ABSTRACT

Title of Dissertation: ESSAYS ON DISPLACED WORKERS AND RESIDENTIAL MIGRATION

Ken Masahiro Ueda
Doctor of Philosophy, 2016

Dissertation directed by: Professor Judith Hellerstein
Department of Economics

In this dissertation, I explore how workers' human capital, local industry composition, and business cycles affect employment outcomes and residential migration for job losers and other workers.

I first examine whether the poor employment outcomes of job losers are due to a lack of jobs that require their human capital within their local labor market. I answer this question by analyzing the extent to which the industry composition in the job loser's local labor market affects employment outcomes when job loss occurs during expansions and during recessions. I find that if job losers reside in an area with a high employment concentration of their original industry of employment, they are 2.1-2.8 percent more likely to be re-employed at another job if job loss occurs during an expansion; I find an insignificant relationship in most specifications when job loss occurs during a recession, and in some specifications I even find a negative relationship between industry concentration and employment. I conclude that the

industry composition within an area matters for job losers, since firms are more willing to hire workers from within their own industry, as these workers have more relevant accumulated human capital. However, firms are less likely to hire during a recession, making job losers' human capital less important for job finding.

Next, Erika McEntarfer, Henry Hyatt, and I examine whether the business cycle affects earnings changes for job losers, and the factors that explain these differences across time. We find that job losers who lost their job during the Great Recession have earnings changes that are 10 percent more negative relative to other job losers from other periods. This result is driven primarily by longer non-employment lengths and worse subsequent job matches.

Finally, Erika McEntarfer, Henry Hyatt, Alexandria Zhang, and I explore the extent to which residential migration is driven by job opportunities. We use four databases and find that changes in job moves explain some of the changes in residential migration, but the relationship is not as strong as previously documented. We find that migration patterns differ across databases, with some databases documenting steeper declines and more cyclicality.

ESSAYS ON DISPLACED WORKERS AND RESIDENTIAL MIGRATION

by

Ken Masahiro Ueda

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
2016

Advisory Committee:
Professor Judith Hellerstein, Chair
Professor John Ham
Professor Lesley Turner
Professor Ethan Kaplan
Professor Waverly Ding

Acknowledgements

I would like to thank Judy Hellerstein and Lesley Turner for guiding me in my research and writing over the past few years. I am indebted to John Ham for his mentorship, which will continue even beyond graduate school.

I thank Ethan Kaplan and Waverly Ding for serving on my dissertation committee. I have learned a great deal from Stephanie Rennane, Marisol Rodriguez Chatruc, Robert Kulick, Brian Quistoff, Giordano Palloni, and the rest of the students who entered during the Fall of 2010 and 2011.

Finally, I would like to thank Henry Hyatt, Erika Mcentarfer, and Alexandria Zhang for their feedback and professionalism. Any opinions and conclusions expressed here are those of the authors and do not necessarily represent the views of the U.S. Census Bureau. All results have been reviewed to ensure that no confidential data are disclosed.

Table of Contents

Acknowledgements ii
List of Tables v
List of Figures vii
Chapter 1: Introduction 1
Chapter 2: How are Employment Outcomes Affected by Local Labor Markets After Job Loss? 7
  2.1 Introduction 7
  2.2 Conceptual Framework 11
  2.3 Data 15
    2.3.1 Databases 15
    2.3.2 Sample Creation 16
    2.3.3 Concentrated Industry Definition, Outcomes of Interest 20
  2.4 Methodology 23
  2.5 Results 25
    2.5.1 Sample Characteristics 25
    2.5.2 Baseline Estimates 29
    2.5.3 Labor Supply Investigation 37
    2.5.3.1 Labor Supply Decisions Made After Job Loss 37
    2.5.3.2 Labor Supply Decisions Made Before Job Loss, Firm Selection 38
  2.6 Conclusion 46
Chapter 3: The Consequences of Job Loss in the Great Recession 88
  3.1 Introduction 88
  3.2 Data 91
  3.3 Methodology 93
    3.3.1 Empirical Strategy 94
  3.4 Results 99
    3.4.1 Worker and Firm Characteristics 99
    3.4.2 Baseline Regressions 101
    3.4.3 Non-employment and Job Quality 104
    3.4.4 Does Unemployment Insurance Mitigate Earnings Losses 108
  3.5 Conclusion 109
Chapter 4: Interstate Migration and Job-to-Job Flows 128
  4.1 Introduction 128
  4.2 Data 133
    4.2.1 Current Population Survey (CPS) 134
    4.2.2. American Community Survey (ACS) 136
    4.2.3 Internal Revenue Service (IRS) 137
    4.2.4 Longitudinal Employer-Household Dynamics (LEHD) 137
  4.3 Interstate Migration in the U.S. 1981-2014 141
    4.3.1 Comparison of Interstate Migration Rates in LEHD, CPS, ACS, and IRS Data 141
    4.3.2 Evidence from Matched CPS and LEHD Data 143
  4.4 Interstate Migration and Job-to-Job Flows 146
    4.4.1 Trends in the Job-to-Job Flows 146

4.4.2 Economic Migration 148
4.5 The Decline in Interstate Migration and Economic Migration: 2000-2010 ... 150
4.5.1 The Role of Composition in Declining Interstate Migration 150
4.5.2 Changes in the Return to Interstate Migration 153
4.6 Conclusion 154
Bibliography 177

List of Tables

Chapter 2 Tables

Table 2.1: Demographic and Work History Characteristics of Displaced Workers 56
Table 2.2: Firm and CBSA Characteristics of Displaced Workers 58
Table 2.3: Earnings Change Regression Estimates for Displaced Workers: 60
Effects of Concentrated Industries Within The Business Cycle 60
Table 2.4: Employment Regression Estimates for Displaced Workers: 62
Effects of Concentrated Industries Within The Business Cycle 62
Table 2.5: Earnings Loss Effects by Human Capital Accumulation 64
Table 2.6: The Effects of Concentrated Industries on Change in Earnings for Low-Income Workers 65
Table 2.7: The Effects of Concentrated Industries on Change in Earnings (Control Function Approach) 66
Table 2.8: The Effects of Concentrated Industries on Change in Earnings by the Severity of Local Shocks (Control Function Approach) 67
Appendix Table 2.1: Evolution of Concentrated and Non-Concentrated Industries by Year 69
Appendix Table 2.2: Demographic and Work History Characteristics of Job Stayers 70
Appendix Table 2.3: Firm and CBSA Characteristics of Job Stayers 72
Appendix Table 2.4: The Effects of Concentrated Industries on Change in Earnings (Prior to Displacement) 74
Appendix Table 2.5: The Effects of Concentrated Industries (Removing Recalls) 75
Appendix Table 2.6: The Effects of Concentrated Industries (Same Firm Restriction) 76
Appendix Table 2.7: The Effects of Concentrated Industries (Only Low Values for Concentrated Status) 77
Appendix Table 2.8: The Effects of Concentrated Industries by Percentile 78
Appendix Table 2.9: The Effects of Concentrated Industries on Migration 79
Appendix Table 2.10: The Effects of Concentrated Industries on Change in Earnings by Age 80
Appendix Table 2.11: The Effects of Concentrated Industries on Change in Earnings by Educational Attainment 81
Appendix Table 2.12: The Effects of Concentrated Industries on Employment by Educational Attainment 82
Appendix Table 2.13: The Number of Co-workers Voluntarily Separating, by Concentrated Industry Status 84
Appendix Table 2.14: The Effects of Concentrated Industries on Change in Earnings (Within Plant Closers, Birth State Residence) 85
Appendix Table 2.15: The Effects of Concentrated Industries on Change in Earnings by the Severity of Local Shocks (Within Plant Closers, Birth State Residence) 86

Chapter 3 Tables

Table 3.1: Demographic Characteristics 121
Table 3.2: Firm Characteristics 122
Table 3.3: Shift Share Analysis 123
Appendix Table 3.1: Baseline Regression Estimates, Earnings Losses 125

Chapter 4 Tables

Table 4.1: Correlations Between Migration and Job-to-Job Flow Measures ... 166
Table 4.2: CPS-LEHD Matches: Migration Observed Only in CPS in Year t . 167
Table 4.3: CPS-LEHD Matches: Immediately Reversed Migration 168
Table 4.4: Employment, Migration, and Job-to-Job Flows 169
Table 4.5: Decomposition of Residential and Economic Migration Rates 170

List of Figures

Chapter 2 Figures

Figure 2.1: Earnings Evolution for Displaced Workers and Job Stayers 52
Figure 2.2: Earnings Evolution for Displaced Workers and Job Stayers: Business Cycle 53
Figure 2.3: Earnings Evolution for Displaced Workers Relative to Job Stayers: Business Cycle and Concentrated Industry Status 55

Chapter 3 Figures

Figure 3.1: Displacement Rate Time Series 112
Figure 3.2: Conditional Earnings Evolution - Displaced Workers Relative to Job Stayers 113
Figure 3.3: Conditional Earnings Losses Post-Estimation 115
Figure 3.4: Non-employment Periods for Displaced Workers 116
Figure 3.5: Supersector Switchers Share 117
Figure 3.6: Supersector, Non-employment Combinations 118
Figure 3.7: Household Unemployment Insurance Benefits 119
Figure 3.8: Household Earnings Losses 120
Appendix Figure 3.1: Conditional Log Earnings Evolution - Displaced Workers Relative to Job Stayers 124

Chapter 4 Figures

Figure 4.1: Interstate Residential Migration Rates 160
Figure 4.2: CPS and LEHD Migration Rates vs. Matched CPS-LEHD Subset 161
Figure 4.3: Matched CPS-LEHD Migration Rates 162
Figure 4.4: Job-to-Job Flow Rates 163
Figure 4.5: Interstate Migration, Residential and Economic 164
Figure 4.6: Change in Log Earnings 165
Figure 4.A1: Economic Migration Concepts in the CPS 171
Figure 4.A2: Comparison of CPS Interstate Migration Rates Series for Subset of States with Consistent LEHD Data 172
Figure 4.A3: Economic Migration in CPS and LEHD, CPS only for subset of states 173
Figure 4.B1: Change in Log Earnings, Propensity Score Matching 175

Chapter 1: Introduction

When workers lose their jobs due to a layoff, why do so many of them have long, persistent spells of nonemployment? Is this result due to a lack of nearby jobs, or are there jobs that would have hired them if these workers had different skills? While there is substantial documentation of these "displaced workers" having poor employment outcomes after job losses, we know very little as to how their situation can be improved. We know that the labor market outcomes of displaced workers are slightly better if they lose their jobs during an expansion relative to a recession (Davis and von Wachter, 2011). However, the literature does not indicate whether other factors, such as worker human capital and local firm composition, have additional impacts on the outcomes of displaced workers during an expansion. We also do not know the extent to which these factors affect other labor-related outcomes, such as residential migration, for other U.S. workers.

In this dissertation, I analyze how worker human capital, firm composition, and the business cycle affect the labor outcomes of displaced workers after job losses. In addition, I investigate how other workers within the U.S. labor force are affected by some of these factors. In the first chapter, I examine whether the earnings and employment rates of displaced workers improve if these workers reside in an area with a high employment concentration of the industry in which they were initially employed. I evaluate this relationship when job loss occurs during a recession and during an expansion. In the second chapter, my co-authors and I evaluate how changes (before and after job loss) in the earnings of displaced workers are affected by recent recessions and expansions, and how much of this variation across time can be explained by (i) the variation in the job match quality upon re-employment, and (ii) the non-employment duration. In the third chapter, my co-authors and I evaluate how job moves have affected residential migration over the past few decades. In the first two chapters, I look at different factors behind the large "earnings losses" (defined as earnings changes for displaced workers before and after job loss relative to those who retained their job during the same time) for displaced workers, and in the third chapter, I analyze how some of these factors can affect the labor market outcomes of other U.S. workers.

There is an extensive literature on the employment outcomes of job losers (Jacobsen, Lalonde, and Sullivan, 1993; von Wachter, Song, and Manchester, 2009; Schmieder, von Wachter, and Bender, 2010; Couch and Placzek, 2010). On average, displaced workers earn a small fraction of what they used to earn even several years after job loss, and we do not know if this result is due to a lack of demand for their human capital. At their previous jobs, displaced workers accumulated human capital that can be used at all jobs (general human capital), as well as human capital that can be used only within their occupation (occupation-specific human capital), industry (industry-specific human capital), or firm (firm-specific human capital). There is little research on firms' demand for the various types of human capital of displaced workers, and how this demand might vary between expansions and recessions.

If firms have high demand for relevant industry-specific human capital, then a higher concentration of firms in the displaced worker's original industry should increase his chances of finding a good job after a job loss, as long as firms are hiring.

I test whether this relationship is true in the first chapter by analyzing how industry concentration affects changes in earnings of displaced workers before and after job loss. When job loss occurs during an expansion, displaced workers who lose their jobs within one of these “concentrated industries” have earnings changes that are 6-12 percent higher than other displaced workers, and are 2.1-2.8 percent more likely to be employed after job loss. However, I find little evidence that concentrated industries improve employment outcomes when job loss occurs during a recession, and in some specifications I even find a negative relationship between industry concentration and earnings changes. I interpret these results as evidence that firms have high demand for the industry-specific human capital of displaced workers; however, if firms are not hiring, displaced workers’ industry-specific human capital is less likely to matter.

I then evaluate whether displaced workers with higher levels of industry-specific human capital (approximated by industry experience) have even better outcomes within concentrated industries during an expansion. I test whether industry-specific human capital is a large factor for the strong effects found in concentrated industries during an expansion. I find that displaced workers with more industry experience have even better earnings changes within concentrated industries when job loss occurs during an expansion; however, industry experience has little effect during a recession. This result provides more support that the baseline effects during an expansion are a reflection of the valuation placed by firms on industry-specific human capital.

I glean the importance of certain types of human capital through evaluating the local labor market’s effects on job losers. Furthermore, the fact that the effects of concentrated industries vary by the different phases of the business cycle shows that the displaced workers' "returns" to having certain specific human capital are dependent on local conditions. I argue that industry composition, business cycles, and human capital are all important factors in providing a comprehensive analysis of displaced worker outcomes.

One of my findings from the first chapter is that average earnings losses increase substantially between an expansion and a recession. In my first chapter, I used the Great Recession (2007-2009) as my recession period, and the 2005-2006 expansion years as my expansion period; these periods recorded respectively some of the lowest and highest monthly unemployment rates experienced in the US during the last fifteen years. The stark differences in outcomes across these two periods lead to the question of whether there could be variation in displaced worker outcomes across different periods of recessions and expansions.

There is reason to believe that different recessions could affect displaced workers differently. The 2001 Recession, for example, had a peak unemployment rate of 5.7 percent in December 2001, while the Great Recession's (December 2007- June 2009) peak was close to 10.0 percent in June 2009.[1] In addition, the expansion period between 2001 and the Great Recession had an unemployment rate at or above 6.0 percent during April 2003-October 2003, but it dipped to 4.4 percent in December 2006. This variation in the unemployment rate suggests that job availability varies substantially across time, which could lead to variation in earnings losses across time based on when a worker loses his job.

[1] US Bureau of Labor Statistics online

In my second chapter, Henry Hyatt, Erika McEntarfer, and I investigate whether overall job availability affects the degree of variation in displaced workers' earnings losses across recent expansions and recessions. We also investigate the extent to which variation in earnings losses across time is explained by variation in non-employment duration after job loss and variation in subsequent job match quality. We find that there is considerable variation in earnings losses across different recessions and expansions; displaced workers who lost their job during the Great Recession experience earnings losses which are nine percent more severe relative to those who lost their job during the 2001 Recession, and 16 percent more severe relative to those who lost their job during recent expansions. We also find that variations in both non-employment length and in job match quality explain a large portion of earnings loss variation across time. Of the two factors, non-employment plays a bigger role, suggesting that job availability is critical for displaced workers.

We also investigate the extent to which unemployment insurance might possibly mitigate earnings losses across time. We find that even though the unemployment insurance was more generously provided during the Great Recession relative to other periods, it was not enough to compensate for the higher earnings losses suffered by displaced workers. This result shows that job availability is critical to the well-being of displaced workers, since external sources do not provide the necessary compensation.

In my dissertation, I also study the effects of business cycles on labor market outcomes of U.S. workers who do not suffer job losses. Since job vacancies typically decline during a recession, workers are less likely to transition to higher paying jobs during this period (Topel, 1992). Therefore, even workers who do not lose their jobs are negatively affected by recessions. Kaplan and Schulhofer-Wohl (2012) argues that residential migration has declined, which could signal a problem for U.S. workers if migration is mostly driven by job change. If people are moving less because there are fewer job opportunities, then workers are experiencing less earnings growth than before, assuming earnings growth is largely due to job change. In addition, a decline in migration suggests that many areas are experiencing little to no economic growth, since migration matches productive workers to geographic locations (Hsieh and Moretti, 2016).

To test the link between recent long-distance residential migration and job moves, Henry Hyatt, Erika McEntarfer, Alexandria Zhang, and I investigate how interstate migration and job moves have evolved over the past fifteen years (which includes multiple recession and expansion periods). We use four different databases, and we find that the Current Population Survey (the dataset previously used by most researchers), records the largest decline in the interstate residential migration rate with the lowest cyclicality. We find that other databases (American Community Survey, IRS public use data, Composite Person Record) record a less steep decline with more cyclicality in the residential migration rate. We also find that while job moves are associated with interstate residential moves, there is still substantial unexplained variation, suggesting that there are additional factors driving the recent decline in interstate residential migration.

Chapter 2: How are Employment Outcomes Affected by Local Labor Markets After Job Loss?

2.1 Introduction

After involuntary job loss, many workers either drop out of the labor force or take jobs that pay a small fraction of what they used to earn. In fact, workers displaced as part of a mass layoff do not recover to their pre-displacement earnings level even several decades after job loss (Jacobsen, Lalonde, and Sullivan, 1993; von Wachter, Song, and Manchester, 2009; Schmieder, von Wachter, and Bender, 2010; Couch and Placzek, 2010). While these effects are well documented by now, we have very limited evidence as to why many of these workers cannot find jobs that pay them as much as their previous jobs did. One compelling potential explanation is that losing one's job entails a loss of accumulated human capital that cannot be easily transferred across jobs, and as a result, other firms are reluctant to hire these workers after job displacement. However, we know that displaced workers' employment outcomes improve when there are more jobs available in the vicinity (Davis and von Wachter, 2011), which suggests that some of their human capital can be transferred. Understanding the type of human capital that is transferrable after job loss, as well as how surrounding local labor market characteristics determine job availability, are both critical components in fully describing and potentially mitigating the negative economic outcomes of displaced workers.

In this chapter, I investigate the extent to which subsequent employment outcomes of displaced workers are due to a lack of jobs in the vicinity where the human capital of these workers are transferable. I take the displaced worker's pre-displacement firm's industry employment share within the worker's local labor market as a proxy for the prevalence of jobs where their human capital is relevant. I then define "concentrated industries" as industry-CBSA combinations that have a high local employment share value, and I evaluate their effects on earnings losses, which are defined as earnings changes for displaced workers relative to job stayers. I evaluate these effects when job loss occurs during an expansion and during a recession. I use the rich panel available in the Longitudinal Employment Household Dynamics (LEHD) data, and find that when job loss occurs during an expansion, earnings losses are mitigated by 6-12 percent for displaced workers within concentrated industries relative to other displaced workers. Therefore, the earnings changes for displaced workers relative to job stayers within concentrated industries are higher than the same comparison within less concentrated industries. However, when job loss occurs during a recession, there is no significant difference in earnings losses between displaced workers in "concentrated industries" and other displaced workers in most of my empirical specifications, and in other specifications, I find that concentrated industries exacerbate earnings losses during a recession. I interpret these results as strong evidence that displaced workers can transfer their accumulated human capital across firms within an industry when jobs are available. However, when jobs are not available, displaced workers are unable to find firms willing to hire them even if they have relevant human capital. My results show that the poor employment outcomes of displaced workers are partially driven by a scarcity of jobs where their human capital is easily transferable.

I identify the earnings loss effects of concentrated industries via a triple difference regression, where I compare earnings changes for displaced workers relative to job stayers by different levels of industry concentration. I find that 50 to 80 percent of the earnings loss effects by concentrated industries is due to hiring effects, with the rest due to differences in earnings conditional on employment. Furthermore, I find that the employment effects from concentrated industries are largely determined by the worker being employed by another firm in the same industry as that of the original firm.

To further justify my argument that the employment and earnings loss effects from concentrated industries reflect the importance of transferrable human capital, I examine whether displaced workers with more industry experience, and therefore more industry-relevant human capital, have even more of a mitigation in earnings losses within concentrated industries during an expansion. I find this relationship to be true, further suggesting that the employment effects of industry concentration are driven by the valuation of human capital that can be used at all firms within an industry.

I use concentrated industries as a proxy for potentially suitable jobs for three reasons. First, many workers primarily rely on local firms for employment, and most of their job offers will come from these firms. It is crucial to understand how local labor markets affect outcomes for workers who lose their jobs since labor market recovery is typically very slow after a large negative shock (Topel, 1986; Blanchard and Katz, 1992; Bound and Holzer, 2000). Second, there is evidence that it is easier for workers to transition across firms within an industry relative to firms in a different industry. When workers transition across jobs within an industry, earnings typically increase more relative to when they transition to a job outside the industry (Freedman, 2008). Third, as I describe in more detail below, the LEHD data I use do not contain information on the occupation of workers, and so I cannot use occupation as a measure of human capital.

To see why some firms would want to hire only certain displaced workers, it is important to consider what these workers learn at their previous job, and what type of tasks they can perform. While some of their tasks can only be performed at their previous firm (firm-specific), displaced workers may also be capable of performing tasks that are valued by other firms within the same industry (industry-specific). My results suggest that this transferrable "industry-specific human capital" can mitigate some of the earnings loss effects from losing firm-specific human capital.

My results also provide evidence on the magnitude of the return to industry-specific human capital. My results are consistent with studies within the human capital literature that argue that returns to transferrable knowledge for these and other types of workers are large (Neal, 1995; Parent, 2000; Kambourov and Manovskii, 2009; Gathmann and Schönberg, 2010; Pavan, 2011). In a departure from this previous work, however, I allow for potential heterogeneity in these returns across the business cycle, which I show to be a very important distinction. I argue that transferrable human capital returns vary across time for displaced workers, since the earnings loss effects in concentrated industries are large during an expansion but disappear or are sometimes even negative during a recession. Returns to transferrable human capital for displaced workers are negligible during recessions since local firms are less likely to hire during this period, which makes it less likely for a displaced worker to find a job regardless of the industrial composition in the vicinity or their accumulated human capital.

The richness of the LEHD data allows me to control for a variety of threats to identification. The data include migration into favorable economic conditions prior to job loss, firm choice of whom to let go, and time-invariant characteristics with fixed effects.

This chapter proceeds as follows: The next section provides my conceptual framework; section 3 describes the data and presents the empirical model; section 4 explains the methodology; section 5 discusses the results, and section 6 concludes.

2.2 Conceptual Framework

For a worker who is displaced from his firm, any new firm that is considering hiring the worker will evaluate the worker's expected future stream of marginal productivity for the firm relative to the worker's expected future stream of wages, and will only hire the worker if the latter is at least as big as the former. Any new firm that is considering hiring the worker will evaluate the worker's expected future stream of marginal productivity for that firm relative to the worker's expected future stream of wages, and hire the worker only if the former is at least as big as the latter.

If the worker had accumulated firm-specific human capital at the previous employer, this human capital raises the productivity of the worker only in that particular firm, and so it was lost upon displacement. As a result, the expected future stream of the worker's marginal productivity will be lower in any firm that chooses to hire the worker relative to that in the firm that displaced the worker. Assuming that the stream of expected future wages paid to a worker will be equal to the expected future marginal product of the worker, the worker therefore will earn less following displacement due to the loss of firm-specific human capital, and so that worker will be worse off following displacement.

When a worker has accumulated industry-specific human capital and becomes displaced, that industry-specific human capital is only lost if the worker cannot find new employment in the pre-displacement industry, and so the returns to that industry-specific human capital are only lost if the worker switches industries.

My focus is specifically on the impact of the loss of industry-specific human capital. In particular, I consider whether industry concentration in a local labor market can mitigate the cost of displacement, and how this varies across the business cycle as firms expand and contract their hiring. Although the LEHD data I use for estimation has many strengths that allow me to carefully estimate the impact of industry concentration on the cost of displacement, one weakness is that it does not contain information on the occupations of workers, and thus I cannot separately consider occupation-specific human capital. As a result, some of what I estimate as human capital that is specific to an industry may be a proxy for human capital that is specific instead to an occupation. Further research using other data would be necessary to disentangle the two.

The expected earnings of the displaced worker who has accumulated industry-specific human capital for the period after job loss can be expressed as the sum of the expected earnings from employment with a firm within and outside his pre-displacement industry, weighted by the probability of a job offer from within or outside his industry, respectively. In my empirical work, I approximate the probability of re-employment in the pre-displacement industry with the local labor market employment share in the industry.

I label the wages and hours offered from firms within the same industry as the displaced worker's original industry as w_s and L_s, respectively. I label the wages and hours offered from firms outside of the displaced worker's original industry as w_o and L_o, respectively. The expected earnings for a displaced worker after job loss can be written as:

$$E[earn] = w_s L_s \eta_s + w_o L_o \eta_o \quad (1)$$

where η_s and η_o represent the employment share within and outside of the displaced worker's original industry (where the denominator includes those who are non-employed). The local employment share is $\eta_s + \eta_o$. The remainder is the non-employed share, which approximates the probability that no job offer arrives (with zero earnings).

To see how a higher concentration of within-industry firms can affect earnings for displaced workers, I rearrange equation (1) to yield the following:

$$E[earn] = (w_s L_s - w_o L_o)\eta_s + \left(1 - \eta_{nonemp}\right) w_o L_o \eta_o \quad (2)$$

where $\eta_{nonemp} = 1 - \eta_s - \eta_o$. The first term in equation (2) denotes the difference in earnings offered between firms within and outside of the displaced worker's industry. The second term depends on the share of the non-employed, which is a proxy for the probability of not being able to find a job; the share of the non-employed will also be smaller with a higher share of within-industry firms.

Intuitively, equations (1) and (2) can be summarized as follows: When a worker finds himself displaced because of a very small local labor demand shock affecting only a small number of firms (and perhaps only his previous firm itself), the displaced worker is expected to have lower earnings due to the loss of firm-specific human capital. Further earnings losses will be a function of the probability that he can find employment in the same industry, which is a function of that industry's concentration in the local labor market. In contrast, if the worker is displaced because of a significant local labor demand shock, he is at risk of earnings losses due not only to lost firm-specific human capital, but also due to lost earnings from non-employment as well as the possible depreciation of general human capital.

In my empirical work, I ask two specific questions in order to gauge the importance of industry-specific human capital:

Empirical question 1: Do concentrated industries mitigate poor labor market outcomes when job loss is a result of a small local shock that affects only a small number of firms? The extent to which concentrated industries mitigate these poor employment outcomes will depend on firms' valuation of industry-specific human capital.

If concentrated industries mitigate poor labor market outcomes for displaced workers, a natural follow-up question to empirical question 1 is does the extent of this mitigation vary by the extend of accumulated industry-specific human capital?

Empirical question 2: Do concentrated industries mitigate poor labor market outcomes when job loss is a result of a larger local shock that affects many surrounding firms? The effects of industry-concentration will be small if either industry-specific human capital is less valued during large negative shocks or if firms that would have hired these workers in the absence of a negative shock are unable to do so because overall demand is low.

To summarize, the earnings and employment effects of concentrated industries is a reflection of displaced workers' skill transferability within an industry. Furthermore, these effects may vary depending on the magnitude of the local labor market, providing evidence that general human capital is not always transferrable if there is significant job destruction within a local labor market. In order to empirically evaluate these effects, I need to discuss how I define shocks and concentrated industries within my data. I also need to control for other factors on the demand side, such as firm migration and the firm's selection to let go of certain workers, decisions that I do not discuss in here, but will nonetheless affect my estimates. Lastly, I need to control for possible labor supply decisions made by workers before job loss and after in order to capture the relevant labor demand relation. I discuss and do all of these in the subsequent sections.

2.3 Data

2.3.1 Databases

I use the Longitudinal Employer-Household Dynamics (LEHD) and Composite Person Record (CPR) administrative databases from the United States Census Bureau for this analysis. These datasets contain earnings, employment, and geographic information for U.S. workers.

The LEHD dataset is submitted by states and the Quarterly Census of Employment and Wages (QCEW). It is an employer-employee linked dataset that has comprehensive earnings information for many U.S. workers.[2] Since employers subject to state UI laws are required to submit accurate worker and workplace information to the wage records and QCEW, I am able to observe precise quarterly earnings for each worker-employer combination that took place during that quarter (with the exception of people within institutions and the federal, state, and local governments). The LEHD microdata includes basic demographic information about employees (age, sex, education) and employers (NAICS code, location, size).

The CPR dataset contains detailed geographic information for households who submitted tax information or from address information contained in other administrative federal data sources. For each year, I am able to observe the zip code, state, county, and consequently, Core Based Statistical Area (CBSA) of residence.[3] I am able to link the CPR with the LEHD, which allows me to attribute a geographic location for each worker for a given year.[4]

2.3.2 Sample Creation

In order to estimate the effect of concentrated industries on employment outcomes during expansions and recessions, I need a sample of displaced workers

---

[2] John Abowd et al. (2009) provide a thorough description of how the LEHD is constructed.

[3] A CBSA is defined as a geographical area around an urban center of at least 10,000 people that are connected via commuting. CBSAs are defined by the Bureau of Economic Analysis (BEA). The CBSA is very similar in concept to a Metropolitan Statistical Area (MSA), with the only difference coming from the minimum population threshold required for classification (MSAs require at least 50,000 people). CBSAs are defined by a union of state-counties, and no state-county combination is mapped to more than one CBSA.

[4] Although the CPR is broad in its coverage, it does not contain information on individuals who do not appear in tax filings or other federal data. Abowd et al (2009) describe this data in detail.

who exogenously lose their jobs during an expansion, and another sample of displaced workers who exogenously lose their jobs during a recession. I also need a sample of workers who did not lose their jobs ("job stayers") with similar characteristics to my displaced worker sample. These job stayers will allow me to argue that had they not lost their jobs, the displaced workers' earnings changes would have been similar to the job stayers'. Since I am comparing the earnings changes of displaced workers and job stayers by industry concentration, I also require similar characteristics for these worker types across this dimension. Two characteristics that need to be comparable are the workers' level of industry-specific human capital, and the firm-specific human capital in their previous firms for displaced workers and in current firms for job stayers. To plausibly argue that the employment effects of industry concentration are due to more firms demanding the same level of transferrable knowledge across firms in an industry, my sample of workers needs to have similar levels of industry-specific human capital across displaced workers and job stayers, as well as across industry concentration. I also need to have comparable firm-specific human capital cross displaced workers/job stayers and industry concentration, since any differences in firm-specific human capital across industry concentration can also affect the estimated effects.[5]

I therefore need to select displaced workers with long job tenure prior to job loss in order to have workers with comparable firm-specific and industry-specific human capital. Tenure is a common proxy for firm-specific human capital accumulation (Topel, 1991; Altonji and Shakotko, 1989; Abraham and Farber, 1987;

[5] If one sample of workers is more reliant on firm-specific knowledge, then they will be less able to find another job, since this knowledge can only be applied at their original firm.

Altonji and Williams, 2005), but those with long tenure by definition also have significant industry experience, since they have spent more time within the firm's industry. This industry experience leads to accumulation of knowledge that is transferrable within an industry, i.e., a higher level of industry-specific human capital. My sample of job stayers must also have the same restriction to ensure they have similar levels of firm-specific and industry-specific human capital.

Since I am using expansions and recessions as approximations for high and low job availability, I need to select an expansion phase with high hiring rates, and a recession phase with very low hiring rates. I use data from 2005 Q1 and 2006 Q4 as the expansion phase, as this two-year period recorded the lowest unemployment rate and the highest job opening rate since 2000. I use data from 2007 Q4 to 2009 Q2 (i.e., the Great Recession) as the recession phase.[6] Most industries experienced net employment losses during the Great Recession (BLS Spotlight on Statistics 2012), allowing me to obtain a more representative sample of displaced workers across all industries during these years.

Taking into consideration the business cycle and tenure requirements I outlined, I take the following steps to create my sample. I first take all workers who experienced displacement during any quarter during my selected expansion and

[6] The Great Recession is defined by the NBER as occurring from December 2007 to June 2009. Since I can only use quarters, I define the Great Recession as occurring between 2007 Q4 and 2009 Q2. I cannot definitively state how well one can extrapolate my findings to other expansions/recessions. Most industries experienced very high employment losses during the Great Recession, while in other recessions, most of the losses were dominated by a select few industries. Also, the U.S. economy experienced a much slower recovery period after the Great Recession relative to other recessions. I therefore interpret my estimates for the Great Recession as a lower bound relative to other recessions. I also interpret my estimates for 2005-2006 as a lower bound relative to other expansion periods, since the unemployment decrease here was lower than most expansion periods.

recession phases.[7] I then limit the sample to displaced workers with at least five consecutive quarters of tenure to ensure that they had accumulated a sufficient amount of firm-specific and industry-specific human capital.[8] For each quarter in my displaced worker sample, I also construct a one percent random sample of job stayers with at least five quarters of tenure prior to that quarter (inclusive), and an additional 12 consecutive quarters of tenure subsequently. I require all workers to be part of both the LEHD and the CPR, and reside within a valid CBSA, the unit I use to define a local labor market. Lastly, I require the worker to have never worked within nine states that submitted information to the LEHD only recently, since earnings histories for these workers are incomplete.[9]

---

[7] One problem I encounter within my data is I cannot directly observe displacement from an establishment. This is a common problem for displaced worker analysis using administrative data, so I use a common measure from the literature to proxy for displaced workers. I define a worker to be displaced during period t if three criteria are met: (i) There is a 30 percent decrease in the worker's establishment size. To calculate the 30 percent decrease, I use the establishment's maximum employee count from periods $t-2$ and $t-1$, and the minimum value from periods $t+1$ and $t+2$, in order to capture potential declining trends in the establishment size; (ii) The maximum employee count in periods $t+1$ and $t+2$ is above 50 people; (iii) The worker has to have a "dominant job separation" from the establishment in period t, i.e., the worker either completely loses employment at this establishment, or that establishment no longer provides him with the highest earnings. An example is if an establishment has 55, 50, 45, 5, 3 workers during 2002 Q1, Q2, Q3, Q4, and 2003 Q1, and if the worker separates during 2002 Q3, then the worker is flagged as a displaced worker during 2002 Q3.

[8] The five quarter tenure restriction includes the quarter of displacement. Therefore, if a worker loses his job during 2002 Q1, he needs to have had that job for every quarter from 2001 Q1 to 2002 Q1, inclusive.

[9] The first year in my sample is 2005, but I use historical information for workers that go back several years before that year. Unfortunately, nine states did not submit information to the LEHD before 2001 (Alabama, Kentucky, Wyoming, Arkansas, Mississippi, New Hampshire, Arizona, DC, Massachusetts). Therefore, if somebody worked within one of these states before 2001, I am unable to observe which firm s/he worked at or how s/he earned. I therefore restrict the analysis to those who I never observe to work within these nine states after 2001, with the assumption that those that work within these states in later years are more likely to have worked there in previous years. I do this restriction to mitigate some of the measurement error in the employment history, but obviously, there is likely to be some measurement error that remains, since some included workers could have worked within one these states before 2001.

2.3.3 Concentrated Industry Definition, Outcomes of Interest

I use the earnings changes recorded in the LEHD as the primary outcome in my analysis. I define earnings changes by first aggregating earnings across all jobs within a six-month interval (half-year), and then taking the difference of the aggregated earnings across time for each worker. I define "earnings losses" as the earnings change for displaced workers relative to job stayers; the assumption is that had the displaced worker not lost his job, his earnings growth would have been the same as the job stayers' during this period. My main empirical question is how concentrated industries affect earnings losses within each half-year interval for up to three years after job loss for a displaced worker. Since my time window is short relative to most papers within the literature, my results can be interpreted as either short-run or possibly medium-run effects.[10]

I categorize a displaced worker as displaced from a "concentrated industry" based on his pre-displacement industry's employment share within his local labor market, using the three-digit North American Industry Classification System to group firms as an industry.[11] Similarly, I categorize a job stayer as within a concentrated industry based on his industry's employment share. By using the industry share instead of the industry size (i.e., employment totals), I avoid over-representing large CBSAs in my classification of industry concentration. However, an industry's high

[10] Jacobsen, Lalonde, and Sullivan (1993) and Couch and Placzek (2011) evaluate earnings losses over a six-year window; Schmeider, von Wachter, and Bender (2010) evaluate earnings losses over a 15-year window. Davis and von Wachter (2011) and von Wachter, Manchester, and Song (2009) evaluate earnings losses over a 20-year window.

[11] NAICS defines their three digit codes as a "Subsector", which is a more coarse definition than an industry (defined at the five-digit level). There is little difference in my results using the three, or four-digit NAICS definitions, but I choose the broadest category to capture firms that are more likely to be part of the same production process (Delgado, Porter, and Stern, 2010; 2012). For shorthand purposes, I still refer to the NAICS three-digit codes as industries.

employment share is not necessarily a product of being within a labor market with a large population.

I attribute a CBSA to each displaced worker from the CPR during the calendar year, and the industry from the firm where he lost his job. For job stayers, I assign the CBSA in the same manner, and I assign the industry based on their firm. I then define a concentrated industry based on the employment share of that industry within the attributed CBSA four quarters prior to the quarter of job loss for displaced workers, and the quarter of sample inclusion for job stayers. I choose this timing because I want to measure how earnings losses depend on any initial industrial composition within the labor market, without incorporating any potential changes in the industrial composition after a shock. Formally, my measure is:

$$s_{jct} = \frac{\#\, empl\ ind_{j,}\ CBSA_c, Quarter\ t}{\#\, empl\ CBSA_c, Quarter\ t}$$

where j refers to the industry of the worker's initial job, c is the CBSA of residence during job loss, and t refers to the quarter four quarters before job loss. The denominator is the total number of employed workers within CBSA c, and the numerator is the total number of employed workers within CBSA c and industry j. For example, if a worker is displaced from the "electrical equipment, appliance, and computer manufacturing" industry (NAICS code 335) within the Akron (Ohio) CBSA during 2003 Q3, I use the employment share of that industry within 2002 Q3 for that worker. Assuming that job vacancies are positively correlated with availability of employment, the industrial concentration has an ordinal property for potential jobs. However, I cannot say this metric has a cardinal property, since I cannot quantify the exact relationship between industry job vacancies and employment shares. Therefore, rather than looking at a linear relationship, I classify CBSA-industry combinations with a s_{jct} value higher than the 75th percentile within an industry across CBSAs as a "Concentrated Industry," and those lower as a "Non-Concentrated Industry."[12] I choose to use the industry's national distribution as a reference point, since different industries have different distributions in the nation, and my concentration definition needs to reflect this fact.[13] The drawback with this approach is that I will flag some industry-CBSAs as concentrated only because the industry has a larger local employment share relative to its presence in other CBSAs (e.g., even if industry j has only one percent of employment in CBSA c, if that share is higher than industry j's local share in other CBSAs, the industry j-CBSA c combination will be flagged as concentrated). I compare earnings changes for workers within an industry, so occasionally classifying industry-CBSAs with a small local employment share as concentrated will still be appropriate given that the industry is less concentrated in other CBSAs. Continuing with my previous example, displaced workers will be flagged as being displaced within a concentrated industry during 2002 Q3 if they: (i) reside within CBSA c in 2002 Q3; and (ii) the $s_{335\,c\,2001\,Q3}$ is above the 75th percentile for industry 335.[14]

[12] The distribution is based on the CBSA level. Therefore, the 75th percentile is determined within an industry across CBSAs for a point in time. I restrict CBSA-industries to those with at least 100 workers.

[13] Ellison and Glaeser (1997), Dumais et al. (2002), Freedman (2008), and Ellison et al. (2010) use similar logic.

[14] I find my results to be robust to several percentiles above the median. Another concern is the volatility of my industry concentration measure across time, since both firms and workers migrate into and out of different locations. If the flow rate into or out of a local labor market has a greater effect on one industry relative to other industries, then the industrial employment share will change within a local labor market. Appendix Table 2.1 verifies that my classification of industry concentration is fairly constant across time.

2.4 Methodology

I use the sample of displaced workers and job stayers, and run the following regression to evaluate the effects of concentrated industries on earnings losses during an expansion and during a recession:

$$\Delta_{\mathrm{t}} Y_i = \beta_0 + \gamma'_{c(i)} + \gamma'_{q(i)} + \gamma'_{j(i)} + \beta_1 X_i + \alpha_1 D_i + \alpha_2 Conc_{c(i)j(i)} + \alpha_3 Conc_{c(i)j(i)} * E_{q(i)} + \alpha_4 D_i * E_{q(i)} + \lambda_1 D_i * Conc_{c(i)j(i)} + \lambda_2 D_i * Conc_{c(i)j(i)} * E_{q(i)} + u_i \qquad (E1)$$

$$\lambda_{expansion} = \lambda_1 + \lambda_2, \; \lambda_{recession} = \lambda_1$$

where $\Delta_{\mathrm{t}} Y_i$ is the change in earnings for worker i across the two half-year intervals. For displaced workers, the initial earnings level refers to his earnings during the six months prior to job loss, while the subsequent earnings level refers to his earnings within a six-month period after job loss.[15] I estimate six different regressions for each six-month period within the three years following job loss, so the subsequent earnings level refers to either: the six months after job loss, 6-12 months after job loss, 12-18 months after job loss, etc. For job stayers, the initial and subsequent earnings levels refer to the same time span, but does not involve job loss.

$\gamma'_{c(i)}$ are dummies for worker i's CBSA residence $c(i)$. Since I am first-differencing my empirical equation, these dummies capture heterogeneity in the earnings trends across CBSAs, allowing me to compare earnings losses within a CBSA. These dummies control for observable changes in CBSA characteristics such as population changes, as well as characteristics such as changing natural resource

[15] I omit the quarter of job loss for this analysis. Since I am unable to pinpoint whether a worker lost his job during the first week of a quarter (where he has only one week of earnings for the quarter) or the last week (where he earns every week except the last week of the quarter), including this quarter could cause unwanted variation for the first earnings change outcome.

advantages and susceptibility to shocks, which are more difficult to quantify and which could affect my estimated parameters of interest.

$q(i)$ is a set of dummies defined as the quarter of displacement for displaced workers, and as the same calendar quarter for job stayers.[16] These dummies control for differences across a business cycle that are unrelated to being in a concentrated industry. $\gamma'_{j(i)}$ are dummies for workers i's industry, $j(i)$, which refer to either the firm he is currently employed at for job stayers, or the firm he was displaced from for displaced workers. These dummies capture national industry trends, including possible changing rent-sharing practices by certain industries, which could affect the interpretation of my estimates. I also include a vector of demographic characteristics X_i (education, gender, race-ethnicity, and age) that I assume to have time-varying effects on earnings, which may be correlated with the covariate of interest.[17]

My covariates of interest are $D_i * Conc_{c(i)j(i)}$ and $D_i * Conc_{c(i)j(i)} * E_{q(i)}$. The first term is a product of a dummy that equals 1 if worker i is displaced (D_i), and another dummy that equals 1 if worker i is part of a CBSA-industry that is flagged as concentrated ($Conc_{c(i)j(i)}$). The second term is a product of the previous two dummies as well as another dummy, $E_{q(i)}$, which equals 1 if the worker lost his job during an expansion. The coefficient on the first term, λ_1, represents the comparison of earnings changes for displaced workers relative to stayers within concentrated industries relative to non-concentrated industries during a recession. Therefore, I label $\lambda_{recession} = \lambda_1$. The sum of the first and the second term, $\lambda_1 + \lambda_2$, represents the

[16] For example, the 2005 Q2 dummy equals 1 for a displaced worker if he loses his job during that quarter, and equals 1 for a job stayer if he had at least five quarters of tenure prior to that quarter.
[17] These are the only demographic characteristics available within the LEHD.

analogous comparison during an expansion. Therefore, I label $\lambda_{expansion} = \lambda_1 + \lambda_2$. With my empirical specification, I am able to track the evolution of these estimates across time.[18] Since concentrated industry status depends on the CBSA-industry combination, I cluster my standard errors by this dimension.

My identifying assumption relies on no other unobservable factors being correlated with either interaction term, conditional on the controls. I do not need to worry about time-invariant characteristics that have time-invariant effects, since I first-difference my empirical model. I also do not need to worry about trends of unobservable characteristics correlated with the CBSA or the industry, since these are included as dummies in my model. I discuss and control for specific potential threats to these assumptions (endogenous displacement by firms, labor supply decisions by workers) in the robustness subsection, and largely focus on my baseline empirical model for the results.

2.5 Results

2.5.1 Sample Characteristics

Figure 2.1 displays the overall earnings trend for the workers in my sample. Displaced workers report annual earnings of just below \$38,000 prior to job loss, \$23,000 a year after job loss, \$27,000 two years after job loss, and \$28,000 three years after job loss. These figures suggest an initial earnings decline of 50 percent, and a net decline of 30 percent three years after job loss. Job stayers, on the other

[18] For notational ease I run these changes within separate regressions; joint estimations yield similar results.

hand, start with a higher earnings level, and continue to have steady growth during the same period. A similar pattern emerges when I compare the earnings of displaced workers and those of job stayers during the expansion and recession periods, since within both the expansion and recession periods displaced workers have a large decline in earnings while job stayers experience continued earnings growth. However, Figure 2.2. shows that the earnings dip for displaced workers is considerably smaller relative to job stayers when job loss occurs during an expansion relative to a recession. This difference in earnings dip across expansions and recessions could be due to the overall scarcity of jobs, or to the scarcity of jobs suited for the displaced worker's skillset. To quantify the extent to which concentrated industries mitigate earnings losses, I use the unconditional earnings losses (i.e., the difference in earnings changes for displaced workers relative to job stayers) within each period as a reference point. For example, if earnings losses for displaced workers are mitigated in concentrated industries by $500 across two years during an expansion, and the unconditional earnings losses are -$7,500 during this same period, I say that earnings losses are mitigated in concentrated industries by 500/7500 = 6.67 percent.

A natural concern with comparing displaced workers and job stayers across the business cycle and by industry concentration is that these workers could have differences in characteristics that also affect changes in earnings. Table 2.1 compares the demographic characteristics for displaced workers across the business cycle and industry concentration. There are some differences across industry concentration, but I am unable to conclude whether displaced workers within concentrated industries are likely to have higher earnings losses relative to other displaced workers based solely on these differences. Relative to concentrated industries, non-concentrated industries have a higher share of white displaced workers, with the largest difference occurring during an expansion (a seven percentage point difference). This difference might suggest that displaced workers in non-concentrated industries should have less severe earnings losses, for whites have (on average) higher employment rates and higher earnings relative to other groups. However, relative to non-concentrated industries, concentrated industries have a higher share of male displaced workers, again with the largest difference occurring during an expansion (a four percentage point difference), suggesting that displaced workers in concentrated industries could have less severe earnings losses (since men have higher employment rates and wages relative to women). The average age of the workers within my sample is 42 years old, with 11 years of observed experience, and there is very little difference in both of these characteristics between workers in concentrated and non-concentrated industries.[19] Since there are small differences in age and experience, and there is a higher prevalence of males in concentrated industries, and a higher prevalence of whites in non-concentrated industries, I cannot say which set of workers is more likely to experience more severe earnings losses based on compositional differences.[20]

Table 2.2 provides the characteristics of the displaced workers' original firms. Unlike demographic characteristics, there are noticeable differences in firm

[19] Experience is defined as the number of quarters where the agent had any earnings from any job, divided by four. The experience averages increase across time, but that is most likely due to the fact that the historical data is more complete (i.e., more states are included) across time. Even though I impose a restriction on the states for 2001-2010, I do not impose a restriction for the experience statistic.

[20] Appendix Tables 2.2 and 2.3 show that job stayers' demographic and firm characteristics do not change substantially across time; hence I focus mostly on the sample of displaced workers. The patterns for job stayers are similar to those of displaced workers, i.e., there are some small differences in demographic characteristics across industry concentration within a recession/expansion, and there are large differences in supersector origin across industry concentration.

characteristics across the business cycle, suggesting that I could be comparing displaced workers with different unobservable characteristics across the business cycle. The most noticeable difference is that the percentage of workers displaced from construction and manufacturing is much larger within the recession period relative to the expansion period. During recessions, construction and manufacturing shares comprise roughly 33 percent of the sample, while during expansions they comprise only 24 percent of the displaced workers. Although there are also noticeable differences by industry concentration within expansions/recessions, I am unable to conclude which set of workers is likely to experience higher earnings losses based on these differences. Although firm size and age differences are negligible, there are noticeable supersector differences by industry concentration. Relative to non-concentrated industries, concentrated industries have a higher share of displaced workers from the professional and financial services. Relative to concentrated industries, non-concentrated industries have a higher share of displaced workers from manufacturing, leisure/hospitality, and education/health. Professional and financial services jobs are usually more lucrative than manufacturing or leisure/hospitality jobs, but education/health jobs are usually more stable during recessions (BLS Spotlight on Statistics, February 2012), so it is not clear whether there should be more severe earnings losses in concentrated industries.

Figure 2.3 shows the earnings evolution of displaced workers relative to job stayers by industry concentration status. There are negligible differences in the unconditional changes, within both expansions and recessions, but these differences could be due to compositional effects from comparing different types of workers.

Given the clear differences in some demographic and firm characteristics across displaced worker categories, the demographic controls as well as the industry and CBSA dummies are necessary in order to provide credible estimates.

2.5.2 Baseline Estimates

Table 2.3 presents the estimates from equation ($E1$). Columns (1) and (2) show job loss during an expansion and during a recession, respectively. During an expansion, there are more available jobs, allowing displaced workers to have easier access out of non-employment, as evidenced by the unconditional earnings losses being roughly $2,660 ($10,389-$7,734) less severe six months after job loss during an expansion relative to six months after job loss during a recession. This pattern persists throughout my time window, for earnings losses are $3,220, $3,000, $2,760, $2,080, and $1,620 less severe for each of the 6 months windows after job loss. [21] There are significant positive effects in concentrated industries when job loss is concurrent with an expansion; earnings losses in concentrated industries are mitigated by roughly $400 to $600 across the different earnings changes outcomes, translating to a 6.2-11.8 percent mitigation in earnings losses over the three year time window.[22] Conversely,

[21] Just like any difference-in-difference/triple difference estimation strategy, I assume that in the absence of the "treatment," the outcome of the control group(s) would have been similar to that of the treatment group. Appendix Table 2.4 has the estimates when I use time intervals before displacement, which essentially compares the earnings growth of the workers who eventually lose their jobs. Differences are largely insignificant, suggesting that displaced workers are just as comparable to job stayers within concentrated industries relative to non-concentrated industries.

[22] The effects of concentrated industries for the six outcomes are: $476, $404, $529, $448, $612, and $496. The unconditional average difference in earnings changes for displaced workers versus job stayers is -$7,734. The percentage is calculated as follows for the first half-year period: $476/-$7,734 = 6.2 percent.

during a recession, earnings losses in concentrated industries are not significantly different than those in non-concentrated industries, and the unconditional average earnings losses, as approximated by average displaced worker earnings changes minus average job stayer earnings changes, is $2,000 to $3,000 more severe during recessions relative to expansions.

To test the robustness of my results, I first run two initial tests to see how changes in my original sample choice affect my baseline estimates. In one test, I remove from my sample displaced workers who were recalled back to their original firm, since their earnings losses could be mitigated by retaining their original firm-specific human capital. Appendix Table 2.5 shows that removing these workers from my sample has little influence on my results. I calculate another set of estimates after restricting my sample to displaced workers and job stayers who are part of the same firm; once again, as shown in Appendix Table 2.6, this specification does not affect my qualitative results.

I also test how sensitive my concentrated industry metric is to various definitions. As noted earlier, I could flag some industry-CBSAs as concentrated because the industry has a high employment share in a CBSA relative to the same industry in other CBSAs, and not necessarily because the industry has a high absolute local employment share. I therefore remove all workers who are: (i) labelled as workers from a concentrated industry; and (ii) from an industry with an industry share greater than two percent of the local population (two percent corresponds to roughly the 25^{th} percentile of the local employment share for all the workers within concentrated industries). That way, I compare non-concentrated industries to concentrated industries with a low local employment share. Appendix Table 2.7 shows that most estimates during an expansion are still positive, and the estimates within the recession are still insignificant.

I also make comparisons across different percentiles to see whether these patterns hold across different levels of industry concentration. Appendix Table 2.8 shows comparisons for the 25th to 50th percentile relative to below the 25th percentile, 50th to 75th percentile relative to below the 25th percentile, and above the 75th percentile relative to below the 25th percentile.[23] The results show that within an expansion, the mitigation effects increase monotonically across percentile thresholds, although the differences are not always significant. During a recession, however, mitigation effects (mostly) decrease monotonically, although again the differences are not always significant.

While displaced workers in concentrated industries still incur earnings losses during an expansion, their earnings losses are smaller than those of displaced workers in less concentrated industries. However, during a recession, local industry concentration has less of an impact, sometimes even a negative impact for displaced

---

[23] The regression specification is:

$$\begin{aligned}\Delta_t Y_i = \beta_0 + \gamma'_{c(i)} + \gamma'_{q(i)} + \gamma'_{j(i)} + \beta_1 X_i + \alpha_1 D_i + \alpha_2 C2550_{c(i)j(i)} + \alpha_3 C5075_{c(i)j(i)} + \alpha_3 C75_{c(i)j(i)} \\ + \alpha_4 C2550_{c(i)j(i)} * E_{q(i)} + \alpha_5 C5075_{c(i)j(i)} * E_{q(i)} + \alpha_6 C75_{c(i)j(i)} * E_{q(i)} + \alpha_7 D_i \\ * E_{q(i)} + \lambda^1_{2550} D_i * C2550_{c(i)j(i)} + \lambda^1_{5075} D_i * C5075_{c(i)j(i)} + \lambda^1_{75} D_i * C75_{c(i)j(i)} \\ + \lambda^2_{2550} D_i * C2550_{c(i)j(i)} * E_{q(i)} + \lambda^2_{5075} D_i * C5075_{c(i)j(i)} * E_{q(i)} + \lambda^2_{75} D_i \\ * C75_{c(i)j(i)} * E_{q(i)} + u_i \qquad (1)\end{aligned}$$

Where $C2550_{c(i)j(i)}$ is a dummy that equals 1 if the worker is in an industry-CBSA within the 25th percentile to the median of local employment share across CBSAs within an industry. $C5075_{c(i)j(i)}$ is the equivalent dummy for the median to the 75th percentile, and $C75_{c(i)j(i)}$ is the equivalent dummy for above the 75th percentile.

The λ^1_{2550} estimate compares earnings losses for workers in industry-CBSAs within the 25th percentile to the median (i.e., $C2550_{c(i)j(i)} = 1$) relative to workers in industry-CBSAs that are below the 25th percentile during a recession; the $\lambda^1_{2550} + \lambda^2_{2550}$ estimate is the analogous estimate during an expansion. The other λ^1, $\lambda^1 + \lambda^2$ estimates are analogous estimates that compare workers within industry-CBSAs in that particular percentile range relative to industry-CBSAs that are below the 25th percentile (during a recession for λ^1 and during an expansion for $\lambda^1 + \lambda^2$).

workers, since firms are less willing to hire. Alternatively, firms might be slightly more willing to hire displaced workers with relevant industry-specific human capital during a recession, but only temporary or part-time work with possibly lower wages, which may not provide a sufficient increase in the average earnings effects. Likewise, the effects during an expansion may also be driven primarily by hiring or by better paying jobs. The positive effects of industry concentration during expansions provide evidence that displaced workers have skills that are demanded by firms within an industry, and if more of these firms are hiring, displaced workers are more likely to find jobs.

Table 2.4 presents the results from the same kind of regression model with employment as the dependent variable, allowing me to gauge whether the earnings losses in Table 2.3 are driven primarily by hiring differences or by other factors.[24] In Columns 1 and 2, I define employment as binary in this model, with 1 indicating positive earnings during that half-year and 0 indicating negative earnings.[25] I find that the overall employment regression estimates mirror the earnings loss estimates closely, but there are some interesting differences; during expansions, displaced workers in concentrated industries experience a significant 1.6-2.1 percentage point increase in the probability of being employed after job loss throughout the period of my analysis. Using a back of the envelope calculation, these effects imply that employment probability differences explain roughly 50 to 80 percent of the earnings

---

[24] While this is now a cross-sectional regression and not a first-difference regression, since by definition everyone in my sample is employed during the 6 month period prior to job loss, this regression is very similar to evaluating employment status changes.

[25] I do not include job stayers in these estimates, since job stayers are by definition always employed. Therefore, my empirical equation for the employment regressions is:$\mathrm{Y_i} = \beta_0 + \gamma'_{c(i)} + \gamma'_{q(i)} + \gamma'_{j(i)} + \beta_1 X_i + \lambda_1 Conc_{c(i)j(i)} + \lambda_2 Conc_{c(i)j(i)} * E_{q(i)} + u_i$. Where $\lambda_{expansion} = \lambda_1 + \lambda_2$ and $\lambda_{recession} = \lambda_1$.

loss effects of industry concentration during expansions.[26] This result provides evidence that hiring is an important factor behind the earnings effects of concentrated industries, but the combination of hours and wages plays a non-negligible role as well. For recessions, however, a slightly different pattern emerges. For the first six months after job loss, concentrated industries experience significant and negative effects, followed by a year and a half of no significant effects, and then significant and positive effects for the last year. Initially, these results suggest that despite slight differences in hiring patterns, the jobs that do hire do not pay well enough to produce a noticeable effect in mitigating average earnings losses during a recession.[27]

To further investigate the type of employment behind the earnings change effects in concentrated industries, I assess whether the displaced worker is re-hired by his original industry. The results are reported in Columns 3 and 4 of Table 2.4. I define the outcome as a binary variable, with 1 representing the displaced worker re-employed within the same industry as his initial job, and 0 representing all other outcomes. Unsurprisingly, during an expansion, the effects of a concentrated industry are larger for this outcome relative to overall employment, with effects ranging from 3.3-3.8 percentage points, which translates to a 7.8-9.6 percent increase. What is more interesting, however, is that during a recession, most of the effects are positive and

[26] I obtain these percentages by doing the following: I first take the employment effect estimate, and divide by the unconditional employment probability to get a percentage effect. I then multiply this percentage effect by the unconditional post-job loss earnings average. I then take this product, and divide by the earnings change effect from Table 2.3. For example, within an expansion, 18 months after job loss, the estimate is 0.019, the percentage effect is 0.019/0.81 = 2.3 percent, the unconditional earnings average is $14,146, and the earnings change effect is $529.. Therefore, the percentage explained by employment is 0.023*14,146/529 = 62.7 percent..

[27] This is not a product of migration outflows, i.e., workers from concentrated industries moving away from a shock. Appendix Table 2.9 shows that during an expansion, workers from a concentrated industry are more likely to stay, suggesting that these workers think that they have better opportunities of finding jobs if they stay.

significant. For the first half-year, the effect is insignificant, but the within-industry employment effects subsequently range from 1.5-2.8 percentage points, which translates to a 4-8.7 percent increase. This result suggests that concentrated industries are still more likely to hire workers during a recession, but given the lack of earnings loss effects, these jobs are not paying sufficiently high wages to have an overall positive effect in terms of mitigating average earnings losses.

To test whether industry-specific human capital is the driving force behind these effects, I look at potential heterogeneity by assessing the degree of industry experience the displaced worker had accumulated prior to job loss. If earnings losses are mitigated in concentrated industries through higher valuation of industry-specific human capital, then displaced workers in concentrated industries with more industry-specific human capital should experience an even larger mitigation in earnings losses during an expansion. Furthermore, during recessions, since the available jobs do not pay that much, it is unlikely that having more industry-specific human capital will help mitigate earnings losses.

Formally, I estimate:

$$\Delta_t Y_i = \beta_0 + \gamma'_{j(i)} + \gamma'_{c(i)} + \gamma'_{q(i)} + \beta_1 X_i + \pi HC_i + \lambda_{Iexp} Conc_{c(i)j(i)} * Iexp_i * D_i + \lambda_{exp} Conc_{c(i)j(i)} * Exp_i * D_i + \lambda_{ten} Conc_{c(i)j(i)} * Ten_i * D_i + u_i \quad (E2)$$ [28]

[28] Note that πHC_i represents the appropriate intercepts/interaction terms.

$$\pi HC_i \equiv \pi_1 Conc_{c(i)j(i)} + \pi_2 Iexp_i + \pi_3 Exp_i + \pi_4 Ten_i + \pi_5 D_i + \pi_6 Conc_{c(i)j(i)} * Iexp_i + \pi_7 Conc_{c(i)j(i)} * Exp_i + \pi_8 Conc_{c(i)j(i)} * Ten_i + \pi_9 Conc_{c(i)j(i)} * D_i + \pi_{10} D_i * Iexp_i + \pi_{11} D_i * Exp_i + \pi_{12} D_i * Ten_i$$

I use the same notation as in equation $(E1)$. I estimate the equation above separately within my expansion and recession period.[29] I include three different variables to proxy for three different forms of human capital: tenure, which approximates firm-specific human capital; industry experience, which approximates industry-specific human capital; and overall work experience, which approximates general human capital. I include experience and tenure to test whether the effects from the industry experience parameter are spurious, since industry-specific human capital should be the only component that influences the effects of concentrated industries. The parameters λ_{Iexp}, λ_{exp}, and λ_{ten} capture the earnings changes for displaced workers relative to job stayers by industry concentration, and across human capital levels (for industry-specific, general, and firm-specific human capital, respectively). Unsurprisingly, λ_{Iexp} most resembles the patterns in Tables 2.3 and 2.4, as shown in Table 2.5. During an expansion, when the effects of industry concentration on reducing earnings losses are positive and significant, the λ_{Iexp} coefficient is likewise positive and significant, implying that those with higher levels of industry-specific human capital experience greater mitigation of earnings losses.[30] Furthermore, λ_{exp} is insignificant, while λ_{ten} is negative and significant. These results reinforce my hypothesis that industry-specific human capital allows displaced workers to find jobs with higher pay in concentrated industries when job vacancies are more prevalent. During a recession, λ_{Iexp} is not significant, λ_{ten} is negative and significant, while

[29] The alternative is that I estimate one model with data from both my expansion and recession periods, and include an additional interaction term to reflect differential effects during these two periods. The results do not change significantly either way.
[30] Within my sample, the 10^{th}, 25^{th}, 75^{th} , and 90^{th} percentile of industry experience is 2.5, 4, 17, and 26 half years. Therefore, when incorporating these levels with the effects in Table 2.5, it is clear that λ_{Iexp} is a large estimate.

λ_{exp} is positive (but small in magnitude) and significant for all periods after job loss. Given that earnings losses were not mitigated in concentrated industries during a recession, these results show that industry experience effects are correlated with the earnings loss effects of industry concentration when job loss occurs during a recession.[31]

My interpretation of the results so far is that these earnings loss estimates are driven by firm decisions, and do not consider the worker's decision making process. However, workers could either systematically move to more favorable conditions prior to job loss or wait for better opportunities after job loss, and if these actions vary by prevalence across industry concentration, I could be picking up the effects from these labor supply decisions in my estimates. In addition, concentrated industries could have firms that are more or less selective in whom they displaced, which could also affect my estimates, since this is not part of my intended relationship of interest. In the next section, I explore the possibility of selection and other threats to my identification strategy.

[31] The accumulation of the effects of industry-specific human capital are not likely due to any spurious correlation with age, i.e., the possibility that older workers are enjoying additional positive effects. In Appendix Table 2.10, I show that this is not a concern, since there is very little variation in the effects of concentrated industries by age. Note that there are varying effects of concentrated industries by education level. Appendix Table 2.11 and 2.12 show earnings and employment breakdown by education. The earnings losses of displaced workers with high education are mitigated in an expansion, providing evidence that firms value the interaction of industry-specific human capital with the general human capital associated with higher education.

2.5.3 Labor Supply Investigation

2.5.3.1 Labor Supply Decisions Made After Job Loss

One threat to my identification strategy comes from possible labor supply decisions made by displaced workers after job loss. Some displaced workers may choose to wait for better job offers if they are unsatisfied with the job offers they receive shortly after job loss. As a result, these workers would prolong their non-employment duration, which would obviously exacerbate their earnings losses. This scenario may be more likely to occur during an expansion, where the option value of waiting for a job offer is higher than the option value during a recession. If workers are more likely to wait for better offers in a concentrated industry, my OLS estimates are more likely to be biased downward during an expansion. Given that I am interested in how firm decisions drive the employment effects of concentrated industries, I do not need to remove labor supply effects entirely; rather, I need to ensure that any labor supply effects are the same across industry concentration status.

Workers are more likely to wait for better job offers if they have sufficient liquidity to remain in non-employment.[32] In order to mitigate this effect, I look at the effects of concentrated industries for people who I consider to be liquidity constrained, as proxied by worker earnings of less than $20,000 during the year prior to displacement.[33] Table 2.6 provides the results from this specification. Positive

---

[32] Another factor is if the worker believes that the shock is temporary, and that these firms will offer high paying jobs in the near future. If the worker believes that the shock is permanent, then he is more likely to switch industries.

[33] $20,000 corresponds to roughly the 25th percentile in the sample of displaced workers, and the 20th percentile in the sample of job stayers. I assume that other sources of income are constant. Income provided by the government, such as unemployment insurance, should be partially accounted for by the CBSA dummies in my empirical model to the extent that they are similar across individuals within a CBSA. I assume that other sources of non-labor income such as family transfers are the same on average between concentrated and non-concentrated industries.

effects remain for job loss during an expansion, but they are smaller in magnitude and are less persistent. For the first 18 months after job loss, the effects are significant, ranging from 4.8 to 14.8 percent of overall earnings losses. However, these effects become insignificant for the remainder of the period. The effects during a recession are significant and negative for the first year after job loss, but they are very small (less than 3.7 percent).

There is little evidence consistent with labor supply effects during a recession, but there is seemingly some evidence during an expansion, since some of the effects of concentrated industries are insignificant after 18 months. While workers could wait for better jobs during an expansion, these labor supply decisions will most likely affect earlier estimates rather than later ones, since workers are more likely to make these decisions shortly after job loss, and not two years after job loss. Therefore, I cannot definitively say that this new result is a product of removing labor supply effects. This result could simply be due to selection, since I am focusing on low-income workers who likely had not accumulated a lot of industry-specific human capital. Therefore, more rigorous econometric techniques are required to purge labor supply effects from my baseline analysis.

2.5.3.2 Labor Supply Decisions Made Before Job Loss, Firm Selection

In this section, I control for labor supply decisions made before and after job loss, as well as the firm's decision to displace certain workers over others.

For now, I express these labor supply decisions and firm decisions as:

$$\Delta_t Y_i = \beta_0 + \gamma'_{c(i)} + \gamma'_{q(i)} + \gamma'_{j(i)} + \beta_1 X_i + \alpha_1 D_i + \alpha_2 Conc_{c(i)j(i)} + \alpha_3 Conc_{c(i)j(i)} * E_{q(i)} + \alpha_4 D_i * E_{q(i)} + \lambda_1 D_i * Conc_{c(i)j(i)} + \lambda_2 D_i * Conc_{c(i)j(i)} * E_{q(i)} + u_i$$

$$\lambda_{expansion} = \lambda_1 + \lambda_2, \lambda_{recession} = \lambda_1$$

$$u_i = \varepsilon_{1i} + \varepsilon_{2i} + \varepsilon_{3i} + v_i$$

where ε_1 represents the threat to identification from the firm's decision to displace certain workers over others, ε_2 represents labor supply decisions made before job loss, and ε_3 represents labor supply decisions made after job loss. Finally, v_i is noise assumed to be uncorrelated with being employed in a concentrated industry ($Conc_{c(i)j(i)}$), being displaced (D_i), or being included in the sample during an expansion or a recession ($E_{q(i)}$). Formally, I test:

$$cov(\varepsilon_{ki}, D_i * Conc_{c(i)j(i)} \mid X_i, D_i, Conc_{c(i)j(i)}, Conc_{c(i)j(i)} * E_{q(i)}, D_i * E_{q(i)}) \neq 0$$

and

$$cov(\varepsilon_{ki}, D_i * Conc_{c(i)j(i)} * E_{q(i)} \mid X_i, D_i, Conc_{c(i)j(i)}, Conc_{c(i)j(i)} * E_{q(i)}, D_i * E_{q(i)}) \neq 0, k = 1, 2, 3.$$

I treat ε_{1i} and ε_{2i} as time-varying threats, even though job loss is a one-time occurrence for most of the displaced workers in my sample, and sorting to different areas occur prior to the start of my analysis. Displacement in general is a one-time event for most workers, but have clear time-varying implications, so firm decisions leading to displacement could also have time-varying implications. Since sorting can lead to different types of workers with varying characteristics and skillsets that could produce time-varying effects, it is important to control for this action as well.

My first potential threat (ε_1) is if displacement itself is related to industry concentration. For example, if firms are more likely to let go of skilled workers in concentrated industries relative to non-concentrated industries during an expansion, these workers are more likely to find a higher paying job, causing an upward bias in my $\lambda_{expansion}$ estimate. One way to glean the direction and magnitude of this selection mechanism is by assessing the number of co-workers who left the firm before the displaced worker lost his job. A higher incidence of co-workers voluntarily leaving prior to his job loss suggests that displaced workers were either less aware of potential job loss, or less desirable to firms in the vicinity such that they could not leave the firm until the firm chooses to lay him off. In both scenarios, these displaced workers should have higher earnings losses, since unexpected shocks are usually more detrimental for subsequent employment (Topel, 1986), or these workers are less desirable to potential employers. I therefore investigate whether displaced workers in concentrated industries had co-workers who were more likely to leave relative to other displaced workers. In Appendix Table 2.13, my dependent variable is the total number of co-workers who left via a voluntary job-to-job flow eight quarters before the displaced worker lost his job.[34] While there is on average a high number of co-workers who voluntarily leave the firm prior to the displaced worker's quarter of job loss (on average, 430 co-workers leave during a local shock and 310 co-workers leave during an idiosyncratic shock), none of the estimates are significant, indicating that

[34] I cannot observe voluntary job-to-job flows within the data. Therefore, I use job-to-job flows that involve a within quarter or adjacent quarter dominant job-to-job flow as a proxy. Hyatt and McEntarfer (2012) show that this measure is very pro-cyclical, which is an expected property from voluntary separations. I select two years prior to job loss rather than one year prior to job loss to avoid potential misclassification of these people as other displaced workers who were hired in another job immediately after being displaced.

displaced workers in concentrated industries were just as (un)aware of impending job loss as their counterparts in non-concentrated industries.[35] This result suggests that if firms select which workers to let go, the selection process does not vary by industry concentration status.

My second potential threat (ε_2) comes from the fact that prior to job loss, some workers may have moved to a local labor market with better employment or earnings opportunities, or to places that would better for their labor outcomes should job loss occur. Since there are large, positive returns to geographic sorting for some workers (Ham, 2011; Kennan and Walker, 2011), I could unintentionally capture effects from comparing people of different residential migration ability.

In order to purge the endogeneity of ε_1 and ε_2 from my initial estimates, I first limit the data to displaced workers who lost their jobs due to plant closings, since firms do not choose to keep any workers during plant closings. By doing so, I control for the endogeneity of ε_1. I then limit the data once again to those whom I consider to be "low migration ability" workers. By focusing on these workers, the estimated parameter of interest will more likely reflect the true effects from concentrated industries, since these workers are less likely to have sorted into the area and are thus more likely taking the conditions as a given. Empirically, it is very difficult for researchers to observe which workers have high migration ability and which workers do not. In order to approximate workers who cannot easily migrate across regions, I focus on workers who reside within their birth state, a common approximation used within the literature (e.g., Ham, 2011). However, staying in one's birth state is not an

[35] The fact that more co-workers left during an idiosyncratic shock further substantiates that I am more likely capturing voluntary job-to-job flows rather than other displaced co-workers who lost their jobs two years before the displaced worker of interest lost his.

exogenous act, so after restricting my sample to workers who are part of plant closings, I use a control function approach to control for the endogeneity associated with living in one's birth state. Formally, I estimate the following equation:

$$\Delta_t Y_i = \beta_0 + \gamma'_{q(i)} + \gamma'_{j(i)} + \beta_1 X_i + \beta_2 X_{c(i)} + \lambda_1 Conc_{c(i)j(i)} + \lambda_2 Conc_{c(i)j(i)} * E_{q(i)} + K(\hat{p}) + v_i \quad (E3)$$

$$\text{where } K(\hat{p}) \equiv \pi_1 \hat{p} + \pi_2 \hat{p}^2$$

$$\text{and } \lambda_{expansion} = \lambda_1 + \lambda_2, \lambda_{recession} = \lambda_1$$

The notation is the same as before, and $K(\hat{p})$ refers to the control function. I no longer include job stayers in my equation, since the sample is now restricted to displaced workers who are part of plant closings. An additional difference between equation $(E3)$ and equation $(E1)$ is that instead of having CBSA dummies ($\gamma'_{j(i)}$), I now include a set of covariates $X_{c(i)}$ with the CBSA's population interacting with the region of the worker's state of residence. This is because I use a nonlinear function to estimate $K(\hat{p})$; including too many dummy variables will give rise to an incidental parameters problem.[36]

To estimate the control function, after restricting my data to workers who were part of plant closings (and therefore removing job stayers), I estimate a probit model where the dependent variable is whether the worker lives within his/her birth state. The covariates in the probit model are all of the variables in equation $(E3)$ (with the exception of the $K(\hat{p})$ term) and the following additional variables: the number of children under 6, the number of children under 18, and a vector of

---

[36] The baseline estimates do not change by a significant amount when I use the CBSA characteristics as opposed to the original CBSA dummies, so any differences in the results is not due to this specification change.

dummies for the state s/he was born in.[37] I then estimate a linear and quadratic form of the propensity score, which form the $K(\hat{p})$ term in equation $(E3)$.

Unfortunately, I am unable to directly control for labor supply decisions made after job loss, since I have exhausted most of the variables in the datasets. I therefore assume that by controlling for labor supply decisions before job loss, I control for labor supply decisions made after job loss. For this analysis, this assumption is realistic, since the main labor supply channel after job loss is whether workers will voluntarily stay out of employment to wait for better jobs to become available. This decision is only feasible if the worker has sufficient liquidity. Residential moves also require considerable liquidity, so if I control for one supply decision, I can reasonably assume that I control for the other.

Table 2.7 shows the estimated effects from equation $(E3)$. The estimated effects are now insignificant, which implies that either all of my baseline estimates were driven by selection, or my current specification masks the effects I am interested in.[38] To explore the latter scenario, rather than using expansions and recessions, I use a more precise way of measuring local job availability. I use the Bartik Index (Bartik, 1991), which predicts the magnitude of a local labor market's demand shock by using national industry employment trends along with the local labor market's initial

[37] I assume that if the worker is residing with a person under 18 years old, then he/she is a guardian of the child. I use a linear and a squared term for the propensity score to approximate the true function of the selection.

[38] Although I do not present the results, my original specification is not sensitive to removing job stayers from my analysis, i.e., the results in Table 2.3 are driven primarily by displaced workers, and not job stayers. An alternative explanation as to why the results in Table 2.7 differ from Table 2.3 is the composition of workers in Table 2.7 is not representative of the original sample. Appendix Table 2.14 shows that the estimates for the specification without the propensity score inclusion are not significant. The estimates are significant when the sample is restricted to those who were part of plant closings, but lose significance when the sample is further restricted to those who living in their birth state.

industrial composition to classify what type of local demand shock the displaced worker's job loss was concurrent with. I assume that every displaced worker loses his/her job due to a negative demand shock that affected his/her original firm, but other firms in the vicinity could either be experiencing employment growth due to positive demand shocks, or employment losses due to negative demand shocks. I use the Bartik Index's value of a CBSA-year to classify the extent to which other firms were likely to hire a displaced worker at the time of job loss. I categorize each CBSA-year as having either a "large" negative local demand shock, a "medium" sized negative local demand shock, or a "small" negative local demand shock, with the last group including cases where the local labor market experienced a positive shock. For brevity, however, I classify both local labor markets with small local negative demand shocks and local labor markets with positive demand shocks as "small negative demand shocks".[39] After classifying CBSA-year combinations as having one

[39] I use the Bartik Index instead of total CBSA employment changes because the latter can be driven by local supply changes, whereas the Bartik Index usually captures local demand changes (Bound and Holtzer, 2000). Formally, the index is: $\widehat{gr_c} = \sum_j \gamma_{jc} gr_j$, where gr_j is the national employment growth within the six-digit industry j between two years of interest, and γ_{jc} is the share of employment for six-digit industry j within CBSA c during the year prior to the two years of interest. For a given year, I evaluate this index three separate times for the one-, two-, and four-year industry employment changes. Therefore, if a displaced worker loses his job in 2005, the one-year difference looks at the difference in employment from 2004 to 2005; the two-year difference, from 2003 to 2005, and the four-year difference, from 2001 to 2005. I use three separate differences to account for potential persistent effects a few years after the occurrence of a negative shock. To determine the magnitude of CBSA-specific shocks, I first calculate one-, two-, and four-year Bartik indices for each CBSA-year combination, for all years from 2004 to 2010. I then limit the data to the CBSA-years that had positive or negative Bartik index values for each of the one-, two- and four-year calculations for the next step in determining idiosyncratic or local shocks. I then calculate a distribution of the Bartik indices for just the CBSA-years that had positive or negative Bartik values. I then flag the CBSA-years as having a small shock if either the one-, two-, or four-year Bartik indices was larger than the median of the corresponding distribution, and a large shock if either of the indices was smaller than the median. I classify all other CBSA-years as medium. For the one year calculations, if the CBSA-year has a Bartik Index above 0.02, it is a small shock, while if it is below -0.035 it is a large shock. For the two year calculations, if the CBSA-year has a Bartik Index above 0.03, it is a small shock, while if it is below -0.065 it is a large shock. For the four year calculations, if the CBSA-year has a Bartik Index above 0.05, it is a small shock, while if it is below -0.11 it is a large shock.

of these three types of shocks based on the Bartik Instrument's value, I run the following specification:

$$\Delta_t Y_i = \beta_0 + \gamma'_{c(i)} + \gamma'_{q(i)} + \gamma'_{j(i)} + \beta_1 X_i + \alpha_1 S_{c(i)q(i)} + \alpha_2 L_{c(i)q(i)} + \lambda_1 Conc_{c(i)j(i)}$$
$$+ \lambda_2 Conc_{c(i)j(i)} * S_{c(i)q(i)} + \lambda_3 Conc_{c(i)j(i)} * L_{c(i)q(i)} + K(\hat{p})$$
$$+ v_i \quad (4)$$

$$K(\hat{p}) \equiv \pi_1 \hat{p} + \pi_2 \hat{p}^2$$

$$\lambda_{small} = \lambda_1 + \lambda_2, \lambda_{medium} = \lambda_1, \lambda_{large} = \lambda_1 + \lambda_3$$

where $S_{c(i)q(i)}$ is a dummy that equals 1 if the local shock is a small shock, and $L_{c(i)q(i)}$ is a dummy that equals 1 if it is a large shock; the omitted category is a medium shock. The estimates of interest are λ_{small}, λ_{medium}, and λ_{large}, which describe the earnings loss effects for displaced workers relative to job stayers by industry concentration when job loss is concurrent with a small, medium, or large negative demand shock, respectively. Table 2.8 presents the results using this specification, both with and without the propensity score used as a control. Without the control function, the effects of concentrated industries are large, positive, and significant during small local shocks, mitigating earnings losses by 7-12 percent. The effects are negligible during medium local shocks, but the most noticeable result is that earnings losses in concentrated industries are significantly and negatively exacerbated during large local shocks by roughly 6.5-8.5 percent, which is evidence that these workers' industry-specific human capital is actually a hindrance to earnings recovery, at least initially. However, given that the exacerbation effects of concentrated industries during large shocks disappear a year after job loss, this pattern is not too different from the effects of concentrated industries that occur during a recession as reported in my baseline specification. The effects remain significant after including the control function, which suggests that either selection plays a very small effect in this part of my analysis, or it was already handled by the first-differencing in my empirical model. These results show that by focusing on the selection effects within expansions and recessions, I hid some of the heterogeneity in the earnings loss mitigation effects by concentrated industries, since even within an expansion, some local labor markets experience large negative demand shocks (and conversely, some local labor markets experience employment growth during a recession). By focusing on local shocks, I more accurately portray the availability of jobs in the local labor market.

2.6 Conclusion

Displaced workers often struggle to find jobs that paid their initial salary after job loss, and a popular explanation is that this outcome is due to a loss of firm-specific human capital, which cannot be transferred across firms. I highlight the fact that the loss of industry-specific human capital is also an important driver of earnings losses, since when there are more firms in the same industry in the vicinity, the employment outcomes of displaced workers are not as severe in expansions, but may actually be more severe in recessions when firms are contractingimprove These results contribute to the extensive work done in the human capital transferability literature.

There is scope for future work focusing on the role of occupation or other factors that more closely resemble a worker's day-to-day activity. Pavan (2011) and Kamberouv and Manovskii (2009) both highlight the importance of occupation, but unfortunately, as mentioned above, the LEHD does not contain the same rich data on occupation as it does on industry. Future work can also analyze how input-output linkages affect displaced workers in a concentrated industry. If firm production is more dependent on other nearby firms due to product space relations, then it is likely that a negative demand shock in one firm will affect other firms. If there are some industry clusters with high input-output relations, then displaced workers within these clusters are unlikely to have similar outcomes to those in other types of clusters, since more jobs are likely to be destroyed in the former scenario.

References

Abowd, John M., Stephens, Bryce E., Vilhuber, Lars, Andersson, Fredrik, McKinney, Kevin L., Roemer, Marc, and Woodcock, Simon "The LEHD Infrastructure Files and the Creation of the Quarterly Workforce Indicators", *Producer Dynamics: New Evidence from Microdata*, 2009, 149-230.

Abraham, Katherine G., and Farber, Henry S. "Job Duration, Seniority, and Earnings." *The American Economic Review*, June 1987, 77, 278-97.

Altonji, Joseph G. and Shakotko, Robert A. "Do Wages Rise with Job Seniority", *Review of Economic Studies*, July 1987, 54, 437-459

Altonji, Joseph G. and Williams, Nicole "Do Wages Rise with Job Seniority? A Reassessment", *Industrial and Labor Relations Review*, April 2005, 58, 370-397

Bartik, Timothy J. "Who Benefits from State and Local Development Policies?", *W.E. Upjohn Institute for Employment Research*, 1991, 1-16

Becker, Gary S. "Investment in Human Capital: A Theoretical Analysis", *The Journal of Political Economy*, October 1962, 70, 9-49

Bound, John and Holzer, Harry J. "Demand Shifts, Population Adjustments, and Labor Market Outcomes during the 1980s", *Journal of Labor Economics*, January 2000, 18, 20-54

Blanchard, Olivier, and Katz, Lawrence "Regional Evolutions," *Brookings Papers on Economic Activity*, January 1992, 1, 1-76.

Couch, Kenneth A., and Placzek, Dana W. "Earnings Losses of Displaced Workers Revisited." *The American Economic Review*, March 2010, 100, 572-89

Delgado, Mercedes, Porter, Michael E. and Scott, Stern "Clusters, Convergence, and Economic Performance", *NBER Working Paper Series*, July 2012, Working Paper 18250

Delgado, Mercedes, Porter, Michael E. and Scott, Stern "Clusters and Entrepreneurship," *Journal of Economic Geography*, May 2010, 10, 495-518

Dustmann, Christian and Meghir, Costas "Wages, Experience and Seniority", *Review of Economic Studies*, February 2005, 72, 77–108

Ellison, Glenn, Glaeser, Edward L., Kerr, William R. "What Causes Industry Agglomeration? Evidence from Coagglomeration Patterns", *The American Economic Review*, June 2010, 100, 1195-1213

Freedman, Matthew L. "Job Hopping, Earnings Dynamics, and Industrial Agglomeration in the Software Publishing Industry", *Journal of Urban Economics*, November 2008, 64, 590-600

Gathmann, Christina and Schönberg, Uta "How General Is Human Capital? A Task-Based Approach", *Journal of Labor Economics*, January 2010, 28, 1-49

Jacobson, Louis S., LaLonde, Robert J. and Sullivan, Daniel G. "Earnings Losses of Displaced Workers", *The American Economic Review*, September 1993, 83, 685-709

Jovanovic, Boyan "Job Matching and the Theory of Turnover", *The Journal of Political Economy*, October 1979, 87, 972-990.

Kambourov, Gueorgui and Manovskii, Iourii "Occupational Specificity of Human Capital", *International Economic Review*, February 2009, 50, 63-115.

Kaplan, Greg and Schulhofer-Wohl, Sam "Understanding the Long-Run Decline in Interstate Migration", *NBER Working Paper Series*, November 2012, Working Paper 18507

Lazear, Edward P. "Agency, Earnings Profiles, Productivity, and Hours Restrictions," *The American Economic Review*, September 1981, 71, 606-620.

Lazear, Edward P. "Firm-specific human capital: A skill-weights approach" *Journal of Political Economy*, October 2009, 117, 914–40.

Ljungqvist, Lars and Sargent, Thomas J. "The European Unemployment Dilemma", *Journal of Political Economy*, June 1998, 106, 514-550

Moretti, Enrico "Workers' Education, Spillovers, and Productivity: Evidence from Plant-Level Production Functions", *The American Economic Review*, June 2004, 94, 656-690

Molloy, Raven, Smith, Christopher L. and Wozniak, Abigail "Internal Migration in the United States" *Journal of Economic Perspectives*, June 2011, 25, 173-196

Neal, Derek "Industry-Specific Human Capital: Evidence from Displaced Workers", *Journal of Labor Economics*, October 1995, 13, 653-677

Parent, Daniel "Industry-Specific Capital and the Wage Profile: Evidence from the National Longitudinal Survey of Youth and the Panel Study of Income Dynamics", *Journal of Labor Economics*, April 2000, 18, 306-323

Pavan, Ronni "Career Choice and Wage Growth", *Journal of Labor Economics*, July 2011, 29, 549-587

Porter, Michael E., "Clusters and Competition: New Agendas for Companies, Governments, and Institutions." *Harvard Business School Press*, March 1998, pp. 197-299.

Schmieder, Johannes F., von Wachter, Till, and Bender, Stefan "The Long Term Impact of Job Displacement in Germany during the 1982 Recession on Earnings, Income, and Employment," *Columbia University Working Paper*, November 2009

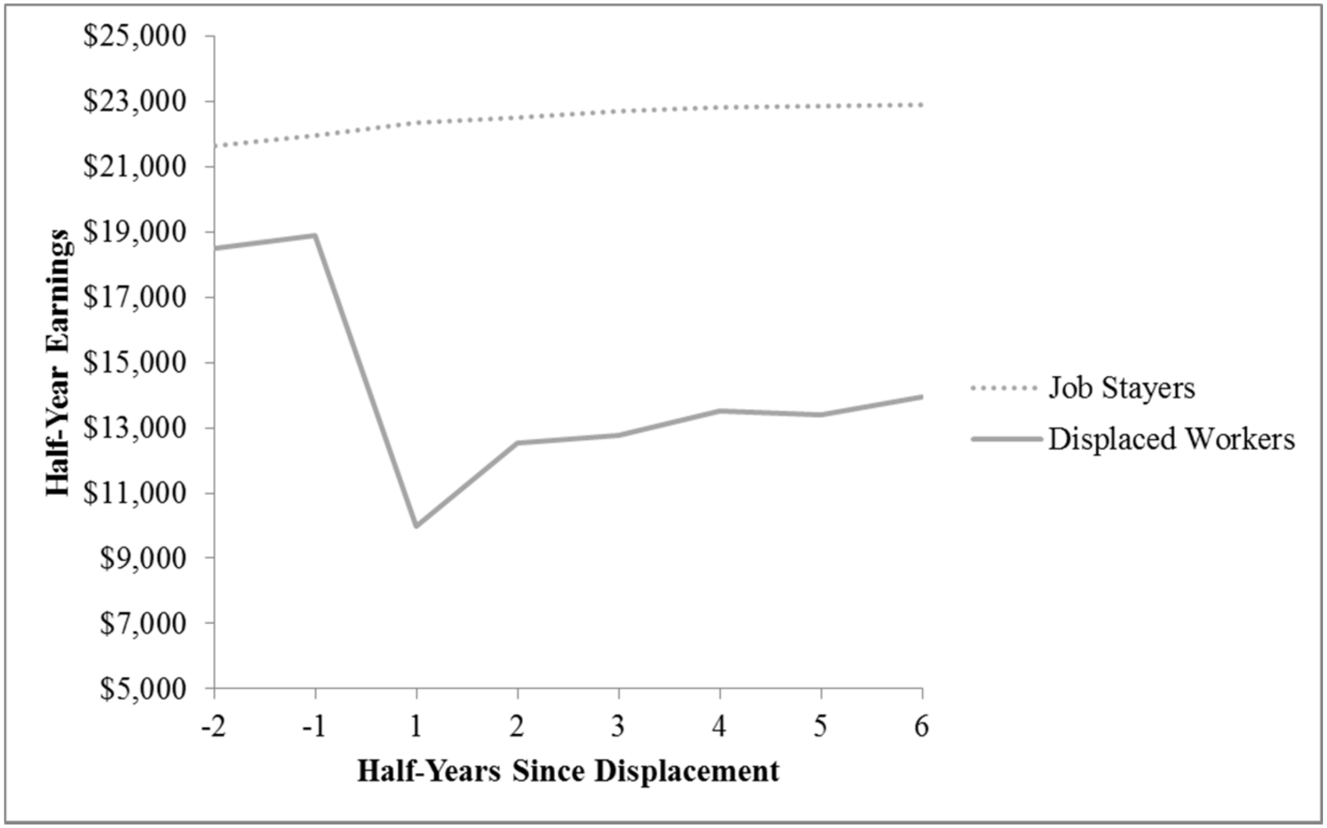
Topel, Robert H. "Local Labor Markets", *Journal of Political Economy*, June 1986, 94, 111-143

Topel, Robert H. "Specific Capital, Mobility, and Wages: Wages Rise with Job Seniority", *The Journal of Political Economy*, February 1991, 99, 145-176.

von Wachter, Till, Song, Jae, and Manchester, Joyce "Long-Term Earnings Losses due to Mass Layoffs During the 1982 Recession: An Analysis Using U.S. Administrative Data from 1974 to 2004", *Columbia University Working Paper*, April 2009

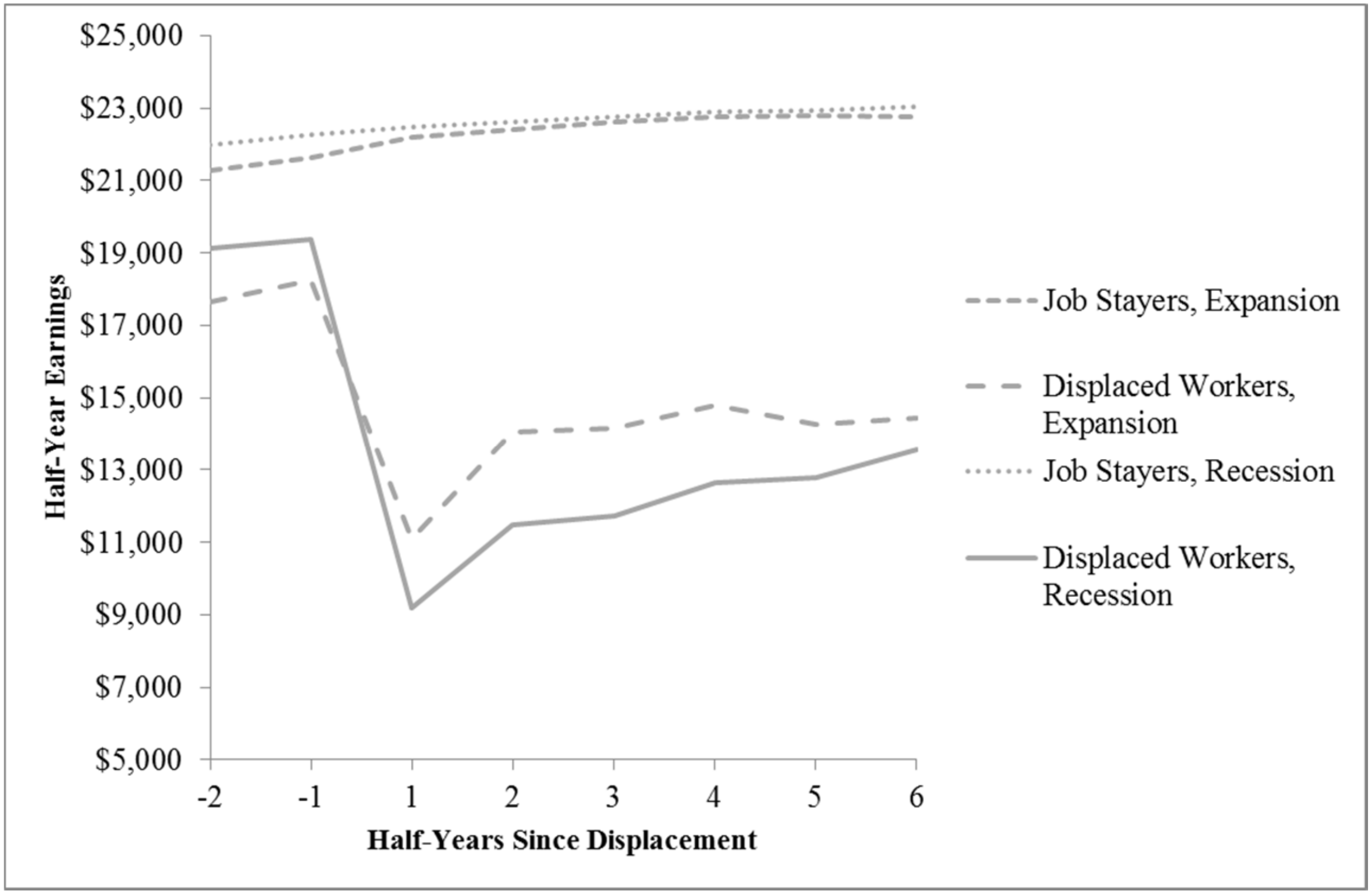
Figures

Figure 2.1: Earnings Evolution for Displaced Workers and Job Stayers



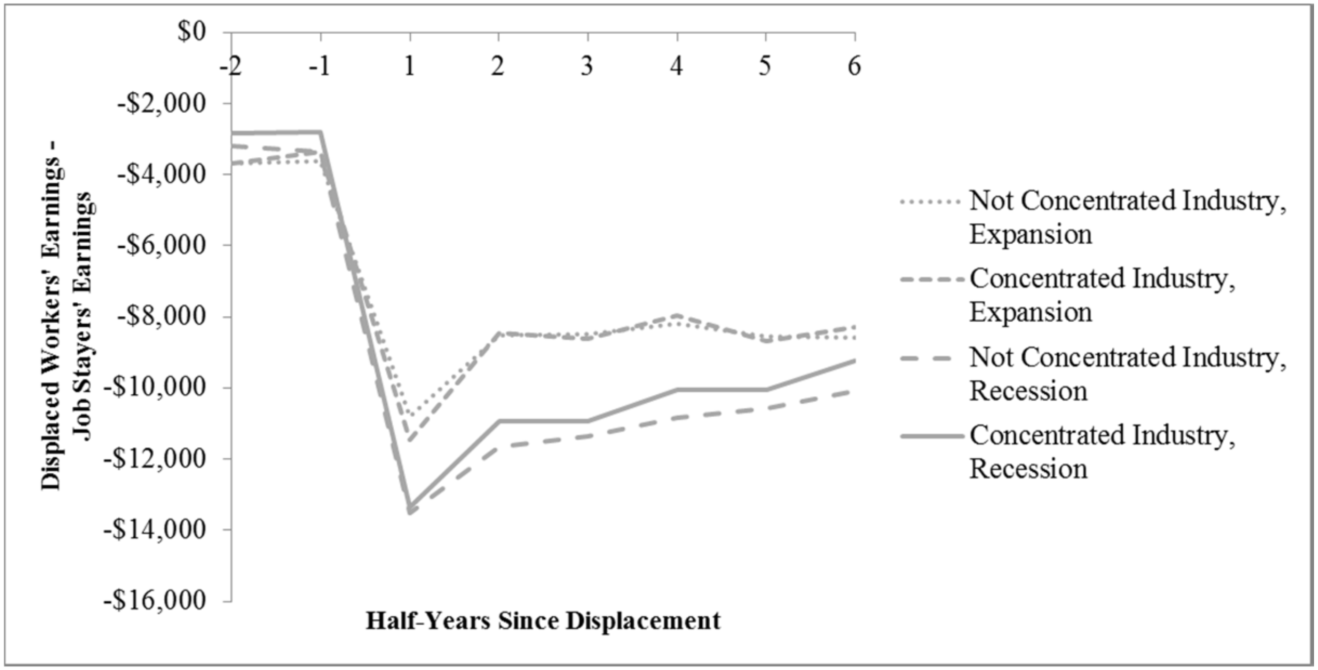
Note: The displaced worker sample is the full set of U.S. displaced workers with at least five quarters of tenure for states that submitted worker earnings to the LEHD from 2005 to 2012. The job stayer sample is a one percent sample of workers who met the following criteria: (i) They had five quarters of tenure as of any quarter from 2005 to 2012; (ii) They then proceeded to have 12 consecutive quarters of tenure at the same firm after that quarter. I use earnings from all of the worker's jobs. Earnings are adjusted to 2011 dollars using an internal CPI deflator.

Figure 2.2: Earnings Evolution for Displaced Workers and Job Stayers: Business Cycle



Note: The displaced worker sample is the full set of U.S. displaced workers with at least five quarters of tenure for states that submitted worker earnings to the LEHD from 2005 to 2012. The job stayer sample is a one percent sample of workers who met the following criteria: (i) They had five quarters of tenure as of any quarter from 2005 to 2012; (ii) They then proceeded to have 12 consecutive quarters of tenure at the same firm after that quarter. The "Displaced Workers, Expansion" category consists of displaced workers who lost their jobs during any quarter from 2005 Q1 to 2006 Q4. The "Job Stayers, Expansion" category consists of displaced workers who met the job stayer requirements during any quarter from 2005 Q1 to 2006 Q4. The "Displaced Workers, Recession" category consists of displaced workers who lost their jobs during any quarter from 2007 Q4 to 2009 Q2. The "Job Stayers, Recession" category consists of displaced workers who met the job stayer requirements during any quarter from 2007 Q4 to 2009 Q2.

Figure 2.3: Earnings Evolution for Displaced Workers Relative to Job Stayers: Business Cycle and Concentrated Industry Status



Note: The displaced worker sample is the full set of U.S. displaced workers with at least five quarters of tenure for states that submitted worker earnings to the LEHD from 2005 to 2012. The job stayer sample is a one percent sample of workers who met the following criteria: (i) They had five quarters of tenure as of any quarter from 2005 to 2012; (ii) They then proceeded to have 12 consecutive quarters of tenure at the same firm after that quarter. "Concentrated industry" is defined based on the employment share of the displaced worker's pre-displacement industry. The y-axis shows the difference in the average earnings of the displaced workers relative to job stayers.

Tables

Table 2.1: Demographic and Work History Characteristics of Displaced Workers

	All		Expansion		Recession	
	Concentrated	Not Concentrated	Concentrated	Not Concentrated	Concentrated	Not Concentrated
Demographic Characteristics						
Hispanic	24%	21%	24%	20%	23%	22%
White, Not Hispanic	54%	61%	54%	61%	55%	60%
Black, Not Hispanic	14%	12%	14%	12%	14%	11%
Asian, Not Hispanic	6%	4%	5%	4%	6%	4%
Other, Not Hispanic	2%	2%	2%	1%	2%	2%
Male	58%	56%	55%	51%	60%	59%
Age	42	42	41	42	42	42
Work History						
Experience	10.8	11.0	10.0	10.1	11.4	11.6
Industry Experience	4.6	5.1	4.4	4.8	4.8	5.3
Tenure	2.4	2.7	2.2	2.5	2.5	2.8
Count	1,132,000	1,577,000	462,000	672,000	671,000	905,000

Notes: The sample is the full set of U.S. displaced workers with at least five quarters of tenure for states that submitted worker earnings to the LEHD from 2005 to 2012. Experience is defined as the total number of quarters in which the agent had any earnings. Industry experience is defined as the total number of quarters in which the agent had any earnings in the same industry as his pre-displacement industry. Tenure is defined as the total number of consecutive quarters at the firm he was displaced from. The percentages in the Hispanic, White, Not Hispanic, etc. rows represent the sample share in the respective columns.

Table 2.2: Firm and CBSA Characteristics of Displaced Workers

	All		Expansion		Recession	
	Concentrated	Not Concentrated	Concentrated	Not Concentrated	Concentrated	Not Concentrated
Firm Size, Firm Age Characteristics						
Firm size > 499	43%	42%	43%	43%	42%	41%
Firm age > 3yrs	90%	91%	89%	91%	91%	92%
Supersector Characteristics						
Construction	15%	15%	13%	13%	16%	17%
Manufacturing	10%	18%	8%	15%	11%	20%
Trade, Transportation, and Utilities	13%	17%	13%	18%	13%	17%
Information	3%	2%	3%	2%	3%	2%
Financial Activities	9%	3%	9%	2%	8%	3%
Professional and business services	36%	6%	35%	6%	36%	7%
Education and health services	4%	24%	6%	28%	3%	20%
Leisure and hospitality	4%	10%	5%	12%	4%	9%
Other	6%	5%	7%	5%	5%	5%
CBSA Characteristics						
Total People	3.8 Million	3.9 Million	4.0 Million	4.0 Million	3.7 Million	3.8 Million
Count	1,132,000	1,577,000	462,000	672,000	671,000	905,000

Notes: The sample is the full set of U.S. displaced workers with at least five quarters of tenure for states that submitted worker earnings to the LEHD from 2005 to 2012. Firm size, firm age, and supersector are determined by the firm at which the worker was displaced. Average CBSA population size is determined by first attributing the CBSA of residency to each agent at the time of displacement, then determining the population size of the CBSA during that time, and then taking the sample average of the population size across all workers within the respective categories.

Table 2.3: Earnings Change Regression Estimates for Displaced Workers: Effects of Concentrated Industries Within The Business Cycle

	$\lambda_{Expansion}$	$\lambda_{Recession}$
0-6 Months After Job Loss	$476***	-$69
	($135)	($146)
Unconditional Earnings Losses	*-$7,734*	*-$10,389*
6-12 Months After Job Loss	$404***	-$101
	($110)	($125)
Unconditional Earnings Losses	*-$5,004*	*-$8,225*
12-18 Months After Job Loss	$529***	$96
	($118)	($141)
Unconditional Earnings Losses	*-$5,100*	*-$8,104*
18-24 Months After Job Loss	$448***	$21
	($106)	($135)
Unconditional Earnings Losses	*-$4,598*	*-$7,360*
24-30 Months After Job Loss	$612***	$129
	($122)	($133)
Unconditional Earnings Losses	*-$5,170*	*-$7,247*
30-36 Months After Job Loss	$496***	-$1
	($121)	($120)
Unconditional Earnings Losses	*-$4,947*	*-$6,563*

Note: The displaced worker sample is the full set of U.S. displaced workers with at least five quarters of tenure for states that submitted worker earnings to the LEHD from 2005 to 2012. The job stayer sample is a one percent sample of workers who met the following criteria: (i) They had five quarters of tenure as of any quarter from 2005 to 2012; (ii) They then proceeded to have 12 consecutive quarters of tenure at the same firm after that quarter. The outcome variable for the first three rows is the change in earnings defined as total earnings six months after job loss minus total earnings six months prior to job loss. The outcome for the next three rows is total earnings twelve months after job loss minus total earnings six months prior to job loss, and so on. Unconditional earnings losses is the difference in the average earnings changes of the displaced workers relative to job stayers. $\lambda_{\text{Expansion}}$ is defined as $\widehat{\lambda_1 + \lambda_2}$ from equation $(E1)$, while $\lambda_{\text{Recession}}$ is defined as $\widehat{\lambda_1}$ from the same equation. *** represents significance at the 1 percent level; ** represents significance at the 5 percent level; * represents significance at the 10 percent level.

Table 2.4: Employment Regression Estimates for Displaced Workers: Effects of Concentrated Industries Within The Business Cycle

	Employment		Same Industry Employment	
	(1)	(2)	(3)	(4)
	$\lambda_{Expansion}$	$\lambda_{Recession}$	$\lambda_{Expansion}$	$\lambda_{Recession}$
0-6 Months After Job Loss	0.021***	-0.010**	0.034***	0.005
	(0.004)	(0.004)	(0.005)	(0.005)
Unconditional Employment	*0.754*	*0.643*	*0.432*	*0.369*
6-12 Months After Job Loss	0.019***	-0.004	0.038***	0.015***
	(0.003)	(0.004)	(0.006)	(0.005)
Unconditional Employment	*0.811*	*0.695*	*0.434*	*0.375*
12-18 Months After Job Loss	0.019***	0.000	0.035***	0.018***
	(0.003)	(0.003)	(0.005)	(0.005)
Unconditional Employment	*0.810*	*0.708*	*0.402*	*0.352*
18-24 Months After Job Loss	0.017***	0.003	0.033***	0.020***
	(0.003)	(0.003)	(0.005)	(0.005)
Unconditional Employment	*0.807*	*0.726*	*0.383*	*0.343*
24-30 Months After Job Loss	0.017***	0.007***	0.033***	0.026***
	(0.003)	(0.003)	(0.005)	(0.005)
Unconditional Employment	*0.792*	*0.737*	*0.359*	*0.329*
30-36 Months After Job Loss	0.016***	0.009***	0.033***	0.028***
	(0.003)	(0.002)	(0.005)	(0.004)
Unconditional Employment	*0.775*	*0.743*	*0.343*	*0.322*

Notes: The sample is the full set of U.S. displaced workers with at least five quarters of tenure for states that submitted worker earnings to the LEHD from 2005 to 2012. Columns (1) and (2) use any employment as the dependent variable. Columns (3) and (4) use "same industry employment" as the dependent variable, which is a dummy equaling 1 if the displaced worker is re-employed in the same industry as his pre-displacement industry, and 0 otherwise. $\lambda_{\text{Expansion}}$ is defined as $\widehat{\lambda_1 + \lambda_2}$ from equation $(E1)$, while $\lambda_{\text{Recession}}$ is defined as $\widehat{\lambda_1}$ from the same equation. *** represents significance at the 1 percent level; ** represents significance at the 5 percent level; * represents significance at the 10 percent level.

Table 2.5: Earnings Loss Effects by Human Capital Accumulation

	Expansion			Recession		
	(1)	(2)	(3)	(4)	(5)	(6)
	$\lambda_{\text{ind exp*Disp*Conc}}$	$\lambda_{\text{exp*Disp*Conc}}$	$\lambda_{\text{tenure*Disp*Conc}}$	$\lambda_{\text{ind exp*Disp*Conc}}$	$\lambda_{\text{exp*Disp*Conc}}$	$\lambda_{\text{tenure*Disp*Conc}}$
0-6 Months After Job Loss	$38***	$4	-$56***	$23***	$7**	-$39**
	($10)	($6)	($14)	($5)	($3)	($15)
6-12 Months After Job Loss	$23***	-$5	-$26***	$6	$6**	-$34***
	($5)	($4)	($9)	($5)	($3)	($12)
12-18 Months After Job Loss	$31***	-$5	-$27***	$12	$6*	-$30***
	($5)	($4)	($8)	($6)	($3)	($12)
18-24 Months After Job Loss	$26***	-$1	-$32***	$7	$8**	-$28**
	($5)	($4)	($8)	($6)	($3)	($11)
24-30 Months After Job Loss	$27***	-$1	-$26***	$10	$8**	-$23**
	($6)	($4)	($8)	($6)	($3)	($10)
30-36 Months After Job Loss	$24***	$1	-$32***	$7	$9***	-$25**
	($6)	($4)	($8)	($6)	($3)	($10)

Note: The displaced worker sample is the full set of U.S. displaced workers with at least five quarters of tenure for states that submitted worker earnings to the LEHD from 2005 to 2012. The job stayer sample is a one percent sample of workers who met the following criteria: (i) They had five quarters of tenure as of any quarter from 2005 to 2012; (ii) They then proceeded to have 12 consecutive quarters of tenure at the same firm after that quarter. The dependent variable is change in earnings. The estimates are the output from equation ($E2$). Columns (1)-(3) contain the estimates when estimating equation ($E2$) during an expansion, while Columns (4)-(6) contain the estimates when estimating equation ($E2$) during a recession.

Table 2.6: The Effects of Concentrated Industries on Change in Earnings for Low-Income Workers

	$\lambda_{Expansion}$	$\lambda_{Recession}$
0-6 Months After Job Loss	$133***	-$114**
	($59)	($51)
Unconditional Earnings Losses	*-$2,799*	*-$3,543*
6-12 Months After Job Loss	$238***	-$99*
	($66)	($59)
Unconditional Earnings Losses	*-$1,610*	*-$2,737*
12-18 Months After Job Loss	$154**	-$62
	($74)	($63)
Unconditional Earnings Losses	*-$1,773*	*-$2,893*
18-24 Months After Job Loss	$92	-$80
	($77)	($66)
Unconditional Earnings Losses	*-$1,381*	*-$2,519*
24-30 Months After Job Loss	-$23	-$45
	($83)	($68)
Unconditional Earnings Losses	*-$1,783*	*-$2,672*
30-36 Months After Job Loss	-$11	$19
	($86)	($75)
Unconditional Earnings Losses	*-$1,520*	*-$2,305*

Notes: The displaced worker sample is the full set of U.S. displaced workers with at least five quarters of tenure and who earned less than $20,000 prior to job loss for states that submitted worker earnings to the LEHD from 2005 to 2012. The job stayer sample is a one percent sample of workers who met the following criteria: (i) They had five quarters of tenure as of any quarter from 2005 to 2012; (ii) They then proceeded to have 12 consecutive quarters of tenure at the same firm after that quarter; (iii) They had annual incomes of less than $20,000 prior to the quarter of interest.

Table 2.7: The Effects of Concentrated Industries on Change in Earnings (Control Function Approach)

	$\lambda_{Expansion}$	$\lambda_{Recession}$
0-6 Months After Job Loss	\$290	-\$235
	(\$224)	(\$216)
Unconditional Earnings Losses	*-\$8,084*	*-\$10,739*
6-12 Months After Job Loss	\$293	-\$178
	(\$201)	(\$200)
Unconditional Earnings Losses	*-\$4,933*	*-\$8,817*
12-18 Months After Job Loss	\$219	-\$40
	(\$200)	(\$217)
Unconditional Earnings Losses	*-\$4,276*	*-\$8,189*
18-24 Months After Job Loss	\$223	-\$26
	(\$179)	(\$197)
Unconditional Earnings Losses	*-\$4,031*	*-\$7,420*
24-30 Months After Job Loss	\$248	\$108
	(\$186)	(\$180)
Unconditional Earnings Losses	*-\$4,122*	*-\$6,846*
30-36 Months After Job Loss	\$132	\$131
	(\$182)	(\$199)
Unconditional Earnings Losses	*-\$4,273*	*-\$6,260*

Notes: The sample is the full set of U.S. displaced workers: (i) with at least five quarters of tenure; (ii) who are part of a company shut-down; (iii) who resided in the same state as the state of birth during job loss; (iv) who resided in a state that submitted worker earnings to the LEHD from 2005 to 2012. A linear and qudratic propensity score is included in the model. The dependent variable in the propensity score estimate is whether the displaced worker resided in the same state as the state of birth. The excluded instruments for that stage are state of birth and the number of children under six and under 18. The first stage is estimated via a probit model. $\lambda_{Expansion}$ and $\lambda_{Recession}$ are defined in empirical equation ($E3$).

Table 2.8: The Effects of Concentrated Industries on Change in Earnings by the Severity of Local Shocks (Control Function Approach)

	Plant closers and reside within the same state			Plant closers and reside within the same state - Control Function		
	(1)	(2)	(3)	(4)	(5)	(6)
	λ_{small}	λ_{medium}	λ_{large}	λ_{small}	λ_{medium}	λ_{large}
0-6 Months After Job Loss	$594**	-$256	-$698***	$582**	-$275	-$723***
	($269)	($235)	($279)	($267)	($236)	($279)
Unconditional Earnings Losses	*-$8,516*	*-$9,752*	*-$12,003*	*-$8,516*	*-$9,752*	*-$12,003*
6-12 Months After Job Loss	$552***	-$180	-$583**	$541***	-$196	-$604**
	($181)	($236)	($298)	($180)	($236)	($296)
Unconditional Earnings Losses	*-$5,347*	*-$7,785*	*-$10,053*	*-$5,347*	*-$7,785*	*-$10,053*
12-18 Months After Job Loss	$603***	-$82	-$654*	$582***	-$109	-$689**
	($186)	($232)	($346)	($184)	($231)	($344)
Unconditional Earnings Losses	*-$4,993*	*-$7,365*	*-$8,032*	*-$4,993*	*-$7,365*	*-$8,032*
18-24 Months After Job Loss	$519***	-$72	-$448	$497***	-$97	-$480
	($174)	($210)	($306)	($173)	($210)	($304)
Unconditional Earnings Losses	*-$4,704*	*-$6,627*	*-$7,375*	*-$4,704*	*-$6,627*	*-$7,375*
24-30 Months After Job Loss	$579***	$46	-$386	$557***	$24	-$408*
	($192)	($196)	($239)	($191)	($196)	($238)
Unconditional Earnings Losses	*-$4,834*	*-$6,354*	*-$5,944*	*-$4,834*	*-$6,354*	*-$5,944*
30-36 Months After Job Loss	$578***	-$69	-$269	$551***	-$96	-$299
	($206)	($193)	($323)	($204)	($193)	($323)
Unconditional Earnings Losses	*-$4,791*	*-$5,858*	*-$5,731*	*-$4,791*	*-$5,858*	*-$5,731*

Notes: The sample is the full set of U.S. displaced workers: (i) with at least five quarters of tenure; (ii) who are part of a company shut-down; (iii) who resided in the same state as the state of birth during job loss; (iv) who resided in a state that submitted worker earnings to the LEHD from 2005 to 2012. Columns (1)-(3) is the output from equation (4), excluding the propensity score. Column (1) is $\widehat{\lambda_1 + \lambda_2}$, Column (2) is $\widehat{\lambda_1}$, and Column (3) is $\widehat{\lambda_1 + \lambda_3}$. A linear and quadratic propensity score is included in the model. The dependent variable in the first stage is whether the displaced worker resided in the same state as the state of birth. The excluded instruments for that stage are state of birth and the number of children under six and under 18. This is estimated via a probit model. λ_{snall} , λ_{medium} , λ_{large} are defined in equation (4).

Appendix: Tables

Appendix Table 2.1: Evolution of Concentrated and Non-Concentrated Industries by Year

	Concentrated	Non-Concentrated
# in 2004 Quarter 1	4,796	9,118
% Concentrated in 2005	85%	7%
% Concentrated in 2008	80%	9%

Notes: The unit in this table is CBSA-industry combinations. The second row refers to the percent of CBSA-industries that were concentrated in 2005 given their 2004 status. The third row refers to the percent of CBSA-industries that were concentrated in 2008 given their 2007 status.

Appendix Table 2.2: Demographic and Work History Characteristics of Job Stayers

	All		Expansion		Recession	
	Concentrated	Not Concentrated	Concentrated	Not Concentrated	Concentrated	Not Concentrated
			Demographic Characteristics			
Hispanic	16%	16%	15%	16%	17%	17%
White, Not Hispanic	68%	66%	69%	67%	67%	65%
Black, Not Hispanic	9%	11%	9%	10%	9%	11%
Asian, Not Hispanic	6%	6%	5%	5%	6%	6%
Other, Not Hispanic	1%	1%	1%	1%	1%	1%
Male	49%	45%	49%	45%	49%	45%
Age	46	46	46	47	46	46
			Work History			
Experience	12.1	12.1	11.3	11.3	12.9	12.8
Industry Experience	7.8	8.0	7.5	7.7	8.1	8.3
Tenure	2.4	5.4	2.2	5.3	2.5	5.6
Count	299,000	606,000	148,000	299,000	152,000	307,000

Notes: The sample is the one percent sample of U.S. job stayers for states that submitted worker earnings to the LEHD from 2005 to 2012. These workers must have 17 quarters of tenure—four quarters before the quarter of interest, the quarter of interest, and 12 quarters after the quarter of interest. Experience is defined as the total number of quarters in which the agent had positive earnings. Industry experience is defined as the total number of quarters where the agent had positive earnings in the same industry as his pre-displacement industry. Tenure is defined as the total number of consecutive quarters at the firm he was displaced from. The percentages in the Hispanic, White, Not Hispanic, etc. rows represent the sample share within the respective columns.

Appendix Table 2.3: Firm and CBSA Characteristics of Job Stayers

	All		Expansion		Recession	
	Concentrated	Not Concentrated	Concentrated	Not Concentrated	Concentrated	Not Concentrated
			Firm Size, Firm Age Characteristics			
Firm size > 499	53%	59%	52%	58%	54%	59%
Firm age > 3yrs	96%	97%	96%	97%	96%	97%
			Supersector Characteristics			
Construction	5%	4%	5%	4%	4%	3%
Manufacturing	11%	13%	11%	13%	11%	12%
Trade, Transportation, and Utilities	20%	21%	19%	20%	20%	21%
Information	3%	2%	3%	2%	4%	2%
Financial Activities	12%	3%	12%	3%	12%	3%
Professional and business services	21%	3%	21%	3%	22%	3%
Education and health services	13%	36%	13%	36%	13%	36%
Leisure and hospitality	4%	7%	4%	7%	4%	7%
Other	11%	13%	11%	13%	11%	12%
			CBSA Characteristics			
Total People	3.5 Million	4.0 Million	3.4 Million	4.0 Million	3.5 Million	4.0 Million
Count	299,000	606,000	148,000	299,000	152,000	307,000

Notes: The sample is the one percent sample of U.S. job stayers for states that submitted worker earnings to the LEHD from 2005 to 2012. These workers must have 17 quarters of tenure—four quarters before the quarter of interest, the quarter of interest, and 12 quarters after the quarter of interest. Firm size, firm age, and supersector are determined by the worker's firm. Average CBSA population size is determined by first attributing the CBSA of residency to each agent at the time of displacement, then determining the population size of the CBSA during that time, and then taking the sample average of the population size across all workers within the respective categories.

Appendix Table 2.4: The Effects of Concentrated Industries on Change in Earnings (Prior to Displacement)

	$\lambda_{Expansion}$	$\lambda_{Recession}$
$\Delta Y_{8\text{ hf-yrs before, 7 hf-yrs before}}$, PE	-$28	-$46
	($63)	($69)
$\Delta Y_{7\text{ hf-yrs before, 6 hf-yrs before}}$, PE	-$55	$61
	($64)	($71)
$\Delta Y_{6\text{ hf-yrs before, 5 hf-yrs before}}$, PE	-$10	-$221***
	($65)	($58)
$\Delta Y_{5\text{ hf-yrs before, 4 hf-yrs before}}$, PE	$4	$190***
	($66)	($57)
$\Delta Y_{4\text{ hf-yrs before, 3 hf-yrs before}}$, PE	$35	-$43
	($72)	($53)
$\Delta Y_{3\text{ hf-yrs before, 2 hf-yrs before}}$, PE	$72	$119**
	($67)	($56)
$\Delta Y_{2\text{ hf-yrs before, 1 hf-yrs before}}$, PE	-$83	-$127***
	($57)	($49)

Note: The displaced worker sample is the full set of U.S. displaced workers with at least five quarters of tenure for states that submitted worker earnings to the LEHD from 2005 to 2012. The job stayer sample is a one percent sample of workers who met the following criteria: (i) They had five quarters of tenure as of any quarter from 2005 to 2012; (ii) They then proceeded to have 12 consecutive quarters of tenure at the same firm after that quarter. The dependent variable is change in earnings. This is the output from equation $(E1)$, using earnings change prior to job loss as the dependent variable. The first two rows report the point estimate and standard error for the earnings change seven half-years before displacement relative to eight half-years; the next two rows report the point estimate and standard error for the earnings change six half-years before displacement relative to seven half-years, and so on.

Appendix Table 2.5: The Effects of Concentrated Industries (Removing Recalls)

	$\lambda_{Expansion}$	$\lambda_{Recession}$
0-6 Months After Job Loss	\$442***	-\$105
	(\$126)	(\$140)
Unconditional Earnings Losses	*-\$7,082*	*-\$9,917*
6-12 Months After Job Loss	\$397***	\$2
	(\$107)	(\$129)
Unconditional Earnings Losses	*-\$5,302*	*-\$8,645*
12-18 Months After Job Loss	\$443***	\$98
	(\$112)	(\$139)
Unconditional Earnings Losses	*-\$5,139*	*-\$8,310*
18-24 Months After Job Loss	\$389***	\$87
	(\$103)	(\$136)
Unconditional Earnings Losses	*-\$4,829*	*-\$7,736*
24-30 Months After Job Loss	\$507***	\$119
	(\$115)	(\$130)
Unconditional Earnings Losses	*-\$5,162*	*-\$7,365*
30-36 Months After Job Loss	\$450***	\$59
	(\$115)	(\$122)
Unconditional Earnings Losses	*-\$5,145*	*-\$6,870*

Notes: The sample is the same as Table 2.3, but displaced workers who were recalled to their original jobs are removed from the sample. These are workers whose first job after job loss is the firm they were displaced from.

Appendix Table 2.6: The Effects of Concentrated Industries (Same Firm Restriction)

	$\lambda_{Expansion}$	$\lambda_{Recession}$
0-6 Months After Job Loss	\$525***	-\$1
	(\$190)	(\$179)
Unconditional Earnings Losses	*-\$7,421*	*-\$10,232*
6-12 Months After Job Loss	\$596***	\$52
	(\$160)	(\$158)
Unconditional Earnings Losses	*-\$4,700*	*-\$8,232*
12-18 Months After Job Loss	\$584***	\$27
	(\$167)	(\$171)
Unconditional Earnings Losses	*-\$4,784*	*-\$7,898*
18-24 Months After Job Loss	\$694***	\$18
	(\$154)	(\$168)
Unconditional Earnings Losses	*-\$4,327*	*-\$7,319*
24-30 Months After Job Loss	\$809***	-\$30
	(\$173)	(\$164)
Unconditional Earnings Losses	*-\$4,959*	*-\$7,020*
30-36 Months After Job Loss	\$703***	-\$139
	(\$172)	(\$158)
Unconditional Earnings Losses	*-\$4,762*	*-\$6,503*

Notes: The sample is the same as Table 2.3, but displaced workers are restricted to those from firms with at least one job stayer in his firm during that quarter. Job stayers are restricted to those from firms with at least one displaced worker during the quarter of his sample eligibility.

Appendix Table 2.7: The Effects of Concentrated Industries (Only Low Values for Concentrated Status)

	$\lambda_{Expansion}$	$\lambda_{Recession}$
0-6 Months After Job Loss	\$153	\$230
	(\$220)	(\$228)
Unconditional Earnings Losses	*-\$8,037*	*-\$10,464*
6-12 Months After Job Loss	\$479***	\$186
	(\$180)	(\$197)
Unconditional Earnings Losses	*-\$5,082*	*-\$8,137*
12-18 Months After Job Loss	\$630***	\$321
	(\$188)	(\$189)
Unconditional Earnings Losses	*-\$5,212*	*-\$8,110*
18-24 Months After Job Loss	\$526***	\$125
	(\$159)	(\$174)
Unconditional Earnings Losses	*-\$4,605*	*-\$7,282*
24-30 Months After Job Loss	\$750***	\$133
	(\$178)	(\$159)
Unconditional Earnings Losses	*-\$5,239*	*-\$7,251*
30-36 Months After Job Loss	\$646***	-\$1
	(\$150)	(\$151)
Unconditional Earnings Losses	*-\$4,905*	*-\$6,485*

Notes: The sample is the same as Table 2.3, except displaced workers and job stayers are taken out of the sample if: (i) they are in a concentrated industry; and (ii) the industry employment share is above two percent of the local employment share.

Appendix Table 2.8: The Effects of Concentrated Industries by Percentile

	Expansion			Recession		
	$\lambda_{25\text{-}50}$	$\lambda_{50\text{-}75}$	λ_{75+}	$\lambda_{25\text{-}50}$	$\lambda_{50\text{-}75}$	λ_{75+}
0-6 Months After Job Loss	$948***	$1,079***	$1,434***	-$428**	-$1,198***	-$943***
	($238)	($207)	($185)	($185)	($214)	($181)
6-12 Months After Job Loss	$968***	$1,355***	$1,565***	-$896***	-$1,433***	-$1,293***
	($162)	($144)	($135)	($141)	($175)	($135)
12-18 Months After Job Loss	$1,081***	$1,609***	$1,806***	-$772***	-$1,293***	-$944***
	($188)	($132)	($131)	($130)	($165)	($128)
18-24 Months After Job Loss	$1,016***	$1,384***	$1,590***	-$733***	-$1,206***	-$961***
	($140)	($135)	($127)	($126)	($182)	($126)
24-30 Months After Job Loss	$791***	$1,198***	$1,513***	-$356***	-$811***	-$477***
	($181)	($138)	($139)	($133)	($160)	($134)
30-36 Months After Job Loss	$442***	$711***	$1,022***	-$276**	-$607***	-$457***
	($164)	($132)	($127)	($120)	($150)	($125)

Note: The displaced worker sample is the full set of U.S. displaced workers with at least five quarters of tenure for states that submitted worker earnings to the LEHD from 2005 to 2012. The job stayer sample is a one percent sample of workers who met the following criteria: (i) They had five quarters of tenure as of any quarter from 2005 to 2012; (ii) They then proceeded to have 12 consecutive quarters of tenure at the same firm after that quarter. $\lambda_{25\text{-}50}$ compares the 25th-50th percentile in the distribution of local employment share across CBSAs in an industry relative to the below 25th percentile range; $\lambda_{50\text{-}75}$ compares the 50th-75th percentile range relative to the below 25th percentile range; λ_{75+} compares the above 75th percentile range relative to the below 25th percentile range.

Appendix Table 2.9: The Effects of Concentrated Industries on Migration

	$\lambda_{Expansion}$	$\lambda_{Recession}$
1 Year After Job Loss	-0.0081***	-0.0009
	(0.0017)	(0.0011)
2 Years After Job Loss	-0.0078***	-0.0024
	(0.0020)	(0.0015)
3 Years After Job Loss	-0.0073***	-0.0025
	(0.0021)	(0.0017)

Note: The displaced worker sample is the full set of U.S. displaced workers with at least five quarters of tenure for states that submitted worker earnings to the LEHD from 2005 to 2012. The job stayer sample is a one percent sample of workers who met the following criteria: (i) They had five quarters of tenure as of any quarter from 2005 to 2012; (ii) They then proceeded to have 12 consecutive quarters of tenure at the same firm after that quarter. The outcome is whether the worker moved to a different CBSA relative to their initial CBSA at the time of sample inclusion.

Appendix Table 2.10: The Effects of Concentrated Industries on Change in Earnings by Age

	25-35 Year Olds		35-45 Year Olds		45+ Year Olds	
	$\lambda_{Expansion}$	$\lambda_{Recession}$	$\lambda_{Expansion}$	$\lambda_{Recession}$	$\lambda_{Expansion}$	$\lambda_{Recession}$
0-6 Months After Job Loss	$548***	-$196	$529***	-$62	$361**	$10
	($143)	($142)	($151)	($171)	($149)	($154)
Unconditional Earnings Losses	*-$6,829*	*-$9,155*	*-$7,796*	*-$10,621*	*-$8,491*	*-$11,195*
6-12 Months After Job Loss	$420***	-$109	$476***	-$146	$297**	-$52
	($120)	($124)	($125)	($147)	($125)	($135)
Unconditional Earnings Losses	*-$4,585*	*-$7,253*	*-$5,123*	*-$8,430*	*-$5,493*	*-$8,956*
12-18 Months After Job Loss	$536***	$53	$575***	$128	$420***	$125
	($135)	($136)	($136)	($169)	($129)	($147)
Unconditional Earnings Losses	*-$4,788*	*-$7,153*	*-$5,255*	*-$8,324*	*-$5,615*	*-$8,884*
18-24 Months After Job Loss	$343***	-$13	$536***	$22	$365***	$53
	($129)	($137)	($122)	($165)	($121)	($141)
Unconditional Earnings Losses	*-$4,406*	*-$6,554*	*-$4,795*	*-$7,608*	*-$5,093*	*-$8,112*
24-30 Months After Job Loss	$493***	$40	$764***	$189	$503***	$145
	($147)	($135)	($143)	($162)	($133)	($140)
Unconditional Earnings Losses	*-$4,978*	*-$6,518*	*-$5,410*	*-$7,504*	*-$5,735*	*-$8,026*
30-36 Months After Job Loss	$441***	-$109	$623***	-$53	$382***	$76
	($147)	($128)	($145)	($145)	($131)	($131)
Unconditional Earnings Losses	*-$4,796*	*-$5,927*	*-$5,224*	*-$6,860*	*-$5,523*	*-$7,338*

Notes: The sample is the full set of U.S. displaced workers with at least five quarters of tenure for states that submitted worker earnings to the LEHD from 2004 to 2012. The job stayer sample is a one percent sample of workers who met the following criteria: (i) They had five quarters of tenure as of any quarter from 2005 to 2012; (ii) They then proceeded to have 12 consecutive quarters of tenure at the same firm after that quarter. The dependent variable is change in earnings. The estimates are the output from equation $(E1)$, limited to specific age groups.

Appendix Table 2.11: The Effects of Concentrated Industries on Change in Earnings by Educational Attainment

	Education: Low		Education: High	
	$\lambda_{Expansion}$	$\lambda_{Recession}$	$\lambda_{Expansion}$	$\lambda_{Recession}$
0-6 Months After Job Loss	$338***	$27	$999***	-$315
	($126)	($138)	($196)	($202)
Unconditional Earnings Losses	*-$7,472*	*-$10,049*	*-$8,646*	*-$11,664*
6-12 Months After Job Loss	$240**	-$4	$1,022***	-$406**
	($102)	($119)	($174)	($176)
Unconditional Earnings Losses	*-$4,929*	*-$8,032*	*-$5,205*	*-$8,888*
12-18 Months After Job Loss	$343***	$180	$1,202***	-$146
	($112)	($139)	($176)	($171)
Unconditional Earnings Losses	*-$4,996*	*-$7,916*	*-$5,378*	*-$8,754*
18-24 Months After Job Loss	$313***	$123	$942***	-$316*
	($101)	($132)	($166)	($176)
Unconditional Earnings Losses	*-$4,549*	*-$7,203*	*-$4,653*	*-$7,874*
24-30 Months After Job Loss	$468***	$224*	$1,095***	-$146
	($119)	($130)	($177)	($169)
Unconditional Earnings Losses	*-$5,070*	*-$7,067*	*-$5,382*	*-$7,865*
30-36 Months After Job Loss	$396***	$105	$854***	-$315*
	($116)	($115)	($184)	($169)
Unconditional Earnings Losses	*-$4,900*	*-$6,426*	*-$4,943*	*-$6,994*

Note: The displaced worker sample is the full set of U.S. displaced workers with at least five quarters of tenure for states that submitted worker earnings to the LEHD from 2005 to 2012. The job stayer sample is a one percent sample of workers who met the following criteria: (i) They had five quarters of tenure as of any quarter from 2005 to 2012; (ii) They then proceeded to have 12 consecutive quarters of tenure at the same firm after that quarter. The dependent variable is change in earnings. The estimates are the output from equation ($E1$), limited to those with either low education (less than a Bachelor's degree) or high education (Bachelor's degree or higher).

Appendix Table 2.12: The Effects of Concentrated Industries on Employment by Educational Attainment

	Employment				Same-Industry			
	Education: Low		Education: High		Education: Low		Education: High	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$\lambda_{Expansion}$	$\lambda_{Recession}$	$\lambda_{Expansion}$	$\lambda_{Recession}$	$\lambda_{Expansion}$	$\lambda_{Recession}$	$\lambda_{Expansion}$	$\lambda_{Recession}$
0-6 Months After Job Loss	0.018***	-0.009**	0.029***	-0.011**	0.035***	0.006	0.032***	-0.002
	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.007)	(0.007)
Unconditional Employment	*0.754*	*0.638*	*0.757*	*0.663*	*0.423*	*0.361*	*0.466*	*0.400*
6-12 Months After Job Loss	0.016***	-0.004	0.030***	-0.005	0.036***	0.015***	0.040***	0.011
	(0.003)	(0.004)	(0.005)	(0.005)	(0.006)	(0.005)	(0.007)	(0.007)
Unconditional Employment	*0.810*	*0.690*	*0.814*	*0.715*	*0.423*	*0.366*	*0.475*	*0.409*
12-18 Months After Job Loss	0.016***	0.000	0.028***	0.001	0.035***	0.018***	0.037***	0.013**
	(0.003)	(0.003)	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)	(0.006)
Unconditional Employment	*0.808*	*0.703*	*0.816*	*0.729*	*0.389*	*0.342*	*0.449*	*0.391*
18-24 Months After Job Loss	0.016***	0.003	0.022***	0.002	0.032***	0.021***	0.031***	0.015**
	(0.003)	(0.003)	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)	(0.006)
Unconditional Employment	*0.805*	*0.722*	*0.814*	*0.743*	*0.370*	*0.333*	*0.431*	*0.381*
24-30 Months After Job Loss	0.015***	0.006***	0.023***	0.007**	0.033***	0.025***	0.033***	0.024***
	(0.002)	(0.003)	(0.004)	(0.004)	(0.005)	(0.004)	(0.006)	(0.006)
Unconditional Employment	*0.789*	*0.734*	*0.803*	*0.751*	*0.345*	*0.320*	*0.410*	*0.368*
30-36 Months After Job Loss	0.014***	0.008***	0.021***	0.010***	0.033***	0.027***	0.030***	0.026***
	(0.003)	(0.002)	(0.004)	(0.004)	(0.005)	(0.004)	(0.006)	(0.006)
Unconditional Employment	*0.771*	*0.740*	*0.789*	*0.756*	*0.329*	*0.312*	*0.395*	*0.360*

Notes: The sample is the full set of U.S. displaced workers with at least five quarters of tenure for states that submitted worker earnings to the LEHD from 2005 to 2012. The dependent variable is any employment in Columns (1)-(4), and same industry employment in Columns (5)-(8). Columns (1)-(2) are the $\lambda_{Expansion}$ and $\lambda_{Recession}$ employment estimates for those with low education, and Columns (3)-(4) are the analogous statistics for those with high education. Columns (5)-

(6) are the $\lambda_{Expansion}$ and $\lambda_{Recession}$ within-industry employment estimates for those with low education, and Columns (7)-(8) are the analogous statistics for those with high education.

Appendix Table 2.13: The Number of Co-workers Voluntarily Separating, by Concentrated Industry Status

	$\lambda_{Expansion}$	$\lambda_{Recession}$
Point Estimate	59	-5
STD Error	73	25
Unconditional Average	432	309

Notes: The sample is the full set of U.S. displaced workers with at least five quarters of tenure for states that submitted worker earnings to the LEHD from 2005 to 2012. The job stayer sample is a one percent sample of workers who met the following criteria: (i) They had five quarters of tenure as of any quarter from 2005 to 2012; (ii) They then proceeded to have 12 consecutive quarters of tenure at the same firm after that quarter. The dependent variable is the number of displaced workers' co-workers with adjacent or within quarter dominant job-to-job flow prior to job loss. The covariates are the same as those in equation ($E3$), with the exception of the control function.

Appendix Table 2.14: The Effects of Concentrated Industries on Change in Earnings (Within Plant Closers, Birth State Residence)

	Plant Closers		Plant Closers and Reside Within the Same State as Birth State	
	(1)	(2)	(3)	(4)
	$\lambda_{Expansion}$	$\lambda_{Recession}$	$\lambda_{Expansion}$	$\lambda_{Recession}$
0-6 Months After Job Loss	$359*	-$202	$287	-$198
	($203)	($197)	($224)	($215)
Unconditional Earnings Losses	*-$8,037*	*-$10,741*	*-$8,084*	*-$10,739*
6-12 Months After Job Loss	$399**	-$111	$291	-$151
	($177)	($187)	($202)	($202)
Unconditional Earnings Losses	*-$4,960*	*-$8,792*	*-$4,933*	*-$8,817*
12-18 Months After Job Loss	$396**	$54	$217	$1
	($181)	($203)	($200)	($220)
Unconditional Earnings Losses	*-$4,361*	*-$8,165*	*-$4,276*	*-$8,189*
18-24 Months After Job Loss	$351**	$78	$223	$15
	($166)	($185)	($180)	($200)
Unconditional Earnings Losses	*-$4,121*	*-$7,453*	*-$4,031*	*-$7,420*
24-30 Months After Job Loss	$346**	$164	$249	$141
	($169)	($170)	($186)	($182)
Unconditional Earnings Losses	*-$4,202*	*-$6,899*	*-$4,122*	*-$6,846*
30-36 Months After Job Loss	$227	$186	$132	$173
	($168)	($181)	($182)	($201)
Unconditional Earnings Losses	*-$4,366*	*-$6,357*	*-$4,273*	*-$6,260*

Notes: Columns (1)-(2) include the following sample: the full set of U.S. displaced workers: (i) with at least five quarters of tenure; (ii) who were part of a company shut-down; (iii) who resided in a state that submitted worker earnings to the LEHD from 2005 to 2012. Columns (3)-(4) include the following sample: the full set of U.S. displaced workers: (i) with at least five quarters of tenure; (ii) who were part of a company shut-down; (iii) who resided in the same state as the state of birth during job loss; (iv) who resided in a state that submitted worker earnings to the LEHD from 2005 to 2012.

Appendix Table 2.15: The Effects of Concentrated Industries on Change in Earnings by the Severity of Local Shocks (Within Plant Closers, Birth State Residence)

	Plant Closers			Plant closers and reside within the same state		
	(1)	(2)	(3)	(4)	(5)	(6)
	λ_{small}	λ_{medium}	λ_{large}	λ_{small}	λ_{medium}	λ_{large}
0-6 Months After Job Loss	$630**	-$214	-$709**	$594**	-$256	-$698***
	($248)	($204)	($273)	($269)	($235)	($279)
Unconditional Earnings Losses	*-$8,451*	*-$9,822*	*-$12,021*	*-$8,516*	*-$9,752*	*-$12,003*
6-12 Months After Job Loss	$592***	-$64	-$558**	$552***	-$180	-$583**
	($169)	($214)	($264)	($181)	($236)	($298)
Unconditional Earnings Losses	*-$5,408*	*-$7,815*	*-$10,070*	*-$5,347*	*-$7,785*	*-$10,053*
12-18 Months After Job Loss	$728***	$39	-$592**	$603***	-$82	-$654*
	($174)	($210)	($293)	($186)	($232)	($346)
Unconditional Earnings Losses	*-$5,104*	*-$7,397*	*-$8,109*	*-$4,993*	*-$7,365*	*-$8,032*
18-24 Months After Job Loss	$589***	$62	-$369	$519***	-$72	-$448
	($164)	($194)	($264)	($174)	($210)	($306)
Unconditional Earnings Losses	*-$4,820*	*-$6,696*	*-$7,509*	*-$4,704*	*-$6,627*	*-$7,375*
24-30 Months After Job Loss	$617***	$150	-$344	$579***	$46	-$386
	($177)	($181)	($211)	($192)	($196)	($239)
Unconditional Earnings Losses	*-$4,945*	*-$6,410*	*-$6,144*	*-$4,834*	*-$6,354*	*-$5,944*
30-36 Months After Job Loss	$595***	$38	-$222	$578***	-$69	-$269
	($186)	($174)	($274)	($206)	($193)	($323)
Unconditional Earnings Losses	*-$4,896*	*-$5,943*	*-$5,991*	*-$4,791*	*-$5,858*	*-$5,731*

Notes: Columns (1)-(2) include the following sample: the full set of U.S. displaced workers: (i) with at least five quarters of tenure; (ii) who were part of a company shut-down; (iii) who resided in a state that submitted worker earnings to the LEHD from 2005 to 2012. Columns (3)-(4) include the following sample: the full set of U.S. displaced workers: (i) with at least five quarters of tenure; (ii) who were part of a company shut-down; (iii) who resided in the same state as the state of birth during job loss; (iv) who resided in a state that submitted worker earnings to the LEHD from 2005 to 2012. The Bartik Instrument is used to label a local shock as either small, medium, or large.

Chapter 3: The Consequences of Job Loss in the Great Recession

(Co-Authored with Henry Hyatt and Erika McEntarfer)

3.1 Introduction

The U.S. Labor market is recovering from the deepest employment downturn since the end of World War II. Millions of workers lost their jobs during the Great Recession of 2007-2009, with employment losses occurring across several broad sectors of the economy. Previous studies have shown that job losers receive much lower earnings (if any) after job loss during a recession, but we still do not know why these "earnings losses" are more severe during these downturns. Recent work (Fallick et al 2012) suggests that longer non-employment duration within the Great Recession is primarily responsible for worse earnings losses during this period relative to expansions, but we do not know if this relationship still holds when comparing it to less severe recessions.

In this chapter, we use the rich longitudinal data from the Longitudinal Employer Household Dynamics (LEHD) to first calculate how displaced workers' earnings losses, as proxied by earnings changes, vary across recent recessions and expansions. We find that displaced workers who lost their jobs between 2008 and 2012, which includes the Great Recession, had roughly 9% more severe earnings losses relative to those who lost their job during the 2001 Recession. We also find that displaced workers who lost their jobs during an expansion had 17% -31% less severe earnings losses relative to those who lost their job during the Great Recession.

We then attribute how much of the variation in earnings losses across these different time periods can be explained by the variation in non-employment length, quality of job match in subsequent employment (if applicable), and the interaction between these two outcomes. We find that non-employment length differences across time explain a very high share of the variation in earnings losses across the Great Recession, the 2001 Recession, and recent expansions. The quality of job match, as approximated by whether the displaced worker found employment within the same supersector as his previous firm, explains a sizeable portion as well, but not as high as the non-employment length.[40] We also find that the interaction between these two outcomes explains a very high share of the earnings loss variation across time. We interpret these results as evidence that not only are there less jobs available during a recession, but the quality of available jobs are also lower. We thus conclude that both factors contribute to higher earnings losses for job losers during a recession.

We estimate the effects of job displacement by comparing the earnings changes for displaced workers relative to workers who did not lose their job (job stayers) within each of the time periods (i.e. within the Great Recession, 2001 Recession, etc). We calculate an effect for each quarter for three years after job loss, to see not only how displaced workers are affected initially, but also to evaluate how persistent job loss effects are. We then calculate a net present discounted value of earnings losses within each time period, and then perform a shift share analysis to see whether other outcomes or demographic characteristics can explain the variation in earnings losses across time.

[40] Supersector is a BLS defined term for the groupings of NAICS sectors. The mapping from NAICS sectors to Supersectors can be found here: http://www.bls.gov/ces/cessuper.htm.

There is evidence within the literature that indicates macroeconomic conditions can affect the earnings losses for displaced workers by looking across business cycles. The evidence of the persistence in earnings losses from displacement during the 1982 recession was brought to light from Jacobson, Lalonde, and Sullivan (1993) and von Wachter, Manchester, and Song (2008), who show that earnings losses persist for more than six years and more than a decade, respectively. Couch and Placzek (2010) show similar persistence patterns in earnings losses by looking at displaced workers from another recession, the 2001 recession. The most striking evidence of potential variation across time comes from Farber (2011), where he shows that there are clear cyclical patterns in job loss rate (countercyclical) and the re-employment rate (pro-cyclical). The lack of job opportunities during and after these recessions naturally amplified and increased the persistence of earnings losses.

We are also interested in how the subsequent job match quality after job loss can affect displaced workers' earnings across time. If a worker has to learn a new set of skills at his new job he will not be as productive and therefore will earn less . Since workers are less likely to find jobs that suit their skills during a recession, it is likely that variation in job match quality contributes to variation in earnings losses across business cycle phases.

We then conclude our findings by analyzing how unemployment benefits mitigate earnings losses for displaced workers during worse economic conditions. We find that although unemployment compensation rises during worse conditions, this increase is not enough to compensate for the higher earnings losses experienced during the Great Recession. We do find, however, that they compensate for losses experienced in less severe recessions.

This chapter proceeds as follows: The next section provides the data description; section 3 provides the methodology; section 4 describes the results, and section 5 wraps up with the conclusion.

3.2 Data

We use the Longitudinal Employer-Houshold Dynamics (LEHD) data as the primary database within this analysis. The LEHD is an employer-employee linked longitudinal database that contains quarterly information for most US worker-establishment combinations.[41] We are also able to track an employee's job spell for a given establishment over time, since attrition is of little concern in this administrative database. An additional feature of the LEHD is that we are able to observe the worker's demographic characteristics such as age and gender, both of which we use as control variables for our main empirical analysis.[42]

The main relation we analyze is how displaced workers' earnings evolved relative to those who never lost their job (i.e. job stayers) within recent expansions and recessions. We aggregate the worker's earnings across all jobs within a quarter,

[41] We observe the earnings and employment for all of the workers who were ever employed within the United States. The exception is employment for the federal government and the self-employed. Additionally, we are not able to observe hours with this database.
[42] For more description on the LEHD data, see Abowd et al. (2009) and Haltiwanger et al. (2014).

and evaluate the earnings evolution within a six year window, starting from three years before job loss, and ending three years after.[43]

Since we are unable to directly identify displaced workers within the LEHD, we use an approximation that is similar to other approximations used within the displaced worker literature. We first restrict our sample of displaced worker candidates to all workers who had a minimum of twelve quarters of tenure, since we are interested in the earnings outcomes for workers who lost their jobs after initially having an extended period of stable employment. Fortunately we are able to calculate the duration of job tenure for each worker within the data, as well as identify which establishment pays the worker the most, which we will refer to as the "dominant job". We then further restrict our candidates to those who had a dominant job separation (i.e. when the job is no longer their dominant job) in a given quarter.[44] Finally, to approximate that this separation is due to an involuntary job loss due to a mass-layoff, we exploit the firm dynamics information that is available in the LEHD for these workers. If the worker's establishment has at least a thirty percent decrease during the same quarter as his separation quarter, we flag him as a displaced worker.[45]

---

[43] Results for a future draft (not currently shown) extend the number of quarters of tenure and over which losses are calculated to eight.

[44] We use a prototype job-to-job flows database to identify separations. This database employs definitions similar to Hyatt and McEntarfer (2012a, 2012b), and which is also used by Haltiwanger, Hyatt, and McEntarfer (2014).

[45] 30% is an approximation for a mass-layoff incidence. This threshold is determined by the maximum establishment employment count for the two quarters prior to a quarter of interested and the minimum establishment employment count for the two quarters after. For example, if the establishment had 100, 99 employees during 2005 Q1, Q2, and 30, 25 employees during 2005 Q4, 2006 Q1, then it had a (25-100)/100 = 75% reduction, and therefore a mass-layoff during 2005 Q3. In reality this type of separation can be the result of a voluntary job-to-job flow, termination due to cause, or an involuntary job loss. We provide evidence that, while it is certainly possible some of these workers are incorrectly labelled as people who lost their job due to a mass-layoff, on average we capture these workers with this definition. Other papers have used similar measures to flag displaced workers, but use years instead of quarters to calculate the percentage decline of a firm (Jacobsen Lalonde and Sullivan (1993), von Wachter et al (2009)).

Our sample of job stayers is intended to be a comparable group of workers to our sample of displaced workers. We first restrict all workers to have the same minimum tenure restriction at a quarter, just like our displaced worker sample.[46] We also require these workers to have an additional three years of tenure after the quarter, to approximate what the displaced workers' earnings would have been in the absence of job loss.

3.3 Methodology

Our main empirical question is how much of the earnings loss variation across time is explained by non-employment length variation, and how much is due to poor subsequent job matches. We are unable to distinguish between non-employment and unemployment, because we are unable to identify workers who drop out of the labor force. This data shortcoming is not a problem for our analysis, since we want to include workers who were discouraged from the labor market.[47] We follow the methodology used within Fallick, McEntarfer, Haltiwanger (2012) to calculate the ex-post non-employment, where we use the count of full quarter non-employment.[48]

We approximate a poor subsequent job match by whether the displaced worker works for a firm that is part of a different supersector than his previous job's.

---

[46] For example, for 2005 quarter 1, the worker needs to be employed at the same firm for every quarter from 2002 quarter 1 to 2005 quarter 1.

[47] The shortcoming associated with zero earnings is that we are currently not including all of the states within our sample because different states started to submit their earnings during different years. We therefore restrict the sample to workers who worked within the states that had started to submit their information prior to 1996, inclusive. The consequence of this is that zero earnings could be due to the worker moving to a state not within our sample. We have done alternative runs where we include these other states earnings for our cohort, and we do not see noticeable differences.

[48] One consequence from this method is we are unable to identify workers who experience short-term non-employment (i.e. less than 3 months).

If a worker works for a firm outside his original supersector after job loss, it could be because that job was a good match for somebody with his skillset. However, it is also possible that he works there because he has no job offers that are a good match for him, and this was the best job available. The latter scenario is more likely to arise if the worker is liquidity constrained, which will occur if he has an extended non-employment length, which is more likely to occur in a severe economic downturn. Therefore, it is important to consider the interaction of non-employment length interacts and job match quality affects earnings loss variation over time.

We then evaluate to what extent these earnings losses were mitigated by unemployment benefits. We link the LEHD with the Current Population Survey (CPS) March Supplement to get the unemployment benefit information for workers that are in both databases. By using this sample, we are able to infer whether existing policies have provided enough assistance during recessions.

3.3.1 Empirical Strategy

We evaluate earnings losses within five periods: 1998 quarter 2 – 2001 quarter 1; 2001 quarter 2 – 2001 quarter 4 (the 2001 recession); 2002 quarter 1 – 2007 quarter 3; 2007 quarter 4 – 2009 quarter 2 (the Great Recession); and 2009 quarter 3 – 2010 quarter 2. We choose to partition in this manner so that we evaluate the effects from job loss during severe downturns (Great Recession, 2001 Recession, 2009 quarter 3 – 2010 quarter 2), economic recoveries (2002 quarter 1 – 2007 quarter 3), and economic expansions (1998 quarter 2 – 2001 quarter 1).

For each quarter within each of the five periods, we first classify a worker that meets the tenure restriction as either a displaced worker or a job stayer. We then compute the worker's earnings for each quarter within the following time window: 12 quarters before, the quarter of interest, and 12 quarters after, giving us 25 quarters total. We then combine all displaced workers and job stayers across all quarters within a period, and subset the data to men 35-55 years old. We label each quarter within a period as q, and we label each quarter within its time window as t. To give an example, within the 2001 Recession period, q's values are: 2001 quarter 2, 2001 quarter 3 and 2001 quarter 4. The workers within this period are displaced workers and job stayers from any of these three quarters. Since we are looking at a 25 quarter time window for each of these three q quarters, t's values within the 2001 Recession will range from 1998 quarter 2 (12 quarters before 2001 quarter 2, the first q in this period) to 2004 quarter 4 (12 quarters after 2001 quarter 4, the last q in this period).

Our main empirical equation, which we estimate for each of the 5 periods separately, is the following:

$$Y_{it} = \alpha_{iq} + \gamma_t + \beta_1 age_{it} + \beta_2 age_{it}^2 + \sum_{k=m_0}^{k=m_1} \delta_k I(Disp_i) * I(t - q = k) + \varepsilon_{it} \quad (1)$$

Y_{it} represents total earnings for worker i within a quarter t. The α_{iq} term controls for heterogeneity associated with what q the worker was included in the sample (which will be the quarter of job loss for displaced workers, and quarter of inclusion for job stayers). The γ_t term represents the quarter of the observation, which control for aggregate effects. We also include age and age squared to control for possible effects resulting from workers getting older.

We focus largely on how the δ_k estimates evolve across time, especially when $k \geq 0$. These estimates are defined by how high earnings are for displaced workers (i.e. $I(Disp_i) = 1$) relative to job stayers (i.e. $I(Disp_i) = 0$) when the observation during quarter t is k quarters away from the job loss quarter q relative to a left out quarter, which we will discuss shortly. Negative values of k represent observations that occur k quarters before job loss, positive values represent observations that occur k quarters after job loss, and $k = 0$ is the quarter of job loss. For example, if the observation is 2005 quarter 3, and the displaced worker loses his job in 2004 quarter 3/worker is a job stayer around 2004 quarter 3, then $k = 4$. If the observation is 2004 quarter 1, then $k = -2$. Since we analyze how earnings evolve 12 quarters before to 12 quarters after job loss within our baseline specification, we set $m_0 = -11$ and $m_1 = 12$, making 12 quarters prior to job loss as the left out category.

We expect the $\delta_k, k \geq 0$ terms to be negative, since displaced workers' earnings changes after job loss are typically much lower than those who didn't lose their jobs. We expect δ_k to be the lowest (i.e. highest magnitude) for the first few positive k values, since a displaced worker is more likely to be out of work during the first few quarters after job loss as opposed to several years later. Most displaced worker studies show that earnings losses are persistent, so we expect $\delta_{10}, \delta_{11}, \delta_{12}$ to be negative as well to reflect this.

In order to present an aggregate number for each of the 5 periods, we calculate a net present discounted value (net PDV) to provide an aggregate summary of earnings losses for displaced workers within a given period. We calculate this statistic for each period as:

$$PDV_p = \sum_{k=-1}^{m_1} \left(\frac{\widehat{\delta_k} - \widehat{\delta_{-1}}}{(1+r)^{k+1}}\right) \qquad (2)$$

Where p represents one of the 5 periods, and the $\hat{\delta}$ term is the estimate from equation 1 for that period. We use a discount rate r of 0.01 to weight earnings losses from earlier quarters more than later ones, to reflect possible present-discounted preferences. Our metric represents the weighted earnings losses for all m_1 quarters after job loss, using the quarter before job loss as the base quarter (hence why we subtract the $\widehat{\delta_{-1}}$ term). We use the first quarter before displacement rather than earlier periods, since we do not want any variation in the earnings dip before displacement to affect our interpretation (ex: if we were to use, say $\widehat{\delta_{-12}}$ as the base estimate and use $\widehat{\delta_{-11}}$, $\widehat{\delta_{-10}}$, $\widehat{\delta_{-9}}$, etc as the post periods, we will include the earnings changes prior to job loss. If the earnings dip prior to job loss is more severe during some time periods relative to others, this effect will be captured in the PDV statistic. We are not interested in capturing that effect in our net PDV, and instead we focus solely on the effects after job loss).[49]

We use a shift share analysis to calculate how non-employment length and subsequent job match quality variation explain earnings loss variation across each of the 5 periods. As stated earlier, we use re-employment at a different supersector as an approximation for poor subsequent match quality. Using the subscript p^{init} as the base period, p^{post} as the period of interest, and j as different values within an

[49] We do not use the quarter of displacement as the base quarter either. This is due to the fact that we cannot observe when during the quarter the agent is displaced, and if there is any variation across time quarters within a quarter when the agent is displaced, it could affect our results. The occurrence of earnings dips before displacement is a common finding within the displaced worker literature.

explanatory variable of interest, we decompose the changes in the net PDV across periods as:

$$PDV_{p^{post}} - PDV_{p^{init}} = \sum_j \Delta PDV_{jp} S_{j.} + \sum_j PDV_{j.} \Delta S_{jp} \quad (3)$$

The left hand side of (3) is the unconditional change of the net PDV across two periods. The $\Delta \mathrm{PDV_{jp}} = PDV_{jp^{post}} - PDV_{jp^{init}}$ represents the change in the net present discounted value across periods within category j.[50] The $PDV_{j.}$ term represents the average values between the two periods within category j (i.e. $\frac{PDV_{jp^{post}} + PDV_{jp^{pre}}}{2}$). S_j is the share of the people within category j, and the ΔS_{jp} and $S_{j.}$ terms are the equivalent statistics for the share of people.

The right hand side of equation 3 is composed of two parts: The "within effect" and the "composition effect". The first term is the "within effect", which shows how much of the unconditional PDV change is due to PDV changes within each category across the two periods. The second term is the "composition effect", which shows how much of the unconditional PDV change is due to changes in the category shares across the two periods. We focus on the second term, since we are interested in whether any changes in the prevalence of certain outcomes (i.e. changes in the share of displaced workers with poor subsequent fit, changes in the share of displaced workers with high non-employment length) can explain earnings loss variation. Although we focus mostly on the non-employment length and job match

---

[50] To calculate the net PDV within category j, we restrict the displaced worker sample to those within that category, and re-estimate (1) and (2) using the same stayer group as the control sample.

quality, we also investigate how compositional changes in age, education, and race-ethnicity influence earnings loss variation across periods.[51]

One of the assumptions within our empirical model is that the earnings trends for job stayers are comparable across each of the five periods, i.e. we assume that estimated differences across periods is due to variation in the displaced workers' earnings and not the job stayers. There are many reasons why our assumption is appropriate. If we assume that displacement is exogenous across time, these effects should also be comparable across time. In addition, if unobserved differences between displaced workers and job stayers are determined by time-invariant characteristics, and these characteristics differ across periods, our inclusion of fixed effects will control for these differences.

3.4 Results

3.4.1 Worker and Firm Characteristics

Tables 3.1 and 3.2 provide demographic and establishment characteristics for the workers within our sample. There are several differences in characteristics between displaced workers and job stayers within each of the five periods, and some of these differences are larger for some periods relative to others. Displaced workers are on average younger, less educated, and have a smaller percentage of white workers relative to job stayers, suggesting that there are also differences in

[51] Education is not observed for all of the agents within the sample. The LEHD does impute the educational attainment for most of the workers who did not take part within a survey, and we use these imputed values.

unobservable characteristics between the two types of workers.[52] We also see that displaced workers' supersector composition is different from the job stayers'.[53] Displaced workers have higher representation within the construction, manufacturing, leisure/hospitality and professional/business services supersectors, while job stayers have higher representation within the education/health service, and trade/transportation supersectors, again suggesting differences between the two types of workers. These differences strongly suggest that displacement is not exogenous, but this will only be a concern if effects from "selection" into job loss varies across the five periods. These effects will likely vary, but in a way that will be favorable for our interpretation. Firms are likely to let go of their least productive workers during a mass layoff, but they are less likely to be as selective during a recession as evidenced by the higher incidence of plant closures (i.e. when they let go of all their workers). If displaced workers are less productive, then any OLS bias will be negative (i.e. less productive workers are less likely to be re-employed, and OLS estimates will capture this phenomenon), but they will likely be more negative during our expansion periods. Since we expect recessions to have more negative earnings losses, and OLS biases are more negative during expansions, selection effects are not a problem for our analysis.

There is considerable variation in the displaced workers' supersector composition across each of the five periods, especially when comparing it to job stayers. Relative to job stayers, displaced workers were more likely to come from the

[52] This assumes that differences in demographic characteristics is a proxy for differences in unobservable characteristics.
[53] Firm size is also noticeably smaller for displaced workers relative to job stayers. However, this is most likely due to our displaced worker definition.

construction sector over time, with a 4.3 percentage point (pp) difference before the 2001 recession (10.3%-6%) to a 17.6 pp difference after the Great Recession (24.0%-6.4%). There is also a noticeable difference in the displaced worker representation relative to job stayers within the manufacturing sector. The difference in prevalence between the two worker categories is more severe during recessions, since there is roughly a 9 pp difference during the Great Recession and the 2001 Recession (30.1%-21.3%, 34.3%-25.6%, respectively), while there is only roughly a 2.5 pp difference within our expansion periods (28.4%-25.8% for the 1999-2001 qtr 1 expansion, 26.3%-23.5% 2002 qtr 1-2007 qtr 3, 23.3%-20.8% for 2009 qtr 3-2010 qtr 2). Also, job stayers have a higher representation within the Trade, Transportation, and Utilities supersector over time; they have a 3 pp higher prevalence before the 2001 recession (23%-19.8%), which increases to an 8 pp higher prevalence at the end of the time series (24.0%-15.8%). These changes across periods suggest that initial supersector of employment may also affect subsequent earnings for displaced workers relative to job stayers. As a result, we include this as one of the categories for our shift share analysis.

3.4.2 Baseline Regressions

Figure 3.1 has the national displacement rates across our time series.[54] Figure 3.1 shows that displaced workers are more likely to lose their job during a recession, especially during the Great Recession. The Great Recession's peak is more than 57%

[54] The denominator within the time series is the count of dominant jobs within that quarter, which are defined as the number of jobs associated with the highest earnings for a worker.

higher than any other peak, which reflects the deteriorating labor market condition in that period. The expansion period following the Great Recession also had displacement rates that were higher than the 2001 Recession and the other expansion periods in our time series, which again shows how poor the economic recovery was during those years. The third highest peak occurs during the 2001 Recession, which is then followed by the 2002-2007 expansion and the pre-2001 expansion. If earnings losses are commensurate with national job loss rates, we should expect to find that the Great Recession has the highest earnings losses, followed by its "recovery" period, then the 2001 Recession, then the 2002-2007 expansion, and then the pre-2001 Recession expansion period.

We find that earnings losses move in a very similar pattern to the displacement rates, as evidenced by Figure 3.2 and Appendix Table 3.1. The net PDV for the Great Recession is the largest at roughly 60,020 dollars over a 12 quarter period, followed closely by the recovery period after the Great Recession (58,150), the 2001 Recession (54,844), the expansion period during 2002-2007 (50,829), and the expansion period before the 2001 Recession (43,587).

One reason why the net PDV is so large during the Great Recession is due to the large initial earnings dip that occurs during this period, as shown by the $\widehat{\delta_0}, \widehat{\delta_1}$ estimates. The dip is large for each of the five periods, but is largest for the Great Recession and the recovery period after the Great Recession, contributing to a high net PDV value. The smallest dip occurs during the expansion period prior to the 2001 Recession, and there are small differences in the dip magnitude between the 2001 Recession and the 2002-2007 years.

A similar ranking across periods appears when looking at earnings loss persistence (i.e. the $\widehat{\delta_k}, k \geq 2$ estimates). Earnings losses are persistent for all of the periods, as evidenced by the negative $\widehat{\delta_{12}}$ estimate (which means displaced workers' earnings changes is substantially lower than job stayers even 12 quarters after job loss), but once again the Great Recession has the most severe losses. The $\widehat{\delta_{12}}$ estimate is very similar in magnitude for the other periods in our sample, but we see that the expansion period before the 2001 Recession has the least severe persistence, followed by the 2002-2007 expansion period. The 2001 Recession and the recovery period after the Great Recession have almost identical $\widehat{\delta_{12}}$ estimates, and are both roughly 500 dollars more severe than equivalent estimate in the 2002-2007 expansion period.

The most interesting pattern found in Figure 3.2/Appendix Table 3.1 is that even though the recovery period after the Great Recession has one of the most severe dips in earnings, it also has the biggest growth in the estimates across the three years. For this period, the $\widehat{\delta_1}$ estimate is -\$9,089, but the $\widehat{\delta_{12}}$ estimate is -\$4,933, implying that displaced workers' earnings improved by roughly \$4,100 between their first and their twelfth quarter after job loss. None of the other periods have as large of an improvement in earnings losses, especially 6 quarters after job loss (relative to the first quarter after job loss). This pattern suggests that the high net PDV for the "recovery" period after the Great Recession is driven primarily by the high initial dip in earnings and the slow recovery during the first year and a half after job loss for this period. Given that there is considerable variation in earnings losses within the three year span after job loss, a natural question follow up question is how much variation there is across quarters within a period.

To explore how much heterogeneity is within each of the five periods, we re-estimate equations (1) and (2) for each quarter of analysis, rather than pooling our results within five periods. Figure 3.3 has the time series of the net PDV, which shows that there is considerable heterogeneity within two of our periods. Within the 2002-2007 expansion, the net PDV is substantially lower for people who lost their job during 2004-2006 relative to those who lost their job during 2002-2003. This pattern reinforces the idea that the economy experienced a "jobless recovery" for the first two years after the 2001 Recession, and jobs did not recover until a few years after the recession was declared to be over. There is also considerable variation within the Great Recession, and the net PDV is the highest for 2008 quarter 4. This quarter also has one of the highest displacement rates within Figure 3.1, and is roughly the time when the Dow fell by roughly 800 points. This event, coupled with the persistent climb in the unemployment rate for the next two years are likely to be big reasons why earnings losses were so high for workers who lost their job during the Great Recession. We do not see much heterogeneity within the 2001 Recession nor the period after the Great Recession, and the heterogeneity within the expansion prior to the 2001 Recession is most likely due to the economy approaching a recession towards the end of 2000.

3.4.3 Non-employment and Job Quality

Figure 3.4 provides suggestive evidence that non-employment length contributes to the earnings loss variation across the five periods. Figure 3.4 shows the percent of displaced workers who lost their job in the quarter who had zero quarters, 1-4 quarters, 5-8 quarters, and at least 9 quarters of non-employment. The share of displaced workers with zero quarters of non-employment is pro-cyclical, and has a very large decrease during the Great Recession, implying that the share of these workers with any non-employment increases substantially during that period. In addition, the share of workers with long term non-employment (i.e. more than five quarters of non-employment) have a very large increase during the Great Recession. The share of displaced workers with long term non-employment is 35 percent higher during the Great Recession relative to the 2001 Recession (comparing peak to peak), while the share of displaced workers with zero quarters of non-employment is 32 percent lower (comparing trough to trough) across the same two periods. Given that non-employment lengths were so much longer during the Great Recession relative to other periods, it highly suggests that non-employment contributes a substantial amount to the earnings loss variation across time.

To see how subsequent job match quality affects earnings losses, Figure 3.5 shows the time series of the share of displaced workers who have poor job matches after job loss, as proxied by working at a different supersector. For this figure, we categorize displaced workers within three mutually exclusive categories: cross supersector switchers (workers who worked at a firm outside his previous firm's supersector during any quarter for three years after job loss), within supersector workers (workers who never worked at a firm outside his previous firm's supersector during any quarter for three years after job loss, and found employment after job loss), and people who were unable to find a job within twelve quarters after displacement. The share of cross-supersector switchers fluctuates mildly throughout the time series, with a global maximum of 42% during the 2001 recession. There is mild pro-cyclicality in the share of within supersector workers, and mild counter-cyclicality within the people who are never re-employed. When classifying displaced workers by these three categories, the initial thought is poor job match quality may not explain as much of the earnings loss variation as non-employment length. However, our way of classifying job match may hide some heterogeneity: Some workers could switch supersectors due to finding a good job at a firm within another supersector, and some workers could switch because they have exhausted all liquidity after extended nonemployment, and are simply picking the best or first job offer they find. One cannot reasonably expect both scenarios to have the same effects on earnings losses, so we look to see how non-employment interacts with job match quality.

Figure 3.6 shows the time series of the interaction between supersector switching prevalence and non-employment length. There is strong pro-cyclicality for the within supersector workers with 0 quarters of non-employment, and strong counter-cyclicality for the supersector switchers with 1 or more quarters of non-employment. The supersector switchers with 0 non-employment quarters and the within supersector workers with 1 or more quarters of non-employment time series do not exhibit cyclicality; the former has close to a monotonically decreasing time trend, and the latter time series has close to a monotonically increasing time trend. Most of the transitions across categories during a recession occur from supersector non-switchers with 0 quarters of non-employment to the supersector switchers with one or more quarters of non-employment. These results provide evidence that displaced workers are more likely to have bad job matches during a recession, since they are more likely to have extended non-employment (in this particular case, at least three months of non-employment), and if they find a job, it is more likely to be within a different supersector (i.e. a job where they will have to perform different functions from what they are used to).

Table 3.3 provides a more explicit relation between earnings losses and these other outcomes. This table contains the shift share analysis described in equation (3), and some clear patterns emerge. The first is non-employment lengths play a very large role in determining earnings loss variation across time. Non-employment length determines anywhere from 34% of the earnings loss variation across time to more than 100% of the variation (which is possible with the way shift shares are calculated), suggesting it is a major component in explaining why earnings losses are so much larger during the Great Recession relative to other periods. The second pattern is that the prevalence of supersector switches explains a good portion of the earnings loss changes as well, ranging from 16-43% of the total changes. The largest effect, however, comes when analyzing how the interaction between supersector switching and non-employment length affects earnings loss variation. This interaction explains anywhere from 42% to more than 100% of the earnings loss variation. This increase in explanatory power suggests that non-employment and job match quality should not be treated as two separate entities, since workers with longer non-employment duration are more likely to end up in a lower quality job after job loss. This relation could be the result of either a depreciation in human capital from not working as stated by Ljunqvist and Sargent (1998), or it could be due to accepting the first job offer after running out of liquidity or unemployment benefits. As an additional test, we also perform the shift share calculation using different demographic characteristics to see if these can explain any of the earnings loss variation across time; None of them contribute nearly as much as non-employment or job match quality.

3.4.4 Does Unemployment Insurance Mitigate Earnings Losses

During recessions, the federal government usually extends unemployment benefits in order to mitigate financial stress for the unemployed. We investigate to what extent benefit expansion helps displaced workers by incorporating unemployment insurance information provided by the CPS March Supplement database. We take our sample of displaced workers and job stayers within the LEHD and merge with the corresponding yearly CPS March Supplement survey.[55] Figure 3.7 presents the average yearly unemployment compensation for our displaced worker sample who is also in the CPS. Figure 3.7 shows that the unemployment compensation amount is higher during the periods with higher earnings losses. However, Figure 3.8 shows that this benefit increase is not enough to compensate for the earnings loss increase during the Great Recession. We still see high losses across periods even after adding the average unemployment benefits to the CPS average earnings losses, and we see that there is still considerable loss variation across time.[56]

[55] The CPS year we use is determined based on the year the displaced worker lost his job / the year the job stayer is included within our sample.
[56] We define the CPS average earnings losses as the average earnings displaced workers had recorded in the CPS minus the average earnings job stayers had recorded in the CPS.

3.5 Conclusion

Clearly earnings losses are higher for displaced workers within the Great Recession relative to the 2001 recession. We find that the differences within the earnings losses are explained more by longer non-employment durations rather than the subsequent poor matches, which we proxy with a supersector switch. However, we find that poor matches explain some of the variation in earnings losses.

While this chapter provides a useful descriptive analysis on earnings losses across different business cycles, future work can analyze this question in a more rigorous manner. Rather than focusing on the supersector of the worker, occupation-specific human capital has been shown to be a very important transferrable human capital component, and it would be useful to see how shocks that affect certain occupations affect workers who lose their jobs. In order to paint a more accurate picture of job match quality, future analysis should incorporate occupation, which is unfortunately not available in panel form within the LEHD.

Additionally, another way to expand upon this work is to see how an increase in displaced workers' earnings losses possibly affects other workers and other outcomes labor economists are interested in. If these shocks that cause job loss are not temporary, would an increase in the prevalence of displaced workers cause a decrease in housing demand within an area, and if so, by how much? How would migration be affected by job loss, and how would this affect potential entrepreneurs who are considering investing within an area across expansions and recessions?

References

Abowd, John M., Bryce E. Stephens, Lars Vilhuber, Fredrik Andersson, Kevin L. McKinney, Marc Roemer, and Simon D. Woodcock. 2009. "The LEHD Infrastructure Files and the Creation of the Quarterly Workforce Indicators." In *Producer Dynamics: New Evidence from Micro Data*, Vol. 68, Studies in Income and Wealth, ed. Timothy Dunne, J.Bradford Jensen and Mark J. Roberts, 149-230. Chicago: University of Chicago Press.

Bureau of Labor Statistics. "BLS spotlight on statistics." February 2012.

Couch, Kenneth, and Dana Placzek. 2010. "Earnings Losses of Displaced Workers Revisited." *American Economic Review* 100(1): 572-589.

Davis, Steven, and Till von Wachter. 2011. "Recessions and the Costs of Job Loss." *Brookings Papers on Economic Activity* 43(2): 1-72.

Fallick, Bruce, John Haltiwanger, and Erika McEntarfer. 2012. "Job-to-Job Flows and the Consequences of Job Separations." Federal Reserve Board Finance and Economics Discussion Series Working Paper #2012-73.

Farber, Henry. "Job Loss in the Great Recession: Historical Perspective from the Displaced Workers Survey, 1984-2010." NBER Working Paper #17040.

Haltiwanger, John, Henry Hyatt, and Erika McEntarfer. 2014. "Cyclical Reallocation of Workers Across Large and Small Employers." Mimeo.

Haltiwanger, John, Henry Hyatt, Erika McEntarfer, Liliana Sousa, and Stephen Tibbets. 2014. "Firm Age and Size In the Longitudinal Employer-Household Dynamics Data" Center for Economic Studies Discussion Paper CES-14-16.

Hyatt, Henry and Erika McEntarfer. 2012a. "Job-to-Job Flows in the Great Recession" *American Economic Review: Papers & Proceedings 2012*, Vol. 102: Iss. 3: 580–583.

Hyatt, Henry and Erika McEntarfer. 2012b. "Job-to-Job Flows and the Business Cycle." Center for Economic Studies Working Paper #12-04, U.S. Census Bureau

Jacobson, Louis, Robert LaLonde and Daniel Sullivan. 1993. "Earnings Losses of Displaced Workers." *American Economic Review*, 83(4): 685-709.

Kambourov, Gueorgui and Iourii Manovskii. 2009. "Occupational Specificity of Human Capital." *International Economic Review*, 50(1): 63-115.

Neal, Derek, 1995. "Industry-Specific Human Capital: Evidence from Displaced Workers." *Journal of Labor Economics*, 13(4): 653-677.

Parent, Daniel. "Industry-Specific Capital and the Wage Profile: Evidence from the National Longitudinal Survey of Youth and the Panel Study of Income Dynamics." *Journal of Labor Economics*, 18(2): 306-323.

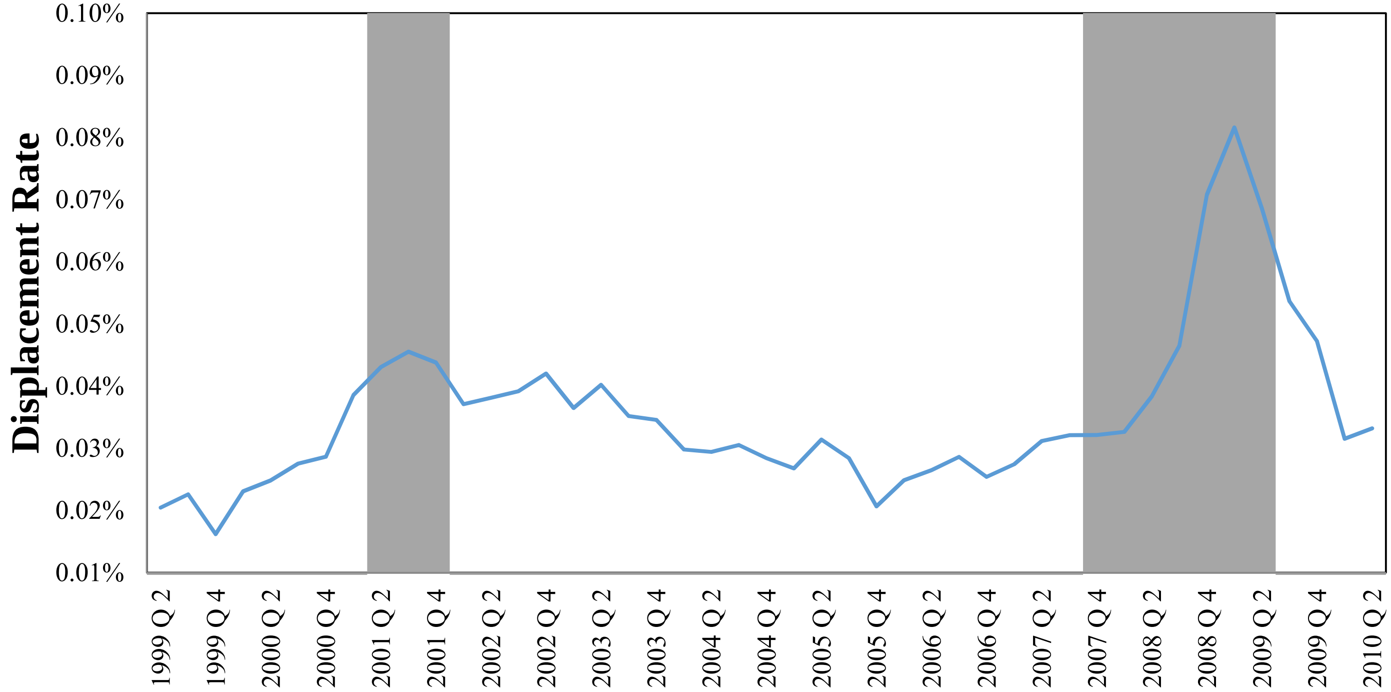
Poletaev, Maxim and Chris Robinson, 2008. "Human Capital Specificity: Evidence from the Dictionary of Occupational Titles and Displaced Worker Surveys, 1984–2000." *Journal of Labor Economics*, 26 (3): 387-420.

von Wachter, Till, Elizabeth Handwerker, and Andrew Hildreth. 2009. "Estimating the 'True' Cost of Job Loss: Evidence Using Matched Data from California 1991-2000. Mimeo.

von Wachter, Till, Jae Song, and Joyce Manchester. 2009. "Long-Term Earnings Losses due to Mass-Layoffs During the 1982 Recession: An Analysis Using Longitudinal Administrative Data from 1974 to 2004." Mimeo.

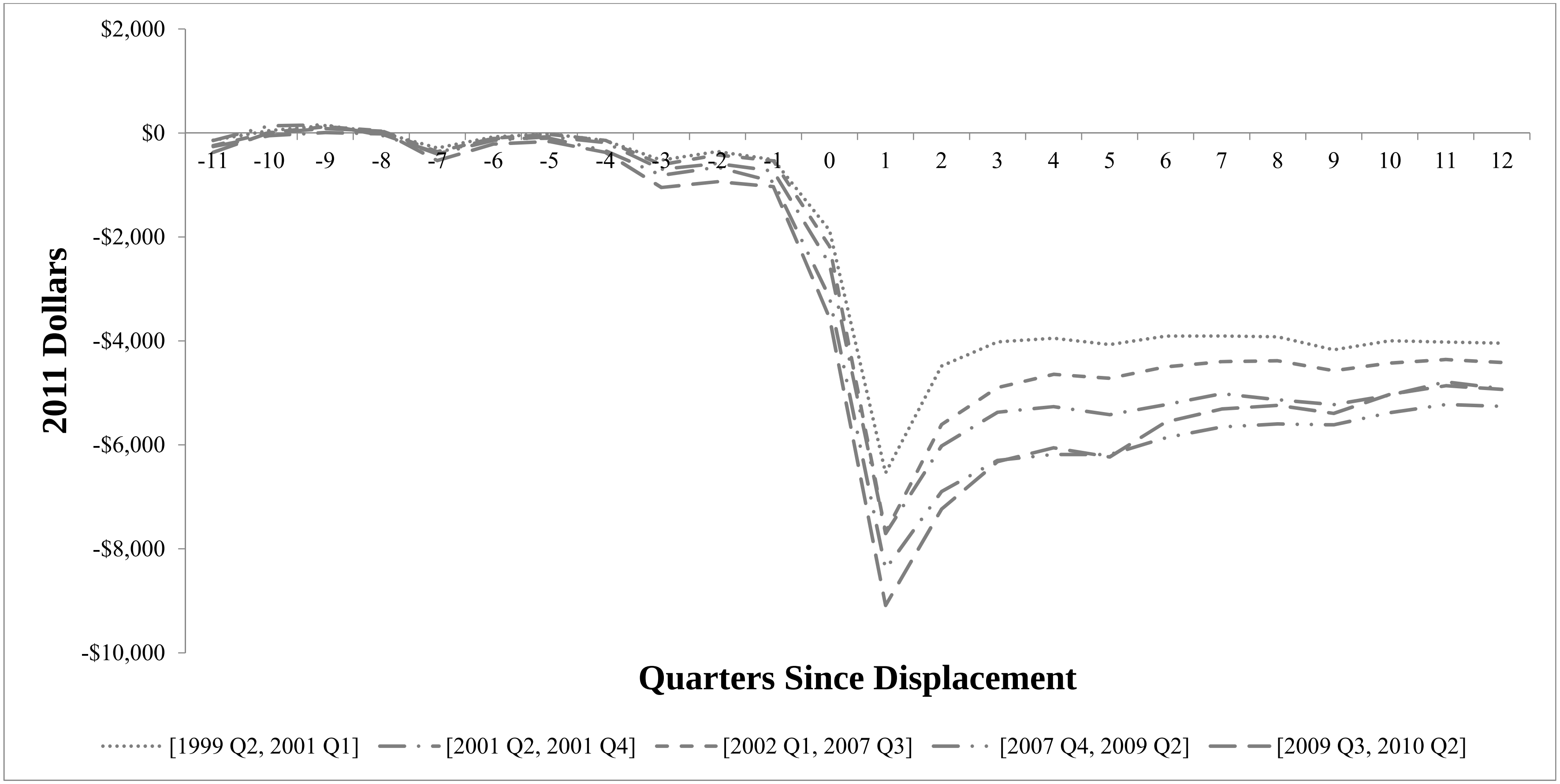
Figures

Figure 3.1: Displacement Rate Time Series



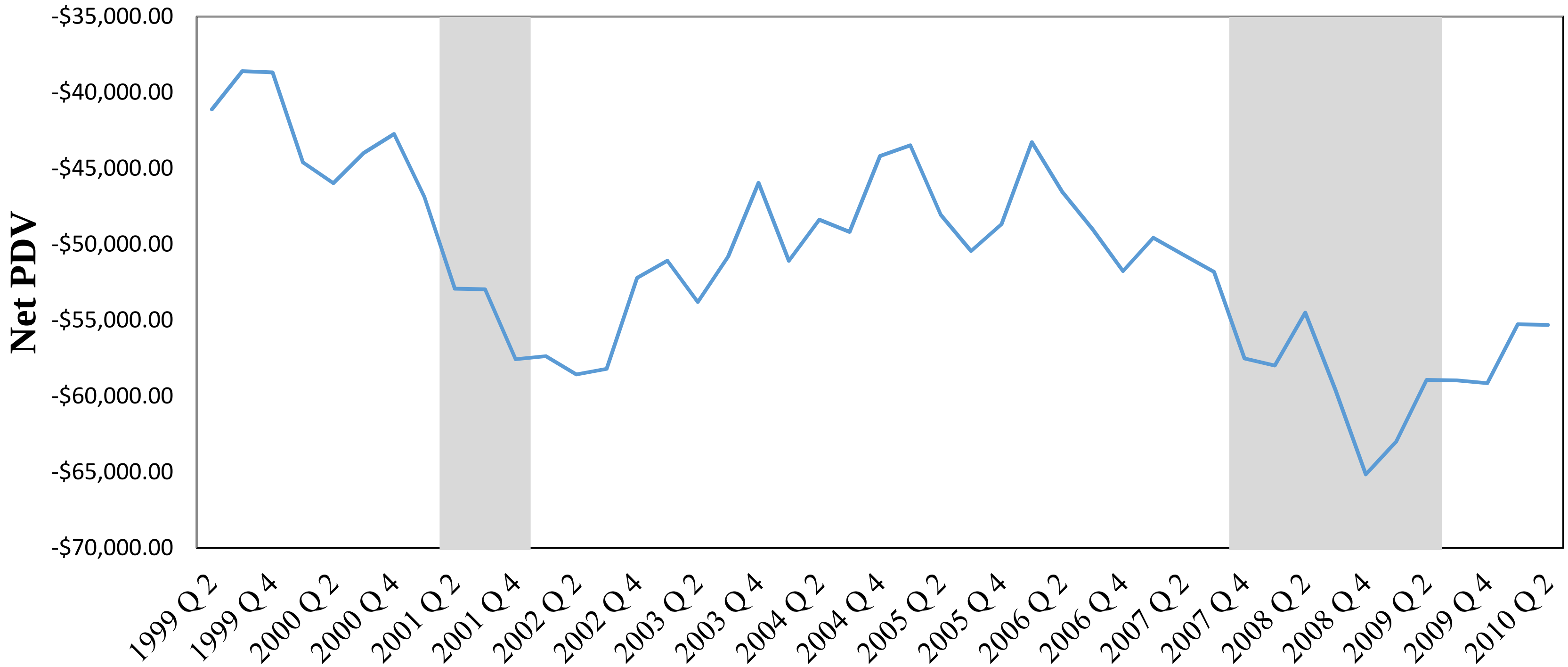
Notes: A worker is defined as displaced if he has a) an observed separation from an establishment when the establishment has more than a 30% decrease in employment during the quarter of interest and b) at least twelve quarters of tenure from that establishment prior to displacement. The denominator is the number of dominant jobs in existence for that quarter. Dominant jobs are defined as the establishment that is associated with the highest earnings for a worker within a given time period. All time series are seasonally adjusted. Only men 35-55 years old are analyzed.

Figure 3.2: Conditional Earnings Evolution - Displaced Workers Relative to Job Stayers



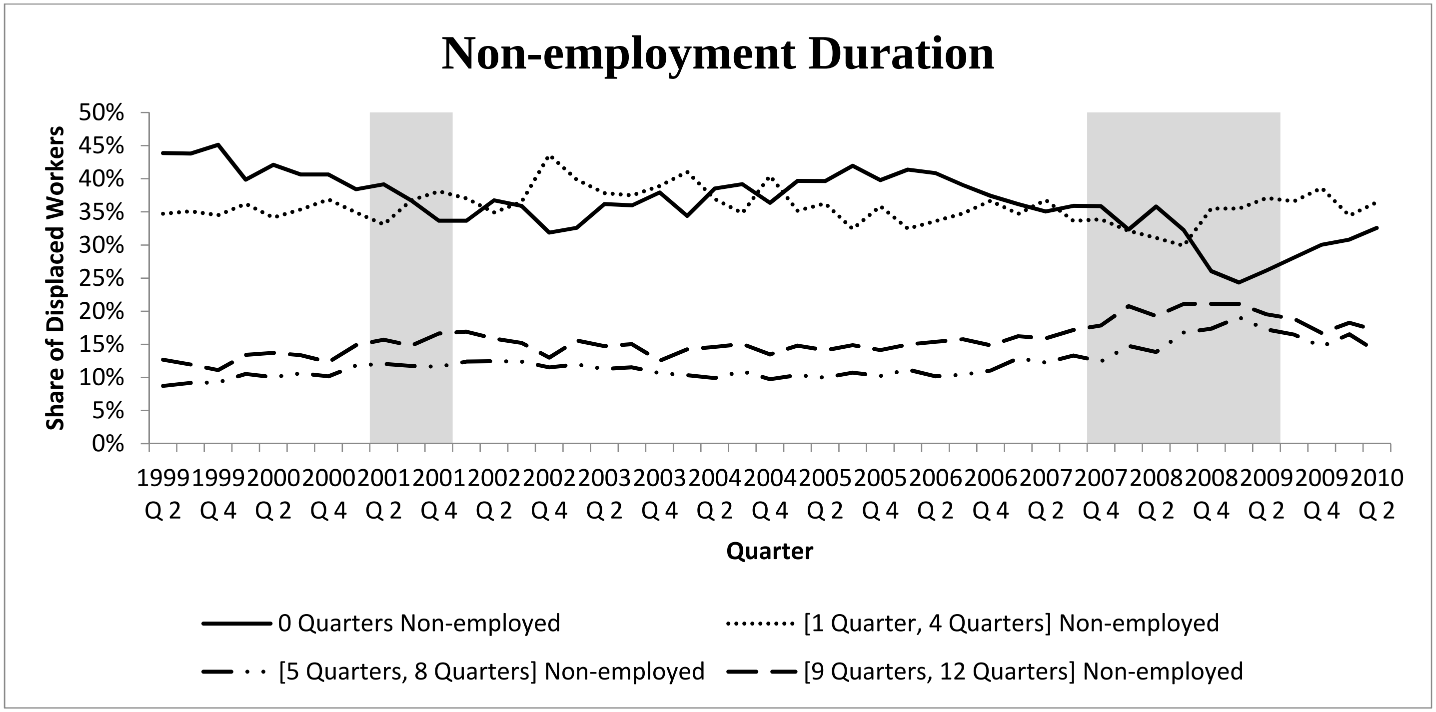
Note: The samples used were the displaced workers and job stayers during the respective periods. For example, the [1999 Q2, 2001 Q1] sample has workers who were classified as either displaced workers or job stayers for any of the quarters from the second quarter of 1999 to the first quarter of 2001. To determine job stayers, we ensured that the worker had 25 consecutive quarters of employment at the same establishment; 12 quarters before quarter t, quarter t, and 12 quarters after quarter t. To determine displaced workers, we used the same methodology as in figure 3.1. These are the estimates of δ_2^k from empirical specification (1). Earnings were deflated by the CPI, and are deflated for all of the figures below. Only men 35-55 years old are analyzed.

Figure 3.3: Conditional Earnings Losses Post-Estimation



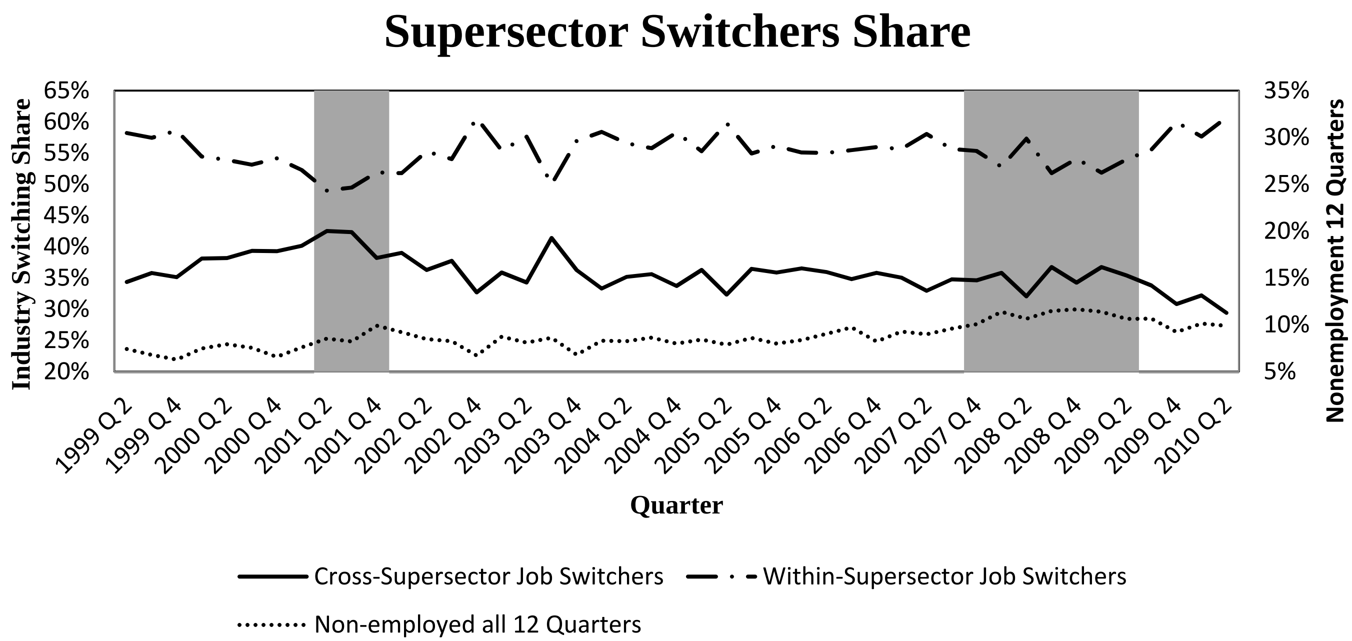
Notes: Empirical specification (1) is run for each quarter from the second quarter of 1999 to the second quarter of 2010. The net PDV is defined post-estimation using empirical specification (2). The net present discounted value is seasonally adjusted.

Figure 3.4: Non-employment Periods for Displaced Workers



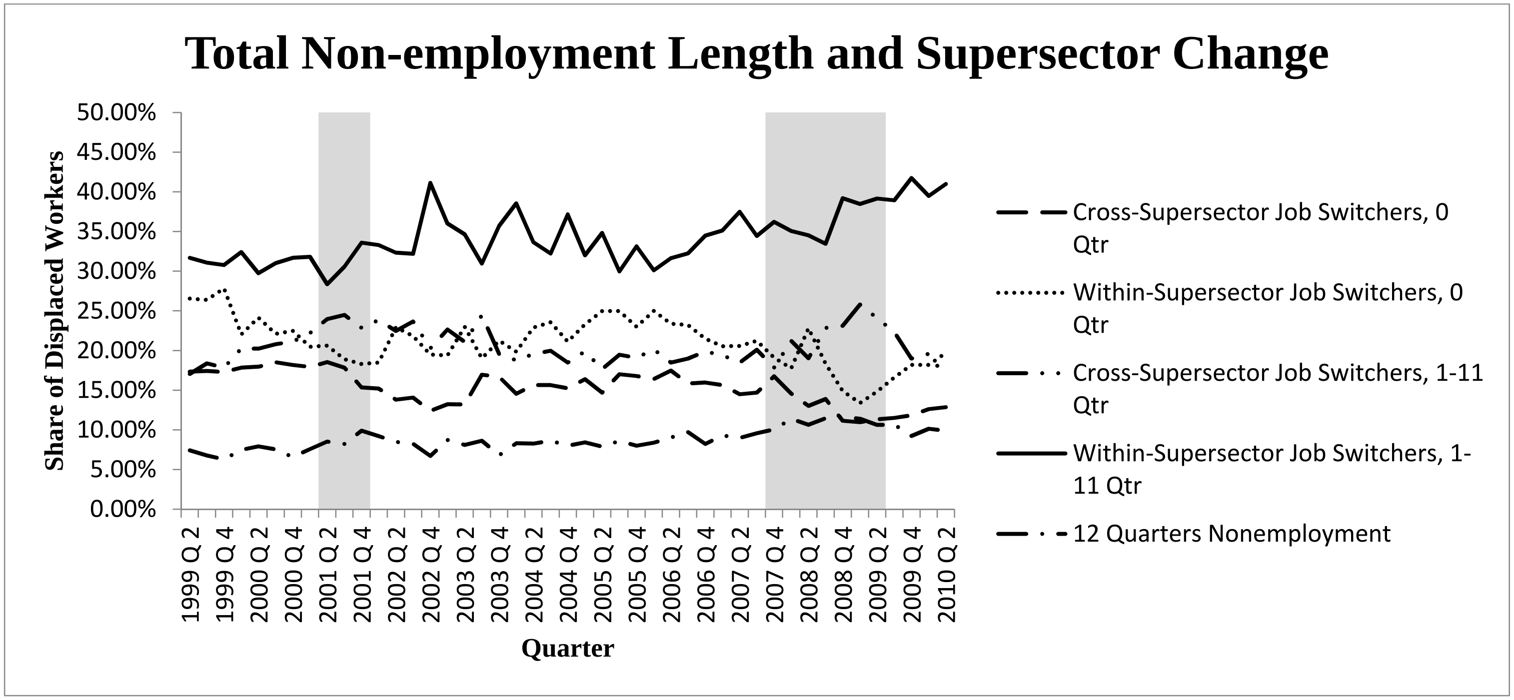
Notes: The denominator is the displaced worker sample for a given quarter. The blue time series represents the share of displaced workers with exactly 0 quarters of non-employment during the subsequent 12 quarters. The red time series represents the share of displaced workers who had between one and four quarters of non-employment, inclusive. The green time series represents the share of displaced workers who had between five and eight quarters of non-employment, inclusive. The purple time series represents the share of displaced workers who had between nine and twelve quarters of non-employment, inclusive. These are mutually exclusive and exhaustive categories, and the shares sum to 1 for a given point in time.

Figure 3.5: Supersector Switchers Share



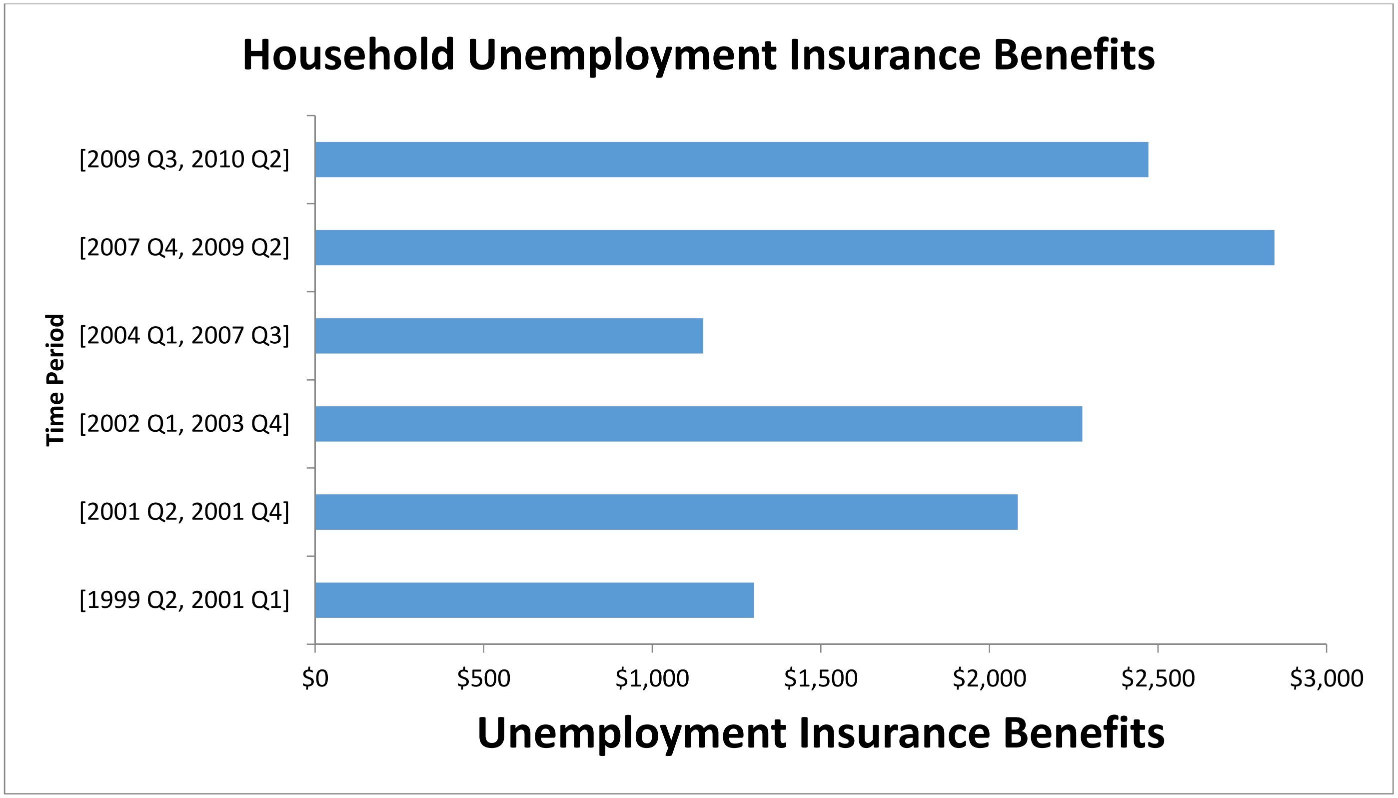
Notes: The denominator is the displaced worker sample for a given quarter. “Cross-Supersector Job Switchers” are displaced workers who were re-employed within a different Supersector from their origin job during any quarter within the three years after a job loss. The “Within-Supersector Job Switchers” are all other displaced workers who were re-employed during any quarter within the three years after a job loss. The “Non-employed all 12 Quarters” cohort are displaced workers who were not employed for every quarter for all three years after a job loss. These are mutually exclusive and exhaustive categories, and the shares sum to 1 for a given point in time.

Figure 3.6: Supersector, Non-employment Combinations



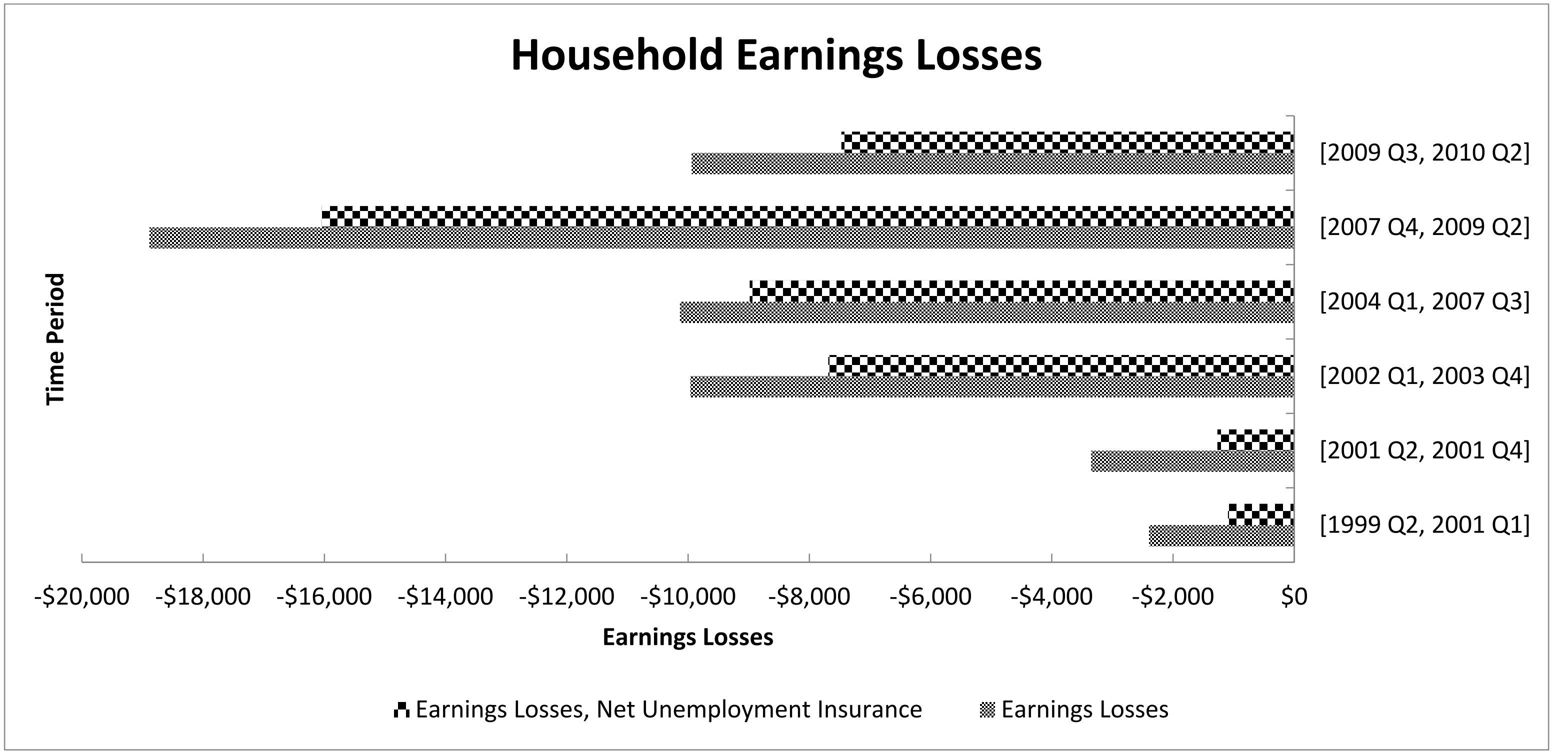
Notes: The denominator is the displaced worker sample for a given quarter. The definitions of "Cross-Supersector Job Switchers" and "Within-Supersector Job Switchers" are provided in figure 3.5. These definitions are partitioned based on the count of non-employment quarters the displaced worker had after a job loss. The purple time series represents the displaced workers who were never re-employed during the subsequent 12 quarters.

Figure 3.7: Household Unemployment Insurance Benefits



Notes: The sample here is the set of households who have at least one displaced worker (as defined by LEHD data) within the CPS. If a worker is displaced during calendar year t, I use the t+1 year for the CPS. For example, if a displaced worker is displaced during the second quarter of 2002, the 2003 CPS is used for him. The unemployment insurance benefits presented are the yearly unemployment benefits received by all displaced workers within a household.

Figure 3.8: Household Earnings Losses



Notes: The sample is the set of households that contain at least one displaced worker or job stayer (as defined within the LEHD) that took part in the CPS for that year. Earnings losses is defined as the difference in the average earnings of households who have at least one displaced worker relative to the same statistic for households with at least one job stayer (and no displaced worker). The Earnings Losses, Net Unemployment Benefits is defined as the sum between the average unemployment benefits and the earnings losses.

Tables

Table 3.1: Demographic Characteristics

	1999 Q2-2001 Q1		2001 Recession		2002 Q1-2007 Q3		Great Recession		2009 Q3-2010 Q2	
	Displaced	Stayers	Displaced	Stayers	Displaced	Stayers	Displaced	Stayers	Displaced	Stayers
Age										
35 to 40	31%	29%	29%	27%	28%	25%	27%	24%	26%	24%
41 to 45	26%	27%	26%	27%	26%	26%	24%	24%	24%	24%
46 to 50	24%	25%	24%	25%	25%	27%	26%	27%	27%	27%
51 to 55	19%	20%	21%	20%	21%	22%	23%	25%	24%	26%
Race/Ethnicity										
White, Not Hispanic	77%	81%	77%	81%	74%	79%	70%	77%	70%	75%
Black, Not Hispanic	8%	6%	9%	7%	10%	8%	10%	8%	9%	8%
Asian, Not Hispanic	3%	3%	3%	3%	3%	3%	3%	4%	4%	4%
Other, Not Hispanic	1%	1%	1%	1%	1%	1%	1%	1%	1%	1%
Hispanic	12%	9%	10%	8%	12%	9%	16%	11%	16%	12%
Education										
< High School	13%	9%	13%	9%	13%	9%	16%	10%	15%	10%
High School	29%	27%	31%	28%	30%	28%	33%	28%	31%	28%
Some College	30%	31%	29%	31%	30%	31%	30%	31%	30%	31%
College	29%	33%	27%	32%	27%	32%	22%	31%	24%	31%
Count	178,330	1,356,654	110,877	651,075	598,702	5,575,129	289,818	1,879,251	123,228	1,101,161

Notes: The displaced workers and job stayers are defined in the same way as within the figures. Age is defined as the age when the job loss occurred.

Table 3.2: Firm Characteristics

	Pre-2001 Recession		2001 Recession		2002 Q1-2007 Q3		Great Recession		2009 Q3-2010 Q2	
	Displaced	**Stayers**	**Displaced**	**Stayers**	**Displaced**	**Stayers**	**Displaced**	**Stayers**	**Displaced**	**Stayers**
Supersector										
Natural Resources and Mining	5%	2%	2%	2%	3%	2%	3%	2%	3%	2%
Construction	10%	6%	11%	6%	15%	7%	20%	7%	24%	6%
Manufacturing	28%	26%	34%	26%	26%	24%	30%	21%	23%	21%
Trade, Transportation, and Utilities	20%	23%	19%	23%	19%	24%	17%	24%	16%	24%
Information	3%	3%	4%	3%	3%	3%	2%	3%	2%	3%
Financial Activities	4%	4%	4%	5%	4%	5%	4%	5%	5%	5%
Professional and Business Services	14%	8%	15%	9%	14%	10%	13%	10%	13%	11%
Education and Health Services	8%	13%	5%	13%	9%	12%	5%	13%	8%	13%
Leisure and Hospitality	5%	3%	4%	3%	5%	3%	4%	4%	5%	4%
Other Services	2%	2%	1%	2%	2%	2%	1%	2%	1%	2%
Government	2%	10%	1%	10%	1%	9%	1%	9%	1%	9%
Firmsize										
x>=500	29%	50%	28%	48%	26%	47%	23%	46%	21%	45%
50<=x<500	51%	30%	51%	31%	50%	32%	53%	32%	51%	32%
50>x	20%	20%	21%	21%	23%	22%	24%	22%	29%	23%
Count	178,330	1,356,654	110,877	651,075	598,702	5,575,129	289,818	1,879,251	123,228	1,101,161

Notes: Supersector refers to the job stayer's firm's Supersector, or the Supersector of the firm where the displaced worker lost his job. Firm size is defined analogously.

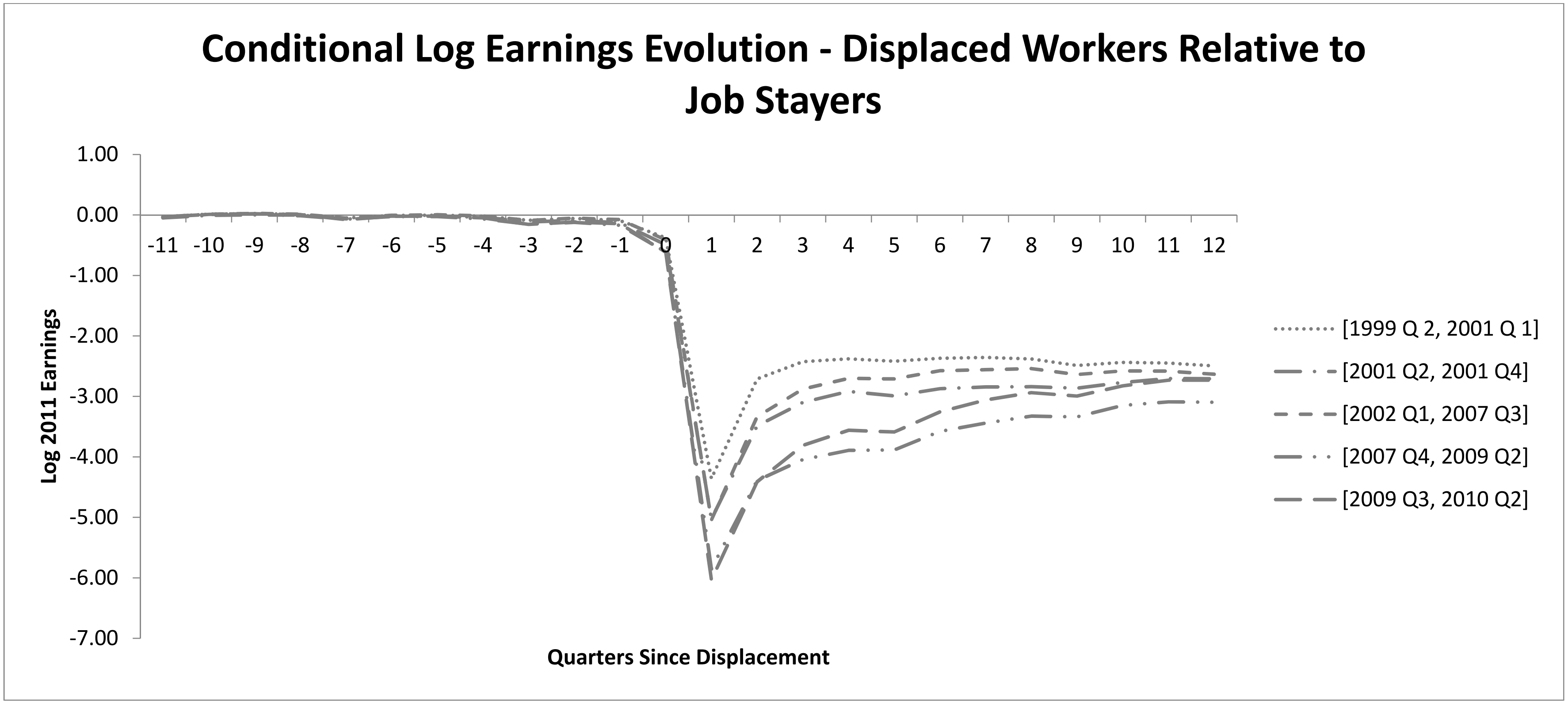
Table 3.3: Shift Share Analysis

Category	[2001 Q2, 2001 Q4]	[2002 Q1, 2007 Q3]	[2007 Q4, 2009 Q2]	[2009 Q3, 2010 Q2]
Age	2%	-2%	1%	-4%
Education	0%	-3%	-3%	-8%
Race-Ethnicity	2%	6%	-2%	0%
Non-employment	48%	34%	110%	108%
Supersector Change	16%	22%	23%	43%
Non-employment, Supersector Change	50%	42%	110%	111%
Initial Supersector	6%	20%	14%	-10%

Notes: The shift share is calculated using equation (3). The baseline (t-1) period used here is [1999 Quarter 2, 2001 Quarter 1]. The outcome of interest is the Net PDV as defined by equation (2). The shares are the share of the difference in the Net PDV across time explained by the category. For age, the brackets used were [35, 39], [40, 44], [45, 49], and [50, 55]. For education, the categories used were less than high school completion, only high school completion, some college, and college completion. For race-ethnicity, the categories used were Hispanic, White and not Hispanic, Black and not Hispanic, Asian and not Hispanic, Other and not Hispanic. For initial Supersector, I used the BLS defined Supersectors as the separate categories (Natural resources and mining, Construction, Manufacturing, Trade Transportation and Utilities, Information, Financial Activities, Professional and Business Services, Education and Health Services, Leisure and Hospitality, Other Services, Government). For non-employment duration, the categories used were each quarter of potential values for non-employment within the subsequent 12 quarters (so thirteen values from 0 to 12, inclusive). For Supersector change, the categories used were Supersector change, no change in the Supersector, and twelve quarters of non-employment. For Supersector change interacted with non-employment duration, the categories used are each of the first 12 values of the possible non-employment duration values interacted with either changing Supersector or not (so 24 categories), as well as twelve quarters of non-employment as another category (so 25 categories total).

Appendix Figures and Tables

Appendix Figure 3.1: Conditional Log Earnings Evolution - Displaced Workers Relative to Job Stayers



Note: The cohorts used within this figure are the same as the cohorts used within the level regressions. The dependent variable used here is the natural log of earnings. We added the value of 1 to all of the earnings within the dataset.

Appendix Table 3.1: Baseline Regression Estimates, Earnings Losses

Quarters Since Displacement	[1999 Q2, 2001 Q1]	[2001 Q2, 2001 Q4]	[2002 Q1, 2007 Q3]	[2007 Q4, 2009 Q2]	[2009 Q3, 2010 Q2]
-11	-$139	-$262	-$243	-$146	-$372
-10	$46	-$51	$6	$137	$5
-9	$143	$7	$120	$156	$85
-8	$11	-$18	$44	-$43	$35
-7	-$290	-$404	-$360	-$380	-$531
-6	-$75	-$142	-$101	-$108	-$213
-5	-$17	-$69	-$27	-$102	-$165
-4	-$144	-$185	-$141	-$341	-$373
-3	-$523	-$699	-$610	-$815	-$1,047
-2	-$358	-$588	-$423	-$659	-$938
-1	-$514	-$733	-$536	-$937	-$1,031
0	-$1,873	-$2,522	-$2,183	-$3,167	-$3,557
1	-$6,539	-$7,707	-$7,680	-$8,391	-$9,089
2	-$4,484	-$6,021	-$5,609	-$6,898	-$7,237
3	-$4,019	-$5,372	-$4,897	-$6,299	-$6,322
4	-$3,948	-$5,265	-$4,644	-$6,183	-$6,055
5	-$4,071	-$5,420	-$4,718	-$6,187	-$6,233
6	-$3,909	-$5,226	-$4,499	-$5,864	-$5,560
7	-$3,904	-$5,014	-$4,400	-$5,654	-$5,310
8	-$3,923	-$5,130	-$4,382	-$5,597	-$5,241
9	-$4,171	-$5,226	-$4,573	-$5,614	-$5,395
10	-$3,999	-$5,045	-$4,425	-$5,380	-$5,028
11	-$4,022	-$4,787	-$4,359	-$5,224	-$4,864
12	-$4,043	-$4,921	-$4,416	-$5,258	-$4,933
Earnings Losses	-$43,587	-$54,844	-$50,829	-$60,020	-$58,150

Notes: These are the estimates displayed within figure 3.2. Positive values of "Quarters Since Displacement" represent the number of quarters after job loss, negative values represent the number of quarters before job loss, and 0 indexes the quarter of job loss. All point estimates after "Quarters Since Displacement" = 0 (inclusive) are significant at the 5% level when using OLS standard errors. The earnings losses statistic displayed is calculated from empirical equation (2). The cohorts used were the displaced workers and job stayers during the respective periods. For example, the [1999 Q2, 2001 Q1] cohort have the people who were displaced during any of the quarters from 1999 Quarter 2 to 2001 Quarter 1 (inclusive) and the people who were considered job stayers during any of the quarters from 1999 Quarter 2 to 2001 Quarter 1 (inclusive). To determine job stayers, we ensured that the worker had 24 consecutive quarters of employment at the same establishment; 12 quarters before quarter t and 12 quarters after quarter t. To determine displaced workers, we used the same methodology as in figure 3.1. These are the estimates of β_2^k from empirical specification (1). Earnings were deflated by the CPI, and are deflated for all of the figures below.

Chapter 4: Interstate Migration and Job-to-Job Flows

(Co-Authored with Henry Hyatt, Erika McEntarfer, and Alexandria Zhang)

4.1 Introduction

When households move to a new state, they commonly change employers. This economic migration generally results in better outcomes for workers, who often receive pay increases when changing jobs.[57] Recent work by Hsieh and Moretti (2016) argues that migration plays a key role in placing workers in high productivity areas, leading to greater economic growth, and studies by Dahl (2002) and Wozniak (2010) show that labor markets that offer higher wages attract more migrants than those that offer lower wages. Because of the importance of population reallocation in the U.S., recent studies such as Molloy, Smith, and Wozniak (2014) and Kaplan and Schulhofer-Wohl (2015) have explored an apparent decline in the interstate migration rate in recent decades. Moreover, because people often switch jobs when they move to a different state, some studies including Hyatt and Spletzer (2013) and Molloy et al. (2016) have speculated that changing interstate migration may be part of a broader phenomenon of slowing of U.S. labor market dynamics, which exhibited a sharp decline starting in 2000.[58]

In this chapter, we explore the relationship between job switching and interstate migration for the working-age population, taking advantage of recently available administrative records data. We show that the timing and extent of changes in interstate migration are different from those of job-to-job flows, the latter of which

[57] See Topel and Ward (1992) and Hyatt and McEntarfer (2012).
[58] See, among many others, Lazear and Spletzer (2012), Hyatt and Spletzer (2013), and Davis and Haltiwanger (2014).

exhibits its most dramatic decline after the year 2000. We also document that there are considerable differences across data sources on the timing and magnitude of changes in interstate migration, while there is substantial agreement across data sources on the trends in job change. We explore matched survey and administrative data, and although we can explain some of the overall disagreement between data sources, we make little progress in explaining divergent trends. There is considerably more agreement on the recent trend in job-to-job flows between different data sources, and we also find that economic migration is associated with a similar share of overall interstate migration in survey and administrative records data sources. We also explore changing demographics and employer composition, as well as the earnings changes when workers move across states, in understanding recent changes to interstate migration.

We present evidence from a number of data sources on interstate migration and job-to-job flows for the U.S., including both household surveys and administrative records. Our most distinctive contribution is we are able to explore the evolution of interstate migration using administrative records from the U.S. Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) program, a topic that has been explored in some detail in previous work using the same data (Goetz 2013). The LEHD data permit calculation of the interstate migration at an annual frequency from the year 2000 onwards, and we have a longitudinally consistent series for 2000-2010. The LEHD data also contain rich data on job-to-job flows, see Hyatt et al. (2014), so it is possible to identify the subset of people who change their state of residence at the same time as they change employers. We also employ a number of data sources that have been considered in previous studies of interstate migration rates and job-to-job flows. We use the Current Population Survey (CPS), the most commonly used dataset for the study of job-to-job flows, as well as interstate migration. The LEHD data have also been linked to the CPS, which permit a comparison of survey with administrative records data. In addition to the LEHD and CPS, we also consider interstate migration from published aggregates of interstate migration from the Internal Revenue Service (IRS) Statistics of Income, which, like the LEHD data, are of administrative records, as well as survey data from the American Community Survey (ACS).

We begin our empirical analysis by documenting the trend in interstate migration for the working age population. We present a simple comparison of the trends of these different sources. Although previous studies such as Molloy, Smith, and Wozniak (2011) and Kaplan and Schulhofer-Wohl (2012) have included comparisons of different data series on interstate migration, there have been no satisfactory explanations for the substantial divergence in levels and trends between these different data sources. From the 1980s to 2000, the IRS and CPS both show declines that are concentrated in the 1990s, however, the CPS and IRS are noticeably different in the years following 2000, with the CPS showing a strong decline but the IRS not. This post-2000 decline appears after dropping imputed and allocated values following Kaplan and Schulhofer-Wohl (2012), and if we had not dropped such observations, the post-2000 decline would have been much stronger. To better understand what happened to interstate migration rates from the year 2000 onward, we consider interstate migration rates that are also available from the ACS and

LEHD, and these two data sources are quite similar to the IRS, and show modest procyclicality without a strong decline.

To better understand the idiosyncratic post-2000 decline in the CPS, we conduct an analysis using CPS microdata linked with LEHD administrative records, and explore the frequency with which migration is report in the CPS, LEHD, or both. This is important because the CPS is the data source of reference on papers that consider the decline in interstate migration such as Molloy, Smith, and Wozniak (2011, 2014) and Kaplan and Schulhofer-Wohl (2015). The matched CPS-LEHD dataset shows a similar migration rate to both the overall CPS and LEHD migration rates, suggesting that the difference in interstate migration rates is a measurement issue rather than an indication of which households are present in administrative records data relative to survey data. We also make some progress in understanding, at the person level, how survey responses and administrative records disagree. Some of the differences are due to different timing in the CPS relative to the LEHD (residential migration generally appears later in administrative data than in survey responses), as well as the greater tendency of the LEHD data to have moves to a state and back to the original state after exactly one year. However, these explanations do not have an evident time trend and so we can only rule out these as potential explanations for why the post-2000 decline in the interstate migration rate is so much larger in the CPS than in other data series.

Our data also allow us to account for the relationship between interstate migration and job change. The CPS and LEHD allow us to measure the frequency with which workers switch jobs. These same data sources allow us to distinguish economic migration from other forms of migration. Our descriptive analysis yields a few conclusions. First, we see substantial agreement between the CPS and LEHD in the extent and cyclicality of job-to-job transitions. Second, we note that workers change jobs several times more frequently than people change their state of residence. Third, the LEHD data provide results consistent with the existent evidence from the CPS that on the order of one-third of all interstate migration is associated with job change. Fourth, changes in economic migration account for somewhat more of the (albeit smaller) decline in the LEHD interstate migration compared to that of the CPS.

Our LEHD data allow us to examine in detail the changes in interstate migration and economic migration from 2000 to 2010. We use the LEHD data to conduct two exercises. First, we compute shift-share decompositions that allow us to understand the role of the aging of the U.S. population, changing industry mix, and other composition aspects of employers and the workforce. Whenever the data permit, we also compute corresponding estimates from the CPS. Overall, our findings are similar to Molloy et al. (2016) who find that the ageing of the population can explain some of the change in interstate migration, but that changes in the industry composition in the U.S. have an offsetting effect.

Second, we measure the earnings of workers before and after changing their state of work and state of residence. We document that the earnings changes that are associated with interstate migration are lower than those associated with job change overall, but much higher than those changes experienced by those who do not switch jobs. We also show that these earnings changes are procyclical, dropping during and after the 2001 and 2007-2009 recessions. This analysis shows how the LEHD data permit a time series that the CPS cannot: because the CPS does not track movers, it is only possible to use that data source to measure earnings after migration, and not before, so it is not possible to measure the earnings changes associated with interstate migration using the CPS alone.

This chapter proceeds as follows. In the next section, we describe the data sources used in this chapter. We then present evidence on trends in residential migration, comparing the LEHD administrative data to the more widely available survey data and conducting an analysis on linked survey and administrative records data. We then consider shifts in job mobility and the role of economic migration and its relationship to changes in interstate migration. Next, we conduct shift-share decompositions of the declines in the residential and economic migration rates to quantify the role of observable and unobservable factors in explaining these declines. Thereafter, we explore the role of earnings changes associated with staying in a job or moving to another job, including those associated with residential moves. A brief conclusion follows.

4.2 Data

We utilize several data sources on interstate migration and job-to-job flows. We start with a description of the most commonly used data source, the Current Population Survey (CPS), then discuss the American Community Survey (ACS). In our analysis of both the CPS and ACS, we restrict our sample to the civilian population aged 16 to 64 (in the ACS analysis we also exclude the group quarters sample), the same age restriction imposed on the LEHD sample. We have the most data for the years 2000 to 2010, so in the sections that follow, we pay more attention to that interval. However, we also put those years in the broader context of the 1980s to 2014 as data permit.

4.2.1 Current Population Survey (CPS)

Because of its long time series and public-use microdata, the CPS is the most commonly used dataset for the study of interstate migration, as well as for job-to-job flows. Interstate migration in the March CPS (Annual Social and Economic Supplement (ASEC) asks respondents where they lived one year ago.[59] It is possible to use this data source to create a time series that spans several decades, however, in order to draw conclusions about the post-2000 trend in interstate migration, it is important to note that CPS data processing has changed over time. As recommended by Kaplan and Schulhofer-Wohl (2012), we exclude respondents with imputed migration responses, which change discontinuously in the early 2000s for reasons that have little or nothing to do with actual interstate migration.[60] The CPS collects information on an individual's reason for migrating, one of which is starting or transferring to a new job, which has been analyzed by both Molloy et al. (2011) and Kaplan and Schulhofer-Wohl (2015) and so serves as the main measure of economic migration from the CPS.

[59] We use the CPS March supplement (ASEC) data from the University of Minnesota's Integrated Public Use Microdata Series, see Ruggles et al. (2015).

[60] In excluding imputed responses, it is also necessary to drop individuals with migration responses assigned from the householder (i.e., primary respondent) if the householder's response is imputed. These assigned values also inflate the migration rate considerably, for spouses in particular.

Job-to-job transitions can be measured from the CPS ASEC, as well as the monthly CPS. The ASEC began asking a retrospective question on the number of jobs a respondent held in the last year starting in 1976. If the respondent reports working for more than one employer in the last year, this suggests that the worker experienced a job-to-job transition, and the time trend of this proxy for job-to-job transitions has been analyzed by Farber (1999), Stewart (2007), Hyatt (2015), and Molloy et al. (2016). In addition to this proxy measure, it is possible to measure the job-to-job flow rate directly from the monthly CPS data, following Fallick and Fleischman (2004).[61] We use both the annual and monthly job-to-job transition rates from the CPS as a comparison to the job-to-job flow series we construct from the LEHD data, which is the only other data source in our analysis that has job-to-job flow information.

It is important to note that the CPS does not track movers, and so does not permit comparisons of earnings before and after a job-to-job transition. Unlike the LEHD, it does not permit the measurement of the returns to economic migration. Therefore, while we are able to compare job-to-job flow rates across the two databases, we are unable to compare returns to economic migration across the two databases.

[61] We obtain the CPS monthly data from NBER at http://www.nber.org/data/cps_basic.html (last accessed: May 18, 2016).

4.2.2. American Community Survey (ACS)

The American Community Survey is a large nationally representative cross-sectional survey of the United States.[62] To identify migrants, the ACS asks a retrospective question that is similar to the one asked in the CPS. There are some notable differences between the two surveys, however. First, the ACS pursues nonrespondents more intensively than the CPS (Koerber 2007). As people who move more frequently may be less likely to respond to surveys, migration rates in the ACS may be higher than the CPS. Second, the ACS asks respondents about their current address, not their usual address (the concept used in the CPS). This largely impacts college students who, in the ACS, are primarily in the group quarters sample (which we exclude). In the CPS, households that include college students residing elsewhere are instructed to include them in the household. The ACS spans 2001 to present, and provides migration information as well as demographic characteristics, although it neither asks people their reason for moving, nor does not collect information on job-to-job transitions, and so we are unable to measure the rate of economic migration using the ACS.[63] We also obtain ACS data for the year 2000 from the Census 2000 Supplemental Survey, the precursor to the ACS.[64]

---

[62] We download ACS data for the year 2001 onward from the iPUMS website at https://usa.ipums.org/usa/ (last accessed: May 18, 2016), see Ruggles et al. (2015).

[63] Kaplan and Schulhofer-Wohl (2012) exclude pre-2005 ACS data from their study due to concerns about changes in survey methodology that occurred between 2001 and 2005, the year in which it reached its final size as a 1-in-60 sample of the U.S. population. However, we include estimates for pre-2005 years here as the ACS migration rate does not appear to us to change for reasons that can be demonstrably related to changes in survey methodology.

[64] Migration in the ACS for 2000 is calculated using data rom http://www2.census.gov/programs-surveys/acs/data/pums/2000/.

4.2.3 Internal Revenue Service (IRS)

In line with recent studies, we also use data provided by the IRS's Statistics of Income program to calculate gross interstate migration rates.[65] As mentioned above, we are able to produce a longer time series with these data that is comparable to the length of the CPS series. Moreover, the IRS migration rates are calculated from year-to-year changes in state of residence on individual tax returns so they should track the LEHD data quite closely, and indeed they do as we will see in the next section. Specifically, the number of personal exemptions on each tax return is used to approximate the number of individuals (versus households).

4.2.4 Longitudinal Employer-Household Dynamics (LEHD)

We use linked employer-employee data from the LEHD program at the U.S. Census Bureau to examine the connection between trends in declining job mobility and declining residential migration. The LEHD data consist of quarterly worker-level earnings submitted by employers for the administration of state unemployment insurance (UI) benefit programs, linked to establishment-level data collected for the Quarterly Census of Employment and Wages program. This core linked employer-employee data is merged with Census survey and administrative data for additional information on workers and employers. As of this writing, all 50 states and the District of Columbia have shared these data as part of the Local Employment

[65] Recent years (1990-2014) of IRS data are available from the IRS website at https://www.irs.gov/uac/soi-tax-stats-migration-data (last accessed: May 18, 2016). Data for earlier years were downloaded from the website of the National Archives, and although there is not a main page with liks to these files, they can be downloaded by following the URL that stores a query for these data, see https://catalog.archives.gov/search?q=*:*&rows=20&tabType=all&facet=true&facet.fields=oldScope,level,materialsType,fileFormat,locationIds,dateRangeFacet&highlight=true&f.parentNaId=646447&f.level=fileUnit&sort=naIdSort%20asc (last accessed: May 18, 2016).

Dynamics federal-state partnership. LEHD data coverage is quite broad; state UI covers about 95% of private sector employment, as well as state and local government.[66] The LEHD data has also been linked to the CPS, allowing for a person-level comparison of interstate migration as captured by administrative records with responded migration obtained from survey data.

Part of the LEHD data infrastructure is an annual residential file called the Composite Person Record (CPR). We use this file to identify migrants in the LEHD data. The CPR is constructed from multiple administrative data sources, including some federal program data and federal tax filings, see Leggieri, Pistiner, and Farber (2002). Using tax data to study migration has some disadvantages, such as the ability of filers to file early or late also means any change in residential location between tax years could have taken place anytime between two filing windows. Given this limitation, we use the following assignment rule to time migrations in LEHD. Because most households file their returns between January and early April, and the seasonal pattern of migration is such that winter months have the lowest migration rates, if the residential address associated with an individual's tax return changes from the previous year its subsequent year, we label the migration as a subsequent year move. Unfortunately, a longitudinally consistent CPR is only available until 2010, and so our primary analysis of the LEHD trend in interstate migration must end then.[67] The CPR is constructed from data that is similar to the input data for the IRS

[66] For an overview of the LEHD data, see Abowd et al. (2009).

[67] A somewhat differently constructed 2011 CPR allowed the calculation of a migration rate comparing 2010 and 2011. We have less confidence in this data point, and so although we include it in our first figure for completeness, we do not attempt to explain it in subsequent results or analyses. A replacement residential file for LEHD has been constructed from tax records for later years, which is called the LEHD Residence Candidacy File (RCF) and is available for years 2012 and 2013. Comparing the 2011 CPR to the 2012 RCF is difficult to interpret, but the migration rate implied by

Statistics of Income public-use migration series, which accounts for the similarity between these series in the results that follow.

The LEHD data allow us to identify job movers who also had a residence change at the time of the job change. One objective of this paper is to evaluate role of job-to-job flows in explaining changes in the interstate migration rate. To identify these job-to-job flows in the LEHD data, we link the main jobs in each quarter of a worker's employment history.[68] When a worker separates from a job and begins work at a new job within a short time period (starting work at the new employer in either the same quarter or the next), we classify this move as a job-to-job flow.[69] Transitions with longer periods of nonemployment (at least one full-quarter of zero earnings) are not considered job-to-job flows, but flows to and from employment.

In this paper, we define a cross-state migration in the LEHD data as an economic migration as follows: if we observe a person to have both an interstate residential move and a cross-state job to job flow during the same pair of years, we assume that the job change motivated the move. This approach has a few limitations. A worker who has been long unemployed in one location who moves to a better labor market to find work would not be classified as making an "economic" move under our definition (this is, however, one of the least cited reasons for making a residential

---

this is 2.5%. Comparing the 2012 RCF to the 2013 RCF yields a migration rate of 2.6%, which is similar to the IRS migration rate that compares those two years.

[68] Linking all main jobs (defined as the employer at which a given worker had maximal earnings in a given quarter) in a worker's employment history is also used to identify job-to-job flows in Hyatt and McEntarfer (2012) and Haltiwanger et al. (2015). The new Census Job-to-Job Flows statistics use a slightly different methodology, linking main jobs held on the first day of each quarter (and so taking the job with maximum earnings from summing earnings in the two respective quarters), see Hyatt et al. (2014).

[69] Specifically, between employers measured at the level of the state Unemployment Insurance account. These accounts can and often do contain multiple establishments, so within-state transfers between establishments are not counted as job-to-job flows. In contrast, transfers within a firm but across states are counted as job-to-job flows, as the accounts are state-specific.

move in the CPS). Similarly, a trailing spouse who does not work (either voluntarily or involuntarily) in either the origin or destination market would also not be classified as making an economic move, even if the move was motivated by a new job for the household head. Consequently, we will undercount moves driven by relative job opportunities. We will also misclassify some moves as being economic that are driven by family reasons or relative amenities, if the migrant moves very quickly between jobs.

LEHD residence data is available for the entire U.S. back to the year 1999. Therefore, we calculate national interstate migration rates for the year 2000 (which is a comparison of the year 2000 state of residence with the 1999 state of residence) onward. However, because employment data from different states become available in different years, when considering economic migration in the LEHD, we make a state restriction on both the origin and destination of the residential move and the job flow (see Henderson and Hyatt 2012). Specifically, we only look at the set of states with complete data beginning in 1999.[70] Although this additional restriction induces a level shift downward in our overall and economic migration rate, the changes in the interstate migration rate match those of the national series.

Despite the constraints of the LEHD data, it offers a number of advantages over survey data. As noted previously, the coverage of the data is expansive and thus gives us a much larger sample size than the CPS or ACS. Additionally, survey data often contain measurement error that arises from self-reporting, but the LEHD data is

[70] These 34 states are California, Colorado, Connecticut, Delaware, Florida, Georgia, Hawaii, Idaho, Illinois, Indiana, Iowa, Kansas, Louisiana, Maine, Maryland, Minnesota, Missouri, Montana, Nevada, New Jersey, New Mexico, North Carolina, North Dakota, Oregon, Pennsylvania, Rhode Island, South Carolina, South Dakota, Tennessee, Texas, Virginia, Washington, West Virginia, and Wisconsin. These states account for about 74% of the U.S. population in 2010.

free from this particular type of error. A third advantage of the LEHD data is that it allows us to identify earnings changes associated with a job transition and thus test the hypothesis that the decline in migration has been driven by declining returns to migration.

4.3 Interstate Migration in the U.S. 1981-2014

4.3.1 Comparison of Interstate Migration Rates in LEHD, CPS, ACS, and IRS Data

We begin by comparing the rates of residential migration in these different data series. This is a useful starting point, but some caution is warranted when comparing these data because these series do change at different times and for different reasons. Recent changes to these data series are reasonably well documented. For example, thanks to Kaplan and Schulhofer-Wohl (2012) it is known that substantial changes occur to the edit and imputation procedures applied to the CPS which make the decline look even larger than the series we present. Also the IRS changed its methodology between 2011 and 2012 and the interstate migration rate jumps between these years.[71] However, changes may be present in earlier years, and when we see sudden changes in data series, it is unknown to us whether these are due to data phenomena or to genuine changes in interstate migration.

Figure 4.1 presents the interstate residential migration rates calculated from the CPS, ACS, IRS, and LEHD data sources.[72] The CPS and IRS data have the longest time-series, and both show a decline in interstate migration from

[71] See Internal Revenue Service (2012).
[72] We also provide CPS rates for the restricted set of states, comparable to the set used in the LEHD in Appendix Figure 4.A.2.

approximately 3% in 1985 to 2.5% in 2000. Starting in 2000, however, trends in the CPS and IRS series diverge substantially. Interstate migration in the CPS continues its decline to 1.4% by 2010, with the exception of a slight recovery in 2005 and 2006 that we see reflected in the IRS data, corresponding to the economic recovery. However, the IRS migration rate is more procyclical, increasing the in early 2000s and reaching 2.7% in 2006 – compared to 1.9% in the CPS that only drops to 2.2% by 2010. The ACS shows a nearly identical cyclical pattern to the IRS. As stated earlier, the LEHD interstate migration rate also closely mirrors the IRS migration rate for all available years.

Table 4.1 presents the correlations between the series in Figure 4.1. The post-2000 correlation between the IRS and CPS migration rate is reasonably strong at 0.665, as well as the post-2000 correlation between the CPS and the LEHD at 0.688, but these correlations are far lower than the correlation of the overall (i.e. 1981-2014) time series of 0.841 between the CPS and IRS. The ACS is also more strongly correlated with the CPS (0.835), which is another recent correlation since we do not have ACS data prior to 2000. These correlations indicate that despite the stronger downward trend in the CPS relative to the other data sources, they tend to move together. The LEHD and IRS series are very strongly correlated at 0.917.

Any post-2000 decline in interstate migration is largest as measured by the CPS. The ACS, IRS, and LEHD data exhibit procyclicality with perhaps a lower level in the years following the 2007-2009 recession. However, given that the CPS is the most commonly used data for the study of migration, it is worth exploring this discrepancy in more detail. We undertake this endeavor by linking the LEHD and CPS, the findings from which we detail below.

4.3.2 Evidence from Matched CPS and LEHD Data

We consider linked CPS-LEHD data for the years 2002-2009, where the divergence between the CPS and the other data series is concentrated. We select individuals who appear in the CPS as well as have LEHD residential addresses in the preceding and subsequent two years in order to evaluate mis-timed migration flows, as well as the phenomenon of migration that is immediately reversed in the next year. The Protected Identification Key (PIK) is a variable available in both the CPS and the LEHD that uniquely identifies each individual and allows us to construct a common sample. Figure 4.2 shows that the CPS-LEHD has surprisingly comparable migration as measured in each of these respective data sources. The population is the same by construction (the set of individuals who respond to the CPS and match to administrative records). This suggests that much of the difference between the CPS and LEHD interstate migration rates is due to something other than the set of individuals who appear in either the CPS or administrative records.

Figure 4.3 presents migration rates using this linked CPS-LEHD sample with the same sample selection criteria used in the previous figure. The dotted line shows the migration rate calculated using the individuals in the linked sample whose outcomes agree between the CPS and LEHD. The rate for this group is somewhat cyclical but otherwise exhibits a persistent downward trend much like the original CPS migration rate. In other words, the agreement between the two data sets lends support to the story that interstate residential migration has unambiguously declined over this period.

The dashed line in the figure shows the rate at which individuals who reported moving across states only in the CPS, and the solid line shows the analogous LEHD rates. The puzzle that emerges here is the significant jump in migration for the LEHD-only set of individuals from 2004 to 2005, leading to a higher level of migration that persists over a majority of the remaining years. The sudden jump in the LEHD Only line is puzzling, and suggests that part of the increase in the LEHD data (and, because of similar data sources, perhaps also the IRS data) may be due to a data phenomenon rather than a true increase in migration. Alternatively, the CPS migration rate may have fallen at the same time as interstate migration exhibited a strong increase. Caution is warranted here because we do not have a good understanding of what caused this jump. We can, however, rule out a few explanations with the following exercises.

To further explore the disagreement between the CPS and LEHD, we measure the extent to which we can attribute it to timing issues, arising either from imprecision in survey responses or from late tax filers affecting he timing of LEHD migration, shown in Table 4.2. The first two columns of Table 4.2 show the percentage of the matched data for which the observed CPS move (between a given pair of origin and destination states) appears in the LEHD in either preceding or subsequent year. That the LEHD migration appears after the CPS rather than before suggests that late tax filers affect the disagreement between the CPS and LEHD. The third column captures the percentage of the sample that move to an adjacent state in the CPS where the origin state is the same in both the CPS and LEHD, suggesting that CPS responses tend to include short-term or temporary moves. Unfortunately, there is no question in the CPS that sheds light on residential moves that last less than one year. These timing issues account for a substantial two-thirds of these CPS-only migration outcomes, while the residual one-third remains unexplained.

We also investigate a pattern that is more prevalent in the LEHD than the CPS due to the nature of administrative data. Table 4.3 explores the prevalence of interstate migration that is immediately reversed. Transition statistics that employ a strategy for choosing a single outcome (one of 50 states of residence, one of several million employers, etc.) commonly have more such immediate reversals than economic activity might indicate, see Hyatt and McEntarfer (2012) on job-to-job flows. Immediately reversed migration is the observed event where the individual moves from state X to state Y in year t, and then moves back to state X in year $t+1$. Table 4.2 shows the frequency with which this phenomenon is associated with interstate migration that appears in the CPS co-incident with the LEHD, as well as those that appear in the LEHD only. Migrations that appear in the LEHD but not the CPS are twice as likely to be part of an immediately reversed migration than those that appear in both data sources. Nevertheless, immediately reversed migrations only occur in 12% to 19% of migration that only appears in the LEHD.

That the LEHD contains more such moves could be related to the way the administrative data deal with individuals reporting multiple residences, i.e. error in predicting primary residence, and, thus, to the likelihood that the observed LEHD migration is in fact not a true migration. A more general conclusion we can draw from both Table 4.1 and 4.2 is that none of these issues we have detailed has trended in such a way that could explain the jump in the migration rate seen in the LEHD-only line in Figure 4.2, discussed above. However, we made some progress toward explaining the disagreement and the difference in levels between the CPS and LEHD migration rates.

4.4 Interstate Migration and Job-to-Job Flows

4.4.1 Trends in the Job-to-Job Flows

We now examine the role of job-to-job flows in interstate migration. Given the large divergence in interstate migration trends between the CPS and LEHD data during this decade, we check to see if they show similar declines in labor market fluidity, measured by declines in job-to-job moves. While the annual metric of job-to-job flows in the CPS can be compared to interstate migration directly in the microdata, the job-to-job flows in the CPS only are present when a household does not move. Figure 4.4 shows quarterly job-to-job flow rates calculated from the CPS and LEHD, as well as an annual proxy from the CPS: the fraction of those employed who worked for multiple employers in the last year.[73] Perhaps surprisingly given the divergence in migration rates in these two series, here all three measures track each other very closely between 2000 and 2010, though differences widen after 2010 with the LEHD data showing more of a recovery than the CPS.[74] In all three series, the job-to-job flow rate is procyclical, and the implied quarterly job-to-job flow rate

[73] We also show CPS rates for the set of restricted states in Figure 4.A3.
[74] The later years in the CPS monthly series also show greater prevalence of missing job-to-job flow data, which makes interpretation of the most recent years of this time series difficult.

derived from the monthly CPS job-to-job flows series is very similar in levels to the LEHD series. The LEHD job-to-job flow rate reached a high of 7.5% in 2000, declined to 5.8% in 2003, had recovered to 6.6% by 2005, reached a low of 4.4% during the 2007-2009 recession, and rebounded to 5.8% by the end of 2013. This evidence on job-to-job flows is consistent with the findings of Hyatt and McEntarfer (2012) and Hyatt (2015).

The much longer annual job-to-job transition series from the CPS suggests that while job-to-job flows were also cyclical between 1980 and 1998, they did not exhibit a downward trend.[75] Therefore, declines in job-to-job moves overall do not appear to drive the observed decline in residential migration in the CPS and IRS data from the 1980s to 2000.[76] Moreover, this similarity between the CPS and the LEHD data suggests the discrepancy in residential migration rates is not caused by a systemic error in the CPS, such as a weighting issue that would affect other outcomes, which motivates a separate investigation into where and why the CPS and LEHD data differ in identifying residential moves.

The correlations between the different job-to-job flow and interstate migration series are of interest, and are included in Table 4.1. We see that the frequency with

---

[75] Although job-to-job flow rates show no obvious trend during the 1980s and 1990s, Decker et al. (2014) and Molloy et al. (2016) suggest that labor market fluidity may have declined from the 1970s to the 1990s. Moscarini and Thomsson (2007) and Kambourov and Manovskii (2008) provide evidence that mobility across industries and occupations increased over the span of those decades, while Hyatt (2015) and Hyatt and Spletzer (2016) provide evidence that labor market fluidity had little trend over these decades. These seeming contradictions are due to differences in data sources and measurement, Decker et al. (2014) and Molloy et al. (2016) emphasize job reallocation rates across firms. The other studies emphasize occupation and industry transitions, as well as data on job-to-job transitions and job tenure from household survey data. In any case, all data sources agree that the 15 years following the year 2000 have shown more of a decline than any decline apparent in previous decades.

[76] The March CPS ASEC asks about both migration and the number of employers worked in the last year, so one might alternatively measure economic migration as the rate with which individuals both changed states and worked at multiple employers in the last year. Although the rates are similar for this alternative measure, there is also substantial disagreement, see Appendix Figure 4.A.1.

which workers report multiple employers in the past year is very strongly correlated with the interstate migration rate, with a correlation of 0.867 for the longer 1981-2014 series and even higher at 0.916 for the more recent years. This higher correlation likely reflects that the CPS responses of multiple employers and different states of residence in the past year are trending downward post-2000.

4.4.2 Economic Migration

To examine the impact of declines in job mobility on interstate migration, we first quantify the extent to which residential migration has been driven by economic migration, i.e. the proportion of individuals moving across state lines at the same time as they also changed their state of employment. This is shown in Figure 4.5, where we compare economic moves in the LEHD and CPS data. In both data sources, economic migration accounts for about a third of total residential migration in 2000. The cyclicality of the economic migration rate, like in the residential migration rate, is again more pronounced in the LEHD data than in the CPS. However, interstate economic migration rates in the CPS and LEHD data are much more similar than overall residential migration, with respect to both levels and trends.[77]

Table 4.4 provides an overview of the trends shown in Figure 4.5 of interstate migration, job-to-job, and economic migration rates, when available, in the LEHD, CPS, and ACS from the period 2000 to 2010. Given the stronger decline in interstate migration in the CPS, we are surprised to find that levels and trends for interstate moves for job-related reasons are more similar in the CPS and LEHD data. In the

[77] For CPS data with the same state-based sample selection criteria, see Appendix Figure 4.A.3.

CPS, overall interstate migration rates fell by 50.6% between 2000 and 2010, compared to 20.1% in the LEHD data.[78] There are also large differences in the decline in migrations for job-related reasons, which are 66.8% and 45.5% in the CPS and LEHD, respectively. Additionally, economic migration rates in both series are so small that these percentage differences in the decline exaggerate the differences between the two series. Economic interstate migration in the CPS fell from 0.9% in 2000 to 0.5% in 2010, and in the LEHD data, the rate fell from 0.8% in 2000 to 0.5% in 2010.

The CPS and LEHD data show somewhat different patterns in the role of economic migration in changing migration rates. These differences in the overall decline in interstate migration do mean that we find a stronger role for declining job mobility in explaining declining interstate migration in the LEHD data. 62% of the decline in the interstate migration rate between 2000 and 2010 in the LEHD data is accounted for by the decline in job-to-job moves, compared to 49% of the decline in interstate migration rates for the corresponding period in the CPS data. Both numbers highlight the importance of investigating how returns to economic migration in particular have changed over time.

Table 4.4 also lists the other components of the decline in interstate migration in the CPS data, showing the CPS responses to the question about the reason for migration. The decline in family-related moves accounts for the largest share of the decline in non-economic migration between 2000 and 2010. The 'new job/job transfer' reason is by far the largest component of economic migration in the CPS, as

[78] These percentages are proportionate changes, defined as the difference between the rate in 2010 and in 2000 divided by the average of the rate in 2000 and in 2010.

well as one-third of all interstate migrations in 2000, with ‘lost job/job search’ having the lowest frequency of all reasons for migration, or less than 5% of migrations in 2000. Additionally, ‘new job/job transfer’ experienced the largest percentage decline over the period, with ‘other job-related’ a close second but at a much lower level of occurrence, while ‘lost job/job search’ actually increased slightly. This stands as further evidence that job-to-job flows are the primary mechanism driving interstate migrations that have an economic motive.

4.5 The Decline in Interstate Migration and Economic Migration: 2000-2010

4.5.1 The Role of Composition in Declining Interstate Migration

Because we have rich microdata on migration from the CPS and LEHD, we can explore new mechanisms that may have affected interstate migration and its economic component. The LEHD data especially provides information such as whether the decline in startups explains changes in interstate migration or its economic component. We perform a shift-share analysis following Hyatt and Spletzer (2013), Decker et al. (2014). Formally, this decomposition can be expressed as:

$$\Delta Y_t = \sum_i \Delta S_{it} \bar{Y}_i + \sum_i \Delta Y_{it} \bar{S}_i,$$

where ΔY_t is the change in the migration rate from 2000 to 2010, i represents each group within a demographic category (e.g. age), $\bar{Y}_i$ is the average transition rate for each i, $\bar{S}_i$ is the average share of each i, ΔY_{it} is the change in the transition rate for each i, and ΔS_{it} is the change in the share of individuals within each i. The first component of the right-hand side captures the fraction of the change attributable to compositional changes, or explained variation, while the second component captures the fraction attributable to within-group changes, or unexplained variation.

With both the CPS and LEHD data, we decompose residential and economic migration by age, gender, race/ethnicity, education, and industry. Table 4.5 presents the results for the residential and economic migration rates using the CPS and LEHD. Compositional changes in age contribute the most among demographic characteristics to the change in residential migration in both data sets: 7% in the CPS and 16% in the LEHD. The U.S. workforce has been aging and older workers are less likely to move than younger workers. We also find that earnings have a large degree of explanatory power, which can also be attributed to young workers who are initially at low earnings levels.

Similar results hold for the economic migration rate. Again, compositional changes in the age account for a large proportion of the decline from 2000 to 2010 (9.0% for the CPS, 12.0% for the LEHD), while other demographic characteristics have little explanatory power, which is consistent with our residential migration findings. Given that there was relatively little change in labor market composition by race, ethnicity, and gender over this time period, it is not surprising to see that there were very little change in either the residential or the economic migration rate that arise from composition changes across any of the demographic categories other than age.

We see large differences in the explanatory power of economic characteristics between the CPS and the LEHD. Although Table 4.5 shows the majority of these economic categories matter little for residential migration, different patterns arise when analyzing the economic migration rate. For economic migration, in the CPS, the employment composition effect is roughly 2%, while it is 25% in the LEHD, while for economic migration, the effect of employment is offsetting in both the CPS and LEHD. This large difference likely arises from the way we define an economic migration in the LEHD: we use employment change during a residential move to define economic migration, which is more likely to include someone who was previously employed. We observe large effects from earnings and firm characteristics within the LEHD only when we include the non-employed as a category, and these effects disappear almost entirely when we subset to workers with positive earnings.[79] Industry compositional changes have negligible explanatory power in the CPS as well. The extra step we take with the LEHD data is to explore firm age and size, which Hyatt and Spletzer (2013), Decker et al. (2014) find play a small but noticeable role in explaining the trend in the job reallocation rate. The small effects of firm age and size, taken together with the finding of Hyatt and Spletzer (2013) and Decker et al. (2014), suggest that declining entrepreneurship, which has been documented as having a modest role in explaining other job and worker reallocation rates, does not explain much of the change in economic migration. Overall, these decompositions provide further evidence that most of the decline in the two migration rates should be attributed to changes in migration behavior *within* demographic and employment groups.

[79] Since the non-employed do not have a firm size, age, or industry, we code them as a separate category.

4.5.2 Changes in the Return to Interstate Migration

In corroborating the findings of recent studies that shifts in observables contribute a small part to the declines in migration, we go on to investigate earnings changes associated with economic migrations, which is a feasible endeavor as the LEHD has a record of each worker's longitudinal earnings history. We calculate the log earnings changes associated with an interstate job move as compared to earnings changes associated with other job changes, and then look at potential explanations for these earnings changes. This is done on a quarterly basis as we have earnings observations at the quarterly level, and so for the subset of workers who have a state-to-state job-to-job flow for a pair of years that indicate residential migration, we can date the migration to the quarter.

Figure 4.6 shows the trend in annual log earnings changes associated with two types of labor market transitions: an interstate job-to-job flow with a change in state of residence, and a within-state job-to-job flow that may or may not be linked to a change in state of residence.[80] As a baseline, those who are continuously employed for two years at the same employer and exhibit no job-to-job flows, i.e. job stayers, are also shown. The difference in log earnings is a measure of the percentage change in earnings, and so is straightforward to interpret. Job stayers have far lower earnings increases, which dip down to below zero during the height of the Great Recession. In contrast, and consistent with the evidence in Hyatt and McEntarfer (2012), workers

[80] It is possible that these changes in earnings are driven by selection. Therefore, we conducted two propensity score analyses that hold constant the probability of migrating. If the earnings changes associated with migrating are higher for those whose observable characteristics indicate that they are more likely to migrate, then the predicted returns to migration may be constant. In Appendix B, we find that Figure 4.6 is robust to an elementary selection correction technique that uses propensity score matching.

who exhibit either type of job-to-job flow see substantial increases in earnings, and these earnings increases are highly procyclical. During the late 1990s, in the year 2005, and in the year 2010, earnings increase by about 15% for workers who change jobs but do not change the state of work or residence. This falls to 9% during the 2001 recession and 4% during the 2007-2009 recession.

Individuals who change both states of work and residence have lower earnings changes, which are also procyclical and in the range of 3% to 14%. It initially seems counterintuitive that interstate migrants have lower earnings increases than non-migrants since there is a positive cost to moving. However, when we look at levels of earnings over this time period, we find that migrants have substantially higher earnings than non-migrants – about 30-40% higher – while at the same time experience approximately the same earnings change in absolute terms as non-migrants. These findings support the conventional wisdom that it tends to be the more highly-skilled who undergo long-distance moves. With that said, there is no notable trend in Figure 4.4 that would indicate returns to economic migration have declined over the period.

4.6 Conclusion

We have shown that the declines in interstate migration and job change occur at different times and are of different magnitudes. The decline in job change is concentrated after the year 2000, and interstate migration differs based on the data series under consideration. The CPS shows an increase in the interstate migration rate during the 1980s, followed by declines in the 1990s and 2000s. The IRS shows substantially less of a decline over a similar period, and the more recently available ACS and LEHD show a small post-2000 decline that is consistent with the IRS series. The timing and extent of the decline in the residential migration rate still seems to be an open question.

Nevertheless, a considerable amount of interstate migration involves job change. In the CPS, survey respondents indicate that their interstate migration was due to fining a new job or changing jobs in about one-third of such transitions, which is confirmed by LEHD data. Between the years 2000 and 2010, interstate migration is procyclical with a declining trend in both the CPS and LEHD, although the declining trend is more apparent in the CPS than LEHD. Furthermore, earnings increase substantially when workers change jobs and move across states, and that these earnings changes are procyclical.

References

Abowd, John, Bryce E. Stephens, Lars Vilhuber, Fredrik Andersson, Kevin L. McKinney, Marc Roemer, and Simon Woodcock. 2009. "The LEHD Infrastructure Files and the Creation of the Quarterly Workforce Indicators." In *Producer Dynamics: New Evidence from Micro Data*, ed. Timothy Dunne, J. Bradford Jensen, and Mark J. Roberts. Chicago, IL: University of Chicago Press:149-230.

Dahl, Gordon. 2002. "Mobility and the Return to Education: Testing a Roy Model with Multiple Markets." *Econometrica* 70(6): 2367-2420.

Davis, Steven, and John Haltiwanger 2014. "Labor Market Fluidity and Economic Performance" NBER Working Paper #20479.

Decker, Ryan, John Haltiwanger, Ron S Jarmin, and Javier Miranda. 2014. "The Secular Decline in Business Dynamism in the U.S." Unpublished mimeograph, University of Maryland.

Faberman, Jason. 2008. "Job Flows, Jobless Recoveries, and the Great Moderation." Federal Reserve Bank of Philadelphia Working Paper #08-11.

Fallick, Bruce, and Charles Fleischman. 2004. "Employer-to-Employer Flows in the U.S. Labor Market: The Complete Picture of Gross Worker Flows." Federal Reserve Board Finance and Economics Discussion Series Working Paper 2004-34.

Farber, Henry. 1999. "Mobility and Stability: The Dynamics of Job Change in Labor Markets." In: *Handbook of Labor Economics*. Volume 3. Orley Ashenfelter and David Card (eds). Amsterdam: North Holland: 2439-2483.

Goetz, Christopher. 2013. "Falling House Prices and Labor Mobility: Evidence from Matched Employer-Employee Data" US Census Bureau Center for Economic Studies Paper No. CES-WP- 13-43

Haltiwanger, John, Henry Hyatt, and Erika McEntarfer. 2015. "Cyclical Reallocation of Workers Across Employers by Firm Size and Firm Wage." NBER Working Paper #21235.

Haltiwanger, John, Henry Hyatt, Erika McEntarfer, Liliana Sousa, and Stephen Tibbets. 2014. "Firm Age And Size In The Longitudinal Employer-Household Dynamics Data" Center for Economic Studies Discussion Paper CES-14-16.

Henderson, Cody, and Henry Hyatt. 2012. "Estimation of Job-to-Job Flow Rates under Partially Missing Geography." U.S. Census Bureau Center for Economic Studies Working Paper #CES-12-29.

Hsieh, Chang-Tai, and Enrico Moretti. 2015. "Why Do Cities Matter? Local Growth and Aggregate Growth." Unpublished draft, University of Chicago.

Hyatt, Henry. 2015. "The Decline in Job-to-Job Flows." *IZA World of Labor* 175.

Hyatt, Henry, Erika McEntarfer, Kevin McKinney, Stephen Tibbets, and Douglas Walton. 2014. "Job-to-Job (J2J) Flows: New Labor Market Statistics From Linked Employer-Employee Data." *JSM Proceedings 2014*, Business and Economics Statistics Section, 231-245.

Hyatt, Henry, and Erika McEntarfer. 2012. "Job-to-Job Flows and the Business Cycle." U.S. Census Bureau Center for Economic Studies Discussion Paper #CES-12-04.

Hyatt, Henry, and James Spletzer. 2013. "The Recent Decline in Employment Dynamics," *IZA Journal of Labor Economics* 2(5): 1-21.

Hyatt, Henry, and James Spletzer. 2016. "The Shifting Job Tenure Distribution." Forthcoming, *Labour Economics*.

Internal Revenue Service. 2012. "2011-2012 Migration Data Users Guide." Online documentation. Available at: https://www.irs.gov/pub/irs-soi/1112inpublicmigdoc.pdf (last accessed: May 19, 2016).

Kambourov, Gueorgui, and Iourii Manovskii. 2008. "Rising Occupational and Industry Mobility in the United States: 1968-97." *International Economic Review* 49(1): 41-79.

Kaplan, Greg, and Sam Schulhofer-Wohl. 2012. “Interstate Migration Has Fallen Less Than You Think: Consequences of Hot Deck Imputation in the Current Population Survey. *Demography* 49(3): 1061-1074.

Kaplan, Greg, and Sam Schulhofer-Wohl. 2015 “Understanding the Long-Run Decline in Interstate Migration.” NBER Working Paper #18507.

Koerber, Kin. 2007. Comparison of ACS and ACES Data on Geographic Mobility: 2004. Mimeo, U.S. Census Bureau.

Lazear, Edward, and James Spletzer. 2012. Hiring, Churn, and the Business Cycle. *The American Economic Review: Papers & Proceedings 2012*, 102(3), 575.

Leggieri, Charlene Arona Pistiner and James Farber. 2002. "Methods for Conducting an Administrative Records Experiment in Census 2000." *JSM Proceedings 2002*, Survey Research Methods Section, 2709-2713.

Molloy, Raven, Christopher L. Smith, Abigail Wozniak. 2011. “Internal Migration in the United States”, *Journal of Economic Perspectives*, 25(3): 173-196.

Molloy, Raven, Christopher L. Smith, and Abigail Wozniak. 2014. “Declining Migration Within the U.S.: The Role of the Labor Market.” NBER Working Paper #20065.

Molloy, Raven, Christopher Smith, Riccardo Trezzi, and Abigail Wozniak. 2016. “Understanding Declining Fluidity in the U.S. Labor Market.” Conference draft, *Brookings Papers on Economic Activity*.

Moscarini, Giuseppe, and Kaj Thomsson. 2007. “Occupational and Job Mobility in the U.S.” *Scandinavian Journal of Economics* 109(4): 807-836.

Ruggles, Steven, Matthew Sobek, Trent Alexander, Catherine A. Fitch, Ronald Goeken, Patricia Kelly Hall, Miriam King, and Chad Ronnander. 2004. Integrated Public Use Microdata Series: Version 3.0 (Machine-readable database). Minneapolis, MN: Minnesota Population Center (producer and distributor).

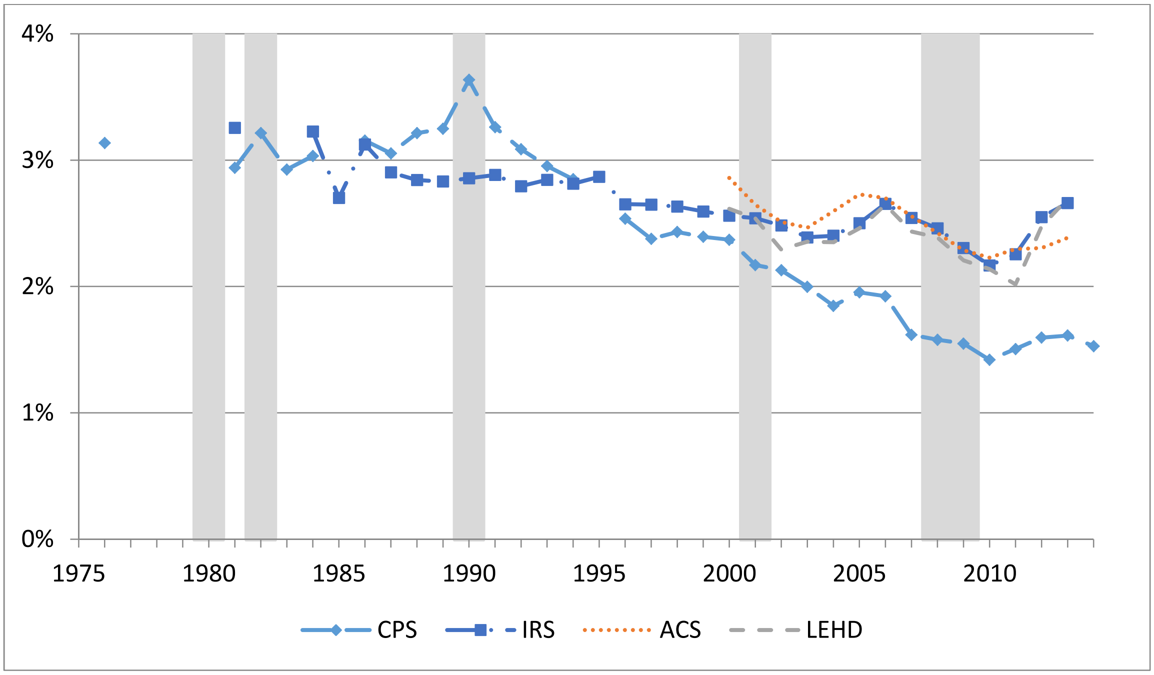
Stewart, Jay. 2007. “Using March CPS Data to Analyze Labor Market Transitions.” *Journal of Economic and Social Measurement* 32(2-3): 177-197.

Topel, Robert, and Michael Ward. 1992. Job Mobility and the Careers of Young Men. *The Quarterly Journal of Economics*, 107(2): 439-479.

Wozniak, Abigail. 2010. "Are College Graduates More Responsive to Distant Labor Market Opportunities?" *Journal of Human Resources* 45(4): 944-970.

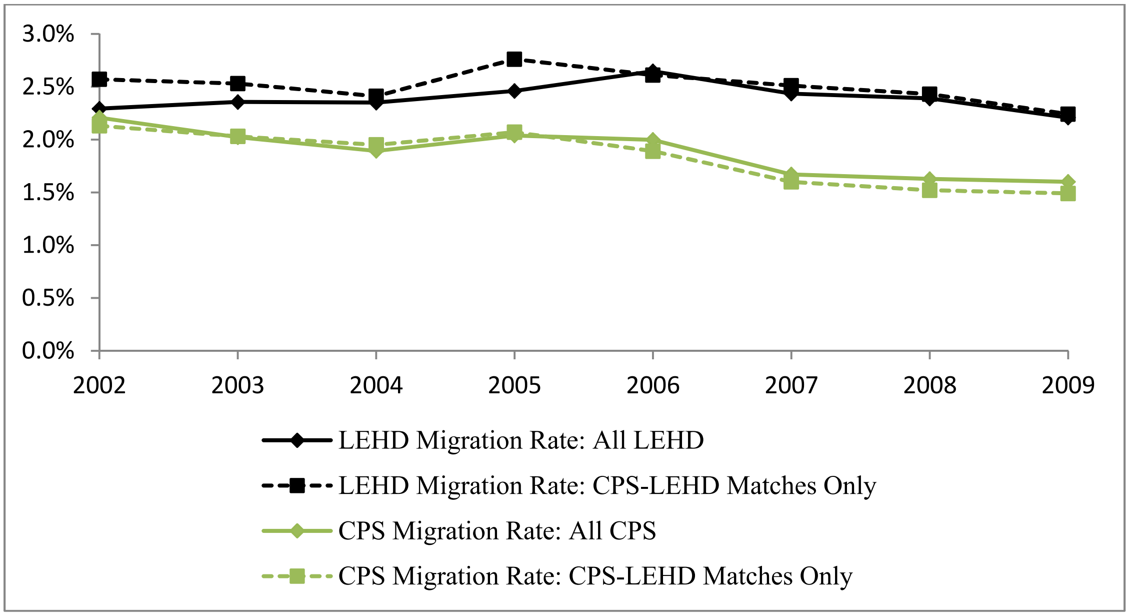
Figures

Figure 4.1: Interstate Residential Migration Rates



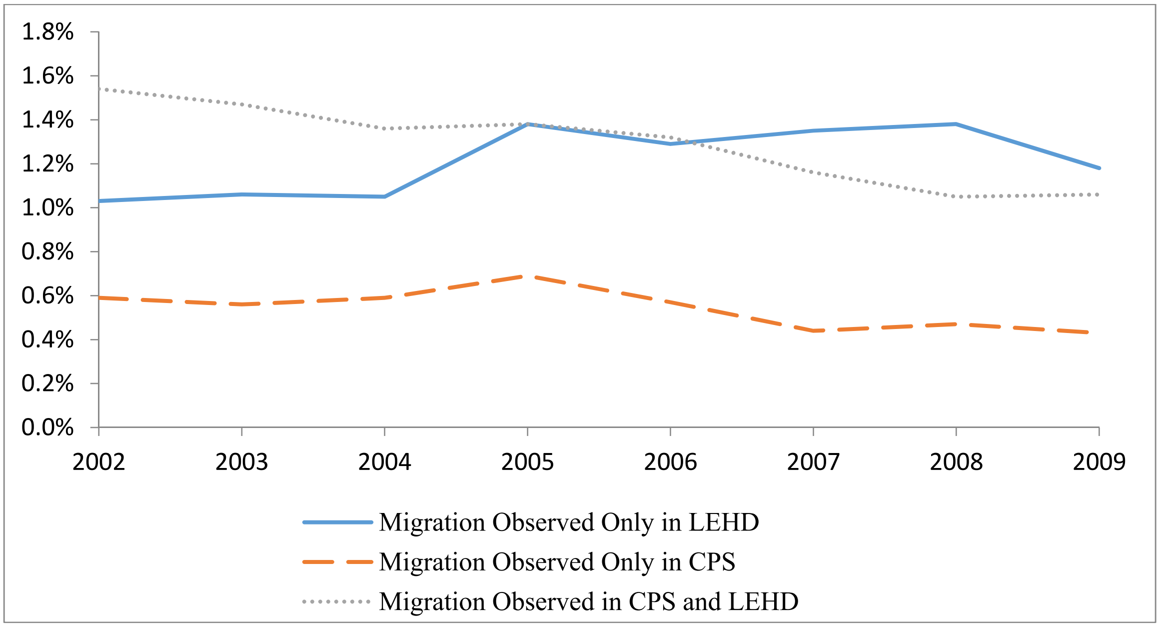
Notes: Shaded areas denote recessions as determined by the National Bureau of Economic Research. Authors' tabulations of Current Population Survey (CPS), American Community Survey (ACS), and the Longitudinal Employer-Household Dynamics (LEHD) microdata, as well as published tabulations of Internal Revenue Service (IRS) data. CPS, ACS, and LEHD migration rates are calculated for the population age 16-64.

Figure 4.2: CPS and LEHD Migration Rates vs. Matched CPS-LEHD Subset



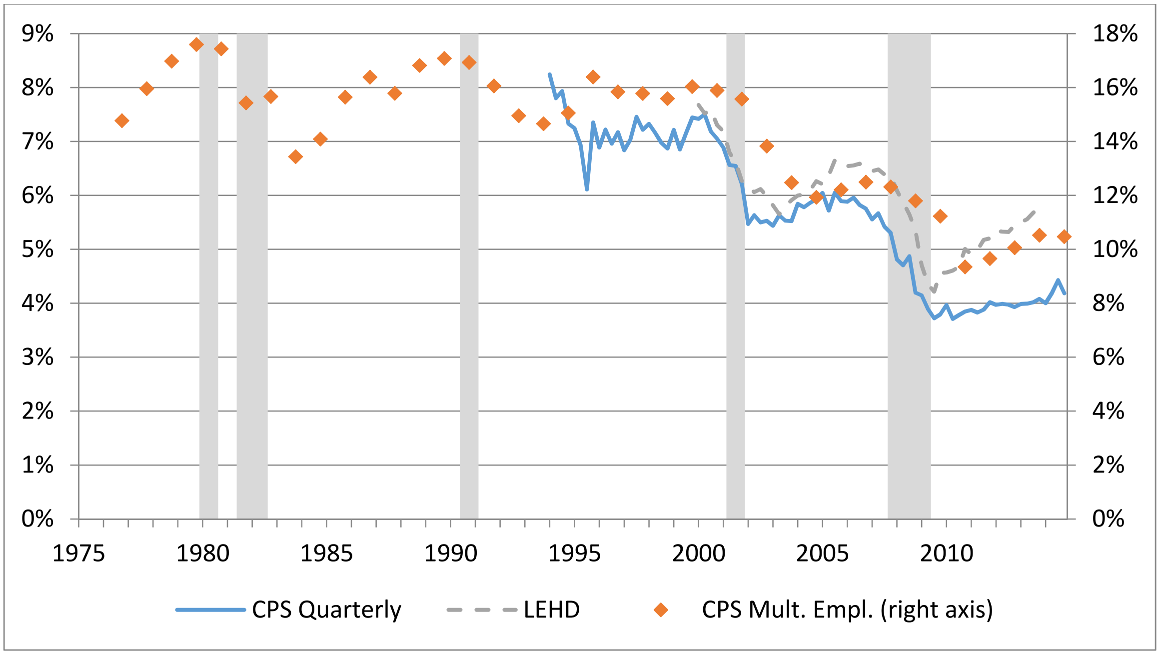
Notes: For the "All LEHD" and "All CPS" migration rates, see notes to Figure 4.1, as these are the "LEHD" and "CPS" lines from that series for the interval 2002-2009. The "CPS-LEHD Matches" are defined as follows: March CPS ASEC respondents for a given year, who match to LEHD and have residential information for that year, the three previous years (i.e., the year pair for which states of residence are compared to determine migration, plus the two previous years), and the two subsequent years. All series are calculated for the population age 16-64 in the reference year.

Figure 4.3: Matched CPS-LEHD Migration Rates



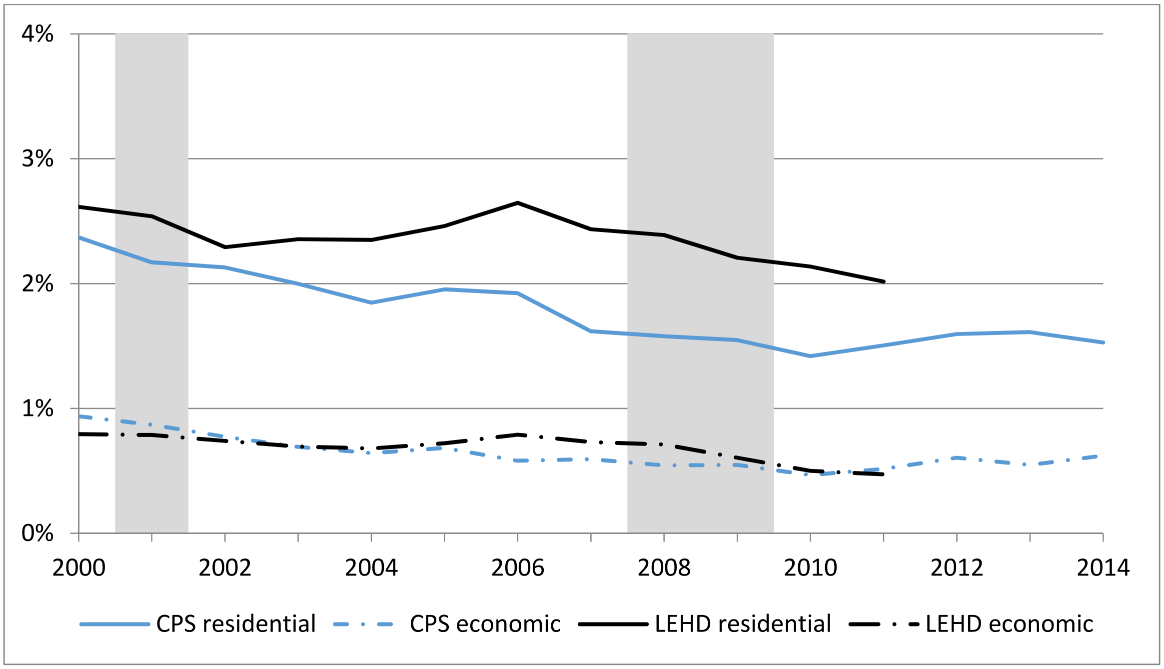
Notes: March CPS ASEC respondents for a given year age 16-64 who match to LEHD and have residential information for that year, the three previous years (i.e., the year pair for which states of residence are compared to determine migration, plus the two previous years), and the two subsequent years.

Figure 4.4: Job-to-Job Flow Rates



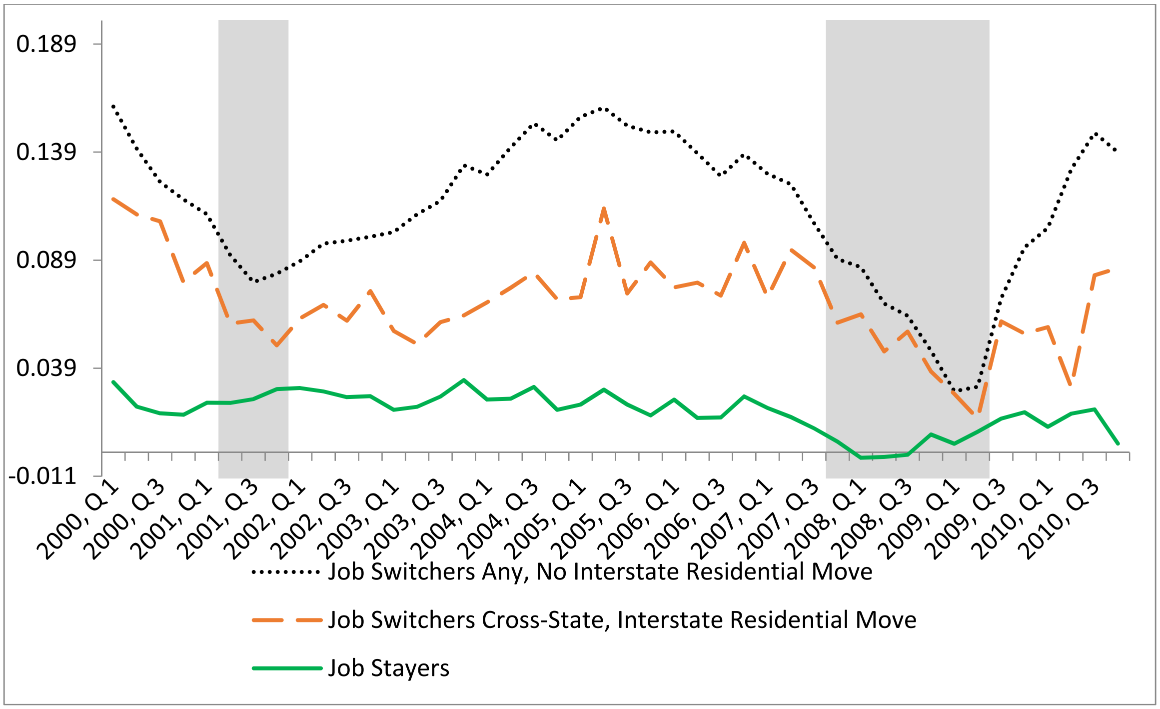
Notes: Shaded areas denote recessions as determined by the National Bureau of Economic Research. Quarterly data are seasonally adjusted. CPS monthly data are converted to quarterly via addition following Hyatt and Spletzer (2013). The second and third quarters of 1995 are missing one month each due to changes made between June and August in the CPS monthly files. The annual CPS series is constructed from the March CPS and coded to the first quarter and ask respondents about the number of jobs worked in the last year.

Figure 4.5: Interstate Migration, Residential and Economic



Note: Shaded areas denote recessions as determined by the National Bureau of Economic Research. *Source*: CPS, LEHD, and CPR.

Figure 4.6: Change in Log Earnings



Note: For each category, a point represents the median difference of: log earnings for one year after the quarter minus log earnings one year prior to the quarter. The sample is initially restricted to people who have earnings every quarter for 4 quarters prior to a quarter of interest and 4 quarters after. The "Job Stayers" Sample are people who stayed at the same job during these two years around the quarter. "Job Switchers Any, No Interstate Residential Move" are people who had any dominant job-to-job flow during that quarter, and who did not have an interstate residential move. "Job Switchers Cross-State, Interstate Residential Move" are people who had a dominant job-to-job flow across two states during that quarter, and who did had an interstate residential move. Shaded areas denote recessions as determined by the National Bureau of Economic Research. *Source*: CPS, LEHD, and CPR.

Tables

Table 4.1: Correlations Between Migration and Job-to-Job Flow Measures

Correlations: 1981-2014

	CPS Migrat.	IRS Migrat.	CPS Multiple Empl.
CPS Migration	1		
IRS Migration	0.841	1	
CPS Multiple Jobs	0.867	0.703	1

Correlations: 2000-2014

	CPS Migrat.	IRS Migrat.	CPS Multiple Empl.	ACS Migrat.	LEHD-CPR Migrat.	LEHD Job-to-Job	CPS Job-to-Job
CPS Migration	1						
IRS Migration	0.665	1					
CPS Multiple Employers	0.916	0.749	1				
ACS Migration	0.835	0.859	0.816	1			
LEHD Migration	0.688	0.917	0.768	0.890	1		
LEHD Job-to-Job	0.820	0.865	0.835	0.951	0.868	1	
CPS Job-to-Job	0.915	0.800	0.910	0.951	0.856	0.933	1

Notes: Quarterly LEHD and CPS job-to-job flows data are annualized by taking an average for each year. Correlations are using the year intervals listed above, with the following exceptions. Migration data are missing from the CPS series for 1985, and are missing from the IRS series for 1982 and 1983, and years 2012 and 2013 are excluded from tabulation because of a significant methodology change that affects those years. The ACS and LEHD job-to-job flows series end in 2013. The LEHD migration series ends in 2011. The LEHD job-to-job flows series uses a subset of states that are available in the year 2000, see text for additional details of the construction of each data series. The LEHD and CPS quarterly job-to-job flow rates have correlations with the ACS migration rate that distinct at the fifth decimal point and so appear identical due to rounding.

Table 4.2: CPS-LEHD Matches: Migration Observed Only in CPS in Year t

Year	Observed CPS Move Occurs One Year Later in LEHD	Observed CPS Move Occurs One Year Before in LEHD	Border State move observed in CPS, no move in LEHD, but same origin state	Residual
2002	23.2%	11.0%	20.9%	45.0%
2003	26.4%	11.7%	19.2%	42.7%
2004	31.6%	11.7%	23.0%	33.6%
2005	27.1%	9.4%	24.3%	39.3%
2006	32.1%	8.0%	22.4%	37.5%
2007	32.3%	11.2%	20.2%	36.4%
2008	30.6%	10.2%	19.4%	39.8%
2009	29.9%	15.0%	23.1%	32.0%

Notes: March CPS ASEC respondents for a given year age 16-64 who match to LEHD and have residential information for that year, the three previous years (i.e., the year pair for which states of residence are compared to determine migration, plus the two previous years), and the two subsequent years, who also reported moving across states in the CPS but who did not change residence in the LEHD.

Table 4.3: CPS-LEHD Matches: Immediately Reversed Migration

Year	LEHD migration and no CPS migration	Migration in CPS and LEHD
2002	18.2%	6.8%
2003	15.8%	5.9%
2004	14.9%	7.7%
2005	15.5%	7.6%
2006	12.5%	11.3%
2007	14.9%	7.2%
2008	14.2%	8.8%
2009	17.7%	9.2%

Notes: March CPS ASEC respondents for a given year age 16-64 who match to LEHD and have residential information for that year, the three previous years (i.e., the year pair for which states of residence are compared to determine migration, plus the two previous years), and the two subsequent years, who also changed residence in the LEHD between the reference year and the prior year. Percentages report the rate at which such moves are immediately reversed: i.e., the state of residence in the years previous and subsequent to the reference year are identical.

Table 4.4: Employment, Migration, and Job-to-Job Flows

	2000	2010	% pt change	Proportionate change
LEHD				
Job-to-Job Flow Rate	7.3%	5.0%	-2.3%	-37.3%
Interstate Migration Rate	2.6%	2.1%	-0.5%	-20.1%
Economic Migration Rate	0.8%	0.5%	-0.3%	-45.5%
CPS				
Mult. Empl. in the Last Year Rate	7.0%	3.9%	-3.2%	-58.5%
Interstate Migration Rate	2.5%	1.5%	-1.0%	-50.2%
CPS Migration Reason				
New job/job transfer	0.9%	0.5%	-0.5%	-67.0%
Lost job/job search	0.1%	0.1%	0.0%	2.5%
Other job-related	0.2%	0.1%	-0.1%	-67.2%
Family	0.6%	0.4%	-0.2%	-40.0%
Housing	0.3%	0.1%	-0.1%	-53.2%
Other	0.4%	0.3%	-0.1%	-37.3%

Note: Rates are calculated for individuals ages 16-64, excluding those in the Armed Forces and residing in group quarters, and any observations in the CPS with allocated or imputed migration values. Within the CPS, gross residential rates are weighted with the supplement weight. We calculate annual job change rates in the CPS March supplement using the method outlined in Farber (1999). We follow the methodology outlined in Fallick and Fleischman (2004) to calculate monthly job flows from the CPS basic files, then sum them and seasonally adjust to get the quarterly rates. The LEHD job-to-job flow rate includes both within-quarter and adjacent-quarter transition of a worker's dominant job (i.e. the job associated with the highest earnings). The denominator is the total number of dominant jobs. We use the J2J rates from the first quarter of 2000 and the first quarter of 2010 after seasonal adjustment. Interstate migration is calculated from the LEHD data. We consider a person to migrate when his residential state within the subsequent year is different from the residential state within the previous year. Proportionate change takes the difference between the rate in 2010 and in 2000, and divides it by the average of the rate in 2000 and in 2010. Some percentage point do not equal the difference in columns due to rounding (differences and proportionate change are exact at two decimal points). CPS migration reason categories were chosen following Kaplan and Schulhofer-Wohl (2015).

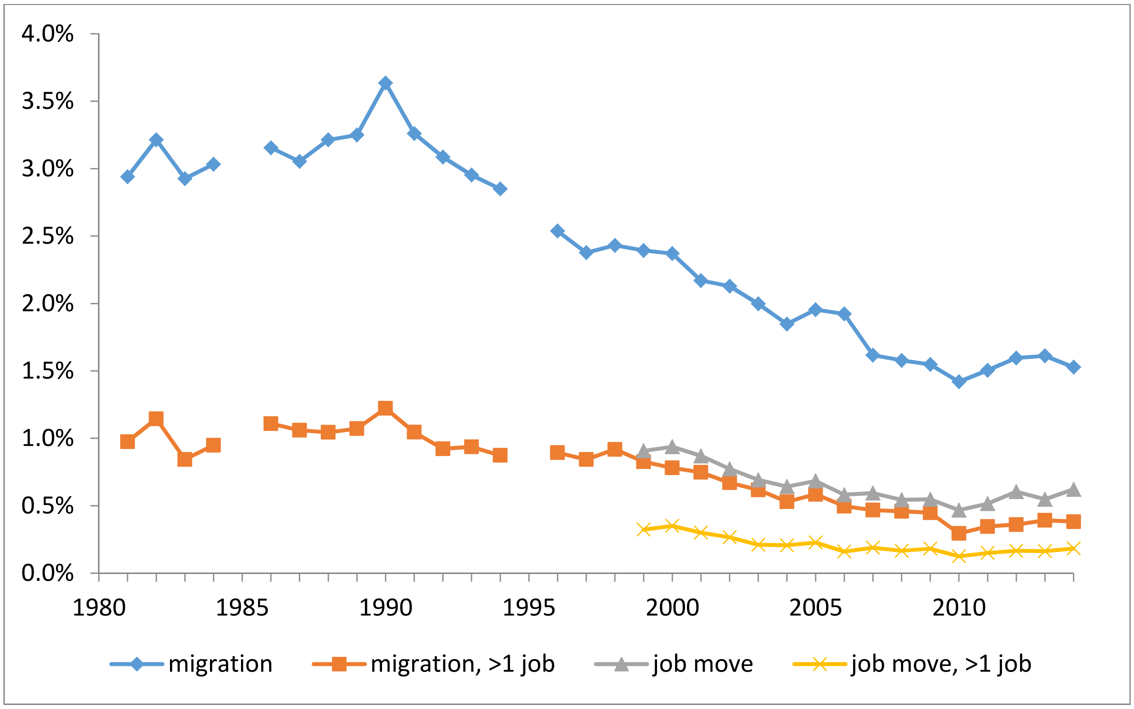
Table 4.5: Decomposition of Residential and Economic Migration Rates

	Residential Migration		Economic Migration	
	CPS	LEHD	CPS	LEHD
2000	2.4%	2.6%	0.9%	0.8%
2010	1.5%	2.1%	0.5%	0.5%
Change	-0.9%	-0.5%	-0.5%	-0.3%
% of change explained by:				
Worker Characteristics				
Gender	0.0%	0.4%	0.0%	0.2%
Age	7.1%	15.5%	9.1%	12.0%
Race and Ethnicity	1.3%	0.8%	1.1%	1.2%
Education	-4.7%	-0.1%	-7.8%	0.4%
Employment, Previous Year	--	-1.9%	--	11.4%
Employment, Subsequent Year	-4.2 %	-1.2%	1.8%	24.6%
Earnings, Previous Year	6.1%	18.4%		
Earnings, Subsequent Year	--	14.8%		
Firm characteristics				
Industry, Previous Year	--	-2.3%	--	-1.1%
Industry, Subsequent Year	-1.1%	-1.4%	0.1%	-0.3%
Firm Size, Previous Year	--	-0.5%	--	-1.2%
Firm Size, Subsequent Year	--	-0.4%	--	-1.0%
Firm Age, Previous Year	--	3.7%	--	0.8%
Firm Age, Subsequent Year	--	4.7%	--	2.1%

Note: We group age into the following groups: 16-18, 19-21, 22-24, 25-34, 35-44, 45-54, and 55-64. We group race and ethnicity into Hispanic of any race, White and non-Hispanic, Black and not Hispanic, Asian and not Hispanic, and a final category that includes those who are not Hispanic and any other race or more than one race. We group education into less than high school, high school, some college, and college and beyond. Industries are grouped into NAICS supersectors. Industry in the administrative data refers to the industry associated with the worker's dominant job. Similarly, we use the firm size and age of the dominant job prior to migration for the "Firm Size" and "Firm Age" categories. We group firm age into the following groups: <1 year, 2-3 years, 4-5 years, 6-10 years, >10 years. We group firm size into the following numbers of employees: <20, 20-49, 50-249, 250-49, >499 people. "Earnings, Previous Year" is the total earnings accumulated across all jobs within the year prior to the reference year, and "Earnings, Subsequent Year" is the total earnings accumulated across all jobs within the reference year. We group earnings into global deciles where the percentiles were determined across ten years. We classify an interstate residential migration as workers in the LEHD who resided within a different state the subsequent year. The CPS March Supplement is used for the CPS migration rates. Both shares and rates are weighted with the CPS supplement weight.

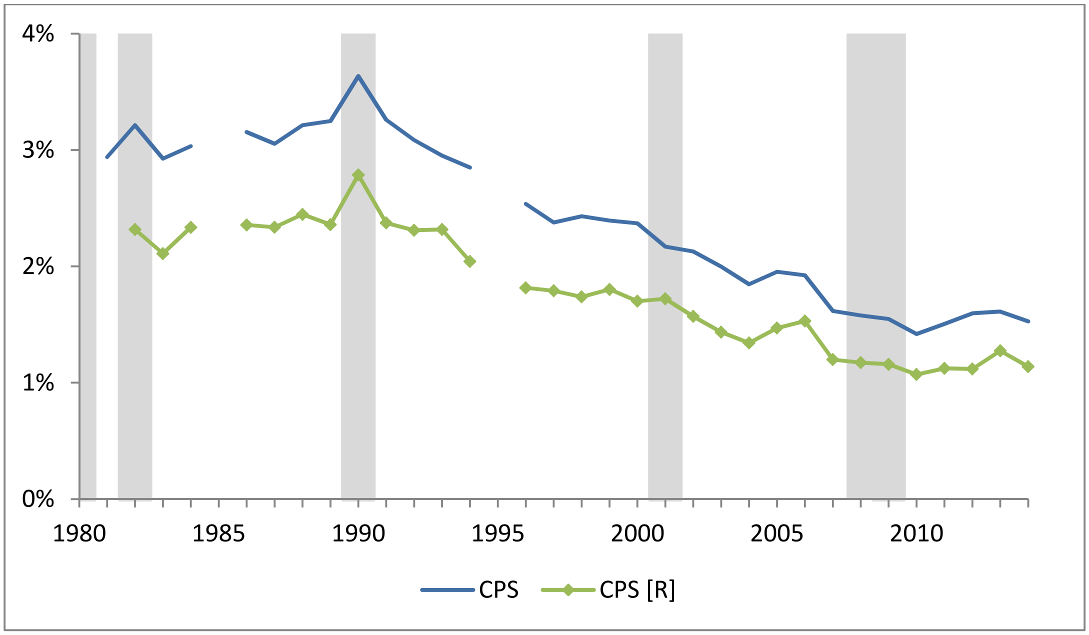
Appendix A: Supplemental Figures

Figure 4.A1: Economic Migration Concepts in the CPS



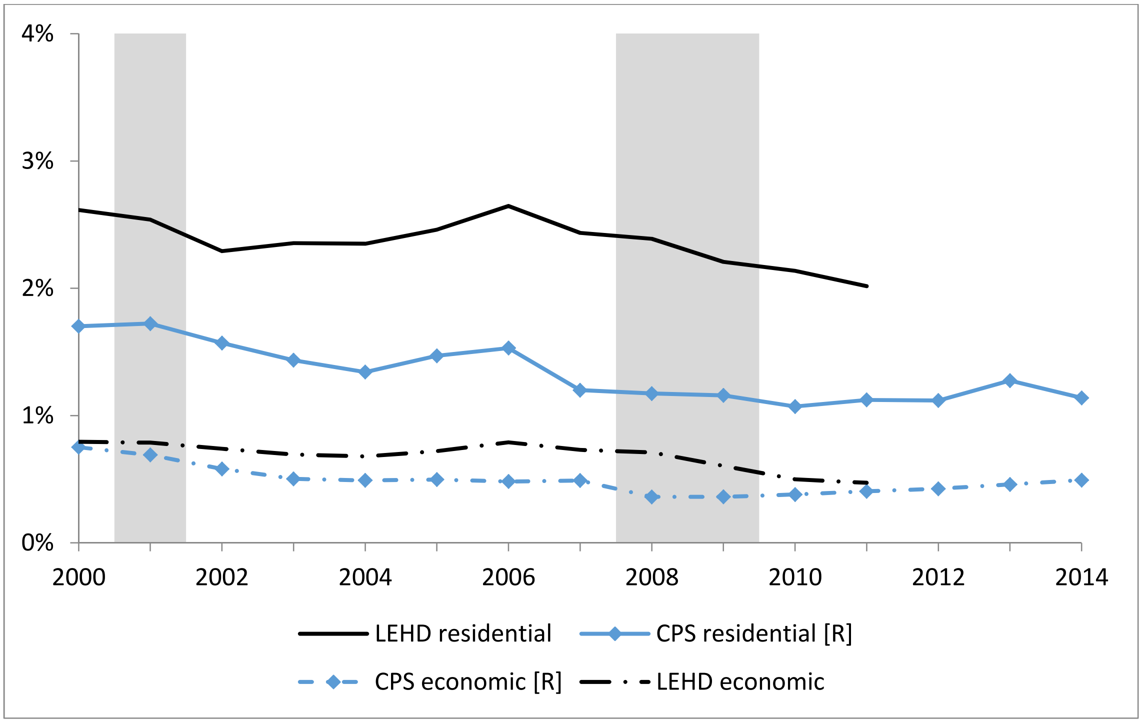
Note: Residential migration line is taken directly from Figure 4.1. Also plotted is the percentage of individuals who undertook residential migration and had more than one employer in the previous year. Economic migration line is taken directly from Figure 4.5. Also plotted is the percentage of individuals who undertook economic migration and had more than one employer in the previous year.

Figure 4.A2: Comparison of CPS Interstate Migration Rates Series for Subset of States with Consistent LEHD Data



Note: CPS [R] contains data for the subset of states that have data in the LEHD going back to the year 2000, for comparison of the CPS time series in Figure 4.1 to the CPS [R] time series in Figure 4.3. Note that CPS microdata on the particular state of residence is only available starting in 1982.

Figure 4.A3: Economic Migration in CPS and LEHD, CPS only for subset of states



Note: CPS [R] contains data for the subset of states that have data in the LEHD going back to the year 2000, for comparison of the CPS time series in Figure 4.1 to the CPS [R] time series in Figure 4.3. Note that CPS microdata on the particular state of residence is only available starting in 1982.

Appendix B: Selection and the Returns to Migration

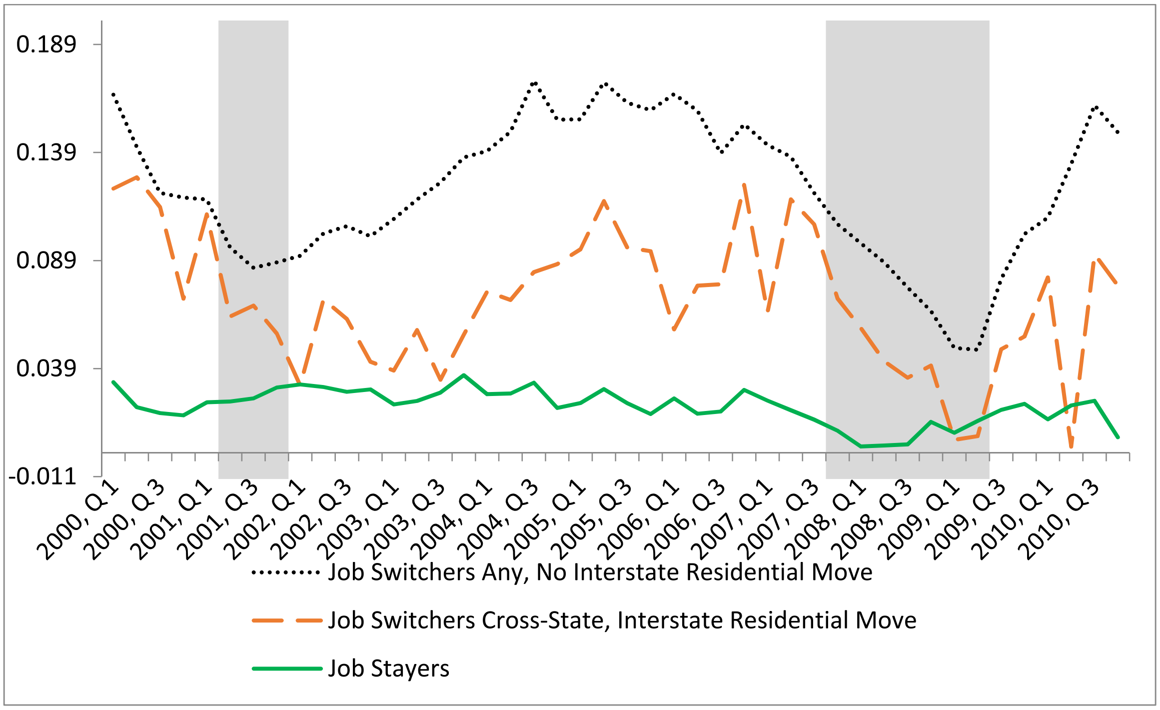
The choice to migrate is endogenous and those we observe to have migrated are workers for whom the benefit exceeds the cost. Therefore we measure whether earnings changes for migrants only affect a smaller subset of those most likely to move, given observable characteristics. Specifically, we use a straightforward propensity-score matching method that assumes selection on observables – that is, conditional on a vector of observable characteristics, X, the choice of migration is as good as random. We assume that the probability of a cross-state move follows a logit distribution and, using the LEHD data pooled across the years 2000-2010, we estimate:

$$\Pr(Transition) \equiv \lambda = \frac{1}{1 + \exp(-X_i \beta)}$$

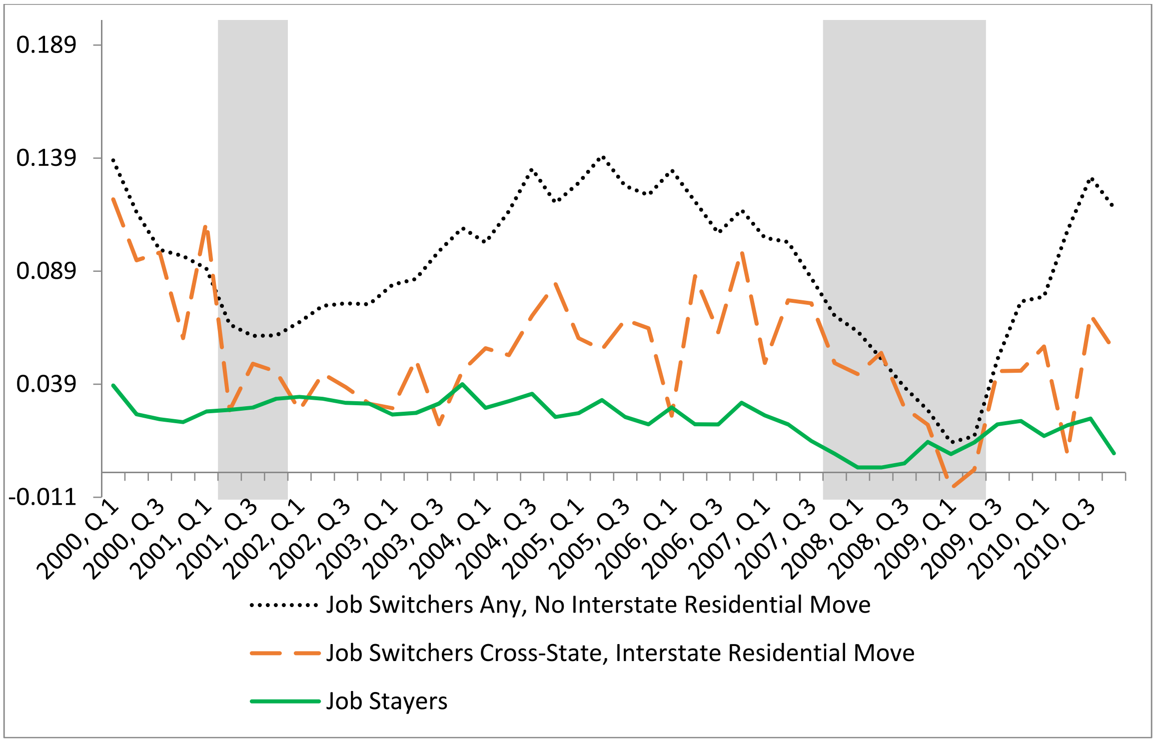
where X includes worker i's age, sex, race earnings, and the age, size, industry, and state of the origin employer.

With these predicted probabilities, we match using the set of migrants and non-migrants with overlapping support. The top panel of Figure 4.B1 presents the results. There is some evidence that the gap between job switchers who do not undergo an interstate move and those who do widened during the most recent recession when we control for observables, suggesting there was much less to gain in the economic downturn from switching to a job that necessitated a long-distance move. Overall, the measures exhibit levels and trends that are highly similar to those in Figure 4.6. The bottom panel of Figure 4.B.1 presents matching on the propensity to switch jobs, regardless of location, which produces levels and trends that are even more similar to the original figure. Overall, the returns to migration appear robust to our basic approach to accounting for selection.

Figure 4.B1: Change in Log Earnings, Propensity Score Matching
(a) Probability of Interstate Residential Move



(b) Probability of Job Change



Note: Panel (a) matches individuals on the probability of undertaking a residential move conditional on observable characteristics. Panel (b) matches individuals on the probability of undertaking *any* job change conditional on observable characteristics. Change in log earnings is calculated in the same way as in Figure 4.6.

Bibliography

Abowd, John M., Stephens, Bryce E., Vilhuber, Lars, Andersson, Fredrik, McKinney, Kevin L., Roemer, Marc, and Woodcock, Simon "The LEHD Infrastructure Files and the Creation of the Quarterly Workforce Indicators", *Producer Dynamics: New Evidence from Microdata*, 2009, 149-230.

Abraham, Katherine G., and Farber, Henry S. "Job Duration, Seniority, and Earnings." *The American Economic Review*, June 1987, 77, 278-97.

Altonji, Joseph G. and Shakotko, Robert A. "Do Wages Rise with Job Seniority", *Review of Economic Studies*, July 1987, 54, 437-459

Altonji, Joseph G. and Williams, Nicole "Do Wages Rise with Job Seniority? A Reassessment", *Industrial and Labor Relations Review*, April 2005, 58, 370-397

Bartik, Timothy J. "Who Benefits from State and Local Development Policies?", *W.E. Upjohn Institute for Employment Research*, 1991, 1-16

Becker, Gary S. "Investment in Human Capital: A Theoretical Analysis", *The Journal of Political Economy*, October 1962, 70, 9-49

Bound, John and Holzer, Harry J. "Demand Shifts, Population Adjustments, and Labor Market Outcomes during the 1980s", *Journal of Labor Economics*, January 2000, 18, 20-54

Blanchard, Olivier, and Katz, Lawrence "Regional Evolutions," *Brookings Papers on Economic Activity*, January 1992, 1, 1-76.

Bureau of Labor Statistics. "BLS spotlight on statistics." February 2012.

Couch, Kenneth A., and Placzek, Dana W. "Earnings Losses of Displaced Workers Revisited." *The American Economic Review*, March 2010, 100, 572-89

Dahl, Gordon. 2002. "Mobility and the Return to Education: Testing a Roy Model with Multiple Markets." *Econometrica* 70(6): 2367-2420.

Davis, Steven, and John Haltiwanger 2014. "Labor Market Fluidity and Economic Performance" NBER Working Paper #20479.

Davis, Steven, and Till von Wachter. 2011. "Recessions and the Costs of Job Loss." *Brookings Papers on Economic Activity* 43(2): 1-72.

Decker, Ryan, John Haltiwanger, Ron S Jarmin, and Javier Miranda. 2014. "The Secular Decline in Business Dynamism in the U.S." Unpublished mimeograph, University of Maryland.

Delgado, Mercedes, Porter, Michael E. and Scott, Stern “Clusters, Convergence, and Economic Performance”, *NBER Working Paper Series*, July 2012, Working Paper 18250

Delgado, Mercedes, Porter, Michael E. and Scott, Stern “Clusters and Entrepreneurship,” *Journal of Economic Geography*, May 2010, 10, 495-518

Dustmann, Christian and Meghir, Costas "Wages, Experience and Seniority", *Review of Economic Studies*, February 2005, 72, 77–108

Ellison, Glenn, Glaeser, Edward L., Kerr, William R. “What Causes Industry Agglomeration? Evidence from Coagglomeration Patterns”, *The American Economic Review*, June 2010, 100, 1195-1213

Faberman, Jason. 2008. “Job Flows, Jobless Recoveries, and the Great Moderation.” Federal Reserve Bank of Philadelphia Working Paper #08-11.

Fallick, Bruce, and Charles Fleischman. 2004. “Employer-to-Employer Flows in the U.S. Labor Market: The Complete Picture of Gross Worker Flows.” Federal Reserve Board Finance and Economics Discussion Series Working Paper 2004-34.

Fallick, Bruce, John Haltiwanger, and Erika McEntarfer. 2012. “Job-to-Job Flows and the Consequences of Job Separations.” Federal Reserve Board Finance and Economics Discussion Series Working Paper #2012-73.

Farber, Henry. 1999. “Mobility and Stability: The Dynamics of Job Change in Labor Markets.” In: *Handbook of Labor Economics*. Volume 3. Orley Ashenfelter and David Card (eds). Amsterdam: North Holland: 2439-2483.

Farber, Henry. “Job Loss in the Great Recession: Historical Perspective from the Displaced Workers Survey, 1984-2010.” NBER Working Paper #17040.

Freedman, Matthew L. “Job Hopping, Earnings Dynamics, and Industrial Agglomeration in the Software Publishing Industry”, *Journal of Urban Economics*, November 2008, 64, 590-600

Gathmann, Christina and Schönberg, Uta “How General Is Human Capital? A Task-Based Approach”, *Journal of Labor Economics*, January 2010, 28, 1-49

Goetz, Christopher. 2013. “Falling House Prices and Labor Mobility: Evidence from Matched Employer-Employee Data” US Census Bureau Center for Economic Studies Paper No. CES-WP- 13-43

Haltiwanger, John, Henry Hyatt, and Erika McEntarfer. 2015. "Cyclical Reallocation of Workers Across Employers by Firm Size and Firm Wage." NBER Working Paper #21235.

Haltiwanger, John, Henry Hyatt, Erika McEntarfer, Liliana Sousa, and Stephen Tibbets. 2014. "Firm Age and Size In the Longitudinal Employer-Household Dynamics Data" Center for Economic Studies Discussion Paper CES-14-16.

Henderson, Cody, and Henry Hyatt. 2012. "Estimation of Job-to-Job Flow Rates under Partially Missing Geography." U.S. Census Bureau Center for Economic Studies Working Paper #CES-12-29.

Hsieh, Chang-Tai, and Enrico Moretti. 2015. "Why Do Cities Matter? Local Growth and Aggregate Growth." Unpublished draft, University of Chicago.

Hyatt, Henry. 2015. "The Decline in Job-to-Job Flows." *IZA World of Labor* 175.

Hyatt, Henry and Erika McEntarfer. 2012a. "Job-to-Job Flows in the Great Recession" *American Economic Review: Papers & Proceedings 2012*, Vol. 102: Iss. 3: 580–583.

Hyatt, Henry and Erika McEntarfer. 2012b. "Job-to-Job Flows and the Business Cycle." Center for Economic Studies Working Paper #12-04, U.S. Census Bureau

Hyatt, Henry, Erika McEntarfer, Kevin McKinney, Stephen Tibbets, and Douglas Walton. 2014. "Job-to-Job (J2J) Flows: New Labor Market Statistics From Linked Employer-Employee Data." *JSM Proceedings 2014*, Business and Economics Statistics Section, 231-245.

Hyatt, Henry, and James Spletzer. 2013. "The Recent Decline in Employment Dynamics," *IZA Journal of Labor Economics* 2(5): 1-21.

Hyatt, Henry, and James Spletzer. 2016. "The Shifting Job Tenure Distribution." Forthcoming, *Labour Economics*.

Internal Revenue Service. 2012. "2011-2012 Migration Data Users Guide." Online documentation. Available at: https://www.irs.gov/pub/irs-soi/1112inpublicmigdoc.pdf (last accessed: May 19, 2016).

Jacobson, Louis S., LaLonde, Robert J. and Sullivan, Daniel G. "Earnings Losses of Displaced Workers", *The American Economic Review*, September 1993, 83, 685-709

Jovanovic, Boyan “Job Matching and the Theory of Turnover”, *The Journal of Political Economy*, October 1979, 87, 972-990.

Kambourov, Gueorgui, and Iourii Manovskii. 2008. “Rising Occupational and Industry Mobility in the United States: 1968-97.” *International Economic Review* 49(1): 41-79.

Kambourov, Gueorgui and Manovskii, Iourii “Occupational Specificity of Human Capital”, *International Economic Review*, February 2009, 50, 63-115.

Kaplan, Greg, and Sam Schulhofer-Wohl. 2012. “Interstate Migration Has Fallen Less Than You Think: Consequences of Hot Deck Imputation in the Current Population Survey. *Demography* 49(3): 1061-1074.

Kaplan, Greg and Schulhofer-Wohl, Sam “Understanding the Long-Run Decline in Interstate Migration”, *NBER Working Paper Series*, November 2012, Working Paper 18507

Koerber, Kin. 2007. Comparison of ACS and ACES Data on Geographic Mobility: 2004. Mimeo, U.S. Census Bureau.

Lazear, Edward P. “Agency, Earnings Profiles, Productivity, and Hours Restrictions,” *The American Economic Review*, September 1981, 71, 606-620.

Lazear, Edward P. “Firm-specific human capital: A skill-weights approach” *Journal of Political Economy*, October 2009, 117, 914–40.

Lazear, Edward, and James Spletzer. 2012. Hiring, Churn, and the Business Cycle. *The American Economic Review: Papers & Proceedings 2012*, 102(3), 575.

Leggieri, Charlene Arona Pistiner and James Farber. 2002. "Methods for Conducting an Administrative Records Experiment in Census 2000." *JSM Proceedings 2002*, Survey Research Methods Section, 2709-2713.

Ljungqvist, Lars and Sargent, Thomas J. “The European Unemployment Dilemma”, *Journal of Political Economy*, June 1998, 106, 514-550

Moretti, Enrico “Workers' Education, Spillovers, and Productivity: Evidence from Plant-Level Production Functions”, *The American Economic Review*, June 2004, 94, 656-690

Molloy, Raven, Smith, Christopher L. and Wozniak, Abigail “Internal Migration in the United States” *Journal of Economic Perspectives*, June 2011, 25, 173-196

Molloy, Raven, Christopher L. Smith, and Abigail Wozniak. 2014. "Declining Migration Within the U.S.: The Role of the Labor Market." NBER Working Paper #20065.

Molloy, Raven, Christopher Smith, Riccardo Trezzi, and Abigail Wozniak. 2016. "Understanding Declining Fluidity in the U.S. Labor Market." Conference draft, *Brookings Papers on Economic Activity*.

Moscarini, Giuseppe, and Kaj Thomsson. 2007. "Occupational and Job Mobility in the U.S." *Scandinavian Journal of Economics* 109(4): 807-836.

Neal, Derek "Industry-Specific Human Capital: Evidence from Displaced Workers", *Journal of Labor Economics*, October 1995, 13, 653-677

Parent, Daniel "Industry-Specific Capital and the Wage Profile: Evidence from the National Longitudinal Survey of Youth and the Panel Study of Income Dynamics", *Journal of Labor Economics*, April 2000, 18, 306-323

Pavan, Ronni "Career Choice and Wage Growth", *Journal of Labor Economics*, July 2011, 29, 549-587

Poletaev, Maxim and Chris Robinson, 2008. "Human Capital Specificity: Evidence from the Dictionary of Occupational Titles and Displaced Worker Surveys, 1984–2000." *Journal of Labor Economics*, 26 (3): 387-420.

Porter, Michael E., "Clusters and Competition: New Agendas for Companies, Governments, and Institutions." *Harvard Business School Press*, March 1998, pp. 197-299.

Ruggles, Steven, Matthew Sobek, Trent Alexander, Catherine A. Fitch, Ronald Goeken, Patricia Kelly Hall, Miriam King, and Chad Ronnander. 2004. Integrated Public Use Microdata Series: Version 3.0 (Machine-readable database). Minneapolis, MN: Minnesota Population Center (producer and distributor).

Schmieder, Johannes F., von Wachter, Till, and Bender, Stefan "The Long Term Impact of Job Displacement in Germany during the 1982 Recession on Earnings, Income, and Employment," *Columbia University Working Paper*, November 2009

Stewart, Jay. 2007. "Using March CPS Data to Analyze Labor Market Transitions." *Journal of Economic and Social Measurement* 32(2-3): 177-197.

Topel, Robert H. "Local Labor Markets", *Journal of Political Economy*, June 1986, 94, 111-143

Topel, Robert H. "Specific Capital, Mobility, and Wages: Wages Rise with Job Seniority", *The Journal of Political Economy*, February 1991, 99, 145-176.

Topel, Robert, and Michael Ward. 1992. Job Mobility and the Careers of Young Men. *The Quarterly Journal of Economics*, 107(2): 439-479.

von Wachter, Till, Elizabeth Handwerker, and Andrew Hildreth. 2009. "Estimating the 'True' Cost of Job Loss: Evidence Using Matched Data from California 1991-2000. Mimeo.

von Wachter, Till, Song, Jae, and Manchester, Joyce "Long-Term Earnings Losses due to Mass Layoffs During the 1982 Recession: An Analysis Using U.S. Administrative Data from 1974 to 2004", *Columbia University Working Paper*, April 2009

Wozniak, Abigail. 2010. "Are College Graduates More Responsive to Distant Labor Market Opportunities?" *Journal of Human Resources* 45(4): 944-970.