 4.2.1 of their paper, although they do not place as much emphasis on this method of ranking workers as they do their poaching hire method of ranking firms.

Kulick and Miranda (2017).<sup>12</sup> We use this firm-level revenue data to calculate a firm’s average deviation over time from the employment-weighted industry average revenue per worker. We then obtain a measure of labor productivity by adding this firm-level measure to industry-level value added per worker as published by the Bureau of Economic Analysis.

Our third method jointly ranks workers and firms using a model that assumes earnings are an additive function of a firm effect and worker effect, as in Abowd, Kramarz and Margolis (1999). This specification has recently been used by Card, Cardoso and Kline (2016) to measure the degree of sorting in the labor market. To overcome estimation issues that follow from the fact that we only observe workers at different parts of their life-cycles, we first regress earnings on a set of year of birth by quarter in time dummies (e.g., born in 1965 and working in 1997Q1). Then, following Guimaraes and Portugal (2010), we apply an iterative method to the residuals from this regression to identify worker effects, firm effects, and updated birth cohort by quarter effects.

Fourth, we apply a technique inspired by the recent work of Hagedorn, Law and Manovskii (2017) and Lopes de Melo (2018). These techniques provide solutions to the inconsistency between the identification assumptions of Abowd, Kramarz and Margolis (1999) and standard models of labor market search.<sup>13</sup> This technique

---

<sup>12</sup>Differences between our revenue measure and that of Haltiwanger, Jarmin, Kulick and Miranda (2017) include: i) our use revenue data starting in 1994 for all industries, and ii) our imputation of missing data, see Appendix Section B.2.2.2.

<sup>13</sup>Readers should note that the random search models proposed by Hagedorn, Law and Manovskii (2017) and Lopes de Melo (2018) do not consider aggregate uncertainty. Therefore, appropriate caution is required in interpreting our cyclical results as they do not have a direct interpretation in the context of the models that correspond to their ranking strategies. However, we think that the computational strategies proposed by these authors, which make intensive use of linked employer-

involves initially ranking workers by their average lifetime earnings, but then re-ranks workers who are employed at the same firm to maximize the likelihood that a worker at a firm who is ranked as more productive than another worker actually earns more from that employer. Firms are afterwards ranked by measuring the minimum earnings received by workers of a given rank, and then taking the difference between earnings received and this implied reservation wage. Firms with a greater difference between earnings paid and the reservation wage have a greater surplus from a match and are considered more productive.

## 2.3 Empirical evidence on composition and sorting

### 2.3.1 Overview and notation

In this section, we document how the sorting of workers into firms of different ranks varies over the business cycle. We seek to characterize how the composition of employed workers and firms, as well as labor market sorting, varies with labor market conditions. We have several outcomes of interest: the share of employment that workers and firms of different ranks constitute, and the relative frequency of particular combinations of worker and firm ranks (i.e., the degree of sorting). We also measure the worker flows into and from nonemployment and poaching flows across firms that account for these changes in shares. For these exercises, we characterize the health of the labor market using the difference of the unemployment rate from its HP trend, as well as the first difference in the unemployment rate, fol-  
employee data, are helpful in assessing the robustness of our overall findings.

lowing Haltiwanger, Hyatt, Kahn and McEntarfer (2018a). These transformations of the unemployment rate serve as our cyclical indicators. The first-difference of the unemployment rate surges during NBER recessions. The difference in unemployment from its HP trend is a measure of times of low vs. high unemployment. We rank firms and workers into three terciles: low, middle, and high based on an employment-weighted ranking of workers and firms across all quarters.

We introduce some notation to document how employment evolves over time, which builds on the framework of Haltiwanger et al. (2018a). Let  $E_{ijt}$  denote the number of workers of rank tercile  $i$  working at firms of rank tercile  $j$  at time  $t$ .<sup>14</sup> Employment for each worker  $i$ , firm  $j$  bin changes from time  $t - 1$  to  $t$  due to separations to nonemployment  $N_{ijt}^s$ , hires (accessions) from nonemployment  $N_{ijt}^a$ , separations from poaching (i.e., employer-to-employer transitions)  $P_{ijt}^s$ , and poaching hires  $P_{ijt}^h$ .<sup>15</sup> Specifically, the change in employment can be expressed as

$$\Delta E_{ijt} = E_{ijt} - E_{ijt-1} = N_{ijt}^a - N_{ijt}^s + P_{ijt}^a - P_{ijt}^s. \quad (2.1)$$

The change in employment for any worker-firm group can be expressed as the sum of net hires from nonemployment  $N_{ijt}^a - N_{ijt}^s$  and net hires from poaching  $P_{ijt}^a - P_{ijt}^s$ . We further express the sum of workers of rank  $i$  across firms of any rank at time  $t$  as  $E_{i\bullet t}$ , and analogously express totals for firm rank  $j$  as  $E_{\bullet jt}$ . Total employment at time  $t$ ,  $E_{\bullet\bullet t}$  is written  $E_t$ . Note that poaching flows do not change the total employment of any worker rank and so  $\Delta E_{i\bullet t} = N_{i\bullet t}^a - N_{i\bullet t}^s$ . This is because an

---

<sup>14</sup>Throughout Section 2.3, the terms “worker rank” and “firm rank” refer their respective terciles.

<sup>15</sup>For formal definitions, see Appendix B.1.3.

employer-to-employer transition implies a separation of a worker of rank  $i$  from one employer and a hire of a worker of that same rank at a different employer. Net poaching flows, however, can affect the composition of firms.

## 2.3.2 Worker and firm composition

### 2.3.2.1 Worker composition

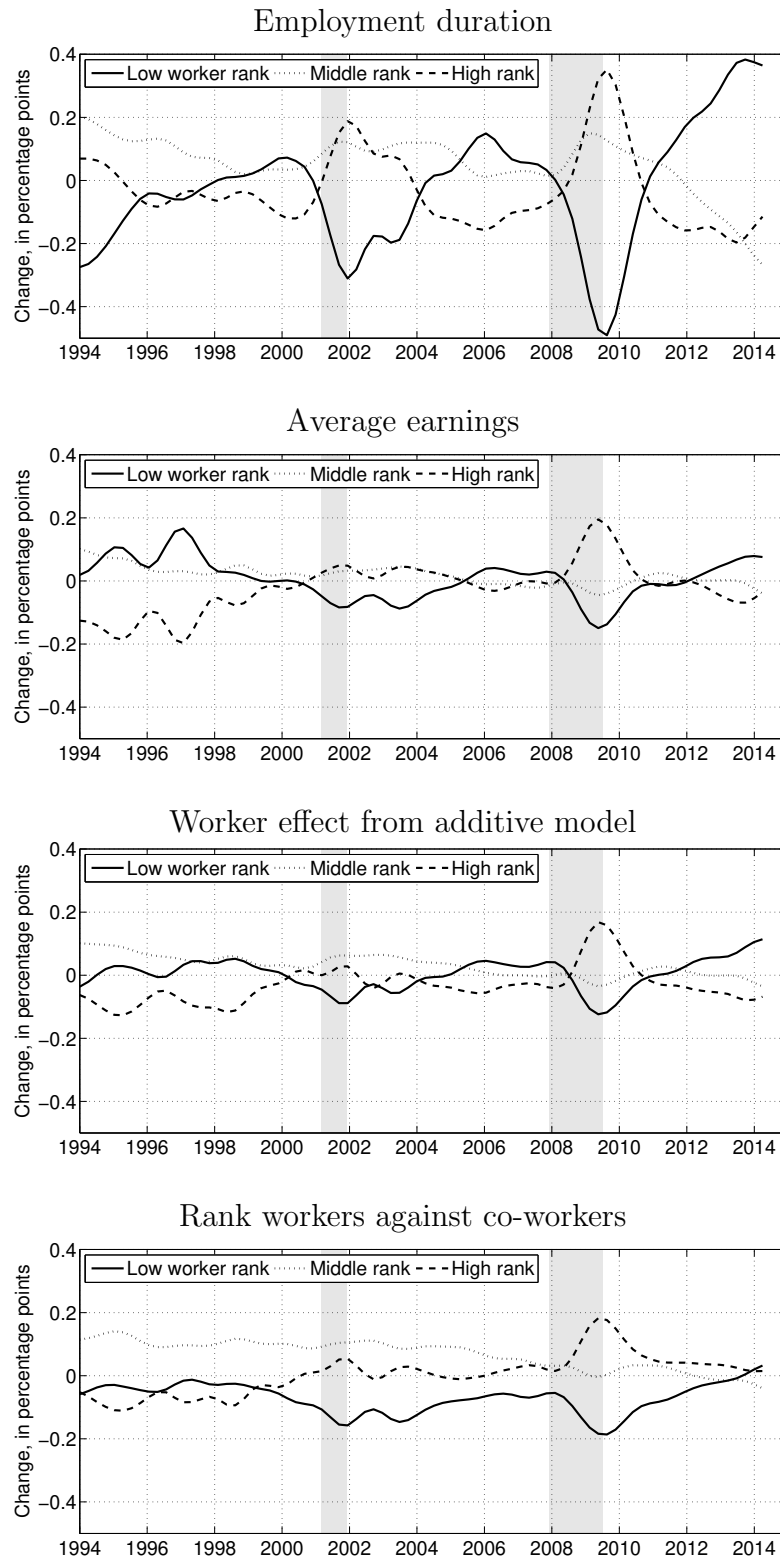
We now document how worker composition evolves over time. By construction, low-, middle-, and high-rank workers will on average each have a share of one-third. However, in any given quarter the shares of employment in these terciles can differ from one-third. From one quarter to another, workers with a time-invariant rank enter and leave employment, and these transitions determine how the employment shares of these different groups evolve over time. For example, if more workers of high-rank enter employment than other groups, the high-rank group will gain a share of employment.

The evolution of the employment shares of low, middle, and high-rank workers is shown in Figure 2.1. We plot the quarterly change in the share of employment of workers of different ranks, using each of our four ranking methods. In terms of the notation introduced in Section 2.3.1, we plot  $E_{i\bullet t}/E_t - E_{i\bullet t-1}/E_{t-1}$ .<sup>16</sup> For example, Panel 2.1(a) shows how the employment shares for each tercile change when we rank workers by their employment duration. A positive value indicates that a rank tercile gains employment share.

---

<sup>16</sup>The changes in the shares for each of the three groups add to exactly zero prior to seasonal adjustment.

Figure 2.1: Changes in worker rank shares



*Notes:* Shaded regions indicate recessions. Data seasonally adjusted and Henderson-filtered using X11.

In 2005Q4 (during an economic expansion), the share of low-rank workers increased by 0.15 percentage points, the share of middle-rank workers increased by 0.01 percentage points, and the share of high-rank workers declined by 0.16 percentage points. It is apparent from Figure 2.1 that the middle and late periods of economic expansions are times when low-rank workers gain as a share of employment, and the share of high-rank workers declines. During and after recessions, the employment share of high-rank workers increases, as that of low-rank workers decreases.

Cyclical changes in worker composition are similar in direction across the different ranking methods, but the largest changes are found when we rank workers by their employment duration as in Panel 2.1(a). Panel 2.1(b) shows how the shares of employment evolve when workers are ranked by average earnings. Panel 2.1(c) shows how the shares evolve when workers are ranked based on the worker effect from our additive model of earnings with worker and firm effects. Panel 2.1(d) shows how composition evolves for workers initially ranked by average earnings, but then re-ranked to ensure that more productive workers at the same firm earn more than their less productive co-workers. Overall, the changes in the shares of these terciles are very small, with the share of workers moving up or down by less than 0.5 percentage points over the span of a quarter. The largest movements occur around the two recessions, where the share of workers in the highest tercile increases, largely at the expense of workers in the lowest tercile.

Table 2.1 shows how the shares of employment by worker rank change with our cyclical indicators. Specifically, we regress  $E_{i\bullet t}/E_t - E_{i\bullet t-1}/E_{t-1}$  on our seasonally-

Table 2.1: Changes in worker rank shares and the unemployment rate

Worker tercile	Employment duration	Average earnings	Additive model worker effects	Rank workers vs. co-workers
<i>Difference in unemployment from its HP trend</i>				
Low	-11.2*** (2.7)	-3.0** (1.4)	-3.2** (1.2)	-2.7*** (1.0)
High	7.4*** (2.0)	2.1 (1.4)	2.4** (1.1)	2.2** (0.9)
<i>First-difference of the unemployment rate</i>				
Low	-44.9*** (5.0)	-13.9*** (3.2)	-12.6*** (2.7)	-11.4*** (2.3)
High	31.6*** (3.9)	16.9*** (2.7)	14.8*** (2.2)	12.9*** (1.7)

*Note:* Estimates from regressing the change in share of employment on the seasonally-adjusted unemployment rate, seasonal dummies, and a time trend. \*, \*\*, and \*\*\* indicate statistical significance at 10%, 5%, and 1%, respectively. Standard errors are in parentheses. To avoid excessive decimal places, the dependent variables range from  $[-100, 100]$ , while the cyclical indicators range from  $[-1, 1]$ .

adjusted cyclical indicators, seasonal dummies, and a time trend.<sup>17</sup> This table summarizes the cyclical features of Figure 2.1. Changes in worker composition are greatest during recessions, rather than the times of high unemployment that follow recessions. Consistent with these features of Figure 2.1, point estimates in Table 2.1 are greater in magnitude for the first-difference of the unemployment rate than the deviation of the unemployment rate from its HP trend. Specifically, a one percentage point increase in the unemployment rate is associated with a decline of 0.114 to 0.449 percentage points in the share of low-rank workers, and a 0.129 to 0.316 percentage point increase in the share of high-rank workers. For every additional

<sup>17</sup>Similar specifications have been used to measure the cyclicalities of job ladders in the labor market by, among others, Haltiwanger et al. (2018a).

percentage point that the unemployment rate is above its HP trend, the low-rank worker share declines by 0.027 to 0.112 percentage points, and the high-rank share increases by 0.021 to 0.074 percentage points. Also consistent with Figure 2.1, workers ranked by employment duration show larger cyclical changes than when ranked by other methods.

Table 2.2: Net nonemployment hiring by worker rank and unemployment

Worker tercile	Employment duration	Average earnings	Additive model worker effects	Rank workers vs. co-workers
<i>Difference in unemployment from its HP trend</i>				
Low	-21.4*** (5.1)	-13.5*** (3.3)	-13.9*** (2.9)	-13.1*** (2.8)
High	-1.7 (1.6)	-7.7*** (2.7)	-8.0*** (2.4)	-8.3*** (2.3)
<i>First-difference of the unemployment rate</i>				
Low	-83.0*** (10.0)	-56.4*** (6.0)	-51.3*** (5.5)	-48.7*** (5.3)
High	-2.8 (4.0)	-27.8*** (6.1)	-23.0*** (5.8)	-25.6*** (5.4)

*Note:* Estimates from regressing the change in employment on the seasonally-adjusted unemployment rate, seasonal dummies, and a time trend. \*, \*\*, and \*\*\* indicate statistical significance at 10%, 5%, and 1%, respectively. Standard errors are in parentheses. To avoid excessive decimal places, the dependent variables range from  $[-100, 100]$ , while the cyclical indicators range from  $[-1, 1]$ .

Table 2.2 explores the transition dynamics that underlie these cyclical shifts in employment composition by worker rank. Specifically, it shows how net hiring from nonemployment changes with labor market conditions. The net nonemployment variable we define is  $(N_{i\bullet t}^a - N_{i\bullet t}^s)/((E_t + E_{t-1})/2)$ .<sup>18</sup> Keeping the denominator on the same scale as in Table 2.1 allows us to sum nonemployment rates across

<sup>18</sup>Appendix Figure B.1 shows the time series of this measure for each worker group.

worker groups to express the total change in employment and more easily interpret changes in the share of employment across worker groups.<sup>19</sup> The differential between the nonemployment transition rates of high-rank and low-rank workers is, by construction, close to the differential between the changes in the shares for those groups.

Cyclical changes in net nonemployment transitions are concentrated among low-rank workers. When the unemployment rate increases by one percentage point, net nonemployment flows of low-rank workers decline by 0.487 to 0.830 percentage points, while flows of high-rank workers decline by 0.028 to 0.278 percentage points. As with worker composition, changes in net nonemployment flows are more closely aligned to recessions (i.e., the first-difference of the unemployment rate) than times of high unemployment (i.e., HP-detrended unemployment). An additional percentage point deviation of the unemployment rate above its HP trend is associated with a decline of 0.131 to 0.214 percentage points in the share of low-rank workers with a low employment duration, but a decline of only 0.017 to 0.083 percentage points for high-rank workers. This exercise provides insight into the mechanisms that generate cyclical changes in employment shares as documented in Table 2.1. The high-rank worker tercile exhibits a relatively small countercyclical decline in net nonemployment flows, and so its share of employment increases.

Our results on cyclical worker composition tell a consistent story and have an intuitive interpretation. In all four of our ranking methods, recessions are times

---

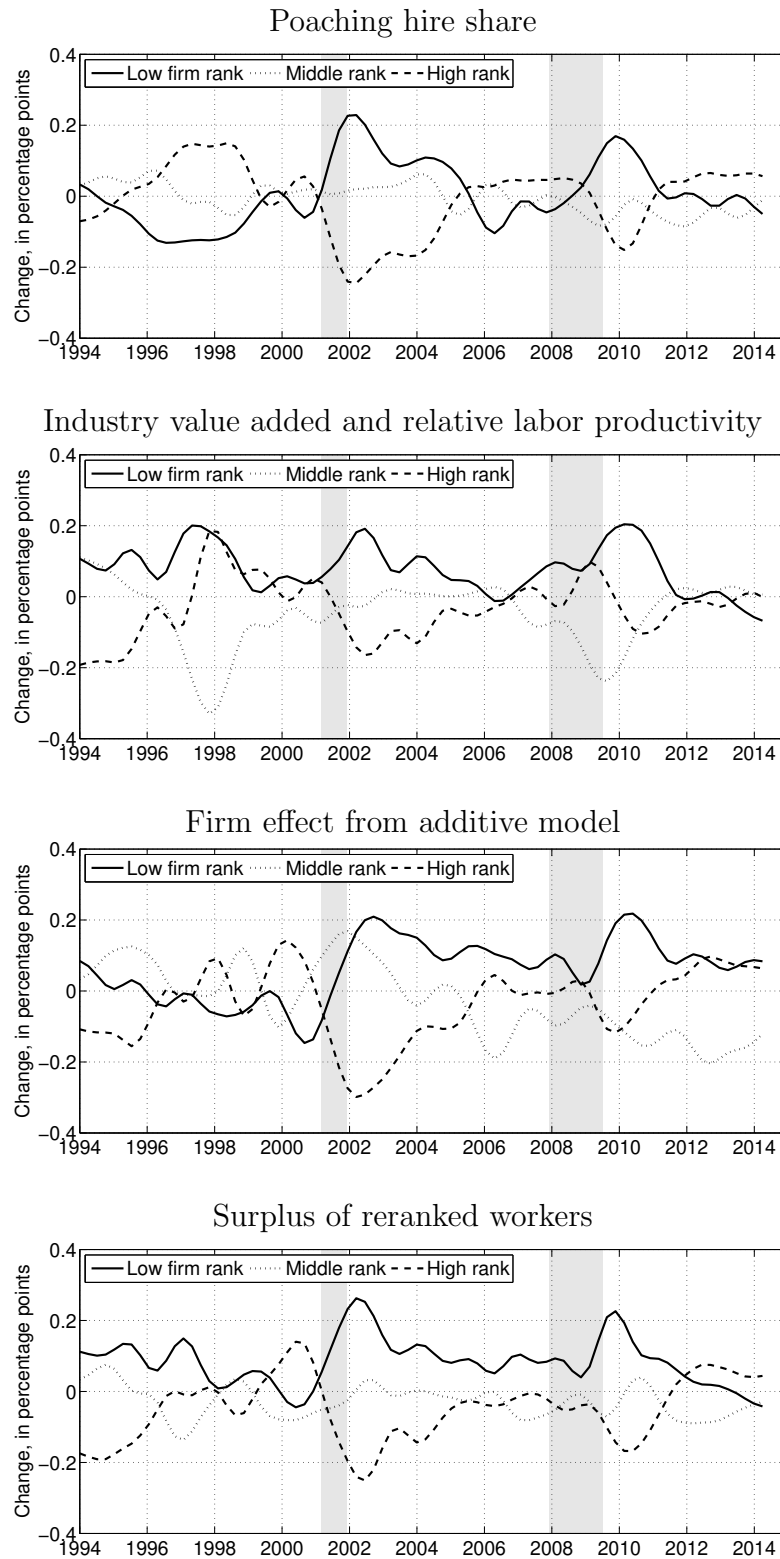
<sup>19</sup>To see this, note that our net nonemployment measure is equivalent to  $(E_{i\bullet t} - E_{i\bullet t-1})/((E_t + E_{t-1})/2)$ .

when the employment distribution shifts away from low-rank workers and towards high-rank workers. This result can be understood as a cleansing effect on the worker distribution. During economic expansions, increasing employment requires hiring relatively unproductive workers. When the economy contracts, there are fewer jobs available. The more productive workers are better able to compete for scarce jobs. Therefore, the employment share of more productive workers increases while that of less productive workers declines.

### 2.3.2.2 Firm composition

We now explore how the employment shares of differently ranked firms change over time. Figure 2.2 shows quarterly changes in firm composition over time, using each of our four ranking methods. In the average quarter, each tercile accounts for one-third of employment, but this share changes over time. We plot the change in employment share  $E_{\bullet jt}/E_t - E_{\bullet jt-1}/E_{t-1}$  for each firm tercile  $j$ . For example, Panel 2.2(a) shows how firm composition evolves when firms are ranked by poaching hire share. In 2005Q4, firms with a relatively low poaching hire share lost 0.09 percent of employment, those with a middle rank gained 0.03 percent of employment, and those with a high poaching share gained 0.02 percent of employment. Panels (b), (c), and (d) of Figure 2.2 shows how firm composition evolves when firms are ranked by labor productivity, the firms' estimated effects from an additive model, or by the match surplus implied by the earnings of workers, respectively.

Figure 2.2: Change in firm rank shares



*Notes:* Shaded regions indicate recessions. Data seasonally adjusted and Henderson-filtered using X11.

Most of the movements are small, with each tercile’s share rarely changing by more than 0.2 percentage points. The exceptions occur during and after each of the two recessions. In expansions, the high-rank firm tercile slowly increases its share of employment, and the share of low-rank firms decreases. During and after the 2001 and 2007-2009 recessions, employment quickly shifts away from high-rank firms and toward low-rank firms.

Table 2.3: Changes in firm rank shares and the unemployment rate

Firm tercile	Poaching share of hires	Labor productivity	Additive worker & firm effects	Surplus of reranked workers
<i>Difference in unemployment from its HP trend</i>				
Low	6.3*** (2.2)	3.2*** (1.7)	5.5*** (1.5)	5.4*** (1.4)
High	-6.9*** (1.8)	-3.9*** (1.4)	-6.6*** (1.8)	-7.2*** (1.7)
<i>First-difference of the unemployment rate</i>				
Low	12.0** (5.5)	14.9** (3.9)	2.0 (3.9)	9.0** (3.6)
High	-8.9* (4.7)	-9.0*** (3.4)	-8.5* (4.7)	-6.6 (4.5)

*Note:* Estimates from regressing the change in share of employment on the seasonally-adjusted unemployment rate, seasonal dummies, and a time trend. \*, \*\*, and \*\*\* indicate statistical significance at 10%, 5%, and 1%, respectively. Standard errors are in parentheses. To avoid excessive decimal places, the dependent variables range from  $[-100, 100]$ , while the cyclical indicators range from  $[-1, 1]$ .

Table 2.3 measures how firm composition varies with the unemployment rate. Each specification regresses the outcome of interest  $E_{\bullet jt}/E_t - E_{\bullet jt-1}/E_{t-1}$  on the unemployment rate (either the difference from its HP trend or its first-difference), as well as a linear time trend and seasonal dummies. Labor market downturns are associated with an increase in the employment share of low-rank firms, and a

corresponding decline for high-rank firms. The change in employment composition by firm rank is qualitatively consistent across ranking methods when we use the HP-detrended unemployment rate as our cyclical indicator, i.e., when we measure times of low vs. high unemployment. For every additional percentage point deviation of the unemployment rate from its HP trend, the employment share of low-rank firms increases by 0.032 to 0.063 percentage points, and that of high-rank firms decreases by 0.039 to 0.072 percentage points.

We find more differences between ranking methods when we consider how firm composition responds to the first-difference of the unemployment rate, i.e., the changes in unemployment that align with recessions. The largest effects are found when we rank firms by their poaching hire share. A one percentage point increase in the unemployment rate is associated with a 0.120 percentage point increase in the share of employment of firms with a low poaching share, and a 0.089 percentage point decrease in that of firms with a high poaching share. Ranking firms by labor productivity, we find that a one percentage point increase in the unemployment rate is associated with a 0.149 percentage point increase in the employment share of low-rank firms, and a 0.090 percentage point decrease in that of high-rank firms. Ranking firms by their additive effects, a one percentage point increase in the unemployment rate increases the employment share of low-rank firms by 0.020 percentage points, and decreases that of high-rank firms by 0.085 percentage points. Using our measure of firm surplus, we find that a one percentage point increase in the unemployment rate increases the employment share of low-rank firms by 0.090 percentage points, and decreases that of high-rank firms by 0.066 percentage points. These ranking

methods usually generate coefficients on the change in unemployment that are of similar sign and magnitude to those generated by the deviation of the unemployment rate from its HP trend. However, they are much less precisely estimated. It is only when we rank firms by their poaching share of hires that we obtain parameter estimates that are in opposite directions and both statistically distinct from zero.

An increase in the employment share of low-rank firms during times of high unemployment is a robust empirical finding, and is consistent with the evidence presented by Haltiwanger et al. (2018a). Note that these four ranking methods rely on three distinct sets of variables. The poaching hire share ranking method uses transitions of workers between firms and from nonemployment to employment. The labor productivity measure uses firm-level revenue and industry-level value added. The other two firm ranking methods rely on earnings. Despite substantial differences in how each ranking method is constructed, each method estimates a firm's rank in the job ladder. We now explore the role of the cyclical job ladder in explaining the countercyclical increase in the employment share of low-rank firms.

Table 2.4 measures how poaching and nonemployment transitions for firms of different ranks vary with the unemployment rate. Our dependent variables are  $(N_{\bullet jt}^a - N_{\bullet jt}^s)/((E_t + E_{t-1})/2)$  for nonemployment and  $(P_{\bullet jt}^a - P_{\bullet jt}^s)/((E_t + E_{t-1})/2)$  for poaching.<sup>20</sup> In weaker labor markets, net hiring from nonemployment declines for both low-rank and high-rank firms. Point estimates for the nonemployment response of low-rank firms are consistently larger in magnitude than for high-rank

---

<sup>20</sup>Appendix Figure B.2 shows the time series of the net nonemployment measure for each firm tercile, and Appendix Figure B.3 shows analogous time series for the net poaching measure.

Table 2.4: Change in net hiring by firm rank and unemployment

Firm tercile	Poaching share of hires	Labor productivity	Additive model firm effects	Surplus of reranked workers
<i>Difference in unemployment from its HP trend</i>				
Nonemployment				
Low	-10.2** (3.9)	-12.0*** (2.5)	-11.9*** (3.2)	-11.9*** (2.9)
High	-9.1*** (2.1)	-10.1*** (2.7)	-10.9*** (2.8)	-10.7*** (2.7)
Poaching				
Low	6.0*** (0.9)	5.1*** (0.9)	6.8*** (1.0)	6.5*** (1.0)
High	-6.5*** (1.0)	-4.4*** (0.8)	-6.2*** (0.9)	-5.9*** (0.9)
<i>First-difference of the unemployment rate</i>				
Nonemployment				
Low	-40.4*** (8.9)	-37.5*** (5.4)	-51.2*** (6.0)	-46.6*** (5.7)
High	-30.1*** (4.5)	-38.0*** (5.8)	-35.1*** (6.3)	-33.3*** (6.1)
Poaching				
Low	12.7*** (2.3)	12.0*** (2.1)	13.7*** (2.6)	14.8*** (2.5)
High	-12.7*** (2.6)	-9.7*** (2.1)	-12.3*** (2.5)	-11.2*** (2.6)

*Note:* Estimates from regressing the change in employment on the seasonally-adjusted unemployment rate, seasonal dummies, and a time trend. \*, \*\*, and \*\*\* indicate statistical significance at 10%, 5%, and 1%, respectively. Standard errors are in parentheses. To avoid excessive decimal places, the dependent variables range from  $[-100, 100]$ , while the cyclical indicators range from  $[-1, 1]$ .

firms, although it is usually not possible to reject equality of the coefficients. When the unemployment rate increases by one percentage point, high-rank firms decrease net hiring from nonemployment by 0.301 to 0.351 percentage points, while low-rank firms increase decrease net hiring by 0.404 to 0.518 percentage points. Nonemployment hiring changes are smaller using HP-detrended unemployment as the cyclical

indicator. Although the point estimates for this response are consistently more negative for low-rank firms, they are not statistically distinct in any ranking method. For every additional percentage point deviation of the unemployment rate above its HP trend, net hiring from nonemployment declines by 0.091 to 0.109 percentage points for high-rank firms, and declines by 0.102 to 0.122 for low-rank firms.

Table 2.4 also measures how net poaching for firms of different ranks vary with the unemployment rate. Here, we find greater differences. Net poaching flows of high-rank and low-rank firms move in opposite directions in response to unemployment. In weaker labor markets, net poaching flows of high-rank firms decline, while net poaching flows of low-rank firms increase. This difference arises because high-rank firms tend to gain employment through poaching, while low-rank firms tend to lose employment through poaching. In weaker labor markets, workers move from low-rank to high-rank firms at a slower pace. When the unemployment rate increases by one percentage point, net hiring through poaching of high-rank firms declines by 0.085 to 0.127 percentage points. For low-rank firms, it increases by 0.119 to 0.148 percentage points. For every additional percentage point deviation of the unemployment rate above its HP trend, net poaching of high-rank firms declines by 0.035 to 0.065 percentage points, and that of low-rank firms increases by 0.060 to 0.076 percentage points.

The transition dynamics in Table 2.4 highlight the role of the cyclical job ladder in changes in employment composition by firm rank. In order for there to be countercyclical increases in the employment share of low-rank firms, the cyclical response of poaching must be greater than that of nonemployment. The net poaching

response dominates in the times of high unemployment that follow recessions. Using the deviation of the unemployment rate from its HP trend as the cyclical indicator yields a differential net poaching response that favors low-rank firms is 0.095 to 0.130 percentage points. Meanwhile, the net nonemployment response, which favors high-rank firms, is only 0.010 to 0.019 percentage points. In the times of high unemployment that follow recessions, there is almost differential net nonemployment response between high-rank and low-rank firms, and so cyclical changes in relative employment are driven by differences in net poaching.

In recessions, as measured by the first difference of the unemployment rate, the effect of the slowdown of the job ladder competes with a larger offsetting effect from nonemployment transitions. Net nonemployment hiring of both low-rank and high-rank firms declines, but the net nonemployment hiring of high-rank firms declines by less. Using the first difference of the unemployment rate, the differential response of the net nonemployment margin of high-rank firms to low-rank firms is 0.103 to 0.216 percentage points.<sup>21</sup> In other words, when unemployment increases, high-rank firms gain employment, on net, from the nonemployment margin. This result is consistent with a cleansing effect of recessions: relatively productive businesses are less affected. However, there is a sullyng effect that offsets this cleaning. The differential response of low-rank firms relative to high-rank firms along the net poaching margin is between 0.204 to 0.260 percentage points.<sup>22</sup> During recessions,

---

<sup>21</sup>These differences are calculated using results presented in Table 2.4 for net nonemployment transitions using the first-difference of the unemployment rate as the cyclical indicator. The smallest differential is found when we rank firms by their poaching hire shares,  $.404 - .301 = 0.103$ , and the largest when we rank firms by labor productivity,  $0.518 - 0.302 = 0.216$ .

<sup>22</sup>These differences are calculated using results presented in Table 2.4 for net poaching that use the first-difference of the unemployment rate as the cyclical indicator. Labor productivity exhibits

the net nonemployment response, which favors high-rank firms, offsets much of the net poaching response, which favors low-rank firms. This analysis therefore helps us understand the results in Table 2.3 that use the first-difference of the unemployment rate as the cyclical indicator. The shift in employment composition from high-rank to low-rank firms begins during recessions, but is most rapid in the years that follow each recession. These are times when output is expanding and unemployment is at a high but relatively stable level.

This relationship between the cyclical job ladder and employment composition also helps us interpret Figure 2.2. One interesting aspect of the countercyclical increases in the employment share of low-rank firms, common to all four firm ranking methods that we employ, is that the increase during the 2001 recession is larger than that of the 2007-2009 recession. This is despite the fact that the latter recession was more severe, both in terms of output and in the associated decline in the health of the labor market. Cyclical changes in employment composition are determined by how firms of different ranks change their behavior with economic conditions. If the primary means of adjustment is via the poaching margin, there will be a larger increase in the employment share of firms that rank lower on the job ladder. Recall that the nonemployment margin favors high-rank firms. If a recession induces a greater differential nonemployment response, there will be less of an increase in the employment share of low-rank firms. In the 2007-2009 recession, the decline in net nonemployment hiring was especially dramatic, and so there was less of an increase

---

the smallest differential  $0.119 + 0.085 = 0.204$ , and the additive and reranking vs. co-worker methods both yield the largest,  $0.137 + 0.123 = 0.148 + 0.112 = 0.260$ .

in the employment share of low-rank firms.<sup>23</sup>

Our results on firm composition tell a consistent story across the four ranking methods. We find that the employment share of low-ranked firms increases in times of high unemployment that follow recessions. This countercyclical sullying of the firm distribution contrasts with the countercyclical cleansing of the worker distribution that we documented in Section 2.3.2.1. The cyclical job ladder has a central role in mediating the increase in the employment share of low-rank firms. The differential employment response of low-rank firms in times of high unemployment is driven by the poaching margin. This finding is consistent with the earlier evidence of Haltiwanger et al. (2018), Haltiwanger, Hyatt, and McEntarfer (2018), and Moscarini and Postel-Vinay (2018) on the cyclical behavior of the job ladder.

### 2.3.3 Cyclical worker-firm rank agreement

We now characterize cyclical sorting in the labor market by measuring the correlation between worker and firm ranks. We move from characterizing cyclical changes in workers and firms considered independently to considering the dynamics of worker-firm rank combinations. Joint worker-firm dynamics largely follow the composition changes that we have documented above.

---

<sup>23</sup>This can be seen in Appendix Figures B.1 and B.2, which show net poaching and net nonemployment hiring for each firm rank tercile. The changes in net poaching are similar in the 2001 and 2007-2009 recessions. However, the nonemployment response to the 2007-2009 recession is much larger than that of the 2001 recession. The countercyclical shift in employment toward low-ranked firms is determined by the differential poaching response (which favors low-rank firms) relative to the differential nonemployment response (which favors high-rank firms). Therefore, the employment share of low-rank firms is greater in the 2001 recession relative to the 2007-2009 recession.

### 2.3.3.1 Employment shares of worker-firm rank combinations

We begin our analysis by considering how the shares of employment for the nine possible combinations of the worker and firm tercile ranks (low, middle, and high) evolve with the labor market conditions. Table 2.5 quantifies how sorting varies with the unemployment rate. Specifically, the dependent variables in our regressions are  $E_{ijt}/E_t - E_{ijt-1}/E_{t-1}$  for each worker tercile  $i$  and firm tercile  $j$ .<sup>24</sup> In weaker labor markets, the employment share of low-rank workers at high-rank firms declines. A one percentage point increase in the unemployment rate is associated with a 0.065 to 0.181 decline in the share of employment of such matches, and an additional percentage point deviation of the unemployment rate above its HP trend is associated with a decline of 0.019 to 0.060 percentage points. In both cases, the effects are largest when workers are ranked by employment duration and firms are ranked by poaching rate. If the share of low-rank workers at high-rank firms declines, this effect increases the correlation between worker and firm rank and therefore strengthens sorting. An analogous change serves to weaken sorting: high-rank workers are more likely to work at low-rank firms in weaker labor markets. A one percentage point increase in the unemployment rate is associated with an increase of the share of employment of high-rank workers at low-type firms of 0.023 to 0.098 percentage points, and an additional percentage point of the unemployment rate above its HP trend is associated with an increased share of 0.017 to 0.026 percentage points. The magnitude of the decline of the share of low-rank workers at high-rank firms is greater than

---

<sup>24</sup>For the cyclicalities of middle-ranked workers and firms, see Appendix Tables B.1 and B.2.

the increase of the share of high-rank workers at low-rank firms, especially during recessions. Therefore, the association between firm rank and worker rank strengthens, on net, among these margins. This suggests a modest increase in worker-firm rank correlation in weaker labor markets.

Changes in the share of similarly ranked workers and firms directly affect worker-firm rank agreement. The share of employment of low-rank workers at low-rank firms shows little cyclical movement. The share of employment of high-rank workers at high-rank firms is highly countercyclical when we use the first-difference of the unemployment rate as our cyclical indicator. The share of employment that consists of high-rank workers at high-rank firms increases by 0.041 to 0.110 percentage points when the unemployment rate increases by one percentage point. This countercyclical increase strengthens positive sorting. The cyclical changes of high-rank workers at high-rank firms are mixed when we use the HP-detrended unemployment rate, with estimates ranging from -0.013 to 0.019 percentage points. An increase in high-rank workers at high-rank firms strengthens the association between worker rank and firm rank during recessions, but contributes less during the times of high unemployment that follow recessions. Overall, the results of Table 2.5 show that the agreement between worker rank and firm rank strengthens more during recessions than during the times of high unemployment that follow.

These cyclical changes in labor market sorting follow from the composition changes that we documented in Sections 2.3.2.1 and 2.3.2.2, and also provide insights into how these composition changes occur. Some countercyclical changes increase the agreement between worker rank and firm rank, while others cause it to decrease.

Table 2.5: Changes in worker-firm rank shares and unemployment

	Employment & poaching share	Earnings & productivity	Additive worker & firm effects	Ranked workers & surplus
<i>Difference in unemployment from its HP trend</i>				
Low-rank firms & Low-rank workers	-0.7 (1.3)	-0.7 (1.0)	0.9 (1.0)	1.1 (0.7)
High-rank workers	2.6*** (0.9)	1.7*** (0.6)	2.0*** (0.5)	1.8*** (0.5)
High-rank firms & Low-rank workers	-6.6*** (1.3)	-1.9*** (0.6)	-3.0*** (0.7)	-3.0*** (0.7)
High-rank workers	1.9** (0.8)	-0.4 (0.7)	-1.2 (1.0)	-1.3* (0.7)
<i>First-difference of the unemployment rate</i>				
Low-rank firms & Low-rank workers	-8.3*** (3.0)	3.4 (2.5)	-0.8 (2.3)	0.9 (1.7)
High-rank workers	9.8*** (1.9)	5.0** (1.4)	2.3* (1.3)	4.6*** (1.3)
High-rank firms & Low-rank workers	-18.1*** (3.1)	-7.7*** (1.4)	-9.1*** (1.6)	-6.5*** (1.7)
High-rank workers	11.0*** (1.6)	4.1*** (1.7)	5.7** (2.3)	4.1** (1.7)

*Note:* Estimates from regressing the change in employment on the seasonally-adjusted unemployment rate, seasonal dummies, and a time trend. \*, \*\*, and \*\*\* indicate statistical significance at 10%, 5%, and 1%, respectively. Standard errors are in parentheses. To avoid excessive decimal places, the dependent variables range from  $[-100, 100]$ , while the cyclical indicators range from  $[-1, 1]$ .

Labor market downturns are times when low-rank workers are less likely to work. The decline in the employment share of low-rank workers is concentrated at high-rank firms. This decline in the share of low-rank workers at high-rank firms increases the agreement between worker rank and firm rank. By contrast, the increase in the employment share of high-rank workers during economic downturns is concentrated at low-rank firms. This countercyclical change weakens positive sorting. The decline of low-rank workers at high-rank firms is larger than the increase of high-rank workers at low-rank firms, and so agreement increases on net. During recessions, the share of employment of high-rank workers at high-rank firms increases, strengthening sorting, but this effect is not as strong in times of high unemployment following recessions. Overall, labor market downturns are times when there is more agreement between worker rank and firm rank, especially during the recessions that initiate these downturns.

### 2.3.4 The poaching and nonemployment margins of sorting

We now explore the role of poaching and nonemployment hiring in generating the changes in employment of particular worker-firm combinations. This analysis can provide insight into what drives countercyclical increases in worker-firm rank agreement. Countercyclical changes in the nonemployment margin are generally driven by cleansing effects. In contrast, cyclical changes related to poaching imply a sully effect. Our results explore the cleansing and sully mechanisms that determine the rank agreement between workers and firms.

Table 2.6: Net nonemployment hires for worker-firm rank shares and unemployment

	Employment & poaching share	Earnings & productivity	Earnings & productivity & firm effects	Additive worker & firm effects	Ranked workers & surplus
<i>Difference in unemployment from its HP trend</i>					
Low-rank firms & Low-rank workers	-57.4*** (16.4)	-43.8*** (8.7)	-41.7*** (9.0)	-40.6*** (8.6)	-40.6*** (8.6)
High-rank workers	-6.8 (11.0)	-25.2** (10.8)	-25.0** (11.1)	-27.6*** (9.1)	-27.6*** (9.1)
High-rank firms & Low-rank workers	-69.5*** (15.2)	-41.8*** (10.8)	-45.8*** (12.2)	-45.7*** (10.1)	-45.7*** (10.1)
High-rank workers	-4.5* (2.7)	-25.4*** (7.9)	-28.7*** (8.4)	-27.6*** (7.9)	-27.6*** (7.9)
<i>First-difference of the unemployment rate</i>					
Low-rank firms & Low-rank workers	-218.6*** (35.2)	-126.1*** (19.9)	-154.6*** (17.4)	-149.0*** (16.6)	-149.0*** (16.6)
High-rank workers	-3.5 (26.9)	-79.0*** (25.8)	-114.5*** (24.9)	-98.8*** (20.7)	-98.8*** (20.7)
High-rank firms & Low-rank workers	-251.1*** (30.6)	-182.7*** (19.7)	-200.6*** (22.8)	-158.4*** (21.0)	-158.4*** (21.0)
High-rank workers	-5.4 (6.6)	-73.7*** (18.7)	-62.1*** (20.9)	-68.1*** (19.3)	-68.1*** (19.3)

*Note:* Estimates from regressing net poaching on the seasonally-adjusted unemployment rate, seasonal dummies, and a time trend. \*, \*\*, and \*\*\* indicate statistical significance at 10%, 5%, and 1%, respectively. Standard errors are in parentheses. To avoid excessive decimal places, the dependent variables range from  $[-100, 100]$ , while the cyclical indicators range from  $[-1, 1]$ .

Table 2.6 shows how the net hiring propensity for each worker-firm combination varies with our cyclical indicators. Specifically, we use  $(N_{ijt}^a - N_{ijt}^s)/((E_{ijt} + E_{ijt-1})/2)$  as our dependent variable. This allows us to assess whether, for workers of a given rank, there is a differential response by firm rank, or whether these changes are spread rather evenly across firms of different ranks.<sup>25</sup> An additional percentage point increase in the unemployment rate is associated with a decline in net nonemployment hiring of low-rank workers into low-rank firms of 1.261 to 2.186 percentage points. This range is similar to the decline in low-rank workers at high-rank firms, which is from 1.584 to 2.511 percentage points. The similar ranges suggest that the employment responses of low-rank workers during a recession are similar at low-rank and high-rank firms. There is some evidence of a cleansing effect that removes high-rank workers from low-rank firms. When we rank workers by employment duration and firms by poaching hire share, we find essentially no response of high-rank workers at either low- or high-rank firms. The other three ranking methods exhibit a decline in high-rank workers at high-rank firms of 0.621 to 0.737 percentage points, but a higher 0.790 to 1.145 percentage points decline at low-rank firms. While these results are small and somewhat mixed, they provide evidence that the cleansing effect of recessions may disproportionately affect worker-firm matches in which the ranks disagree.

However, there is even less evidence of a cleansing effect on worker-firm matches

---

<sup>25</sup>This change in the denominator is needed in order to assess cleansing versus sullyng effects. Although our worker and firm terciles each have one-third of employment on average, this does not imply that the intersections of these tercile groups each has one-ninth of employment. In particular, there are relatively few low-rank workers at high-rank firms. In order to measure the differential response, e.g., of low-rank workers at firms of low-rank vs. high-rank, we need to make this adjustment to our denominator.

in times of high unemployment following recessions. The differential responses for low-rank workers at low-rank vs. high-rank firms are never statistically distinct from zero, nor is there evidence of differential responses for high-rank workers. The ranking methods show a decline in net nonemployment transitions of low-rank workers at low-rank firms of 0.406 to 0.574 percentage points, and of 0.418 to 0.659 percentage points for high-rank firms. For high-rank workers, we find that an additional percentage point deviation of the unemployment rate above its HP trend is associated with a decline in net nonemployment transitions of 0.045 to 0.287 for high-rank firms and 0.068 to 0.276 for low-rank firms. This evidence shows that any cleansing effect on worker-firm rank agreement is concentrated in recessions, rather than the times of high unemployment that follow recessions.

Table 2.7 shows the net poaching response. Specifically, we define our dependent variable as:  $(P_{ijt}^a - P_{ijt}^s)/((E_{ijt} + E_{ijt-1})/2)$ . The net poaching flows of low-rank workers at high-rank firms decline substantially in response to an increase of the unemployment rate. Specifically, a one percentage point increase in the unemployment rate is associated with a decline in net poaching flows for low-rank workers at high-rank firms of 0.452 to 0.771 percentage points. In contrast, net poaching flows of low-rank workers at low-rank firms increase by 0.236 to 0.458 percentage points. This slowdown of the job ladder is substantially different for high-rank workers. Net poaching of high-rank workers at high-rank firms only declines by 0.121 to 0.208 percentage points, while that of low-rank firms declines by 0.177 to 0.404 percentage points.

Table 2.7: Net poaching hires for worker-firm rank shares and unemployment

	Employment & poaching share	Earnings & productivity	Additive worker & firm effects	Ranked workers & surplus
	<i>Difference in unemployment from its HP trend</i>			
Low-rank firms & Low-rank workers	22.1*** (3.1)	16.6*** (3.0)	16.5*** (2.5)	17.0*** (2.6)
High-rank workers	9.1*** (2.1)	28.8*** (4.3)	21.4*** (3.3)	17.1*** (2.7)
High-rank firms & Low-rank workers	-37.6*** (5.2)	-17.0*** (3.7)	-28.5*** (5.0)	-24.1*** (4.2)
High-rank workers	-7.4*** (1.4)	-6.2*** (1.7)	-12.4*** (2.0)	-10.3*** (1.7)
	<i>First-difference of the unemployment rate</i>			
Low-rank firms & Low-rank workers	45.8*** (8.3)	23.6*** (8.3)	34.5*** (6.7)	38.2*** (6.7)
High-rank workers	17.7*** (5.3)	40.4*** (12.8)	31.8*** (9.4)	38.4*** (7.0)
High-rank firms & Low-rank workers	-76.2*** (14.2)	-45.2*** (8.8)	-77.1*** (11.6)	-49.6*** (10.9)
High-rank workers	-13.1*** (3.6)	-12.1*** (4.4)	-19.2*** (5.7)	-20.6*** (4.5)

*Note:* Estimates from regressing net poaching (share of worker-firm employment) on the seasonally-adjusted unemployment rate, seasonal dummies, and a time trend. \*, \*\*, and \*\*\* indicate statistical significance at 10%, 5%, and 1%, respectively. Standard errors are in parentheses. To avoid excessive decimal places, the dependent variables range from  $[-100, 100]$ , while the cyclical indicators range from  $[-1, 1]$ .

Poaching also responds to HP-detrended unemployment. For every additional percentage point deviation of the unemployment rate above its HP trend, the net poaching rate increases by 0.165 to 0.221 percentage points for low-rank firms, and declines by 0.170 to 0.376 for high-rank firms. For high-rank workers, an additional percentage point deviation of the unemployment rate from its HP trend is associated with an increase in the net poaching rate into low-rank firms by 0.091 to 0.288 percentage points, while the net poaching rate into high-rank firms declines by 0.062 to 0.124 percentage points. It is worth noting that the slowdown in movement from low-rank to high-rank firms is greater in magnitude for low-rank workers than for high-rank workers. Therefore, low-rank workers are especially unlikely to move to high-rank firms during recessions. Overall, this evidence suggests that the countercyclical shift of low-rank workers at low-rank firms is driven by a slowdown in the job ladder. As in Haltiwanger et al. (2018b), the fact that this slowdown affects the employment of both low-rank workers and high-rank workers at high-rank firms suggests that workers of all ranks agree on which firms are relatively desirable workplaces.

These results on the cyclical changes in net poaching and net nonemployment flows for worker-firm rank groups help illustrate the role of the cyclical job ladder in increasing and decreasing the agreement between worker and firm ranks. In the times of high unemployment that follow recessions, net poaching basically drives all changes in worker-firm rank agreement. During recessions, net nonemployment transitions have some explanatory effect for changes in employment composition for these worker-firm rank combinations, particularly for high-rank workers

at low-rank firms. For high-rank workers, net nonemployment transitions favor high-rank firms rather than low-rank firms, so this effect offsets the countercyclical change in the poaching margin, which favors low-rank firms. The countercyclical increase in high-rank workers at low-rank firms has an unambiguous interpretation of a sullyng effect. The countercyclical decline in low-rank workers at high-rank firms is due to both differential nonemployment hiring as well as poaching, but the poaching margin explains more of this change.<sup>26</sup> According to our additive model, the nonemployment differential explains at most 29% of the shift in low-rank workers' employment share away from high-ranked firms and towards low-ranked firms.<sup>27</sup> Therefore, the countercyclical decline in the share of low-rank workers at high-rank firms is mostly due to sullyng effects. Most of the countercyclical increase in the agreement between worker rank and firm rank can be attributed to sullyng rather than cleansing effects. The cyclical job ladder drives changes in the agreement of worker ranks and job ranks.

### 2.3.4.1 Worker-firm rank correlation and unemployment

In order to characterize the degree to which sorting varies with the unemployment rate, we also consider the correlation between worker rank and firm rank, and

---

<sup>26</sup>This result suggests that readers should exercise caution in interpreting our finding of an increase in the measured agreement between worker rank and firm rank as itself evidence of a cleansing effect on worker-firm matches. If low-rank workers have higher output at high-rank firms than at low-rank firms, then output can be reduced when low-rank workers are concentrated at low-rank firms.

<sup>27</sup>See the results for the additive model in Tables 2.6 and 2.7 where we use the first-difference of the unemployment rate as the cyclical indicator,  $(2.006 - 1.546)/(2.006 - 1.546 + 0.345 + 0.771) \approx 0.29$ . Ranking workers by employment duration and firms by their poaching hire share, we obtain the lower estimate of  $(2.511 - 2.186)/(2.511 - 2.186 + 0.458 + 0.762) \approx 0.21$ .

how this varies with the unemployment rate.<sup>28</sup> Regression evidence is shown in Table 2.8. Specifically, we allow workers and firms to be in one of 50 employment-weighted rank bins, and we measure the correlation between worker rank and firm rank for those bins. This correlation, which is in the interval  $[-1, 1]$ , is calculated separately for each quarter in the data, and serves as the dependent variable for regressions on our two cyclical indicators.<sup>29</sup> While there are differences across ranking methods, overall the measured correlation between worker rank and firm rank increases in labor market downturns. For example, a one percentage point increase in the unemployment rate is associated with an increase of 0.012 in the correlation between a worker’s rank measured by employment duration and a firm’s rank measured by the poaching hire share. This relationship is stronger for the first-difference of the unemployment rate, which suggests that the correlation between worker ranks and firm ranks increases during recessions more than in the times of high unemployment that follow.

To interpret the results of Table 2.8, it is important to consider the results on changes in the employment of different worker-firm rank combinations that we discussed in Section 2.3.3.1. Countercyclical changes in the degree of agreement between worker rank and firm rank are driven by several mechanisms, some that strengthen and others that weaken this relationship. Our results show that, overall, positive sorting tends to strengthen during labor market downturns.

---

<sup>28</sup>For the correlations and a discussion, see Appendix B.3.3 and Appendix Table B.3.

<sup>29</sup>Note that to avoid excess decimal places in Table 2.8, we multiply the correlations by 100.

Table 2.8: Relationship between worker-firm correlations and the unemployment rate

---



---

Dependent variable: Worker-firm rank correlation

---

Employment & poaching share	Earnings & productivity	Additive worker & firm effects	Ranked workers & surplus
<i>Difference in unemployment from its HP trend</i>			
-0.0 (0.2)	0.3*** (0.1)	0.3 (0.2)	0.3*** (0.1)
<i>First-difference of the unemployment rate</i>			
1.2** (0.5)	0.5*** (0.1)	1.7*** (0.4)	-0.4 (0.3)

*Notes:* Dependent Variable: Correlation of Worker and Firm Ranks within given model for each quarter. Regression of these correlations for each quarter on the seasonally-adjusted unemployment rate after either HP-filtering or first-differencing, season dummies, and a linear time trend. \*, \*\*, and \*\*\* indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Standard errors are in parentheses. To avoid excessive decimal places, the dependent variables range from  $[-100, 100]$ , while the cyclical indicators range from  $[-1, 1]$ .

### 2.3.5 Summary

Our empirical evidence shows how labor market composition and sorting change with aggregate conditions. All four of our ranking methods deliver similar results. During recessions, the employment share of low-rank workers declines while that of high-rank workers increases. This change can be attributed to the differential net nonemployment transitions of low- vs. high-rank workers. Although workers of all ranks are less likely to work during economic downturns, low-rank workers are especially unlikely to work. Thus worker composition can be characterized by a countercyclical cleansing effect: relatively unproductive workers leave employment during downturns.

Cyclical changes in firm composition are quite different. During economic downturns, the employment share of low-rank firms increases. This is true whether we rank firms by poaching hire share, labor productivity, or transformations of worker earnings. This increase in the employment share of low-rank firms is driven by the countercyclical decline in net poaching from low-rank to high-rank firms. During expansions, high-rank firms poach workers away from low-rank firms as workers move up the job ladder. But during downturns, the job ladder shuts down, and relative employment increases for low-ranked firms.

This countercyclical cleansing of the worker distribution and sullyng of the firm distribution drive changes in labor market sorting. As low-rank firms and high-rank workers have an increasing share of employment during labor market downturns, the share of such job matches naturally increases. This weakens the degree of sorting and is driven by a sullyng effect. We also find that low-rank workers are less likely to work at high-rank firms during downturns, which strengthens sorting. This change can mostly be attributed to the slowdown of the job ladder and hence also appears to be a sullyng effect. The decline of low-rank workers at high-rank firms dominates, and the measured agreement between worker rank and firm rank increases during recessions.

## Chapter 3: Measurement of Nominal Wages in Administrative Earnings Data

### 3.1 Introduction

Over the last three decades, many countries have created administrative data sets that track workers' earnings at many or nearly all firms. The rich data contained in these employer-employee linked data sets have enabled researchers to examine the impact of policy changes, worker and firm decision-making, labor market dynamics, and many other topic areas. However, this research has been limited by the fact that most employer-employee linked data sets fail to decompose workers' reported earnings into the base wages, variable compensation, hours or weeks worked, and other factors that determine workers' earnings.

To overcome this limitation, I develop a set of machine learning methods that identify each worker's unobserved payday schedule and nominal wage series from the worker's observed quarterly earnings with an employer. First, I note that variation in the number of paydays due to a worker's payday schedule can generate fluctuations of  $\pm 15\%$  in the worker's quarter-over-quarter earnings. Although earnings fluctuations generated by changes in the number of paydays appear as noise at an individual

level, these payday-related earnings changes can be identified because they are large and common to many workers at the firm. I begin by identifying the individual-level payday schedule that minimizes the implied residual variance of the worker's observed quarterly earnings. I then identify the payday schedule(s) in operation at the firm using a clustering algorithm that determines which payday schedules are common to many workers at the firm. Having identified the payday schedule(s) used by the firm, I then determine which payday schedule (from this more limited set) minimizes the implied residual variance of each worker's quarterly earnings. Knowing the payday schedule allows me to estimate each worker's number of payday weeks in any given period, and thus control for transitory fluctuations in quarterly earnings generated by the worker's payday schedule.

I reframe the problem of identifying persistent changes in each worker's base wage as one of identifying structural breaks in the worker's observed earnings. Each persistent wage change is equivalent to a structural break in the time series of a worker's log earnings over the worker's job spell at a firm. By reframing the problem, I can use methods developed by the extensive literature on identifying structural breaks in time-series data.<sup>1</sup>

Because administrative employer-employee linked datasets contain earnings series for millions of workers, the scalability of the post-Lasso procedure makes it a particularly attractive method for identifying structural breaks in a worker's log

---

<sup>1</sup>See Casini and Perron (forthcoming) for a review of recent advances in the literature on structural break identification. In the downward nominal wage rigidity literature, Gottschalk (2005) addressed measurement error in survey respondents' reported base wages by applying structural break identification procedures proposed by Bai and Perron (1998). Barattieri, Basu and Gottschalk (2014) further extended Gottschalk's method to account for Type I and Type II error in the identification of nominal wage changes.

base wage (i.e. persistent wage changes).<sup>2</sup> The post-Lasso estimation procedure allows for persistent wage changes to occur in any period of a worker's employment history. The procedure minimizes an objective function that has two components. The first component optimizes the model fit by choosing the wage change estimates that minimize the Euclidean distance between the predicted persistent log wage history and the observed log earnings history (this is the same as standard OLS minimization of the sum of squared residuals). The second component addresses model over-fitting by including a penalty parameter for the sum of the absolute value of the estimated log wage changes. This penalty parameter causes the post-Lasso procedure to set the estimated persistent wage change to zero for many periods, which is consistent with workers not receiving base wage changes every quarter.

I implement these methods using a 10% random sample of firms from 30 states in the U.S. Census Bureau's LEHD data set, an employer-employee linked administrative data set covering approximately 96% of employment in each state. I use the estimated persistent wage changes to identify four patterns of nominal wage adjustment: i) estimated persistent wage changes exhibit downward nominal wage rigidity, ii) real wage cuts that would be optimal in a frictionless environment are suppressed by downward nominal wage rigidity, iii) workers' nominal raises follow a Taylor-like pattern, with the probability of a wage raise spiking every four quarters, and iv) the timing of workers' annual raises is synchronized within the firm.

This chapter proceeds as follows. Section 3.2 describes the LEHD data set.

---

<sup>2</sup>Notable studies that use Lasso estimation for structural break identification include Harchaoui and Lévy-Leduc (2010) and Ciuperca (2014).

Section 3.3 describes the set of clustering algorithms that I use to identify each firm’s set of payday schedule(s) and the number of payday weeks for a given worker. Section 3.4 lays out the full details of the post-Lasso estimation procedure and evaluates the quality of the persistent wage change estimates. Section 3.6 presents evidence regarding the four nominal wage adjustment patterns: i) estimated persistent wage changes exhibit downward nominal wage rigidity; ii) downward nominal wage rigidity suppresses real wage changes; iii) workers receive nominal raises according to an annual schedule; and iv) workers’ annual raise schedules are synchronized within firms. Section 3.7 concludes.

## 3.2 Data

This paper uses the U.S. Census Bureau’s LEHD data set - an employer-employee linked data set with quarterly earnings for approximately 96% of all employment in a state. The quarterly earnings data in the LEHD is derived from firms’ mandatory unemployment insurance filings. This earnings data is complemented with both worker characteristics (age, sex, race, and education) and firm characteristics (industry, firm age, and firm size) from other data sources. Individuals are uniquely identified by a Protected Identification Key (PIK) that allows each individual to be tracked across different employers and locations. The LEHD identifies employers at the level of a state employer identification number (SEIN). Firm age and firm size are derived by aggregating one or more SEINs (potentially across states) to the level of the federal employer identification number (EIN). For

simplicity, I refer to each SEIN as a firm.

This paper extracts two samples from the LEHD data set. The primary sample consists of a 10% random sample of SEINs from thirty states covering the period from 1998:Q1 to 2017:Q1.<sup>3</sup> I chose these thirty states because there are no gaps in reported quarterly earnings for any of these states over the sample period. I also employ a secondary sample that is a 10% random sample of SEINs from the four states that also reported quarterly hours paid during the period from 2011:Q1 to 2018:Q1 (MN, OR, RI, and WA). I use this secondary sample because the hours-paid data helps quantify the degree of measurement error in my baseline post-Lasso estimates of workers' nominal wages.

### 3.3 Estimation of payday weeks

A key drawback of the LEHD data set is that the LEHD generally only reports workers' quarterly earnings, which can vary due to fluctuations in overtime pay, bonuses, payday weeks, average weekly hours paid, or the base wage.<sup>4</sup> Many of these components of quarterly earnings are transitory, and thus are unlikely to affect employment decisions in long-term employment relationships (see Appendix C.2). To overcome this limitation of the LEHD, I develop a set of novel machine learning tools that identify persistent changes in workers' unobserved nominal base wages

---

<sup>3</sup>The states included in the primary sample are: CA, CO, CT, FL, GA, HI, ID, IL, IN, KS, LA, MD, ME, MT, NC, ND, NJ, NM, NV, OR, PA, RI, SC, SD, TN, TX, VA, WA, WI, and WV.

<sup>4</sup>The LEHD data set does have total quarterly hours paid for four states from 2011 forward. Although these hours-paid data are helpful for identifying patterns in workers' wage changes, only Washington state has hours-paid data prior to 2009. For more details on patterns of adjustment in the non-wage components of quarterly earnings, see Appendix C.3.

from their observed nominal quarterly earnings. This section describes these machine learning methods and evaluates the quality of the resulting estimated nominal wage changes.

To more formally distinguish between persistent versus transitory components of quarterly earnings, I express each worker’s quarterly earnings as a function of the worker’s base wage, the number of payday weeks in the quarter, the number of regular and overtime hours worked, and any variable compensation paid to the worker (e.g. annual bonuses, tips, and commissions).<sup>5</sup> Specifically,

$$y_{ikt} = w_{ikt} (n_{ikt} \bar{h}_{ikt} + \theta_{ikt}^o n_{ikt} \bar{h}_{ikt}^o) v_{ikt} \epsilon_{ikt} \quad (3.1)$$

where  $y_{ikt}$  is worker  $i$ ’s total nominal earnings at firm  $k$  in quarter  $t$ ,  $w_{ikt}$  is the worker’s base hourly wage,  $n_{ikt}$  is the number of payday weeks in the quarter,  $\bar{h}_{ikt}$  is the worker’s average weekly hours paid,  $\theta_{ikt}^o$  is the overtime premium for overtime pay (typically 1/2),  $\bar{h}_{ikt}^o$  is the worker’s average weekly overtime hours paid,  $v_{ikt}$  is the worker’s variable compensation as a percent of the base wage, and  $\epsilon_{ikt}$  is measurement error (such as dropping or adding a decimal place in the recorded quarterly earnings).<sup>6</sup>

---

<sup>5</sup>When measuring workers’ wages, I only consider “full-quarter” earnings - where the worker has positive earnings from the same SEIN in the quarter immediately before and after the current quarter. This has the benefit of reducing fluctuations in quarterly earnings that result from new hires and job separators working only part of the quarter in which they are hired or separate.

<sup>6</sup>For salaried employees who are exempt from overtime pay requirements, the average reported weekly hours paid always equals 40 (no matter how many hours are actually worked) and the average weekly overtime hours paid is always zero.

### 3.3.1 Payroll schedules

Payroll schedules generate significant fluctuations in quarterly earnings because of variation in the number of pay periods from quarter to quarter. For instance, workers who are paid bi-weekly typically experience  $\pm 15\%$  fluctuations in quarterly earnings from one quarter to the next as the number of quarterly paydays switches between six and seven. Although earnings fluctuations generated by changes in the number of payday weeks appear as noise at an individual level, these payday-related changes can be identified because they are common to many workers at the firm. Knowing the changes that are induced by payroll schedules is useful because such changes are directly related to the number of weeks worked, and thus allow estimation of a worker's average weekly earnings.

The method for identifying a firm's payroll schedule(s) exploits three empirical regularities. First, there are a limited number of potential payday schedules: seven weekly, fourteen bi-weekly, one monthly, and one semimonthly payroll schedule. Second, each of these payday schedules has a distinct time series of payday weeks from quarter to quarter. Importantly, the time series of quarterly payday weeks for each payday schedule can be determined from the annual calendar. And third, firms tend to use a small number of payroll schedules for their employees (typically only one or two, see Burgess (2014)), so the fluctuations in quarterly earnings caused by payday schedules are common to many workers at the firm.

Estimating a worker's payday weeks first requires identifying the set of payroll schedules used by the worker's employer. This firm-specific set of payroll schedules

is determined by iteratively selecting the payroll schedule that best fits the observed quarter-over-quarter earnings changes for the largest number of workers. For each worker at the firm and potential payroll schedule  $p$ , I construct the worker's payroll-schedule-adjusted log earnings change,  $\Delta y_{ikt}^p$ , defined as:

$$\Delta y_{ikt}^p = (\ln(y_{ikt}) - \ln(y_{ikt-1})) - (\ln(n_t^p) - \ln(n_{t-1}^p)) \quad (3.2)$$

where  $n_t^p$  is the number of payday weeks for the payroll schedule  $p$  in quarter  $t$  based on the annual calendar.<sup>7</sup>

The best fitting payroll schedule for a given worker,  $p_{ik}^*$ , is the payroll schedule that implies the lowest variance of  $\Delta y_{ikt}^p$ . The intuition behind this rule is that for the true payroll schedule, the subtracted change in the payroll schedule's payday weeks,  $(\ln(n_t^p) - \ln(n_{t-1}^p))$ , is perfectly negatively correlated with the true payday weeks change component of the quarterly earnings change. This perfect negative correlation, combined with an assumption that changes in wages, weekly hours, and variable compensation are independent of the number of payroll weeks, implies that the true payday schedule minimizes the variance in Equation 3.2 as the duration of the job spell approaches infinity. Once each worker's best-fitting payroll schedule has been identified, I select the payroll schedule that is best for the largest number of workers, where each worker's best schedule is given a weight equal to the duration of the worker's job spell (this accounts for differences in the precision of the individual-

---

<sup>7</sup>One complication from using the annual calendar is that January 1st, New Years Day, is a holiday that occurs at the transition between Q4 and Q1. From the data, it is apparent that some firms shift weeks paid from Q1 to Q4 when the weekday of payment falls on New Years Day.

level variance estimates caused by differences in the job spell duration).

After each iteration, I remove any workers for whom the selected payroll schedule was the best-fitting payday schedule. I then run another iteration for the remaining workers. This process continues until either four payroll schedules have been selected or no remaining payroll schedule is the best-fitting payroll schedule for five or more workers. Having identified the set of potential payday schedules at the firm, I select from this constrained set the worker-specific payroll schedule that minimizes the variance of the worker’s payroll-schedule-adjusted change in log earnings ( $\Delta y_{ikt}^p$ ).

### 3.4 Post-Lasso estimation of persistent nominal wage changes

I next use a post-Lasso procedure to extract persistent changes in each worker’s unobserved base wage ( $w_{ikt}$ ) from their payday-adjusted quarterly earnings ( $y_{ikt}$ ). This procedure involves four steps. First, I express each worker’s base wage in any given period  $t$  as a recursive formulation of the worker’s starting wage ( $w_{ik1}$ ) and all base wage percentage changes up to the current period ( $\Delta_{iks}^w$ ):

$$w_{ikt} = w_{ik1} \prod_{s=2}^t (1 + \Delta_{iks}^w) \tag{3.3}$$

Inserting this recursive formulation into the quarterly earnings decomposition in Equation 3.1 and then taking the natural log allows observed quarterly earnings to

be rewritten as:

$$\ln(y_{ikt}) = \ln(w_{ik1}) + \sum_{s=2}^t \ln(1 + \Delta_{iks}^w) + \ln(n_{ikt}) + \ln(\bar{h}_{ikt} + \theta_{ikt}^o \bar{h}_{ikt}^o) + \ln(v_{ikt}) + \ln(\epsilon_{ikt}) \quad (3.4)$$

Second, although I do not observe any of the right-hand side variables, I can express Equation 3.4 as a standard linear regression model. Specifically, if we observe  $T$  periods of employment for the worker at the firm, then the current period quarterly earnings can be expressed as:

$$\ln(y_{ikt}) = \beta_{ik}^1 d_{ikt}^1 + \sum_{s=2}^T \beta_{ik}^s d_{ikt}^s + \alpha_{ik} \ln(n_{ikt}) + \underbrace{\ln(\bar{h}_{ikt} + \theta_{ikt}^o \bar{h}_{ikt}^o)}_{\tau_{ikt}} + \ln(v_{ikt}) + \ln(\epsilon_{ikt}) \quad (3.5)$$

where  $d_{ikt}^s$  is a set of  $T$  indicator variables that, in any given period  $t$ , take on a value of 1 only if  $t \geq s$ . Thus,  $d_{ikt}^1$  corresponds to the intercept term and its coefficient,  $\beta_{ik}^1$ , represents the log starting weekly wage of the worker:  $\ln(w_{ik1})$ . The coefficient on each subsequent indicator variable,  $\beta_{ik}^s$ , represents the persistent nominal wage change experienced by the worker in period  $s$ :  $\ln(1 + \Delta_{iks}^W)$ .

Although I do not observe the number of payday weeks for each worker ( $n_{ikt}$ ), controlling for the number of payday weeks is important because it generates substantial noise in each worker's quarterly earnings series. Since firms tend to use only one or two payday schedules for all of their workers (see Burgess (2014)), a large share of workers at a firm may exhibit similar persistent earnings changes simply because of their number of payday weeks. To address this concern, I develop a clustering method that estimates the number of payday weeks for each worker.

This clustering method exploits the fact that each firm uses only a small number of payday schedules, which are themselves selected from a total universe of 23 payday schedules. Critically, each of these potential payday schedules has a distinct time series of payday weeks from quarter to quarter - where this time series can be determined from the annual calendar. Thus, the clustering algorithm identifies payday schedules at the firm (and then for a worker) based on patterns of quarter-over-quarter earnings changes that are both common to many workers at the firm and align with one of the potential payday schedules. Section 3.3 contains a complete description of the clustering method for estimating workers' payday weeks ( $\hat{n}_{ikt}$ ).

The number of payday weeks is common to many workers at the firm. The remaining unobserved components, representing average weekly hours paid, overtime, and variable compensation, are included in the  $\tau_{ikt}$  error term. While I would ideally observe these components as well, their absence is less troubling since persistent changes in these unobserved components of earnings are relatively rare.

In the third step, I identify the quarters in which a worker received a wage change. It is impossible to estimate the regression model in Equation 3.5 using standard methods because there are  $T + 1$  right-hand variables and only  $T$  observations. Instead, I exploit the fact that there are presumably many quarters in which a worker has no change in their base wage. This implies that  $\beta_{ik}^s = 0$  in those quarters, making the Lasso variable selection procedure, first proposed by Tibshirani (1996), an ideal method for identifying quarters in which a worker has a persistent wage change (i.e. has a non-zero  $\beta_{ik}^s$  coefficient).<sup>8</sup>

---

<sup>8</sup>Notable studies that use Lasso estimation for variable selection in a time-series context include

The Lasso estimation procedure selects variables to include in a regression model by trading off the improvement in the explanatory power of the model when the variable is allowed a non-zero coefficient (the standard OLS minimization of the sum of squared residuals) against a penalty for the absolute distance of the coefficient from zero. Thus, for every worker-firm job spell, I use the Lasso estimation procedure to select the set of non-zero  $\beta_{ik}^s$  that solve the following minimization problem:<sup>9</sup>

$$\min_{\beta_{ik}^1, \dots, \beta_{ik}^T, \alpha_{ik}} \left( \sum_{t=1}^T \ln(y_{ikt}) - \sum_{s=1}^T \beta_{ik}^s d_{ikt}^s - \alpha_{ik} \ln(\hat{n}_{ikt}) \right)^2 + \lambda_{ik} \left( \|\alpha_{ik}\| + \sum_{s=1}^T \|\beta_{ik}^s\| \right) \quad (3.6)$$

where  $\hat{n}_{ikt}$  is the estimated number of payday weeks from the procedure described in Section 3.3. The  $\lambda_{ik}$  penalty parameter is set using 10-fold cross validation for each worker-firm job spell.<sup>10</sup> If the job spell has fewer than ten full quarters of employment, then I instead use leave-one-out cross validation.

The error term of this Lasso minimization problem includes any error from the estimation of payday weeks plus deviations of log weekly hours, overtime hours, variable compensation, and measurement error from the variables' averages over the job spell. Minimizing the Lasso objective function identifies the quarters for which assigning a non-zero wage change coefficient significantly improves the model fit in both the current period and all subsequent periods. Thus, the Lasso proce-

---

Harchaoui and Lévy-Leduc (2010) and Ciuperca (2014).

<sup>9</sup>For the 4-state sample with quarterly hours-paid data, I replace  $\ln(\hat{n}_{ikt})$  with the log of the reported hours paid ( $\ln(n_{ikt}\bar{h}_{ikt})$ ), which changes the meaning of the  $\beta_{ik}^1$  coefficient to be each worker's initial starting hourly wage rather than their starting weekly wage.

<sup>10</sup> $X$ -fold cross validation randomly partitions the worker's wage history into  $X$  distinct subsets. Each subset then serves as a holdout group that evaluates the prediction quality of the estimated wage changes generated using the other  $X - 1$  subsets. The optimal  $\lambda_{ik}$  penalty parameter is chosen to maximize the prediction quality on the  $X$  hold-out subsets.

dure identifies persistent changes in a worker’s weekly earnings.<sup>11</sup> Although these persistent changes in a worker’s weekly earnings will oftentimes come from changes in the worker’s base wage, the Lasso procedure will also pick up persistent changes in the worker’s average weekly hours worked (e.g. going from part to full-time) or persistent changes in variable compensation (e.g. a permanent change in the sales commission rate), which will mistakenly be attributed to changes in the worker’s persistent base wage.

By penalizing non-zero coefficients based on their absolute distance from zero, the Lasso estimation procedure generates attenuation bias in the coefficient estimates. Thus, the fourth and final step of the wage estimation process addresses this bias by estimating a standard post-Lasso OLS regression model for every worker-firm job spell that only includes variables with non-zero coefficients selected by the Lasso procedure in Step 3. The resulting coefficient estimates from the post-Lasso OLS regression serve as my estimates of each worker’s persistent nominal wage changes.

### 3.4.1 Quality evaluation of post-Lasso estimated nominal wage changes

Because I do not observe workers’ true base wages, it is difficult to validate this post-Lasso estimation procedure. That said, I can evaluate the quality of the post-Lasso estimation procedure in two ways. First, in the four states with hours-paid data from 2011:Q1 to 2018:Q1, I evaluate how often the post-Lasso procedure

---

<sup>11</sup>This Lasso procedure is very similar in spirit to the structural break identification procedure that Gottschalk (2005) adapted from Bai and Perron (1998) in order to correct for measurement error in self-reported wages from survey data. Barattieri, Basu and Gottschalk (2014) further improve upon Gottschalk’s method by explicitly accounting for Type I and Type II errors in the error correction process.

identifies wage changes for workers with hourly earnings at or near the old minimum wage in quarters in which the state changes its minimum wage. Second, I compare the frequency of quarterly wage raises identified by the post-Lasso procedure relative to the frequency of base wage changes that Grigsby, Hurst and Yildirmaz (2019) identify from administrative payroll records.

For the evaluation of minimum-wage workers' post-Lasso estimated wage changes, I begin by identifying quarters in which any of the four states changed their minimum wage. After constructing each worker's average hourly earnings in a given quarter by dividing their reported nominal earnings by their hours paid, I include in the sample all workers in a state whose hourly earnings in  $t-2$  (where  $t$  is the quarter of the state's minimum wage change) were between the old and the new minimum wage. The Lasso estimation procedure identifies that 55.0% of these minimum-wage workers received a wage raise in the quarter of the state's minimum wage change. An additional 33.3% of minimum wage workers are identified as receiving a wage change in the quarter immediately before the minimum wage change, but approximately half of these are workers who are also identified as receiving a wage change in the same quarter as the state's minimum wage change.<sup>12</sup> Thus, I find the post-Lasso procedure identifies wage changes for 70.0% of minimum-wage workers in the quarter of or immediately before a state's minimum wage change. Thus, for minimum wage workers in these four states, 30.0% is a reasonable estimate of the Lasso procedure's Type II error rate (the failure to detect a wage change when there is a

---

<sup>12</sup>The post-Lasso procedure may identify a worker as receiving wage changes in two, back-to-back quarters if the true wage change occurred in the middle of the first quarter. In this case, many of these workers with back-to-back estimated wage changes would have received their true wage change in the month or two immediately before the mandated minimum wage change.

change). In the quarter immediately after the state's minimum wage change takes effect, the Lasso estimation procedure identifies a nominal wage change for 14.7% of workers. This provides an upper-bound estimate of the Type I error rate (detecting a wage change when there is none), since some minimum wage workers may have received both a raise in the state's minimum wage change quarter and an additional wage change in the quarter immediately following the minimum wage change.

Most studies of nominal wage rigidity examine annual changes in workers' wages. This paper, on the other hand, focuses on quarter-over-quarter wage changes because the proposed quasi-experiment requires estimates of nominal wage changes at a sub-annual frequency to identify calendar quarters in which firms tend to raise wages. Thus, for my primary sample, I compare the results of the post-Lasso procedure with the two studies that report quarterly nominal wage adjustment frequencies using U.S. data: Barattieri, Basu and Gottschalk (2014), which uses the SIPP, and Grigsby, Hurst and Yildirmaz (2019) (GHY), which uses ADP administrative payroll data. Table 3.1 reports the frequency of nominal wage raises, freezes, and cuts at both a quarterly and annual frequency. GHY serves as the best benchmark for my post-Lasso persistent wage change estimates because they use administrative data from a large sample of U.S. firms' payroll records for which they observe workers' true base wages. The only downside to using their results as a benchmark is that their sample includes only firms with 50+ workers. Since smaller firms are less likely to raise workers' wages, GHY overestimates the frequency of nominal wage raises in the broader population (and vice-versa for nominal wage freezes).

GHY find that 18.5 percent of workers at 50+ employee firms receive a nominal

wage raise in any given quarter. The post-Lasso procedure identifies 26.5% fewer nominal wage raises, estimating that only 13.6 percent of workers receive a nominal raise each quarter. This difference is similar to the 30% Type II error rate upper bound from the earlier analysis of minimum-wage workers. Although it is not clear how much of the difference is due to GHY's exclusion of small firms (small firms employed 28.2% of workers in 2014 according to the Census Bureau's Quarterly Workforce Indicators), I believe that the post-Lasso estimation procedure fails to identify a non-trivial share of nominal wage raises.

Although this significant Type II error rate would be troubling in many contexts, it is less so for the quasi-experiment described in Chapter 1, which aggregates the post-Lasso individual-level estimated wage changes to the firm level and at a calendar quarter frequency. Given a Type II error rate of approximately 30%, this is equivalent to constructing measures of firms' historical nominal raise patterns based on a sample of 70% of the full set of workers' nominal raises. If the post-Lasso procedure fails to identify true wage changes in a random fashion, or if the non-randomness of this wage change identification is small relative to the underlying wage change patterns, then the measurement error in my estimates of firms' historical raise patterns should be approximately classical in nature.

### 3.5 Comparison of estimated persistent nominal wage changes to literature

The resulting post-Lasso estimated persistent base wages exhibit patterns very similar to the base wage change patterns identified by Grigsby, Hurst and Yildirmaz (2019) using the ADP administrative payroll records. Table 3.1 shows that the post-Lasso procedure estimates that 84.9% of workers experience no quarter-over-quarter persistent wage change, 13.6% receive a nominal raise, and 1.6% receive a nominal cut. Examining workers at firms with 50+ employees (which tend to raise workers' wages more often than smaller firms), Grigsby, Hurst and Yildirmaz (2019) determine that 80.6% of workers experience quarter-over-quarter nominal wage freezes, 18.5% receive a nominal raise, and 0.9% receive a nominal cut.

Table 3.1: Comparison of measures of nominal compensation changes

Quarter-over-Quarter Change in Log Nominal Compensation Measure		Period	Raise	Freeze	Cut
Compensation Measure	Data Source				
<b>Hourly Earnings</b>					
Raw Data	LEHD 4 States	2011-2018	55.5%	5.0%	39.5%
No Hours Rounding	LEHD 4 States	2011-2018	46.4%	22.2%	31.4%
No Bonus + Rounding	LEHD 4 States	2011-2018	36.9%	45.3%	17.8%
Persistent Base Wage	Post-Lasso LEHD 4 States	2011-2018	13.6%	84.9%	1.6%
Base Wage	ADP Payroll (GHY 2019)	2008-2016	18.5%	80.6%	0.9%
Base Wage	SIPP Survey (BBG 2014)	1996-2000		78.4-84.8%	

*Note:* The Hourly Earnings measures are the quarter-over-quarter change in log hourly earnings. The *Raw Data* is calculated from the LEHD quarterly earnings and hours-paid data with no adjustments. The *No Hours Rounding* sets to zero any change that could be accounted for by hours rounding. The *No Bonus + Rounding* also smooths single-period bonuses in addition to correcting for hours rounding errors. See Appendix C.3 for details on how each hourly earnings measure is constructed. *Persistent Base Wage* refers to the post-Lasso estimation results when controlling for the log estimated payday weeks using the secondary sample. Freezes are periods where the Lasso estimation procedure sets the wage change estimate to zero. The *Base Wage* estimates from ADP Payroll Records are from Grigsby, Hurst and Yildirmaz (2019). GHY examine payroll records from 2008-2017 for firms with 50+ workers who use ADP Payroll Services, reweighting the observations to match the firm characteristics of 50+ worker firms in the Census Bureau's Longitudinal Business Database. The *Base Wage* estimates from the SIPP Survey are from Baratieri, Basu and Gottschalk (2014), which use linked SIPP records from 1996-2000. To address measurement error in self-reported wages, the BBG study employs the structural break identification procedure proposed by Gottschalk (2005) and then further corrects for the frequency of Type I and Type II errors. U.S. Census Bureau Disclosure Review Board bypass numbers DRB-B0037-CED-20190327 and CBDRB-2018-CDAR-061.

## 3.6 Patterns of nominal wage adjustment

This section provides evidence regarding four patterns of nominal wage adjustment: i) estimated persistent wage changes exhibit downward nominal wage rigidity, ii) real wage cuts that would be optimal in a frictionless environment are suppressed by downward nominal wage rigidity, iii) workers' nominal raises follow a Taylor-like pattern, with the probability of a wage raise spiking every four quarters, and iv) the timing of workers' annual raises are synchronized within the firm.

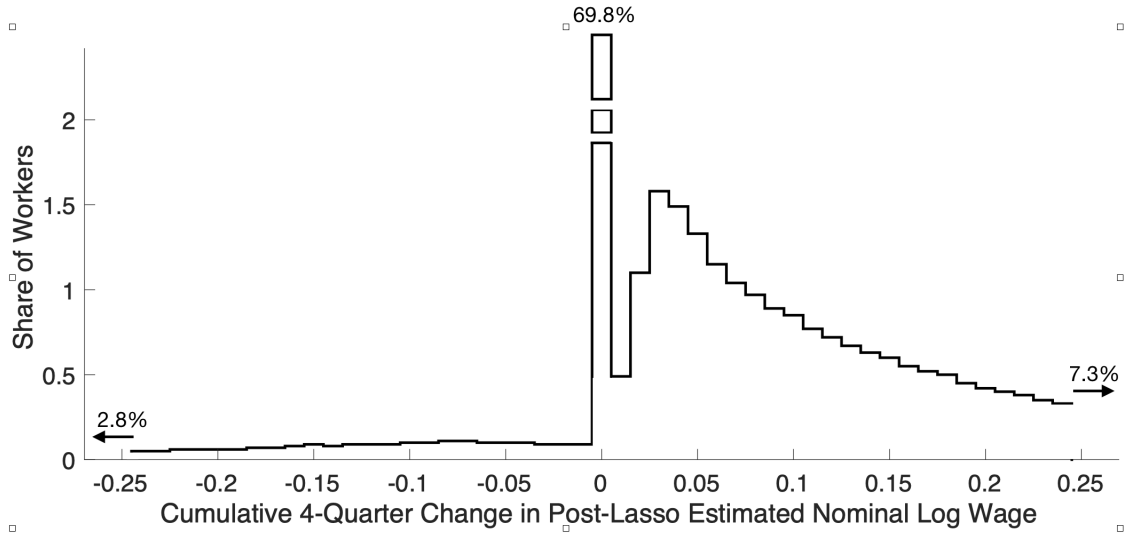
### 3.6.1 Wages exhibit downward nominal rigidity

Consistent with previous studies,<sup>13</sup> the post-Lasso estimated persistent base wages exhibit significant downward nominal wage rigidity. Figure 3.1 shows the histogram of annual post-Lasso estimated nominal wage changes within  $\pm 25$  log points of zero. The histogram of estimated persistent base wage changes indicates that a large mass of workers have no change in their nominal wages year-over-year, and that the distribution of nominal wage changes is missing mass to the left of zero nominal change.

---

<sup>13</sup>Studies documenting downward nominal wage rigidity using the Panel Study of Income Dynamics (PSID) include: McLaughlin (1994); Lebow, Stockton and Wascher (1995); Akerlof, Dickens, Perry, Gordon and Mankiw (1996); Card and Hyslop (1997); Kahn (1997); Altonji and Devereux (2000); and Dickens, Goette, Groshen, Holden, Messina, Schweitzer, Turunen and Ward (2007). Studies using the Current Population Survey (CPS) include: Card and Hyslop (1997); Daly and Hobijn (2014); Elsbj, Shin and Solon (2016); and Jo (2019). Studies using the Survey on Income and Program Participation (SIPP) include: Gottschalk (2005); and Barattieri, Basu and Gottschalk (2014). Studies using the Employer Cost Index (ECI) survey include: Lebow, Saks and Wilson (2003); and Fallick, Lettau and Wascher (2016). Studies using unemployment insurance administrative data include: Kurmann and McEntarfer (2019) and Jardim, Solon and Vigdor (2019). Grigsby, Hurst and Yildirmaz (2019) use ADP payroll data. Hazell and Taska (2018) using job posting data from Burning Glass.

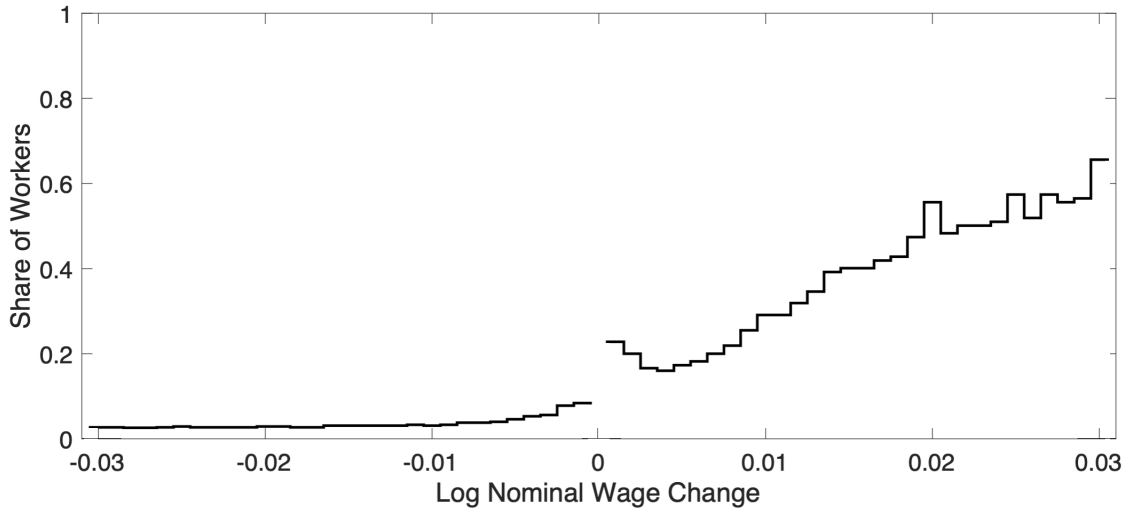
Figure 3.1: Cumulative 4-quarter change in persistent log nominal wage



*Notes:* Histogram of workers' four-quarter cumulative changes in their post-Lasso estimated persistent log nominal wage. Estimated from the Primary Sample after restricting to workers with at least five full quarters of non-zero earnings. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0037-CED-20190327.

DNWR would imply that there is a discontinuous drop in the frequency of nominal wage adjustment immediately below zero nominal change. Such a discontinuity is apparent in Figure 3.2, which shows the histogram of nominal changes in 0.1 log point bins within 3 log points of zero nominal change. In Appendix C.1, I more formally test for the presence of DNWR using a series of regression discontinuity models that check for a discontinuity in the distribution of nominal wage changes at zero nominal change. For all model specifications (changing both bandwidths and polynomials in the running variable), I find evidence of a discontinuity in the distribution at zero nominal change - implying the existence of downward nominal wage rigidity.

Figure 3.2: Histogram of persistent nominal wage changes at annual frequency near zero



*Notes:* Frequency of four-quarter cumulative changes in workers’ post-Lasso estimated persistent log nominal wage grouped into 0.1 log point change bins. Sample is restricted to workers with at least five full-quarters of non-zero earnings. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0037-CED-20190327.

### 3.6.2 Nominal wage rigidity suppresses real wage changes

Given the finding that workers’ persistent wage changes exhibit downward nominal wage rigidity, a natural question is how many real wage changes are suppressed when the change requires a nominal wage cut. This is a useful empirical moment for calibrating models with downward nominal wage rigidity. To estimate the suppression of real wage changes caused by downward nominal wage rigidity, I employ a variant of the method proposed by Kahn (1997) for measuring the effect of downward nominal rigidity on the wage change distribution.<sup>14</sup>

<sup>14</sup>There are two main differences between Kahn’s proposed method and what I do. First, she proposes to use the median of the full wage change distribution, whereas I use the mode of the non-zero changes. I do this because the infrequency of wage changes would mean that the median wage change is always zero (at both quarterly and annual frequencies). Using the mode of the non-zero changes provides a more consistent “real” wage change measure since the median of the

Kahn’s method compares the frequency of similar magnitude real wage changes across different periods, distinguishing between when the same real change corresponds to a positive versus negative nominal change. I begin by generating histograms of the nominal wage change distribution from different periods. The critical assumption of Kahn’s method is that the modal (or in her case median) nominal wage change in each period corresponds to the same optimal modal “real” wage change. Under this assumption, absent any rigidities or frictions, the proportion of wage changes in the histogram bins that are the same distance  $r$  from the period-specific modal nominal wage change bin should be the same across all periods.

To measure the degree to which DNWR suppresses real wage changes, I calculate  $p_{rt}$  - the proportion of all wage changes observed in period  $t$  (including nominal wage freezes) that fall into the  $r$ -distance bin from the modal nominal wage change bin for period  $t$  (where the distances are in 0.1 log points). If DNWR suppresses real wage changes, then, when a given  $r$ -distance bin requires a nominal wage cut, we should expect the proportion of wage changes in that bin to fall by some percent. Thus, I estimate the following regression model:

$$\ln(p_{rt}) = \sum_{x=-5}^5 \alpha_x d_{rt}^x + \beta_1 d^{\Delta w^-} + \beta_2 d^{\Delta w, small+} + \epsilon_{rt} \quad (3.7)$$

$d_{rt}^x$  is an indicator variable equal to one if  $x = r$ , which captures the assumption non-zero changes will change significantly depending on the share of wage changes that are frozen. Second, she includes in the regression model described in Equation 3.7 the zero nominal change bin in each period, along with a build up from the suppressed bins with nominal wage cuts. Including the zero nominal change bin with this build up imposes a restriction that the suppressed nominal wage changes are necessarily wage freezes, whereas excluding the zero nominal change bin relaxes this restriction. By relaxing this assumption, I can estimate the relationship using OLS with a log specification, as opposed to requiring a non-linear estimation procedure.

that, absent rigidities and frictions, the proportion of nominal wage changes in the  $r$  distance bin should be constant over time.  $d^{\Delta w^-}$  is an indicator variable equal to one if the  $r$ -distance bin in period  $t$  corresponds to a nominal wage cut, which captures the effect of downward nominal wage rigidity. And  $d^{\Delta w, small+}$  is an indicator variable equal to one if the  $r$ -distance bin in period  $t$  corresponds to a small positive nominal raise (wage change bins between +0.1 and +0.9 log points), which identifies if small changes are suppressed. For this regression, I use the secondary LEHD sample (which enables me to use hours-paid data in the post-Lasso wage change estimation procedure). I calculate the proportion of nominal wage changes that fall into 0.1 log point bins of the quarterly nominal wage change distributions for each quarter from 2011:Q3 through 2017:Q3.

As shown in Table 3.2, the proportion of wage changes in a given real change bin falls by 55% when the change requires a nominal wage cut versus a nominal wage raise. For comparison, this estimate is slightly above Kahn’s estimate using the PSID survey data that, when a given real change requires a nominal wage cut, DNWR suppresses 47.3% of hourly workers’ wage changes. The estimate is even further above Kahn’s estimate of 38.1% suppression of salaried workers’ wage changes, but within the 95% confidence interval of her estimate.

The finding that the likelihood of observing a real wage change falls significantly when the wage change requires a nominal cut has two implications for economic models that rely on assumptions about the wage adjustment process. First, that such a large share of wage changes are suppressed by DNWR lends empirical support to the various models that examine the role of DNWR in explaining the

asymmetric response of employment and output to contractionary versus expansionary shocks (Kim and Ruge-Murcia (2009); Schmitt-Grohé and Uribe (2016); Evans (2018); Mineyama (2018), Dupraz, Nakamura and Steinsson (2019), and Chodorow-Reich and Wieland (forthcoming)).

Second, Calvo (1983) proposed modeling nominal wage rigidity as the random arrival of opportunities to adjust wages. One testable implication of the Calvo wage adjustment process (and the staggered wage adjustment process proposed by Taylor (1980)) is that the frequency of wage adjustments should not respond to the aggregate state. This implication no longer holds if the wage adjustment process is modified such that the opportunity to cut a worker's nominal wage arrives less frequently than the opportunity to raise the worker's wage. With this minor modification, the frequency of wage adjustments falls in response to negative aggregate shocks because a greater share of jobs will have an optimal wage that requires a nominal wage cut. Since nominal wage cuts are even less likely to occur relative to nominal wage raises, the frequency of wage adjustment will decrease disproportionately for a negative shock versus a similar magnitude positive shock - thus making "time-dependent" Calvo and Taylor wage setting processes also state-dependent.

Table 3.2 also indicates that the likelihood of observing a given real change rises when it requires a small positive nominal change (versus a large positive nominal change). This has two implications for models of the wage adjustment process. First, this finding is consistent with Elsby (2009), which argues that DNWR would generate downward compression in the distribution of positive wage changes since DNWR makes it harder to reverse wage raises. Second, this finding runs counter to

the adjustment cost model of nominal wage changes proposed by Rotemberg (1982). If nominal wage changes incur adjustment costs (which could explain Taylor-style staggering of nominal wage changes), then small nominal wage changes that are close to zero should be suppressed. Kahn's estimation procedure allows for testing of the adjustment cost theory of wage changes by checking whether the proportion of wage changes that fall into a particular real change bin falls when that real change bin corresponds to a small positive versus a large positive nominal change. The results of the post-Lasso estimation procedure are the exact opposite of what we would expect with an adjustment cost model – specifically there are more wage changes in a given real bin when the wage change requires a small nominal raise versus a larger nominal raise. This finding is unlikely to be an artifact of the post-Lasso estimation procedure because the post-Lasso estimation procedure penalizes small wage changes with little explanatory power for a worker's subsequent nominal wage – which implies that the post-Lasso is more likely to under-report small nominal wage changes.

### 3.6.3 Annual schedules of nominal raises

I find workers' nominal wages in the LEHD data set broadly follow an annual schedule of nominal raises. This finding is consistent with Taylor (1980) which theorizes that workers receive wage adjustments at regularly scheduled intervals. Figure 3.3 shows that a worker's nominal wage raise hazard rate spikes four quarters after the worker's last nominal wage change and every subsequent four-quarter

Table 3.2: Suppression of wage changes due to downward nominal wage rigidity

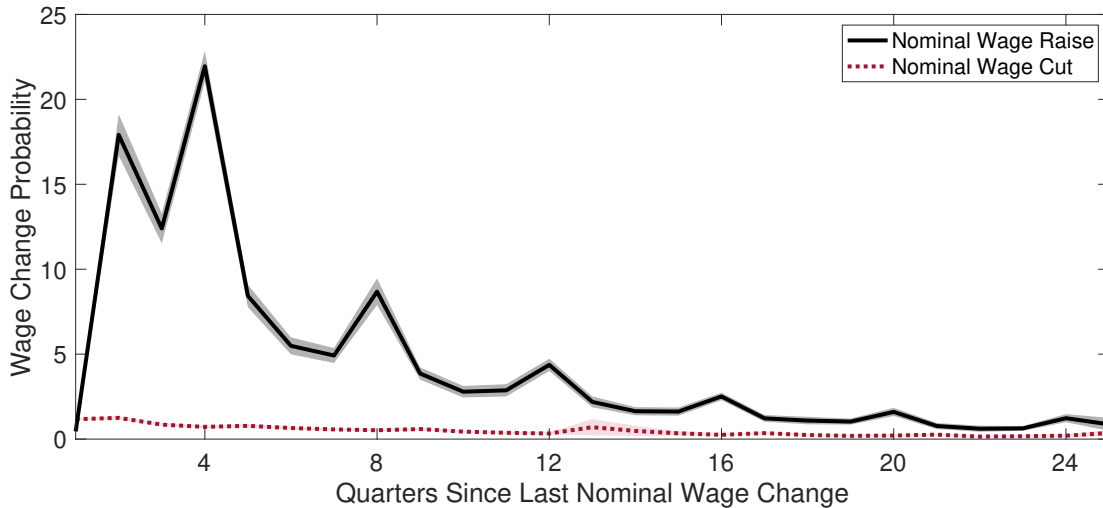
Log Proportion of Nominal Wage Changes in 0.1 Percentile Bin		
	(1)	(2)
Nominal Cut	<b>-0.56***</b> (0.06)	<b>-0.79***</b> (0.07)
Small Nominal Raise [0.1,1.0]	<b>0.12**</b> (0.04)	<b>0.12**</b> (0.04)
Small Nominal Cut [-1.0,-0.1]		<b>0.24***</b> (0.04)
R-Squared	0.958	0.958
Observations	12,000	12,000

*Note:* Outcome variable is the log proportion of wage changes in a given nominal wage change bin that is  $r$ -distance from the modal nominal wage change bin (excluding wage freezes). Distinct nominal wage change distributions are generated for every quarter between 2011:Q3 and 2017:Q3 and all 0.1 percentage point bins between -5.0% and 5.0% (excluding 0.0) are included in the regression sample. Coefficient estimates correspond to the log change in the proportion of wage changes that fall within a given  $r$ -distance bin when the change requires a nominal cut, small nominal raise, or small nominal cut. \*\*\*, \*\*, \* indicate statistical significance at the 0.1%, 1.0%, and 5.0% levels, respectively. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0069-CED-20190725.

anniversary. On the other hand, nominal wage cuts do not exhibit a Taylor-style annual schedule. The figure plots the coefficient estimates (and their 95% confidence intervals) from regressing an indicator variable equal to one if a worker receives a nominal wage raise on a set of dummy variables for the number of quarters since the worker's last wage change (and similarly for nominal wage cuts). The result that nominal wage raises follow an annual adjustment schedule is consistent with the finding of Barattieri, Basu and Gottschalk (2014) that the wage change hazard rate in the PSID spikes twelve months after the last wage change (although they did not find a similar pattern at subsequent annual anniversaries of the last wage

change, and they did not separately examine wage cuts).

Figure 3.3: Probability of wage change by quarters since last wage change



*Notes:* Coefficient estimates from a linear regression model of the probability of an increase (raise) or decrease (cut) in the post-Lasso estimated nominal persistent base wage given the number of quarters since the worker's last wage change. Shaded areas correspond to 95% confidence intervals using robust standard errors clustered at the SEIN level. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0069-CED-20190725.

That nominal raises exhibit a Taylor-style annual raise schedule while nominal cuts do not may have implications for asymmetries in the effectiveness of monetary policy. Dixon and Le Bihan (2012) show that Calvo and Taylor-style wage adjustment assumptions generate different output and employment responses to monetary policy shocks, with greater persistence in the response under the Calvo-style wage adjustment. Thus, if only nominal raises follow a Taylor-style adjustment schedule, then the dynamics and persistence of responses to contractionary versus expansionary monetary policy shocks may also differ.

### 3.7 Summary

This chapter presents a set of machine learning methods that expand the set of questions that can be answered using large employer-employee linked data sets. By identifying each worker's unobserved persistent base wages, paydays weeks, and annual bonuses from the worker's observed quarterly earnings, the methods presented in this paper allow for the examination of questions related to wage rigidity, rent sharing, and the structure of worker compensation. I then implement and evaluate the quality of these machine learning methods using quarterly earnings data in the U.S. Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) dataset, an employer-employee linked dataset for the United States. Using the estimated nominal wages of workers in 30 U.S. states, I document three patterns of nominal wage adjustment: i) estimated persistent wage changes exhibit downward nominal wage rigidity, ii) when a nominal wage cut is required, approximately 55% of optimal real wage changes do not occur, and iii) workers' nominal raises follow a Taylor-like pattern, with the probability of a wage raise spiking every four quarters.

## Appendix A: Appendix to Chapter 1

### A.1 DiD: Endogeneity of revenue change

Two concerns prohibit a causal interpretation of the estimate of Q2-raising firms' differential responses to negative revenue changes in 2008:Q4. First, the revenue change measure is the year-over-year revenue change, but the job destruction measure is for only the fourth quarter of the year. Although the specification does control for firm-specific seasonality (so the comparison is within each firm's fourth-quarter observations), it is still the case that a firm's revenue in the first three quarters of the year may affect the observed job destruction rate in 2008:Q4 through the start-of-quarter employment level rather than through the firm's employment decisions in Q4. For instance, a firm with a greater negative change in revenue in the first three quarters of 2008 may have already laid off a number of workers and thus entered 2008:Q4 with a lower employment level. When I compare this firm against another firm with a similar negative change in annual revenue, I would expect to see fewer fourth-quarter layoffs at the firm with a larger fall in revenue during the first three quarters of the year. If the variation in firms' quarterly revenue is related to the timing of firms' historical typical raise quarters (for instance through seasonal effects), then this would bias the DiD estimate of Q2-raisers differential

responses to negative revenue changes. The direction of this bias for my coefficient estimate is ambiguous because: i) I cannot observe the revenue changes experienced by firms in the first three quarters of the calendar year, and ii) I do not have a prior as to whether Q2-raising firms had larger or smaller negative revenue changes in 2008:Q1-Q3 relative to Q4-raising firms. That said, any concern regarding bias of this sort is mitigated by Q4-raising firms not exhibiting any change in their degree of responsiveness to negative revenue changes (relative to other periods).

The second concern stems from simultaneity between the annual revenue measure and a firm's Q4 job destruction rate. It is natural to assume that a firm's labor inputs contemporaneously affect the firm's revenue. Thus, there is an endogeneity issue created by reverse causality. Exogenous negative revenue shocks generated high rates of job destruction in 2008:Q4. This job destruction, in turn, lowered employment levels, and thus further decreased revenue in 2008:Q4. I expect that this reverse causality attenuates the coefficient estimate for the differential response of Q2-raising firms' job destruction rates to negative revenue shocks ( $\gamma^{Q2}$ ). The attenuation bias results from exogenous negative revenue shocks in 2008:Q4 forcing both Q2 and Q4-raising firms to destroy jobs. This job destruction lowered employment levels at firms, which further lowered firms' revenue (thus generating negative year-over-year revenue changes that were larger than the exogenous revenue shocks). Critically, absent any effect of exposure to DNWR on firms' rates of job destruction, the effect of this simultaneity bias is the same for both Q2 and Q4-raising firms. For the same fundamental negative revenue shock, job destruction should rise similarly at both Q2 and Q4-raising firms, thus generating similar degrees of

simultaneity bias. If, instead, greater exposure to DNWR forces firms to destroy more jobs, then job destruction should rise more at the Q2-raising firms in 2008:Q4. The simultaneity of revenue and employment, in conjunction with this higher job destruction, means that the observed negative revenue change is disproportionately larger than the fundamental negative revenue shock for Q2-raising firms. Thus, the same fundamental negative revenue shock generates a larger observed fall in revenue at Q2-raising firms. As a result, the estimated correlation is weaker between the job destruction rate and the interaction of the observed negative revenue change with the firm's exposure to DNWR (relative to its true correlation with the fundamental negative revenue shock), but does not reverse the sign. Thus, I interpret the result that Q2-raising firms increased their job destruction rates relative to Q4-raising firms in 2008:Q4 by an additional 0.077 percentage points for every one percent fall in year-over-year revenue as a lower bound on Q2-raising firms' actual differential sensitivity to the fundamental negative revenue shocks in 2008:Q4.

To make this argument more explicit, I define  $\tilde{J}D_{kt}$  and  $\tilde{\Delta}R_{kt}^-$  as the within-firm-calendar-quarter residuals of job destruction and year-over-year absolute value of negative revenue changes (i.e. after controlling for the firm-specific calendar quarter dummy variables, as well as fixed effects for industry-by-time, firm age, and firm size). The revenue change I observe, however, is not the fundamental residualized revenue shock ( $\tilde{\Delta}R^{-F}$ ), where:

$$\tilde{\Delta}R_{kt}^- = \tilde{\Delta}R^{-F} + \alpha \tilde{J}D_{kt} + v \tag{A.1}$$

where  $\alpha \geq 0$  since the absolute value of the negative revenue change is weakly increasing in the number of jobs destroyed.

Among the Q2 and Q4 firms with negative revenue changes, my assumed population model is:

$$\tilde{J}D_{kt} = \beta_1 \tilde{\Delta}R^{-F} + \beta_2 d_{kt}^{Q2 \text{ raiser}} \tilde{\Delta}R^{-F} + \epsilon \quad (\text{A.2})$$

where  $d_{kt}^{Q2 \text{ raiser}}$  is an indicator equal to one if the firm has a typical raise quarter in the Q2 calendar quarter. I assume that  $\beta_1 > 0$  since when negative revenue shocks are larger in absolute value, I expect job destruction to rise. I assume the  $\beta_2 \geq 0$ , so if DNWR affects job destruction when the firm has a larger negative revenue shock, then it will increase job destruction.

When I regress the firm's job destruction rate on its observed negative revenue change, I am not estimating the population model. Instead, because of the simultaneity bias, I estimate:

$$\tilde{J}D_{kt} = \frac{\beta_1 + \beta_2 d_{kt}^{Q2 \text{ raiser}}}{1 + \beta_1 \alpha + \beta_2 \alpha d_{kt}^{Q2 \text{ raiser}}} \tilde{\Delta}R_{kt}^{-} + \zeta_{kt} \quad (\text{A.3})$$

Given I am using a difference-in-differences estimation strategy,  $\hat{\beta}_1^{DiD}$  is identified from the Q4-raisers for whom  $d_{kt}^{Q2 \text{ raiser}} = 0$ . Namely

$$\mathbb{E} \left[ \hat{\beta}_1^{DiD} \right] = \frac{\beta_1}{1 + \beta_1 \alpha} \quad (\text{A.4})$$

The difference-in-differences strategy means that the  $\hat{\beta}_2^{DiD}$  estimate is derived after

differencing out the effect of the negative revenue change using the  $\hat{\beta}_1^{DiD}$  estimate.

Essentially, the Q2-raisers are used to estimate the following relationship:

$$\tilde{J}D_{kt}^{Q2} = \frac{\beta_1 + \beta_2}{1 + \beta_1\alpha + \beta_2\alpha} \tilde{\Delta}R_{kt}^- - \hat{\beta}_1^{DiD} \tilde{\Delta}R_{kt}^- + \zeta_{kt} \quad (\text{A.5})$$

Accordingly,

$$\mathbb{E} \left[ \hat{\beta}_2^{DiD} \right] = \frac{\beta_2}{(1 + \beta_1\alpha + \beta_2\alpha)(1 + \beta_1\alpha)} \quad (\text{A.6})$$

Since  $\beta_1 > 0$ ,  $\alpha > 0$ , and  $\beta_2 \geq 0$ , it is the case that the  $\mathbb{E} \left[ \hat{\beta}_2^{DiD} \right]$  will always be attenuated towards zero, relative to the true value of  $\beta_2$ .

## A.2 Biases in OLS regression of job destruction on real wage bill ratio

Due to a combination of measurement error in real wages and persistent unobserved confounders affecting both past wages and current-period job destruction, a simple OLS regression of the model in Equation 1.5 is unlikely to yield a causal estimate of the effect of having a higher real wage bill in 2008:Q4.

The fact that I construct the firm's real wage bill from workers' estimated persistent nominal wages implies that the real wage ratio is subject to measurement error. The measurement error may be correlated with the firm's job destruction rate because the post-Lasso estimation procedure is less likely to detect wage changes towards the end of a worker's tenure at a firm (because persistent changes are truncated upon job separation). If a firm has a higher job destruction rate in 2008:Q4,

then the start-of-quarter real wage estimates of laid-off full-year workers will tend to underestimate their true start-of-quarter real wages. Thus, the measurement error is likely to reinforce the omitted variable bias since it generates a negative correlation between the estimated real wage bill ratio and the firm's job destruction rate.

Any given firm will make wage change decisions by taking into account its expected future job creation and job destruction decisions. This creates a simultaneity problem whereby a firm's job destruction decision in period  $t$  could have affected the firm's wage setting behavior in period  $t - 1$  (from which the real wage ratio is constructed). The forward-looking nature of the firm's wage setting decisions implies that many unobserved factors could affect both the firm's start-of-quarter real wage ratio,  $W_{kt}$ , and the firm's job destruction rate - the most obvious being persistent productivity and product demand shocks. I would expect that persistent positive shocks from the recent past will increase the firm's start-of-quarter real wage ratio while decreasing its current period job destruction rate - generating a negative bias in coefficient estimates.

### A.3 Alternative IV results

In the baseline instrumental variable estimation described in Section 1.4.4, I include as an instrumental variable the firm's historical raise share in calendar quarter Q3 interacted with a 2008:Q4 indicator variable. This raise share may be endogenous since some portion of firms that historically tended to raise their workers' wages in Q3 would have done so after the Lehman Brothers bankruptcy.

The ability of some of these Q3-raisers to observe the negative shock in late 2008:Q3 may have endogenously affected their real wage bill ratio at the start of 2008:Q4. To test this, I estimate the same instrumental variable model but now include the  $t - 1$  historical raise share in 2008:Q4 (which corresponds to the Q3 calendar quarter) as a control variable instead of as an instrumental variable. The original OLS and IV results are reported in columns (1) and (2) for the unweighted and in columns (4) and (5) for the employment-weighted estimation models. Columns (3) and (6) report the IV results when I instead include the  $t - 1$  raise share in 2008:Q4 as a control variable.

Table A.1: Second-stage: Job destruction rate

Dependent Variable: Estimator:	Firm DHS Job Destruction Rate				
	OLS (1)	IV (2)	IV (3)	IV (5)	IV (6)
Real Wage Bill Ratio $W_{kt}$	<b>-0.19***</b> (0.004)	<b>1.26***</b> (0.04)	<b>1.25***</b> (0.04)	<b>-0.13***</b> (0.02)	<b>-0.67***</b> (0.22)
2008:Q4 * $W_{kt}$	<b>-0.15***</b> (0.03)	<b>3.56***</b> (0.55)	<b>7.85***</b> (1.09)	0.038 (0.07)	<b>3.20***</b> (0.83)
Employment Weighted Firm FE	N Y	N Y	N Y	Y Y	Y Y
2008:Q4 Raise Share $t - 1$	-	Instrument	Control	-	Instrument Control
Kleibergen-Paap rk LM		33.4	22.5		14.2
Anderson-Rubin Wald Test		342	407		18.6
R-Squared	0.050			0.054	
Observations			7.07 million		
Clusters			161,000 firm clusters		

*Note:* The outcome variable is the SEIN-level DHS job destruction rate. This is regressed on the predicted endogenous explanatory variables and a set of control variables. The control variables include firm-specific fixed effects as well as dummy variables for firm age, firm size, two-digit industry, and two-digit industry-specific shocks in 2008:Q4. Quarterly LEHD data from 1999:Q1 to 2014:Q4. Sample only includes firms with at least 10 raises observed prior to 2007:Q4. Robust standard errors clustered at the SEIN-level. \*\*\*, \*\*, \* indicate statistical significance at the 0.1%, 1.0%, and 5.0% levels, respectively. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0073-CED-20190910.

## Appendix B: Appendix to Chapter 2

### B.1 Employment and transition definitions

We use LEHD microdata for 11 states that have data available from 1994 to 2014.<sup>1</sup> Our definitions follow the notation established by Abowd, Stephens, Vilhuber, Andersson, McKinney, Roemer and Woodcock (2009), augmented to include employer-to-employer transitions by Hyatt et al. (2014). The starting point is earnings for individual  $i$  from employer  $j$  in quarter  $t$ , denoted  $w_{ijt}$ . If an individual has no earnings from an employer in a given quarter, then the worker did not receive unemployment insurance taxable income from that employer during that quarter. Otherwise, if the worker did receive positive earnings from that employer ( $w_{ijt} > 0$ ), then the worker worked for the employer. Earnings are in real 2014 dollars. The following definitions allow us to measure employment and transitions in administrative records that lack start and end dates.

---

<sup>1</sup>Note that hours data are not available for any state but Washington for our 11 state set in the analysis time period, and we are not allowed to release any results for particular U.S. states in this paper.

### B.1.1 Employment concepts

We consider the jobs that span two consecutive quarters (often called “beginning of quarter” jobs). By definition, in such jobs the employee was employed by the employer at the time of the break between the quarters. This employment measure therefore may reasonably be interpreted as indicative of point-in-time employment. Formally, a worker is employed at the beginning of quarter  $t$  when

$$b_{ijt} = \begin{cases} 1, & \text{if } w_{ijt-1} > 0 \text{ and } w_{ijt} > 0 \\ 0, & \text{otherwise.} \end{cases}$$

For any two-quarter pair, we disambiguate the data by considering jobs that are maximal earning among all jobs a worker holds at the beginning of quarter  $t$ . To do so, the job with the greatest earnings summed across quarter  $t - 1$  and  $t$  is identified, as follows:

$$domb_{ijt} = \begin{cases} 1, & \text{if } b_{ijt} = 1 \text{ and} \\ & w_{ijt} + w_{ijt-1} > w_{ikt} + w_{ikt-1} \forall k \\ & \text{s.t. } b_{ikt} = 1 \text{ and } j \neq k \\ 0, & \text{otherwise.} \end{cases}$$

The set of jobs defined in  $domb_{ijt}$  are those we use in all of our empirical analysis. Such jobs are unique at the person-quarter level.

### B.1.2 Transition concepts

We consider transitions between dominant job status across quarters. These are worker movements between employers, as well as into and from nonemployment.

We consider within-quarter transitions

$$wq_{ijkt} = \begin{cases} 1, & \text{if } domb_{ijt} = 1 \text{ and } domb_{ikt+1} = 1 \\ & \text{and } j \neq k \\ 0, & \text{otherwise,} \end{cases}$$

as well as adjacent quarter transitions

$$aq_{ijkt} = \begin{cases} 1, & \text{if } domb_{ijt-1} = 1 \text{ and } domb_{ikt+1} = 1 \\ & \text{and } domb_{ilt} \neq 1 \forall l \text{ and } j \neq k \\ 0, & \text{otherwise.} \end{cases}$$

Flows into persistent nonemployment in quarter  $t$  have full-quarter earnings when:

$$en2\_doms2_{ijt} = \begin{cases} 1, & \text{if } domb_{ijt} = 1 \\ & \text{and } domb_{ilt+1} \neq 1 \forall l \\ & \text{and } domb_{imt+2} \neq 1 \forall m \\ 0, & \text{otherwise,} \end{cases}$$

Flows from persistent nonemployment into employment in quarter  $t$  have full quarter earnings when:

$$ne2\_doma2_{ikt} = \begin{cases} 1, & \text{if } domb_{ikt+1} = 1 \\ & \text{and } domb_{ilt} \neq 1 \forall l \\ & \text{and } domb_{imt-1} \neq 1 \forall m \\ 0, & \text{otherwise,} \end{cases}$$

We also consider workers who did not change jobs, who are called “job stayers.”

$$dombe_{ijt} = \begin{cases} 1, & \text{if } domb_{ijt} = 1 \text{ and } domb_{ijt+1} = 1 \\ 0, & \text{otherwise.} \end{cases}$$

There are, therefore, seven transition concepts: four for employer-to-employer transitions, two for transitions into and from nonemployment, and an exhaustive residual for those with dominant employers, job stayers.

In addition to these, we create an additional nonemployment hire measure that is useful when calculating a firm's rank when hiring from poaching. This measure excludes recalls.

$$ne2_norecall_{ikt} = \begin{cases} 1, & \text{if } domb_{ikt+1} = 1 \\ & \text{and } domb_{ilt} \neq 1 \forall l \\ & \text{and } domb_{imt-1} \neq 1 \forall m \\ & \text{and } domb_{ikt-2} \neq 1 \\ 0, & \text{otherwise.} \end{cases}$$

### B.1.3 Aggregation

We consider the evolution of total consecutive quarter employment. For workers in group  $i$  and firms in group  $j$ , this is expressed as:

$$E_{ijt} = \sum_{ij} b_{ijt+1}.$$

Total employment evolves via poaching hires and hires from nonemployment. Total poaching hires for workers in group  $i$  and firms in group  $k$  are:

$$P_{ikt}^a = \sum_{ik} (wq_{ijkt} + aq_{ijkt}).$$

Total poaching separations for workers of group  $i$  from firms of group  $j$  are:

$$P_{ijt}^s = \sum_{ij} (wq_{ijkt} + aq_{ijkt-1}).$$

Total nonemployment hires for workers of group  $i$  into firms of group  $k$  are:

$$N_{ikt}^a = \sum_{ik} en2\_doma2_{ikt}.$$

Total nonemployment separations for workers of group  $i$  from firms of group  $j$  are:

$$N_{ijt}^s = \sum_{ij} en2\_doms2_{ijt}.$$

## B.2 Worker ranking implementation details

We here describe in detail each of our four worker and firm ranking algorithms. Earnings are in logs throughout. Whenever earnings are applied in a ranking method, the earnings concept used in ranking is the same as that used to determine a worker's dominant employer in Appendix A, that is  $w_{ijt} + w_{ijt-1}$ .

### B.2.1 Method 1: Worker nonemployment duration and firm poaching hire share

Our third method of ranking workers and firms involves ranking methods that can be implemented quickly on administrative records data. Specifically, we rank firms on the basis of the share of hires that come from poaching relative to nonemployment, as higher productivity firms ought to obtain workers from other

firms more frequently than lower productivity firms. Workers are ranked on the basis of the amount of time they spend employed, the assumption being that more productive workers are more likely to be employed rather than nonemployed.

### B.2.1.1 Ranking firms by poaching share of hires

In a manner similar to Bagger and Lentz (2019), we rank firms according to each firm’s share of hires that are poached from other firms (as opposed to being hired from non-employment). We begin by identifying the total hires from either employment or from non-employment for each firm in the 11 states of the LEHD microdata. We include as employer-to-employer transitions hires both same-quarter  $wq_{ijkt}$  and adjacent-quarter  $aq_{ijkt}$  transitions. A same-quarter transition occurs if the worker has positive earnings from both the previous and the new employer in the transition quarter. An adjacent-quarter transition occurs in period  $t$  if the worker both has positive earnings from the old employer, but not the new employer, in period  $t$ ; and has positive earnings from the new employer, but not the old employer, in period  $t + 1$ . For the calculation of a firm’s nonemployment hires, we exclude all one-quarter recall hires, and so we use  $ne2\_norecall_{ikt}$ . We define a one-quarter recall hire as a three-quarter employment pattern of employment-to-nonemployment-to-employment, where the worker’s dominant employer was the same in the first and last quarter and the worker was non-employed for exactly one full calendar quarter in between.

We estimate the poaching share of hires for each firm  $k$  as the ratio of hires

from other employers to total hires, as follows:

$$\frac{\sum_k wq_{ijkt} + aq_{ijkt}}{\sum_k ne2\_norecall_{ikt}}$$

Firms are then rank ordered into 50 bins according to their poaching share.

### B.2.1.2 Ranking workers by prime-age employment rates

We rank workers by their prime-age quarterly employment rate relative to the average employment rate for individuals born in the same year. For each worker, we construct a 0-1 employment indicator variable for every quarter that the worker is between the ages of 25 to 55 (inclusive). This employment indicator variable is set to one if the worker had positive earnings in that quarter and to zero if they were non-employed for the entire calendar quarter.

We then divide workers into cohorts according to their year of birth. For every quarter, we compute the average employment rate of each birth cohort as the average of the employment indicator for all individuals in that birth cohort in the given quarter. For every quarter in which a worker is between the ages of 25-55, we calculate the deviation of the worker's employment indicator from the birth-cohort average employment rate for the given quarter. The worker's prime-age employment rate is simply the sum of the worker's deviations from the birth-cohort average divided by the number of observed quarters in the LEHD micro data for which the worker was between the ages of 25-55. The worker ranking is determined by a rank ordering of workers into 50 bins according to their prime-age quarterly

employment rate.

## B.2.2 Method 2: Average earnings and labor productivity

### B.2.2.1 Ranking workers based on average earnings

In our fourth method, we rank workers in a way that is motivated by the fact that high-rank workers may exhibit higher average earnings. We simply rank workers by the average of their residual earnings after controlling for age and time-period fixed effects. Note that this is the initial guess of a worker’s rank in our additive model (Method 3) and our reranking workers and surplus approach (Method 4).

### B.2.2.2 Ranking firms based on revenue productivity

We use revenue data from the U.S. Census Bureau’s Business Register to measure labor productivity, i.e., revenue-per-worker. We use all available revenue data from 1994-2014.<sup>2</sup> These revenue data are annual totals. Multiple observations of revenue data are available for each business in each calendar year, and we use revenue data either from the first year with a reported amount, as well as the second year that a recorded amount is available, with priority given to the latter. These data are Winsorized at both the top and bottom 1% of the revenue distribution.

Not all businesses have revenue data in all years. In some cases, a crosswalk was not available between the LEHD employer data and the Business Register (i.e., missing firm identifier), and in others revenue data was missing from the Business

---

<sup>2</sup>Recent work by Haltiwanger et al. (2017) uses the same source data to create firm-level measures of labor productivity for a shorter set of years, and a subset of industries.

Register. We therefore impute these data elements when they are missing, assuming that they are missing-at-random within quarter firm industry, size, and age categories.

Specifically, we assume that revenue is the following linear function of log firm size ( $fsize$ ) and age ( $fage$ ), estimated separately by quarter and four-digit NAICS code:

$$lp = \beta_0^a + \beta_1^a * fsize + \beta_2^a * fage + \beta_3^a * fsize * fage + \beta_4^a * fsize^2 + \beta_5^a * fage^2$$

where  $lp$  is log labor productivity,  $firmage$  is log firm age, and  $firmsize$  is log firm size.

The distribution of the Business Register revenue data shifts discontinuously upward around the year 2002, when the Business Register was redesigned. This is because additional data elements concerning revenue became available and more accurate totals are available. Since we do not want the firms in more recent years to appear more productive simply because of a change in reporting, we also implement a simple imputation. The revenue data for 2000 is all provided under the old regime, that for 2002, all under the new, and the year 2001 is a mix of old and new. We therefore take all businesses that existed in the year 2000 and 2002 and use this as training data for imputation of

$$lp_n = \beta_0^b + \beta_1^b * lp_o + \beta_2^b * lp_o^2 + \beta_3^b * fsize + \beta_4^b * fage + \beta_5^b * fsize * fage + \beta_6^b * fsize^2 + \beta_7^b * fage^2$$

where  $lp_n$  is 2002 revenue data and  $lp_o$  is revenue data from the year 2000 or earlier.

Having attached revenue to all firms in the LEHD data, we proceed in a simple manner to produce firm ranks based on revenue. We rank firms based on the residual firm productivity from year of entry by quarter by industry dummy variable regression. We then add this residual to the value-added per worker data as published by the Bureau of Economic Analysis to obtain a proxy for firm-level value added per worker. We then rank firms based on the average of this sum, over time.

### B.2.3 Method 3: Additive worker and firm effects

We estimate worker and firm fixed effects via an iterative algorithm that follows Guimaraes and Portugal (2010). We fit the following model for earnings outcomes:

$$W = B\xi + D\theta + F\psi$$

where  $W$  is the  $N \times 1$  dimensional vector total earnings observations  $w_{ijt}$ ,  $B$  is an  $N \times G_B$  dimensional matrix of birth cohort by time fixed effects,  $D$  is an  $N \times G_D$  dimensional matrix of person-specific fixed effects, and  $F$  is an  $N \times G_F$  dimensional matrix of firm effects. Our goal is to recover the  $1 \times G_B$  dimensional vector  $\xi$  of

fixed effects for birth cohort  $c$  at time  $t$   $\xi_{ct}$ , the  $1 \times G_D$  dimensional vector  $\theta$  of person-specific fixed effects, and the  $1 \times G_F$  dimensional vector  $\psi$  of firm-specific fixed effects.

We can express the least-squares formula for this problem in terms of a cross-product matrix similar to Abowd, Kramarz and Margolis (1999):

$$\begin{bmatrix} B'B & B'D & B'F \\ D'B & D'D & D'F \\ F'B & F'D & F'F \end{bmatrix} \begin{bmatrix} \xi \\ \theta \\ \psi \end{bmatrix} = \begin{bmatrix} B'W \\ D'W \\ F'W \end{bmatrix}$$

which, after rearranging terms, can be expressed as:

$$\begin{bmatrix} B'B\xi + B'D\theta + B'F\psi = B'W \\ D'B\xi + D'D\theta + D'F\psi = D'W \\ F'B\xi + F'D\theta + F'F\psi = F'W \end{bmatrix}.$$

which is a system of three equations. Solving each of these independently yields:

$$\begin{bmatrix} \xi = (B'B)^{-1}B'(W - D\theta - F\psi) \\ \theta = (D'D)^{-1}D'(W - B\xi - F\psi) \\ \psi = (F'F)^{-1}F'(W - B\xi + D\theta) \end{bmatrix}.$$

We iterate among these sets of equations to obtain the least squares solution. In fact, solving each of these equations can be done using group means since all of our independent variables are dummy variables. As the datasets we use in this analysis contains billions of person-quarter observations (e.g. 50 million workers

times twenty quarters implies one billion person-quarter observations), omitting computational matrix inversion allows us to greatly speed up our computation time. To see how we can skip having a computer run a tediously slow regression program on our massive dataset, note that the first equation of our system specifies a separate indicator for each birth cohort  $c$  at each quarter in time,

$$\xi_{ct} = \frac{1}{\sum_{ij} \mathbb{1}(w_{ijct} > 0)} \sum_{ij} (w_{ijct} - \theta'_i d_i - \psi'_j f_j)$$

and for each worker  $i$ ,

$$\theta_i = \frac{1}{\sum_{jct} \mathbb{1}(w_{ijct} > 0)} \sum_{jct} (w_{ijct} - \xi_{cb} b_{cb} - \psi'_j f_j)$$

and for each firm  $j$ ,

$$\psi_j = \frac{1}{\sum_{ict} \mathbb{1}(w_{ijct} > 0)} \sum_{ict} (w_{ijct} - \xi'_i b_i - \theta'_i d_i).$$

In other words, these are the least squares solutions to a high-dimensional set of mutually exclusive indicator variables. The least squares solutions are sample means of residuals, which can be calculated directly without having a computer multiply or invert matrices.

We can now solve for  $\theta_i$ ,  $\psi_j$ , and  $\xi_{ct}$  for the universe of our 11 states of linked employer-employee data. We first compute the average log earnings of each birth cohort by time cell  $\hat{\xi}_{ct} = \sum_{ij} w_{ijct}$  of each worker, this is our initial guess of the birth cohort by time effect. We then proceed as follows:

1. Estimate the initial worker effects  $\hat{\theta}_i = w_{ijt} - \hat{\xi}_{ct}$ .
2. Estimate the initial firm effects  $\hat{\psi}_j = w_{ijt} - \hat{\xi}_{ct}\hat{\theta}_i$ .
3. Update the birth cohort by time effects  $\hat{\xi}_{ct} = w_{ijt} - \hat{\theta}_i - \hat{\psi}_j$ ,
4. Update the worker effects  $\hat{\theta}_i = w_{ijt} - \hat{\psi}_j - \hat{\xi}_{ct}$ .
5. Update the firm effects  $\hat{\psi}_j = w_{ijt} - \hat{\theta}_i - \hat{\xi}_{ct}$ .
6. Proceed back to step 3 until a goodness-of-fit criterion is reached.

We then group each of the employment-weighted firm effects  $\hat{\psi}_j$ , and the participation-weighted worker effects  $\hat{\theta}_i$  into terciles.

## B.2.4 Method 4: Worker reranking and surplus

We implement an algorithm for ranking workers and firms that borrows heavily from Hagedorn, Law and Manovskii (2017). It is substantially simplified and was not intended to be a direct replication of this method.

### B.2.4.1 Worker residuals for ranking

The first part of our algorithm calculates residual earnings that will then serve as the starting point for the ranking algorithm. We first calculate average log earnings by birth cohort  $c$  (specifically, year of birth) by quarter in time  $t$ . We then estimate an initial guess of worker productivity as the deviation of that worker's earnings from the birth cohort by time mean.

### B.2.4.2 Reranking workers to minimize disagreement

We use the rank order of these residuals as the initial guess of a worker's rank, where workers with a higher residual earnings are more productive. We then look at workers who are employed by the same firm. We evaluate the goodness of fit of our worker ranks as the fraction of the time that a higher ranked worker earns more at a particular firm than a lower ranked worker. We assume that wage observations are the true wages plus iid measurement error. So the observed wage of worker  $i$  at firm  $k$  in period  $t$  is

$$\hat{w}_{i,k,t} = w_{i,k} + \varepsilon_t$$

where  $w_{i,k}$  is the true wage and  $\varepsilon_t$  is iid noise. Then  $n_{i,k}$  is the completed tenure of the worker, the difference in observed wages is

$$\bar{w}_{i,k} - \bar{w}_{j,k} = w_{i,k} - w_{j,k} + \frac{1}{n_{i,k}} \sum_{t=1}^{n_{i,k}} \varepsilon_{i,k,t} - \frac{1}{n_{j,k}} \sum_{t=1}^{n_{j,k}} \varepsilon_{j,k,t}.$$

Suppose that the prior is

$$w_{i,k} \sim \mathcal{N}(\mu_0, \tau_0^2).$$

Then the posterior of  $w_{i,k}$ , given  $Var(\varepsilon_t) = \sigma^2$  is

$$p(w_{i,k} | \bar{w}_{i,k}, n_{i,k}) = \mathcal{N}(\mu_n, \tau_n^2)$$

where  $\mu_n$  is the precision-weighted average of the means

$$\mu_n = \frac{\frac{1}{\tau_0^2} \mu_0 + \frac{n_{i,k}}{\sigma^2} \bar{w}_{i,k}}{\frac{1}{\tau_0^2} + \frac{n_{i,k}}{\sigma^2}}$$

and

$$\frac{1}{\tau_n^2} = \frac{1}{\tau_0^2} + \frac{n_{i,k}}{\sigma^2}.$$

We assume an uninformative prior:  $\tau_0^2 \rightarrow \infty$ . The expressions simplify to

$$\mu_n = \bar{w}_{i,k}$$

and

$$\frac{1}{\tau_n^2} = \frac{n_{i,k}}{\sigma^2}.$$

The “posterior” densities are then

$$p(w_{i,k} | \bar{w}_{i,k}, n_{i,k}) = \mathcal{N}\left(\bar{w}_{i,k}, \frac{\sigma^2}{n_{i,k}}\right)$$

$$p(w_{j,k} | \bar{w}_{j,k}, n_{j,k}) = \mathcal{N}\left(\bar{w}_{j,k}, \frac{\sigma^2}{n_{j,k}}\right)$$

Since everything is independent, the difference in average wages is also normal:

$$p(w_{i,k} - w_{j,k} | \bar{w}_{i,k}, n_{i,k}, \bar{w}_{j,k}, n_{j,k}) = \mathcal{N}\left(\bar{w}_{i,k} - \bar{w}_{j,k}, \frac{\sigma^2}{n_{i,k}} + \frac{\sigma^2}{n_{j,k}}\right)$$

Then we can compute the probability that  $w_{j,k} < w_{i,k}$  using the normal CDF:

$$\mathbb{P}(w_{j,k} < w_{i,k}) = \Phi \left( \frac{\bar{w}_{i,k} - \bar{w}_{j,k}}{\sqrt{\frac{\sigma^2}{n_{i,k}} + \frac{\sigma^2}{n_{j,k}}}} \right)$$

The true ranking of workers is given by  $\Pi(i, j)$ , where  $\Pi(i, j) = 1$  if  $i$  is (strictly) preferred to  $j$  and  $\Pi(i, j) = 0$  otherwise. Let  $c(i, j)$  be the probability that  $\Pi(i, j) = 1$ .

If  $k$  is the only firm where  $i$  and  $j$  both worked, then

$$c(i, j) = \Phi \left( \frac{\bar{w}_{i,k} - \bar{w}_{j,k}}{\sqrt{\frac{\sigma^2}{n_{i,k}} + \frac{\sigma^2}{n_{j,k}}}} \right)$$

Otherwise, we set

$$c(i, j) = \prod_{k \in E(i, j)} \Phi \left( \frac{\bar{w}_{i,k} - \bar{w}_{j,k}}{\sqrt{\frac{\sigma^2}{n_{i,k}} + \frac{\sigma^2}{n_{j,k}}}} \right)$$

where  $E(i, j)$  is the set of firms that have employed both  $i$  and  $j$ , and the product symbol should not be confused with the ranking  $\Pi(i, j)$ .

We estimate  $\Pi$  by choosing  $\hat{\Pi}$  to maximize the number of so-defined correctly ranked workers. Specifically, we seek a transitive, complete ordering  $\hat{\Pi}$  that solves

$$\arg \max_{\hat{\Pi}} \sum_{j=1}^{j=N} \sum_{i=j+1}^N \left\{ c(i, j) \hat{\Pi}(i, j) + c(j, i) \hat{\Pi}(j, i) \right\}$$

where

$$c(i, j) = \prod_{k \in E(i, j)} \Phi \left( \frac{\bar{w}_{i,k} - \bar{w}_{j,k}}{\sqrt{\frac{\sigma^2}{n_{i,k}} + \frac{\sigma^2}{n_{j,k}}}} \right)$$

$$\bar{w}_{i,k} = \frac{1}{n_{i,k}} \sum_{t=1}^{t=n_{i,k}} w_{i,k,t}.$$

We start with an initial guess and make a single arbitrary move, and check the goodness-of-fit measure to see whether it improves. Our method is as follows:

1. Start with an initial ranking  $\hat{\Pi}_0$ . Note that  $i$  and  $j$  are worker names. Any ranking  $\hat{\Pi}_n$  implies at function  $r_n(i)$ , which returns the rank (on  $\{1, 2, \dots, N\}$ ) of the worker  $i$ .
2. Starting from a ranking  $\hat{\Pi}_n$  choose a random worker name  $i$  from  $\{1, 2, \dots, N\}$  and a random worker rank  $r$  from  $\{1, 2, \dots, N\}$ .
3. If changing the rank of worker  $i$  from  $r_n(i)$  to  $r$  improves the fit, make this change. Otherwise do nothing.
4. Return to Step 2. Repeat until no more single move rerankings can be made, or some weaker condition is met.

Worker ranks are grouped into three employment-weighted groups: low, middle, and high.

### B.2.4.3 Surplus-based firm ranking

Pool of nonemployed by worker type : For each worker, we identify the worker as nonemployed in a given quarter if the quarter falls between the workers' first and last quarters of observed earnings and the worker had zero UI earnings for the quarter. We then sum the total number of nonemployed workers in each quarter for each estimated worker type  $\hat{x}$ . This corresponds to the pool of unemployed,  $u(\hat{x})$ , used in the Hagedorn, Law and Manovskii (2017) IDNoise Algorithm.

The IDNoise algorithm : To address noise in the classification of workers' types, Hagedorn, Law and Manovskii (2017) propose an algorithm called IDNoise that aims to identify workers whose worker types are particularly unusual given the set of worker types employed by the workers' employers. Hagedorn, Law and Manovskii (2017) assign these workers with noisy worker types to a set  $\hat{N}$ . For each firm  $j$ , the IDNoise algorithm identifies  $\hat{B}(\hat{x}, j)$ , a set of "cleaned" worker types that the firm hires from nonemployment. The algorithm works as follows for each firm  $j$ .

1. Compute the following four firm-specific variables:
  - $N(j)$ : The number of workers hired from nonemployment by firm  $j$
  - $p(\hat{x}, j)$ : The number of workers of estimated type  $\hat{x}$  hired from nonemployment by firm  $j$
  - $\pi(\hat{x}, j)$ : The theoretical fraction of workers of type  $\hat{x}$  hired from nonemployment by firm  $j$ , which is a function of the types of workers that the

firm hires and the relative number of this worker-type in the pool of nonemployed workers:

$$\pi(\hat{x}, j) = \frac{u(\hat{x})\mathbb{1}[p(\hat{x}, j) > 0]}{\sum_{\hat{x}} u(\hat{x})\mathbb{1}[p(\hat{x}, j) > 0]} \quad (\text{B.1})$$

- $F(p(\hat{x}, j), \pi(\hat{x}, j), N(j))$ : The probability of observing at most  $p(\hat{x}, j)$  hires from nonemployment given the probability  $\pi(\hat{x}, j)$  from  $N(j)$  trials. Assuming that these hires from nonemployment are random draws from the pool of nonemployed workers matching the firm's worker types,  $F(p(\hat{x}, j), \pi(\hat{x}, j), N(j))$  is:

$$F(p(\hat{x}, j), \pi(\hat{x}, j), N(j)) = \sum_{i=0}^{p(\hat{x}, j)} \binom{N(j)}{i} \pi(\hat{x}, j)^i (1 - \pi(\hat{x}, j))^{N(j)-i} \quad (\text{B.2})$$

2. For each worker type  $\hat{x}$ , initialize  $\hat{\mathbb{B}}(\hat{x}, j) = 1$  if the firm hires any workers of that estimated type ( $p(\hat{x}, j) > 0$ )
3. \* for all worker types,  $\hat{x}$ , with  $\hat{\mathbb{B}}(\hat{x}, j) = 1$ 
  - If the worker type,  $\hat{x}$ , is the lowest ( $=1$ ) or highest ( $=50$ ) worker types and  $F(p(\hat{x}, j), \pi(\hat{x}, j), N(j)) \leq 0.1$ , then set  $\hat{\mathbb{B}}(\hat{x}, j) = 0$  and return to \*.
  - For all other worker types, if either  $\hat{\mathbb{B}}(\hat{x} - 1, j) = 0$  or  $\hat{\mathbb{B}}(\hat{x} + 1, j) = 0$  and  $F(p(\hat{x}, j), \pi(\hat{x}, j), N(j)) \leq 0.1$ , then set  $\hat{\mathbb{B}}(\hat{x}, j) = 0$  and return to \*.

After computing the set of types hired by each firm,  $\hat{\mathbb{B}}(\hat{x}, j)$ , a worker  $i$ , with

estimated type  $\hat{x}(i)$  is assigned to the set  $\hat{\mathbb{N}}$  if they are ever employed by a firm  $j$  where  $\hat{\mathbb{B}}(\hat{x}(i), j) = 0$ .

Identifying the reservation wage of each worker type : When determining the reservation wages of each worker type, we follow Hagedorn, Law and Manovskii (2017) in excluding the earnings histories of any worker  $i$  with a noisy worker type ( $i \in \hat{\mathbb{N}}$ ). The reservation wage for each worker type  $\hat{x}$  is calculated using the remaining workers as follows:

1. Construct the set  $J(\hat{x})$  which consists of all firms  $j$  that hire any worker of type  $\hat{x}$  from nonemployment.
2. For each firm  $j \in J(\hat{x})$ , compute  $\bar{w}(\hat{x}, j)$ , the average wage paid by firm  $j$  to workers of type  $\hat{x}$  hired from nonemployment.
3. We define the reservation wage for type  $\hat{x}$ ,  $w^r(\hat{x})$ , is the 10th percentile of the set of  $w(\hat{x}, j)$  where  $j \in J(\hat{x})$ . Note that Hagedorn, Law and Manovskii (2017) propose using the minimum average wage as the reservation wage, but we find that this is a very noisy signal, whereas the 10th percentile is smoothly increasing in worker type.

Ranking firms by their average wage premium : Following Hagedorn, Law and Manovskii (2017), we rank firms by the product of their average wage premium and

their job filling rate. The average wage premium of firm  $j$ ,  $\Omega^u(j)$  is:

$$\Omega^u(j) = \sum_{\hat{x} \text{ s.t. } \hat{\mathbb{B}}(\hat{x},j)=1} \frac{\frac{u(\hat{x})}{U} (\bar{w}(\hat{x},j) - w^r(\hat{x}))}{\sum_{\hat{x} \text{ s.t. } \hat{\mathbb{B}}(\hat{x},j)=1} \frac{u(\hat{x})}{U}} \quad (\text{B.3})$$

The job filling rate for firm  $j$  is a function of the probability that the firm encounters an unemployed worker,  $\mathbb{M}_v$ , times the probability that the worker's type,  $x(i)$ , matches the firm's set of acceptable worker types ( $\hat{\mathbb{B}}(\hat{x}(i),j) = 1$ ). Since the probability that a firm encounters an unemployed worker is constant across all firms, this is simply a scalar factor in the firm ranking and we thus ignore it. Calculate the probability that the encountered workers' type  $x(i)$  matches the firm's set of acceptable worker types,  $\tilde{q}^u(j)$ , as:

$$\tilde{q}^u(j) = \sum_{\hat{x} \text{ s.t. } \hat{\mathbb{B}}(\hat{x},j)=1} \frac{u(\hat{x})}{U} \quad (\text{B.4})$$

## B.3 Supplemental tables and figures

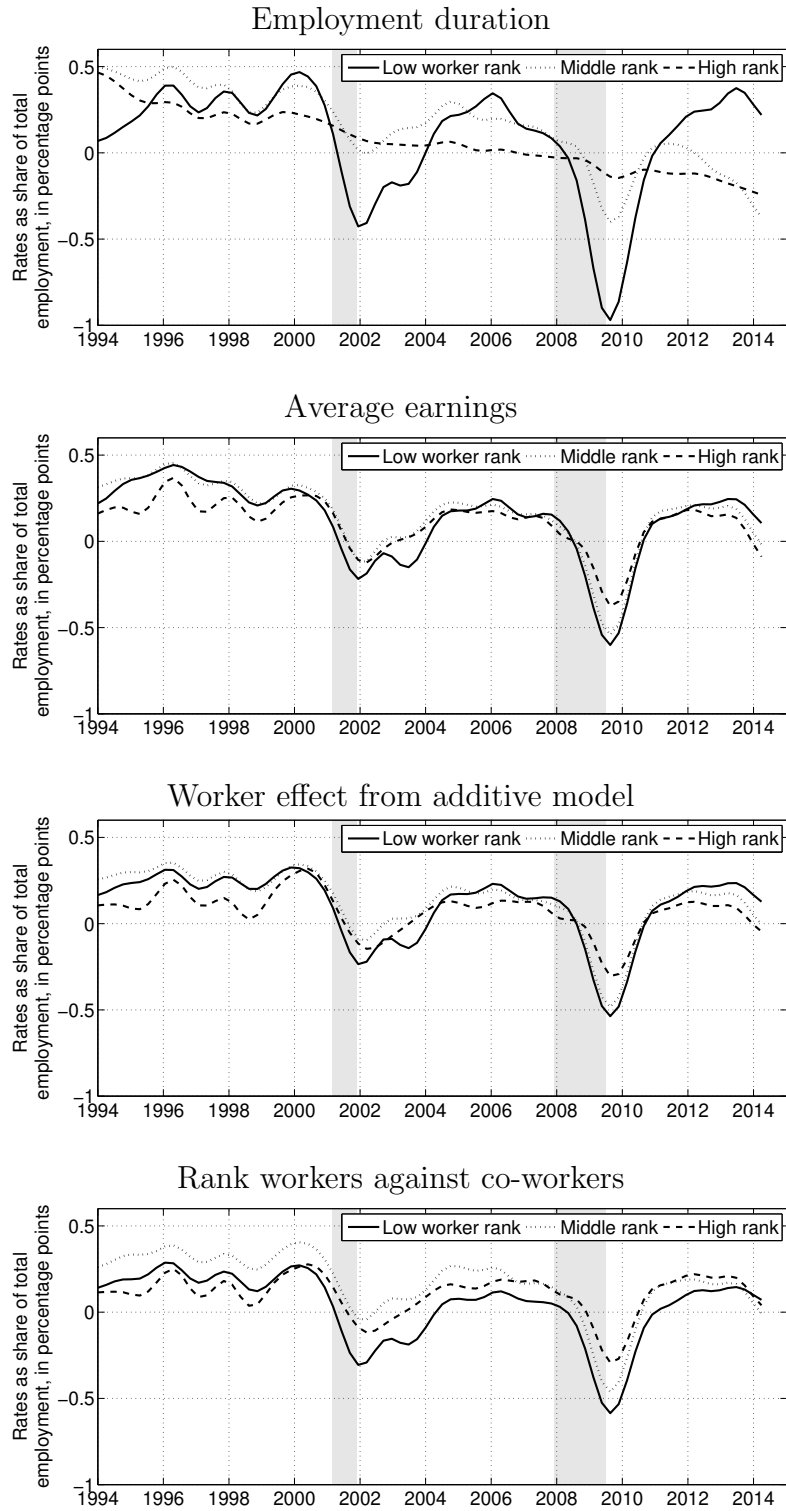
### B.3.1 Poaching vs. nonemployment margins

These changes in employment shares by type are determined by labor market transitions into and out of nonemployment, as well as across employers. We show these transition rates in Figure B.1. Figure B.1 shows net hires from nonemployment by worker type. Net employment growth declines sharply during recessions for all three types of workers. The 2007-2009 recession has more of a decline in employment than the 2001 recession. However, for high productivity workers, especially in the 2007-2009 recession, their employment did not decline nearly as much as it did for the lower productivity groups. When considering the employment transitions across firms of different types, it is helpful to keep in mind the findings of Haltiwanger, Hyatt, Kahn and McEntarfer (2018a) that firms that are higher-ranked in the job ladder are net poachers, and that low-rank firms rely disproportionately on nonemployment to obtain their workers. Figure B.2 shows net hires from nonemployment by firm type. There are level differences between the types of firms, with low-rank firms having more net hiring from nonemployment than the other two groups. Despite these level differences, the cyclicalities are similar, with net nonemployment hiring falling sharply during the two recessions. Figure B.3 shows net poaching by firm type. Note that net poaching for each worker type is equal to zero by construction (each employer-to-employer transition contributes exactly one poaching gain and one poaching loss). Low-rank firms lose workers via poaching flows, and high-

rank firms gain workers throughout the time period, but this movement away from low-rank firms and toward high-rank firms slows substantially during recessions.

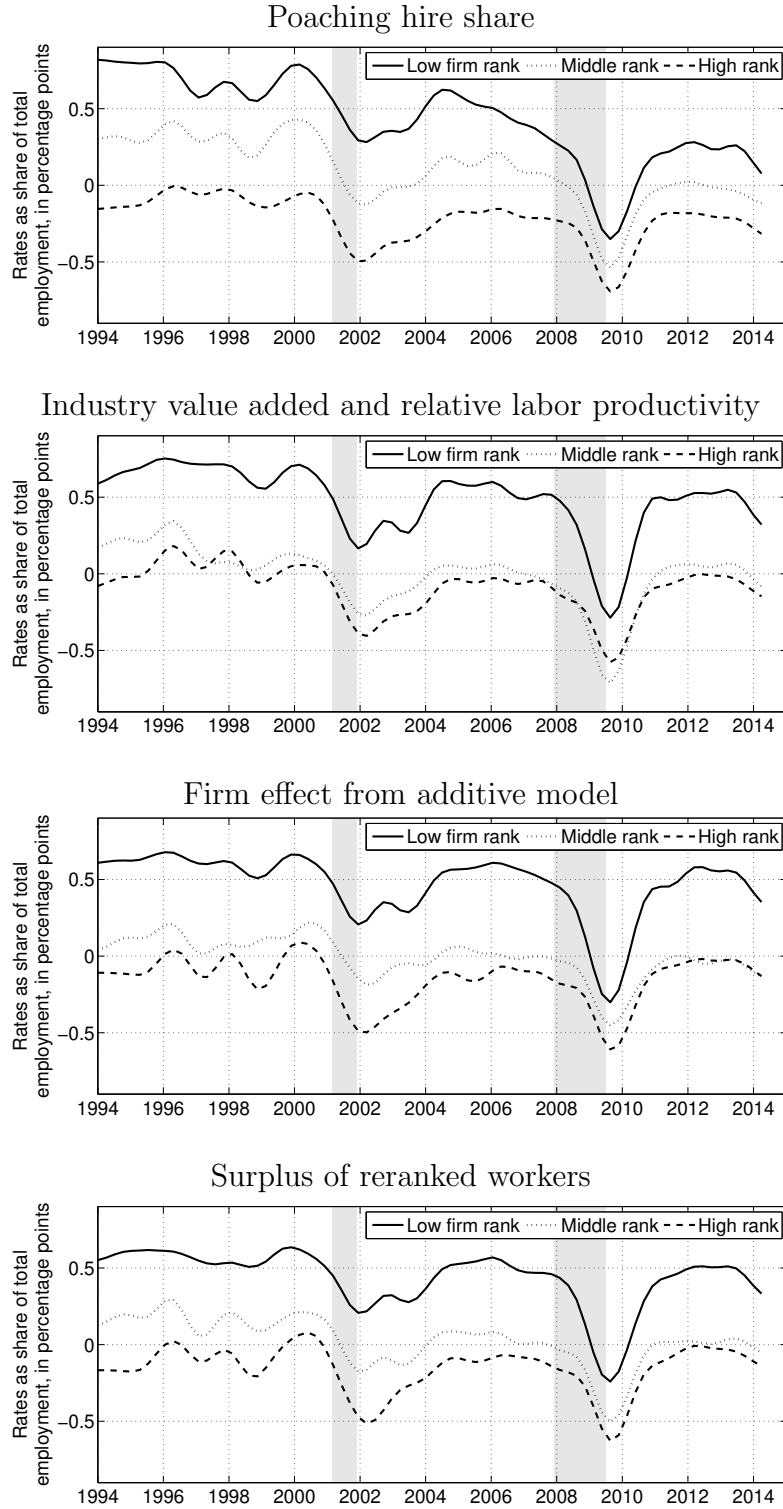
Figures B.1, B.2, and B.3 help illustrate how the employment composition effect led to a larger build-up at low-rank firms in the wake of the 2001 recession than the 2007-2009 recession. Following Haltiwanger, Hyatt, Kahn and McEntarfer (2018a), in order to see a counter-cyclical build-up at the low-end of the job ladder, the “poaching margin” must overwhelm the “nonemployment margin.” In other words, the countercyclical decline in the movement of workers from low-rank firms to high-rank firms must be larger than the decline in nonemployment for low-rank firms. In the wake of the 2001 recession, there was relatively little change in the difference in nonemployment hiring for high- vs. low-rank firms and so the change in poaching dominates. However, in the 2007-2009 recession the excess nonemployment hiring by low-rank firms shut down, mitigating the build-up in the share of employment at low-rank firms.

Figure B.1: Percent change in worker employment



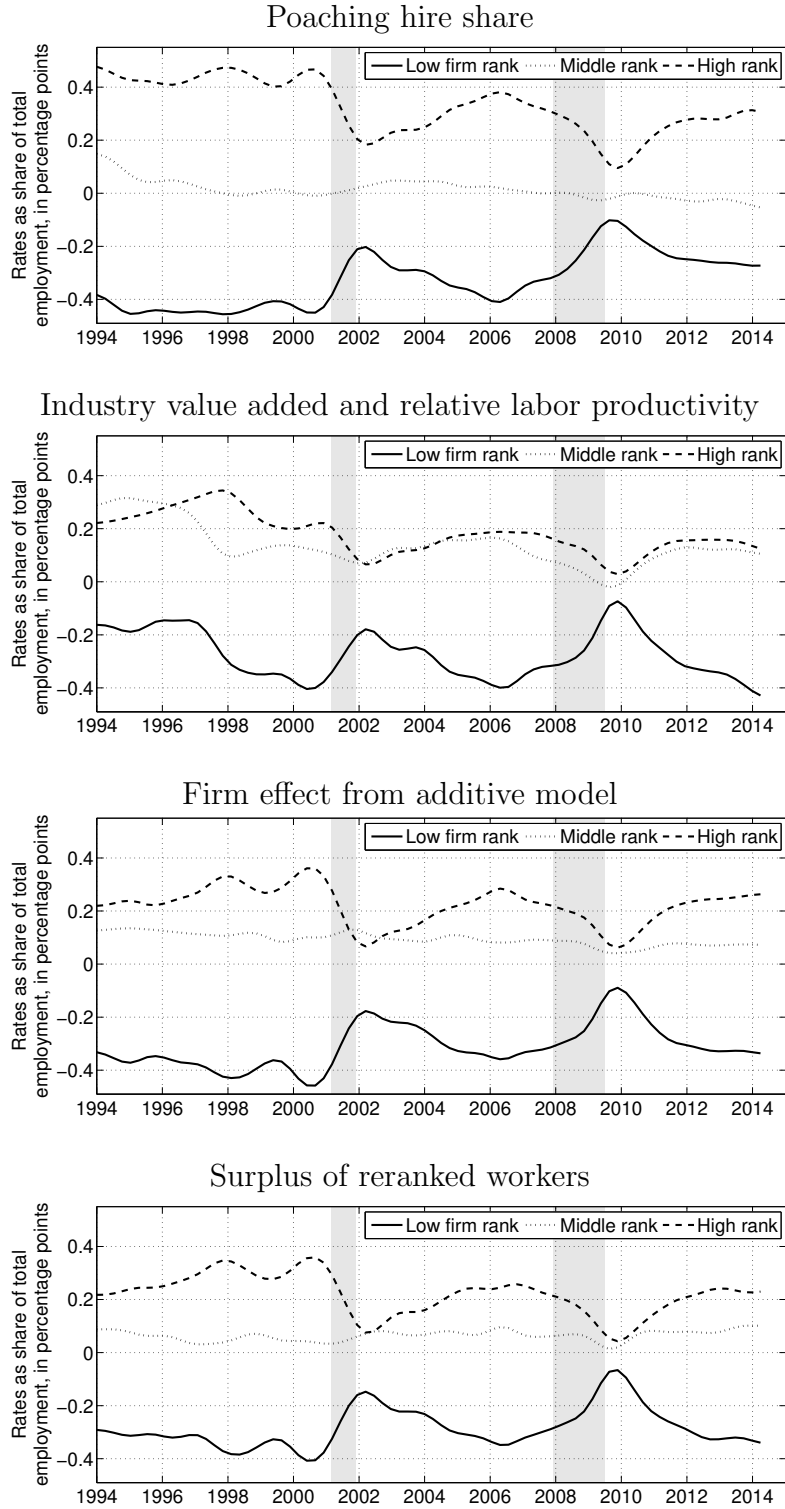
Notes: Shaded regions indicate recessions. Data seasonally adjusted and Henderson-filtered using X11.

Figure B.2: Percent change in firm employment: Nonemployment



*Notes:* Shaded regions indicate recessions. Data seasonally adjusted and Henderson-filtered using X11.

Figure B.3: Percent change in firm employment: poaching



Notes: Shaded regions indicate recessions. Data seasonally adjusted and Henderson-filtered using X11.

### B.3.2 Worker-firm rank shares

Now, we turn from composition to sorting. Cyclical changes are shown in Tables B.1 and B.2. These measure the frequency with which workers in the “low,” “middle,” and “high” categories are employed at similarly distinguished types of firms. These shares are as a fraction of total employment, and so e.g. the share of low-rank workers in all three firm categories sum to the share of low-rank workers.

Table B.1: Change in share and unemployment (HP)

	Nonemployment & poaching share	Earnings & productivity	Additive worker & firm effects	Ranked workers & surplus
<i>Workers</i>		<i>Low-rank firms</i>		
Low-rank	-0.7 (1.3)	0.4 (1.0)	0.9 (1.0)	1.1 (0.7)
Mid-rank	4.4*** (0.9)	2.9*** (0.5)	2.7*** (0.5)	2.4*** (0.7)
High-rank	2.6*** (0.9)	1.8*** (0.5)	2.0*** (0.5)	1.8*** (0.5)
<i>Workers</i>		<i>Middle-rank firms</i>		
Low-rank	-3.9*** (1.1)	-1.4 (0.8)	-1.4** (0.5)	-0.8 (0.6)
Mid-rank	1.6** (0.7)	-0.3 (0.7)	0.8 (0.6)	0.9* (0.5)
High-rank	2.9*** (0.8)	0.8 (0.6)	1.6** (0.7)	1.7*** (0.5)
<i>Workers</i>		<i>High-rank firms</i>		
Low-rank	-6.6*** (1.3)	-2.0*** (0.5)	-2.7*** (0.7)	-3.0*** (0.7)
Mid-rank	-2.2*** (0.7)	-1.7*** (0.4)	-2.7*** (0.6)	-2.8*** (0.6)
High-rank	1.9** (0.8)	-0.5 (1.0)	-1.2 (1.0)	-1.3* (0.7)

*Notes:* Estimates of change in share of employment on the seasonally-adjusted unemployment rate, season dummies, and a time trend. \*, \*\*, and \*\*\* indicate statistical significance at the 10%, 5%, and 1%, respectively. Standard errors are in parentheses.

The upward movement in the share of workers at low-rank firms, which occurs throughout the 2001 and 2007-2009 recessions, is accounted for by a decline in the share of low-rank workers at low- and middle-rank firms. In the late stages of recessions as well as their aftermath, the share of low-rank workers at high-rank firms declines by more than the middle and high-rank firms. Workers of all types, but particularly middle and high-rank workers, are more likely to work at low-rank firms during and immediately after recessions. This movement of high-rank workers

Table B.2: Change in share and unemployment (FD)

	Nonemployment & poaching share	Earnings & productivity	Additive worker & firm effects	Ranked workers & surplus
<i>Workers</i>		<i>Low-rank firms</i>		
Low-rank	-0.8 (2.3)	0.9 (1.7)	-8.3*** (3.0)	0.5 (2.4)
Mid-rank	0.5 (1.5)	3.5* (1.8)	10.4*** (2.3)	4.2*** (1.4)
High-rank	2.3* (1.3)	4.6*** (1.3)	9.8*** (1.9)	3.5** (1.3)
<i>Workers</i>		<i>Middle-rank firms</i>		
Low-rank	-2.7* (1.4)	-5.9*** (1.2)	-18.6*** (2.0)	-10.3*** (1.7)
Mid-rank	2.4* (1.4)	-0.7 (1.3)	4.7*** (1.7)	-6.1*** (1.4)
High-rank	6.8*** (1.7)	4.2*** (1.2)	10.8*** (1.8)	2.8** (1.3)
<i>Workers</i>		<i>High-rank firms</i>		
Low-rank	-9.1*** (1.6)	-6.5*** (1.7)	-18.1*** (3.1)	-4.1*** (1.3)
Mid-rank	-5.1*** (1.6)	-4.2*** (1.5)	-1.8 (1.7)	-1.1 (1.2)
High-rank	5.7** (2.3)	4.1** (1.7)	11.0*** (1.6)	10.6*** (2.2)

*Notes:* Estimates of change in share of employment on the seasonally-adjusted unemployment rate, season dummies, and a time trend. \*, \*\*, and \*\*\* indicate statistical significance at the 10%, 5%, and 1%, respectively. Standard errors are in parentheses.

into low-rank firms more than offsets the decline in employment of low-rank workers at low-rank firms at the outset of each of the two recessions, and so the employment share at low-rank firms exhibits countercyclical increases.

### B.3.3 Correlations among worker and firm ranks

We measure the correlation between worker ranks and firm ranks from each of the four methods, and present these correlations in Table B.3. A sizable literature exists on how these different methods yield different measures of labor market sorting, and so we do not expect perfect agreement.

The different methods of ranking worker and firms are positively correlated with each other, although correlations are generally less than 0.5. The revenue productivity measure has the lowest correlation with other ranking methods.

The different methods yield different correlations in the extent to which low- vs. high-rank workers are employed at low- vs. high-rank firms. The revenue productivity method produces the strongest correlation, at 0.35, while the poaching share and employment duration model produces the lowest correlation, at 0.22. The reranking and reservation wage method yields a correlation of 0.24, and our additive worker and firm effects method yields a correlation of 0.33.

The correlation between worker effects and firm effects in the additive model is larger than some early implementations of Abowd, Kramarz and Margolis (1999) estimators on linked employer-employee data, which suggested that the correlation between worker type and firm type was close to zero. Our estimates are of the same

order of magnitude but smaller than the recently proposed estimator of Bonhomme, Lamadon and Manresa (2019), and much smaller than that of Borovičková and Shimer (2017). We view our relatively large correlation as the effect of having a very large number of workers and firms, a relatively lengthy panel, and using quarterly rather than annual data. Using annual data for the U.S. in a similar time period, Lamadon, Mogstad and Setzler (2019) report a correlation of 0.10 from estimation that follows Abowd, Kramarz and Margolis (1999). These reduce the amount of “limited mobility bias” that can drive correlation estimates based on the additive model to zero, see Andrews, Gill, Schank and Upward (2012). We show in Table B.4 that implementing our additive estimator on subsets of the data yields much smaller correlations between worker type and firm type. Comparing the correlations across columns also yields information about the relative effects of different commonly used sample selection techniques on worker-firm rank agreement in an additive framework.

Table B.3: Correlation of worker and firm ranks across methods

	Firm rankings			Worker rankings		
	Poaching Share	Labor Productivity	Additive Firm Surplus	Employment	Earnings	Additive Worker Reranking
<b>Firm Rankings</b>						
Poaching Share	1.00					
Labor Productivity	0.32	1.00				
Additive	0.43	0.45	1.00			
Surplus	0.44	0.55	0.77	1.00		
<b>Worker Rankings</b>						
Employment	0.22	0.14	0.16	0.18	1.00	
Earnings	0.24	0.35	0.38	0.52	0.29	1.00
Additive	0.23	0.33	0.33	0.49	0.31	0.98
Reranking	0.13	0.18	0.16	0.24	0.24	0.79
						0.31
						1.00

*Notes:* All correlations are statistically distinct from zero at the 0.0001 significance level.

Table B.4: Correlation of additive model worker and firm ranks across implementation methods

Correlation	0.332	0.326	0.165	0.158	0.176	0.172	0.176	0.161	0.173	0.180	0.188
Control Variables											
Age quadratic and time FE	-	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Birth year * time FE	Y	-	-	-	-	-	-	-	-	-	-
Random sample											
10%	-	-	Y	Y	-	-	-	-	-	-	-
100%	Y	Y	-	-	Y	Y	Y	Y	Y	Y	Y
Full-quarters employed											
20+ Quarters	-	Y	-	-	-	-	-	-	-	-	-
Time range											
1994-2003	-	-	Y	Y	-	-	Y	-	-	-	-
2004-2014	-	-	-	-	Y	-	-	-	-	-	-
1994-2014	Y	Y	-	-	-	-	-	-	-	-	-
2004-2013	-	-	-	-	Y	-	-	Y	-	-	-
1994-2013	-	-	-	-	-	Y	-	-	Y	Y	Y
Frequency											
Quarterly	Y	Y	Y	Y	Y	-	-	-	-	-	-
Annual	-	-	-	-	-	Y	Y	Y	Y	Y	Y
Earnings threshold											
\$15,000 2014 dollars	-	-	-	-	-	-	-	-	Y	Y	-
Age restriction											
25 to 55	-	-	-	-	-	-	-	-	-	Y	-
Firm size restriction											
20+ workers	-	-	-	-	-	-	-	-	-	-	Y

Notes: All correlations are statistically distinct from zero at the 0.0001 significance level. Annual estimates end in 2013 because 2014 is only partially available.

## B.4 Reranking production function inversion estimation

To estimate the production implied from the reranking methodology of identifying worker and firm types, we employ the job surplus inversion method described in Hagedorn, Law and Manovskii (2017). The production function is a function of the surplus,  $S(\hat{x}, \hat{y})$ , generated by the matching of a worker of type  $\hat{x}$  with a firm of type  $\hat{y}$  plus the value of a vacancy to a firm of type  $\hat{y}$ ,  $V_v(\hat{y})$  and the value of unemployment to a worker of type  $\hat{x}$ ,  $V_u(\hat{x})$ . These factors are weighted by the time-discount factor ( $\beta$ ) and the job destruction rate ( $\delta$ ). Specifically, the productivity of a specific worker-firm match,  $f(\hat{x}, \hat{y})$  is:

$$f(\hat{x}, \hat{y}) = (1 - \beta(1 - \delta))S(\hat{x}, \hat{y}) + (1 - \beta)V_v(\hat{y}) + (1 - \beta)V_u(\hat{x}) \quad (\text{B.5})$$

### B.4.1 Value of unemployment by worker type

We estimate the value of unemployment,  $V_u(\hat{x})$ , by estimated worker type,  $\hat{x}$ , as the present discounted value of the minimum quarterly earnings from nonemployment accepted by workers of type  $\hat{x}$  from any firm type. For every worker-firm type combination, we calculate  $e^{10p}(\hat{x}, \hat{y})$ , the 10th percentile of residual earnings (after controlling for age). The value of unemployment to a specific worker type  $\hat{x}$  is the minimum of the  $e^{10p}$  across all potential firm-types given the worker type.

$$V_u(\hat{x}) = \frac{1}{1 - \beta} \min_{\hat{y}} e^{10p}(\hat{x}, \hat{y}) \quad (\text{B.6})$$

### B.4.2 Value of employment by worker-firm combination

We estimate the value to a worker of being employed by worker-firm type combination  $V_e(\hat{x}, \hat{y})$  as the average across all observed jobs spells of the present discounted value of earnings of workers of type  $\hat{x}$  over the job spells at firms of type  $\hat{y}$ . If  $i(\hat{x}, \hat{y})$  is an index of job spells of type  $\hat{x}$  workers at type  $\hat{y}$  firms and  $d_i$  is the duration of job spell  $i$  then  $V_e(\hat{x}, \hat{y})$  is:

$$V_e(\hat{x}, \hat{y}) = \sum_{i(\hat{x}, \hat{y})} \frac{1}{N_i} \sum_{t=0}^{d_i-1} \beta^t e_{it} + \beta^d V_u(\hat{x}) \quad (\text{B.7})$$

### B.4.3 Match surplus by worker-firm combination

We estimate the worker-firm type combination match surplus,  $S(\hat{x}, \hat{y})$ , as the scaled difference between the value of a worker being employed at a firm of a given type and the worker's value of employment, where the scaling factor is the measure of worker's bargaining power  $\alpha$ . More specifically,

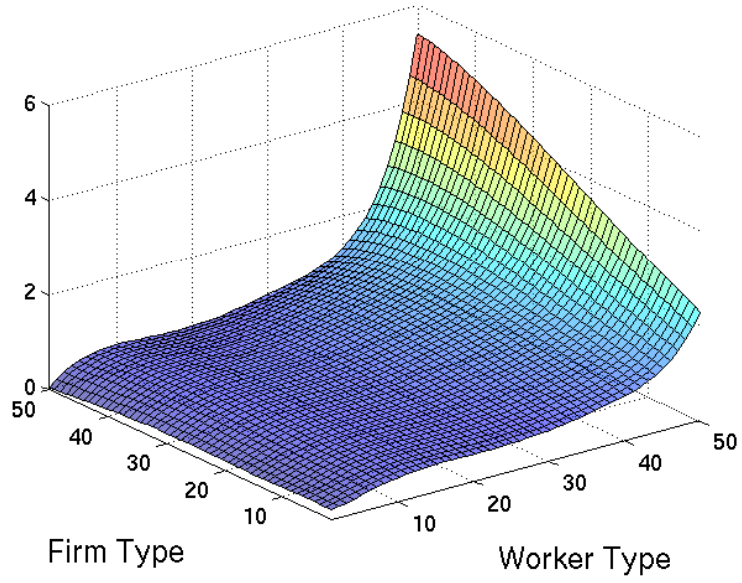
$$S(\hat{x}, \hat{y}) = \frac{V_e(\hat{x}, \hat{y}) - V_u(\hat{x})}{\alpha} \quad (\text{B.8})$$

We use  $\alpha = 0.5$ , as in the model of Shimer and Smith (2000).

### B.4.4 Vacancy value by firm type

We estimate the vacancy value to a firm of type  $\hat{y}$ ,  $V_v(\hat{y})$ . The vacancy value is a function of the discount factor  $\beta$ , the worker's bargaining power  $\alpha$ , the job

Figure B.41: Production Function from Worker Reranking & Surplus Method



*Notes:* Worker and firm type distribution normalized to uniforms.

destruction rate  $\delta$ , and the average firm surplus for firms of type  $\hat{y}$ ,  $\Omega(\hat{y})$ . The firm surplus is estimated using the surplus-based ranking method described in Appendix Section B.2.4.3.

$$V_v(\hat{y}) = \frac{\beta}{1-\beta} \frac{1-\alpha}{\alpha} (1-\delta)\Omega(\hat{y}) \quad (\text{B.9})$$

## Appendix C: Appendix to Chapter 3

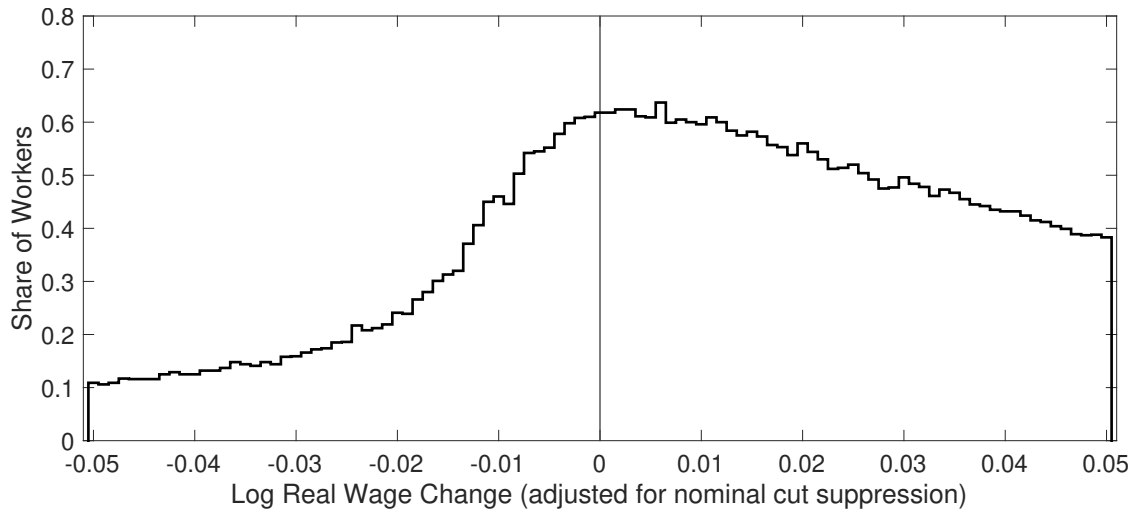
### C.1 RD tests for downward real and nominal rigidity

To more formally test for the existence of a discontinuity at zero nominal change, I use a standard regression discontinuity specification to test for a sharp break in the proportion of workers receiving nominal wage changes immediately below, versus above, zero nominal change. (I exclude nominal wage freezes from this analysis because DNWR implies a discontinuity in realized nominal wage changes at zero nominal change.) As shown in Table C.1, using a variety of polynomials in the running variable (nominal wage change) and bandwidths around zero nominal change, I always find a large and statistically significant break in the histogram of nominal wage changes at zero. This finding of a sharp break in the distribution of nominal wage changes at zero nominal change is consistent with the extensive literature on downward nominal wage rigidity.

I also evaluate whether there is any downward real rigidity after taking into account the downward nominal rigidity. This is of particular interest because the fair-wage theory of efficiency wages proposed by Akerlof and Yellen (1990) implies that workers are reluctant to accept wages that are below some reference wage. Any formulations of the “fair-wage” efficiency wage theory with the reference wage

denoted in real terms would imply that there should be a discontinuity in the real wage change distribution at zero real wage change. To determine whether there is downward real wage rigidity after taking into account nominal wage rigidity, I identify all instances where a worker's nominal wage adjusts and then use the change in the Employment Cost Index between the current period and the period that the worker last received a wage change to calculate the real wage change. The resulting histogram of real wage changes in 0.1 log point bins is shown in Figure C.1. Unlike the nominal wage change distribution, the histogram exhibits no apparent discontinuity at zero real wage change. As shown in Table C.2, estimating a similar set of regression discontinuity models with various bandwidths and polynomials in the running variable delivers ambiguous results, confirming that there is no strong evidence of a discontinuity in the real wage change distribution at zero, and thus it is unlikely that there is any downward real wage rigidity after accounting for downward nominal wage rigidity.

Figure C.1: Histogram of persistent real wage changes at quarterly frequency near zero



*Notes:* Frequency of post-Lasso estimated real persistent wage change grouped into 0.1 log point bins between -5.0% and 5.0%. The real magnitude of the change is calculated as the nominal change since the workers' last wage change. The nominal change is deflated using the BLS Employment Cost Index (ECI). U.S. Census Bureau Disclosure Review Board bypass number DRB-B0069-CED-20190725.

Table C.1: Regression discontinuity test at zero nominal wage change

Polynomial Order	Nominal Wage Change Bandwidth Window		
	[-1.5%, 1.5%]	[-2.0%, 2.0%]	[-3.0%, 3.0%]
First	<b>2.3*</b> (0.90)	<b>1.9*</b> (0.77)	<b>3.0***</b> (0.60)
Second	<b>4.9***</b> (0.79)	<b>3.9***</b> (0.88)	<b>1.9*</b> (0.93)
Third		<b>5.5***</b> (1.10)	<b>3.8**</b> (1.13)

*Note:* Outcome variable is the share of workers with a nominal wage change within each 0.1 percentile bin of the nominal wage change distribution. The percentile bins are constructed from the post-Lasso estimation using hours-paid data from the secondary sample for each quarter from 2011:III to 2017:IV. The RD specification allows for distinct polynomials in the nominal wage above and below zero nominal change. \*\*\*, \*\*, \* indicate statistical significance at the 0.1%, 1.0%, and 5.0% levels, respectively. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0069-CED-20190725.

Table C.2: Regression discontinuity test at zero real wage change

Polynomial Order	Real Wage Change Bandwidth Window		
	[ -1.5%, 1.5% ]	[ -2.0%, 2.0% ]	[ -3.0%, 3.0% ]
First	<b>-3.4*</b> (1.27)	<b>-2.7*</b> (1.01)	2.4 (1.36)
Second	-0.2 (1.56)	-2.9 (1.57)	<b>-5.9***</b> (1.4)
Third		1.0 (1.88)	-2.4 (1.57)

*Note:* Outcome variable is the share of workers with a real wage change within each 0.1 percentile bin of the real wage change distribution (conditional on a nominal wage change). The real wage change distribution includes all nominal wage changes that are then converted into the real wage change using the change in the Employment Cost Index since the worker's last wage change. The percentile bins are constructed from the post-Lasso estimation using hours-paid data from the secondary sample for each quarter from 2011:III to 2017:IV. The RD specification allows for distinct polynomials in the real wage above and below zero real change. \*\*\*, \*\*, \* indicate statistical significance at the 0.1%, 1.0%, and 5.0% levels, respectively. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0069-CED-20190725.

## C.2 Permanent versus transitory earnings changes

If the quasi-experiment were to measure workers' compensation changes using changes in log earnings, then the transitory components of quarterly earnings would present two problems. First, the frequency of earnings changes makes it difficult to identify a firm's typical raise quarter. As shown in Table 3.1, even when controlling for quarterly hours paid, workers experience a change in log earnings almost every quarter. Only 5.0% of workers have no quarter-over-quarter change in log hourly earnings, whereas 55.5% receive a nominal raise and 39.5% receive a nominal cut.<sup>1</sup> In contrast, Grigsby, Hurst and Yildirmaz (2019) use ADP payroll data to examine workers' base wages and they find that 80.6% of workers have no quarter-over-quarter change in their base nominal wage, 18.5% receive a nominal base wage raise, and only 0.9% receive a nominal cut. They further show both that the bonus component of hourly earnings drives much of this difference between hourly earnings and base wage changes and that bonuses exhibit little persistence. That workers' quarterly earnings change so often makes it difficult to use quarterly earnings changes to identify a dominant calendar quarter in which a firm tends to raise its workers' compensation.

---

<sup>1</sup>The relatively small number of quarter-over-quarter freezes in log hourly earnings is consistent with the findings of Kurmann and McEntarfer (2019) and Jardim, Solon and Vigdor (2019) regarding the frequency of four-quarter changes in log hourly earnings. Both studies use quarterly earnings and hours-paid data from Washington state to show hourly earnings exhibit far less year-over-year rigidity relative to the frequency of nominal wage freezes in the survey literature. Grigsby, Hurst and Yildirmaz (2019) uses ADP payroll data to confirm this large difference in the relative frequency of nominal changes in base wages versus hourly earnings. I find that even when controlling for measurement error due to rounding in hours paid and annual bonuses, the share of workers with no quarter-over-quarter change in log hourly earnings remains low at 22.2% (adjusting for rounding in hours paid) and 45.3% (adjusting for annual bonuses and rounding in hours paid).

Second, most fluctuations in quarterly earnings are unlikely to persist into future periods. As a result, historical patterns of workers' quarterly earnings changes are less predictive of firms' start-of-quarter worker compensation costs. To demonstrate the lack of persistence in workers' quarterly earnings changes, I evaluate the relative importance of permanent versus transitory earnings changes using the autocorrelation of workers' four-quarter change in log hourly earnings ( $\Delta_{ik,t,t-4}^{yH}$ ).<sup>2</sup> The four-quarter log hourly earnings change can be decomposed into the sum of the persistent quarterly changes in hourly earnings in  $t-3$ ,  $t-2$ ,  $t-1$ , and  $t$  ( $\Delta_{ik,t-a}^P$ ), plus the transitory change in hourly earnings in  $t$  ( $\Delta_{ikt}^T$ ).

$$\Delta_{ik,t,t-4}^{yH} = \ln\left(\frac{y_{ikt}}{h_{ikt}}\right) - \ln\left(\frac{y_{ikt-4}}{h_{ikt-4}}\right) = \Delta_{ikt}^T + \Delta_{ikt}^P + \Delta_{ikt-1}^P + \Delta_{ikt-2}^P + \Delta_{ikt-3}^P \quad (\text{C.1})$$

Notice that the four-quarter change and its one-period lag ( $\Delta_{ik,t-1,t-5}^{yH}$ ) share three of these persistent components - namely  $\Delta_{ikt-1}^P$ ,  $\Delta_{ikt-2}^P$ , and  $\Delta_{ikt-3}^P$ . Assuming that the persistent change components are distributed iid, then the autocorrelation of  $\Delta_{ik,t,t-4}^{yH}$  is 0.75 if there are no transitory changes. However, I find that the autocorrelation of the four-quarter change in log hourly earnings is only 0.284. Under a set of strong assumptions,<sup>3</sup> I can estimate the relative magnitude of the permanent and transitory changes in hourly earnings as follows:

$$\text{Corr}\left(\Delta_{ik,t,t-4}^{yH}, \Delta_{ik,t-1,t-5}^{yH}\right) = \frac{\mathbb{E}\left[\Delta_{ik,t,t-4}^{yH}\Delta_{ik,t-1,t-5}^{yH}\right]}{\text{Var}\left(\Delta_{ik,t,t-4}^{yH}\right)\text{Var}\left(\Delta_{ik,t-1,t-5}^{yH}\right)} \quad (\text{C.2})$$

---

<sup>2</sup>This uses data for workers in the four states with hours-paid data.

<sup>3</sup>Namely the three assumptions are i) that persistent change components are distributed iid, ii) that temporary change components are distributed iid, and iii) that the temporary and permanent change components are independent within any 5-quarter window.

Assuming that: i) the persistent change components for each quarter follow the same iid distribution, ii) similarly, the transitory change components for each quarter follow an iid distribution, and iii) the persistent and transitory components are independent then

$$Corr\left(\Delta_{ik,t,t-4}^{y^H}, \Delta_{ik,t-1,t-5}^{y^H}\right) = \frac{3Var\left(\Delta_{ik}^P\right)}{4Var\left(\Delta_{ik}^P\right) + Var\left(\Delta_{ik}^T\right)} \quad (C.3)$$

This implies that the relative variation in quarterly earnings from the transitory versus the persistent changes is:

$$\frac{Var\left(\Delta_{ik}^T\right)}{Var\left(\Delta_{ik}^P\right)} = \frac{3 - 4Corr\left(\Delta_{ik,t,t-4}^{y^H}, \Delta_{ik,t-1,t-5}^{y^H}\right)}{Corr\left(\Delta_{ik,t,t-4}^{y^H}, \Delta_{ik,t-1,t-5}^{y^H}\right)} \quad (C.4)$$

Thus, the autocorrelation estimate implies that the variance of the transitory component of the quarterly measure of hourly earnings is 6.6 times greater than the variance of the permanent component. This indicates that transitory changes account for 86% of the quarter-over-quarter fluctuations in hourly earnings.

It is reassuring to note that the post-Lasso procedure identifies persistent wage changes as occurring in periods in which this autocorrelation measure indicates that log earnings changes are more persistent. Using the same metric of the autocorrelation of the four-quarter change in log hourly earnings, I find that the autocorrelation is 0.512 if the post-Lasso procedure detects a wage change in  $t - 1$ , whereas it is only 0.123 if no wage change is detected by the post-Lasso procedure in  $t - 1$ . The estimated wage changes from the post-Lasso procedure are much more likely to persist,

and thus affect firms' start-of-quarter wage bills in future periods.

### C.3 Measures of wage compensation

For every worker  $i$ , firm  $k$ , and quarter  $t$  combination, the LEHD data set provides either: i) quarterly earnings (primary sample), or ii) both quarterly earnings ( $y_{ikt}^Q$ ) and quarterly hours paid ( $h_{ikt}^Q$ ) (secondary sample). For simplicity, I begin by describing various measures of wage compensation that can be constructed when quarterly hours paid is observed.

The literature on nominal wage rigidity has tended to focus on rigidity in workers' base wage ( $w_{ikt}$ ). However, with the LEHD data, I observe workers' quarterly earnings ( $y_{ikt}^Q$ ) or their quarterly-averaged hourly earnings ( $\bar{y}_{ikt}^H$ , hereafter *hourly earnings*). Two recent working papers, Kurmann and McEntarfer (2019) and Jardim, Solon and Vigdor (2019), use administrative UI records data to explore the degree of nominal rigidity in hourly earnings. Both studies find hourly earnings are much less rigid than base wages.<sup>4</sup> Thus, it will be useful to deconstruct the relationship between these two measures of wage compensation. First, note that the quarterly earnings measure can be decomposed as:

$$y_{ikt}^Q = w_{ikt} \left( n_{ikt} \bar{h}_{ikt}^W + \frac{1}{2} n_{ikt} \bar{h}_{ikt}^o \right) v_{ikt} \epsilon_{ikt} \quad (\text{C.5})$$

where  $n_{ikt}$  is the number of payroll weeks in quarter  $t$ ,  $\bar{h}_{ikt}^W$  is the average number

---

<sup>4</sup>This finding of greater nominal wage flexibility in worker earnings (relative to findings from surveys and administrative payroll records) is echoed in the survey of Elsby and Solon (2019). They consolidate findings of many recent international studies of nominal wage rigidity and argue that about 15-25% of workers receive year-over-year nominal wage cuts.

of hours worked per week,  $\bar{h}_{ikt}^o$  is the average number of overtime hours worked per week,  $v_{ikt}$  is any non-overtime variable compensation paid in period  $t$ , and  $\epsilon_{ikt}$  captures measurement error (e.g. the rounding of hours worked to integer digits or order-of-magnitude errors in hours worked). As I show in Appendix 3.3, including the number of payroll weeks in the quarter proves to be quite useful since, depending on the payroll schedule in effect at the firm, the number of payday weeks can fluctuate among 12, 13, or 14 weeks from one quarter to the next. These fluctuations in payday weeks from the payday schedules result in substantial variation in quarterly earnings ( $\pm 7 - 15\%$ ).

Since overtime pay only applies to hours worked in excess of 40 hours per week, I will approximate total quarterly overtime hours as:

$$n_{ikt}\bar{h}_{ikt}^o = \max [0, n_{ikt} (\bar{h}_{ikt}^W - 40)] \quad (\text{C.6})$$

This approximation and the decomposition of quarterly earnings in Equation C.5 imply that a worker's base wage is related to her hourly earnings as follows:

$$y_{ikt}^H = y_{ikt}^Q / h_{ikt}^Q \approx w_{ikt} \left( 1 + \frac{1}{2} \frac{\max [0, h_{ikt}^Q - 40n_{ikt}]}{h_{ikt}^Q} \right) \frac{v_{ikt}\epsilon_{ikt}}{h_{ikt}^Q} \quad (\text{C.7})$$

It is evident from this decomposition that difference in the degree of rigidity between measures of workers' hourly earnings and their base wages could come from three potential sources: overtime compensation ( $\max [0, h_{ikt}^Q - 40n_{ikt}]$ ), non-overtime variable compensation ( $v_{ikt}$ ), or measurement error ( $\epsilon_{ikt}$ ). As to the rele-

vance of these three sources of variation for measuring true rigidity in wage compensation, I discount the importance of fluctuations in hourly earnings caused by measurement error and overtime compensation. Fluctuations in hourly earnings caused by measurement error are simply spurious. Fluctuations in hourly earnings due to changes in overtime compensation are unrelated to the persistence of the worker's base wage, but, instead, reflect a temporary change in the worker's utilization. Thus, it will be useful to explore the relative importance of each of these three sources in the degree of measured rigidity in hourly earnings.

Table C.3 shows the proportion of quarter-over-quarter ( $\ln(\bar{y}_{ikt}^H) - \ln(\bar{y}_{ikt-1}^H)$ ) and four-quarter ( $\ln(\bar{y}_{ikt}^H) - \ln(\bar{y}_{ikt-4}^H)$ ) raises / freezes/ cuts in log hourly earnings for individuals who are employed for the full-quarter in both periods (i.e. they were employed at both the start and the end of the quarter at the same firm). The log hourly earnings exhibit significant variability, with only 5.0% of workers having the same hourly earnings from one quarter to the next.

### C.3.1 Measurement error: Rounding in hours paid

Although I cannot identify all instances of measurement error in hourly earnings changes, I can control for changes in hourly earnings that may be due to state unemployment insurance agencies' instructions that firms report hours paid after rounding them to whole numbers. This instruction has significant implications for the frequency of wage freezes. To measure the impact of this rounding rule, I set to zero change all observed changes in log hourly earnings that could be due to re-

ported hours paid being misreported by  $\pm 1$  in the current and/or the lagged quarter. This provides an upper-bound on the degree to which flexibility in hourly earnings could be due to measurement error from rounding hours paid to whole numbers. I find that resetting to zero any hourly earnings change that could be due to hours-rounding causes the frequency of quarter-over-quarter wage freezes to increase more than four-fold, from 5.0% to 22.2%.

### C.3.2 Overtime compensation

Although the hours paid reported in the LEHD data set makes no distinction between overtime hours and regular pay hours, I use the following method to identify earnings changes that could be due to overtime pay. First, if a worker is reported as having worked 480 or fewer hours in the quarter ( $h_{ikt}^Q \leq 480$ ), then I do not consider whether they worked any overtime because even with the fewest number of payday weeks (12), they could still have attained this number of hours without working any overtime.<sup>5</sup> Focusing, instead, on quarters in which a worker had 481 or more hours paid ( $h_{ikt}^Q > 480$ ), I consider three alternative numbers of payday weeks ( $n_{ikt} \in (12, 13, 14)$ ). Since overtime hours can be approximated as  $\max[0, h_{ikt}^Q - 40n_{ikt}]$ , each of these three scenarios corresponds to a different number of overtime hours worked. I conclude that overtime hours have been worked if a particular overtime adjustment results in the hourly pay in period  $t$  being within three cents of the hourly earnings in at least three of the surrounding four quarters (allowing for adjustments

---

<sup>5</sup>This assumption misses some overtime hours since overtime hours are calculated on a weekly basis. Thus, it is possible for a worker to work more than 40 hours in one week (thus receiving overtime pay) and fewer in another week of the same quarter, and yet still have fewer than 481 hours of pay in the quarter.

to the hourly earnings in the surrounding quarters for overtime pay).

### C.3.3 Variable compensation: Annual bonuses

A worker's non-overtime variable compensation,  $v_{ikt}$ , can include tips, commissions, and bonuses. I describe here a method for estimating one form of variable compensation: annual bonuses. An annual bonus that occurs in a particular quarter will appear as a single-quarter spike in hourly earnings. Thus, I identify quarters in which a worker received an annual bonus as any quarter in which  $y_{ikt}^H > \max [y_{ikt-1}^H, y_{ikt+1}^H]$ . For any such period, I construct an estimate of the bonus as:

$$\hat{b}_{ikt} = \frac{y_{ikt}^H}{\max [y_{ikt-1}^H, y_{ikt+1}^H]} h_{ikt}^Q = \frac{w_{ikt} \left(1 + \frac{1}{2} \frac{\bar{h}_{ikt}^o}{40}\right) \frac{\epsilon_{ikt}}{h_{ikt}^Q}}{\max_{s=t-1, t+1} w_{iks} \left(1 + \frac{1}{2} \frac{\bar{h}_{iks}^o}{40}\right) \frac{\epsilon_{iks}}{h_{iks}^Q}} b_{ikt} \quad (\text{C.8})$$

The estimated  $\hat{b}_{ikt}$  is an accurate measure of the true annual bonus,  $b_{ikt}$ , in cases where there is no measurement error or overtime hours worked in either the annual bonus period or the comparison period and the base wage is constant across periods. When I exclude the estimated annual bonuses from the measure of hourly earnings and set changes to zero when they reflect potential hours rounding errors, the frequency of quarter-over-quarter freezes in log hourly earnings increases to 45.3% from 5% in the raw hourly earnings measure (and from 22.2% in the hourly earnings that exclude potential rounding errors).

Table C.3: Hourly earnings change comparison

Quarter-over-Quarter Nominal Log Hourly Earnings Change						
	Source	Period	Raise	Freeze	Cut	
Raw	LEHD 4 States	2011-2018	55.5%	5.0%	39.5%	
Rounding Adj	LEHD 4 States	2011-2018	46.4%	22.2%	31.4%	
Bonus & Rounding Adj	LEHD 4 States	2011-2018	36.9%	45.3%	17.8%	
4-Quarter Log Nominal Hourly Earnings Change						
	Source	Period	Raise	Freeze	Cut	
Rounding Adj	LEHD 4 States	2011-2018	68.1%	11.8%	20.1%	
Kurmann and McEntarfer (2019)	LEHD WA State	1990-2014		8-16%	20-25%	
Jardim, Solon and Vigdor (2019)	UI WA State	2005-2015		2.5-7.7%	20.4-33.1%	
Grigsby, Hurst and Yildirmaz (2019)	ADP 50+ Workers	2008-2016	75.3%	9.0%	15.7%	

*Note:* ‘Raw’ indicates log change calculated from the original quarterly earnings and hours paid. ‘Rounding Adj’ sets to zero any nominal change that could be explained by adjusting the hours paid by  $\pm 1$  hour to account for potential rounding errors. ‘Bonus & Rounding Adj’ first smooths bonuses in the hourly earnings and then sets to zero any nominal change that could be accounted for by a  $\pm 1$  change in hours paid. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0037-CED-20190327.

## Bibliography

- Abbritti, Mirko and Stephan Fahr**, “Downward wage rigidity and business cycle asymmetries,” *Journal of Monetary Economics*, 2013, 60 (7), 871–886.
- Abowd, John M, Bryce E Stephens, Lars Vilhuber, Fredrik Andersson, Kevin L McKinney, Marc Roemer, and Simon Woodcock**, “The LEHD infrastructure files and the creation of the Quarterly Workforce Indicators,” in “Producer dynamics: New evidence from micro data,” University of Chicago Press, 2009, pp. 149–230.
- , **Francis Kramarz, and David N Margolis**, “High wage workers and high wage firms,” *Econometrica*, 1999, 67 (2), 251–333.
- Akerlof, George A and Janet L Yellen**, “The fair wage-effort hypothesis and unemployment,” *The Quarterly Journal of Economics*, 1990, 105 (2), 255–283.
- , **William T Dickens, George L Perry, Robert J Gordon, and N Gregory Mankiw**, “The macroeconomics of low inflation,” *Brookings Papers on Economic Activity*, 1996, 1996 (1), 1–76.
- Altonji, Joseph G and Paul J Devereux**, “The extent and consequences of downward nominal wage rigidity,” in “Research in labor economics,” Emerald Group Publishing Limited, 2000, pp. 383–431.
- Andrews, Martyn J, Leonard Gill, Thorsten Schank, and Richard Upward**, “High wage workers match with high wage firms: Clear evidence of the effects of limited mobility bias,” *Economics Letters*, 2012, 117 (3), 824–827.
- Bagger, Jesper and Rasmus Lentz**, “An empirical model of wage dispersion with sorting,” *The Review of Economic Studies*, 2019, 86 (1), 153–190.
- Bai, Jushan and Pierre Perron**, “Estimating and testing linear models with multiple structural changes,” *Econometrica*, 1998, pp. 47–78.
- Barattieri, Alessandro, Susanto Basu, and Peter Gottschalk**, “Some evidence on the importance of sticky wages,” *American Economic Journal: Macroeconomics*, 2014, 6 (1), 70–101.
- Barlevy, Gadi**, “The sullyng effect of recessions,” *The Review of Economic Studies*, 2002, 69 (1), 65–96.

- Barro, Robert J**, “Long-term contracting, sticky prices, and monetary policy,” *Journal of Monetary Economics*, 1977, 3 (3), 305–316.
- Bartolucci, Cristian, Francesco Devicienti, and Ignacio Monzón**, “Identifying sorting in practice,” *American Economic Journal: Applied Economics*, 2018, 10 (4), 408–38.
- Benigno, Pierpaolo and Luca Antonio Ricci**, “The inflation-output trade-off with downward wage rigidities,” *American Economic Review*, 2011, 101 (4), 1436–66.
- Bernanke, Ben S and Kevin Carey**, “Nominal wage stickiness and aggregate supply in the Great Depression,” *The Quarterly Journal of Economics*, 1996, 111 (3), 853–883.
- Björklund, Maria, Mikael Carlsson, and Oskar Nordström Skans**, “Fixed-wage contracts and monetary non-neutrality,” *American Economic Journal: Macroeconomics*, 2019, 11 (2), 171–92.
- Bonhomme, Stéphane, Thibaut Lamadon, and Elena Manresa**, “A distributional framework for matched employer employee data,” *Econometrica*, 2019, 87 (3), 699–739.
- Borovičková, Katarína and Robert Shimer**, “High wage workers work for high wage firms,” Technical Report, National Bureau of Economic Research 2017.
- Burgess, Matt**, “How frequently do private businesses pay workers?,” Technical Report 11 2014.
- Caballero, Ricardo and Mohamad L Hammour**, “The Cleansing Effect of Recessions,” *American Economic Review*, 1994, 84 (5), 1350–68.
- Calvo, Guillermo A**, “Staggered prices in a utility-maximizing framework,” *Journal of Monetary Economics*, 1983, 12 (3), 383–398.
- Card, David**, “Unexpected inflation, real wages, and employment determination in union contracts,” *The American Economic Review*, 1990, pp. 669–688.
- , **Ana Rute Cardoso, and Patrick Kline**, “Bargaining, sorting, and the gender wage gap: Quantifying the impact of firms on the relative pay of women,” *The Quarterly Journal of Economics*, 2016, 131 (2), 633–686.
- **and Dean Hyslop**, “Does inflation” grease the wheels of the labor market”?,” in “Reducing inflation: Motivation and strategy,” University of Chicago Press, 1997, pp. 71–122.
- Carlsson, Mikael and Andreas Westermarck**, “Endogenous separations, wage rigidities and employment volatility,” 2016. Working Paper.

- Casini, Alessandro and Pierre Perron**, “Structural breaks in time series,” *Oxford Research Encyclopedia of Economics and Finance*, forthcoming.
- Chodorow-Reich, Gabriel and Johannes Wieland**, “Secular labor reallocation and business cycles,” *Journal of Political Economy*, forthcoming.
- Ciuperca, Gabriela**, “Model selection by LASSO methods in a change-point model,” *Statistical Papers*, 2014, 55 (2), 349–374.
- Coibion, Olivier, Yuriy Gorodnichenko, and Johannes Wieland**, “The optimal inflation rate in New Keynesian models: Should central banks raise their inflation targets in light of the zero lower bound?,” *Review of Economic Studies*, 2012, 79 (4), 1371–1406.
- Daly, Mary C and Bart Hobijn**, “Downward nominal wage rigidities bend the Phillips curve,” *Journal of Money, Credit and Banking*, 2014, 46 (S2), 51–93.
- Davis, Steven J, John C Haltiwanger, Scott Schuh et al.**, “Job creation and destruction,” *MIT Press Books*, 1998, 1.
- de Melo, Rafael Lopes**, “Firm wage differentials and labor market sorting: Reconciling theory and evidence,” *Journal of Political Economy*, 2018, 126 (1), 313–346.
- de Ridder, Maarten and Damjan Pfajfar**, “Policy shocks and wage rigidities: Empirical evidence from regional effects of national shocks,” 2017. Working Paper.
- Dickens, William T, Lorenz Goette, Erica L Groshen, Steinar Holden, Julian Messina, Mark E Schweitzer, Jarkko Turunen, and Melanie E Ward**, “How wages change: micro evidence from the International Wage Flexibility Project,” *Journal of Economic Perspectives*, 2007, 21 (2), 195–214.
- Dixon, Huw and Hervé Le Bihan**, “Generalised Taylor and generalised Calvo price and wage setting: micro-evidence with macro implications,” *The Economic Journal*, 2012, 122 (560), 532–554.
- Dupraz, Stephane, Emi Nakamura, and Jon Steinsson**, “A plucking model of business cycles,” 2019. Working Paper.
- Eggertsson, Gauti B, Neil R Mehrotra, and Jacob A Robbins**, “A model of secular stagnation: Theory and quantitative evaluation,” *American Economic Journal: Macroeconomics*, 2019, 11 (1), 1–48.
- Ehrlich, Gabriel and Joshua Montes**, “Wage rigidity and employment outcomes: Evidence from administrative data,” 2019. Working paper.
- Elsby, Michael W and Gary Solon**, “How prevalent is downward rigidity in nominal wages? International evidence from payroll records and pay slips,” *Journal of Economic Perspectives*, 2019, 33 (3), 185–201.

- Elsby, Michael WL**, “Evaluating the economic significance of downward nominal wage rigidity,” *Journal of Monetary Economics*, 2009, 56 (2), 154–169.
- , **Donggyun Shin**, and **Gary Solon**, “Wage adjustment in the Great Recession and other downturns: Evidence from the United States and Great Britain,” *Journal of Labor Economics*, 2016, 34 (S1), S249–S291.
- Evans, Christopher**, “Optimal monetary policy in the New Keynesian model with downward nominal wage rigidity,” 2018. Working paper.
- Fagan, Gabriel and Julián Messina**, “Downward wage rigidity and optimal steady-state inflation,” 2009. ECB Working paper.
- Fallick, Bruce, Michael Lettau, and William Wascher**, “Downward nominal wage rigidity in the United States during and after the Great Recession,” 2016. Working paper.
- Federal Reserve Bank of Philadelphia**, “Survey of Professional Forecasters,” Technical Report 2008:Q4.
- Fehr, Ernst and Lorenz Goette**, “Robustness and real consequences of nominal wage rigidity,” *Journal of Monetary Economics*, 2005, 52 (4), 779–804.
- Gottschalk, Peter**, “Downward nominal-wage flexibility: real or measurement error?,” *Review of Economics and Statistics*, 2005, 87 (3), 556–568.
- Grigsby, John, Erik Hurst, and Ahu Yildirmaz**, “Aggregate nominal wage adjustments: New evidence from administrative payroll data,” Technical Report, NBER Working Paper March 2019.
- Guimaraes, Paulo and Pedro Portugal**, “A simple feasible procedure to fit models with high-dimensional fixed effects,” *The Stata Journal*, 2010, 10 (4), 628–649.
- Hagedorn, Marcus, Tzuo Hann Law, and Iourii Manovskii**, “Identifying equilibrium models of labor market sorting,” *Econometrica*, 2017, 85 (1), 29–65.
- Haltiwanger, John C, Henry R Hyatt, Lisa B Kahn, and Erika McEntarfer**, “Cyclical job ladders by firm size and firm wage,” *American Economic Journal: Macroeconomics*, 2018, 10 (2), 52–85.
- Haltiwanger, John, Henry Hyatt, and Erika McEntarfer**, “Who moves up the job ladder?,” *Journal of Labor Economics*, 2018, 36 (S1), S301–S336.
- , **Ron Jarmin, Robert Kulick, and Javier Miranda**, “High growth young firms: Contribution to job growth, revenue growth and productivity,” *Measuring Entrepreneurial Businesses: Current Knowledge and Challenges*, NBER, 2017.
- , – , – , and **Veronika Penciakova**, “Augmenting the LBD with firm-level revenue,” Technical Report 2, U.S. Census Bureau 2019.

- Harchaoui, Zaid and Céline Lévy-Leduc**, “Multiple change-point estimation with a total variation penalty,” *Journal of the American Statistical Association*, 2010, *105* (492), 1480–1493.
- Hazell, Jonathon and Bledi Taska**, “Posted wage rigidity,” 2018. Working Paper.
- Hyatt, Henry R, Erika McEntarfer, Kevin L McKinney, Stephen Tibbets, and Doug Walton**, “Job-to-job (J2J) flows: New labor market statistics from linked employer-employee data,” *US Census Bureau Center for Economic Studies Paper No. CES-WP-14-34*, 2014.
- Jardim, Ekaterina S., Gary Solon, and Jacob L. Vigdor**, “How prevalent is downward rigidity in nominal wages? Evidence from payroll records in washington state,” Technical Report, National Bureau of Economic Research 2019.
- Jo, Yoon J**, “Downward nominal wage rigidity in the United States,” Technical Report 2019.
- Kahn, Lisa B**, “The long-term labor market consequences of graduating from college in a bad economy,” *Labour Economics*, 2010, *17* (2), 303–316.
- Kahn, Shulamit**, “Evidence of nominal wage stickiness from microdata,” *The American Economic Review*, 1997, *87* (5), 993–1008.
- Kaur, Supreet**, “Nominal wage rigidity in village labor markets,” *American Economic Review*, 2019, *109* (10), 3585–3616.
- Kim, Jinill and Francisco J Ruge-Murcia**, “How much inflation is necessary to grease the wheels?,” *Journal of Monetary Economics*, 2009, *56* (3), 365–377.
- Kurmann, André and Erika McEntarfer**, “Downward wage rigidity in the United States: New evidence from administrative data,” Technical Report, CES Working paper 2019.
- Lamadon, Thibaut, Magne Mogstad, and Bradley Setzler**, “Imperfect competition, compensating differentials and rent sharing in the US labor market,” Technical Report, National Bureau of Economic Research 2019.
- Lebow, David, David Stockton, and William Wascher**, “Inflation, nominal wage rigidity, and the efficiency of labor markets,” Technical Report, Board of Governors of the Federal Reserve System-Finance and Economics Discussion Series no. 95-45 1995.
- Lebow, David E, Raven E Saks, and Beth Anne Wilson**, “Downward nominal wage rigidity: Evidence from the employment cost index,” *Advances in Macroeconomics*, 2003, *3* (1).

- Lindenlaub, Ilse and Fabien Postel-Vinay**, “Multidimensional sorting under random search,” 2016. Working Paper.
- Lise, Jeremy and Jean-Marc Robin**, “The macrodynamics of sorting between workers and firms,” *American Economic Review*, 2017, *107* (4), 1104–35.
- McLaughlin, Kenneth J**, “Rigid wages?,” *Journal of Monetary Economics*, 1994, *34* (3), 383–414.
- Mineyama, Tomohide**, “Downward nominal wage rigidity and inflation dynamics during and after the Great Recession,” 2018. Working Paper.
- Moscarini, Giuseppe and Fabien Postel-Vinay**, “The cyclical job ladder,” *Annual Review of Economics*, 2018.
- Oi, Walter Y**, “Labor as a quasi-fixed factor,” *Journal of political economy*, 1962, *70* (6), 538–555.
- Olivei, Giovanni and Silvana Tenreyro**, “The timing of monetary policy shocks,” *American Economic Review*, 2007, *97* (3), 636–663.
- Oreopoulos, Philip, Till Von Wachter, and Andrew Heisz**, “The short- and long-term career effects of graduating in a recession,” *American Economic Journal: Applied Economics*, 2012, *4* (1), 1–29.
- Ours, Jan C Van, Geert Ridder et al.**, “Job matching and job competition: Are lower educated workers at the back of job queues?,” *European Economic Review*, 1995, *39* (9), 1717–1731.
- Pischke, Jörn-Steffen**, “Wage flexibility and employment fluctuations: evidence from the housing sector,” *Economica*, 2018, *85* (339), 407–427.
- Rotemberg, Julio J**, “Sticky prices in the United States,” *Journal of Political Economy*, 1982, *90* (6), 1187–1211.
- Schmitt-Grohé, Stephanie and Martín Uribe**, “Downward nominal wage rigidity and the case for temporary inflation in the eurozone,” *Journal of Economic Perspectives*, 2013, *27* (3), 193–212.
- **and** – , “Downward nominal wage rigidity, currency pegs, and involuntary unemployment,” *Journal of Political Economy*, 2016, *124* (5), 1466–1514.
- **and Martín Uribe**, “Liquidity traps and jobless recoveries,” *American Economic Journal: Macroeconomics*, 2017, *9* (1), 165–204.
- Shen, Wenyi and Shu-Chun S Yang**, “Downward nominal wage rigidity and state-dependent government spending multipliers,” *Journal of Monetary Economics*, 2018, *98*, 11–26.

**Shimer, Robert and Lones Smith**, “Assortative matching and search,” *Econometrica*, 2000, 68 (2), 343–369.

**Taylor, John B**, “Aggregate dynamics and staggered contracts,” *Journal of Political Economy*, 1980, 88 (1), 1–23.

**Tibshirani, Robert**, “Regression shrinkage and selection via the lasso,” *Journal of the Royal Statistical Society: Series B (Methodological)*, 1996, 58 (1), 267–288.