Essays in Labor Market Economics

Benedikt Herz

TESI DOCTORAL UPF / ANY 2014

DIRECTOR DE LA TESI
Prof. Thijs van Rens (University of Warwick, Centre for Macroeconomics, IZA and CEPR)

Departament d'Economia i Empresa

Universitat Pompeu Fabra
Barcelona

To my parents

Acknowledgements

First and foremost I would like to thank my advisor Thijs van Rens for his invaluable guidance and his continuous and always insightful support while writing this thesis. I am especially thankful to him for co-authoring the second chapter of this thesis with me. I learned a lot from this collaboration. I am also grateful that he made it possible for me to spend time as a visiting PhD student at the University of Warwick.

Many other people at CREI and Universitat Pompeu Fabra have helped me during these years. An incomplete list includes Albrecht Glitz, Regis Barnichon, Vasco Carvalho, Kris Nimark, and Rosemarie Nagel. Their comments were always helpful and substantially increased the quality of my dissertation. I am particularly thankful to Michael McMahon for his advice and support, in particular while I was visiting the University of Warwick. I am grateful to Gianluca Violante for his help and for inviting me to New York University. Moreover, I owe thanks to Marta Araque and Laura Augustí for their outstanding administrative support.

I would like to thank my colleagues and friends with whom I spent a lot of time at UPF and in Barcelona: Philipp, Fabrizio, Stefan, Lien, Bruno, and my officemates Sofia and Silvio. Finally, I would like to thank my family for the continuous support.

Abstract

This thesis consists of three essays. In the first essay, I empirically evaluate the importance of *wait unemployment*. Instead of taking the next best job, a displaced worker has an incentive to stay unemployed and wait for a vacancy that matches his skills. Using a difference-in-difference approach for identification, I find that this mechanism is an important component of aggregate unemployment in the U.S. labor market. In the second essay (co-authored with Thijs van Rens), we propose an accounting framework to decompose mismatch unemployment into different components and analyze its behavior over the business cycle. In the third essay, I reevaluate the evidence for job polarization in the U.S. labor market. I find that existing evidence is biased. What really mattered for changes in the occupation structure since the 1990s was the education-premium.

Resumen

Esta tesis consta de tres ensayos. En el primer ensayo, se evalúa la importancia del desempleo de espera, en el cual se asume que una persona que ha perdido su empleo, preferirá esperar una vacante que cumpla con sus habilidades, en lugar de tomar el primer empleo disponible. Usando un enfoque de "diferencias en diferencias" por identificación, se encuentra que el desempleo de espera es un componente significativo del desempleo en E.U. En el segundo ensayo (escrito en colaboración con Thijs van Rens), se propone un marco conceptual para descomponer el desempleo estructural y se analiza el comportamiento de cada uno de sus componentes en el ciclo de negocio. En el tercer ensayo, se reevalúa la evidencia empírica existente de la polarización del mercado en el mercado de trabajo de E.U. y se encuentra que la evidencia empírica existente esta sesgada. El principal factor que ha influido en los cambios en la estructura de ocupación desde los 90s ha sido la prima educativa.

Foreword

The thesis consists of three chapters. In the first chapter I show empirical evidence that *wait unemployment* is an important driving force of unemployment in the United States. When facing slack demand for his skills, an unemployed worker might rationally prefer to wait through a long spell of unemployment instead of seeking employment at a lower wage in a job he was not trained for. I evaluate this trade-off empirically using micro-data on displaced workers. To achieve identification, I use a difference-in-difference approach exploiting two sources of variation. Firstly, the more a worker invested in occupation-specific human capital the more costly it is for him to switch occupations and the higher is therefore his incentive to wait. Secondly, I use geographic variation. In a diverse local labor market where employment is not concentrated in few industries but spread out over many sectors it is less likely that a worker will have to switch occupations in the first place; the potential cost of changing occupations are less likely to be binding. My estimates suggest that wait unemployment is a major reason behind extended unemployment spells. Under conservative assumptions, about between 5% and 20% of total unemployment in the United States can be attributed to wait unemployment.

The second chapter is joint work with Thijs van Rens.[1] We investigate unemployment due to mismatch in the US over the past three decades. We propose an accounting framework that allows us to estimate the overall amount of mismatch unemployment as well as the contribution of the frictions that caused the mismatch. Mismatch is quantitatively important for unemployment and the cyclical behavior of mismatch unemployment is very similar to that of the overall unemployment rate. Geographic mismatch is driven primarily by wage frictions. Mismatch across industries is driven by wage frictions as well as barriers to job mobility. We find virtually no role for worker mobility frictions.

In the third chapter I reevaluate the evidence for job polarization, the increasingly U-shaped growth of jobs with respect to their skill requirements since the 1990s. I show that existing evidence of job polarization is based on non-consistent occupation data and therefore potentially biased. Using consistent data provided by the the Current Population Survey, I find that since the 1990s the growth of occupations' employment shares was slightly U-shaped when the average wage

[1] An earlier version of this essay was previously circulated under the title "Structural Unemployment."

is used as a proxy for skill. I find no evidence of polarization when using years of schooling as a proxy. I then combine these findings and demonstrate that instead since the 1990s there is strong evidence of a polarization with respect of the education-premium – the wage conditional on schooling. This is worrying, since this development points towards increasing skill-mismatch.

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Chapter 1

WORKER MOBILITY COST AND WAIT UNEMPLOYMENT

1.1. Introduction

Labor is not a homogeneous commodity. The Dictionary of Occupational Titles (DOT) published by the U.S. Department of Labor distinguishes among over 12000 occupations. A majority of these occupations require highly specialized training. According to the DOT, the majority of the workforce in the United States is employed in occupations that require more than a year of vocational preparation *specific* to that occupation. The U.S. labor market is therefore not a single market where one homogeneous type of labor is traded. Instead, it is more appropriate to think of it as being composed of many skill-specific sub-markets or *islands*.

Two distinct but potentially complementary mechanisms of how this heterogeneity can give rise to unemployment have been discussed in the literature. On the one hand, search models – in particular models based on Lucas Jr. and Prescott (1974) – assume that moving across sub-markets is time-intensive. In a heterogeneous labor market that is subject to reallocation shocks, unemployment can therefore arise as a consequence of workers looking for new job opportunities.

An alternative view is that a worker who has been displaced is still attached to his pre-displacement job and tries to find reemployment in a similar position (e.g., Shimer, 2007; Alvarez and Shimer, 2011). A potential consequence is what I refer to as *wait unemployment*: instead of searching on different islands, workers prefer to wait and sit through long unemployment spells hoping that their old job reappears. Whereas search is a theory of former steel workers looking for positions as nurses, the latter is a theory of former steel workers waiting for their former plant to reopen (Shimer, 2007).

The objective of this paper is to test and quantify the concept of wait unemployment and to assess its importance for aggregate unemployment in the United States. Because human capital is only partially transferable across jobs, a displaced worker strongly prefers to find a new position that is as similar as possible to the job he worked in before. If such a position is not readily available the worker faces a trade-off. On the one hand, he can just work in a different job. Because human capital is usually compensated by a higher wage, this will go along with a wage-loss that I refer to as a *mobility cost*. The alternative is to evade this mobility cost and to instead sit through a long spell of unemployment and wait until a similar job is available.

I quantify this trade-off using micro-data on displaced workers in the United States. To achieve identification I make use of a difference-in-difference strategy in the spirit of Rajan and Zingales (1998) that relies on two sources of variation. Firstly, I exploit that the extent of specific human capital a worker invested in varies by occupation. For example, a physician spent many more years preparing for his job than a barkeeper. I operationalize this by using data on the *specific vocational preparation* (SVP) required to work in a given occupation provided by the Dictionary of Occupational Titles. A displaced worker who leaves an occupation with high SVP gives up a substantial stock of human capital and suffers a wage-loss. The higher the SVP of the occupation a worker is trained in, the higher is therefore the mobility cost this worker is facing when switching occupations. As shown in Figure 1.1, I find that this relation is strongly confirmed in the data.

Secondly, I exploit geographic variation by using local labor market information from the U.S. Census. Local labor markets differ concerning their *diversity*. I refer to a labor market as "diverse" when employment is not concentrated in few industries but spread out over many different sectors. I exploit that in such a diverse market it will be relatively easier to find a job that matches a worker's skill-set, even when highly specialized; mobility cost are therefore less likely to be binding.

Based on these two sources of variation, I construct the following test. I use data from the Current Population Survey Displaced Worker Supplement (CPS-DWS). This data-set covers information on completed unemployment spells of workers displaced between 1983 and 2012 in the US labor market. I examine the sample of displaced workers who managed to find a job in the same occupation they worked in before. I then compare the unemployment spells of more and less specialized workers in local labor markets with high and low diversity. If wait unemployment matters, workers with very specific training should have relatively longer spells in markets that exhibit few diversity and where mobility costs are likely to be binding. In diverse markets, on the other hand, the difference should be smaller or even non-existent.

I find strong support for the existence and quantitative importance of wait unemployment. Importantly, the difference-in-difference approach allows me to control for occupation- (and local-labor market) fixed effects. I can therefore exclude the possibility that my results are driven by any inherent differences between occupations. It is further reassuring that the effect disappears when the sub-sample of occupation *changers* is used for estimation instead. This further corroborates my interpretation that the observed extended unemployment spells are due to workers sitting out bad spells to find work in their previous occupation.

Under the additional assumption that specific vocational preparation (SVP) af-

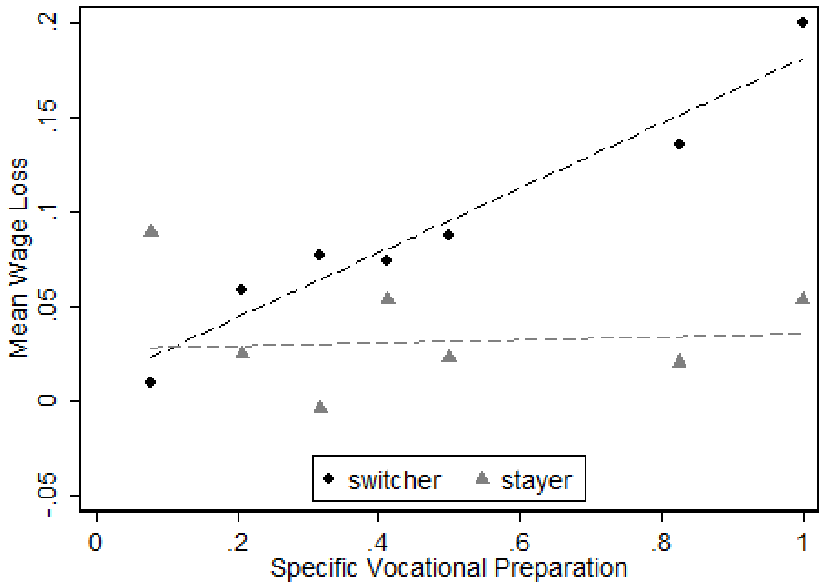

Figure 1.1: The relationship between the wage-loss upon leaving an occupation and the specific vocational preparation (SVP) of an occupation is shown. I differentiate between workers who report to have switched occupations after displacement (black circles) and workers who stayed in the same occupation (gray triangles). The lines visualize weighted linear fits. It is apparent that the cost of switching occupations – what I refer to as *mobility cost* – is strongly increasing in SVP. The fact that a similar relation is not observable for occupation stayers is reassuring evidence that the driving force is indeed the loss of occupation-specific human capital of switchers. Data on SVP comes from the the revised fourth edition of the Dictionary of Occupational Titles (1991). Data on wage-losses comes from the Current Population Survey Displaced Workers Supplement (CPS-DWS).

fects unemployment duration only through mobility cost, I then push the exercise further and use SVP as an instrument in a two-stage least squares regression. My results indicate that worker mobility costs are an important driving force of unemployment. For example, according to data from the U.S. Census, *Raley-Carey, NC* is a substantially less diverse labor market than *St. Louis, MO-IL*. In the latter, mobility cost are therefore less likely to be binding. IV estimates imply that in order to evade a 10% wage loss, workers are willing to sit through 40% longer unemployment spells in *Raley-Carey, NC* relative to *St. Louis, MO-IL*.

My results have important implications for aggregate unemployment. Using a back-of-the-envelope calculation I find that there would be between 5% and 20% less unemployment in the United States between 1985 and 2011 if all human capital would be transferable and switching occupations would not entail any mobility cost.

1.1.1. Related Literature

The idea that specificity of human capital can lead to long spells of wait unemployment is not new. To the best of my knowledge, Murphy and Topel (1987) are the first to mention this channel explicitly. In particular, they note that it is compatible with the observation that increased unemployed tends to go along with reduced inter-sectoral mobility. This finding is strong evidence against sectoral-shift theories of unemployment as, for example, proposed by Lilien (1982).

One strand of literature formalizes this idea in models where workers can undergo spells of "rest unemployment." Jovanovic (1987), Hamilton (1988), King (1990), Gouge and King (1997), and more recently Alvarez and Shimer (2008) extend the basic island model by Lucas Jr. and Prescott (1974). When a worker is subject to an adverse shocks that lowers his wage he might rationally prefer not to work and to wait for better times instead of undertaking a costly search for a better industry or occupation on another "island." A sharp difference to my framework is that in models of rest unemployment wages always fully adjust and clear markets. Rest unemployment exists because workers have a utility from resting that might dominate working at the current market wage. In my framework, on the other hand, the extent of wage adjustments is a critical factor in driving unemployment. Workers are never voluntarily unemployed but they are queuing in order to put their human capital to optimal use.

There is an older literature on transitional or wait unemployment that most resembles the concept of unemployment I have in mind. The basic idea is that due to rigidities there are good and bad jobs that pay workers of equal ability different wages. A fraction of workers rationally decide to queue and go through long unemployment spells in order to get one of the highly paid jobs. This creates unemployment. Recently Alvarez and Shimer (2008), based on ideas by Summers et al. (1986), claim that wage dispersion caused by unions leads to substantial unemployment. In a classical paper Harris and Todaro (1970) identify wage differentials between rural and urban jobs as a source of wait unemployment. Wait unemployment in my framework is different inasmuch that workers are not queuing in order to seize rents but because they want to preserve valuable specific human capital.

I also contribute to a big literature that empirically studies the specificity of human capital, particularly by analyzing earnings losses of displaced workers. Early papers in this literature tried to estimate the cost of of losing firm-specific capital (Abraham and Farber, 1987; Altonji and Shakotko, 1987; Kletzer, 1998; Topel, 1991). Neal (1995) and Parent (2000) analyzed the costs of changing industry after displacement. More recently, there is growing evidence that human capital is mostly occupation-specific (Kambourov and Manovskii, 2009). I extend these findings by showing that the cost of switching differs substantially *across* occupations; leaving an occupation is more costly for workers who underwent lengthy and highly specific occupational training (e.g., physicians) than for workers in occupations that makes use of mostly general skills (e.g., waiters).

This paper also contributes to the literature on labor market mismatch and structural unemployment. This research has attracted increased interest in recent times due to high and persistent unemployment rates during and after the Great Recession of 2008 and because of claims that "structural factors" are behind this development (Kocherlakota, 2010). Sahin et al. (2014) combine unemployment records with data on posted vacancies to calculate mismatch unemployment in the U.S. labor market. They find that mismatch across industries and occupations explains at most one-third of the increase of unemployment in the Great Recession while geographic mismatch does not play a role. Barnichon and Figura (2011b) use CPS data to explore the effect of mismatch on matching efficiency. They find that lower matching efficiency due to mismatch can have significant detrimental effect for unemployment in recessions. In Chapter 2 of this thesis, my co-author Thijs van Rens and me push the analysis a step further by identifying several potential driving forces of mismatch unemployment and estimate their relative importance. This paper is complementary to this literature since I show evidence of how a specific channel – workers "waiting" for reemployment since they made specific investments ex-ante – contributes to mismatch unemployment. Shedding light on such a concrete mechanism is important since it makes the problem more approachable from a policy perspective.

The remainder of this article proceeds as follows. In Section 1.2, I describe the different data sources that I use for estimation. In Section 1.3, I discuss the estimation framework and the results. I then assess the importance of this mechanism for the aggregate unemployment rate in the United States with a "back-of-the-envelope" calculation in Section 1.4. In Section 1.5 I conduct further tests and explore the robustness of the results. Section 1.6 concludes. An empirical framework that describes the idea of wait unemployment in a more formal way and that motivates the identification strategy can be found in Appendix 1.8.1.

1.2. Data and Measurement

1.2.1. Displaced Workers

My primary dataset is the Current Population Displaced Workers Supplement (CPS-DWS) that has been widely used for research on earnings-losses of displaced workers.[1] The CPS-DWS was part of the CPS in January 1988, February 1994, 1996, 1998, and 2000, and in January 2002, 2004, 2006, 2008, 2010, and 2012. CPS respondents are asked whether they lost a job in the three years prior to the survey date (five years in 1988). Those individuals who report having lost a job are part of the CPS-DWS and asked follow-up questions. This ex-post design is the great advantage of the CPS-DWS because it allows the researcher to observe *completed* unemployment spells and provides information about a worker's old and new job. In particular, job-losers are asked about both their pre- and post-

[1] Some of the classic papers are Topel (1990), Gibbons and Katz (1991), Carrington (1993), Neal (1995), Farber et al. (1993), and Farber et al. (1997).

displacement weekly earnings, their pre- and post-displacement occupation,[2] reasons for displacement, and about the length of their initial unemployment spell.[3]

My baseline sample consists of workers between 20 and 67 years of age who lost a job in the private sector between 1983 and 2012 due to plant closing, insufficient work, or because their shift was abolished. Note that this does not only exclude quits but also – according to the *CPS Interviewer Memorandum* – it explicitly excludes events where workers are fired for "poor work performance, disciplinary problems, or any other reason that is specific to that individual alone" (Farber et al., 1993). That is, as a first approximation the displacement can be seen as being an exogenous shock. I further restrict the sample to those who lost a full-time job and are currently full-time re-employed. This is necessary because I only observe hours worked for the current job, but not for the pre-displacement job. Weekly earnings is therefore the only wage measure available for both the pre- and post-displacement job. All earnings are deflated by the GDP deflator with base year 2005 that I obtained from the Bureau of Economic Analysis. Following earlier papers in the literature, I drop workers who report pre- or post-displacement earnings below 100$ per week

The CPS-DWS only asks follow-up questions about at most one lost job. If an individual lost more than one job within the three year period, he is only asked about the job he held the longest. To guarantee that the "initial unemployment spell" is from the spell immediately preceding the current job, I exclude multiple job losers included in the sample.[4] Moreover, note that due to an error in the construction of the survey the unemployment spell is not reported in the 1994 CPS-DWS.

Advance notice In most specifications I further restrict the sample to workers who report not having received an advance notice of displacement. Not making this restriction might lead to misleading results, in particular when the dependent variable is the unemployment duration reported by a worker. The reason is that an advance notice of displacement gives workers the possibility to undertake on-the-job-search which is not captured by the reported unemployment duration. Moreover, it seems that the efficiency of this potential on-the-job-search systematically varies with other co-variates such as education and specific vocational preparation. It would therefore not be sufficient to account for this effect by simply including an advance notice indicator variable as a control.

[2]Occupation codes used in the CPS underwent several changes between 1988 and 2012. I therefore construct 384 time-consistent occupation codes by using the conversion tables provided by Meyer and Osborne (2005).

[3]The exact wording of the question is "After that job ended, how many weeks went by before you started working again at another job?" That is, this is not exactly an unemployment spell but includes workers who are inactive. So the best fit between my empirical framework and the data that is available is obtained when one thinks of unemployed workers in the framework as comprising both inactive and unemployed workers.

[4]For example, compare Rodríguez-Planas (2011).

Table 1.1: Specific Vocational Preparation

	SVP	Time required
1	0.1%	Short demonstration only
2	8.0%	Anything beyond short demonstration up to and including 1 month
3	22.1%	Over 1 month up to and including 3 months
4	33.0%	Over 3 months up to and including 6 months
5	43.1%	Over 6 months up to and including 1 year
6	52.9%	Over 1 year up to and including 2 years
7	83.7%	Over 2 years up to and including 4 years
8	99.9%	Over 4 years up to and including 10 years
9	100.0%	Over 10 years

Notes: Definitions of the various levels of specific vocational preparation from the 1991 revised fourth edition of the Dictionary of Occupational Titles are reported. The first column shows the original (ordinal) variable. The second column shows the transformed (cardinal) variable. The latter was generated by constructing an empirical cumulative distribution function of SVP based on the 1995 basic monthly CPS data. For example, 52.9% of the workforce in 1995 were employed in occupations requiring at most 2 years of specific vocational preparation. Note that there is only one occupation in the highest category (*judges*) and one in the lowest category (*refuse and recyclable materials collectors*).

Plant closing As mentioned above, according to the CPS-DWS definitions the displacement of a worker should be an exogenous event. However, it might still be that *de-facto* workers with (unobserved) low ability are more likely to be displaced. This would result in a selection problem. If, for example, low ability workers are also systematically endowed with less specific vocational training they might also be less willing to engage in wait unemployment. The consequence would be an omitted variable problem that potentially leads to downward-biased estimates.

As a robustness check, in some specifications I therefore restrict the sample to workers who report having been displaced because of plant closing and I exclude workers who state an abolished shift or insufficient work as the reason for displacement. When a plant is closed down, all workers are displaced by definition; the firm does not have any discretion with respect to whom to lay off. This greatly reduces concerns about a potential selection problem.[5] The restricted sample is therefore arguably preferable to the baseline sample but it comes at the cost of a substantially reduced sample size.

1.2.2. Specific Vocational Preparation

The cost of the occupation-specific training required by an occupation is not observable. I therefore proxy for this variable by drawing on information provided by the revised fourth edition of the Dictionary of Occupational Titles (DOT)

[5] See Gibbons and Katz (1991).

published by the U.S. Department of Labor in 1991. The DOT evaluates 12741 occupations along several dimensions, such as physical and cognitive demands. In particular, the DOT reports the "specific vocational preparation" (SVP) required to work in a given occupation. SVP is defined as "the amount of lapsed time required by a typical worker to learn the techniques, acquire the information, and develop the facility needed for average performance in a specific job-worker situation."[6] That is, I proxy the *cost* of required SVP by the *time* needed to acquire this training.

The variable is categorical and ranges from 1 to 9 where 9 refers to very high specificity. In order to make occupation codes comparable with the CPS data, I first use a crosswalk provided by the National Crosswalk Service Center[7] to match each of the 12741 DOT occupations to one of 469 U.S. Census 1990 occupations. I then apply the conversion table from Meyer and Osborne (2005) to match each U.S. Census occupation into one of 368 consistent occupation codes. The SVP of an occupation is then defined as the median SVP of all matched DOT occupations.[8]

I generate a cardinal variable by calculating the empirical cumulative distribution function of SVP using occupational employment data from the 1995 CPS.[9] The transformed variable can then be interpreted as the share of the employed workforce in 1995 that works in occupations with equal or smaller required specific vocational preparation. The original variable and its transformation are described in Table 1.1.

1.2.3. Diversity of Local Labor Markets

My identification strategy exploits that mobility cost should only matter when workers are forced to switch occupations and mobility is necessary. To capture this source of variation empirically, I rely on differences across local labor markets in the United States. More precisely, I exploit that the more diversified a local labor market is, the less likely it is it that switching occupations is necessary. In a highly diversified market it is therefore more likely that a displaced worker can make use of this specific human capital endowment than in a concentrated local labor market.

I follow a popular approach in labor economics by assuming that local labor markets are well captured by the concept of Metropolitan Statistical Areas (MSA) as defined by the Office of Management and Budget (OMB) (e.g. Card, 2001; Mazzolari and Ragusa, 2011).[10] A problem is that the MSA delineations are not con-

[6]See Appendix 1.8.2 for a detailed definition.

[7]http://www.xwalkcenter.org

[8]Optimally, one would use the employment-weighted mean of the matched DOT occupations. However, DOT occupation categories are very narrowly defined such that reliable employment figures are not available. Since there is few variation of the SVP measure within the DOT occupations that are matched to the same consistent occupation code the results of applying weights would be negligible in any case.

[9]Results in the paper are robust to the choice of the base year.

[10]Competing concepts are to use U.S. states (Topel, 1986), counties (Gould et al., 2002), or so-called commuting zones (Tolbert and Killian, 1987; Tolbert and Sizer, 1996; Autor et al., 2013). See

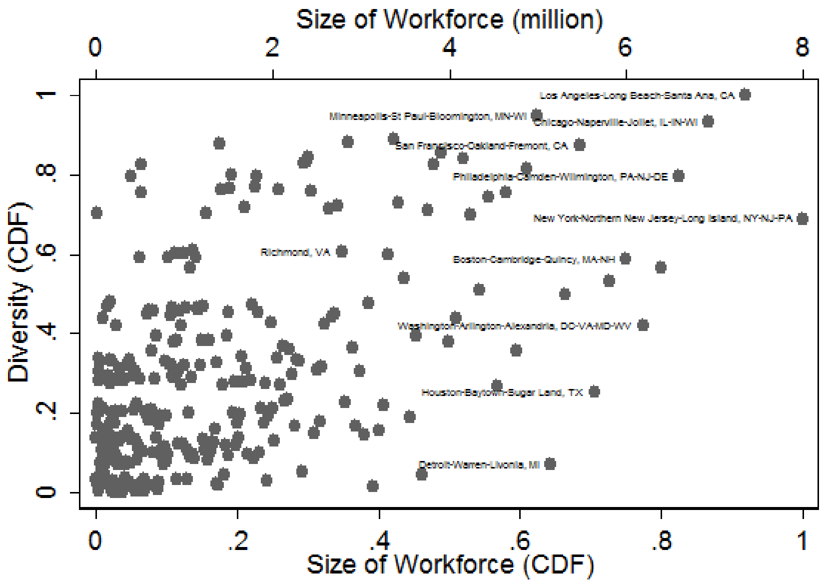

Figure 1.2: The relation between the variable $DIVERSITY_m$ and the size of the workforce in a given metropolitan area is shown for the year 2000 Census. The upper X-axis shows the workforce in million persons while the cumulative distribution function is reported on the lower X-axis shows. Labels are shown for some selected metropolitan areas. For example, the *San Francisco-Oakland-Fremont, CA* metropolitan area has high industrial diversity relative to the size of its workforce. While only 12.5% the US urban workforce reside in metropolitan areas with higher $DIVERSITY_m$, 31.6% live in metropolitan areas that have a larger workforce. The data comes from the Integrated Public Use Microdata Series (IPUMS-USA) 1% sample of the U.S. Census for the year 2000 (Ruggles et al., 2010).

stant over time. Moreover, by construction they do not cover workers living in rural areas.

My preferred empirical measure of the *diversity* of a local labor market m is given by

$$DIVERSITY_m = 1 - \sum_{k=1}^{K} \tau_{mk}^2 \tag{1.1}$$

where τ_{mk} captures the share of the workforce in local labor market m employed in industry k obtained from the Integrated Public Use Microdata Series (IPUMS-USA) 1% sample of the U.S. Census for the years 1980, 1990, 2000 (Ruggles et al., 2010).[11] $DIVERSITY_m$ is a measure of industry fractionalization and

the appendix A.2.1 of Dorn (2009) for a detailed discussion of local labor market concepts.

[11]There are some challenges to matching the CPS-DWS data to the U.S. Census data. Between 1988 to 2012, the CPS-DWS uses three different MSA classifications. In 1988 and 1992 it uses the U.S. Office of Management and Budget (OMB) 84 definitions, from 1994 to 2004 the OMB 93 definitions, and from 2006 on the OMB 2003 definitions. Before 2006 the match-

related to a Herfindahl index.[12] It has a straightforward interpretation: it captures the probability that two individuals who are randomly sampled from local labor market m are employed in different industries.

The motivation for using an *industry*-based measure of diversification is that SVP captures human capital that is occupation-specific but transferable across industries.[13] The higher $DIVERSITY_m$ of a local labor market m, the more likely is it that a given occupation spans many different industries, leading to a more steady stream of vacancies specific to that occupation. In a very concentrated market, on the other hand, a given occupation might only exist in one industry. If this industry is doing badly, it will be difficult to put specific vocational skills to use elsewhere in the local market.[14] Mobility is therefore more of an issue in a concentrated market and the influence of mobility cost should be higher.

To facilitate interpretation of estimates, I transform the variable by generating an empirical cumulative distribution function. This new variable can then be interpreted as the percentage of the total U.S. metropolitan workforce that lives in a MSA with equal or less diversity. As shown in Figure 1.2, $DIVERSITY_m$ is strongly correlated with the size of the workforce in a given city.

1.3. Estimation Framework and Results

The relation between wait unemployment, mobility cost, and specific vocational preparation can be described by the following two regression equations.[15] The first regression is estimated on the sub-sample of workers whose pre- and post-displacement occupation is *not* the same:

$$MC_{ijt} = \alpha_1 + \alpha_2 \; SVP_j + \theta' \mathbf{X_i} + \epsilon_{ijt} \tag{1.2}$$

ing to the U.S. Census data is straightforward. However, the OMB 2003 classification underwent some more substantial changes which complicates the matching to the 2000 U.S. Census data. I use a "geographic relationship file" provided by the Census that can be found under `https://www.census.gov/population/metro/data/other.html`.

[12]Measures of fractionalization have been widely used in economic research, in particular to analyze the impact of ethnic diversity on corruption, conflict, and various economic or political outcome variables (e.g., Mauro, 1995; Easterly and Levine, 1997; Alesina et al., 1999; Miguel and Gugerty, 2005).

[13]For example when an electrician is switches occupations and works as a waiter, he will lose his specific training. However, when a workers switches from being an electrician in the autmotive industry to the mining industry he does *not* lose his specific vocational preparation. In Section 1.3.1 I show evidence that SVP indeed seems to capture purely occupation-specific training.

[14]Ideally, one would calculate an occupation-specific diversity measure for a given local labor market. However, because I distinguish among 368 occupations and there are about 300 local labor markets (according to the 2003 OMB definition) the number of observations per cell would become too small.

[15]A more formal motivation of the estimation strategy can be found in Appendix 1.8.1.

The second regression is estimated on the sub-sample of workers whose pre- and post-displacement occupation is the same:

$$UNEM_{ijt} = \beta_1 + \beta_2 \; MC_{ijt} + \theta' \mathbf{X_i} + \eta_{ijt} \tag{1.3}$$

MC_{ijt} is the mobility cost of a displaced worker i measured as the wage-loss (the log-earnings difference) he suffers when leaving his pre-displacement occupation j. The first regression captures the relation visualized in Figure 1.1: conditional on switching to another occupation after displacement, there is a strong positive correlation between the extent of occupation-specific training a worker invested in and the wage-loss he experiences. This would imply $\alpha_2 > 0$. As described in Section 1.2.2, I proxy the occupation-specific training by the length of the *required* specific training of the worker's last occupation, SVP_j. I further include a vector of worker-specific demographic control variables $\mathbf{X_i}$ to reconcile the model with the data and account for the fact that in reality workers differ among many more dimensions than the ones captured by the simple model.

The second regression formalizes the idea of wait unemployment. The higher the (expected) mobility cost MC_{ijt} a worker is facing, the longer the unemployment spell $UNEM_{ijt}$ he is willing to go through in order to evade switching occupations. If wait unemployment matters, it should hold $\beta_2 > 0$. $UNEM_{ijt}$ is measured as the natural logarithm of 1 plus the weeks of unemployment: $log(1 + weeks_{ijt})$.[16]

1.3.1. The Effect of SVP on Mobility Cost

Regression equation (1.2) relates to a big literature in labor economics that studies the specificity of human capital by analyzing earnings losses of displaced workers. Early papers in this literature try to shed light on the degree of firm-specificity of human capital (Abraham and Farber, 1987; Altonji and Shakotko, 1987; Kletzer, 1998; Topel, 1991) while Neal (1995) and Parent (2000) analyze the costs of switching industry. More recently, there is growing evidence that human capital is actually mostly occupation-specific (e.g., Kambourov and Manovskii, 2009).

Here I contribute to this literature by showing that the extent of human capital lost upon leaving an occupation also differs substantially *across* occupations. In particular, I show that the SVP_j of an occupation is a good predictor of the extent of human capital lost upon switching. For example, a physician who underwent lengthy and highly specific occupational training will lose a substantial amount of human capital upon leaving his occupation. This is reflected in a high wage-loss. On the other hand, for workers in occupations that make use of mostly general skills (e.g., bartender, cashier) switching occupations entails only a limited loss of human capital resulting in only marginal wage-losses.

[16]The results in this paper are robust to instead using $log(weeks_{ijt})$ and dropping observations with an "unemployment spell" of zero weeks.

Column (1) of Table 1.3 shows estimates of equation (1.2). To account for common time effects, I add year-of-displacement fixed-effects. The coefficient on SVP_j is positive and significant at the 1% level. As described in Section 1.2.2, SVP_j is the share of the workforce that works in occupations requiring less or equal specific vocational preparation than occupation j. The estimates therefore imply that every 10 percentage point increase in the SVP distribution leads to a 1.5 percentage point increase in the expected wage-loss when switching occupations after displacement. This magnitude is economically important.

A problem with this simple specification is that I cannot control for occupation fixed-effects. It is therefore possible that the estimated positive coefficient on SVP_j results from unobserved occupation characteristics that systematically vary with SVP_j. My baseline is therefore the modified regression

$$\begin{aligned} MC_{ijt} = \alpha_1 \, SWITCHER_{ijt} + \alpha_2 \, SWITCHER_{ijt} \times SVP_j \\ + \chi_j + \phi_t + \theta' \mathbf{X_i} + \epsilon_{ijt}. \end{aligned} \quad (1.4)$$

I estimate this regression on the whole sample of displaced workers, including both occupation stayers and switchers. $SWITCHER_{ijt}$ is a dummy variable that indicates whether individual i with pre-displacement occupation j found a job in the same occupation. χ_j and ϕ_t capture occupation- and year-of-displacement fixed-effects, respectively.

The estimation follows a differences-in-differences approach. I compare the wage-loss of occupation switchers relative to stayers across occupations characterized by low and high specific vocational preparation. The estimate of interest is therefore the coefficient on the interaction $SWITCHER_{ijt} \times SVP_j$. Note that the mean effect of SVP_j is not identified because it is captured by the occupation fixed-effect χ_j.

Estimates are shown in columns (2) to (7) of Table 1.3. In column (2) I report estimates from a simplified model that does not include $SWITCHER_{ijt} \times SVP_j$ as a regressor. Switching occupations goes along with a wage-loss as the coefficient on $SWITCHER_{ijt}$ is highly significant. This finding is not new (Kambourov and Manovskii, 2009, e.g.). However, the full model in column (3) shows that this simple model masks substantial heterogeneities. The coefficient on the interaction $SWITCHER_{ijt} \times SVP_j$ is estimated to be positive and highly significant while the coefficient on the main effect $SWITCHER_{ijt}$ is not significantly different from zero anymore. This implies that switching occupations *per se* does not lead to a wage-loss. However, switching *is* costly for workers who made substantial investments in specific vocational preparation. The magnitude is economically important: the expected differential wage-loss is increasing by about 1.6 percentage points for a 10% increase of SVP_j. For example, the differential expected wage-loss upon leaving an occupation is about 10 percentage points higher for an electrician (83% percentile) compared to a waiter (22% percentile).

Column (4) reports estimates when occupation fixed-effects are not included and the mean effect of SVP_J is therefore identified. Interestingly, the estimated coefficient on SVP_j is not significantly different from zero, meaning that conditional on staying in the same occupation, the wage-loss workers suffer does *not* differ by the required specific vocational preparation of an occupation. This is reassuring evidence that SVP_j is indeed mostly capturing occupation-specific training and not firm- or match-specific human capital.

Columns (5) and (6) show results when the sample is restricted further. Column (5) reports estimates when only workers who report having been displaced due to plant closing are included. As discussed in Section 1.2.1, this sample is arguably preferable to my overall sample because in this case weak performance on the job cannot have been the reason for displacement and therefore estimates will be less subject to criticism regarding selection bias. The coefficient on the interaction gets substantially larger, implying that estimates based on my baseline sample might be subject to some selection effects.

In column (6) the sample is restricted to workers who did not receive any advance notice of displacement. Again, results are larger than in the baseline. This suggests that the benefits of on-the-job-search are the higher the more specific a worker's training is. In order to account for this effect, I will use the sub-sample of workers who were not noticed in advance of their displacement as my baseline sample when estimating regression (1.3) in Section 1.3.2.

In column (7) I restrict the sample to workers who do not switch occupations. At the same time I add a dummy that captures whether a worker stayed in the same *industry* after displacement or not. The coefficient on the interaction and the mean effect are both not significantly different from zero. This corroborates evidence from column (4): SVP_j is indeed mostly capturing human capital that is occupation- but not *industry*-specific.

1.3.2. Reduced Form Estimates of Wait Unemployment

Equation (1.3) captures the concept of wait unemployment. There is a trade-off between unemployment duration and mobility cost. Switching occupations and leaving behind occupation-specific human capital can entail high wage-losses. Facing such mobility costs workers might be willing to accept long unemployment spells in order to evade switching and secure reemployment in their old occupation instead. My objective is to empirically quantify this trade-off.

A problem hindering estimation is that the (expected) wage-loss a worker faces upon leaving his pre-displacement occupation MC_{ijt} is by definition not observed for the sub-sample of occupation *stayers* equation (1.3) is estimated on. Furthermore, any reasonable measure of mobility cost MC_{ijt} and the worker's unemployment duration $UNEM_{ijt}$ are likely to be simultaneously determined. That is, not only might mobility cost incentivize workers to sit through long spells of wait unemployment, but long unemployment spells might also weaken the bargaining position of workers and therefore lead to lower wages and lower mobility cost. This would lead to a downward bias in the estimation results. I therefore combine equations 1.2 and 1.3 into the following reduced form equation:

$$UNEM_{ijt} = \gamma_1 + \gamma_2\ SVP_j + \theta'\mathbf{X_i} + \nu_{ijt} \tag{1.5}$$

Unlike mobility cost MC_{ijt}, SVP_j is directly observable from the *Dictionary of Occupational Titles* as explained in detail in Section 1.2. Moreover, SVP_j is arguably a pre-determined variable and endogeneity should therefore be much less of a problem.

However, a second challenge for estimation remains. As before the (likely) presence of occupation fixed-effects might result in biased estimates. Occupations associated with high mobility cost might differ in other – potentially unobserved – characteristics from occupations subject to low mobility cost. For example, service occupations might require only few specific vocational preparation and workers therefore are likely to face small mobility cost upon switching occupations. Nevertheless, these workers might have above-average unemployment spells, for example, because of permanent low demand for their skills. When using cross-occupation variation for identification it is difficult to distinguish the effect of variation in mobility cost (I am interested in) from variation in other unobserved occupation characteristics that systematically vary with mobility cost.

To avoid potential omitted variable bias I therefore rely on *within*-occupation differences for identification. To do so I exploit geographic variation: the more diversified a local labor market, the less likely is it that displaced workers need to switch occupations; potential mobility cost are less likely to be binding. I estimate the following regression equation on the sub-sample of occupation stayers:

$$\begin{aligned} UNEM_{ijmt} = {} & \gamma_1\ DIVERSITY_m + \gamma_2\ DIVERSITY_m \times SVP_j \\ & + \chi_j + \phi_t + \theta'\mathbf{X_i} + \nu_{ijmt}. \end{aligned} \tag{1.6}$$

The mean effect of SVP_j is not identified because it is captured by the occupation fixed-effects χ_j. The estimation strategy follows the same logic as a standard difference-in-difference approach. However, note that both $DIVERSITY_m$ and SVP_j are continuous measures. The hypothesis is that highly specialized workers sit through long unemployment spells in order to evade switching occupations. Since in a diverse market it is more likely to be able to find a job in the same occupation, this effect should be the stronger the less diverse a local labor market is. The estimate of interest is therefore the coefficient on the interaction $DIVERSITY_m \times SVP_j$. If my hypothesis is true and wait unemployment is an important driving force of unemployment, then this estimate should be negative and significant.

Results Estimation results are shown in Table 1.5. Again, all specifications contain typical demographic controls and tenure on the previous job. Furthermore, occupation fixed-effects, occupation-specific time trends, year-of-displacement fixed-effects, and state fixed-effects are part of all specifications. Mean effects and the constant are estimated but not shown. In columns (1) to (4) I use my preferred measure of industry diversification. Results in column (1) indicate a coefficient estimate for the interaction term that is negative and statistically significant at the 1-percent level. Adding a linear state time trend in column (2) does not change results. That is, there is a significant differential effect of the required specific vocational preparation of an occupation on unemployment duration. The less diverse a local market labor market, the stronger is the effect of SVP_j on the length of the unemployment spell.

Because in this difference-in-difference setting the interpretation of the magnitude is not straightforward, I follow Rajan and Zingales (1998) and report a *differential unemployment spell* for each specification in Table 1.5. Consider two unemployed workers who have been displaced from occupations at the 25% and 75% SVP percentile, respectively. Think of the first as a waiter and of the latter as an electrician. According to the 2000 U.S. Census, the metropolitan area at the 75 percentile of diversity is *St. Louis, MO-IL* whereas *Raley-Carey, NC* is at the 25 percentile. My estimates predict that the unemployment spell of the electrician would increase by 55 percentage points more than that of the waiter if both were re-located from the diverse market labor market in *St. Louis* to *Raley-Carey, NC*.[17] Considering an average unemployment spell of about three months in my baseline sample, this effect is economically clearly important.

Column (3) adds MSA dummies to the estimation equation. These fixed-effects capture time-invariant MSA characteristics such as temperature, the level of amenities, or permanent differences in the real estate market. The resulting coefficient is less precisely estimated and only significant at the 5% level but its size is virtually unchanged. This suggests that the estimated negative coefficient on the interaction term is not driven by MSA-specific omitted factors.[18]

In column (4) I restrict the sample to workers who report having been displaced due to plant closing. As discussed in Section 1.2.1, this sample is arguably preferable to my baseline sample that also includes workers who lost their job due an abolished shift and "insufficient work." If displaced workers have systematically lower ability and workers with lower ability in turn have lower specific vocational training, then this might result in an underestimation of the effect of SVP_j on wait unemployment. Indeed, the estimated coefficient is substantially bigger than for the baseline sample. However, due to the considerably smaller sample it is imprecisely estimated and only significant at the 10% level. Nevertheless, this suggests that estimates using the baseline sample seem to reflect a lower bound on wait unemployment.

In the last four columns of Table 1.5 all specification are re-estimated using the size of a MSA's workforce as a simple alternative diversification measure. The pattern of results is similar to my preferred measure but magnitudes are overall

[17] This number can be calculated from the estimated coefficients given in Table 1.5 as $\big(exp((0.75 - 0.25)^2 \times 1.755) - 1\big) \approx 55.1\%$. Note that the dependent variable is the *log* unemployment duration.

[18] State fixed-effects do not completely drop out in this specification because some metropolitan areas span more than one state. Results are not affected when a state time trend is included.

slightly smaller. Again, adding MSA fixed-effects does not change results quantitatively but increases standard errors such that the coefficient of interest is only significant from zero at the 10% level. The differential effect of the unemployment spell calculated according to the first three specification is about 70% of the effect found for my preferred measure $DIVERSITY_m$. As before, restricting the sample to workers who have been displaced due to plant closing more than doubles the size of the coefficient relative to the baseline sample. However, due to the reduction of the sample to one-third of its original size estimates become less precise.

1.3.3. Instrumental Variable Estimates of Wait Unemployment

In the last section I showed strong evidence that workers endowed with higher specific training are willing to go through disproportionately long spell of unemployment in order to evade switching occupations. I now push the analysis further and directly estimate the effect of mobility cost on wait unemployment captured by equation (1.3). In order to do so, I make the additional identifying assumption that SVP_j affects unemployment duration only through the mobility cost a worker is facing. I can then estimate equation (1.3) by using SVP_j as an instrument for the mobility cost MC_{ijt}.[19] To control for occupation fixed-effects, I use the same difference-in-difference approach as in regression (1.6). Equation (1.3) then becomes

$$\begin{aligned} UNEM_{ijmt} = &\beta_1\, DIVERSITY_m + \beta_2\, MC_{ijmt} \\ &+ \beta_3\, DIVERSITY_m \times MC_{ijmt} \\ &+ \chi_j + \phi_t + \theta' \mathbf{X_i} + \eta_{ijmt}. \end{aligned} \tag{1.7}$$

A challenge to use two-stage least squares in my setting is that mobility cost MC_{ijmt} are only observed for the sample of occupation *switchers* while the dependent variable in equation (1.7) is the unemployment duration of *stayers*. That is, the endogenous regressor and the dependent variable are not included in the same dataset.

As first shown in an influential article by Angrist and Krueger (1992), under certain conditions estimation is still possible by using a two-sample two-stage least squares (TS2SLS) procedure.[20] The principal idea of TS2SLS is that the first and second-stage can be estimated on two separate samples as long as all control variables and the instrument are present in both samples. Moreover, both samples have to be drawn from the same population. In my setting the last assumption is not innocuous. It implies that there are no systematic difference between occupation stayers and switchers. In particular, the wage-loss actually suffered from occupation switchers has to be a good predictor of the wage-loss stayers *would have* suffered in the case of switching.

[19] Note that that SVP_j only needs to be *a source* of exogenous variation in mobility costs. It does not need to be the only source or even the main source of exogenous variation.

[20] See also Angrist and Krueger (1995), Inoue and Solon (2010), and Chapter 4.3 in Angrist and Pischke (2008).

The estimate of interest in regression (1.7) is the coefficient on the interaction $DIVERSITY_m \times MC_{ijmt}$ where the mobility cost MC_{ijmt} is likely to be endogenous. I therefore use $DIVERSITY_m \times SVP_j$ as an instrument for this interaction.[21] The first-stage regression on the sample of switchers is then given by

$$\begin{aligned} DIVERSITY_m \times MC_{ijmt} = &\ \alpha_1\ DIVERSITY_m \\ &+ \alpha_2\ DIVERSITY_m \times SVP_j \\ &+ \chi_j + \phi_t + \theta'\mathbf{X_i} + \kappa_{ijmt}. \end{aligned} \tag{1.8}$$

In order for $DIVERSITY_m \times SVP_j$ to be a valid instrument it needs to be relevant, exogenous, and fulfill the exclusion restriction. An instrument is relevant if it has sufficient explanatory power for the explanatory variable, that is, if $corr(DIVERSITY_m \times SVP_j, DIVERSITY_m \times MC_{ijmt})$ is not only marginally different from zero. If this is not the case IV estimates are unlikely to be informative. This condition is testable and – as shown below – indeed holds in my data for most specifications. The instrument is also arguably exogenous because SVP_j is a pre-determined variable.

Exclusion Restriction The exclusion restriction holds if, conditional on the control variables, $DIVERSITY_m \times SVP_j$ is uncorrelated with any other determinants of unemployment duration. The instrument $DIVERSITY_m \times SVP_j$ must affect the unemployment duration of a worker only through the interaction $DIVERSITY_m \times MC_{ijmt}$. A potential challenge for the exclusion restriction is that occupations with higher SVP might have systematically lower unemployment duration even when mobility is not binding (when $DIVERSITY_m$ is "high"). As explained in Appendix 1.8.1, one scenario where this might be the case is when workers invest in specific vocational preparation not with the objective to earn a higher wage but to be able to find a job faster.

As always when using an IV approach, unlike instrument relevance the exclusion restriction is untestable because the error term ν_{ijt} in equation (1.7) is unobserved. However, empirical evidence strongly suggests that workers primarily invest in occupation-specific training to raise their wages, not to be able to find a job faster. There is a strong positive correlation between wages and the extent of specific vocational training a worker invested in. On the other hand, there is no evidence in the data that would suggest that workers with higher SVP have shorter unemployment spells. In particular, note that the fact that more educated workers tend to have lower unemployment rates is entirely driven by these workers being less often fired, *not* by having short unemployment spells following displacement (e.g., Cajner and Cairo, 2011).

Results Table 1.6 presents two-sample two stage least squares estimates of the effect of mobility cost on unemployment duration. The associated first stage esti-

[21] See Ozer-Balli and Sorensen (2010) for a discussion on how to use instrumental variables in linear regressions that include interaction effects.

mates on the sample of occupation switchers are shown in Table 1.9. Columns (1) to (3) show results when my preferred measure of industry diversification is used. As before, all specification include the usual demographic controls, occupation-specific time trends, and state fixed-effects. In my baseline specification in column (2) I allow for state time trends. In the first two specifications the coefficient on the interaction is negative and significant at the 1% level.[22] As reported in Table 1.9, the first-stage is relatively strong with an F-statistic on the instrument of about 12, well above the threshold of 10 recommended by Staiger and Stock (1997). This indicates that weak instruments should not be an important concern. As for the reduced form estimates, I compute a *differential unemployment spell* to make it easier to put magnitudes into perspective. As before, the metropolitan area at the 75 percentile of diversity is *St. Louis, MO-IL* whereas *Raley-Carey, NC* is at the 25 percentile. The differential unemployment spell reports the differential increase in the unemployment spell of a worker facing a 10% mobility cost compared to a worker who does not face a mobility cost when moving from the diverse *St. Louis* labor market to the less diverse *Raley-Carey, NC* market. For specifications (1) and (2) I find that this number is about 40%.

In column (3) I include MSA fixed-effects. I find that the coefficient barely changes in magnitude but quite some power is lost in the first stage with the F-statistic on the instrument falling to about 9.6. The resulting coefficient is therefore imprecisely estimated and not significantly different from zero. I do not show results when the sample to workers displaced due to plant-closing only. The reason is that due to a substantial reduction in sample size the first stage regression on this sample is weak, as can be seen in column (4) in Table 1.9.

In columns (4) to (6) I use the size of a MSA's workforce as an alternative measure of local labor market diversification. The estimated interaction term is smaller in absolute value then when using my baseline measure and only significant at the 10% level. The differential unemployment spell is about 40% percent of my baseline measure. The effect in column (3) when MSA dummies are included is again not significant although here the first-stage estimates show a relatively strong relation. Again, I omit a specification restricted to workers displaced by plant closing due to a weak first stage.

1.4. Aggregate Implications

In this section I explore the effect of wait unemployment on the aggregate unemployment rate in the U.S. labor market. One should think of the U.S. labor market as an agglomeration of many small submarkets or *islands* for specific human capital instead of a single market where one type of homogeneous labor is exchanged. In the presence of reallocation shocks, a situation can arise where

[22] Standard errors are corrected for the fact that in the second-stage regression (1.7) the interaction $DIVERSITY_m \times MC_{ijmt}$ is estimated rather than known. I use the adjustment proposed by Inoue and Solon (2010) for a two-sample two-stage least squares (T2SLS) setting.

firms are urgently looking to hire workers on one island while there is an excess of unemployed and a lack of vacancies on another. When moving across islands is costless, this situation is not sustainable as workers will move out of occupations that are facing slack demand. This reshuffling will continue until a point is reached where no dispersion across markets is left.

The evidence I present in this paper, however, shows that workers actually *do* face substantial mobility cost when moving from one submarket to another. One reason is that they invested in specific human capital that they do not want to leave behind. The extent of worker mobility that will take place is therefore limited, resulting in a sustained dispersion of labor market conditions across submarkets. This dispersion in turn implies increased aggregate unemployment because the job finding probability is concave in the labor market *tightness*, the ratio of vacancies to unemployed. Worker mobility costs can therefore give raise to aggregate unemployment as they reduce arbitrage possibilities of workers. Schematically, the mechanism can be summarized in the following way:

$$\begin{aligned} \uparrow \text{human capital specificity} &\implies \uparrow \text{workers face mobility cost} \\ &\implies \downarrow \text{worker mobility} \\ &\implies \uparrow \text{dispersion in tightness} \\ &\implies \uparrow \text{aggregate unemployment} \end{aligned}$$

In this section I quantify the effect of wait unemployment on aggregate unemployment by asking: how much smaller would the aggregate unemployment rate be if all human capital would be perfectly transferable across occupations and workers would therefore not face any mobility cost? Based on my regression estimates, the dispersion of labor market conditions across submarkets as a consequence of mobility cost can be expressed as

$$\widehat{UNEM}_{ijmt} = UNEM_{ijmt} - UNEM^{cf}_{ijmt} = \widehat{\gamma}_2 \; DIVERSITY_m \times SVP_j.$$

Here $UNEM^{cf}_{ijmt}$ is the (log) unemployment duration under the counterfactual scenario of completely transferable human capital. As shown in Chapter 2 of this thesis, the counterfactual aggregate job finding probability P^{cf}_{mt} that would prevail in local labor market m can then be expressed as[23]

$$P^{cf}_{mt} = P_{mt} \left\{ \mathbb{E} \left[\left(1 + \widehat{UNEM}_{ijmt} \right)^{\mu} \right]^{\frac{1}{1-\mu}} \right\}$$

where P_{mt} is the observed job finding probability in local labor market m and μ is the concavity of the matching function. By assuming that unemployment is in

[23] I make the standard assumption that the unemployment duration follows a geometric distribution with the success probability given by the job finding probability P_{ijmt}. The expected length of an unemployment spell is therefore given by $UNEM_{ijmt} = \frac{1}{P_{ijmt}}$. Taking logs, this implies $log(UNEM_{ijmt}) = -log(P_{ijmt})$ and $\widehat{UNEM}_{ijmt} = -\widehat{P}_{ijmt}$.

Table 1.2: Counterfactual Unemployment, Averages (1985-2011)

Concavity	Unemployment	Unemployment (cf)	Ratio
$\mu = 0.5$	6.1%	6.0%	97.9%
$\mu = 0.6$	6.1%	5.9%	96.1%
$\mu = 0.7$	6.1%	5.6%	92.1%
$\mu = 0.8$	6.1%	4.9%	79.7%

Notes: The first column reports the average unemployment rate in the U.S. labor market between 1985 and 2011. The second column reports the average counterfactual unemployment rate for the same period. The counterfactual depends on the assumed concavity of the matching function μ. The last column shows the average ratio of the latter and the former.

steady state, the counterfactual unemployment rate is then given by $u_{mt}^{cf} = \frac{\lambda_t}{\lambda_t + P_{mt}^{cf}}$ where λ_t stands for the probability that a workers loses his job. Because of its much larger sample size, I use data from the basic Current Population Survey to construct the counterfactual unemployment rate.

Results for the United States are reported in Table 1.2 and in Figure 1.3. It is apparent that wait unemployment is an important driving force of aggregate unemployment. However, the estimates are sensitive to the assumed concavity of the matching function μ. When $\mu = 0.6$ as in Nagypal and Mortensen (2007), only about 4% of total unemployment between 1985 and 2011 can be attributed to wait unemployment. A higher (but still realistic) concavity of $\mu = 0.8$ increases this share substantially to about 20%.

1.4.1. Development over Time

It is well documented that since the 1980s there has been a rapid increase in the share of employment in occupations that require high education. While in the 10 years between 1980 and 1990 also middle-skill jobs were on the rise, the growth pattern changed in the 1990s. According to Autor et al. (2006), amongst others, since then a "polarization" of the labor market took place. The tendency towards more employment in high-skilled occupations continued but there was a parallel rise in the employment in service occupations located in the lower end of the skill distribution. Employment in middle-skill jobs declined.[24]

As can be seen in Figure 1.4, these changes in the occupational structure are also reflected in the extent to which the labor force is endowed with specific-skills. Since the 1990s, the share endowed with highly specific training is increasing, only suffering a surprisingly strong setback with the arrival of the Great Recession in

[24] In Chapter 3 of this thesis I critically reevaluate the evidence of job polarization in the U.S. labor market.

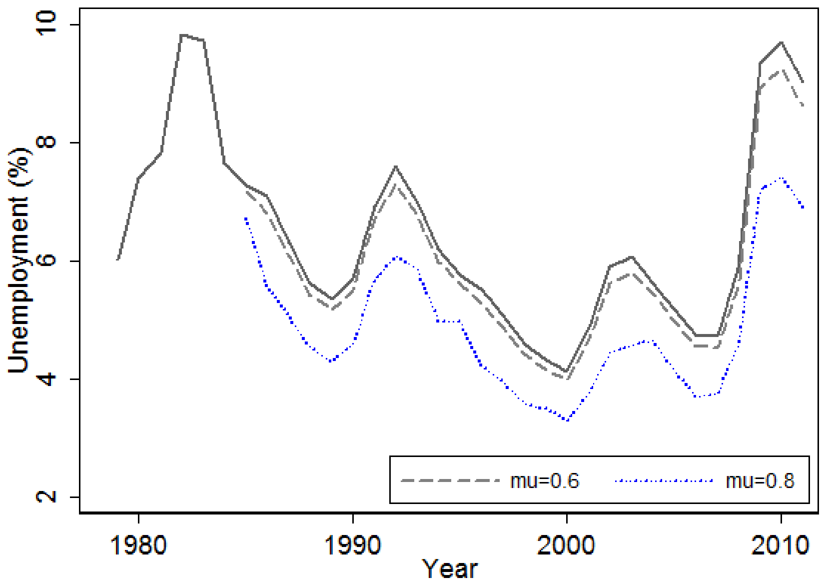

Figure 1.3: The actual and counterfactual unemployment rates are shown. In consider two scenarios. In the first scenario (dashed line) I assume the concavity of the matching function to be $\mu = 0.6$ (Nagypal and Mortensen, 2007). In the second scenario (dotted line) I assume a high concavity of $\mu = 0.8$. The results are quite sensitive to the choice of parameter μ. Note also that the difference between the actual and counterfactual unemployment rate is quite stable over time. This is quite remarkable since the U.S. labor market underwent substantial changes in the last 30 years.

2008. This steady increase came at the cost of the the middling occupations that require between 3 and 24 months of specific training.[25] The lack of variation of the importance of wait unemployment for the aggregate unemployment rate over time documented in Figure 1.3 is therefore somewhat surprising.

The counterfactuals in this section should be seen as "back-of-the-envelope" calculations since I make several simplifying assumptions. Firstly, I assume that the mobility cost only have an influence on the unemployment duration of occupation stayers, not of occupation switchers. As I show at the end of Section 1.5.1, this most likely leads to an underestimation of the effect. Moreover, because non-metropolitan areas are not identified in the CPS-DWS, I do not use information on workers that live in those areas. This again biases my estimates downwards since rural areas are likely to have lowly diversified labor markets where switching occupations might be necessary and mobility costs are therefore important. A third

[25]Note that Figure 1.3 shows data for the U.S. labor force, not the sub-sample of unemployed workers. For unemployed workers, the *changes* (not the levels) of the shares since the 1980s are comparable.

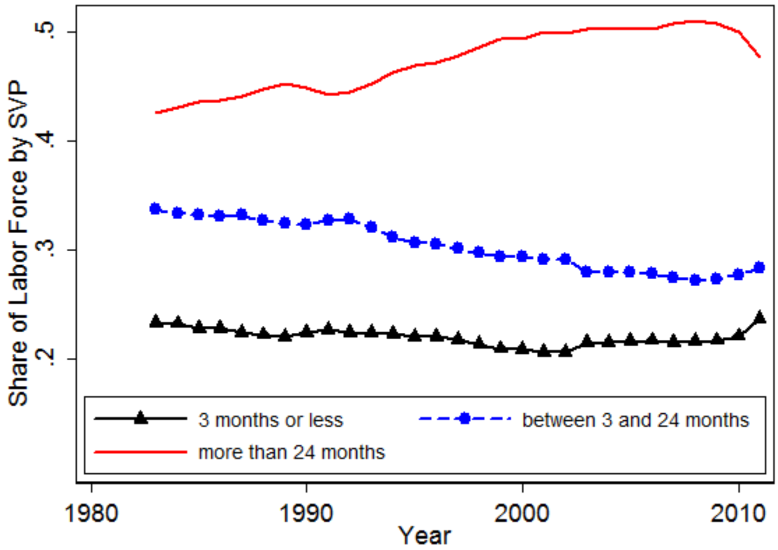

Figure 1.4: The evolution of the labor force shares by specific vocational preparation (SVP) are shown. When a worker is employed (or is unemployed but was last employed) in a given occupation, I assume that he is endowed with the specific skills required by that occupation according to the Dictionary of Occupational Titles as described in Section 1.2.2. The solid line shows the share of the labor force with occupation-specific training of more than 24 months. The lines with triangles and circles show the shares with 3 months or less and with 3 to 24 months of occupation-specific training.

issue is that the basic CPS data that I use for this exercise provides less information then the CPS-DWS about the conditions under which the worker lost his job. For example, there is neither information on the reason of displacement nor whether the workers was warned in advance of his displacement.

1.5. Robustness and Further Tests

1.5.1. Robustness

My interpretation of the effect of SVP on unemployment duration is that higher specialized workers in less diverse local labor markets face relatively higher mobility cost. In order to evade these mobility cost they are willing to sit through long spells of wait unemployment. A possible criticism of this interpretation is that the interaction $DIVERSITY_m \times SVP_j$ is just a proxy for another, unobserved channel. For example, it might be that workers self-select into more or less diverse labor markets according to some characteristic that affects unemployment duration and that is at the same time not captured by the demographic controls. If the extent of this self-selection in addition varies with SVP, then my regressions might pick up these effects. Alternatively, workers with highly specific training might benefit disproportionally from more diverse labor markets due to reasons not linked to wait unemployment. In a diverse labor market there might be more opportunities for workers trained in highly specific tasks, leading to relatively shorter unemployment spells. In this section I therefore provide further tests to address these issues.

Other Samples Firstly, if my interpretation of the estimates is correct then the effect should be weaker for workers who are noticed several months in advance of losing their job. These workers have the opportunity to undertake on-the-job-search and are therefore less likely to enter the pool of unemployed at all. Panel A of Table 1.7 shows estimates of regression (1.6) for the sub-sample of workers who report to have received an advanced notice of displacement. Indeed estimates change substantially compared to Table 1.5. In all specifications the coefficient of interest is now insignificant. Moreover, in all but one specification the coefficient also switched sign. These result are consistent with SVP affecting unemployment duration through mobility cost.

Secondly, if workers sit through long unemployment spells in order to stay in the occupation they have been trained for, then the effect should be absent when the sample of occupation *switchers* is used for estimation instead. Panel B of Table 1.7 shows estimates of regression (1.6) for workers who report to have changed their occupation after displacement. Again, my prediction is confirmed as estimates for all specifications are substantially smaller than in Table 1.6 and not significantly different from zero.

Triple Differences I can push the empirics one step further by extending the estimation sample to include occupation stayers and switchers and adding a third interaction to my basic difference-in-difference regression (1.6):

$$\begin{aligned} UNEM_{ijmt} = {} & \gamma_1 \, SWITCHER_{ijmt} + \gamma_2 \, DIVERSITY_m \times SVP_j \\ & + \gamma_3 \, DIVERSITY_m \times SWITCHER_{ijmt} \\ & + \gamma_4 \, SWITCHER_{ijmt} \times SVP_j \\ & + \gamma_5 \, DIVERSITY_m \times SVP_j \times SWITCHER_{ijmt} \\ & + \chi_j + \phi_t + \theta' \mathbf{X_i} + \nu_{ijmt} \end{aligned} \tag{1.9}$$

$SWITCHER_{ijmt}$ is a dummy variable that indicates whether individual i with pre-displacement occupation j found a job in the same occupation. The coefficient on the interaction $DIVERSITY_m \times SVP_j$ captures the effect of mobility cost on unemployment duration whereas the coefficient on $DIVERSITY_m \times SVP_j \times SWITCHER_{ijmt}$ is the differential effect on workers who report to have left the occupation they were trained for.

The results of this difference-in-difference-in-difference estimation are reported in Table 1.8. It is indeed the case that the effect of mobility cost is much stronger for occupation stayers than for switchers, although the coefficient on $DIVERSITY_m \times$

$SVP_j \times SWITCHER_{ijmt}$ is not significant in most specifications due to low precision of the estimates. For example, in my preferred specification in column (2) the effect on occupation stayers is about two-third of the magnitude in the baseline (-1.118 vs. -1.755) whereas the effect on occupation switchers is close to zero (-1.118+0.801).

One might find it surprising that the effect for occupation switchers is not even smaller. However, note that it is actually reasonable to assume that also occupation switchers are at least somewhat affected by wait unemployment. In the model workers make the decision to leave their pre-displacement occupation based on the trade-off between the wage loss (mobility cost) when leaving and the longer unemployment duration when staying. Therefore, when the model is taken literally, workers who switch should not be affected by wait unemployment at all. In reality, however, workers might first try to find work in their original occupation and only after learning that few vacancies are around decide to switch. Therefore, the difference in unemployment duration between stayers and switchers in the data is probably less clear cut than predicted by the simple model that abstracts from frictions like imperfect information.[26]

1.5.2. Are my Results Realistic?

The IV estimates in Table 1.6 suggest a strong effect of mobility cost on wait unemployment. Here I present a simple net-present value calculation to explore whether these estimates are realistic. Consider an unemployed worker who is looking for a job. The value of search can be described by the following Bellman equation:

$$U_t(P_t, W_t) = \frac{1}{1+r} \left\{ B + P_t \mathbb{E}_t E_{t+1} + (1 - P_t) \, \mathbb{E}_t U_{t+1} \right\} \tag{1.10}$$

When unemployed, the worker collects unemployment benefits B. With probability $1 - P_t$ he stays unemployed in $t + 1$. With probability P_t he finds a job and obtains E_{t+1}. E_t is captured by the following Bellman equation:

$$E_t(P_t, W_t) = \frac{1}{1+r} \left\{ W_t + (1 - \lambda) \mathbb{E}_t E_{t+1} + \lambda \mathbb{E}_t U_{t+1} \right\}$$

When employed, a worker earns a wage W_t. With probability λ he loses his job and finds himself again unemployed in $t + 1$. Under the assumption that $\mathbb{E}_t P_{t+1} = P_{it}$ and $\mathbb{E}_t W_{t+1} = W_t$, I can solve equation (1.10) forward and obtain

$$U_t(P_t, W_t) = W_t \frac{P_t}{r(r + \lambda + P_t)} + B \frac{r + \lambda}{r(r + \lambda + P_t)}.$$

[26]One can also interpret this as a measurement problem. In the data workers only report in what occupation they *eventually* found a job and how many weeks it took them to find this job. It is not clear how the time spend searching was distributed among finding a job in their pre-displacement occupation vs. finding a job in another occupation.

I can then log-linearize this expression around the average finding rate and wage of occupational switchers $(\overline{P}, \overline{W})$. This results in an equation that formalizes the trade-off between the job finding probability and the wage, the counterpart of regression equation (1.3) in Section 1.3

$$p_t - \overline{p} = -\Gamma(\lambda, r, B)\,(w_t - \overline{w})$$

where lower case letters stand for variables in logs. The coefficient $\Gamma(\lambda, r, B)$ is a function of the separation probability λ, the interest rate r, and the unemployment benefit B that I do not show here. The bigger the coefficient Γ, the longer the unemployment spells workers are willing to sit through in order to evade a given wage-loss.

This simple model allows some interesting insights into what determines wait unemployment. Firstly, the higher future payoffs are discounted, the less willing are workers to stay wait unemployed, that is, $\frac{\partial\Gamma(\lambda,r,B)}{\partial r} < 0$. Secondly, it holds $\frac{\partial\Gamma(\lambda,r,B)}{\partial B} > 0$: higher unemployment benefits B make the state of being unemployed less "painful." Thirdly, the benefits of having a job with a higher wage are the greater the lower the probability to lose that job again, $\frac{\partial\Gamma(\lambda,r,B)}{\partial\lambda} < 0$.[27] For example, when the separation probability λ is at extremely low levels, a worker compares (almost) infinite streams of wage payments. Even a small evaded wage-loss then makes a big difference and the worker prefers to wait.

Parameterization By calibrating the parameters to reasonable values, I obtain values than can be seen as theoretical counterparts of the regression estimates I report in Table 1.6. I assume that the average monthly wage is $\overline{W} = 1600$, the monthly job finding rate is $\overline{P} = 0.25$, the monthly probability to lose a job is $\lambda = 2.5\%$. The monthly discount rate r is 0.0041, the equivalent of an annual rate of 5%.

Whereas it is relatively straightforward to calibrate these parameters it is more challenging to set the unemployment benefits B to a reasonable value. Calibrations of $b = \frac{B}{W}$ – the ratio of unemployment benefits to the average wage – vary considerably in the literature. For example, in classic articles a typical value for the replacement ratio is $b = 0.4$ (e.g., Shimer, 2005). However, Nagypal and Mortensen (2007) note that not only the monetary value of unemployment benefits but also the utility of leisure and the value of non-market activity should be included and therefore a high value such as $b = 0.9$ is more realistic.

Figure 1.5 plots the size of coefficient $\Gamma(\lambda, r, B)$ against $b = \frac{B}{W}$. As noted before, the coefficient is increasing in b: the higher the assumed unemployment benefits, the lower the disutility from being unemployed, and therefore the longer the unemployment spell a worker is willing to sit through in order to evade a wage-loss. For example, assuming $b = 0.4$, a worker is indifferent between suffering a

[27] Summers et al. (1986) mentions this mechanism: "Investing in waiting for a high-wage job makes much more sense for mature married men, who as a group have a very low employment turnover rate, than for other demographic groups that have much higher turnover rates."

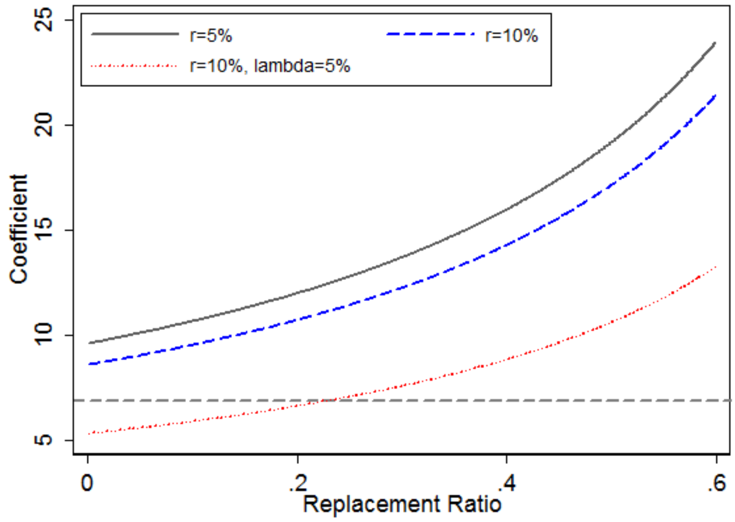

Figure 1.5: The graph plots the coefficient $\Gamma(\lambda, r, B)$ against the replacement ratio $b = B/\overline{W}$. The dashed blue line assumes a annualized discount rate of 10% instead of 5%. The red dotted line additionally assumes the separation rate λ to be 5% instead of 2.5%. The remaining parameters are kept constant: the average monthly wage is $\overline{W} = 1600$, the monthly job finding rate is $\overline{P} = 0.25$. The dashed gray line shows the empirical counterpart of coefficient Γ based on the estimates presented in my baseline specification (2) in Table 1.6.

1% wage-loss and extending his unemployment spell by 16% while not suffering a wage-loss.

It is more intuitive to express this in months of unemployment. Given my calibration, an occupational switcher is on average unemployed for $\frac{1}{0.25} = 4$ months, assuming that unemployment duration follows a geometric distribution. In order to evade a wage loss of 5%, a worker would be willing to be unemployed for about 8.8 months instead of 4 months.[28] That is, under a reasonable parameterization, even small mobility costs have substantial effects on unemployment duration.

To be able to compare these coefficients with the estimates reported in Table 1.6, I make the assumption that mobility cost are irrelevant in the metropolitan area with maximum $DIVERSITY_m$. This assumption is necessary since estimates are based on a difference-in-difference regression. The coefficient implied by my baseline specification (2) in Table 1.6 is represented by the gray dashed line in Figure 1.5. It is apparent that there is a discrepancy between the result of the net-present value calculation and my regression estimates. This is not even resolved when I – as represented by the dashed blue line – assume a high yearly discount rate $r = 10\%$. Only when I at the same time assume a very high separation rate $\lambda =$

[28]Note that variables are in logs. Therefore $exp(16 * 0.05) - 1 = 122.5\%$.

5%, I find that the results of the two exercises are compatible for a low replacement ratio $b = \frac{B}{W}$. My findings are not compatible with a very "generous" calibration of unemployment benefits along the lines of Nagypal and Mortensen (2007).

There are several reasons why my estimates might be smaller than the ones predicted by the model. One explanation, for example, is that workers try to evade extended unemployment spells because it is attached with a stigma and might send a negative signal to potential future employers. This leads to a de-facto decreased replacement ratio b. In any case, it seems that the aggregate effect of wait unemployment shown in Section 1.4 – although already substantial – is a rather conservative estimate.

1.6. Conclusions

In this paper, I showed empirical evidence that wait unemployment is an important source of aggregate unemployment in the United States. Labor market skills are not perfectly transferable across jobs. In order not to experience a wage-loss, a displaced worker therefore has a strong incentive to wait and find a job that is as similar to his old job as possible. I empirically assessed this trade-off between waiting and suffering a wage-loss by using a difference-in-difference approach in the spirit of Rajan and Zingales (1998). I used two different sources of variation. Firstly, I exploited that the specificity of the human capital a worker invested in varies by occupation. The more specific a workers human capital, the higher the potential wage-loss he is facing when switching to a very different job. Secondly, I exploited variation across local labor markets. In a "diverse" labor market where employment is spread out over many different sectors and industries, it will be relatively easy to find a job that matches a worker's skill-set – even when he is highly specialized; the mobility cost to switching to a very different job is therefore less likely to be binding.

I constructed the following test. Using the CPS Displaced Worker Supplement, I looked at the sample of displaced workers who managed to find a job in the same occupation they worked in before. I then compared the unemployment spells of more and less specialized workers in local labor markets with high and low diversity. I found that in labor market with low diversity the more specialized workers were unemployed for much longer time relatively to the less specialized workers. There was no such difference observable in the labor markets with high diversity. I therefore concluded that wait unemployment is an important source of unemployment. Using a simple "back-of-the-envelope" calculation, I find that about 5 to 20% of total unemployment in the United States is due to wait unemployment.

1.7. Tables

Table 1.3: Mobility Cost and SVP

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
SVP$_j$	0.147***			0.0137			
	(0.0276)			(0.0252)			
SWITCHER$_{ijt}$		0.0784***	-0.0223	0.00710	-0.0538*	-0.0413	
		(0.0114)	(0.0260)	(0.0227)	(0.0301)	(0.0264)	
SVP$_j$ $\times$ SWITCHER$_{ijt}$			0.157***	0.128***	0.195***	0.183***	
			(0.0328)	(0.0291)	(0.0439)	(0.0351)	
IND SWITCHER$_{ijt}$							0.0424
							(0.0480)
SVP$_j$ $\times$ IND SWITCHER$_{ijt}$							-0.0100
							(0.0602)
Observations	7,002	11,040	11,040	11,040	4,332	7,652	3,703
R-squared	0.063	0.099	0.102	0.059	0.138	0.117	0.138
Occupation fixed effects	no	yes	yes	no	yes	yes	yes
Sample							
Occupation switchers only	yes	no	no	no	no	no	no
Occupation stayers only	no	no	no	no	no	no	yes
Plant closing only	no	no	no	no	yes	no	no
No advance notice only	no	no	no	no	no	yes	no

Notes: Column (1) reports estimates of regression (1.2) whereas columns (2)-(7) report estimates of variations of regression equation (1.4). The method of estimation is least squares. The dependent variable is the wage-loss defined as the log-difference between deflated weekly earnings on the pre-displacement jobs and the current job. All regressions include year-of-displacement dummies, four education dummies (dropout, high-school, some college, college or more), a female dummy, a non-black dummy, age (cubic), and tenure on the pre-displacement job (cubic). Only the sub-sample of displaced workers who report that the current job was the first job after displacement is used for estimation, see Section 1.2. As noted at the bottom of the table, the sample is further restricted in columns (1) and columns (5)-(7). Standard errors clustered at the occupation level are reported in parenthesis. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table 1.4: CPS Displaced Worker Supplement, Descriptive Statistics

	Whole sample		Switcher		Stayer		Baseline	
	mean	sd	mean	sd	mean	sd	mean	sd
Previous weekly earnings ($)	703.3	516.0	662.4	496.9	782.2	542.4	813.2	563.7
Current weekly earnings ($)	664.6	493.4	606.9	459.4	775.8	535.7	805.7	556.5
Wage-loss	0.0993	0.457	0.126	0.482	0.0483	0.397	0.0399	0.394
Previous tenure (years)	59.76	77.97	59.86	80.26	59.59	73.39	53.17	65.67
Unemployment spell (weeks)	12.76	18.54	13.78	19.38	10.80	16.61	10.91	16.70
Spec. vocational preparation (ecdf)	0.602	0.310	0.583	0.313	0.640	0.299	0.648	0.298
Female	0.356	0.479	0.375	0.484	0.320	0.467	0.304	0.460
Age (years)	38.84	10.70	38.34	10.82	39.80	10.39	40.16	10.76
Not black	0.923	0.267	0.916	0.278	0.936	0.245	0.919	0.272
Education								
High-school dropout	0.0885	0.284	0.0838	0.277	0.0975	0.297	0.104	0.305
High-school graduate	0.344	0.475	0.357	0.479	0.318	0.466	0.355	0.479
Some college	0.312	0.463	0.321	0.467	0.294	0.456	0.299	0.458
$\geq$ College	0.256	0.437	0.238	0.426	0.291	0.454	0.243	0.429
Reason for displacement								
Plant closing	0.403	0.491	0.396	0.489	0.417	0.493	0.315	0.465
Insufficient work	0.347	0.476	0.341	0.474	0.358	0.479	0.474	0.500
Shift abolished	0.250	0.433	0.263	0.440	0.225	0.418	0.211	0.408
Occupation switcher	0.658	0.474	1	0	0	0	0	0
Moved after displacement	0.166	0.372	0.169	0.375	0.161	0.368	0	0
Current job first since displacement	0.685	0.465	0.667	0.471	0.720	0.449	1	0
Noticed of displacement in advance	0.290	0.454	0.288	0.453	0.293	0.455	0	0
Observations	15566		10245		5321		999	

Notes: Descriptive statistics of the CPS Displaced Worker Supplement are shown. Reported numbers have to be interpreted as shares unless mentioned otherwise. Wage-loss is defined as the difference in log real weekly earnings (2005 $). *Whole sample* refers to the CPS-DWS when both occupation stayers and switchers are included in the sample. As described in detail in Section 1.2, the sample is restricted to workers between 20 and 67 years of age who lost a job in the private sector due to plant closing, insufficient work, or because their shift was abolished between 1983 and 2012. Moreover, they lost a full-time job and are currently full-time re-employed. In the columns *Switcher* (*Stayer*) the sample is further restricted to workers who report that their current job is in a different (same) occupation than their pre-displacement occupation. *Baseline* refers to the sample that I use in the estimation of my preferred specification (2) in Tables 1.5 and 1.6. In this case the sample is again restricted to occupation stayers. Further, it is restricted to (1) workers who were not noticed in advance of their displacement, (2) who report that the current job is the first job after displacement, (3) who report not to have moved after displacement, and (4) who live in a metropolitan statistical area (MSA) that is identified in the CPS-DWS.

Table 1.5: Reduced Form Estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DIVERSITY$_m$ $\times$ SVP$_j$	-1.696***	-1.755***	-1.614**	-4.073*				
	(0.558)	(0.616)	(0.804)	(2.120)				
SIZE$_m$ $\times$ SVP$_j$					-1.360**	-1.307**	-1.361*	-3.227
					(0.647)	(0.585)	(0.809)	(2.285)
Observations	999	999	999	308	999	999	999	308
R-squared	0.506	0.536	0.668	0.802	0.502	0.531	0.668	0.797
Diff unem spell	0.528	0.551	0.497	1.769	0.405	0.386	0.405	1.240
Occupation time trend	yes	yes	yes	yes	yes	yes	yes	yes
State fixed effect	yes	yes	yes	yes	yes	yes	yes	yes
State time trend	no	yes	no	no	no	yes	no	no
MSA fixed effect	no	no	yes	no	no	no	yes	no
Sample								
Plant closing only	no	no	no	yes	no	no	no	yes

Notes: The regressions are least squares estimates of equation (1.6). The dependent variable is the log unemployment spell of displaced workers. In columns (1) to (4), I measure the diversity of a local labor market using my preferred measure $DIVERSITY_m$ as described in Section 1.2.3. In the last four columns I use the size of workforce in a given local labor market as a simple alternative measure. The *differential unemployment spell* measures the relative increase in the unemployment duration of a displaced worker with high SVP relative to a worker with low SVP (75th vs. 25th percentile) when located in a less rather in a more diverse local labor market (25th vs. 75th percentile). Only the sub-sample of displaced workers who report not to have changed occupations after displacement, whose current job was the first job after displacement, and who were not noticed in advance of their displacement is used for estimation, see Section 1.2. In columns (4) and (8) the sample is further restricted to workers who report to have been displaced due to a plant closing. Standard errors clustered at the occupation level are reported in parenthesis. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table 1.6: Instrumental Variable Estimates

	(1)	(2)	(3)	(4)	(5)	(6)
DIVERSITY$_m \times$ MC$_{ijmt}$	-6.763***	-6.876***	-6.415			
	(3.032)	(3.322)	(4.213)			
SIZE$_m \times$ MC$_{ijmt}$				-5.014*	-4.758*	-4.898
				(2.824)	(2.476)	(3.366)
Observations	999	999	999	999	999	999
R-squared	0.506	0.536	0.668	0.502	0.531	0.668
Diff unem spell	0.402	0.410	0.378	0.285	0.269	0.277
Occupation time trend	yes	yes	yes	yes	yes	yes
State fixed effect	yes	yes	yes	yes	yes	yes
State time trend	no	yes	no	no	yes	no
MSA fixed effect	no	no	yes	no	no	yes
Sample						
Plant closing only	no	no	no	no	no	no

Notes: The regressions are two-sample two-stage least squares (TS2SLS) estimates of equation (1.7). The dependent variable is the log unemployment spell of displaced workers. The *differential unemployment spell* measures the differential impact of a 10% mobility cost on the unemployment duration of a worker located in an MSA at the 25th percentile of diversification rather than in one at the 75th percentile. The associated first-stage estimates are shown in Table 1.9. Only the sub-sample of displaced workers who report not to have changed occupations after displacement, whose current job was the first job after displacement, and who were not noticed in advance of their displacement is used for estimation, see Section 1.2. Standard errors clustered at the occupation level and corrected as proposed by Inoue and Solon (2010) are reported in parenthesis. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table 1.7: Reduced Form Estimates: Other Samples

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel A: Advance Notice of Displacement				
DIVERSITY$_m$ $\times$ SVP$_j$	0.767	0.552	0.236	1.272				
	(1.033)	(1.161)	(2.569)	(2.278)				
SIZE$_m$ $\times$ SVP$_j$					1.357	1.294	2.400	-1.807
					(1.311)	(1.473)	(2.449)	(3.001)
Observations	564	564	564	297	564	564	564	297
				Panel B: Occupation Switchers				
DIVERSITY$_m$ $\times$ SVP$_j$	-0.227	-0.263	-0.0761	1.173				
	(0.421)	(0.427)	(0.546)	(1.088)				
SIZE$_m$ $\times$ SVP$_j$					-0.503	-0.506	-0.663	0.763
					(0.467)	(0.474)	(0.552)	(1.228)
Observations	1,763	1,763	1,763	488	1,763	1,763	1,763	488
Occupation time trend	yes	yes	yes	yes	yes	yes	yes	yes
State fixed effect	yes	yes	yes	yes	yes	yes	yes	yes
State time trend	no	yes	no	no	no	yes	no	no
MSA fixed effect	no	no	yes	no	no	no	yes	no
Sample								
Plant closing only	no	no	no	yes	no	no	no	yes

Notes: The regressions are least squares estimates of equation (1.6). In both cases the dependent variable is the length of the unemployment spell $UNEM_{ijmt}$, measured as the natural logarithm of 1 plus the weeks of unemployment: $log(1 + weeks_{ijt})$. Recall that estimates in Table 1.5 are based on occupation stayers who did not receive an advance notice of displacement. Panel A reports estimates for occupation stayers who *did* receive an advance notice of displacement. Panel B instead reports estimates for the sample of occupation *switchers*. In columns (1) to (4), I measure the diversity of a local labor market using my preferred measure $DIVERSITY_m$. In the last four columns I use the size of the workforce in a given local labor market as a simple alternative measure. All regressions include year-of-displacement dummies, four education dummies (dropout, high-school, some college, college or more), a female dummy, a non-black dummy, age (cubic), and tenure on the pre-displacement job (cubic). Only the sub-sample of displaced workers who report that the current job was the first job after displacement is used for estimation, see Section 1.2. In columns (4) and (8) the sample is further restricted to workers who report to have been displaced due to a plant closing. Standard errors clustered at the occupation level are reported in parenthesis. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table 1.8: Reduced Form Estimates: Triple Differences

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DIVERSITY$_m$ $\times$ SVP$_j$	-1.102**	-1.118**	-0.987**	-2.477*				
	(0.457)	(0.502)	(0.469)	(1.397)				
SWITCHER$_{ijmt}$ $\times$ DIVERSITY$_m$ $\times$ SVP$_j$	0.862	0.801	0.899	2.757*				
	(0.625)	(0.676)	(0.591)	(1.478)				
SIZE$_m$ $\times$ SVP$_j$					-0.918	-0.959*	-1.038*	-0.206
					(0.564)	(0.553)	(0.592)	(1.167)
SWITCHER$_{ijmt}$ $\times$ SIZE$_m$ $\times$ SVP$_j$					0.352	0.370	0.380	-0.368
					(0.809)	(0.795)	(0.709)	(1.758)
Observations	2,762	2,762	2,762	796	2,762	2,762	2,762	796
R-squared	0.307	0.323	0.419	0.536	0.307	0.323	0.419	0.531
Occupation time trend	yes	yes	yes	yes	yes	yes	yes	yes
State fixed effect	yes	yes	yes	yes	yes	yes	yes	yes
State time trend	no	yes	no	no	no	yes	no	no
MSA fixed effect	no	no	yes	no	no	no	yes	no
Sample								
Plant closing only	no	no	no	yes	no	no	no	yes

Notes: The regressions are least squares estimates of equation (1.9). The dependent variable is the log unemployment spell of displaced workers. In columns (1) to (4), I measure the diversity of a local labor market using my preferred measure $DIVERSITY_m$. In the last four columns I use the size of workforce in a given local labor market as a simple alternative measure. All regressions include year-of-displacement dummies, four education dummies (dropout, high-school, some college, college or more), a female dummy, a non-black dummy, age (cubic), and tenure on the pre-displacement job (cubic). The sample contains both workers who have and who have not changed occupations after displacement. The sample is restricted to workers whose current job was the first job after displacement, and who were not noticed in advance of their displacement, see Section 1.2. In columns (4) and (8) the sample is further restricted to workers who report to have been displaced due to a plant closing. Standard errors clustered at the occupation level are reported in parenthesis. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table 1.9: First-Stage Estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DIVERSITY$_m$ $\times$ SVP$_j$	0.251***	0.255***	0.252***	0.127				
	(0.0694)	(0.0724)	(0.0812)	(0.121)				
SIZE$_m$ $\times$ SVP$_j$					0.271***	0.275***	0.278***	0.289**
					(0.0611)	(0.0617)	(0.0706)	(0.135)
Observations	1,949	1,949	1,949	553	1,949	1,949	1,949	553
R-squared	0.318	0.330	0.463	0.608	0.315	0.335	0.509	0.645
F-statistic excl. instr.	13.07	12.41	9.597	1.088	19.72	19.81	15.47	4.580
Occupation time trend	yes	yes	yes	yes	yes	yes	yes	yes
State fixed effect	yes	yes	yes	yes	yes	yes	yes	yes
State time trend	no	yes	no	no	no	yes	no	no
MSA fixed effect	no	no	yes	no	no	no	yes	no
Plant closing only	no	no	no	yes	no	no	no	yes

Notes: The regressions are least squares estimates of equation (1.8). The associated second-stage regression is equation (1.7). The dependent variable is the interaction $DIVERSITY_m \times MC_{ijmt}$ in columns (1)-(4) and $SIZE_m \times MC_{ijmt}$ in columns (5)-(8). All regressions include year-of-displacement dummies, four education dummies (dropout, high-school, some college, college or more), a female dummy, a non-black dummy, age (cubic), and tenure on the pre-displacement job (cubic). Only the sub-sample of displaced workers who report to have changed occupations after displacement, whose current job was the first job after displacement, and who were not noticed in advance of their displacement is used for estimation, see Section 1.2. In columns (4) and (8) the sample is further restricted to workers who report to have been displaced due to a plant closing. Standard errors clustered at the occupation level are reported in parenthesis. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

1.8. Appendices

1.8.1. Model

To guide empirical work, I propose a stylized model of occupational choice that illustrates how specificity of human capital, worker mobility costs, and wait unemployment are interconnected. The model describes an economy with many occupations. Occupations differ concerning how specific the human capital is that they require. Displaced workers can either leave or stay in their original occupation. I show that under quite general conditions for workers who ex-ante invested in highly specific human capital switching occupations entails a high expected wage loss. These *mobility costs* in turn can incentivize workers to go through long spells of wait unemployment in order to evade switching occupations and leaving behind their human capital. Using the model, I then sketch the identification strategy that I follow in Section 1.3.

Setup

In the economy there are both vacant jobs and unemployed workers. I abstract from workers who are not in the labor force. The labor market is segmented into N occupations. Occupations differ in the extent of specific vocational preparation a worker needs to acquire to be eligible to work in that occupation. For example, working as a waiter requires less specific job training than working as a physician. All unemployed workers are "experienced" in the sense that they worked in one of the N occupations before. An unemployed worker's pre-displacement occupation defines his type. That is, a worker of type i is endowed with the specific vocational preparation that is required to work in occupation i. Workers are homogeneous within their types.

At time t unemployed workers decide in what occupation to look for a job by comparing the expected value of search across occupations. To keep the framework as general as possible, I only assume that this value depends positively on the (expected) probability to find a job P_{it}, the (expected) wage payment W_{it}, time-specific factor ϕ_t, and time-invariant occupation-specific factors χ_i. The latter captures, amongst others, so-called compensating differentials (Rosen, 1986). Occupations that offer favorable working conditions or non-pecuniary amenities attract labor at a lower wage. For example, for employment in an occupation associated with higher social standing, a worker might forgo a potential higher wage in another occupation. On the other hand, occupations that are characterized by unfavorable working conditions such as shift work must compensate workers by paying a premium.

The value of looking for a job in occupation i at time t can then be written as

$$U(P_{it}, W_{it}; \phi_t, \chi_i)$$

where I assume that U is strictly increase and concave in P_{it} and W_{it} and fulfills the Inada conditions. By writing the value of search in this simple form I also implicitly assume that the stochastic processes governing P_{it} and W_{it} fulfill the Markov property such that expectations of future values are fully determined at time t.

Before a worker is eligible to work in occupation he needs to invest in costly *specific vocational preparation*. A worker of type i who is looking for work in occupation j receives the payoff

$$U(P_{jt}, W_{jt}; \phi_t, \chi_j) - C_{ij}.$$

where C_{ij} is the training cost. In this general form C_{ij} depends *both* on the occupation of origin i and the new occupation j. However, in this paper I make the simplifying assumption that all specific human capital is lost as soon as a worker leaves an occupation. That is, the investment needed to join a new occupation is independent of the occupation of origin.[29] Formally, this assumption implies $C_{ij} = C_j$ for $i \neq j$ where C_j is the extent of specific training needed to work in occupation j.[30] Without loss of generality I can then also normalize $C_{ij} = 0$ if $i = j$; workers who stay in their occupation of origin do not suffer a depreciation of their occupation-specific human capital. Under these simplifying assumptions the payoff of a worker of type i who is looking for work in occupation j can be written as

$$\begin{aligned} U(P_{jt}, W_{jt}; \phi_t, \chi_i) - C_j \qquad & \text{if } i \neq j \\ U(P_{jt}, W_{jt}; \phi_t, \chi_i) \qquad & \text{if } i = j. \end{aligned}$$

From the perspective of an occupation switcher C_j can be interpreted as an *entry cost*. For example, the cost C_j is high if a worker is changing occupations and decides to become a physician but low if he becomes a waiter. On the other hand, a worker who is staying in his pre-displacement occupation is assumed not to lose his knowledge and does not need to invest again.

Why would a worker ever invest in costly specific vocational preparation? In this framework there are two possible reasons. Either investment in specific training leads to higher wages W_{it}. In equilibrium the wage gain would then exactly compensate for the cost of investment. As I will show later, this is strongly supported by the data: extent of specialization of an occupation and wages are strongly correlated. However, my framework allows for another reason: it might be that investment leads to a higher probability to find a job, P_{it}.

Equilibrium

Transferable Human Capital To simplify the exposition, I first consider the equilibrium in the simple case $C_j = 0$ for all j. Here all human capital is general

[29]This is not an innocuous assumption because it is natural to assume that some occupational pairs are more "related" than others. See for example Gathmann and Schönberg (2010). human simplifying assumption that human capital is *not* transferable across occupations and that human capital is fully transferable within the same occupation. gathman

[30]that is paid by the worker

and therefore completely transferable across occupations. At each time t unemployed workers decide to look for work in the occupation that offers the highest expected value of search. In equilibrium, the value of search has then to be identical across all occupations, $U(P_{it}, W_{it}; \phi_t, \chi_j) = U(P_{jt}, W_{jt}; \phi_t, \chi_j)$ for all i, j. If this would not be the case then there would be a pair of occupations i and j for which $U(P_{it}, W_{it}; \phi_t, \chi_i) > U(P_{jt}, W_{jt}; \phi_t, \chi_j)$. Because of the superior value of search in occupation i, workers would then enter occupation i or leave occupation j. These worker flows would drive down the relative job finding rate and wage of occupation i until in equilibrium equality was reestablished.

Specific Human Capital The equilibrium is more complex if C_j differs across occupations. First consider the payoff of an occupation switcher, that is, the payoff received by a worker conditional on leaving his pre-displacement occupation. Such a worker would be indifferent between joining occupations in the set

$$\Xi_t = \arg\max_j \left\{ U(P_{jt}, W_{jt}; \phi_t, \chi_j) - C_j | j \in [1, \dots, N] \right\}.$$

Note that the set Ξ_t is never empty because there has to be always at least one "best" occupation at each point in time t. By definition, for all occupations $j \in \Xi_t$ the value of looking for a job is identical. I refer to this value as the value of switching occupations $\overline{U}$. It then holds

$$U(P_{jt}, W_{jt}; \phi_t, \chi_j) - C_j \leq \overline{U} \qquad \text{for all } j \in [1, \dots, N]$$

where the equality is strict for all $j \in \Xi_t$.

A worker of type j leaves his pre-displacement occupation if $U(P_{jt}, W_{jt}; \phi_t, \chi_j) < \overline{U}$. In this case, the cost C_j he invested in the past to obtain specific vocational preparation turn out to be valueless ex-post. As workers flow out of occupation j wages and probabilities to find a job will adjust such that equality is reestablished in equilibrium. For the moment I leave it open whether the adjustment is primarily due to changes in P_{jt} or W_{jt}. Therefore the value of search is bounded from below and it holds

$$U(P_{jt}, W_{jt}; \phi_t, \chi_j) \geq \overline{U} \qquad \text{for all } j \in [1, \dots, N]. \tag{1.11}$$

In equilibrium each occupation j is therefore in one of three states.

1. When the value of search in occupation j is sufficiently low, workers are *leaving* the occupation. In equilibrium, the remaining workers[31] are indifferent between leaving and staying and it holds

$$U(P_{jt}, W_{jt}; \phi_t, \chi_j) = \overline{U}. \tag{1.12}$$

[31]The remaining workers are the marginal worker are identical because workers are assumed to be homogeneous within their types.

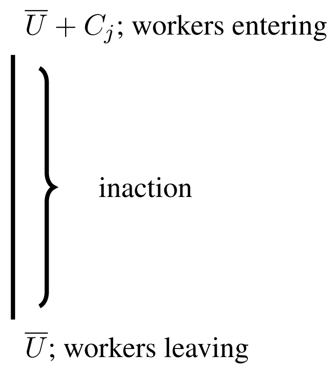

Figure 1.6: $U(P_{jt}, W_{jt}; \phi_t, \chi_j)$ and different equilibria. In equilibrium, each occupation j can be in one of three possible states. If $U(P_{jt}, W_{jt}; \phi_t, \chi_j) = \overline{U}$ workers are *leaving* occupation j. If $U(P_{jt}, W_{jt}; \phi_t, \chi_j) + C_j = \overline{U}$ workers are *entering* occupation j. If $\overline{U} < U(P_{jt}, W_{jt}; \phi_t, \chi_j) < \overline{U} + C_j$ occupation j is in a state of *inaction*; workers are neither joining nor leaving. The higher the required specific vocational preparation of an occupation, the bigger the region of inaction.

2. Worker are *entering* occupation j. For occupations in this state it holds

$$U(P_{jt}, W_{jt}; \phi_t, \chi_j) - C_j = \overline{U}. \tag{1.13}$$

3. Thirdly, occupation j is in a state of *inaction*.

$$\overline{U} < U(P_{jt}, W_{jt}; \phi_t, \chi_j) < \overline{U} + C_j \tag{1.14}$$

   Here the value of search in an occupation is not high enough for outsiders to invest in specific human capital and join. On the other hand, it is not low enough for insiders to exit and leave their human capital investment behind.

Figure 1.6 visualizes these three equilibria. Note that the region of inaction is increasing in the extent of specific vocational preparation C_j. Workers who invested a lot in occupation specific training are reluctant to give up their human capital and leave their occupation behind. This can lead to wait unemployment.

Wait Unemployment

Here I explore under what conditions specific vocational preparation gives rise to mobility costs and wait unemployment. It is convenient to log-linearize equations 1.12 and 1.13 around $(\overline{P}, \overline{W})$, the cross-sectional and time-series global mean of occupational switchers. This yields

$$p^l_{jt} = -b\left(w^l_{jt} - \overline{w}_j\right) + \phi_t + \chi_j \tag{1.15}$$

$$p^e_{jt} = b\left(c_j - (w^e_{jt} - \overline{w}_j)\right) + \phi_t + \chi_j \tag{1.16}$$

where lower-case letters denote variables in logs.[32] Supscripts "l" and "e" stand for "leave" and "enter."

Equation (1.16) is a free-entry condition. It captures a worker's decision to enter an occupation and to invest in costly occupation specific training c_j. Why would a worker join an occupation that requires costly specific vocational preparation? The main motivation is certainly to earn a higher wage.[33] That is, the wage gain $w^e_{jt} - \overline{w}_j$ is increasing in c_j. However, there is an alternative motivation: a worker might expect to be compensated by a higher likelihood to find a job. This is captured by the term $c_j - (w^e_{jt} - \overline{w}_j)$. If $w^e_{jt} - \overline{w}_j = c_j$ and this expression equals zero, the decision to invest in training is purely driven by the desire to earn a higher wage, leaving p^e_{jt} unaffected.

Equation (1.15) formalizes the concept of wait unemployment. When the utility of search in occupation j becomes sufficiently low, workers leave until the remaining workers are indifferent between leaving and staying in occupation j. The higher the (expected) mobility cost $w^l_{jt} - \overline{w}_j$ a worker faces upon leaving occupation j, the higher is the unemployment duration he is willing to accept in order *not* to leave that occupation.

Identification

The objective is to estimate the trade-off between mobility cost and unemployment duration captured by the coefficient b. A factor hindering estimation are the unobserved occupation and time fixed-effects χ_j and ϕ_t.

Ideal Data In a first best world with ideal labor market data, one could use the following identification strategy. Mobility cost only matter for wait unemployment when displaced workers actually consider mobility, that is, switching occupations. In the model, this is the case in equilibrium *leave*, captured by equation (1.15). The state *enter* described by equation (1.16) serves as a control group: here mobility cost are irrelevant because leaving the pre-displacement occupation is not profitable in the first place. One could now use a difference-in-difference approach to estimate coefficient b based on *within*-occupation variation.

Consider two occupations h and l. The former requires costly investment in specific vocational training c_h whereas the latter does not and $c_l = 0$. For both occupations I can now calculate the difference of the job finding probabilities between the states *leave* and *enter* and then construct the difference-in-difference as

$$(p^l_{ht} - p^e_{ht'}) - (p^l_{lt} - p^e_{lt'}) = -b\left(w^l_{ht} - \overline{w}_h\right) - \underbrace{b\left(c_h - (w^e_{ht} - \overline{w}_h)\right)}_{\text{potential bias}}. \quad (1.17)$$

Note that the unobserved occupation and time fixed-effects have been differenced out. However, a bias in the estimates remains if $c_h - (w^e_{ht} - \overline{w}_h) > 0$.

[32] c is normalized

[33] Note that non-pecuniary motivations such as raise social standing are captured by the occupation fixed-effects χ_j.

A challenge to identification is the potential endogeneity of mobility cost $w^l_{ht} - \overline{w}_h$. To resolve this problem, I exploit that mobility cost are at least partially determined by c_j. Note that in general mobility cost can be written as $w^l_{jt} - \overline{w}_j = \rho_j \left(w^e_{jt} - \overline{w}_j \right)$ where $0 \leq \rho_j \leq 1$ can be interpreted as a measure of wage rigidity. The bigger the rigidity ρ_j and the higher the extent to which occupations requiring higher specific training pay higher wages $\left(w^e_{jt} - \overline{w}_j \right)$, the higher are the mobility costs that workers are facing. Without loss of generality, I can rewrite $w^e_{jt} - \overline{w}_j = \xi_j c_j$ where $0 \leq \xi_j \leq 1$ stands for the extend to which investment in training leads to higher wages. It is then convenient to express mobility cost as a function of occupation-specific investment c_j:

$$w^l_{jt} - \overline{w}_j = \rho_j \xi_j c_j \tag{1.18}$$

As I show in Section 1.3, there is a strong correlation between mobility cost an the extent of occupation-specific training in the data, implying that $\rho_j \xi_j$ is not only marginally different from zero. Because c_j is also arguably exogenous, I can use this relation for identification in an instrumental variable approach.

Operationalization Neither the variable c_j nor whether an occupation is "shedding" workers nor not – that is, the states "enter" and "leave" – are easy to observe in the data. As described in Section 1.2 and 1.3, I therefore operationalize the model by, firstly, proxying c_j with the *length* of a workers specific vocational preparation (SVP). Secondly, I exploit that in a more "diverse" labor market the mobility cost a worker is facing should be less likely to be binding.

1.8.2. Definition of *Specific Vocational Preparation*

The Dictionary of Occupational Titles (DOT) published by the U.S. Department of Labor in 1991 defines the variable *Specific Vocational Preparation* as follows:

> Specific Vocational Preparation is defined as the amount of lapsed time required by a typical worker to learn the techniques, acquire the information, and develop the facility needed for average performance in a specific job-worker situation.
>
> This training may be acquired in a school, work, military, institutional, or vocational environment. It does not include the orientation time required of a fully qualified worker to become accustomed to the special conditions of any new job. Specific vocational training includes: vocational education, apprenticeship training, in-plant training, on-the-job training, and essential experience in other jobs.
>
> Specific vocational training includes training given in any of the following circumstances:
>
> a. Vocational education (high school; commercial or shop training; technical school; art school; and that part of college training which is organized around a specific vocational objective);
>
> b. Apprenticeship training (for apprenticeable jobs only);
>
> c. In-plant training (organized classroom study provided by an employer);
>
> d. On-the-job training (serving as learner or trainee on the job under the instruction of a qualified worker);
>
> e. Essential experience in other jobs (serving in less responsible jobs which lead to the higher grade job or serving in other jobs which qualify).

Chapter 2

ACCOUNTING FOR MISMATCH UNEMPLOYMENT (JOINT WITH THIJS VAN RENS)

2.1. Introduction

Unemployment has been at persistently very high levels in the United States since the start of the Great Recession in December 2007. One explanation that has been suggested is a mismatch in the skills or geographic location of the available jobs and workers (Kocherlakota (2010)). A rise in mismatch seems to be supported by a decline in aggregate matching efficiency (Elsby et al. (2010), Barnichon and Figura (2012)) and geographic mobility (Frey (2009), Katz (2010)). There is, however, little empirical work on mismatch using disaggregated data.

In this paper, we estimate mismatch unemployment on the U.S. labor market, study its evolution over time and explore what frictions caused the mismatch. This exercise is interesting because of its implications for both economic policy and economic theory. In the context of the policy debate, it has been argued that mismatch unemployment is 'structural,' in the sense that it is more persistent than the business cycle and not responsive to stablization policy.[1] We find no evidence for this claim. Mismatch increased not only in the Great Recession but also in previous

[1]The most prominent proponent of this view was the president of the Federal Reserve Bank of Minneapolis, Narayana Kocherlakota (2010), who argued in a speech that "it is hard to see how the Fed can do much to cure this problem. Monetary stimulus has provided conditions so that manufacturing plants want to hire new workers. But the Fed does not have a means to transform construction workers into manufacturing workers." Kocherlakota also argued that given the nature of mismatch unemployment, we should expect the high unemployment rate to be persistent: "Given the structural problems in the labor market, I do not expect unemployment to decline rapidly." Shortly after the 2001 recession, Groshen and Potter (2003) made a similar argument that misallocation of workers over industries might explain the so called jobless recoveries.

recessions. Over the entire sample period, mismatch unemployment is as cyclical as as the overall unemployment rate and no more persistent.[2]

Our contribution to economic theory is to provide a detailed empirical analysis of mismatch as a possible micro-foundation for unemployment. In most modern macroeconomic models of the labor market there is unemployment because of search frictions. But the micro-foundations for search frictions and the aggregate matching function are not very well developed. If unemployment is truly due to a time cost of search, it seems there should be a secular downward trend in the unemployment rate as computers and the internet improve the search technology available to firms and workers. Instead, we should think of search frictions as "a modeling device that captures the implications of the costly trading process without the need to make the heterogeneity and the other features that give rise to it explicit" ((Pissarides, 2000, p.4)). Mismatch generates heterogeneity and therefore gives rise to unemployment. The results in this paper shed light on the question what are the frictions that give rise to mismatch.[3]

We use an accounting framework that puts just enough structure on the data to allow us to quantify the sources of mismatch unemployment. In this framework, the labor market consists of multiple submarkets or segments. Conditions in each segment are characterized by four variables: the job finding rate, which measures how hard it is for workers to find a job; the job filling or worker finding rate, which measures how hard it is for firms to find a worker; workers' surplus from having a job over being unemployed; and firms' surplus of having a filled position over a vacancy. Within segments, frictions prevent the instantaneous matching of unemployed workers to vacant jobs, resulting in search unemployment in the tradition of

[2]To many, this conclusion may not come as a surprise. From December 2007 to December 2009, 2.3 million manufacturing workers lost their jobs and from December 2009 to December 2011 no more than 300 thousands jobs were created in this sector (BLS Current Employment Statistics). It seems, therefore, that there is no need for the Fed to turn construction workers into manufacturing workers. Given the lack of this type of direct evidence, Kocherlakota's view has been heavily criticized (Krugman (2010), DeLong (2010)). In his Nobel lecture, Peter Diamond (2011) draws attention to the fact that this is not the first time that a recession is mistake for a structural change: "There is no surprise that we are hearing claims of higher structural unemployment - such statements appear when unemployment is high. A similar debate unfolded as I was a new student of economics. (...) Indeed, there is a long history of claims that the latest technological or structural developments make for a new long-term high level of unemployment, but these have repeatedly been proven wrong." (page 1065). In fact, Kocherlakota himself changed his views in light of the evidence, New York Times (2014).

[3]Some recent studies discuss this issue from a theoretical perspective. Shimer (2007) formally shows that mismatch between the distributions of workers and jobs over segments of the labor market gives rise to a relation between the job finding probability and labor market tightness that is very similar to the relation obtained if there are search frictions and an aggregate matching function. Stock-flow matching, as in Coles et al. (2010), rest unemployment, as in Alvarez and Shimer (2011), reallocation unemployment as in Carillo-Tudela and Visschers (2011) and waiting unemployment as in Birchenall (2011) are all closely related to this concept of unemployment due to mismatch. As opposed to these studies, the focus of our paper is empirical. One way to think about the contribution of this paper is to provide a set of facts unemployment that can be used to test the theoretical models of mismatch unemployment.

Diamond (1982), Mortensen (1982) and Pissarides (1985). Across segments, adjustment costs lead to dispersion in labor market conditions, generating mismatch unemployment. There are four sources of mismatch unemployment: worker mobility costs, job mobility costs, wage setting frictions and heterogeneity in matching efficiency. Figure 2.1 visualizes the framework.

In order to estimate mismatch unemployment and its sources, we need data on job and worker finding rates and worker and job surplus by labor market segments, which we operationalize as states or industries. We construct these variables over the 1979-2009 period using data on worker flows and wages from the Current Population Survey (CPS) and data on profits from the National Income and Product Accounts (NIPA). Since in our accounting framework all workers and all jobs are assumed to be identical, we verify that our results are robust to controlling for observable worker characteristics and for unobservable but time-invariant worker and job characteristics (compensating differentials) by allowing for state and industry-specific fixed effects in all variables.

Mismatch is an important reason for unemployment. A back-of-the-envelope calculation to correct our estimates for aggregation bias suggests that mismatch is responsible for 84% of the level and all of the fluctuations in the unemployment rate. While suggestive, these estimates should be interpreted with care, because our framework is not ideal to estimate the overall amount of (changes in) mismatch unemployment. Nevertheless, our estimates are in line with other studies that use different estimation methods, which are more suitable to answer the question what is the total amount of mismatch in the US labor market (Sahin et al. (2014), Barnichon and Figura (2011a)). Compared to these studies, the strengths of our framework are that (i) we have a much longer time series so that we can explore the cyclical behavior of mismatch unemployment, and (ii) we not only estimate the overall amount of mismatch unemployment but decompose it into its sources. We now turn to our results on these topics.

The cyclical behavior of mismatch unemployment is very similar to that of the overall unemployment rate. This finding is driven by the fact that dispersion in labor market conditions across states and industries moves closely with the business cycle, similar to what Abraham and Katz (1986) documented over two decades ago.[4] The unemployment that derives from this dispersion is as cyclical as the overall unemployment rate and no more persistent. As a corollary, the nature of the increase in unemployment in the Great Recession is no different from previous recessions, although it is of course more severe.[5] In terms of policy implications,

[4]In response to the structural shifts view of recessions put forward by Lilien (1982), which holds that recessions are periods of reallocation between industries akin to mismatch, Abraham and Katz show that aggregate shocks can give rise to countercyclical fluctuations in dispersion of employment growth across sectors.

[5]This result is not inconsistent with observation that there was an outward shift in the Beveridge curve, the negatively sloped relation between vacancies and unemployment, which indicates a decline in aggregate matching efficiency and provides much of the basis for the argument that there was an unprecedented increase in mismatch in the Great Recession (Kocherlakota (2010), Elsby et al. (2010)). While an increase in mismatch indeed reduces matching efficiency (Shimer (2007)), there

this result casts doubt on Kocherlakota (2010)'s claim that stabilization policy is not effective against mismatch unemployment. In terms of the implications for economic theory, the result is consistent with, although of course not sufficient evidence for, the view that all unemployment is due to mismatch.

Our second and most interesting set of results concerns the sources of mismatch. Our framework has strong predictions for patterns we should observe in the data in the absence of the various frictions that can give rise to mismatch. If there are no barriers to worker mobility, we expect a strong negative correlation between wages (measuring how attractive it is to have a job in a given state or industry) and job finding rates (how hard it is to find these jobs). In the data, we find that deviations from this predicted correlation are small and non-systematic. Similarly, if there are no barriers to job mobility, jobs that are attractive to firms should be hard to fill, generating a strong negative correlation between profits and job filling rates. We observe this correlation in the data across states and to a lesser extent across industries as well. Most mismatch is caused by barriers to job mobility across industries and by deviations from surplus sharing in equal proportions across both states and industries. Industries and particularly states with high wages tend to have low profits. This implies that states and industries that are attractive to workers are unattractive to firms and vice versa, generating dispersion in vacancy-unemployment ratios and mismatch unemployment. Little to no mismatch comes from worker mobility frictions. As a result, policies aimed at increasing worker mobility, as advocated e.g. by Katz (2010), are likely to have small effects and may even be counterproductive.

Empirical studies on mismatch tend to focus on shifts in the Beveridge curve, trying to use aggregate data to estimate matching efficiency (Lipsey (1965), Abraham (1987), Blanchard and Diamond (1989), Barnichon and Figura (2012)) and there is little recent empirical work using disaggregated data.[6] Two recent contributions are closely related to this paper. Sahin et al. (2014) use disaggregated data on unemployment and vacancies to construct indices of mismatch, using data from the JOLTS and the HWOL for the 2001-2011 and 2005-2011 periods respectively. Barnichon and Figura (2011a) use the CPS to explore how much dispersion in labor market conditions contributes to movements in matching efficiency. Our findings are consistent with these papers in terms of the contribution of mismatch across states and industries to the increase in unemployment in the Great Recession. The finding that geographic mismatch cannot explain why the increase in unemployment in the Great Recession is so much larger than in previous recessions is also consistent with work by Kaplan and Schulhofer-Wohl (2010), who show that most of the a drop in interstate migration in the Great Recession is a statistical artifact. Compared to Sahin et al. (2014), we provide an alternative method to estimate mismatch unemployment, which gives us a much longer time series. Compared to

are many other causes for shifts in the Beveridge curve as well, including changes in the separation rate and demographics. Controlling for these factors, the remaining role for mismatch is very small (Barnichon and Figura (2012)).

[6]Older studies include work by Padoa Schioppa (1991) and Phelps (1994).

Barnichon and Figura (2011a), our focus is on unemployment rather than matching efficiency. Compared to both papers, we contribute by providing a framework that allows us to decompose mismatch into it sources and estimating the contribution of each of these sources to unemployment.

This paper is organized as follows. In the next section we present the accounting framework to formalize the sources of dispersion in labor market conditions across submarkets of the labor market. We identify four sources of mismatch, three of which we can estimate: worker mobility costs, job mobility costs and wage setting frictions. Section 2.3 describes the data used in the estmation, and explains in detail how we construct the empirical counterparts of the variables that define a labor market segment in our model. Section 2.4 presents the empirical results and Section 2.5 concludes.

2.2. Accounting Framework

The theoretical framework presented here allows us to formalize the mechanisms, by which heterogeneity in labor market conditions across submarkets of the labor market leads to mismatch unemployment. In addition, we use the framework to guide the empirical exercise how to estimate structural unemployment and how to decompose it into its sources. We try to make as little assumptions as possible. In particular, we do not assume anything about vacancy creation, but model only the distribution of vacancies and unemployed workers over submarkets.

Unemployed workers look for jobs, and firms with vacancies look for unemployed workers on the labor market. But not each unemployed worker can match with each vacancy. We model this by thinking of the labor market as being segmented into submarkets. A submarket is defined as the subset of jobs that a given unemployed worker searches for, or the subset of unemployed workers that can form a match with a given vacancy. We assume that there is a one-to-one mapping of the set of workers and firms that search for each other, ruling out that workers or firms spread out their search effort over several submarkets.[7] In addition, we assume that in each submarket, there is a matching technology of unemployed workers and vacancies.[8]

Under these assumptions, labor market conditions in a submarket can be completely characterized by four variables: the probability that an unemployed workers

[7]This assumption is without loss of generality as long as the total amount of search effort is limited. It is, of course, difficult to operationalize this concept of a submarket empirically. In practice, we use either states or industries in most of our estimates, which is a much higher level of aggregation compared to the ideal. In Sections 2.2.2 and 2.4.2, we discuss how this affects our estimates.

[8]Our accounting framework is based on worker and job mobility arbitraging away differences in the value of searching in each submarket. In order for arbitrage to be possible, we need the (plausible) assumption that the matching technology has positive and diminishing returns in each of its inputs. In other words, we assume that adding an additional unemployed worker to a submarket, ceteris paribus, makes it harder for workers to find jobs and easier for firms to fill vacancies (and similar for adding an additional vacancy).

finds a job, the increase in life-time earnings by a worker who finds a job, the probability that a vacant job finds a worker, and the increase in life-time profits by a firm that fills a vacant job. These four variables are the job finding rate f_i^W, worker surplus S_i^W, the worker finding rate f_i^F and job surplus S_i^F in submarket i respectively.

Any labor market model with a segmented labor market must describe how labor market conditions are related across submarkets. We show which relations effectively reduce the segmented labor market to a single market, as in the standard search and matching model with homogeneous workers and jobs, in the tradition of Diamond (1982), Mortensen (1982) and Pissarides (1985). We take these relations as a benchmark and explore the effect of deviations from these relations. Unemployment that results under the benchmark model may be due to a variety of frictions, for instance search frictions. We refer to this unemployment as 'frictional.' Unemployment that results from deviations from this model and is therefore due to dispersion in labor market conditions is called mismatch unemployment.

2.2.1. Benchmark Relations

The relation between the job finding rate f_i^W and worker surplus S_i^W across submarkets is determined by assumptions about worker mobility between submarkets, the relation between the worker finding rate f_i^F and job surplus S_i^F by assumptions about job mobility (mobility of vacancies), the relation between worker and job surplus by assumptions about wage determination, and the relation between job and workers finding rates by assumptions about the matching technology. These four relations, which are summarized in Figure 2.1, fully determine conditions in submarkets of the labor market. We now discuss each of these four relations in turn.

Worker Mobility

An unemployed worker, searching for a job in submarket i, receives an unemployment benefit b_i^W (which, as usual, includes the utility from leisure). With probability f_i^W, this worker finds a job, in which case she receives the worker surplus S_i^W from the match. Thus, the per-period value of searching for a job in submarket i, assuming it is constant over time, is given by $z_i^W = b_i^W + f_i^W S_i^W$.[9]

If workers may freely decide in which submarket to search, i.e. if there are no barriers to worker mobility, it must be that the value of searching is equalized across submarkets, so that $z_i^W = z^W$ for all i. Using a bar over a variable to denote its mean over all submarkets and a hat to denote relative deviations from this mean, e.g. $\hat{f}_i^W = \left(f_i^W - \bar{f}^W\right)/\bar{f}^W$, equalization of the value of searching in all submarkets implies the following relation between f_i^W and $\hat{S}_i^W$, which we label

[9] The assumption that z_i^W is in steady state seems reasonable, because average unemployment duration, compared to the length of a typical business cycle, is short in the US.

the worker mobility curve.[10]

$$\hat{f}_i^W + \hat{S}_i^W = -\frac{\bar{b}^W}{z^W - \bar{b}^W}\hat{b}_i^W \tag{2.1}$$

Assuming unemployment benefits are the same in all submarkets, we get $\hat{f}_i^W = -\hat{S}_i^W$. The worker mobility curve is a no-arbitrage condition. It states that attractive jobs must be hard, and unattractive jobs easy to find, in order for workers to be indifferent which job they search for. If unemployment benefits differ across submarkets, then submarkets with high unemployment benefits must have low job finding rates or low worker surplus or both.

If there are barriers to worker mobility, for example because it is costly to move from one state to another, or because moving into a different industry requires costly retraining, then there may be differences in the value of searching across submarkets. We denote these differences by α_i^{WM}, so that the worker mobility curve is given by

$$\hat{f}_i^W + \hat{S}_i^W = \alpha_i^{WM} \tag{2.2}$$

If unemployment benefits are the same across submarkets, the dispersion in α_i^{WM} is a measure of worker mobility costs. If the difference in the value of searching in a particular submarket i becomes to high compared to the average, it becomes worth for workers to pay the mobility cost and move into that submarket. Unemployed workers moving into market i makes it harder to find a job in that submarket, reducing f_i^W and therefore α_i^{WM}. If unemployment benefits vary across submarkets, then differences in the value of searching may also reflect differences in unemployment benefits, $\alpha_i^{WM} = -\frac{\bar{b}^W}{z^W - \bar{b}^W}\hat{b}_i^W$.

Job Mobility

Having a vacancy looking for a worker in submarket i yields the firm b_i^F, which may be a negative number, i.e. vacancy posting costs. With probability f_i^F, this vacancy gets filled, in which case the firm gets surplus S_i^F from the match. Thus, the (steady state) per-period value of searching for a worker in submarket i is given by $z_i^F = b_i^F + f_i^F S_i^F$.

If firms can freely relocate vacancies across submarkets, no-arbitrage requires that the value of searching for a worker must be equal across submarkets. Analogous to the worker mobility curve, we get a job mobility curve, which states that jobs that are attractive to firms must be hard to fill. If there are barriers to job mobility, these give rise to differences in the value of a vacancy across submarkets.

$$\hat{f}_i^F + \hat{S}_i^F = \alpha_i^{JM} \tag{2.3}$$

Dispersion in α_i^{JM} may reflect job mobility costs or dispersion in vacancy posting costs, $\alpha_i^{JM} = -\frac{\bar{b}^F}{\bar{z}^F - \bar{b}^F}\hat{b}_i^F$.

[10] The condition is exact for log-deviations but only a first-order approximation for relative deviations from the mean. The reason we nevertheless prefer relative deviations is because empirically log-deviations are problematic in the (rare) cases that variables are negative.

Wage Determination

The relation between worker and firm surplus is determined by assumptions on how worker and firm divide the total surplus from their match. The instrument that is used to divide the surplus is the wage. In our benchmark relation, which is the only relation that does not give rise to any mismatch, firm and worker share the surplus in fixed proportions across segments. This relation would be true in standard labor market models, which commonly assume that wages are set by generalized Nash bargaining. Here, however, we are not making any specific assumptions on the wage determination process, but merely stating a benchmark relation for surplus sharing that does not give rise to mismatch.

If the share of match surplus that goes to the worker ϕ_i, often referred to as the worker's bargaining power, is constant across submarkets, then worker and job surplus are proportional across submarkets, $\hat{S}_i^W = \hat{S}_i^J$. In general, wages may deviate from this benchmark relation, for example because bargaining power varies across segments or because wages are not rebargained in each period. This is captured by deviations from the wage determination curve.

$$\hat{S}_i^W = \hat{S}_i^F + \alpha_i^{WD} \tag{2.4}$$

Dispersion in α_i^{WD} may reflect wage bargaining costs or heterogeneity in workers bargaining power, $\alpha_i^{WD} = \widehat{\frac{\phi_i}{1-\phi_i}}$, but may also reflect that wages are determined by a completely different mechanism than bargaining.

Matching Technology

The final relation needed to close the model, between worker and job finding rates, is determined by assumptions on the matching technology. In our benchmark relation, the probability that workers find jobs and the probability that firms find workers are inversely log-proportional. This is true, for instance, if matches are formed from unemployed workers and vacancies through a constant returns to scale Cobb-Douglas matching function. Under this assumption, the worker and job finding rates are both iso-elastic functions of the vacancy-unemployment ratio θ_i, often referred to as labor market tightness, $f_i^F = B_i\theta_i^{-\mu}$ and $f_i^W = B_i\theta_i^{1-\mu}$, where μ is the elasticity of unemployment in the matching function and B_i is matching efficiency. This gives rise to the following curve, describing the matching process.

$$\hat{f}_i^F = -\frac{\mu}{1-\mu}\hat{f}_i^W + \alpha_i^{MT} \tag{2.5}$$

Dispersion in α_i^{MT} reflects dispersion in matching efficiency across submarkets, $\alpha_i^{MT} = \frac{\hat{B}_i}{1-\mu}$. If the elasticity of the matching function is not constant across submarkets, then the above relation still holds in first order approximation, and α_i^{MT} reflects all differences in the matching function across submarkets, $\alpha_i^{MT} = \frac{\hat{B}_i}{1-\mu} - \frac{\mu}{1-\mu}\left(\bar{f}^W - \bar{f}^F\right)\hat{\mu}_i$.

Our data do not allow us to test the benchmark relation on the matching technology. Therefore, in the empirical work we will assume that $\alpha_i^{MT} = 0$ for all i. There is some evidence from other data sources that this assumption may not be too far from the truth (Sahin et al. (2014)), see Section 2.3.2 for a short discussion. In the description of the framework in this section, we will allow for α_i^{MT} to be non-zero for completeness.

2.2.2. Mismatch Unemployment

We combine equations (2.2), (2.3), (2.4) and (2.5) to solve for the distribution of job finding rates across segments.

$$\hat{f}_i^W = (1-\mu)\left(\alpha_i^{WM} - \alpha_i^{JM} - \alpha_i^{WD} + \alpha_i^{MT}\right) \tag{2.6}$$

Note that the benchmark relations were defined so that only deviations from these equations give rise to dispersion in labor market conditions. If there is perfect worker mobility, perfect job mobility, wages are set to share match surplus in constant proportions, and there is a matching function with constant matching efficiency, then labor market conditions are identical in all submarkets. Setting $\alpha_i^{WM} = \alpha_i^{JM} = \alpha_i^{WD} = \alpha_i^{MF} = 0$ in equation (2.6) gives $\hat{f}_i^W = 0$ or $f_i^W = \bar{f}^W$ for all i. Substituting back into the various equations, it is straightforward to show that the worker finding rate, and worker and firm surplus are equalized as well. In this case, the model reduces to a standard labor market model, in which we can effectively think of the labor market as a single, unsegmented market. Unemployment in this case is entirely due to frictions within submarkets, e.g. search frictions.

Dispersion in labor market conditions generates unemployment because the job finding rate is concave in labor market tightness. Therefore, the distribution of vacancies and unemployed workers that results in the highest aggregate job finding rate, keeping fixed the total number of vacancies and unemployed fixed, is to equalize labor market tightness over submarkets. To formalize this, consider a mean-preserving change in the distribution of labor market tightness from θ_i to θ_i'. The counterfactual unemployment rate u^{CF} that prevails under the new distribution is given by,

$$\frac{u^{CF}}{u} \simeq \frac{\bar{f}^W}{\bar{f}^{W,CF}} = \left(\frac{E\left[\left(1+\hat{f}_i^{W,CF}\right)^{\frac{1}{1-\mu}}\right]}{E\left[\left(1+\hat{f}_i^{W}\right)^{\frac{1}{1-\mu}}\right]}\right)^{1-\mu} \propto \frac{V\left[\theta_i^{CF}/\bar{\theta}^{CF}\right]}{V\left[\theta_i/\bar{\theta}\right]} \tag{2.7}$$

where $0 < \mu < 1$ is the elasticity of unemployment in the matching function. See appendix 2.7.1 for the derivation of equation (2.7).

The aggregate job finding rate is higher and therefore the unemployment rate lower, $\bar{u}^{CF} < \bar{u}$, if and only if the dispersion in $f_i^{W,CF}$ is smaller than the dispersion in f_i^W, in the sense that θ_i is a mean-preserving spread of θ_i^{CF} (i.e. the distribution of θ_i^{CF} second-order stochastically dominates the distribution of θ_i).[11] For example, if f_i^W are the actual job finding rates and $f_i^{W,CF}$ the finding rates that would prevail without mismatch, then $\left(u - u^{CF}\right)/u$ is the fraction of unemployment that is due to mismatch.

2.2.3. Mismatch Accounting

Deviations from any of the four benchmark relations generate dispersion in labor market tightness and job finding rates. There are four sources of dispersion across submarkets of the labor market segments: α_i^{WM} represents heterogeneity in unemployment benefits and barriers to worker mobility, α_i^{JM} heterogeneity in vacancy posting costs and barriers to job mobility, α_i^{WD} heterogeneity in wage bargaining power and wage rigidities, and α_i^{MT} heterogeneity in matching efficiency. All four sources lead to unemployed workers and vacancies being in different submarkets and thus cause mismatch unemployment. For example, if $\alpha_i^{WM} > 0$, too few unemployed workers are searching for jobs in submarket i, either because unemployment benefits are relatively low there or because mobility costs prevent more unemployed workers from moving into that submarket. If $\alpha_i^{WD} > 0$, too many unemployed workers and too few vacancies are in submarket i, because wages are higher (and profits lower) than in comparable jobs in other submarkets so that workers reap a disproportionally large share of match surplus in this submarket.

Equations (2.6) and (2.7) allow us to decompose structural unemployment into its four sources. The idea is that if we remove, for example, the worker mobility costs, setting $\alpha_i^{WM} = 0$, but leave the job mobility costs, wage bargaining costs and heterogeneity in matching efficiency in place, then α_i^{JM}, α_i^{WD} and α_i^{MT} would stay the same. Notice that this is probably not a good assumption for the short run, because worker or job mobility or wage rebargaining affects equations (2.2), (2.3) and (2.4) simultaneously. In the long run, however, after many shocks have hit the labor market, we would expect deviations because of job mobility and wage bargaining costs or heterogeneity in matching efficiency to be similar to what they were. Thus, the question we can answer is what unemployment rate would prevail in the long run, if we removed one or more deviations from the benchmark model.

The procedure to decompose mismatch unemployment into its sources is implemented in three steps. First, we estimate the α's using equations (2.2), (2.3), (2.4) and (2.5) and data on the surpluses and finding rates (Section 2.3 below describes how we obtain these data). Second, given estimates for α_i^{WM}, α_i^{JM}, α_i^{WD} and α_i^{MT}, we use equation (2.6) to calculate what the job finding rates in each submarket would be if we set one or more of the α 's equal to zero. Finally, using

[11] The first approximation is just for ease of interpretation. In the empirical work, we calculate the counterfactual job finding rate using (2.7) and then calculate the counterfactual steady state unemployment rate as $u = \lambda / (\lambda + f)$.

equation (2.7), we calculate the unemployment rate that would prevail under these scenarios. We refer to this exercise as mismatch accounting.

The last step of the decomposition is always relative to a baseline level of unemployment, see equation (2.7). This means we can always estimate the contribution of each source in two different ways, introducting the friction with respect to the baseline that the friction was not there or removing the friction with respect to the baseline that it was there. In general, the two approaches will give different answers because the α's may be correlated.[12] More importantly, the contribution of each friction will depend on the order in which we introduce or remove the various frictions. In appendix 2.7.2 we show that the contribution of a friction we remove includes the contribution of the covariance of that friction with other frictions in place, whereas the contribution of a friction we introduce does not. Therefore, we calculate the contribution of each friction in both ways and average it, attributing the covariance between two frictions in equal proportions to each of the frictions. This way, we make sure that our decomposition adds up to the total amount of mismatch unemployment.

2.3. Data and Measurement

To test the relations we derived in the previous section, we need empirical measures of the job-finding rate f_i^W, the worker-finding rate f_i^F, worker surplus S_i^W, which is closely related to wages, and job surplus S_i^F, closely related to profits, for submarkets of the labor market. In this section, we describe how we obtain these measures. In Section 2.3.1, we describe the micro-data we use to extract disaggregated measures for finding rates, wages and profits. Then, in Sections 2.3.2 and 2.3.3, we describe how we use these data to calculate the theoretical measures we need for our accounting exercise. Here, we need to make a good number of auxilliary assumptions and these sections anticipate a large number of robustness checks that we will revisit after discussing our results in Section 2.4.4.

The first empirical difficulty is how to define a labor market segment or submarket. A submarket of the labor market is defined as a subset of unemployed workers or vacant jobs that are similar to each other but different from other workers or jobs, so that each unemployed worker and each firm with a vacant job searches in one submarket only. In our theoretical framework, we assumed that submarkets

[12]Note that this also means that removing one or more of these sources of mismatch does not necessarily decrease unemployment as the different frictions may reinforce or counteract each other. This result is intuitive. Imagine two otherwise identical submarkets of the labor market, one with high wages and one with low wages. Suppose these wage differentials can exist because of wage bargaining costs, but that labor market tightness is nevertheless equal in both submarkets, because mobility costs prevent workers and jobs from moving from one submarket to the other. Now suppose we removed the mobility costs but left the wage bargaining costs in place. Unemployed workers would move to the submarket where wages are high, whereas vacancies would move to the submarket where wages are low. The result would be a decrease in the aggregate job finding rate and an increase in structural unemployment. In the empirical analysis in Section 2.4, we will show that this is in fact a realistic mechanism.

are mutually exclusive, so that two workers that are searching for some of the same jobs are searching for all of the same jobs, and if a worker is searching for a job, then that job is searching for that worker. In practice, these assumptions are likely to be violated, unless we define submarkets as very small and homogeneous segments of the labor markets, based on geographic location as well as the skill set required to do a job.

We use 50 U.S. states to explore geographic mismatch and around 33 industries to explore skill mismatch.[13] This choice is driven by data limitations and follows other empirical contributions in this literature (Sahin et al. (2014), Barnichon and Figura (2011a)). Unfortunately, it is not possible to use very small submarkets, because we would have too little data about each submarket.[14] It is also not possible to use occupations, as Sahin et al. (2014) do, even though occupations arguably better describe categories of jobs that require similar skills than industries, because data on profits by occupation are not available.

2.3.1. Data Sources

Our primary data sources are the January 1979 to December 2009 basic monthly files of the Current Population Survey (CPS) administered by the Bureau of Labor Statistics (BLS). We limit the sample to wage and salary workers between 16 and 65 years of age, with non-missing data for state and industry classification. From the matched basic monthly files we construct job finding and separation rates, using the variable labor force status, which indicates which workers are unemployed and which are employed. We aggregate the monthly data to an annual time series in order to increase the number of observations. Our estimates of finding and separation rates are based on about $23,000$ and $500,000$ observations per year, respectively. From the outgoing rotation groups, we get wages, calculated as usual weekly earnings divided by usual weekly hours. Again, we aggregate the data to an annual time series, ending up with a sample of about $150,000$ workers per year. Tables 2.1, 2.2, and 2.3 list the states and industries we use and summarize the number of observations used to calculate the job finding rate and the average wage for the state-year and industry-year cells. The average cell size for job finding rates is 569 per year for the state-level data and 679 per year for the industry-level data and the smallest cells have 158 and 102 observations respectively.

Data on profits by state and industry come from the National Income and Product Account (NIPA) data collected by the Bureau of Economic Analysis (BEA). We use gross operating surplus per employee as our measure of profits. Gross operating surplus equals value added, net of taxes and subsidies, minus compen-

[13] Precisely, we have 33 industries based on the SIC classification for the 1983-1997 period and 32 industries based on the NAICS classification for the 2003-2009 period.

[14] Shimer (2007), for instance, suggests using the interaction of 800 occupations and 922 geographic areas (362 MSAs plus 560 rural areas), which gives a total of $740,000$ submarkets. In our dataset, we have information on about $150,000$ workers in a given year, so that we would have 1 datapoint for each 5 submarkets.

sation of employees. Net operating surplus equals gross operating surplus minus consumption of fixed capital and is the measure of business income from the NIPA that is closest to economic profits. Since data on net operating surplus are not readily available and fixed capital does not differ much across states and industries, we use gross operating surplus. Under the assumptions of a Cobb-Douglas production technology and perfect capital markets, profits per employee equal the marginal profits from hiring an additional worker.[15] We drop the industries "Mining,""Utilities,""Real estate and rental and leasing," and "Petroleum and coal products manufacturing" because reported profits are extremely large in these industries.

In 1998, the industry classification system changes from the SIC to the NAICS. Using a consistent industry classification over the entire sample period would force us to aggregate at a higher level. Instead, we use the SIC classification until 1997 and the NAICS from 1998 onwards, using approximately the same number of industries in both subsamples. This allows us to calculate comparable cross-industry variances for $\hat{f}_i^W$, $\hat{f}_i^F$, $\hat{S}_i^W$ and $\hat{S}_i^F$ over the full sample period. The only problem with this approach is that the change in classification may introduce jumps in the variances in 1998 because of sampling error (although the industries are subsamples of the data with on average the same size before and after 1998, they are different draws). We solve this problem by imposing that the variances may change smoothly over time but may not jump in 1998. We implement this by regressing the squares of the four variables on a polynomial time trend and a post-1998 dummy. Because all variables are in deviations from their mean, the average of the square equals the variance and the polynomial trend captures smooth changes in this variance. We then correct the post-1998 data for the estimated jump in the variance.

We use nominal data on wages and profits and do not use a price deflator in our baseline estimates. The reason is that if we were to use an aggregate series for the deflator, this would not affect our results, which use only the cross-sectional variation in the data.

Finally, we need to make assumptions on unemployment benefits (including the utility from leisure) b_{it}^W, vacancy posting costs $-b_{it}^F$, the discount rate r and the elasticity of the matching function μ. In our baseline results, we assume the replacement ratio b_{it}^W/w_{it} equals 0.73, which is the value preferred by Hall (2009) and Nagypal and Mortensen (2007). We explore the robustness of our results to setting the replacement ratio to 0.4 (as in Shimer (2005)) or 0.95 (as in Hagedorn and Manovskii (2008)), as well as to allowing for the replacement ratio to vary across states according to the weekly benefit amounts published by the United

[15]Let $Y = AK^\alpha L^{1-\alpha}$ be output, produced according to a Cobb-Douglas technology from capital K and labor L. Profits (or net operating surplus) are given by $\Pi = Y - rK - wL$, where r is the rental rate of capital and w is the wage rate. The marginal profits from an additional employee are $d\Pi/dL = (1-\alpha)\,Y/L - w$, where $dK/dL = 0$ by the envelope theorem if capital is chosen optimally by the firm. Profits per employee are $\Pi/L = Y/L - rK/L - w$. If capital markets are frictionless, then the rental rate equals the marginal product of capital, $r = \alpha Y/K$, so that $\Pi/L = (1-\alpha)\,Y/L - w = d\Pi/dL$.

States Department of Labor (2010). We assume $\mu = 0.6$ in our baseline results, again following Nagypal and Mortensen (2007), and explore robustness to setting $\mu = 0.5$ or $\mu = 0.7$, the lower and upper bound of the plausible range of estimates in Petrongolo and Pissarides (2001). We set the annual discount rate $r = 0.04$ and vacancy posting costs $-b^F_{it}/\pi_{it} = 0.03$, but these assumptions matter very little for the results.

2.3.2. Finding Rates

We calculate job finding rates of workers from Current Populations Survey as the number of workers whose status changes from unemployed to employed as a fraction of the total number of unemployed workers in a submarket.[16] Workers are attributed to the state where they live and the industry where they work. Unemployed workers, who do not work and therefore have no information about industry, are attributed the industry where they last held a job, following standard practice at the BLS. If a worker changes state or industry over the time she finds a job, she is attributed to the state or industry of origin.

To calculate worker finding rates of firms, we would need firm-level data, which are available from the Job Openings and Labor Turnover Survey (JOLTS), but only from the year 2000 onwards. To obtain data on worker finding rates for a longer sample period, we give up on testing equation (2.5) and impose this equation holds with $\alpha^{MT}_i = 0$ for all i. Then, we use this relation to construct data for worker finding rates of firms f^W_i from data on job finding rates of workers f^W_i. Although second-best, we prefer this solution over limiting the time period, mostly because a longer time series is important for studying mismatch over the business cycle, but also because heterogeneity in matching efficiency is arguably the least interesting of the four sources of labor market mismatch. Our choice is further supported by the evidence from the JOLTS reported in Sahin et al. (2014). Sahin et al. (2014) estimate submarket-specific matching efficiency by regressing matches on unemployment and vacancies. They find that while there is substantial variation in matching efficiency, this seems to not affect the amount of mismatch. If there is mismatch coming from heterogeneity in the matching technology, then this will not alter our estimates of the level and cyclical behavior of mismatch unemployment, but it will affect the decomposition of mismatch into its sources.[17]

[16] This is a common way to measure worker flows, see Shimer (2012). There are several reasons why the level of worker flows constructed in this way is biased, like measurement error (Abowd and Zellner (1985)) and time aggregation bias. Since we use only worker flows in deviations from the average across submarkets, these biases should not affect our results.

[17] The direction of the bias is not clear. If, for example, states with high job finding rates tend to have higher matching efficiency, $\alpha^{MT}_i > 0$, we would tend to underestimate the worker finding rate in those states, see equation (2.5). This would then bias our estimates of the job mobility costs, see equation (2.3). Whether we would over- or underestimate these costs would depend on whether states with high job finding rates tend to have higher or lower than average profits.

2.3.3. Match Surplus

We assumed that matches in submarket i are formed by combining an unemployed worker and a vacant job, both of which were searching in submarket i. If we further assume that when matches are destroyed, both worker and vacancy remain in submarket i, at least initially, then the surplus of match in submarket i must satisfy the following Bellman equation,

$$(1+r)\,S_{it} = y_{it} + E_t\left[(1-\tau_{it+1})\,S_{it+1}\right] \tag{2.8}$$

where S_{it} may be worker or firm surplus, y_{it} is the flow payoff from the match (to worker or firm) and τ_{it} is turnover in submarket i.

We observe match payoffs y_{it} and turnover τ_{it} in our dataset. For the worker, payoffs y_{it}^W equal wages minus unemployment benefits and the disutility from working, and turnover equals the separation rate λ_{it} plus the job finding rate, $\tau_{it}^W = \lambda_{it} + f_{it}^W$. For the firm, payoffs from a filled job y_{it}^F equal profits gross of vacancy posting costs, and turnover equals the separation rate plus the worker finding rate, $\tau_{it}^F = \lambda_{it} + f_{it}^F$. We use these data and equation (2.8) to calculate match surplus for the worker and firm, S_{it}^W and S_{it}^F respectively. In the context of the standard search and matching model, it is straightforward to derive equation (2.8) from the Bellman equations for workers and firms, see appendix 2.7.3.

For our exercise, what matters is the dispersion in surplus across submarkets of the labor markets. Dispersion in surplus is sensitive to the persistence in payoffs and turnover. The persistence of payoffs matters because match surplus equals the expected net present value of all future payoffs from the match. If payoffs are very persistent, then current payoff differentials will persist into the future, thus generating more dispersion in the expected net present value. Persistence in turnover matters because it determines to what extent turnover is segment-specific. Segment-specific turnover introduces a negative correlation between surplus and turnover across segments, pushing towards the correlation expected in the WM and JM curves.

We assume payoffs and turnover follow autoregressive process that reverts to the average across all submarkets.

$$y_{it+1} = (1-\delta_y)\,y_{it} + \delta_y \bar{y}_t + \varepsilon_{y,it+1} \Rightarrow E_t y_{it+s} = \bar{y}_t + (1-\delta_y)^s\,(y_{it} - \bar{y}_t) \tag{2.9}$$

$$\tau_{it+1} = (1-\delta_\tau)\,\tau_{it} + \delta_\tau \bar{\tau}_t + \varepsilon_{\tau,it+1} \Rightarrow E_t \tau_{it+s} = \bar{\tau}_t + (1-\delta_\tau)^s\,(\tau_{it} - \bar{\tau}_t) \tag{2.10}$$

By varying the parameters δ_y and δ_τ, we explore the robustness of our results to the amount of persistence in match payoffs and turnover. In the baseline results, we assume the processes for payoffs y_{it} and turnover τ_{it} are independent.

The first-order autocorrelation in wages is 0.92 per year in the state-level data and 0.84 in the industry-level data. This is consistent with Blanchard and Katz (1992), who find an autocorrelation of 0.94 across U.S. states, and Alvarez and Shimer (2011), who find 0.90 for 75 industries at the 3-digit level of disaggregation

(CES data, 1990-2008), and conclude that wages are nearly a random walk. Autocorrelation in profits is lower: 0.58 in the state-level data and 0.54 in the industry-level data. In our baseline results, we assume wages and profits are a random walk, $\delta_y = 0$, but our results are robust to a higher degree of mean-reversion.[18] The first-order autocorrelation in turnover is 0.68 per year in the state level data and 0.59 in the industry-level data based on the NAICS classification. Although turnover seems to be further from a random walk than payoffs, we still use the random walk assumption as our baseline. However, in Section 2.4.4 we explore the robustness of our results to higher degrees of mean-reversion.

Using stochastic processes (2.9) and (2.10), we can solve equation (2.8) recursively to obtain match surplus. For convenience, we approximate around turnover being a random walk so that we can obtain an explicit expression for the solution, see appendix 2.7.3 for the derivation. The approximation will be good for relatively small deviations of δ_τ from our baseline value of zero.

$$S_{it} \simeq \frac{(r+\tau_{it})(r+\tau_{it}+\delta_\tau)}{(r+\tau_{it})(r+\tau_{it}+\delta_\tau)+\delta_\tau(1+r+\tau_{it})(\bar{\tau}_t-\tau_{it})} \left(\frac{\bar{y}_t}{r+\tau_{it}} + \frac{y_{it}-\bar{y}_t}{r+\tau_{it}+\delta_y} \right) \tag{2.11}$$

If match payoffs follow a random walk, $\delta_y = 0$, and turnover is constant, $\delta_\tau = 0$, as in our baseline, then match surplus is the annuity value of the current payoff, $S_{it} = \frac{y_{it}}{r+\tau_{it}}$, evaluated at an effective discount rate which includes not only the rate of time preference, but also the turnover rate. The higher the wage in a submarket, the higher is the surplus of having a job in that submarket. The more likely it is to lose that job in the future – that is, the higher is λ_{it} and therefore τ_{it} – the lower is the surplus. Also, the easier it is for an unemployed person in this market to find a job – the higher f_{it}^W and therefore τ_{it} – the smaller is the advantage of already having a job.

Some assumptions were needed to derive expression (2.11) for match surplus in addition to the ones discussed above. Implicit in Bellman equation (2.8) are the assumptions that workers cannot vary their search effort and that they cannot search while holding a job. Implicit in the stochastic processes for match payoffs (2.9) and turnover (2.10) is the assumption that these processes are independent. In particular, we may be concerned that turnover depends on payoffs because of endogenous match destruction. We will explore the robustness of our results if this is the case, see Section 2.4.4. Finally, in the calculation of match payoffs themselves, we assume the replacement ratio is constant across segments, implicitly assuming that unemployment benefits and/or the value of leisure depend positively on wages. We will explore the robustness of our results to this assumption as well.

[18] Strictly speaking, what matters is not the persistence in average wages and profits, but the persistence of wages and profits of a given match. However, as shown by Haefke et al. (2013) and Kudlyak (2010), wages paid out over the duration of a match are more persistent than average wages, so if anything these estimates understate the autocorrelation in wages. The reason that the persistence of payoffs does not affect the results very much, is that mean-reversion enters additively with turnover, see equation (2.11), which is close to 1 at annual frequency.

2.3.4. Heterogeneity

We estimate mismatch unemployment from the dispersion in wages, profits and finding rates. Heterogeneity is a concern, because it may generate dispersion that is unrelated to mismatch. Our benchmark conditions were derived assuming all workers and jobs are the same. In reality, wages, profits, and even job finding rates may vary across workers not only because of deviations from these conditions, but also because workers have different education, experience or other characteristics. If we do not control for these differences, we may spuriously attribute the dispersion they generate as mismatch.

In our baseline results, we do not control for heterogeneity. There are three reasons for this. First, we will find that the data show remarkably small deviations from our benchmark worker and job mobility curves. Since worker and firm heterogeneity would tend to generate deviations from these conditions, we interpret this as evidence that heterogeneity seems to largely 'average out' between states and industries. Second, there is a price to pay for controlling for heterogeneity: we can no longer estimate the overall level of mismatch unemployment. However, we do check the robustness of our results about the cyclicality and the sources of mismatch. If anything, these results become stronger when we control for worker and job heterogeneity, which is the third reason why we feel comfortable ignoring heterogeneity in the baseline. Results controlling for heterogeneous workers and firms are reported in Section 2.4.4 along with a number of other robustness checks. However, because of the importance of this particular robustness check, we describe it here, before turning to the results.

Differences across workers are to a large degree observable. Our approach to deal with this type of heterogeneity is to calculate surplus and finding rates for homogeneous groups of workers and then to average the values we get for $\hat{S}_i^W$, $\hat{S}_i^F$, $\hat{f}_i^W$ and $\hat{f}_i^F$ over all groups of workers. We use 40 groups of homogeneous workers based on all observable worker characteristics in our dataset: education, experience, gender, race and marital status, see appendix 2.7.4 for details. Our results change very little if we do this. However, as one may still be concerned about unobservable differences across workers and – more importantly – across firms, we pursue a second approach of controlling for heterogeneity as well.

There are other differences between jobs than just the wage. In particular, residual wage differentials have been interpreted as compensating differentials: non-monetary job amenities like flexible hours or safe working conditions, in return for which workers are willing to accept lower wages, see Rosen (1979) and Roback (1982).[19] These differences are completely unobservable in our dataset. Therefore, as our second approach to deal with heterogeneity, we assume compensating differences are constant over time and remove the time-series average of the values for $\hat{S}_i^W$, $\hat{S}_i^F$, $\hat{f}_i^W$ and $\hat{f}_i^F$ in each year. Details on this procedure, which is similar in

[19] One of these compensating differentials is explicitly taken into account in our calculations, which is the separation probability. However, this is only one of many unobservable differences between jobs.

spirit to a fixed-effects regression, are in appendix 2.7.4. The advantage of this approach is that it controls for all time-invariant heterogeneity, observable as well as unobservable and across workers as well as across firms. The disadvantage is that we can no longer estimate the size of the deviations from our benchmark conditions, but only their relative size compared to the time-series averages. As a result, equation (2.7) no longer gives the correct level of the unemployment rate that is due to mismatch. We do show, however, that our results regarding the cyclicality and decomposition of mismatch are not only robust to controlling for heterogeneity, but in fact look even stronger than the baseline results.

2.4. Results

We start the description of our results by exploring how well our benchmark conditions (2.2), (2.3) and (2.4) hold in the data. Then, we present our estimates for mismatch unemployment that results from deviations of these conditions in Section 2.4.2 and explore its behavior over the business cycle in Section 2.4.2. Finally, in Section 2.4.3, we present the results of our mismatch accounting exercise decomposing mismatch unemployment into the contribution of each of its three sources.

2.4.1. Benchmark Relations

Figure 2.2 shows scatterplots for states around the worker mobility, job mobility and wage determination curves. These graphs are for 2000, but look similar for other years. Deviations across states from worker mobility condition (2.2) and job mobility condition (2.3) are small and non-systematic. On the other hand, there are large and systematic deviations from the benchmark wage determination curve (2.4).

These graphs suggest that mobility of workers and jobs across states seems to be sufficient to arbitrage away most differences in the values of being unemployed and having a vacancy across states, a finding that we will confirm in the accounting exercise in Section 2.4.3. Mismatch is primarily due to variation across states in the share of match surplus that is attributed to workers versus firms. If workers and firms were to share surplus in fixed proportions, as in benchmark wage determination condition (2.4), then states that are attractive to firms are attractive to workers as well. If total match surplus varies across states, for example because labor productivity is different in different states, this maps out the benchmark wage determination condition. In reality, it seems that differences in wages across states are much larger than differences in labor productivity. Since states with high wages generate high surplus for workers but low surplus for firms, this generates mismatch as firms with vacancies and unemployed workers move away from each other.

Figure 2.3 shows similar results for the benchmark conditions across industries.

The worker mobility plot looks qualitatively similar to that for states, although the dispersion around the curves is larger. Barriers to job mobility seem to play a role in mismatch across industries, unlike for states, but deviations from the benchmark wage determination curve are large and systematic as well. In Section 2.4.3 we will show that the importance of barriers to job mobility depends on the time period, but throughout the sample variation in the surplus share of workers is an important source of mismatch across industries as well, although less important than for mismatch across states.

The patterns in the data that we reveal are surprising to many, possibly because most of the debate about labour market mismatch has focused on worker mobility frictions, see e.g. Kocherlakota (2010), Frey (2009), Katz (2010), Kaplan and Schulhofer-Wohl (2010) and Sahin et al. (2014). Moreover, the evidence that restrictions to worker mobility seem to not contribute at all to mismatch is very striking and the correlation in the scatter plots looks almost 'too good to be true.' One might think, therefore, that there is something in our treatment of the data that spuriously generates these patterns or that we make convenient assumptions that make the results look stronger than they really are. We will try to convince the reader that this is not the case with an extensive robustness analysis, discussed in Section 2.4.4. First, however, we complete the description of the results by exploring how important mismatch is as a source of unemployment, and by formalizing the finding that mismatch is primarily driven by deviations of wage determination from the benchmark condition, both in terms of the average level of mismatch and for fluctuations in mismatch over time.

2.4.2. Mismatch Unemployment

Figure 2.4 plots the unemployment rate that is due to mismatch across states over the 1979-2009 period. Figure 2.5 shows a similar graph for mismatch across industries. These counterfactual unemployment rates were constructed using the observed dispersion in job finding rates as explained in Section 2.2.2. For comparison, the graphs also show the actual unemployment rate over the same period, although on a different scale on the right-hand side axis of the graphs.[20]

We will use the series in these graphs to address the questions how large is the impact of labor market mismatch on unemployment and how does it fluctuate over the business cycle. Estimating the impact of mismatch on unemployment is complicated by the fact that the level of disaggration matters. We discuss this issue in Section 2.4.2 below. However, it is worth noting that the similarity in the fluctuations in mismatch and overall unemployment are striking. We return to this in section 2.4.2 where we discuss the cyclicality of mismatch.

[20]In this graph, as well as in all other graphs in the paper, the 'overall' or 'total' unemployment rate is the steady state unemployment rate corresponding to the average finding and separation rates across states or industries. This steady state unemployment rate, which is comparable to our estimates for structural unemployment, is very close to the actual unemployment rate.

Level of Mismatch Unemployment

Our measure of the contribution of labor market mismatch to unemployment is simply the ratio of the average mismatch unemployment over the average actual unemployment rate over our full sample period. In Figure 2.4, unemployment due to mismatch across states averages around 0.1%-points compared to an average unemployment rate of around 5%. Mismatch across states contributes 2.3% to the overall unemployment rates according to these estimates. The estimates in Figure 2.5 show that mismatch across across industries contributes around 2.1% to unemployment. Taken at face value, the contribution of mismatch to unemployment seems very small. However, clearly the level of disaggregation matters for the observed amount of mismatch. Since there is likely to be substantial mismatch within states and within industries, we underestimate the contribution of mismatch to unemployment.

We try to address the aggregation issue in two ways. First, we disaggregate further. For the purposes of this subsection only, we use data that are disaggregated by both state and industry. Instead of 50 states or 33 industries, this gives us $50 * 33 = 1650$ labor market segments. Although 1650 submarkets is probably a more realistic segmentation of the U.S. labor market, it is in all likelihood still to coarse. Therefore, the second part of our solution is to find a correction factor that relates the observed amount of mismatch in our data to the amount of mismatch we would observe if we were to disaggregate to the right level.

An ideal labor market segment would consist of very similar jobs within a geographic area that allows workers to commute to these jobs without moving house. Using UK data, Barnichon and Figura (2011a) estimate the correct level of disaggregation would be to use 232 so-called travel-to-work areas and 353 detailed occupational groups. They then aggregate these data to a level that is comparable to U.S. states and major occupational categories and find that the observed amount of mismatch decreases by a factor 6. Thus, we will correct the observed amount of mismatch unemployment in the data that are disaggregated by both states and industries by multiplying our estimates with 6. Appendix 2.7.5 describes the details of this correction.

Disaggregation by both states and industries, while alleviating the aggregation problem, gives rise to a different bias because of sampling error. Barnichon and Figura (2011a) use a very large dataset consisting of the universe of job seekers in the UK. The U.S. data, however, are survey-based and in our dataset we have only about $23,000$ unemployed workers per year, which means that the 1650 labor market segments on average contain only 14 observations and because not all states and industries are equally large, some cells are even much smaller than that. As a result, our estimates for the job finding rate in each segment will be very imprecise. This sampling error will translate into dispersion across segments and bias our estimate for the amount of mismatch unemployment. We address this issue by estimating the variance of the sampling error in each segment and correcting the estimated variance of the job finding rates by subtracting the average variance of the sampling error, see appendix 2.7.5 for more details.[21]

Table 2.4 reports estimates for the contribution of mismatch to the unemployment rate corrected for sampling error and aggregation bias. Mismatch across state*industry segments contributes 15% to unemployment, substantially more than mismatch across states or industries only. The bias because of sampling error is fairly small, bringing the contribution of mismatch down to 14%, indicating the dispersion in job finding rates across segments is large compared to the sampling error. After correcting for aggregation, these estimates suggest that mismatch is responsible for 84% of unemployment. It is important to note that a good amount of guesswork was needed for the aggregation correction and the estimate is therefore rather imprecise. Nevertheless, these estimates indicate that it is at least a possibility that mismatch is an important contributor to unemployment and that potentially even all of unemployment may be due to mismatch.[22]

Our estimates are, roughly, in line with Sahin et al. (2014), who –using very different data from us– find that geographic mismatch is very small, but industry-level mismatch (at the two-digit level) explains around 14% of the increase in unemployment in the Great Recession. Although they do not report this in the text, the estimates in their Figure 3 imply a similar contribution of mismatch to the level of unemployment. Consistent with our argument that aggregation importantly biases the estimate of the contribution of mismatch, Sahin et al. (2014) also find that when they disaggregate further, to three-digit occupation level, the contribution of mismatch increases to 29%. However, we emphasize that our estimates of the contribution of mismatch to the level of unemployment are very rough and the estimates in Sahin et al. (2014) are the more credible ones. The contribution of the current study is in the estimates of the cyclicality of mismatch and its sources, to which we now turn.

Cyclicality of Mismatch Unemployment

Figures 2.4 and 2.5 clearly show that the cyclical fluctuations in mismatch unemployment are very similar to those of the overall unemployment rate. Mismatch unemployment closely follows the business cycles in the overall unemployment rate. Mismatch rises in the 1982, 1991, 2001 and 2008 recessions, declining slowly

[21]Workers in each segment find a job with probability f_i^W. The variance of the realization of this Bernoulli process equals $f_i^W \left(1 - f_i^W\right)$, so that the variance of the observed mean probability is equal to $f_i^W \left(1 - f_i^W\right)/N_i$, where N_i is the number of observations in segment i. The variance of the signal in f_i^W across segments, by the ANOVA formula, is then given by the observed variance $var\left(f_i^W\right)$ minus the average variance of the sampling error $E\left[f_i^W \left(1 - f_i^W\right)/N_i\right]$. We do not use segments with less than 5 observations because these would contribute more noise than signal.

[22]Note that Table 2.4 also provides implicit correction factors for estimates based on data disaggregated by states or industries only. If we assume the estimated 84% contribution of mismatch to unemployment from the state*industry disaggregation is correct, then we need to multiply estimated mismatch from state-level or industry-level data roughly by a factor of 40. We will use these implied correction factors occasionally in the remainder of the paper to get a feeling for the magnitudes of the estimates, but none of our results beyond the ones in this subsection will rely on this correction.

during the recovery as does the unemployment rate. The relative amplitude of these fluctuations is very similar to those in the total unemployment rate. There is no evidence that mismatch unemployment is less cyclical or more persistent than the overall unemployment rate or that the increase in unemployment in the Great Recession was more than in other recessions due to mismatch.

The volatility of the two series is not directly comparable, unless we correct for aggregation bias discussed above in Section 2.4.2. To obtain a summary statistic for the importance of mismatch to the overall unemployment rate, we regress mismatch unemployment on a constant and the overall unemployment rate in deviation from its average.

$$u_t^{MM} = \beta_0 \bar{u} + \beta_1 \left(u_t - \bar{u} \right) \tag{2.12}$$

The intercept in this regression –after correcting for aggregation bias– measures the contribution of mismatch to the average level of unemployment, which we reported in Section 2.4.2, whereas the slope coefficient measures the contribution of mismatch to fluctuations in unemployment.[23]

Our estimates for the contribution of mismatch to fluctuations in unemployment are somewhat similar to our estimates for the contribution to the level of unemployment: 3.4% for mismatch across states (cf. 2.3% of the level) and 1.2% for mismatch across industries (cf. 2.1% of the level). After a rough correction for aggregation bias, as explained in Section 2.4.2 above, these estimates imply that mismatch may be responsible for a large part to all of fluctuations in unemployment (precisely, the estimates range from 48 to 136%, but as mentioned before should be expected to be very imprecise).

These results suggest that unemployment may to a large extent be due to labor market mismatch. It is important to note that there is nothing in our estimation procedure that would introduce a comovement of mismatch unemployment with the overall unemployment rate by construction. In fact, all our estimates are relative to the cross-sectional mean in each year, so we explicitly remove any aggregate fluctuations from our data. The fact that we find such strong comovement therefore seems to suggest that we may think of mismatch as a micro-foundation for 'search frictions' in the tradition of Diamond (1982), Mortensen (1982) and Pissarides (1985).[24]

2.4.3. Sources of Mismatch

We now turn to the part of our results that is arguably the most interesting: the decomposition of mismatch unemployment into the sources of the mismatch.

[23] The contribution to fluctuations $\beta_1 = corr\left(u_t^{MM}, u_t\right) sd\left(u_t^{MM}\right) / sd\left(u_t\right)$ depends not only on the correlation, but also on the relative standard deviation of the two series, which is why the same correction for aggregation bias is appropriate.

[24] Pissarides (2000) describes search frictions as "a modeling device that captures the implications of the costly trading process without the need to make the heterogeneity and the other features that give rise to it explicit" (p.4). Our mismatch accounting framework makes the underlying heterogeneity explicit and allows us to explore the causes of this heterogeneity.

From Section 2.4.1 we know that benchmark conditions for worker mobility and job mobility approximately hold in the data, whereas there are large and systematic deviations from the benchmark condition for wage determination. This suggests that most mismatch is driven by wage setting. Here, we formalize that conclusion.

Figures 2.6 and 2.7 show the results of our mismatch accounting exercise described in Section 2.2.3. The figures show the evolution over time of mismatch unemployment as well as its three sources, for mismatch across states and industries, respectively.

Mismatch unemployment due to wage bargaining costs alone closely tracks total unemployment due to mismatch across states, see Figure 2.6, reflecting the fact that variation in the share of match surplus that is captured by wage earners is the most important impediment to equalization of job finding rates across states. The contribution of deviations from free mobility of workers and jobs is very small and largely acyclical. Removing any frictions to geographic worker or job mobility, while leaving existing wage determination mechanisms in place, would reduce unemployment very little and might even increase it.[25] For mismatch across industries the picture is slightly more complicated, see Figure 2.7. The contribution of worker mobility frictions is again very small, but the contribution of barriers to job mobility seems to increase over the sample. Wage determination is the most important source of mismatch in the first half of the sample, but its importance declines since the 1990s and becomes particularly small or even negative in the Great Recession.

To summarize the contribution of each source of mismatch to the unemployment rate, we regress unemployment due to each source on the total unemployment rate due to mismatch (in deviation from its mean).

$$u_t^{XX} = \beta_0^{XX} \bar{u}^{MM} + \beta_1^{XX} \left(u_t^{MM} - \bar{u}^{MM} \right) \tag{2.13}$$

where XX stands for the source of mismatch, i.e. $XX \in \{WM, JM, WD\}$. The intercept in this regression measures the contribution of each of the frictions to the average level of mismatch unemployment, so that $\beta_0^{XX} = \bar{u}^{XX}/\bar{u}^{MM}$, whereas the slope coefficient measures the contribution of mismatch to fluctuations

[25] How can the contribution of barriers to worker mobility to unemployment be negative? The answer is related to the correlations between the deviations from the worker mobility curve (2.2), the job mobility curve (2.3) and the wage determination curve (2.4). States with high worker surplus and low job surplus because of relatively high worker bargaining power, i.e. states with high α_i^{WD}, tend to attract unemployed workers and loose jobs, resulting in a lower than average job finding rate and higher than average worker finding rate in that state, everything else equal. However, the same states tend to have low α_i^{WM} and α_i^{JM}, meaning frictions to worker and job mobility costs tend to keep more unemployed workers and vacancies in the state than we would expect based on worker and job surplus there. The barriers to worker mobility reduce job finding rates, reinforcing the effect of the high wage, but the barriers to job mobility costs reduce worker finding rates as well, partially offsetting the effect. These conclusions are interesting in terms of their policy implication. The effects on the unemployment rate of a policy that reduces worker mobility costs, for example relocation or retraining subsidies to unemployed workers, are likely to be small and may even be negative.

in unemployment. The slope coefficient captures both the degree of correlation of unemployment due to a particular source of mismatch with the total mismatch unemployment rate and the size of fluctuations in mismatch due to that source, i.e. $\beta_1^{XX} = corr\left(u_t^{XX}, u_t^{MM}\right) sd\left(u_t^{XX}\right) / sd\left(u_t^{MM}\right)$. Note that because $u_t^{WM} + u_t^{JM} + u_t^{WD} = u_t^{MM}$, the contributions of the three sources to the total add up to one, i.e. $\beta_0^{WM} + \beta_0^{JM} + \beta_0^{WD} = 1$ and $\beta_1^{WM} + \beta_1^{JM} + \beta_1^{WD} = 1$, so that this is a true decomposition.

Frictions to worker mobility contribute 6% to the level of and 15% to the fluctuations in mismatch across states and -10% to the level and -2% to the fluctuations of mismatch across industries. Barriers to job mobility account for none of the mismatch across states (0% of the level and 1% of the fluctuations), but for a substantial part of the level of mismatch across industries (48%) and all of the fluctuations (113%), although –as already pointed out– the summary statistics hide a clear change over time in the importance of this type of frictions to mismatch unemployment. As a result, variation in the share of match surplus that is paid out to workers in the form of wages accounts for almost all of the level and fluctuations in mismatch across states (93% and 83%, respectively), a good share of the level of mismatch across industries (64%), but none of the fluctuations in industry-level mismatch (-11%).

2.4.4. Robustness

A number of assumptions were necessary to construct the data needed for our analysis. In this subsection we explore the robustness of our results to these assumptions. We summarize the results in terms of the contribution of mismatch to the level and fluctuations of the overall unemployment rate, as explained in Sections 2.4.2 and 2.4.2, and the contribution of barriers to worker mobility, barriers to job mobility and deviations from the benchmark wage determination equation to labor market mismatch, as described in Section 2.4.3. These summary statistics are presented for a number of robustness checks in Table 2.5. The first line in the top and bottom panels of this table shows our baseline estimates for state-level and industry-level data respectively.

For the construction of job filling rates from job finding rates, we made the assumption that the matching technology is well described by a Cobb-Douglas matching function with an elasticity of unemployment μ of 0.6, see Sections 2.2.1 and 2.3.2. The second and third line in the table shows the effect of assuming an elasticity of 0.5 or 0.7. A higher (lower) elasticity increases (decreases) the concavity of the aggregate job finding rate in the segment-specific job finding rates, see equation (2.7), and therefore increases (decreases) the estimated contribution of mismatch to unemployment. This effect is fairly strong, but for the (commonly accepted) range of values for mismatch unemployment considered, the result that mismatch is an important contributor to unemployment does not change qualitatively. A higher elasticity also increases the dispersion in job filling rates given the same job finding rates and therefore attributes more of a role to wage determination and less to job mobility frictions as a source of mismatch. This effect is small, however.

For the construction of match surpluses, we made a number of choices, see Section 2.3.3, among which the assumption that the replacement ratio equals 0.73 and the assumption that match payoffs (wages or profits) follow a random walk and match turnover is constant. Rows 3 through 8 explore the robustness of our results to these assumptions. Since none of these assumptions affect the observed dispersion in job finding rates, the estimates of the contribution of mismatch to unemployment are not affected at all. The composition of mismatch into its sources is affected, but the effects are small. The only exception is mean-reversion in match turnover, which generates a larger role for worker and job mobility frictions. We cannot rule out, therefore, that these frictions are more important than our baseline estimates suggest. Even with mean reversion of 60% per year, however, deviations from the benchmark wage determination curve are a very important source of mismatch across states, whereas the finding that job mobility frictions are the most important source of mismatch across industries is actually strengthened with respect to the baseline estimates.

Finally, we explore the effect of heterogeneity, as described in detail in Section 2.3.4. Controlling for observed worker heterogeneity affects the results remarkably little. If anything, controlling for this type of heterogeneity makes mismatch across industries look more important. When we control for unobserved heterogeneity by removing the time series mean from all our data series, akin to controlling for fixed effects in a regression, the importance of mismatch for unemployment seems to fall. This, however, is by construction and should not be misinterpreted: by removing the average dispersion across states and industries we are removing part of the mismatch from the data. The results of the mismatch accounting exercise are largely (and surprisingly) robust to removing all time-invariant unobserved heterogeneity from the data. The only thing that changes when we control for observed worker heterogeneity or for all time-invariant heterogeneity, is that we find a larger role for deviations from the benchmark wage determination curve for mismatch across industries, lending further relevance to our earlier caveat that the overriding importance of barriers to job mobility for this type of mismatch is hiding the fact that wage determination is important as well, especially in the first part of the sample.

In order to better understand what drives the variation in job finding rates and worker surplus (wages) that maps out the worker mobility curve and leads us to conclude that barriers to worker mobility are not an important source of mismatch, Figures 2.8 and 2.9 plot the evolution over time of these variables in five states and industries respectively. These states and industries were chosen to be somewhat representative, while making the graphs easy to read. States travel over the entire worker mobility curve, indicating that this curve largely mapped out by variation in job finding rates and wages within states over time. For industries the pictures is not quite as clear. Over time, the within-industry variation maps out a section of the worker mobility curve, but these movements are relatively small compared to the variation across industries, suggesting that persistent difference across industries also contribute to the dispersion in job finding rates (while largely respecting the worker mobility benchmark relation). This explains why controlling for unobserved heterogeneity ('fixed effects') affects the estimates for industry-level mismatch more than it affects the estimates for mismatch across states.

2.5. Conclusions

Mismatch unemployment is unemployment due to dispersion in job finding rates across submarkets of the labor market, which results in mismatch in the distribution of vacancies and unemployed workers over submarkets. We proposed an accounting framework using two arbitrage equations and a benchmark wage determination equation that allows us to estimate mismatch unemployment and decompose it into its sources. Since this framework takes data on the values of unemployment and vacancies rather than their quantitities as inputs, available data allowed us to present estimates for the 1979-2009 period, much further back in time that previous studies, in particular Sahin et al. (2014). This paper is also the first to report on the causes of mismatch.

We find that mismatch is an important reason for unemployment, in line with earlier studies. The cyclical behavior of mismatch unemployment is very similar to that of the overall unemployment rate. This finding is driven by the fact that dispersion in labor market conditions across states and industries moves closely with the business cycle. The unemployment that derives from this dispersion is as cyclical as the overall unemployment rate and no more persistent. As a corollary, the nature of the increase in unemployment in the Great Recession is no different from previous recessions, although it is of course more severe.

The underlying frictions that cause mismatch to exist and persist are barriers to job mobility (across industries) and deviations from surplus sharing in equal proportions across industries and particularly across states. States with high wages tend to have low profits. This implies that states and industries that are attractive to workers are unattractive to firms and vice versa, generating dispersion in vacancy-unemployment ratios and mismatch unemployment. Little to no mismatch comes from worker mobility frictions. This finding is perhaps surprising in light of the debate on policies aimed at increasing worker mobility.

2.6. Tables and Figures

Table 2.1: State-level data, cell sizes 1979-2009

		job finding rate			wage		
		min	mean	max	min	mean	max
Alabama	AL	246	463	1050	5572	7405	10385
Arizona	AZ	204	361	615	5723	7194	10136
Arkansas	AR	205	390	573	5394	6955	9695
California	CA	1193	2288	4367	33428	46222	55085
Colorado	CO	186	448	929	6735	10056	13724
Connecticut	CT	176	391	1020	5125	8475	13579
Delaware	DE	188	316	603	3915	6839	9680
District of Columbia	DC	221	362	616	3031	6014	8470
Florida	FL	613	979	1878	17616	23416	28878
Georgia	GA	279	448	1034	6686	9797	12993
Hawaii	HI	166	294	532	4980	6792	9031
Idaho	ID	222	402	633	5929	7672	9340
Illinois	IL	602	1172	2117	17472	22925	26513
Indiana	IN	217	508	1125	7001	9360	13773
Iowa	IA	186	414	727	7269	9593	12266
Kansas	KS	234	359	539	7041	8412	10860
Kentucky	KY	231	446	806	6219	7505	10646
Louisiana	LA	219	409	828	4993	6408	9327
Maine	ME	212	405	763	5563	8023	12070
Maryland	MD	223	409	866	6225	9627	14703
Massachusetts	MA	350	717	1712	8758	16852	26276
Michigan	MI	541	1237	2477	12401	20266	26395
Minnesota	MN	193	488	1019	7525	10782	15800
Mississippi	MS	205	424	825	4585	6568	9960
Missouri	MO	209	467	936	6185	8852	11739
Montana	MT	229	394	600	5053	7570	9731
Nebraska	NE	158	301	464	6045	8830	10966
Nevada	NV	214	413	967	5925	8014	12561
New Hampshire	NH	175	336	709	5198	8183	14088
New Jersey	NJ	395	833	1701	12034	18992	26772
New Mexico	NM	200	372	583	4399	6622	9359
New York	NY	779	1479	2426	23758	33195	43087
North Carolina	NC	329	674	1134	10670	17115	28086
North Dakota	ND	215	324	483	7093	8385	10462
Ohio	OH	562	1163	2461	15462	22371	27942
Oklahoma	OK	191	341	648	5588	7574	10028
Oregon	OR	220	473	827	5980	7626	11589
Pennsylvania	PA	605	1128	2357	16717	23033	27018
Rhode Island	RI	207	415	985	4127	7269	11423
South Carolina	SC	190	390	660	5600	7197	8994
South Dakota	SD	198	322	468	7450	9320	10964
Tennessee	TN	206	415	860	6452	7630	9445
Texas	TX	629	1200	1802	24797	28094	31313
Utah	UT	191	359	634	6780	8127	12329
Vermont	VT	186	322	539	5121	7141	10211
Virginia	VA	218	421	819	8204	10689	13362
Washington	WA	264	497	874	6501	8557	10981
West Virginia	WV	270	476	1040	5267	6407	8814
Wisconsin	WI	242	509	970	8277	10397	13490
Wyoming	WY	191	311	501	5153	7110	9524

Notes: Entries in the table are the number of observations used to calculate the job finding rate and the average wage in a state-year cell.

Table 2.2: Industry-level data (SIC), cell sizes 1979-1997

		job finding rate			wage		
		min	mean	max	min	mean	max
Construction	CON	1792	3383	5301	26427	35743	42343
Lumber & wood prods, excl. furniture	LUM	202	373	689	2831	3985	5405
Furniture & fixtures	FUR	122	249	382	2188	2935	3526
Stone, clay, concrete, glass prods	MNR	134	258	460	1939	2978	4095
Primary metals	PMT	116	377	1071	2564	4180	7121
Fabricated metals	FMT	195	494	1021	4368	6495	10061
Machinery, ex electrical	MAC	249	647	1606	8025	12245	16888
Electrical machinery, equip supplies	ELC	196	542	1004	5850	9929	13868
Motor vehicles & equip	MVH	167	487	1150	4477	5552	6305
Other transportation equip	OVH	161	321	588	3057	5551	6788
Professional & photo equip, watches	PHO	123	197	301	2390	3284	3901
Misc mfg industries	MMA	189	280	460	2321	2927	3557
Food & kindred prods	FOO	289	676	1173	5759	8881	11817
Textile mill prods	TEX	153	282	532	2231	3626	4698
Apparel & other finished textile prods	APP	257	506	780	3555	5451	7740
Paper & allied prods	PAP	126	205	313	2379	3550	4578
Printing, publishing & allied inds	PUB	235	385	543	6343	8496	9520
Chemicals & allied prods	CHE	156	281	483	4564	6301	7940
Rubber & misc plastic prods	RUB	163	274	464	2730	3623	4255
Leather & leather prods	LEA	102	181	327	545	1113	2134
Transportation	TRA	641	1014	1705	18969	23240	25327
Communications	COM	180	243	326	5445	7701	9537
Wholesale trade	WHO	489	874	1413	16195	21548	24065
Retail trade	RET	2677	4753	7064	71748	92571	104841
Banking & other finance	FIN	283	424	580	12118	15675	17562
Business services	BSV	546	1181	1667	10913	19947	25720
Automobile & repair services	ASV	281	505	796	7332	9202	10389
Personal serv ex private hhs	PSV	474	836	1250	11642	16321	18720
Entertainment & recreation	ENT	430	582	783	5233	7476	9724
Health services	HEA	696	1116	1637	35425	44720	50893
Educational services	EDU	508	946	1481	36027	45360	54301
Social services	SOC	297	469	616	7694	10604	14509
Misc professional services	MSV	340	563	864	12861	20610	25762

Notes: Entries in the table are the number of observations used to calculate the job finding rate and the average wage in an industry-year cell. Industries are defined according to the 2-digit Standard Industrial Classification (SIC).

Table 2.3: Industry-level data (NAICS), cell sizes 1998-2009

		job finding rate			wage		
		min	mean	max	min	mean	max
Construction	CON	1651	2644	5491	33309	40992	46426
Nonmetallic mineral product manufacturing	MNR	119	166	269	2013	2160	2330
Primary metals and fabricated metal products	MET	251	363	673	6150	7448	8054
Machinery manufacturing	MAC	200	314	554	4869	6145	7784
Computer and electronic product manufacturing	CEM	170	294	524	3906	5172	6279
Electrical equipment, appliance manufacturing	ELC	135	220	486	1698	3519	5812
Transportation equipment manufacturing	VEH	260	400	882	7423	8366	8780
Wood products	LUM	146	197	304	1810	2480	2946
Furniture and fixtures manufacturing	FUR	132	189	340	1860	2470	2922
Miscellaneous and not specified manufacturing	MMA	206	290	507	4318	4842	5249
Food manufacturing, beverage and tobacco	FOO	272	389	523	7418	7670	8533
Textile, apparel, and leather manufacturing	TEX	205	308	402	2252	3761	5636
Paper and printing	PAP	180	272	388	3795	5814	7661
Chemical manufacturing	CHE	148	219	431	4564	4857	5573
Plastics and rubber products	RUB	143	198	288	1768	2839	3486
Wholesale trade	WHO	457	602	882	14396	18216	21820
Retail trade	RET	1909	2531	4094	58587	62809	67348
Transportation and warehousing	TRA	616	817	1392	22394	23884	26988
Publishing industries (except internet)	PUB	112	166	271	1968	2899	3811
Broadcasting and Telecommunications	COM	193	344	621	6729	7425	9733
Information and data processing services	INF	107	237	647	1509	4810	9828
Finance	FIN	297	488	897	14925	17440	18960
Professional and technical services	PSV	651	983	1813	27312	32028	35463
Administrative and support services	ASV	708	1341	2485	8745	16145	21859
Educational services	EDU	643	991	1580	42900	49378	53350
Hospitals	HOS	295	452	724	23158	26792	35410
Health care services, except hospitals	HEA	328	707	1336	18172	28243	35478
Social assistance	SOC	359	488	756	10284	12040	14479
Arts, entertainment, and recreation	ENT	554	656	950	10297	11475	14963
Accommodation	ACC	329	433	637	6188	7093	8229
Food services and drinking places	FSV	1317	1724	2677	25960	29458	31395
Other services (excl. government)	MSV	682	894	1360	23891	25986	27923

Notes: Entries in the table are the number of observations used to calculate the job finding rate and the average wage in an industry-year cell. Industries are defined according to the 2-digit North American Industrial Classification System (NAICS).

Table 2.4: Disaggregation and the Level of Mismatch

	N	Est MMU $\bar{u}^{MM}/\bar{u}$	Corr sampl error	Implied MMU (corr for agg)
States	50	2.3%	2.2%	
Industries	33*	2.1%	2.0%	
States*Industries	1650*	15.0%	14.0%	84%

Notes: Procedure to correct for sampling error and aggregation is explained in Section 2.4.2 and appendix 2.7.5.

*We use 33 broad industries for the SIC classification 1979-1997, and 32 for the NAICS classification 1998-2009. As a result, we have 1650 state*industry cells before until 1997 and 1600 cells from 1998 onwards.

Table 2.5: Robustness Analysis

	MMU		Sources of MMU					
	level	cycle	level			cycle		
Mismatch across states			WM	JM	WD	WM	JM	WD
Baseline	2.3	3.4	6	0	93	15	1	83
Elasticity matching function, $\mu = 0.5$	1.6	2.4	8	5	88	20	10	70
____, $\mu = 0.7$	3.4	5.1	5	−9	104	11	−13	102
Flow payoff unemployment, $b = 0.4$	2.3	3.4	6	0	93	15	1	83
____, $b = 0.9$	2.3	3.4	6	0	93	15	1	83
Mean-reversion payoffs, $\delta_y = 0.3$	2.3	3.4	7	2	90	16	5	79
____, $\delta_y = 0.5$	2.3	3.4	8	3	89	16	6	77
Mean-reversion turnover, $\delta_\tau = 0.3$	2.3	3.4	23	39	47	27	26	47
____, $\delta_\tau = 0.5$	2.3	3.4	29	38	33	31	33	36
Control for observed worker heterogeneity	2.0	3.1	10	1	89	16	5	79
Control for unobserved heterogeneity	1.4	2.0	12	7	81	20	18	62
Mismatch across industries			WM	JM	WD	WM	JM	WD
Baseline	2.1	1.2	−10	46	64	−2	113	−11
Elasticity matching function, $\mu = 0.5$	1.5	1.0	−13	57	56	1	103	−3
____, $\mu = 0.7$	3.0	1.6	−6	21	86	0	88	12
Flow payoff unemployment, $b = 0.4$	2.1	1.2	−10	46	64	−2	113	−11
____, $b = 0.9$	2.1	1.2	−10	46	64	−2	113	−11
Mean-reversion payoffs, $\delta_y = 0.3$	2.1	1.2	−2	36	67	6	89	5
____, $\delta_y = 0.5$	2.1	1.2	0	34	66	7	76	17
Mean-reversion turnover, $\delta_\tau = 0.3$	2.1	1.2	11	61	28	18	114	−32
____, $\delta_\tau = 0.5$	2.1	1.2	16	70	14	21	117	−38
Control for observed worker heterogeneity	3.6	3.2	−6	39	67	0	43	57
Control for unobserved heterogeneity	1.1	0.3	8	14	79	30	31	39

Notes: The contributions of mismatch to the level and cyclicality of unemployment is estimated using the following regression $u_t^{MM} = \beta_0 \bar{u} + \beta_1 (u_t - \bar{u})$ where $\beta_0 = \bar{u}^{MM}/\bar{u}$ measures the contribution to the level and $\beta_1 \left(= \Delta u^{MM}/\Delta u\right)$ the contribution to fluctuations in unemployment. Similarly, the contributions of the various sources to mismatch are estimated using $u_t^{XX} = \beta_0^{XX} \bar{u}^{MM} + \beta_1^{XX} \left(u_t^{MM} - \bar{u}^{MM}\right)$ where XX stands for the source, i.e. $XX \in \{WM, JM, WD\}$.

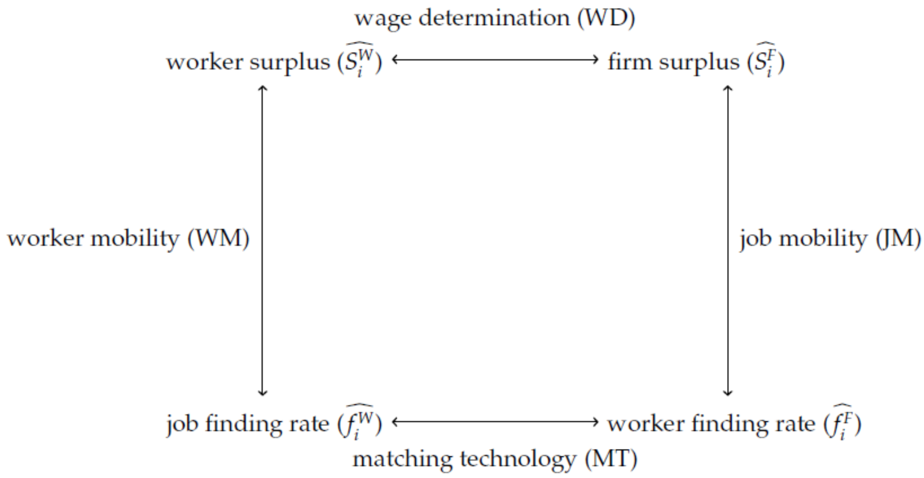

Figure 2.1: The sources of labor market mismatch.

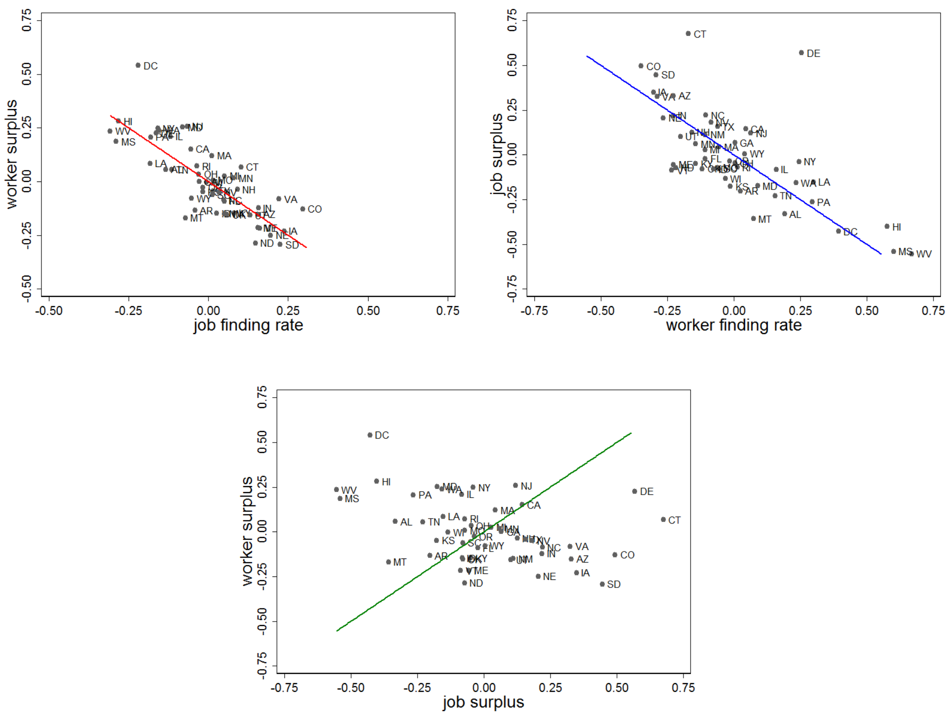

Figure 2.2: Worker mobility, job mobility and wage determination curves across states. Lines represent the benchmark relations corresponding to a labor market without any mismatch. Data for the year 2000 are shown.

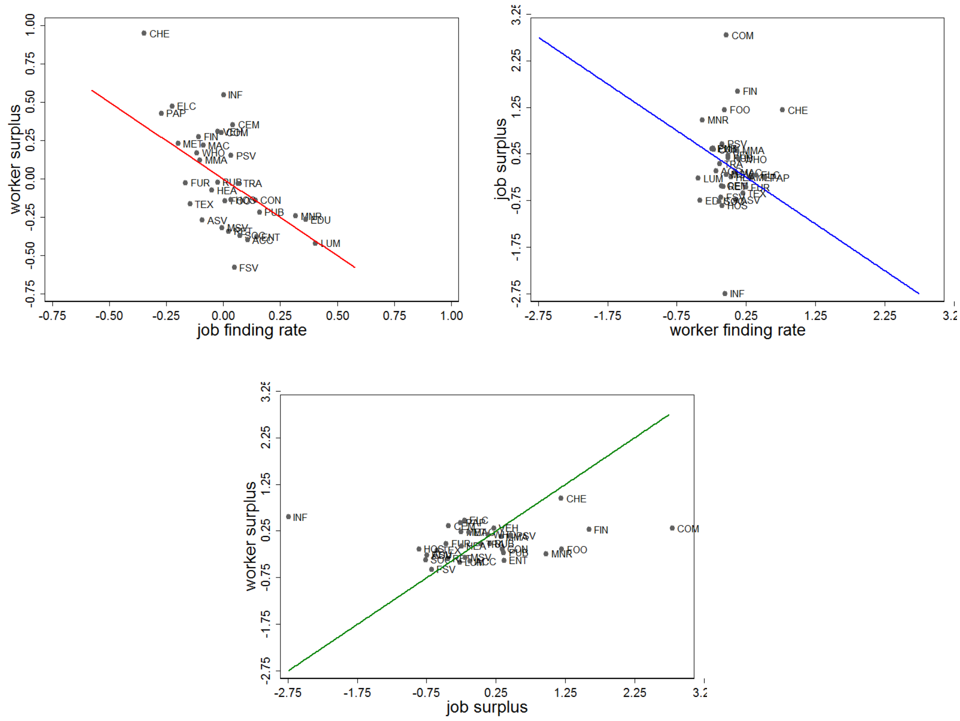

Figure 2.3: Worker mobility, job mobility and wage determination curves across industries. Lines represent the benchmark relations corresponding to a labor market without any mismatch. Data for the year 2000 are shown.

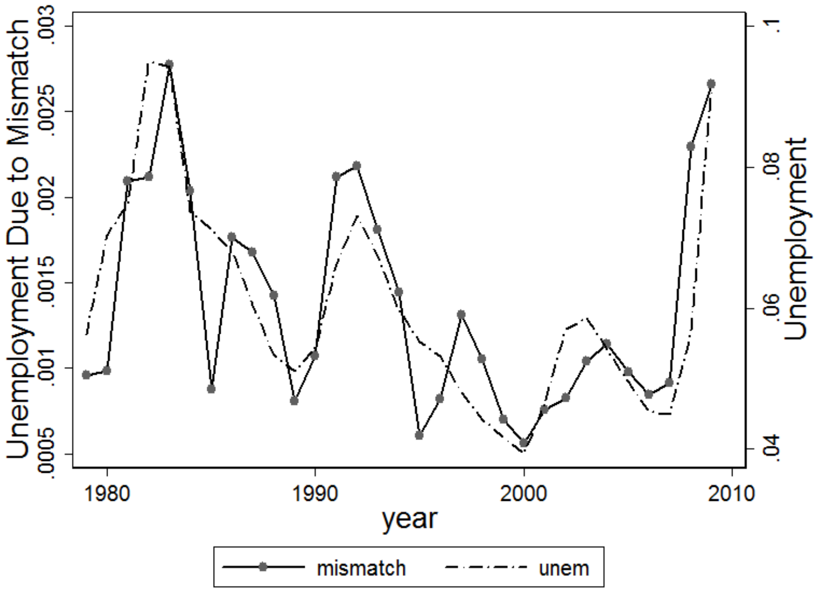

Figure 2.4: Cyclicality of mismatch unemployment across U.S. states. Unemployment due to mismatch across U.S. states, calculated as explained in Section 2.2.2. The dashed line shows the actual unemployment rate for comparison (right-hand side axis).

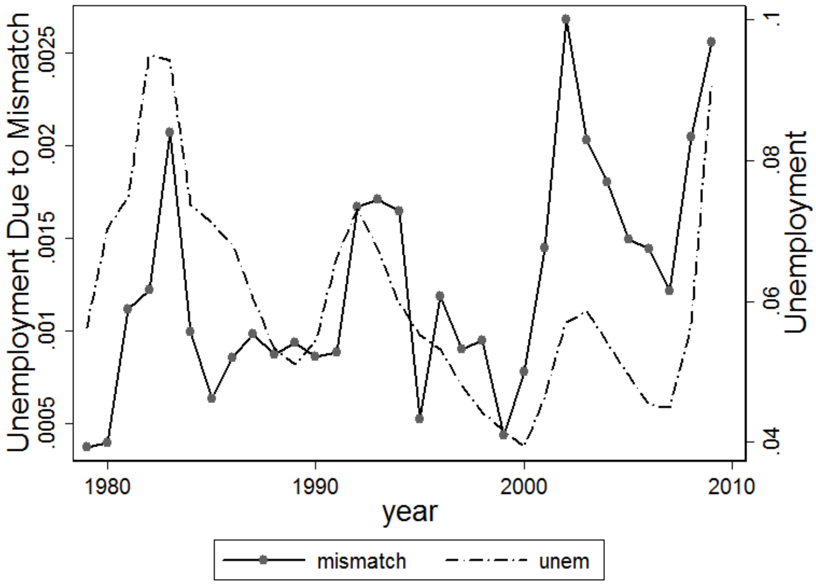

Figure 2.5: Cyclicality of mismatch unemployment across industries. Unemployment due to mismatch across U.S. states, calculated as explained in Section 2.2.2. The dashed line shows the actual unemployment rate for comparison (right-hand side axis). Unemployment due to mismatch across industries, calculated as explained in Section 2.2.2. The dashed line shows the actual unemployment rate for comparison (right-hand side axis).

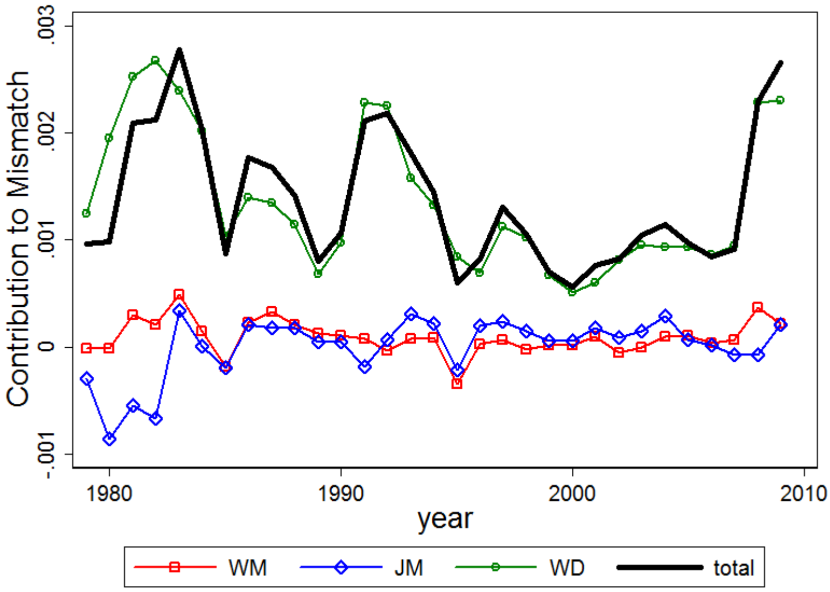

Figure 2.6: This figure documents the sources of labor market mismatch across U.S. states. The solid line is our baseline estimate for mismatch unemployment, calculated as explained in Section 2.2.2. The other lines show the contribution of worker mobility costs (WM), job mobility costs (JM) and wage bargaining costs (WB) to mismatch, see Section 2.2.3.

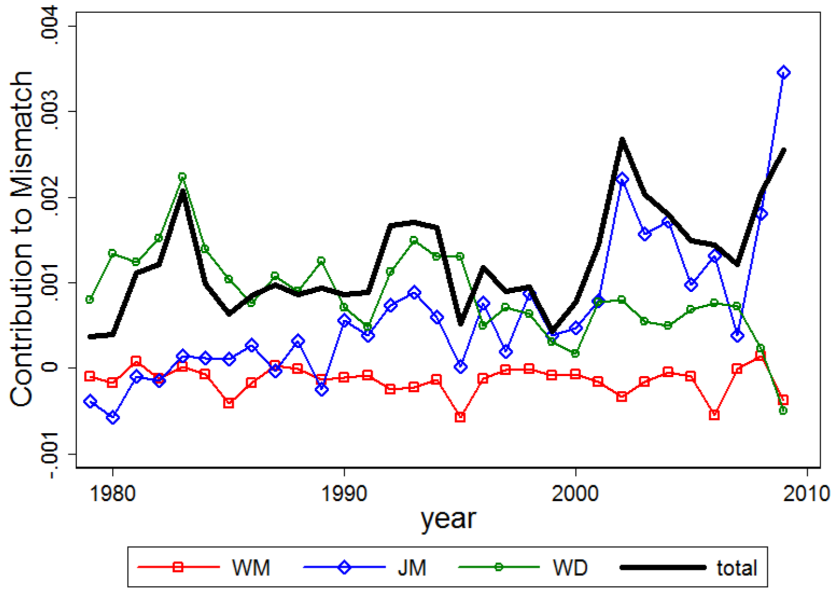

Figure 2.7: This figure documents the sources of labor market mismatch across industries. The solid line is our baseline estimate for mismatch unemployment, calculated as explained in Section 2.2.2. The other lines show the contribution of worker mobility costs (WM), job mobility costs (JM) and wage bargaining costs (WB) to mismatch, see Section 2.2.3.

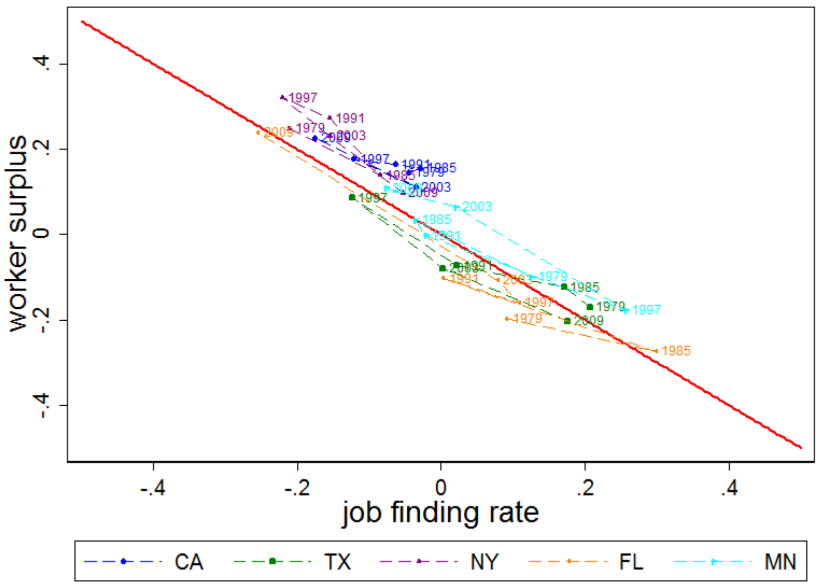

Figure 2.8: This figure maps out the worker mobility curve across states. The graph shows the evolution of the job finding rate f_i^W and worker surplus S_i^W over time in five states: California, Texas, New York, Florida and Minnesota. Since this graph is meant to be illustrative, these states were chosen partly based on size, but also partly based on making the graph easier to read. However, the evolution of these variables looks similar in other states.

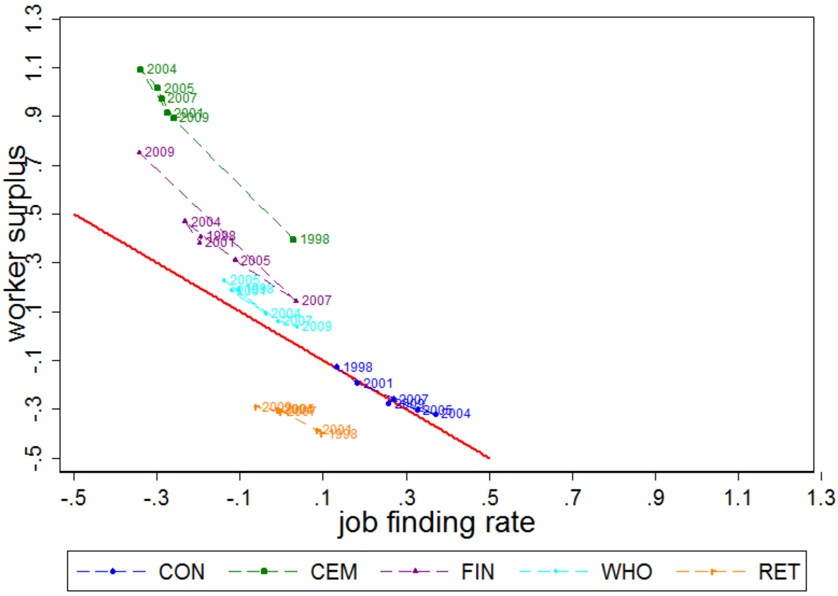

Figure 2.9: This figure maps out the worker mobility curve across industries. The graph shows the evolution of the job finding rate f_i^W and worker surplus S_i^W over time in five industries: construction, computer and electronics manufacturing, finance, wholesale trade and retail trade. Since this graph is meant to be illustrative, these industries were chosen because they are of particular interest for our story or particularly large (wholesale and retail trade). However, the evolution of these variables looks similar in other industries.

2.7. Appendices

2.7.1. Mismatch Unemployment, Derivation of Equation (2.7)

Since we are considering a mean-preserving change in the distribution of labor market tightness, we know that $\bar{\theta} = \bar{\theta}^{CF}$. Then, with $f_i^W = B\theta_i^{1-\mu} \Leftrightarrow \theta_i = \left(f_i^W/B\right)^{\frac{1}{1-\mu}}$, we get

$$\bar{\theta} = E\left[\left(\frac{f_i^W}{B}\right)^{\frac{1}{1-\mu}}\right] = E\left[\left(\frac{f_i^{W,CF}}{B}\right)^{\frac{1}{1-\mu}}\right] = \bar{\theta}^{CF} \tag{2.14}$$

The unemployment rate follows from the aggregate job finding rate by assuming steady state, i.e. $u = \frac{\lambda}{\lambda + \bar{f}^W}$, where $\lambda << \bar{f}^W$ is the aggregate separation rate, which implies $u^{CF}/u = \left(\bar{f}^W + \lambda\right)/\left(\bar{f}^{W,CF} + \lambda\right) \simeq \bar{f}^W/\bar{f}^{W,CF}$. Substituting $\hat{f}_i^W = \left(f_i^W - \bar{f}^W\right)/\bar{f}^W \Leftrightarrow f_i^W = \bar{f}^W\left(1 + \hat{f}_i^W\right)$ and re-arranging gives equation (2.7) in the main text.

To show that u^{CF}/u as in equation (2.7) is proportional to the ratio of the variances $\theta_i^{CF}/\bar{\theta}^{CF}$ and $\theta_i/\bar{\theta}$, take logs and assume $1 + \hat{f}_i^W$ is log-normally distributed to get

$$\begin{aligned}
\ln\left(\frac{u^{CF}}{u}\right) &\simeq (1-\mu)\left(\ln E\left[\left(1+\hat{f}_i^{W,CF}\right)^{\frac{1}{1-\mu}}\right] - \ln E\left[\left(1+\hat{f}_i^{W}\right)^{\frac{1}{1-\mu}}\right]\right) \\
&= (1-\mu)\left(E\left[\ln\left(1+\hat{f}_i^{W,CF}\right)^{\frac{1}{1-\mu}}\right] + \tfrac{1}{2}V\left[\ln\left(1+\hat{f}_i^{W,CF}\right)^{\frac{1}{1-\mu}}\right]\right. \\
&\left. - E\left[\ln\left(1+\hat{f}_i^{W}\right)^{\frac{1}{1-\mu}}\right] - \tfrac{1}{2}V\left[\ln\left(1+\hat{f}_i^{W}\right)^{\frac{1}{1-\mu}}\right]\right) \\
&= E\left[\ln\left(1+\hat{f}_i^{W,CF}\right)\right] + \tfrac{1}{2}\frac{1}{1-\mu}V\left[\ln\left(1+\hat{f}_i^{W,CF}\right)\right] \\
&- E\left[\ln\left(1+\hat{f}_i^{W}\right)\right] - \tfrac{1}{2}\frac{1}{1-\mu}V\left[\ln\left(1+\hat{f}_i^{W}\right)\right] \\
&= \tfrac{1}{2}\frac{1}{1-\mu}\left(V\left[\ln\left(1+\hat{f}_i^{W,CF}\right)\right] - V\left[\ln\left(1+\hat{f}_i^{W}\right)\right]\right)
\end{aligned}$$

where we used that the $E\hat{f}_i^W = 0$. Using $\hat{f}_i^W = \left(f_i^W - \bar{f}^W\right)/\bar{f}^W$ and $f_i^W =$

$\theta_i^{1-\mu}$ we get

$$\begin{aligned}
\ln\left(\frac{u^{CF}}{u}\right) &\simeq \tfrac{1}{2}\frac{1}{1-\mu}\left(V\left[\ln\left(1+\hat{f}_i^{W,CF}\right)\right]-V\left[\ln\left(1+\hat{f}_i^{W}\right)\right]\right) \\
&= \tfrac{1}{2}\frac{1}{1-\mu}\left(V\left[\ln\left(\frac{f_i^{W,CF}}{\bar{f}^{W,CF}}\right)\right]-V\left[\ln\left(\frac{f_i^{W}}{\bar{f}^{W}}\right)\right]\right) \\
&= \tfrac{1}{2}\frac{1}{1-\mu}\left(V\left[\ln f_i^{W,CF}\right]-V\left[\ln f_i^{W}\right]\right) \\
&= \tfrac{1}{2}\left(1-\mu\right)\left(V\left[\ln \theta_i^{CF}\right]-V\left[\ln \theta_i\right]\right) \\
&\simeq \tfrac{1}{2}\left(1-\mu\right)\left(V\left[\theta_i^{CF}/\bar{\theta}^{CF}\right]-V\left[\theta_i/\bar{\theta}\right]\right)
\end{aligned}$$

where the last equality is simply a first order Taylor approximation saying that $\ln\theta_i \simeq \left(\theta_i-\bar{\theta}\right)/\bar{\theta}$ where $\bar{\theta}$ is the mean of θ_i. Then,

$$\begin{aligned}
\frac{u^{CF}}{u} &\simeq \exp\left(\tfrac{1}{2}\left(1-\mu\right)\left(V\left[\theta_i^{CF}/\bar{\theta}^{CF}\right]-V\left[\theta_i/\bar{\theta}\right]\right)\right) && (2.15)\\
&= \exp\left(\tfrac{1}{2}\left(1-\mu\right)V\left[\theta_i/\bar{\theta}\right]\left(\frac{V\left[\theta_i^{CF}/\bar{\theta}^{CF}\right]}{V\left[\theta_i/\bar{\theta}\right]}-1\right)\right) && (2.16)\\
&\simeq \exp\left(\tfrac{1}{2}\left(1-\mu\right)V\left[\theta_i/\bar{\theta}\right]\right)\frac{V\left[\theta_i^{CF}/\bar{\theta}^{CF}\right]}{V\left[\theta_i/\bar{\theta}\right]} && (2.17)
\end{aligned}$$

2.7.2. Counterfactual Decompositions

Let $u_{XX=YY=0}$ denote the unemployment rate that prevails if we set $\alpha_i^{XX} = \alpha_i^{YY} = 0$. Then, there are two ways to define the contribution of a particular frictions to unemployment.

$$\begin{aligned}
contrib_1^{WM} &= u - u_{WM=0} && (2.18)\\
contrib_2^{WM} &= u_{JM=WS=0} - u_{WM=JM=WS=0} && (2.19)
\end{aligned}$$

By using both estimators, we can disentangle the direct contribution of a friction from its contribution through its correlation with other frictions and thus design a decomposition that is (approximately) additive.

From equation (2.7), taking a second order Taylor approximation around $\hat{f}_i^W = 0$, we get that

$$u_{XX=0} - u_{XX=YY=0} \simeq \kappa u\left(V\left[\hat{f}_i^W|\hat{\alpha}_i^{XX}=0\right]-V\left[\hat{f}_i^W|\hat{\alpha}_i^{XX}=\hat{\alpha}_i^{YY}=0\right]\right) \quad (2.20)$$

where κ is some constant of proportionality, which is not of interest here, and u is the actual unemployment rate. Using this approximation, the estimators can be written as

$$\begin{aligned} contrib_1^{WM} &\simeq \kappa\left(1-\mu\right)^2 u\left(V\left[\alpha_i^{WM}-\alpha_i^{JM}-\alpha_i^{WS}\right]\right. \\ &\left.-V\left[-\alpha_i^{JM}-\alpha_i^{WS}\right]\right) \quad (2.21) \\ &= \kappa\left(1-\mu\right)^2 u\left(V\left[\alpha_i^{WM}\right]-2Cov\left[\alpha_i^{WM},\alpha_i^{JM}\right]\right. \\ &\left.-2Cov\left[\alpha_i^{WM},\alpha_i^{WS}\right]\right) \quad (2.22) \end{aligned}$$

$$contrib_2^{WM} \simeq \kappa\left(1-\mu\right)^2 u\left(V\left[\alpha_i^{WM}\right]-0\right) = \kappa\left(1-\mu\right)^2 uV\left[\alpha_i^{WM}\right] \quad (2.23)$$

So, the difference between the two estimators is that $contrib_1^{WM}$ includes the covariance terms involving α_i^{WM}, whereas $contrib_2^{WM}$ does not.

To get an (approximately) additive decomposition, we use

$$contrib\_add^{WM} = \tfrac{1}{2}\left(contrib_1^{WM}+contrib_2^{WM}\right) \quad (2.24)$$

Because this estimator includes half of the covariance terms and the other half will be attributed to the other frictions, it satisifies

$$contrib\_add^{WM}+contrib\_add^{JM}+contrib\_add^{WS} \simeq contrib^{MMtotal} \quad (2.25)$$

Figures 2.6 and 2.7 show that this approximation is good in the actual data.

2.7.3. Match Surplus

Match Surplus in the DMP model

The value of an employed worker in submarket i, W_{it}, and the value of an unemployed worker in that submarket, U_{it}^W, satisfy the following set of Bellman equations,

$$\left(1+r\right)W_{it} = w_{it}+\lambda_{it}E_tU_{it+1}^W+\left(1-\lambda_{it}\right)E_tW_{it+1} \quad (2.26)$$

$$\left(1+r\right)U_{it}^W = b_{it}+f_{it}^W E_tW_{it+1}+\left(1-f_{it}^W\right)E_tU_{it+1}^W \quad (2.27)$$

where λ_{it} is the separation rate, f_{it}^W is the job finding rate, w_{it} is the wage and b_{it} is the flow value of being unemployed, which consists of unemployment benefits and the value of leisure. Worker surplus equals the difference between the payoff from having a job in submarket i minus the payoff of looking for a job in that submarket, $S_{it}^W = W_{it}-U_{it}^W$, so that

$$\left(1+r\right)S_{it}^W = w_{it}-b_{it}+\left(1-\lambda_{it}-f_{it}^W\right)E_tS_{it+1}^W \quad (2.28)$$

where $w_{it}-b_{it}$ is the worker's flow payoff from having a job net of the payoff from being unemployed, and $\lambda_{it}+f_{it}^W$ is worker turnover.

The value of a filled job in submarket i, J_{it}, and the value of a vacancy in that submarket, U_{it}^J, satisfy the following set of Bellman equations,

$$\left(1+r\right)J_{it} = \pi_{it}+\lambda_{it}E_tU_{it+1}^J+\left(1-\lambda_{it}\right)E_tJ_{it+1} \quad (2.29)$$

$$(1+r)\, U_{it}^{J} = -k_{it} + f_{it}^{F} E_t J_{it+1} + \left(1 - f_{it}^{F}\right) E_t U_{it+1}^{J} \tag{2.30}$$

where f_{it}^{F} is the worker finding rate, π_{it} are flow profits and k_{it} are vacancy posting costs. Job surplus equals the difference between the payoff from having a filled job in submarket i minus the payoff of having a vacancy in that submarket, $S_{it}^{J} = J_{it} - U_{it}^{J}$, so that

$$(1+r)\, S_{it}^{J} = \pi_{it} + k_{it} + \left(1 - \lambda_{it} - f_{it}^{F}\right) E_t S_{it+1}^{J} \tag{2.31}$$

where $\pi_{it} + k_{it}$ is the firm's flow payoff from having a filled job gross of vacancy posting costs, and $\lambda_{it} + f_{it}^{F}$ is job turnover.

Match Surplus with Time-Varying Payoffs and Turnover

In order to be able to solve forward for match surplus, take a linear approximation of the Bellman equation around $\tau_{it} = \tau_i^*$ and $S_{it} = S_i^*$.

$$\begin{aligned}(1+r)\, S_{it} &= y_{it} + E_t\left[(1-\tau_{it+1})\, S_{it+1}\right] \\ &\simeq y_{it} + (1-\tau_i^*)\, E_t S_{it+1} + E_t\left[\tau_i^* - \tau_{it+1}\right] S_i^* \end{aligned} \tag{2.32}$$

Now, we can solve forward as if turnover were constant:

$$S_{it} \simeq \frac{1}{1+r}\left\{y_{it} + E_t\left[\tau_i^* - \tau_{it+1}\right] S_i^*\right\} + \frac{1-\tau_i^*}{1+r} E_t S_{it+1} \tag{2.33}$$

$$= \frac{1}{1+r}\sum_{s=0}^{\infty}\left(\frac{1-\tau_i^*}{1+r}\right)^{s} E_t\left[y_{it+s} + \left(\tau_i^* - \tau_{it+s+1}\right) S_i^*\right] \tag{2.34}$$

From the autoregressive processes (which do not need to be independent because of the linearity)

$$E_t y_{it+s} = \bar{y}_t + (1-\delta_y)^s\,(y_{it} - \bar{y}_t) \tag{2.35}$$

$$E_t\left[\tau_i^* - \tau_{it+s+1}\right] = \tau_i^* - \bar{\tau}_t + (1-\delta_\tau)^{s+1}\,(\bar{\tau}_t - \tau_{it}) \tag{2.36}$$

Substituting into the expression for surplus

$$\begin{aligned} S_{it} &\simeq \frac{1}{1+r}\sum_{s=0}^{\infty}\left(\frac{1-\tau_i^*}{1+r}\right)^{s}\left\{\bar{y}_t + (1-\delta_y)^s\,(y_{it}-\bar{y}_t) + (\tau_i^* - \bar{\tau}_t)\, S_i^* + (1-\delta_\tau)^{s+1}\,(\bar{\tau}_t - \tau_{it})\, S_i^*\right\} \\ &= \frac{1}{1+r}\sum_{s=0}^{\infty}\left(\frac{1-\tau_i^*}{1+r}\right)^{s}\left\{\bar{y}_t + (\tau_i^* - \bar{\tau}_t)\, S_i^*\right\} + \frac{1}{1+r}\sum_{s=0}^{\infty}\left(\frac{(1-\tau_i^*)\,(1-\delta_y)}{1+r}\right)^{s}(y_{it}-\bar{y}_t) \\ &+ \frac{1-\delta_\tau}{1+r}\sum_{s=0}^{\infty}\left(\frac{(1-\tau_i^*)\,(1-\delta_\tau)}{1+r}\right)^{s}(\bar{\tau}_t - \tau_{it})\, S_i^* \\ &= \frac{\bar{y}_t + (\tau_i^* - \bar{\tau}_t)\, S_i^*}{r+\tau_i^*} + \frac{y_{it}-\bar{y}_t}{r+\tau_i^*+\delta_y-\delta_y\tau_i^*} + \frac{(1-\delta_\tau)\,(\bar{\tau}_t-\tau_{it})\, S_i^*}{r+\tau_i^*+\delta_\tau-\delta_\tau\tau_i^*} \\ &\simeq \frac{\bar{y}_t + (\tau_i^* - \bar{\tau}_t)\, S_i^*}{r+\tau_i^*} + \frac{y_{it}-\bar{y}_t}{r+\tau_i^*+\delta_y} + \frac{(1-\delta_\tau)\,(\bar{\tau}_t-\tau_{it})\, S_i^*}{r+\tau_i^*+\delta_\tau} \end{aligned}$$

Finally, setting $\tau_i^* = \tau_{it}$ and $S_i^* = S_{it}$ and rearranging we get the expression in the main text.

$$S_{it} \simeq \frac{(r+\tau_{it})(r+\tau_{it}+\delta_\tau)}{(r+\tau_{it})(r+\tau_{it}+\delta_\tau)+\delta_\tau(1+r+\tau_{it})(\bar{\tau}_t-\tau_{it})}\left(\frac{\bar{y}_t}{r+\tau_{it}} + \frac{y_{it}-\bar{y}_t}{r+\tau_{it}+\delta_y}\right) \tag{2.37}$$

2.7.4. Heterogeneity

Observable Worker Heterogeneity

We implement this approach in two steps. First, we regress the variable of interest on observable worker characteristics using a flexible specification. The variable of interest is either the wage, or an dummy variable indicating whether a worker lost or found a job. Second, we calculate fitted values for 40 worker cells, defined based on 2 gender, 5 education groups (less than high school, high school graduate, some college, college graduate, or more than college), and 4 categories for potential labor market experience (0-10 years, 11-20 years, 21-30 years, 31-40 years after completion of schooling), and calculate worker and job surplus and job finding rates for the average worker in each of these 40 cells.

The reasons for the first step are threefold. First, it allows us to control for observable characteristics, race and marital status, which are not used to define worker cells because doing so would result in too few observations per cell. When we calculate fitted values, we set these variables equal to a reference category, effectively calculating hypothetical wages and worker flows as if all workers were white, non-hispanic and married. Second, the regression allows us to control for differences in education and experience within cells. Third, using fitted values makes sure that there are no missing values: if there are no workers in a given cell, we generate a virtual worker with gender, education and experience equal to the cell average.

The regression specification we use must be flexible enough to not change the features of the data, but restrictive enough so that we can identify fitted values for all cells. We include fourth order polynomials in all controls, plus interactions of the first order effects of all controls with each other as well as with state or industry dummies, so that we get the following specification for worker w in state or industry i,

$$\begin{aligned} y_{wi} = {} & D_i'\beta_0 + \beta_1 f_{wi} + \beta_2 b_{wi} + \beta_3 m_{wi} + \beta_4 s_{wi} + \beta_5 x_{wi} \\ & + \beta_6 s_{wi}^2 + \beta_7 s_{wi}^3 + \beta_8 s_{wi}^4 + \beta_9 x_{wi}^2 + \beta_{10} x_{wi}^3 + \beta_{11} x_{wi}^4 \\ & + \beta_{12} f_{wi} * s_{wi} + \beta_{13} f_{wi} * s_{wi}^2 + \beta_{14} f_{wi} * x_{wi} + \beta_{15} f_{wi} * x_{wi}^2 \\ & + s_{wi} * D_i'\beta_{16} + x_{wi} * D_i'\beta_{17} + x_{wi} * S_{wi}'\beta_{18} + x_{wi}^2 * S_{wi}'\beta_{19} + \varepsilon_{wi} \quad (2.38) \end{aligned}$$

where D_i is a vector of dummies for states or industries, f_{wi} is a dummy variable for female workers, b_{wi} a dummy for African-American workers, m_{wi} a dummy for married workers, s_{wi} is schooling in years, x_{wi} is potential labor market experience (age minus schooling minus 6) and S_{wi} is a vector of dummies for the five education categories.

The dependent variable y_{wit} is either the logarithm of the wage or a dummy variable indicating whether that worker lost or found a job. If y_{wit} is a dummy variable, we use a probit model to guarantee that the fitted values lie between 0 and 1. For wages we use a log-linear specification, as is common in the literature, see Card (1999). In order to get fitted values for wages, we use the fitted values for log wages and apply the correction factor suggested by Cameron and Trivedi (2010). For the regressions of the probability to find or loose a job we use the sample weights from the basic monthly files. For regressions of wages we use the earnings weights, because wages are only available in the outgoing rotation groups.

The second step controls for differences in gender, education and experience across cells in a fully non-parametric manner. Because we first take relative deviations from the average across submarkets, and only then average over worker groups, any differences in dispersion because of differences in the composition of the work force over the 40 cells are controlled for.

Controlling for worker heterogeneity in profits is more difficult, because we do not observe profits at the worker level. We attempt to still control for heterogeneity, by assuming that worker heterogeneity affects profits in the same way it affects wages. Then, we can control for heterogeneity by multiplying profits by the ratio of wages controlled for worker heterogeneity w_{it}^* over raw wages w_{it}, $\log \pi_{it}^* = \log \pi_{it}^{\text{NIPA}} - \log w_{it}^{\text{CPS}} + \log w_{it}^{*\text{CPS}}$ or $\log \pi_{it}^* = \log \pi_{it}^{\text{NIPA}} - \log w_{it}^{\text{NIPA}} + \log w_{it}^{*\text{CPS}}$. We explore the robustness of our results if we do not control profits and wages for worker heterogeneity.

Unobservable Heterogeneity

If job amenities are constant over time, true worker surplus is given by $\hat{S}_{it}^W + c_i^W$ and the true job surplus equals $\hat{S}_{it}^F + c_i^F$. Then, we can control for compensating differentials by using $\hat{S}_{it}^W$, $\hat{S}_{it}^F$, $\hat{f}_{it}^W$ and $\hat{f}_{it}^F$ in deviations from their time series averages. To see how this works, note that equations (2.2), (2.3) and (2.4) hold in each year, so that,

$$\hat{f}_{it}^W + \hat{S}_{it}^W + c_i^W = \alpha_{it}^{WM} \Rightarrow \widehat{\hat{f}}_{it}^W + \widehat{\hat{S}}_{it}^W = \hat{\alpha}_{it}^{WM} \tag{2.39}$$

$$\hat{f}_{it}^F + \hat{S}_{it}^F + c_i^F = \alpha_{it}^{JM} \Rightarrow \widehat{\hat{f}}_{it}^F + \widehat{\hat{S}}_{it}^F = \hat{\alpha}_{it}^{JM} \tag{2.40}$$

$$\hat{S}_{it}^W + c_i^W - \hat{S}_{it}^F - c_i^F = \alpha_{it}^{WD} \Rightarrow \widehat{\hat{S}}_{it}^W - \widehat{\hat{S}}_{it}^F = \hat{\alpha}_{it}^{WD} \tag{2.41}$$

where $\widehat{\hat{x}}_{it}$ denotes a variable in deviation from its time series average, where the variable itself is in deviation from its average across submarkets, $\widehat{\hat{x}}_{it} = \hat{x}_{it} - \bar{\hat{x}}_i$ and $\hat{x}_{it} = (x_{it} - \bar{x}_t)/\bar{x}_t$ and $\hat{\alpha}_{it} = \alpha_{it} - \bar{\alpha}_i$ denotes the adjustment costs in deviations from their time series average. For the industry data, we calculate deviations from the time series average separtely for the SIC sample 1979-1997 and the NAICS sample 1998-2009. Taking deviations from the time series averages is like including state or industry-specific fixed effects and controls for time-invariant compensating differentials.

2.7.5. Disaggregration and the Level of Mismatch

In appendix 2.7.1, we showed that

$$\frac{u^{CF}}{u} \simeq \exp\left(\tfrac{1}{2}\left(1-\mu\right)V\left[\theta_i/\bar{\theta}\right]\right)\frac{V\left[\theta_i^{CF}/\bar{\theta}^{CF}\right]}{V\left[\theta_i/\bar{\theta}\right]} \tag{2.42}$$

which is observable except the variance ratio, which we get from Barnichon and Figura. Notice that $\exp\left(\frac{1}{2}\left(1-\mu\right)V\left[\theta_i/\theta\right]\right) \simeq 1$ so that we can safely ignore this part of the correction factor.

Barnichon and Figura show that

$$\ln\left(V_n\left[\frac{\theta_i}{\bar{\theta}}\right]\right) \simeq \ln a_0 + a_{geo}\ln n_{geo} + a_{occ}\ln n_{occ} \tag{2.43}$$

where V_n is the variance of θ_i based on a higher level of aggregation and $n = N/N^{CF}$ is the ratio of the observed versus the correct number of labor market segments. They also estimates the parameters of this relation using UK data to and find $a_{geo} = 0.13$ and $a_{occ} = 0.67$. This implies

$$\ln\left(\frac{V\left[\theta_i^{CF}/\bar{\theta}^{CF}\right]}{V\left[\theta_i/\bar{\theta}\right]}\right) = a_{geo}\ln\left(\frac{1}{n_{geo}}\right) + a_{occ}\ln\left(\frac{1}{n_{occ}}\right) \tag{2.44}$$

because by assumption θ_i^{CF} are the finding rates for the right level of disaggregation so that $n_{geo}^{CF} = n_{occ}^{CF} = 1$.

In the UK data that Barnichon and Figura use, the correct number of geographic areas is about 232 (travel to work areas). The U.S. population is larger than the UK population, but the land area is larger as well. Therefore, Barnichon and Figura assume the number of geographic units is the same in the same in the two countries. Since we work with 50 states, $1/n_{geo} = 232/50 = 4.64$. The same UK data have 353 detailed occupational groups, which should be similar in the US. We use 33 broad industries. Assuming these broad industry categories are comparable to broad occupations categories, we get $1/n_{occ} = 353/33 = 10.7$. This implies a correction factor for the variance of labor market tightness of,

$$\frac{V\left[\theta_i^{CF}/\bar{\theta}^{CF}\right]}{V\left[\theta_{geo*ind,i}/\theta_{geo*ind}\right]} = \exp\left(0.13\ln\left(4.64\right) + 0.67\ln\left(10.7\right)\right) = 6.0 \tag{2.45}$$

which is the correction factor for aggregation that we use in Section 2.4.2 in the main text.

Chapter 3

REEVALUATING THE POLARIZATION OF THE U.S. LABOR MARKET: EVIDENCE FROM THE CPS

3.1. Introduction

"Job polarization" is one of the most prominent developments in the U.S. labor market in recent times (e.g., Acemoglu (1999), Autor et al. (2006), Autor et al. (2008), Acemoglu and Autor (2011)). While in the 1980s employment growth was more rapid in occupations with higher skill-requirements, since the 1990s we observe a U-shaped pattern of job growth: jobs in the middle of the skill distribution disappear while there is modest growth of low- and strong growth of high-skilled jobs. Similar patterns have been documented, amongst others, for the UK (Goos and Manning, 2007), Germany (Dustmann et al., 2009), and other European countries (Goos et al., 2009).

Job polarization is appealing for at least two reasons. Firstly, it has a sound theoretical basis in the routinization hypothesis (Autor et al., 2003). Due to the fall in the cost of computer capital, there has been a decreasing demand for jobs dealing with routine tasks that can be automated. These jobs usually have intermediate skill requirements. At the same time, computer capital does not replace non-routine manual labor – e.g., waiters or truck drivers; jobs with usually low skill requirements – and it complements non-routine abstract tasks performed by the highest skilled occupations. Secondly, job polarization is in line with the well-documented fact that the upper-half of the wage distribution became steadily more spread out while the lower-half stopped increasing after the late 1980s (e.g., Autor et al. (2006), Autor et al. (2008), Acemoglu and Autor (2011) for the U.S. and Goos and Manning (2007) for the UK).

In this paper, I firstly show that existing evidence of job polarization in the U.S.

labor market since the 1990s is biased since it is based on occupational employment growth rates from the U.S. Census that are non-consistent over time. I then reevaluate the evidence for job polarization by deviating in two important aspects from the existing literature: firstly, I rely on data from the Current Population Survey (CPS) instead of the U.S. Census, and secondly, I slightly adjust the time periods that I study. These two changes allow me to calculate occupational employment growth using occupational categories that are – contrary to existing studies – fully consistent over time.

I document that – based on this consistent data – job polarization is sensitive to the way an occupation's skill-requirements are measured. Contrary to previous studies, I find that occupations' employment shares grew monotonously in years of schooling. There is slight evidence of polarization, however, when an occupation's mean (or median) wage is used as a measure of skill. Motivated by this evidence, I show that since the 1990s employment growth is actually polarized with regard to the *education-premium* of an occupation, that is, the wage conditional on years of schooling. I argue that, firstly, this finding is compatible with the routinization hypothesis, and secondly, that this can be seen as evidence for growing skill-mismatch in the U.S. labor market.

3.2. Reevaluating Changes in the U.S. Occupational Structure

3.2.1. Replicating Autor et al. (2006)

Figure 3.1 replicates the pattern of job polarization in the 1990s in the United States as documented by Autor et al. (2006) (AKK in the following).[1] Using data from the U.S. Census (IPUMS, Ruggles et al. (2010)), I first sort occupations into percentiles according to their distribution in the 1980 years of schooling distribution. For each percentile, I calculate the growth in the share of hours worked from 1980 to 1990 and from 1990 to 2000. I then plot the employment growth of an occupation against its percentile in the years of schooling distribution. Like AKK, I find that that while between 1980 and 1990 there was stronger employment growth in occupations with high education, this pattern changed substantially in the 1990s when a "hollowing out" of the occupational structure took place: there were modest employment gains in low-skilled jobs, losses in middling jobs, and strong gains in high-skilled jobs. The same pattern holds when instead of years of schooling the average (or median) wage is used as a proxy for skill.

[1] In this paper I refer to Autor et al. (2006) as the baseline study of job polarization. I could have instead used Autor et al. (2008) or Acemoglu and Autor (2011) as the baseline. This would not have changed the results.

3.2.2. Non-Consistent Occupation Categories

The occupation classification used in the U.S. Census underwent substantial changes in both 1990 and 2000. AKK deal with this challenge by applying a crosswalk provided by Meyer and Osborne (2005). While this crosswalk is in general very useful, I show here that it is not well-suited for the calculation of occupational growth rates. I demonstrate this with data from the Current Population Survey (CPS) because it has a higher frequency than the U.S. Census while using very similar occupation categories. The same changes the occupational categories in the U.S. Census went through in 1980,1990 and in 2000 took place in the CPS in 1983, 1992, and in 2003.

Figure 3.2 shows the standard deviations of year-to-year occupational employment growth rates when the Meyer and Osborne (2005) crosswalk is applied to the CPS data. It is apparent that – although the crosswalk is supposed to guarantee consistency over time – there are substantial "jumps" visible at the break dates. In particular, the crosswalk between the 1990 and 2000 Census occupation classification seems to be problematic. This implies that employment growth rates calculated across these break dates is subject to substantial measurement error.

3.2.3. Evidence from Consistent Data

I reevaluate the evidence for job polarization using consistent occupational categories. In order to do so, I use data from the CPS and slightly adjust the time periods that I study. Since changes took place in January of 1983, 1992, 2003, and 2011[2], the only way to calculate consistent growth rates is to use 1983 to 1991, 1992 to 2002, and 2003 to 2010. Apart from this difference, I follow the approach by AKK.

The graph on the top left of Figure 3.3 shows the results when years of schooling is used to measure occupational skill. The lines represent locally weighted regressions with bandwidths 0.8. Employment growth is higher in occupations with more years of schooling. Importantly, this pattern looks surprisingly similar in all three time-periods. There is no evidence of a "twisting" of the distribution of employment across occupations over time as documented by AKK. In particular, there is no evidence of job polarization or of a "hollowing out" of the occupational distribution in the 1990s.

Following the literature, in the graph on the top right of Figure 3.3 I use the average wage of an occupation in 1983 as an alternative skill-measure.[3] The results now look much more similar to the findings by AKK replicated in Figure 3.1. While between 1983 and 1991 employment growth was higher in occupations with higher pay, there is some evidence of polarization in 1992-2002 and stronger evidence in 2003-2010.

[2]In the CPS, the change to the 2010 Census occupational classification only took place in 2012. However, there are several other small changes in January 2011.

[3]Using the median wage leads to very similar results.

These findings imply, firstly, that – although highly correlated – pay and education differ substantially for many occupations. Secondly, the occupations experiencing growth at the lower end of the wage distribution since the 1990s are not the occupations with the lowest levels of schooling. That is, while there are signs of employment growth in low-paid occupations, there is no evidence of employment growth in occupations with low levels of education. The occupations that grow are therefore the ones with that pay badly but have intermediate education, that is, occupations offering a small education-premium. I explore this more systematically using the following regression approach:

$$wage_{i,1983} = \alpha + \beta \times years\,of\,schooling_{i,1983} + \epsilon_{i,1983} \tag{3.1}$$

The fitted residuals $\widehat{\epsilon}_{i,1983} = wage_{i,1983} - \hat{\alpha} - \hat{\beta} \times years\,of\,schooling_{i,1983}$ can be seen as a simple way to quantify the education-premium of occupation i: the larger $\widehat{\epsilon}_{i,1983}$, the higher is the pay in an occupation conditional on its formal education. The graph on the bottom left of Figure 3.3 shows employment growth plotted against the education-premium calculated in this way. While employment growth is monotonous in 1983-1991 there is strong evidence of polarization in 1992-2002. Occupations with low and high education-premia grew almost equally strong. In 2003-2010, there is substantial growth in occupations with low education-premia and no growth in occupations with high premia. In general, the "twisting" of the pattern of employment growth over time is now much more visible than in the top right of Figure 3.3.

Table 3.1 shows estimates from a quadratic regression model (compare Goos and Manning (2007)). Occupations' log employment growth between 1992 and 2002 is regressed on the 1983 average years of schooling, the average wage, or the education-premium. The results confirm the visual evidence from Figures 3.3: employment growth is linear in years of schooling; the quadratic term is insignificant. However, in model (4) the linear term is negative and the quadratic term is positive, implying a U-shaped relationship between employment growth an initial wages. The same holds for the education-premium in model (6). When both the mean wage and the education-premium are part of the model in (7), only the coefficients on the education-premium variables are significantly different from zero.

Discussion

My findings are in line with the routinization hypothesis. According to Autor et al. (2003), the continued fall in the cost of computing capital in the last 30 years led to a decreased demand for routine labor that is easily automated. On the other hand, demand for non-routine manual and abstract labor stayed constant or grew. Occupation in the last two categories are likely to have low and high returns to formal education, respectively, while occupations dealing with routine labor have intermediate returns. The predicted changes in demand for labor are therefore compatible with the pattern of employment growth that I documented here.

My finding that the growth at the low end of the wage distribution is concentrated in occupations with intermediate education is concerning since it can be interpreted as a development towards more skill-mismatch in the US labor market. Growth is particularly strong in many service occupations (waiters, bartenders, e.g.,) that often employ workers who do not make adequate use of their formal education. Policy makers should be concerned about this increasing misallocation of human capital.

3.3. Conclusions

In this paper I reevaluated the evidence for job polarization in the U.S. labor market. I documented that calculating occupational employment growth from U.S. Census data is problematic since occupational classifications changed over time. When using consistent occupational data from the Current Population Survey (CPS) I found no evidence of job polarization when years of schooling is used as a measure of skill. There is slight evidence of polarization since the 1990s, however, when I used the average occupational wage instead. That is, while employment in poorly paid occupations was growing modestly, employment in occupations with low education was shrinking. I combined these two findings and showed that the education-premium has substantially more explanatory power for the pattern of employment growth: since the 1990s employment gains were concentrated in occupations with low and high education-premia while occupations with intermediate premia suffered losses. I argued that my findings are is in line with the routinization hypothesis. Moreover, they point towards a potentially increased extent of skill-mismatch in the U.S. labor market.

3.4. Tables and Figures

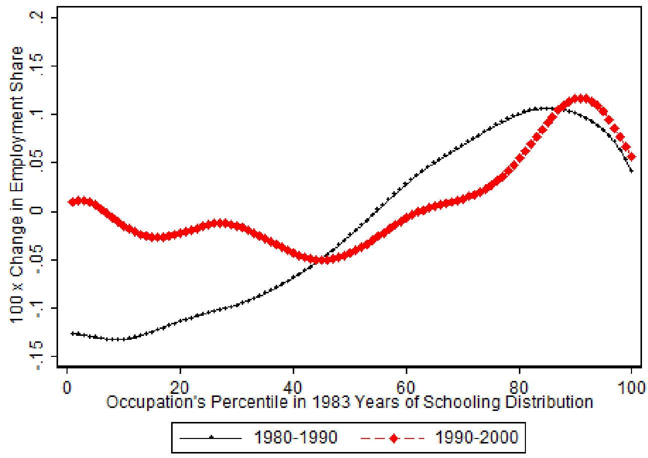

Figure 3.1: This figure replicates the findings of Autor et al. (2006). Using data from the IPUMS (Ruggles et al., 2010), I first sort occupations into percentiles according to their distribution in the 1980 education distribution. For each percentile, I calculate the growth in the share of hours worked from 1980 to 1990, and 1990 to 2000. I then plot the employment growth of an occupation against its percentile in the education distribution. The lines show locally weighted regression with bandwidths 0.8.

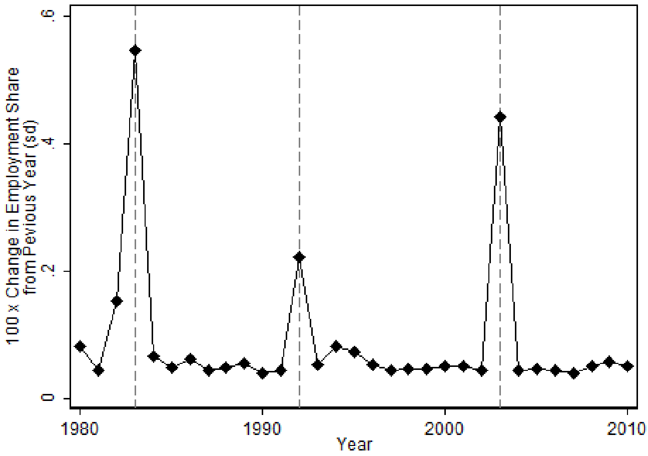

Figure 3.2: The figure shows standard deviations of year-to-year occupational employment growth rates based on CPS data. Since occupations are not consistent over time, I apply a cross-walk povided by Meyer and Osborne (2005). Despite application of the cross-walk big “jumps” are clearly visible at the break dates.

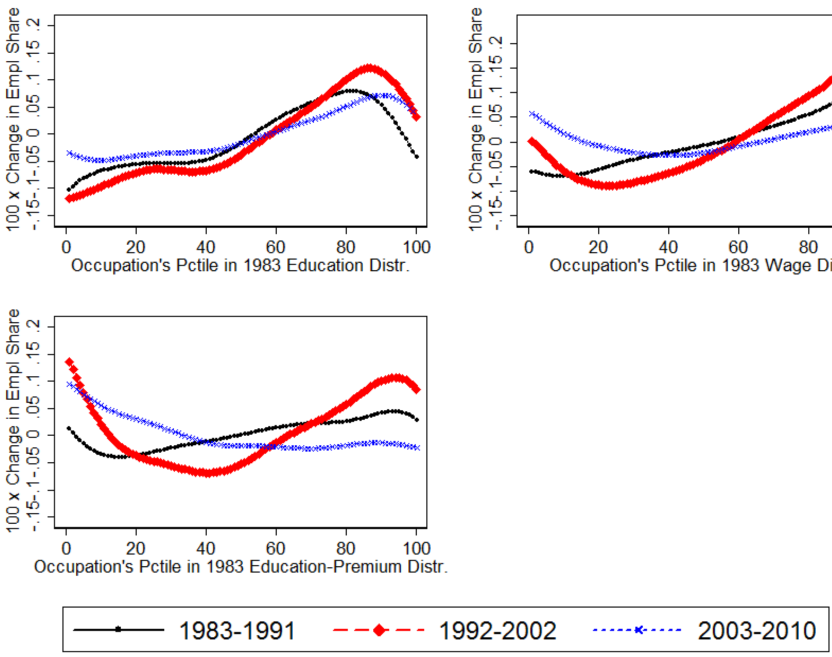

Figure 3.3: Using data from the CPS, I first sort occupations into percentiles according to their distribution in the 1983 education distribution (top left), the 1983 wage distribution (top right), and the distribution in the 1983 education-premium distribution as measured by the fitted residuals of regression equation (3.1) (bottom left). For each percentile, I calculate the growth in the share of hours worked from 1983 to 1991, 1992 to 2002, and 2003 to 2010. I then plot the employment growth of an occupation against its percentile in the education distribution. The lines show locally weighted regression with bandwidths 0.8.

Table 3.1: Employment Growth Regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1983 Years of Schooling	0.00336***	0.00038					
	(0.00053)	(0.00223)					
1983 Years of Schooling Square		0.00003					
		(0.00002)					
1983 Wage			0.00304***	-0.00312*			0.00301
			(0.00049)	(0.00187)			(0.00230)
1983 Wage Square				0.00006***			0.00002
				(0.00002)			(0.00002)
1983 Education-Premium					0.00146***	-0.00939***	-0.01038***
					(0.00051)	(0.00206)	(0.00251)
1983 Education-Premium Square						0.00011***	0.00009***
						(0.00002)	(0.00003)
Constant	-0.08678***	-0.03292	-0.06000**	0.04091	0.01713	0.19017***	0.12632***
	(0.03080)	(0.04973)	(0.02727)	(0.03999)	(0.02807)	(0.04201)	(0.04403)
Observations	492	492	492	492	492	492	492
R-squared	0.07495	0.07853	0.07359	0.09519	0.01663	0.07232	0.13284

Notes: Estimates of regressions of the form $\log(hours\ worked_{i,2002}) - \log(hours\ worked_{i,1992}) = \alpha + \beta \times x_{i,1983} + \gamma \times x^2_{i,1983} + \epsilon_{i,1983}$ are shown. $x_{i,1983}$is either the average years of schooling, the average wage, or the education-premium calculated according to equation (3.1) of occupation i in 1983. The method of estimation is generalized least-squares. Occupations are weighted by hours worked in 1992.

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