ESSAYS IN EMPIRICAL LABOR ECONOMICS

A dissertation presented

By

Shahriar Sadighi

to
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In partial fulfillment of the requirements for the degree of
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In the field of
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ABSTRACT OF DISSERTATION

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My dissertation consists of three essays in empirical labor economics which are self-contained and can be read independently of the others. The first essay, coauthored with Professor Modestino, measures mismatch unemployment in US economy in the post-recession era and explores the heterogeneity among educational groupings. The second essay estimates the changing effects of cognitive ability on wage determination of college bound and non-college bound young adults between 1980s and 2000s. The third essay, coauthored with Professor Dickens, examines the impact of measurement error in survey data on identifying the extent of downward nominal wage rigidity in US economy.

**Essay I: No Longer Qualified? Changes in the Supply and Demand for Skills within Occupations**

In this study, we extend the framework developed by Sahin et al. (2014) to measure mismatch unemployment since the end of the Great Recession and explore the heterogeneity among educational groupings. Our findings indicate that mismatch across two-digit industries and two-digit occupations explains around 17-20 percent of the recent recovery in the US unemployment rate since 2010. We also capture movements in employer education requirements over time using a novel database of 87 million online job posting aggregated by Burning Glass Technologies and further show that mismatch is not only greater in magnitude for high-skill occupations but also is more persistent over the course of the recent labor market recovery, possible accounting for the shift rightward that has been observed in the aggregate Beveridge Curve by other researchers. Furthermore, we shed light on at least one of the potential causes of mismatch on the demand side, providing evidence that labor demand shifts among high-skilled occupation groups exhibit a permanent increase in the share of employers requiring a Bachelor’s degree as well as other baseline, specialized,
and software skills listed on job postings, suggesting a role for structural shifts associated with changes in technology or capital investment. Our results demonstrate that equilibrium models where unemployed workers accumulate specific human capital and, in equilibrium, make explicit mobility decisions across distinct labor markets, can mean that workers are chasing a moving target—at least among high-skilled occupations. Furthermore, our findings inform debates focused on workforce development strategies and related educational policies where decision making could benefit from the use of real-time labor market information on employer demands to provide guidance for both job placement as well as program development.

**Essay II: The Changing Impacts of Cognitive Ability on Determining Earnings of College Bound and Non-College Bound Young Adults**

Using data on young adults from the 1979 and 1997 National Longitudinal Survey of Youth, I investigate the changing impact of cognitive ability, as captured by performance on AFQT tests, on wage determination of college bound and non-college bound young adults. My findings indicate that cognitive ability plays a substantially diminished role for the most recent cohort and its impact on wage determination has undergone a drastic change between 1980s and 2000s. My results tend to corroborate the findings of previous studies which emphasize the lifecycle path of technological development from adoption to maturation and trace back the labor market outcomes observed over these periods to pre- and post-2000 patterns in technology investment and its consequent boom-and-bust cycles in the demand for cognitive skills.

In this study, we employ data drawn from the 1996, 2001, 2004 and 2008 panels of the SIPP, which cover the years 1996-2013, to assess the effectiveness of dependent interviewing at reducing bias in the estimates of the extent of downward nominal wage rigidity in the US economy. In the 2004 and 2008 panels of the SIPP, dependent interviewing was used much more extensively than in the past. This questioning method by focusing on changes rather than levels of wages and using responses from prior interviews to query apparent inconsistencies over time reduces the incidence of reporting and measurement errors. Our change-in-wage distributions derived from SIPP 2004 and 2008 panels exhibit remarkably larger zero-spikes and asymmetries vis-à-vis those derived from 1996 and 2001 panels before dependent interviewing was used. These results are consistent with the findings of previous studies that used payroll data or statistical techniques to correct for reporting error. We apply one such technique to the SIPP panels before and after the introduction of dependent interviewing. In the pre-2004 panels the correction is large and results in a distribution that closely resembles the uncorrected distributions of the 2004 panel. When the correction is applied to the 2004 panel no evidence of errors is found.
Acknowledgments

I am indebted to my advisors, Alicia Sasser Modestino, William T. Dickens and Mindy Marks, for their patience and valuable suggestions as this work evolved.
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1. Introduction 55
Chapter 1 – No Longer Qualified? Changes in the Supply and Demand for Skills within Occupations

Co-authored with Alicia Sasser Modestino

1. Introduction

The persistent weakness of the U.S. labor market during the period following the Great Recession remains poorly understood. As of 2012, two years after the official end of the Great Recession, the unemployment rate still hovered around 8 percent, despite an increase in employer-reported vacancies. This shift in the relationship between unemployment and vacancies, known as the Beveridge curve, has highlighted the need to focus not just on the number of vacancies, but on their composition and skill requirements as well (Diamond and Sahin 2014). Indeed, Figure 1 indicates that the shift outward in the Beveridge curve has persisted through the end of 2016 even as the labor market has continued to recover.

A number of explanations for this shift have been proposed, each with potentially different policy implications. In particular, some have interpreted the shift as a deterioration in the matching/hiring process in the economy, such that idle workers may be seeking employment in sectors different from those where the available jobs are. For example, Sahin et al. (2014) measure the degree of mismatch between vacancies and workers across occupations and geographies and find that mismatch can potentially account for one-third of the increase in the unemployment rate during the Great Recession. Yet the lack of wage growth observed during the recovery period, even within industries and occupations with
relatively strong demand in the United States, would suggest little or no role for labor market mismatch. As a result, the economics literature has largely concluded that the weak labor market is mostly not a result of skills mismatch or other structural factors, but instead due to weak aggregate demand that increased unemployment across worker types, industry sectors, and occupation groups (Ghayad and Dickens 2012, Daly et al. 2012, Lazear and Spletzer 2012, Rothwell 2012, Carnevale et al. 2012, Sahin et al. 2014, Capelli 2014, Osterman and Weaver 2014). This observation has prompted others to explore the importance of the composition of the workforce and the motivation of job seekers in seeking to explain recent movements in the Beveridge curve (Veracierto 2011; Barnichon and Figura 2010; Shimer 2012; Fujita and Moscarini 2015; Hall and Schulhofer-Wohl 2015; Mukoyama, Patterson, Sahin 2014; Hagedorn et al. 2014).

Yet business leaders and policy makers, especially at the state and local level, continue to cite a “skills gaps” as a major obstacle to current and future job growth. Numerous media reports and employer surveys have suggested that a lack of skilled workers has made it difficult to fill jobs that are in high demand during the economic recovery, leading to slower than expected improvement in the labor market.\(^1\) One potential explanation for the disconnect between the anecdotal evidence and academic findings is that the skill demands of employers are changing within occupations, such that even workers with pre-recession experience in a given occupation are not fully-equipped to fill newer jobs in the same field during the recovery. For example, Modestino, Shoag, and Ballance (2016) find that

during the Great Recession a significant share of employers engaged in “upskilling”—raising
the education requirements for a given job, in part to take advantage of the weak labor
market. While they also subsequently find that employers decreased skill requirements as
the labor market tightened, their results indicate that at most only 20 percent of the total
increase in skill requirements that took place during the Great Recession was reversed,
suggesting that there may have been structural changes in the nature of some jobs in recent
years perhaps related to concurrent investments in new technology or production processes.
Such structural changes might help explain employers’ claims that they can’t find qualified
workers for some jobs, while at the same time the occupational distribution of job-seekers
appears roughly in balance with the occupational distribution of job vacancies, suggesting
little indication of mismatch as measured by economists.

In this paper, we measure changes in skill demands within occupations, in terms of
education requirements using data on 82.5 million online job vacancy postings from Burning
Glass Technologies covering the near universe of electronic posts in the U.S. between 2010
and 2016. We compare these demand-side changes to changes in the educational
qualifications of workers by occupation, using data from the Current Population Survey. If
workers’ qualifications are lagging in relation to employers’ changing demands within an
occupation, this might help to explain employers’ complaints of skills gaps even in fields not
lacking in the supply of experienced job-seekers. We further stratify our mismatch indices
by occupational skill type (low/medium/high) to determine whether there is heterogeneity
in mismatch across these segmented labor markets. We also distinguish between
occupations where we observe a temporary increase in skill requirements versus those
where these shifts have persisted as the labor market has recovered to determine the
characteristics of occupations experiencing more permanent shifts that can point to the potential structural forces underlying these observed trends.

We find that the degree and persistence of mismatch varies across different skill sectors of the economy. Grouping occupations by skill level according to the education of employed workers prior to the Great Recession, we find that the mismatch index was higher for high versus medium versus low skilled occupations. Moreover, high skill occupations were more likely to increase skill requirements over the recession and maintain those requirements during the recovery compared to middle- and low-skill occupations which exhibited upskilling as well as subsequent downskilling.

The finding that mismatch varies across different skill sectors of the economy has important implications for understanding the dynamics of the labor market. In particular, recognizing that the degree to which changing skill requirements are driven by structural versus cyclical forces is correlated with the initial skill level of the occupation, suggests that aggregate measures of mismatch can understate the difficulty of filling vacancies in certain skill sectors—particularly high-skilled occupations for which a Bachelor’s degree or higher is required. Our finding that the demand for skills within low- and middle-skill occupations is more sensitive to the business cycle also explains why one observes higher-skilled workers moving down the “career ladder” during recessions (Beaudry et al. 2013 Carnevale et al 2012). Finally, the observation that high-skilled occupations are more subject to structural forces that persist over the business cycle is consistent with other studies that find the college wage premium has stalled for those with only a bachelor’s degree but has grown for workers with advanced degrees.
Our findings also speak to policy debates focused on workforce development strategies and related educational policies where decision making could benefit from the use of real-time labor market information on employer demands to provide guidance for both job placement as well as program development. Armed with the understanding that qualifications for low- and middle-skill jobs increase temporarily during recessions would suggest that displaced workers from these skill sectors will experience longer spells of employment such that a targeted extension of UI benefits beyond the usual 26 weeks may be warranted.

The paper proceeds as follows. Section 2 lays out a theoretical framework for mismatch followed by a description of our empirical approach. Section 3 describes the unique features of the online job vacancy dataset used in our estimation. Section 4 reports the baseline mismatch index by industry and occupation as well as by skill level. Section 5 concludes.

2. Methodology

In this section, we will review our methodology to construct various measures of labor mismatch across different markets. As described in prior sections, we will estimate these measure across industries and occupations. We will then divide the occupations into three major skill categories (high, medium, and low). We closely follow Sahin et al. (2014) in developing a dynamic stochastic model that allows heterogeneity across markets (industry sectors and occupations) regarding productivity efficiencies as well as job destruction rates. In this framework, the labor market is frictional meaning that the hiring process is governed by a matching function and each industry sector or occupation category is considered to be a distinct labor market.
2.1 Measures of Mismatch

To derive the mismatch measure, we assume a common Cobb-Douglas aggregate matching function:

\[ h_{it} = \Phi_t \varphi_{it} m(u_{it}, v_{it}) = \Phi_t \varphi_{it} u_{it}^{1-\delta} v_{it}^\delta \]

where \( h_{it} \) are matches in industry (occupation) \( i \) at date \( t \), \( \Phi_t \varphi_{it} \) measures the level of fundamental frictions in industry (occupation) \( i \). \( u_{it} \) is the number of unemployed workers in industry (occupation) \( i \) at date \( t \), \( v_{it} \) is the number of vacant jobs in industry (occupation) \( i \) at date \( t \), \( \delta \in [0,1] \) is the vacancy share. We assume that the vacancy share is common across labor markets. Summing across these markets, the total number of hires can be written as:

\[ h_t = \Phi_t u_t^{1-\delta} v_t^\delta \left[ \sum_{i=1}^I \varphi_{it} \left( \frac{u_{it}}{u_t} \right)^{1-\delta} \left( \frac{v_{it}}{v_t} \right)^\delta \right] \]

In this framework, the total level of hires at each date is optimized subject to the matching frictions imposed by each market. The optimal planner's solution moves the unemployed across sectors to allocate more unemployed workers search in markets with higher efficiencies and vacancies.

Formally speaking, the optimality condition is:

\[ \Phi_{1t} m_{u1} \left( \frac{v_{1t}}{u_{1t}^c} \right) = \cdots = \Phi_{it} m_{ui} \left( \frac{v_{it}}{u_{it}^c} \right) = \cdots = \Phi_{It} m_{ul} \left( \frac{v_{It}}{u_{It}^c} \right) \]

Rewriting the optimal condition using a Cobb-Douglas function allocates the number of unemployed in the absence of mismatch across markets using the following expression:
\[
\left( \frac{v_{it}}{u_{it}^{cf}} \right) = \left( \frac{\varphi_{jt}}{\varphi_{it}} \right)^{\delta} \left( \frac{v_{jt}}{u_{jt}^{cf}} \right)
\]

Substituting into the second equation, the optimal number of hires is then:

\[
h_{t}^{cf} = \overline{\varphi_{t}} \varphi_{t} v_{t}^{\delta} u_{t}^{1-\delta}
\]

Where:

\[
\overline{\varphi_{t}} = \left[ \sum_{i=1}^{l} \frac{1}{\varphi_{it}^{\delta}} \left( \frac{v_{it}}{v_{t}} \right) \right]^{\delta}
\]

Therefore, we obtain the following expression for the mismatch index:

\[
\mathcal{M}_{\varphi_{t}} = 1 - \frac{h_{t}}{h_{t}^{cf}} = 1 - \sum_{i=1}^{l} \left( \frac{\varphi_{it}}{\varphi_{t}} \right) \left( \frac{v_{it}}{v_{t}} \right)^{\delta} \left( \frac{u_{it}}{u_{t}} \right)^{1-\delta}
\]

This baseline measure of the mismatch index is defined as fraction of hires that are lost because of inefficient allocation of unemployed workers across labor markets by industry (occupation). Thus, this index answers the basic question: if the planner had \(u_t\) available unemployed workers and used the optimal allocation rule, how many additional jobs would she be able to create? These additional hires are generated because, by better allocating the same number of unemployed, the planner can increase the aggregate job-finding rate and achieve more hires compared to the equilibrium (the “direct” effect of mismatch). It is useful to note that, in addition to this direct effect, \(u_t^*\) is in general lower than \(u_t\) which, for any given allocation rule, translates into a higher aggregate job-finding rate and more hires (the
“feedback” effect of mismatch). \( M_{qt} \) measures only the direct effect of mismatch on hires, but the counterfactual of Section 2.2 fully incorporates the feedback effect as well.\(^2\)

Sahin et al. (2014) demonstrate three useful properties of the index that we note here. First, \( M_{qt} \) is between zero (no mismatch) and one (maximal mismatch). Second, the index is invariant to aggregate shocks that shift the total number of vacancies and unemployed up or down, but leave the vacancy and unemployment shares across markets unchanged. Third, \( M_{qt} \) is increasing in the level of disaggregation. This last property suggests that every statement about the role of mismatch should be qualified with respect to the degree of sectoral disaggregation used.

2.1.1 Estimation

In order to compute the mismatch indices, we estimate the parameters of the model described above for the vacancy share, the market-specific matching efficiencies \( \varphi_{it} \) and the aggregate level matching efficiency \( \Phi_t \). Starting with equation (1), we can redefine the function in terms of the job seekers’ job finding rate:

\[
F_{it} = \frac{h_{it}}{u_{it}} = \Phi_t \varphi_{it} \theta_{it}^\delta \quad \text{Where} \quad \theta_{it} = \frac{v_{it}}{u_{it}}
\]

By taking the log of the above equation, we obtain the following relationship:

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\(^2\) Dickens (2010) and Lazear and Spletzer (2012) use an alternative index proposed by Mincer (1966) that only quantifies the number of job seekers searching in the wrong sectors. Yet the one used here developed by Sahin et al, also captures how such misallocation lowers the job-finding rate and raises unemployment. In addition, the analysis in this paper also allows for heterogeneity in productivity and matching efficiency, a key determinant of the optimal allocation of job seekers across labor markets.
\[
\log \left( \frac{h_{it}}{u_{it}} \right) = \log(\Phi_t) + \log(\varphi_{it}) + \delta \cdot \log \left( \frac{v_{it}}{u_{it}} \right)
\]  

(9)

It is straightforward to show that OLS estimation of equation (3) is subject to an endogeneity problem where shocks to the unobserved matching efficiency \((\varepsilon_{it})\) may affect the number of vacancies. Borowczyk-Martins, Jolivet and Postel-Vinay (2012) address the issue that

\[
\text{Cov}(\theta^\delta_{it}, \varepsilon_{it}) \neq 0
\]

by recognizing that the majority of movements inducing the bias in the OLS estimator are low-frequency ones and that including time varying polynomials and structural breaks largely solves this problem. Therefore, at the aggregate level we run the following OLS regression to estimate aggregate level market efficiencies:

\[
\log \left( \frac{h_t}{u_t} \right) = \beta_1 \cdot t + \beta_2 \cdot t^2 + \beta_3 \cdot t^3 + \beta_4 \cdot t^4 + \delta \cdot \log \left( \frac{v_t}{u_t} \right) + \varepsilon_t
\]  

(10)

At the market level (for each industry or occupation level depending on the definition of market) we run similar regressions to estimate the market-specific efficiencies.

2.2. Unemployment Mismatch

To calculate the counterfactual unemployment level that is the level of unemployment in the absence of the sectoral/occupational labor mismatch, we have used the Shimer (2005) methodology. Let’s assume that all unemployed workers find a job with a probability of \(F_t \in [0,1]\) and also employed individuals lose their jobs with probability of \(S_t \in [0,1]\) in each period. Losing the job in each period happens according to a Poisson process.
with the arrival rate \( s_t \equiv -\log(1 - S_t) \). Similarly, finding a job happens according to a Poisson process with the arrival rate of \( f_t \equiv -\log(1 - F_t) \).

The number of unemployed at date \( t + 1 \) can be solved as:

\[
 u_{t+1} = (1 - F_t)u_t + u^s_{t+1} 
\]  

(11)

Where \( u^s_{t+1} \) denotes the short-term unemployed workers. Inverting the above formula, we can retrieve probability of job finding as a function of unemployment and short-term unemployment:

\[
 F_t = 1 - \frac{u_{t+1} - u^s_{t+1}}{u_t} 
\]  

(12)

Shimer (2005) solves the unemployment and short-term unemployment differential equations to obtain an implicit expression for the separation probability:

\[
 u_{t+1} = \frac{(1 - e^{-f_t - s_t})s_t}{f_t + s_t} l_t + e^{-f_t - s_t}u_t 
\]  

(13)

Where \( l_t \equiv u_t + e_t \) is the size of the labor force during period \( t \).

Job finding rate in the absence of mismatch in each period is calculated using the following formula:

\[
 f_{t}^{cf} = \frac{f_t}{1 - \hat{M}_t \left( \frac{u_t}{u^{cf}_t} \right)^a} 
\]  

(14)
Where $M_t$ is the estimated mismatch measure in each period. Finally, the level of unemployment in the absence of mismatch is:

$$u_{t+1}^{cf} = \frac{(1 - e^{-f_t^{cf} - s_t})s_t}{f_t^{cf} + s_t}l_t + e^{-f_t^{cf} - s_t}u_t^{cf}$$

(15)

How should one interpret this measure of mismatch? Essentially, this measure of mismatch unemployment developed by Sahin et al. (2014) can be thought of as a “distance from a benchmark allocation” similar to the literature on misallocation and productivity (Lagos 2006; Restuccia and Rogerson 2008; Hsieh and Klenow 2009; Moll 2011; Jones 2013). Note that the implementation of this approach does not require solving for equilibrium allocations which would require additional assumptions about firms’ and workers’ behavior, their information set, or price determination. We simply take the empirical joint distribution of unemployment and vacancies across sectors as the equilibrium outcome. In addition, the

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3 It is worth noting that the steady state of unemployment rate during each period could be derived as a function of job finding rate and job separation rate. In the steady state situation we have:

$$s_t, e_t = u_t, f_t$$

Therefore, the steady state rate of unemployment is:

$$\frac{u_t}{l_t} = \frac{s_t}{(s_t + f_t)}$$

The initial values of job finding rate and unemployment in the absence of mismatch are calculated as follows:

$$f_1^{cf} = \frac{f_1}{1 - M_1}$$

and

$$u_1^{cf} = l_1 \cdot \frac{s_1}{(s_1 + f_1^{cf})}$$
counterfactual distribution (in absence of mismatch) that is derived from a simple planner’s problem can be solved analytically. As a result, the approach is robust and easily implementable, even with a high number of labor markets, and multiple sources of heterogeneity, idiosyncratic shocks, and aggregate fluctuations.

It is important to point out a few caveats and limitations associated with the approach described above. First, this methodology still yields a measure of mismatch across sectors—defined by the jointly observable characteristics of job vacancies and unemployed job seekers—not within sectors. As a result, concluding that mismatch plays a small role at the level of two-digit occupations does not necessarily rule out its importance at the three- or six-digit level. Second, it is important to note that the measure of mismatch used in this paper captures the sectoral misallocation between job vacancies and unemployed job seekers, excluding employed workers who search on the job. However, Sahin et al. (2014) verify that mismatch between vacancies and unemployment behaves very similarly to an index that also includes, among the job seekers, employed workers who report to search on the job. Finally, the mismatch indices we construct enable us to determine the relative importance of different dimensions of mismatch by partitioning the labor market based on several characteristics (e.g., industry, occupation, education) but does not allow us to separately quantify more nuanced sources of misallocation such as those related to costs of retraining or migration, relative wage rigidity, risk-aversion, imperfect insurance, or certain government policies that may hamper the reallocation of idle labor from shrinking to expanding sectors. Given that the benchmark planner’s allocation is derived under the assumption of costless mobility between sectors, our calculations on the role of mismatch are likely to reflect an upper bound of the “true” impact.
3. Data

We measure mismatch at both the industry and occupation levels. To construct the baseline mismatch index \( M_t \) requires sectoral data on vacancies, unemployment, and the vacancy share of the matching function. Data on vacancies and vacancy shares come from both the JOLTS (industry) and BGT (occupation). Data on unemployment comes from the CPS.

To construct the other mismatch indices requires additional data on market-specific matching efficiency parameters for the \( M_{t\phi} \) index, and information on productive efficiency (productivity and separation rates) by sector for the \( M_{xt} \) index and its corresponding counterfactual. To derive market-specific matching efficiencies and vacancy shares we will also require data on hires to estimate the appropriate matching functions.

To compute the measures of labor mismatch based on the method that we explained in section two, we need detailed information on unemployment, vacancies, flows in and out of labor force, productivity rates, destruction rates, etc. We gather this information at the industry and occupational level.4

3.1. Vacancy Measures

3.1.1 Using the JOLTS data to Measure Baseline Industry Trends

The Bureau of Labor Statistics (BLS) collects and compiles Job Opening and Labor Turnover Survey (JOLTS) data monthly from a sample of roughly 16000 nonfarm establishments since December 2000. Data are collected for total employment, job openings, hires, quits, layoffs and discharges, other separations, and total separations. Detailed

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4 "Industry" refers to the work setting and economic sector, while "occupation" relates to the worker's specific technical function.
information on the number of vacant jobs\(^5\) and hires\(^6\) are available by the industry classification. JOLTS data provides 17 industrial classification which is comparable with two digits North American Industry Classification System (NAICS). \(^7\)

Even though JOLTS estimates are survey based and therefore are subject to both sampling and non-sampling errors meaning that there is a chance that sample estimates may differ from the “true” population values that they present, they are still well known and widely used and more importantly designed to be comparable the supply side data that we use for our analysis that is unemployment estimates from Current Population Survey.\(^8\) However, we are not able to study structural imbalance at the occupational level and further analysis of labor mismatch by skill level using JOLTS. So instead we make use of the BGT vacancy measures for the occupation level analysis.

3.1.2 Using BGT Data to Measure Occupation Trends by Educational Groups

The other source of vacancy data used in this paper is collected by Burning Glass Technologies (BGT), one of the leading vendors of online job posting data. BGT collects

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\(^5\) According to JOLTS documentation vacancy definition is as follows: “Job openings information is collected for the last business day of the reference month. A job opening requires that: 1) a specific position exists and there is work available for that position, 2) work could start within 30 days whether or not the employer found a suitable candidate, and 3) the employer is actively recruiting from outside the establishment to fill the position.”

\(^6\) According to JOLTS documentation hires definition is as follows: “The hires level is the total number of additions to the payroll occurring at any time during the reference month, including both new and rehired employees, full-time and part-time, permanent, short-term and seasonal employees, employees recalled to the location after a layoff lasting more than 7 days, on-call or intermittent employees who returned to work after having been formally separated, and transfers from other locations. The hires count does not include transfers or promotions within the reporting site, employees returning from strike, employees of temporary help agencies or employee leasing companies, outside contractors, or consultants. The hires rate is computed by dividing the number of hires by employment and multiplying that quotient by 100.”


\(^8\) Sampling error estimates are available at [www.bls.gov/jlt/jolts_median_standard_errors.htm](http://www.bls.gov/jlt/jolts_median_standard_errors.htm).
detailed information on the more than seven million current online job openings daily from over 40,000 sources including job boards, newspapers, government agencies, and employer sites.\textsuperscript{9} The data are collected via a web crawling technique that uses computer programs called “spiders” to browse online job boards and other web sites and systematically text parse each job ad into usable data elements. BGT mines over seventy job characteristics from free-text job postings including employer name, location, job title, occupation, years of experience requested and level of education required or preferred by the employer, as well as other dimensions of skill.\textsuperscript{10}

The collection process employed by BGT provides a robust representation of hiring, including job activity posted by small employers. The process follows a fixed schedule, “spidering” a pre-determined basket of websites that is carefully monitored and updated to include the most current and complete set of online postings. BGT has developed algorithms to eliminate duplicate ads for the same job posted on both an employer website as well as a large job board by identifying a series of identically parsed variables across job ads such as location, employer, and job title. In addition, to avoid large fluctuations over time, BGT places more weight on large job boards than individual employer sites which are updated less frequently.\textsuperscript{11}

In the database provided by BGT, a snapshot of vacancies is reported on a monthly basis and are pooled over the year without duplication. This data is unique in allowing

\textsuperscript{9} See http://www.burning-glass.com/realtime/ for more details.\textsuperscript{10} Note that the BGT data do not contain any information on the duration of the vacancy, how many applications a vacancy received, nor whether a vacancy was filled.\textsuperscript{11} BGT has also provided access to their Labor/Insight analytical tool that enables us to access the underlying job postings to validate many of the important components of this data source including timeframes, de-duplication, and aggregation.
geographical analysis of occupation-level labor demand for a variety of skills including education and experience over time. Using the entire universe of job vacancies collected by BGT, allows us to expand the labor mismatch analysis to occupational level. The data are available for detailed occupation by Standard Occupation Code (SOC) down to the six-digit level for 2010 through end of 2015.\textsuperscript{12}

It should be noted that although Burning Glass Technologies consistently applies the same filtering and de-duplication algorithm across years, even retroactively as improvement are made, the number of sources scraped may have evolved over time. Figure 2 plots JOLTS vacancies and BGT ads at the national level. The total count of active vacancies in BGT is below that in JOLTS although the correlation between the two series is quite strong at about 0.85. To the extent that the trend in online vacancies is similar to that of JOLTS across sectors, our calculations should not be affected. Yet, there are also specific occupations which are underrepresented in all on-line job posting data. For example, construction jobs are not typically posted online. We utilize a re-weighting scheme introduced by BGT that adjust the total number of postings at the industry level by the number of monthly vacancies from JOLTS. It also utilizes other reliable sources of labor data such as quarterly workforce indicator (QWI), occupational employment statistics (OES), etc. to adjust the number of postings in cases that the number of aggregated BGT postings substantially differs from national trend. The reweighting process produces a BGT vacancy data series that is consistent and comparable with the JOLTS and other vacancy data series that use a similar weighting methodology such as HWOL.

\textsuperscript{12} We have aggregated the data to the two-digit occupational categories using the appropriate mappings from the 2010 SOC codes.
Using the weighted BGT series as our starting point, we then make use of the detailed occupation and skill measures contained in the BGT data to construct mismatch indices for two-digit occupations by education level. Specifically, we use the education requirements contained in the BGT data to calculate the education distribution each year by six-digit Standard Occupational Classification (SOC) categories and classify six digit occupations as high-, middle-, and low-skill. We then aggregate the number of vacancies in each skill category up to the two digit levels to match up with the supply side measures in our mismatch analysis. This method yields separate vacancy measures by skill level at the two digit occupation classifications that consist only of those detailed occupations within that occupation grouping that meet those specific education criteria, rather than labeling the whole occupation grouping into one of those buckets. We follow the same procedure to construct our supply-side measures using the Current Population Survey in the next section.

13 High-skill occupations are those where at least 40 percent of the vacancies require a Bachelor’s degree or higher, low-skill occupations are those where at least 40 percent of the vacancies require a High School degree or less, and middle-skill occupations are those where the education requirements lie somewhere in between or require an Associate’s degree or some college.
14 For example, vacancies within the two-digit Management occupation grouping (SOC 2010 = 11) would be allocated across high-, middle-, and low-skill levels according to the educational distribution for of the six digit occupations within it.
3.2 Constructing Other Variables: Unemployment, Hiring and Job Destruction Rates, and Productivity Measures

We use Current Population Survey (CPS) basic monthly data and applied person-level weights to estimate the monthly aggregate unemployment rates from January 2001 to December 2015.\textsuperscript{15} We construct estimates of unemployment and labor force counts by skill level for both industries as well as two-digit occupations. We also calculate aggregate unemployment rates for the same 17 industry category that we have the vacancy information from JOLTS to be able to replicate the results in Sahin et al. 2014 for comparison purposes.\textsuperscript{16}

We estimate the job finding rate for individuals who are surveyed in adjacent months of the CPS and use this proxy to calculate estimates of hires at the two-digit level for the occupation analysis. We use a similar strategy to calculate job destruction rate proxies at the occupational level. We use direct measure of hires and separations from the JOLTS data to calculate hiring and job destruction rates at the industry level.

Productivity measures at the industry level are calculated based on value added by employment level. Information on value added is gathered from the Business Employment Dynamics (BED) database.\textsuperscript{17} Data on employment levels is constructed from the

\textsuperscript{15} We utilize IPUMS CPS datasets for the supply side of our analysis. Please see \url{https://cps.ipums.org/cps/} for detailed documentation on variables.

\textsuperscript{16} We have created crosswalk to map 17 JOLTS industries to NAICS categories and Census industry categories which is used in the CPS data. Appendix 1 covers some of the mappings used in the analysis. In the CPS for persons who were employed at the time of the survey, IND relates to the industrial sector in which the respondent worked during the preceding week. For unemployed persons and those not currently in the labor force, IND characterizes the industrial sector of the respondent's most recent job. The CPS interviewer collected information by asking what kind of work the person was doing, and Census Bureau staff coded the information into the CPS or census industrial classification.

\textsuperscript{17} \url{https://www.bls.gov/bdm/}
Establishment Survey for the 17 industry categories in JOLTS. We use median wage data as the proxy for occupational productivity using Occupational Employment Statistics (OES) Survey.

4. Results

Below we describe our main results. The first set of results replicate the analysis of Sahin et al 2014 using the JOLTS and the BGT data and confirm that our method and data produce very similar findings, although extended through the full recovery period. The second set of results advance this literature by exploring whether mismatch varies by skill level using a refined method that takes advantage of the strength of the BGT data. Specifically, we use demand-side measures of skill (education) requirements derived from the BGT vacancy postings rather than relying on proxies derived from the education levels of incumbent workers as found in previous mismatch studies.

4.1 Aggregate Measures by Industry and Occupation

A. Correlation between Vacancy and Unemployment Shares

The planner’s allocation rule implies a perfect correlation between unemployment shares and vacancy shares with a correlation coefficient below one indicating mismatch. Similar to frictional unemployment, one would expect that the correlation coefficient is likely to be less than one as the matching process is dynamic and subject to inherent frictions in the labor market that prevent full and immediate adjustment. Nevertheless, this measure is still informative as long as we can interpret a declining correlation as a signal of worsening mismatch much as we interpret an increasing unemployment rate as a signal of deteriorating labor market conditions.
Following Sahin et al. (2014), Figure 3 plots the time series of this correlation coefficient across industries from 2001 through 2016 for the two different measures derived in Section II. These are \( \rho \): between \( \left( \frac{u_{it}}{u_t} \right) \) and \( \left( \frac{v_{it}}{v_t} \right) \) and \( \rho_x \): between \( \left( \frac{u_{it}}{u_t} \right) \) and \( \left( \frac{x_{it}}{x_t} \right)^\frac{1}{a} \left( \frac{v_{it}}{v_t} \right) \). Across industries, the two series behave similarly with a sharp drop from late 2006 to mid-2009 and improvement thereafter, indicating a temporary worsening of mismatch during the Great Recession. The correlation coefficient returns to its pre-recession peak near the end of 2014 for \( \rho \) and by the end of 2015 for \( \rho_x \).

Across occupations, the series can only be constructed from 2010 forward using the BGT data. Yet, Figure 4 shows a pattern that is similar to the industry correlations during this period where the correlation coefficient for \( \rho \) increases from 2010 through the end of 2014. The correlation for \( \rho_x \) is quite noisy at the occupation level but also rises through 2014, indicating that mismatch abated during this period. However, it should be noted that both correlation coefficients decline between 2014 and 2016, suggesting that mismatch across occupations may be on the rise again.

**B. Industry-Level Mismatch**

The top panel of Figure 5 plots \( M_t \) and \( M_{xt} \) across two-digit industries.\(^{18}\) This figure shows that, before the last recession (in mid-2006), the fraction of hires lost because of misallocation of unemployed workers across industries ranged from 5 to 6 percent per month, depending on the index used. In mid-2009, at the end of the recession, it had increased to roughly 11 to 13 percent per month, before dropping to its pre-recession level

\(^{18}\) All mismatch indexes throughout the paper are HP filtered to eliminate high frequency movements and better visualize the variation in the indexes.
by the end of 2014. To summarize, both indexes indicate a sharp rise in mismatch between unemployed workers and vacant jobs across industries during the recession, and a subsequent fairly rapid decline. Interestingly, our measures indicate a slight uptick in mismatch since 2014 perhaps due to the improvement in labor market conditions that has encouraged some workers who previously dropped out of the labor force to re-enter and begin seeking a job.

How much of the observed improvement in the unemployment rate can be explained by declining mismatch? The bottom panel of Figure 5 shows mismatch unemployment—the difference between the actual and the counterfactual unemployment rates—at the industry level for the 2001–2016 period, computed as described in Section 2B. Mismatch unemployment has declined steadily since early 2010, and was just slightly above pre-recession levels at the end of 2015. Table 1 shows the change in mismatch unemployment between December of 2010 and December 2015. The main finding is that improving mismatch across these 17 industries explains (depending on the index used) between 0.72 and 0.75 percentage points of the fall in US unemployment from 2010 to 2015, i.e., at most 16.7 percent of the decrease.

C. Occupation-Level Mismatch

Figure 6 plots the plots $M_t$ and $M_{xt}$ indices (top panels) and the resulting mismatch unemployment (bottom panels) for two SOCs for 2010 through 2016 (the period for which the BGT data is available). Both indices fall from 2010 through 2014. In general, the $M_{xt}$ index is lower than the $M_t$ index, However, the decline over time is steeper for the $M_{xt}$. 

Similar to the pattern observed for industries, both mismatch indices begin to rise again after 2014.

The indices suggest that between 0.62 and 0.87 percentage points of the decline in the unemployment rate was due to mismatch at the two-digit SOC levels. Similar to what we found for industries, the index that accounts for heterogeneity in matching and productive efficiency across occupations implies a smaller role for mismatch unemployment.

As seen in the figure and in Table 1, based on the $\mathcal{M}_t$ index, around 0.87 percentage points (or 19 percent) of the fall in US unemployment from December 2010 to December 2015 can be attributed to occupational mismatch measured at the two-digit occupation level.

4.2. Occupation Mismatch by Education Level

In this section we attempt to determine whether occupational mismatch a more relevant source of unemployment dynamics for less skilled or for more skilled workers? A priori, the answer is ambiguous: more education means more adaptability, but also more specialized knowledge. To address this question, we analyze mismatch by two-occupations within each of three education groups: high (bachelor’s degree or higher), medium (associate’s degree or some college) and low (high school degree or less).

We measure this for both the supply and demand side. On the supply side, as described above, the CPS provides information on the education level of the unemployed that can be disaggregated to the two-digit SOC groupings. Recall that each job listing captured by BGT reports its six-digit occupation as well as the education requirements associated with it. Using the more detailed nature of the BGT data, we are able to allocate the total count of vacancies from BGT in a given month for a given six-digit occupation to each of the three
education groups we consider based on the predominant education requirement listed in
the actual posting. We then aggregate up to the two-digit level to obtain vacancy counts
for each occupation by education cell. In doing so we are able to avoid a key limitation of
previous studies which were limited to proportionally assigning vacancies by occupational
categories using the educational attainment distributions from the BLS due to data
limitations associated with HWOL where the education level of the job posting was not
reported. Thus, earlier studies seeking to disaggregate the mismatch index by education
groups using HWOL necessarily assumed that the educational requirement of newly created
vacancies, for each occupation, was equal to the educational content in the existing jobs for
that same occupation. Yet recent studies have shown that this assumption did not hold
during the Great Recession with employer requirements increasing and then decreasing
over the business cycle in response to the availability of workers (Modestino et al. 2016,
Hershbein and Kahn 2016). As such, previous estimates of mismatch by education level that
were unable to account for changes in skill requirements within education over time are
likely to have produced underestimates of the true degree of mismatch in the economy over
the business cycle.

The counterfactual exercises summarized in Figure 7 reveal a clear pattern: the
degree of mismatch increases with the level of education required at the two-digit
occupational level. The level of mismatch is highest for high-skill workers (0.25-0.3) and
lowest for lowest skill category (0.05). This confirms the notion that workers in occupations

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19 High-skill occupations are those where at least 40 percent of the vacancies require a Bachelor’s degree or
higher, low-skill occupations are those where at least 40 percent of the vacancies require a High School
degree or less, and middle-skill occupations are those where the education requirements lie somewhere in
between or require an Associate’s degree or some college.
that require higher levels of education are likely to be more specialized and as a result, less substitutable across occupational categories—even at the two-digit level. For example, an individual with a bachelor's degree or higher working in the Architectural and Engineering occupational group is not likely to be able to switch costlessly to a job in another two-digit occupational group—even one that is somewhat related such as the Computer and Mathematical group.

Moreover, consistent with other evidence from the literature, changes over time in our measure of mismatch also vary by education group. For example, Acemoglu and Autor (2011) have argued that job polarization—the increasing concentration of employment in the highest-wage (non-routine) and lowest-wage (routine cognitive) occupations, with job opportunities in middle-skill (routine manual) occupations disappearing—is useful to interpret the long-run aggregate employment dynamics in the United States. In addition, Jaimovich and Siu (2012) extended this analysis to business cycle frequencies finding that most job destruction occurs among middle-skill occupations. Similarly, we find that mismatch among middle-skill occupations has fallen during the recent labor market recovery, dropping from around 0.1 in 2010 to 0.06 in 2016. In contrast, low-skill occupations exhibited little decrease in mismatch during this period while high-skill occupations have exhibited a slight upward trend.

As a result, Figure 8 shows that the degree to which mismatch contributes to the decline in the unemployment rate also varies considerably by education group. Among middle-skill occupations, the indices suggest that at its peak in 2010, as much as 1 percentage points was due to mismatch at the two-digit SOC levels and that unemployment due to mismatch has been almost abated by 2016. In contrast, mismatch among high-skill
occupations requiring a Bachelor’s or greater contributed upwards of 1.5 percentage points at the peak in 2011 and remains elevated with little decline since then. Among low-skill occupations, mismatch contributed more than 1.0 percentage point to unemployment in 2010 and has since declined.

Table 2 summarizes the result of mismatch unemployment estimates across skill levels. The contribution of declining occupational mismatch to the fall in unemployment between December 2010 and December 2015 is greater as we move from the lowest to the highest education category. In particular, for the Low Skill Occupations category, mismatch explains a little less than 0.46 percentage point (7 percent) of the 5.3 percentage point decline in the unemployment rate of that group. For Middle Skill Occupations, mismatch explains 0.52 (16 percent) out of the 3.3 percentage point decline in unemployment, and for High Skill Occupations, mismatch explains about 0.66 (21 percent) out of a 3.16 percentage point fall in unemployment. Also, the share of mismatch unemployment out of total unemployment has risen significantly from 31 percent to 47 percent for High Skill Occupations, however, it slightly dropped for Middle and Low Skill Occupations from 2010 to 2015.

How can these results be used to reconcile the puzzling observation in the literature that the Beveridge Curve appears to have shifted outwards but with little evidence of an increase in wages that would indicate a clear signal of mismatch in the labor market? Since the onset of the Great Recession, the Beveridge Curve has displayed a higher number of vacancies for a given level of unemployment, even as the labor market has recovered. Figure 9 draws the Beveridge Curve for each of our three occupation groups for the period 2010-2016. A notable distinction across education groups is that the slope of the Beveridge Curve
is correlated with education such that the high-skill occupations exhibit the steepest relationship and the low-skill occupations have the flattest. In addition, most of the improvement in the Beveridge Curve has come from movements in the curve among low- and middle-skill occupations which show large reductions in unemployment as the number of vacancies increased. In contrast, among high-skill occupations the reduction in unemployment has been much smaller relative to the number of vacancies created, possibly accounting for the persistent “wedge” that economists have observed in the aggregate Beveridge Curve during this period.

4.3. Mechanisms

In this section, we further exploit the detailed information in the BGT data to better understand the observed heterogeneity in labor market dynamics by education group. Aside from the aforementioned supply side issues regarding adjustment of high-skill individuals who may entail a greater degree of specialization, here we focus on demand side explanations. For example, it may be the case that the skill demands of employers are changing within occupations, such that even workers with experience in a given occupation are not fully-equipped to fill newer jobs in the same field—particularly among high-skill occupations. For example, Modestino et al. (2016) find that during the Great Recession a significant share of employers engaged in “upskilling”—raising the education requirements for a given job, in part to take advantage of the weak labor market. While they subsequently find that some of the previous upskilling was reversed as the labor market tightened in recent years, higher education requirements persisted for some occupations. This finding suggests that there may have been structural changes in the nature of some jobs in recent years perhaps related to concurrent investments in new technology or production processes.
We hypothesize that structural changes in skill requirements may be a potential mechanism that can account for the greater magnitude and more persistent mismatch that we observe among high-skilled occupations. To test this, we explore changes in employer skill requirements at the six-digit occupation level from 2007 through 2014, and distinguish between occupations where we observe a temporary increase in skill requirements versus those where these shifts have persisted as the labor market has recovered. Table 3 shows that across all occupations, 36.8 percent of occupations were “permanent” upskillers—defined here as having at least a 5 percentage point increase in the share of employers within that occupation requiring a Bachelor’s degree between 2007 and 2010 and less than a 5 percentage point decline between 2010 and 2014. In contrast, only 9.2 percent were “temporary” upskillers that had both an increase and a decrease in requiring a BA and 47.7 percent exhibited virtually no change in this requirement. Moreover, nearly three-fourths of high-skill occupations were permanent upskillers compared to only 40.1 percent of middle-skill occupations and only 7.7 percent of low-skill occupations.

Yet it may be the case that the observed increases in education requirements do not reflect structural changes in underlying skills associated with high-skill jobs but instead are a way for employers to screen for better candidates. To test this, Table 4 shows a difference-in-difference comparison between permanent and temporary upskillers for the recession versus the recovery period for both education and other skill requirements. Although permanent upskillers had higher initial shares of employers that required a Bachelors degree, temporary upskillers actually saw a five percentage point greater increase in education requirements between 2007 and 2010. Yet during the recovery period while
permanent upskillers increased the share of employers by an additional 2 percentage points, this share decreased by 11 percentage points for the temporary upskillers.

What’s even more striking is that this same pattern holds when we perform this analysis using other skill requirements from the individual job postings. Specifically, BGT parses each skill listed in the posting and classifies it as baseline (for example, generic skills such as leadership, project planning, and development), specialized (for example, information security), or software (for example, Adobe Dreamweaver). From this, we construct the share of postings requiring each type of skill. Interestingly, these more-detailed skill requirements exhibit the same upskilling and downskilling trends as the education requirements for both the baseline and specialized skill requirements—things that employers can potentially train workers to develop with a stand-alone course or module. However, a different pattern emerges for software skills with both permanent and temporary upskillers experiencing a large increase in the share of employers requesting these skills that does not reverse itself during the recovery.

5. Conclusions

In this paper, we extend the framework developed by Sahin et al. (2014) to measure mismatch unemployment since the end of the Great Recession and explore the heterogeneity among educational groupings. We use this framework to ask how much the abatement in sectoral mismatch contributed to the recovery of US unemployment around the Great Recession. Our findings indicate that mismatch across two-digit industries and two-digit occupations explains 16.7 and 19.6 percent of the recent recovery in the US unemployment rate since 2010 respectively.

Although this measure still should be considered as an upper bound for each level
of disaggregation we analyzed, several refinements suggest that we have reduced the measurement error associated with disaggregating by educational groupings. Most notably, we are able to capture movements in employer education requirements over time using the BGT data that was previously impossible in earlier studies using less detailed data. Our findings reinforce those of Sahin et al. 2014 and further show that mismatch is not only greater in magnitude for high-skill occupations but also is more persistent over the course of the recovery, possible accounting for the shift rightward that has been observed in the aggregate Beveridge Curve by other researchers.

Furthermore, we attempt to make use of the more detailed BGT data to shed light on at least one of the potential causes of mismatch on the demand side. The finding that occupational mismatch plays a nonnegligible role, especially for higher-skill workers, can potentially be explained by some combination of labor demand shifts with human capital specialization, relative wage rigidity, and government policies. We find evidence of labor demand shifts among high-skilled occupation groups that exhibit a permanent increase in the share of employers requiring a Bachelor’s degree. Moreover, this increase in education requirements is correlated with concurrent increases in baseline, specialized, and software skills listed on job postings, suggesting a role for structural shifts associated with changes in technology or capital investment.

These findings have important implications for both the economics literature as well as labor market policy. Regarding the literature, our results demonstrate that equilibrium models where unemployed workers accumulate specific human capital and, in equilibrium, make explicit mobility decisions across distinct labor markets, can be chasing a moving target—at least among high-skilled occupations (Kambourov and Manovskii 2009; Alvarez
and Shimer 2011; Carrillo-Tudela and Visscher 2013; and Wiczer 2013). Going forward, these frameworks can be modified to investigate the structural causes of mismatch unemployment and uncover the dynamics causing job seekers search for work in the wrong sectors.

In terms of policy-making, the characteristics of occupations experiencing more permanent shifts can point to the potential structural forces underlying these observed trends. To that end, our findings can inform debates focused on workforce development strategies and related educational policies where decision making could benefit from the use of real-time labor market information on employer demands to provide guidance for both job placement as well as program development.
References


Mukoyama, Toshihiko, Christina Patterson, and Aysegul Sahin. 2014. "Job search behavior over the business cycle," Staff Reports 689, Federal Reserve Bank of New York.


Figure 1: Beveridge Curve (2001 through August 2016)

Source: Authors’ calculations using vacancy data extracted from JOLTS covering January 2001 to August 2016. Monthly unemployment and labor force estimates are from the Bureau of Labor Statistics.
Figure 2. Trend in Total Job Vacancies for BGT versus JOLTS, 2010-2016.

Source: Authors’ calculations using monthly vacancy data extracted from JOLTS and aggregate monthly Job postings from BGT covering January 2010 to December 2015.
Figure 3. Industry Level Correlation

Source: Authors’ calculations using disaggregated industry-level vacancy data extracted from JOLTS and industry-level monthly unemployment and labor force estimates from CPS covering January 2001 to December 2015
Figure 4  Occupational Level Correlation

Source: Authors’ calculations using occupation-level monthly job postings data from BGT and occupation-level monthly unemployment and labor force estimates from CPS covering January 2010 to December 2015
Figure 5. Industry Level Mismatch Indices and Unemployment Rates

A. Mismatch Indices

B. Unemployment Rates

Source: Authors’ calculations using disaggregated industry-level vacancy data extracted from JOLTS and industry-level monthly unemployment and labor force estimates from CPS covering January 2001 to December 2015
Figure 6. Two Digit Occupation-Level Mismatch

A. Mismatch Indices

B. Unemployment Rates

Source: Authors’ calculations using occupation-level monthly job postings data from BGT and occupation-level monthly unemployment and labor force estimates from CPS covering January 2010 to December 2015
Figure 7. Occupational Mismatch by Education Groups (2 Digit SOC Level)

A. High Skill Occupations

B. Middle Skill Occupations

C. Low Skill Occupations

Source: Authors’ calculations using occupation-level monthly job postings data from BGT and occupation-level monthly unemployment and labor force estimates from CPS covering January 2010 to December 2015
Figure 8. Unemployment Mismatch by Education Groups (2 Digit SOC Level)

A. High Skill Occupations

B. Middle Skill Occupations

C. Low Skill Occupations

Source: Authors’ calculations using occupation-level monthly job postings data from BGT and occupation-level monthly unemployment and labor force estimates from CPS covering January 2010 to December 2015
Figure 9. Beveridge Curve by Education Groups (2-Digit SOC Level)

A. High-Skill Occupations

B. Middle-Skill Occupations

C. Low-Skill Occupations
### TABLE 1- Changes in Mismatch Unemployment At Industry and Occupation Level

<table>
<thead>
<tr>
<th>Index</th>
<th>$u_{10} - u_{10}^*$</th>
<th>$u_{15} - u_{15}^*$</th>
<th>$\Delta(u - u^*)$</th>
<th>$\frac{\Delta(u - u^*)}{\Delta u}$ (percent)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Industry</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$M$</td>
<td>1.35</td>
<td>0.6</td>
<td>-0.75</td>
<td>16.7</td>
</tr>
<tr>
<td>$M_x$</td>
<td>1.2</td>
<td>0.48</td>
<td>-0.72</td>
<td>16.1</td>
</tr>
<tr>
<td>2-digit occupation</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$M$</td>
<td>1.82</td>
<td>0.95</td>
<td>-0.87</td>
<td>19.6</td>
</tr>
<tr>
<td>$M_x$</td>
<td>0.97</td>
<td>0.35</td>
<td>-0.62</td>
<td>13.9</td>
</tr>
</tbody>
</table>

Source: Authors calculations using CPS and BGT. Note that $\Delta u = 4.47$. All calculations are monthly.

### TABLE 2- Changes in Mismatch Unemployment across Two Digit Occupations for Different Skill Groups

<table>
<thead>
<tr>
<th>Skill Group</th>
<th>$u_{10} - u_{10}^*$</th>
<th>$\frac{u_{10} - u_{10}^*}{u_{10}}$ (percent)</th>
<th>$u_{15} - u_{15}^*$</th>
<th>$\frac{u_{15} - u_{15}^*}{u_{15}}$ (percent)</th>
<th>$\Delta(u - u^*)$</th>
<th>$\Delta u$</th>
<th>$\frac{\Delta(u - u^*)}{\Delta u}$ (percent)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Low Skill Occupations</td>
<td>0.87</td>
<td>7</td>
<td>0.46</td>
<td>6</td>
<td>-0.46 ppts</td>
<td>-5.38 ppts</td>
<td>7</td>
</tr>
<tr>
<td>Middle Skill Occupations</td>
<td>0.94</td>
<td>14</td>
<td>0.41</td>
<td>12</td>
<td>-0.52 ppts</td>
<td>-3.33 ppts</td>
<td>16</td>
</tr>
<tr>
<td>High Skill Occupations</td>
<td>1.48</td>
<td>31</td>
<td>0.82</td>
<td>47</td>
<td>-0.66 ppts</td>
<td>-3.16 ppts</td>
<td>21</td>
</tr>
</tbody>
</table>

Source: Authors calculation using CPS and BGT. Note that $\Delta u$ varies by skill level.
Table 3. Share of Occupations that were Permanent vs. Temporary Upskillers

<table>
<thead>
<tr>
<th></th>
<th>Percent of Occupations that Engaged in:</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Permanent Upskilling</td>
</tr>
<tr>
<td>All Occupations</td>
<td>36.8%</td>
</tr>
<tr>
<td>Low-Skill Occupations</td>
<td>7.7%</td>
</tr>
<tr>
<td>Middle-Skill Occupations</td>
<td>40.1%</td>
</tr>
<tr>
<td>High-Skill Occupations</td>
<td>73.0%</td>
</tr>
</tbody>
</table>

Source: Author’s calculations using data on online job vacancies from Burning Glass Technologies for 2007, 2010, and 2014.

Note:
Permanent upskilling is defined as having a 5PP or greater increase in share of employers requiring a BA between 2007 and 2010 and having less than a 5PP decrease in that share between 2010 and 2014.
Low-Skill occupations are defined as those employing at least 40% of workers with a HS education or less according to the 2007-07 combined American Community Survey.
Shares are weighted by the occupation’s share of total employment as of 2006.
Table 4. Difference-in-Difference Analysis of Changing Skill Requirements

<table>
<thead>
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<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Note: Permanent upskilling is defined as having a 5PP or greater increase in share of employers requiring a BA between 2007 and 2010 and having less than a 5PP decrease in that share between 2010 and 2014. Low-Skill occupations are defined as those employing at least 40% of workers with a HS education or less according to the 2007-07 combine American Community Survey</td>
</tr>
</tbody>
</table>
### Appendix (1) – Industry Crosswalk:

**Table 2**

<table>
<thead>
<tr>
<th>Industry Categories</th>
<th>JOLTS</th>
<th>NAICS</th>
<th>Census 2003 to 2015</th>
</tr>
</thead>
<tbody>
<tr>
<td>Mining and Logging</td>
<td>NAICS 1133—Logging, Sector 21—Mining</td>
<td></td>
<td>370-490,270</td>
</tr>
<tr>
<td>Construction</td>
<td>Sector 23—Construction</td>
<td></td>
<td>770</td>
</tr>
<tr>
<td>Durable Goods</td>
<td>NAICS 321, 327, Sector 33</td>
<td></td>
<td>2470–3990</td>
</tr>
<tr>
<td>Manufacturing</td>
<td>Sector 31, NAICS 322, 323, 324, 325, 326</td>
<td></td>
<td>1070–2390</td>
</tr>
<tr>
<td>Wholesale Trade</td>
<td>Sector 42—Wholesale Trade</td>
<td></td>
<td>4070–4590</td>
</tr>
<tr>
<td>Retail Trade</td>
<td>Sectors 44 and 45—Retail Trade</td>
<td></td>
<td>4670–5790</td>
</tr>
<tr>
<td>Transportation</td>
<td>Sectors 48 and 49—Transportation and Warehousing, Sector 22</td>
<td></td>
<td>6070-6390, 570–</td>
</tr>
<tr>
<td>Utilities</td>
<td>Utilities</td>
<td></td>
<td>0690</td>
</tr>
<tr>
<td>Information</td>
<td>Sector 51—Information</td>
<td></td>
<td>6470–6780</td>
</tr>
<tr>
<td>Finance and Insurance</td>
<td>Sector 52—Finance and Insurance</td>
<td></td>
<td>6870–6990</td>
</tr>
<tr>
<td>Real Estate and Rental and Leasing</td>
<td>Sector 53—Real Estate and Rental and Leasing</td>
<td></td>
<td>7070–7190</td>
</tr>
<tr>
<td>Professional and Business Services</td>
<td>Sector 54, 55,56—Professional, Scientific, and Technical Services</td>
<td></td>
<td>7270–7790</td>
</tr>
<tr>
<td>Educational Services</td>
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Chapter 2 – The Changing Impacts of Cognitive Ability on Determining Earnings of College Bound and Non-College Bound Young Adults

1. Introduction

One of the most publicized labor market trends in the United States has been the wage gaps between college bound and non-college bound workers. These earnings gaps rose particularly rapidly during the 1980s (Gottschalk, 1997; Katz and Autor, 1999; Katz and Murphy, 1992; Levy and Murnane, 1992). Afterwards, the pace of growth in education-related wage differentials slowed progressively and the wage gaps across these education groups have changed little since the year 2000 (Lindley and Machin, 2016; Valletta, 2016).

Many of the interpretations and explanations proposed for these trends focus on the changing role of demand for cognitive skills in determining labor market outcomes. Several recent studies suggest that pre- and post-2000 patterns in technology investment and its consequent boom-and-bust cycles in the demand for cognitive skills are at least part of the explanation for these trends (Beaudry et al., 2016; Castex and Dechter, 2014; Gordon, 2014). This study addresses the following research questions: To what extent cognitive

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20 Several studies attribute the rise in the education-related wage differentials during the 1980s to an increase in demand for unobserved skills. For example, Taber (2001) mentions the two most salient characteristics of the decade as (1) a large increase in the college/high school wage differential, and (2) a substantial rise in the variance of wage residuals and argues that the rise in variance of wage residuals in the 1980s is attributable to an increase in the demand for unobserved skill. He finds evidence that suggests an increase in the demand for unobserved ability could play a major role in the growing college premium (see also Chay and Lee 2000).

21 Lindley and Machin (2016) provide evidence that confirms this departure from the earlier pattern and the slowdown in the 2000s, where the college only/high school wage gap increases by only 0.050 log points as compared to increases of 0.139 and 0.083 in the 1980s and 1990s respectively.

22 The dynamics is based on a model in which secular changes in demand for cognitive skills can be explained as a boom-and-bust cycle caused by lifecycle path of technological development from adoption to maturation. This allows for the relationship between technology and skill demands to vary over time and thus runs contrary to the prospect that technology monotonically leads to greater cognitive skill requirements (see Beaudry et al., 2016).
skills pay off in the labor market and account for variations in wages? How significant is the role of the basic cognitive skills in explaining the wage variations for college bound and non-college bound young adults? More importantly, is this role constant or has it perhaps changed in time? Are pre-market cognitive skills becoming less important in determining wages of both college-bound and non-college-bound workers on an economy-wide basis?

To answer these questions, I examine the relationship between wages and measured cognitive ability using the 1979 and 1997 NLSY panel data classified into nationally representative samples of college bound and non-college bound young adult workers (aged 18–31 years) not enrolled in school. I conduct my analysis separately for men and women irrespective of race and also for race irrespective of gender.

In my analysis, I use Armed Forces Qualification Test (AFQT) scores, a composite derived from ASVAB subtests of Arithmetic Reasoning, Word Knowledge, Paragraph Comprehension, and Numerical Operations, to proxy for cognitive ability. These four subtests, which loads heavily on IQ, comprise only a subset of the full set of test components taken by respondents embodied in the ASVAB. It must be stressed at the outset that standardized achievement tests are not the same as a tests of ability. However, as Dickens (2008, page 867) notes, scores on ability tests and standardized achievement tests are strongly correlated with each other:

“It is common to draw a distinction between tests of achievement and tests of ability. Achievement tests measure how much knowledge the test taker has

23 Psychologists distinguish between fluid intelligence (the rate at which people learn) and crystallized intelligence (acquired knowledge). Achievement tests are designed to capture crystallized intelligence whereas IQ tests are designed to capture fluid intelligence (see, e.g., Nisbett, Aronson, Blair, Dickens, Flynn, Halpern, and Turkheimer, 2012).
accumulated in a particular area while ability tests endeavour to measure how quickly a person can solve unfamiliar problems. Typically, scores on the two types of tests are highly correlated. In fact, all tests of ability are, to some degree, tests of achievement as it is impossible to measure ability without also measuring the test taker's reading or verbal comprehension at least."

The model underlying my empirical estimations views measure of cognitive skills as a direct measure of human capital. My estimation results indicate that across the two schooling groups, cognitive ability plays a substantially diminished role for the most recent cohort and its impact on wage determination has undergone a drastic change between 1980s and 2000s. To explore the robustness of my results to the impact of differences in the distributions of age, family background and occupational-industrial-specific characteristics across cohorts, I choose NLSY79 as the reference cohort and reweight the NLSY97 (younger cohort) to match the distributions of characteristics of interest in NLSY79 (older cohort). The cognitive-ability-earnings associations prove immensely robust under all specifications.

In the following, Section 2 introduces the NLSY data. Section 3 presents the empirical framework and estimation results. Section 4 describes the robustness check method and presents results of the sensitivity analysis. Section 5 concludes.

2. Data

The primary requirement for my empirical analysis is a set of data on comparable cognitive ability measures linked to subsequent labor market information across panel surveys that longitudinally track individuals over time. I use the data from the 1979 and

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24 This approach is closely related to the one adopted in Neal and Johnson (1996), Murnane et al. (2000) and Hanushek et al. (2015).
1997 cohorts of the National Longitudinal Survey of Youth (NLSY) which meets this requirement. The NLSY79 is a nationally representative panel survey of 12,686 young men and women who were between the ages of 14 and 22 in 1979. The NLSY79 is made up of three subsamples: The first is a core cross-sectional representative sample of 6,111 noninstitutionalized men and women; the second is a supplemental sample of 5,295 Hispanics, blacks, and economically disadvantaged whites; and the third is a sample of 1,280 military respondents. The NLSY97 is a survey of youths born between 1980 and 1984. At the time of first interview, respondents’ ages ranged from 12 to 18. The NLSY97 is comprised of two subsamples: a core cross-sectional representative sample of 6,748 respondents and a supplemental sample of 2,236 respondents designed to oversample minorities. In my empirical analysis, I utilize both cross-sectional and supplemental samples in the NLSY79 and NLSY97; I exclude military and poor white subsamples of NLSY79 (not available in the NLSY97). These panel surveys collect information on cognitive test scores, schooling decisions, labor market experiences, race and ethnicity, family and parental background (including parental education), and geographical indicators for urban or metropolitan residence. Many of these measures are congruent across the two cohorts, but some need further adjustments to become comparable across samples.

The data analysis and estimations are carried out separately for college bound and non-college bound young adults (across subsamples of gender and race) and excludes those who are enrolled in school, those who work less than 20 hours per week, and those who have reported real hourly wages less than $3 or greater than $100 (deflated by the Bureau of Labor Statistics CPI and expressed in 2007 prices). Since during the latest wave of data available at the time of this study, the oldest individuals in the NLSY97 turned age 31, I
restrict my analysis to young adults age 18 to 31 years. For purposes of this study, 23-year-olds whose Highest Grade Completed was less than grade 14 (second year college) or equivalently whose educational attainment (Highest Degree Received) was at a level lower than junior college or 2-year associate degree were classified as non-college bound group. Correspondingly, for college bound group, we adopt the criterion of the completion of at least two years of college (as of age 23) as the classifier and measure of college going. For the individuals in the sample, college attendance decisions took place in the early 1980s for the 1979 cohort and in the early 2000s for the 1997 cohort. The data reveal that a slightly higher number of males than females were classified as non-college bound, while females were more likely to be college-educated.

The NLSY79 and NLSY97 are unique in that virtually all respondents took the Armed Forces Vocational Aptitude Battery (ASVAB), which is a multiple-choice test used to determine qualification for enlistment in the U.S. armed forces. In this analysis, I use Armed Forces Qualification Test (AFQT) scores, a composite derived from ASVAB subtests of Arithmetic Reasoning, Word Knowledge, Paragraph Comprehension, and Numerical Operations, to proxy for cognitive ability\textsuperscript{25}. These four subtests, which loads heavily on IQ, comprise only a subset of the full set of test components taken by respondents embodied in the ASVAB.\textsuperscript{26}

\textsuperscript{25}The AFQT is often used as proxy for cognitive ability (see for example, Herrnstein and Murray, 1994, Murnane et al., 1995, Neal and Johnson, 1996, Hanushek and Woessmann, 2008, Altonji et al., 2012, Castex and Dechter, 2014). Borghans et al. (2016) list 50 papers that use AFQT scores as proxies for intelligence. They also employ a variety of datasets (including NLSY79) and show that achievement tests (such as the AFQT in NLSY79) are substantially better predictors of important life outcomes than IQ. They point out that the reason for this is that achievement tests capture personality traits that have independent predictive power beyond that of IQ.

\textsuperscript{26}ASVAB contains 10 components that measure knowledge and skill in the following areas: (1) General Science Knowledge, measures knowledge of the physical and biological sciences; (2) Arithmetic Reasoning, tests the ability to solve arithmetic word problems; (3) Word Knowledge, is designed to evaluate the capacity to select
The NLSY79 and the NLSY97 cohorts differ as to both the ASVAB achievement test format and age-at-test. The NLSY79 test is pencil and paper (P&P) while the NLSY97 test follows a computer adaptive design (CAD). Furthermore, ASVAB was administered at different ages to the respondents of both surveys. To account for these differences and achieve comparability across cohorts, I carry out a two-stage adjustment procedure. First, I adjust the NLSY97 raw scores to obtain cross-cohort comparable and format-consistent AFQT test scores. To this end, I make use of the score mapping approach developed by Segall (1997) which is based on equating percentiles on the two tests for a sample of test takers who were randomly assigned to take either the P&P or the CAT test. Second, I adjust for the differing distributions of test-taking age across cohorts and, by considering the observed overlap in these distributions and exploiting the fact that both surveys have a large group that took the test at age 16, I perform an equipercentile mapping to age 16 of the scores of respondents who took the test at other ages (Figures 1-3).

Tables 1a and 1b present summary statistics for key variables used in this study. These statistics point to a number of developments across education groups and over time from the early 1980s to the early 2000s. Increases in schooling are not uniform between education groups and over time. Average years of schooling have been rather flat for both

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27 To implement these adjustments, I use the crosswalk provided by Altonji et al. (2012). This crosswalk is available at the following url: http://www.econ.yale.edu/~fl88/data.html.

28 To achieve population representativeness, the statistics are computed using the Bureau of Labor Statistics standard NLSY79 and NLSY97 cross-sectional weights.
men and women across cohorts in non-college bound category. In contrast, among the college bound, average attainment levels for both men and women have increased across cohorts, though females have slightly gained more than males have over the period. Cross-cohort variations in pre-market skill correlates (AFQT, Verbal and Math scores\(^{29}\)) remain small in both education categories. In college bound group, men and women are about equal in average AFQT scores in early 1980s, but by early 2000s men exceed women in this skill dimension. In non-college bound group, women exceed men in average AFQT scores in both NLSY79 and NLSY97, but cross-cohort changes in average AFQT scores for men and women go in opposite directions, being positive for men and negative for women. The average potential labor market experience, defined as age minus years of schooling minus six, is slightly lower for NLSY97 cohort in both education groups. Average real hourly wages (indexed to 2007 prices) for college bound is more or less constant over time but its dispersion has increased. For non-college bound, real hourly wages have declined across cohorts for men and women. Average parental education increased across NLSY cohorts in both education groups and more substantially in college bound category. Average family income\(^{30}\) is fairly constant over time but its dispersion has increased. Finally, in the college bound sample, the proportions of black male and female workers stay roughly constant across cohorts and consistently higher for females than for males. In the non-college bound group, the corresponding proportions are equal for males and females in 1979 cohort, while

\(^{29}\) I define the Math score as the mean of the Mathematics Knowledge, Numerical Operations and Arithmetic Reasoning tests; Verbal as the mean of Word Knowledge and Paragraph Comprehension.

\(^{30}\) For NLSY79 and NISY97 I use average family income (denominated in year 2007 dollars using CPI for all urban consumers) when respondents were ages 16-17, excluding those not living with their parents at these ages.
these proportions increase over time and across cohorts, with black females exceeding black males.

3. Empirical Framework and Estimation

The framework of my empirical analysis is based on a model in which the natural log of real individual earnings (y) in a given time period is decomposed into an additive function of a linear cognitive ability term, a quadratic experience term and a vector of other (potentially) time-varying factors. To assess and estimate the impact of cognitive ability, as captured by performance on AFQT tests, on wage determination of young adults and its evolutions between 1980s and 2000s, I use panel data from the National Longitudinal Survey of Youth 1979 and 1997, dichotomized into two schooling groups (college bound versus non-college bound) and separated by gender and race. For either of the two cohorts (and the sample groupings thereof), I employ nearly identical estimation specifications of the general form

$$\ln y_{it} = \gamma A_i + \beta_1 E_{it} + \beta_2 E_{it}^2 + X'_{it} \delta + \varepsilon_{it}$$

where $y_{it}$ denotes the real hourly wage rate of individual $i$ at time $t$, $A_i$ refers to cognitive ability as measured by AFQT score, $E_{it}$ is years of potential labor-market experience (age minus years of schooling minus six)$^{31}$, $X_{it}$ is a vector of individual and family background characteristics cumulative to time $t$, and $\varepsilon_{it}$ is a person-time-specific error term summarizing all unobserved factors that influence wages.$^{32}$

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$^{31}$ This assumes that work experience is continuous and starts immediately after completion of schooling. Thus, potential work experience is taken as equal to current age minus age at completion of schooling (Mincer, 1974).

$^{32}$ Given the structure of our pooled panel datasets, we have to relax the assumption of independent and identically distributed errors, allowing for arbitrary correlation between errors within individual-specific clusters of observations (i.e. the observations associated with each individual in our panel datasets). In other words, by clustering the data and assuming that observations from different clusters are uncorrelated, errors...
This specification is an empirical analog to a Mincer human capital earnings function except that it takes the measure of cognitive ability as a direct measure of human capital, while by grouping individuals into homogeneous schooling categories it attempts to avoid the self-selection in education bias [Willis and Rosen (1979)].

The cognitive ability coefficient, $\gamma$, provide an estimate of the rate of return to cognitive ability which is assumed to be constant in this specification. As in Mincer wage equation, the slope and concavity of the observed log earnings-experience profile is captured by the quadratic form of the experience variable, whose coefficients, $\beta_1$ and $\beta_2$, are respectively positive and negative [Willis (1986)].

Table 2 presents the estimated effect of cognitive ability (AFQT) on log wages across NLSY cohorts for college bound and non-college bound young adults by gender. The results indicate that there were statistically significant marked differences across cohorts, which suggests there was a secular decline in the estimated returns to cognitive ability between 1980s and 2000s by education categories.

Overall, the magnitude of the decline in the returns to cognitive ability across cohorts for college bound and non-college bound young adults are 47% and 50% respectively. For college bound male workers an increase in the AFQT score by one standard deviation is

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Griliches (1977) in his Econometric Society Presidential Address discusses at length the issue of endogeneity of schooling in the Mincerian wage equation and illustrates "the possibility that schooling and the disturbance in the earnings function may be negatively correlated, leading to a downward bias in the usual least squares estimates of the schooling coefficient...".

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Since the cognitive ability measure has been rescaled to have a mean of zero and a standard deviation of one, the parameter of interest $\gamma$, can be interpreted as the percentage increase in earnings associated with a one-standard-deviation increase in measured cognitive ability.
associated with a 16.0% increase in real hourly wage rate for the 1979 cohort but only with
9% increase for 1997 cohort. For non-college-bound male workers, the effect of one standard
deviation increase in AFQT score on the real wage rate drops from 12.0 % to 5.0 %. Thus, for
men, returns to cognitive skills across cohorts have declined by 43.8% for college bounds
and by 58.0% for non-college bounds. Correspondingly, for women, the effect of one
standard deviation increase in AFQT score on the real wage rate drops from 18.0% to 9.0%
for college bounds and from 12.0% to 8.0% for non-college bounds. Therefore, for women
returns to cognitive skills across cohorts have declined by 50.0% and 33.3% for college
bounds and non-college bounds respectively.

The same pattern is revealed in Table 3 which presents the estimation results by race.
Again we observe a statistically significant secular decline and striking cohort differences in
the estimated returns to cognitive ability for both college-bound and non-college-bound
young adults, albeit the magnitude of the decline for the college bound young adults is
noticeably smaller for black workers (-11.7% for black workers as compared to -55.5% for
white workers). One possible explanation for this could be the finding that educational
attainment, conditional on AFQT, is higher among blacks than it is among whites. A cross-cohort comparison of AFQT coefficients by education categories also shows a
consistent pattern of lower skill prices for non-college bound workers (relative to college
bound workers) implying complementarity between education and ability.

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35 The differences in the cognitive ability coefficients across cohorts are statistically significant at the 1% level for
black and white workers within college bound and non-college bound education categories.
36 Lang and Manove (2011) put forward the hypothesis that education is generally a more valuable signal of
productivity for blacks than for whites and, as a result, blacks have incentives to overinvest in education, compared to
equivalently able whites. Using data from 1979 National Longitudinal Survey of Youth (NLSY79), they find that, for
most levels of the AFQT score, blacks have higher educational attainment than similarly skilled whites.
Given the empirical relevance of concerns from omitting predictors that are related to cognitive ability and to earnings, I also controlled for a number of additional family inputs and individual-industry-specific characteristics as part of the robustness checks. The inclusion of additional control variables does not change the overall pattern of the secular decline of returns to cognitive ability across NLSY cohorts. Adjustments attenuate AFQT coefficients, but cross-cohort differences remain statistically significant between the 1980s and 2000s under all specifications.

4. Returns to Cognitive Ability: A Counterfactual Robustness Check

To explore the robustness of my results to the impact of differences in the distributions of age, family background and occupational-industrial-specific characteristics across cohorts, I choose NLSY79 as the reference cohort and reweight the NLSY97 (younger cohort) to match the distributions of characteristics of interest in NLSY79 (older cohort).37

The weighting procedure I implement is an adaptation of the semiparametric method developed by DiNardo, Fortin, and Lemieux (1996) to examine the effects of institutional and labor market factors on changes in the U.S. wage distribution over time.

For either of cohorts (NLSY79 or NLSY97), the overall wage density \( f(w_{\text{cohort}}) \) can be written, à la DiNardo-Fortin-Lemieux (1996), in terms of the conditional wage densities, where conditioning is on a set of observed background characteristics, \( x \), whose effects on earnings are to be analyzed:

37 While both data sources contain comparable measures of demographics and occupational-industrial-specific characteristics, the distributions of key covariates across birth cohorts are substantially different. By reweighting the NLSY97 to match the distribution of covariates in NLSY79, I intend to circumvent these incompatibilities and examine if and how such differences in distributions of age, family background as well as shifts in the composition of industries and occupations across cohorts may have potentially affected my results.
\[ \int f(w^{79})dw \equiv \int_X f(w^{79}|x) h(x|79)dx \]  
(1)

\[ \int f(w^{97})dw \equiv \int_X f(w^{97}|x) h(x|97)dx \]  
(2)

These expressions simply indicate that, for either of cohorts, density of wages can be recovered by the density of wages conditional on a set of individual attributes integrated over the distribution of individual attributes.

In this robustness analysis, the counterfactual of interest is one that mixes the observed conditional density for 1997 cohort with the observed characteristics distribution for 1979 cohort.\(^ {38} \)

\[ f(w^{97}|79) \equiv \int_X f(w^{97}|x) h(x|79)dx \]  
(3)

The counterfactual thus defined differs from its actual counterpart only in the set of \( x \) variables that are to be “integrated over.”

Here, an important underlying assumption is that the distribution of the unobservables conditional on the observable characteristics is the same for the two cohorts. More formally, **Conditional Independence Assumption**: Let \( D_g \) denote an indicator variable for cohort, and \( \epsilon \) denote unobservables that affect wages conditional on \( X \). Also let \( (D_g, X, \epsilon) \) have a joint

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\(^{38}\) As in DiNardo et al. (1996), I assume that the 1997 cohort’s structure of wages does not depend on the distribution of \( x \)’s, that is, the general equilibrium effects of changes in the distribution of \( x \) on the structure of wages are ignored. To the extent to which the underlying variable \( x \) cannot be regarded as exogenous, such hypothetical unobservable constructs should be interpreted with caution. For instance, some cohort-specific labor market observed characteristics such as rising unemployment or falling female labor supply might lead to changes in the wage structure.
distribution. For all $x$ in $X$: $\epsilon$ is independent of $D_g$ given $X = x$ or, equivalently, $D_g \perp \epsilon | X$ (Fortin et. al. 2011)\(^{39}\).

The estimation of the counterfactual density [Eq. (3) above] can be made simple by noting that Bayes’s Law implies,

\[
h(x) = \frac{h(x|97)P_{97}}{P(97|x)}
\]

\[
h(x) = \frac{h(x|79)P_{79}}{P(79|x)}
\]

Equating these two expressions and rearranging we get,

\[
h(x|79) = \frac{h(x|97)P(79|x)P_{97}}{P(97|x)P_{79}}
\]

Or equivalently,

\[
h(x|79) = \frac{h(x|97)P(79|x)P_{97}}{[1 - P(79|x)]P_{79}}
\]

We do not observe the counterfactual hypothetical construct, Eq. (3), but we can easily compute it. Using Eq. (7) we can rewrite Eq. (3) as

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\(^{39}\)This implies that any dissimilarity between the cohorts as to the marginal distribution of unobserved factors ($\epsilon$) can be taken care of by dissimilarities between the cohorts in the distribution of observed correlates ($x$). More formally, $g(\epsilon|x, 1979) = g(\epsilon|x, 1997)$, where $g(\epsilon|x, 1979)$ and $g(\epsilon|x, 1997)$ are the conditional densities of $\epsilon$ given $x$ for the 1979 and the 1997 cohorts, respectively. As Fortin et al. (2011) note in their handbook survey article on decomposition, differences in the distribution of the $\epsilon$ are fairly constrained under the conditional independence assumption. The conditional distribution of $\epsilon$ has to be the same for the two cohorts, although due to differences in the distribution of background observed covariates the unconditional distribution of $\epsilon$ may differ between 1979 and 1997 cohorts. Obviously, given the unobservability of $\epsilon$, there is no way to directly test this assumption. However, as Altonji et.al. (2012) point out, factors such as unobserved differences in school quality, neighborhood and family contexts or behavioral responses to differences in skill prices across 1979 and 1997 cohorts as well as changes in compulsory schooling laws, college tuition subsidies, or race and gender discrimination might lead to the violation of the assumption.
\[ f(w^{97}|79) = \int_x f(w^{97}|x) h(x|97) \frac{P(79|x)P_{97}}{[1 - P(79|x)]P_{79}} dx \] (8)

Note that the counterfactual (8) and the factual (2) distributions are similar up to a factor \( \psi(x) \) where

\[ \psi(x) = \frac{P(79|x)P_{97}}{[1 - P(79|x)]P_{79}} \] (9)

That is, the distribution of 1997 cohort’s background characteristics appropriately weighted is equal to the distribution of 1979 cohort’s background characteristics. We can ignore the term \( \frac{P_{97}}{P_{79}} \) which is a constant for all observations and therefore does not change the relative weighting of the data.

I implement the reweighting procedure (8) and counterfactual robustness check as follows:

1) I pool the data from the NLSY cohorts (1979 and 1997)\(^4\) and split pooled dataset into education categories (college bound vs non-college bound).

2) For each pooled dataset by education category, I run the probit model for the probability belonging to NLSY79 cohort and estimate the propensity score \( P(79|x) \) using the measures of interest (age, family background, industrial-specific characteristics), \( x \), that are observed for both cohorts (NLSY79 and NLSY97)

\(^4\) To achieve population representative samples, I use the sampling weights provided by Bureau of Labor Statistics for the NLSY79 and NLSY97.
3) I generate the sets of alternative propensity weights \( \hat{\psi}(x) = \frac{\hat{p}(D_{79}=1|x) / \hat{p}(D_{79}=1)}{\hat{p}(D_{79}=0|x) / \hat{p}(D_{79}=0)} \) and use them to reweight the NLSY97 data.

4) The reweighted data are then used to estimate the returns to cognitive ability for comparison across cohorts. Tables 4a and 4b report estimated returns to cognitive ability for college bound and non-college bound young adults using propensity score reweighting. The reweighting of NLSY97 sample by age\(^{41}\), by age and family background\(^{42}\), and finally, by combination of age, family background, industry and occupation\(^{43}\) barely affects the estimates of AFQT coefficients which suggests that differences in the distributions of demographics and covariates related to structural shifts in the labor market cannot explain the secular pattern of decline in returns to cognitive ability between the 1980s and 2000s.

5. Conclusions

Using data on young adults from the 1979 and 1997 National Longitudinal Survey of Youth, this study examines the changing impact of cognitive ability, as proxied by performance on AFQT tests, on wage determination of college bound and non-college bound young adults. My findings indicate that cognitive ability plays a substantially diminished role for the most recent cohort and its impact on wage determination has undergone a drastic change between 1980s and 2000s. My results tend to corroborate the findings of previous studies which emphasize the lifecycle path of technological development from adoption to

\(^{41}\) To reweight the NLSY97 by age I estimated the propensity score \( p(D_{79}|\text{age}, \text{age}^2, \text{age}^3) \) where \( D_{79} \in \{0,1\} \). I then generated the corresponding weights and applied them to NLSY97 data.

\(^{42}\) Family background covariates include mother’s and father’s education, family income, intact family indicator, number of siblings, and an indicator for Hispanic origin.

\(^{43}\) Inclusion of occupational-industrial specific characteristics are intended to capture the impact of skill-biased technological change and other long-run structural shifts in the labor market on the changing wage structure (see for example, Acemoglu 2002)
maturation and trace back the labor market outcomes observed over these periods to pre-and post-2000 patterns in technology investment and its consequent boom-and-bust cycles in the demand for cognitive skills (Beaudry et al., 2016; Castex and Dechter, 2014).

The impact of technology on the demand for cognitive skills, either explicitly or by implication, has been analyzed in two alternative perspectives. The technology-skill complementarity perspective predicts that the demand for cognitive skills rises as long as the use of such technology increases (Griliches, 1969; Tinbergen, 1975; Berman, Bound and Griliches, 1994). The alternative perspective – that has come to be known as “Schultz/Nelson-Phelps hypothesis” – focuses on the role of education or skill in the process of adopting new technologies and implies that the demand for cognitive skills diminishes as the adoption process is completed (Nelson & Phelps, 1966; Welch, 1970; Schultz, 1975; Bartel & Lichtenberg, 1987; Caselli, 1999; Galor and Moav, 2000; Greenwood and Yorukoglu, 1997). For example, Greenwood and Yorukoglu (1997, page 87) conclude their analysis by arguing that,

“Setting up, and operating, new technologies often involves acquiring and processing new information. Skill facilitates this adoption process. Therefore, times of rapid technological advancement should be associated with a rise in the return to skill.”

Findings from a burgeoning literature suggest that the post-1972 pace of technological change peaked in 1996-2000 and has been slowing down since then (Gordon, 2014). Thus my results provide consistent support for the Schultz/Nelson-Phelps hypothesis. While causal identification is evidently difficult, the cross-cohort association of
skill returns with pre- and post- 2000s technological change offers itself as a plausible explanation of the pattern of changing returns between 1980s and 2000s.
References


Constructed AFQT Distributions for 1979 and 1997 Cohorts

The NLSY79-scores are the P&P scores reported by the NLSY79. The NLSY-97 scores are based on the CAT scores from NLSY97 and the equation by Segal (1997). Both populations are weighted to be population representative.

Figure 1: AFQT Scores

Figure 2: AFQT Scores at Age 16

Figure 3: AFQT Scores Age adjusted
<table>
<thead>
<tr>
<th></th>
<th>Men</th>
<th>Women</th>
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<tr>
<td></td>
<td>NLSY79</td>
<td>NLSY97</td>
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<tr>
<td></td>
<td>Mean</td>
<td>SD</td>
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<tr>
<td>Real wage rate</td>
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Note: AFQT scores are adjusted using the Altonji et al. (2009) methodology. Hourly wages are inflation adjusted using CPI-U. Both samples are weighted using standard cross-sectional weights.
Table 1b. Summary Statistics
NonCollege-Bound Cohort

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Note.—AFQT scores are adjusted using the Altonji et al. (2009) methodology. Hourly wages are inflation adjusted using CPI-U.

Both samples are weighted using standard cross-sectional weights.
Table 2
Ordinary Least Squares Estimates of Returns to Cognitive Ability Across Panels

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Table 3
OLS Estimates of Returns to Cognitive Ability differentiated by Race and College Attendance
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Chapter 3 – Measurement Error in Survey Data and its Impact on Identifying the Extent of Downward Nominal Wage Rigidity

Co-authored with William Dickens

1. Introduction

A key question in the extensive empirical literature on wage rigidity is the extent to which downward nominal wage rigidity exists. This literature has produced remarkably inconsistent and at times inconclusive results, not only across different approaches and countries but also across different datasets for the same country (Dickens et al., 2007). Some studies find almost no evidence of downward nominal wage rigidity. McLaughlin (1994) uses Panel Study of Income Dynamics (PSID) data from 1976 to 1986 on household heads who report a wage or salary in consecutive years and he finds that about 7% of individuals have rigid nominal wages, while approximately 17% of the sample faced nominal wage cuts. McLaughlin concludes wages are generally flexible.44 Similarly, Smith (2000) studies the degree of downward rigidity in nominal wages in the United Kingdom using micro-data. Her initial results indicate about 9% of job stayers (employees remaining within single job from one year to the next) have zero wage growth and that 23% experience nominal wage reductions. But on examining the causes of rigidity she finds that up to nine-tenths can be ascribed to `symmetric’ causes (such as contracts and menu costs) or to error and she

44In a comparable analysis of the extent of nominal rigidity in wage data derived from the PSID, Kahn (1997) also examines the distribution of nominal wage changes. She uses data from 1970 to 1988 on non-self-employed household heads who have the same employer in consecutive years and finds 7% of respondents working for the same employer with rigid wages and 18% with nominal pay cuts. Her focus on the spike at zero rather than on the cumulative density below zero leads her to conclude that there is significant downward nominal rigidity, and some evidence of “menu cost” effects.
concludes that merely one percent of workers have wages that may be downwardly rigid. On the contrary, studies of specific labor markets by Akerlof et al. (1996) and of specific firms by Altonji and Devereux (2000), Baker et al. (1994), Fehr and Goette (2005), and Wilson (1999) all find that nominal wage declines are rare and that the frequency of constant wages is to a great significant extent higher than that found in the large nationally representative data sets of U.S., such as PSID (see Gottschalk, 2005).

A possible reconciliation of these inconsistent findings is that much of the reported downward flexibility based upon panel survey data on wages reflects measurement error. Measurement error in survey data has been a subject of concern and study for a long time (see for example Bound et al. 2001; Biemer et al., 1991). A distinctive feature of estimates of the extent of downward nominal wage rigidity is that they are based upon comparing survey reports given at two or more different points in time. The relationship between measurement error in such estimates of change and measurement error in the reporting of wage levels at a single point in time is unclear. For example, a mismeasurement at one point in time will not necessarily lead to an error in the estimate of change if the same mismeasurement error is also made at the second time point. However, random error in the reporting of wage levels at one point in time can translate to systematic error (bias) in estimates of change and produce spurious variability in wage changes, false wage "cuts" and may thus lead to understating the true extent of downward nominal rigidity.45 This concern is supported by evidence from personnel files, which consistently show many more workers

45 Assuming that the observed wage \( w_t \) differs from the actual wage \( \omega_t \) by an error \( u_t \), the observed wage change is \( \Delta w_t = \Delta \omega_t + \Delta u_t \). If the distribution of true wage changes is continuous, only individuals with truly rigid wages who accurately report their wage change contribute to observed rigidity. The fraction of individuals with observed wage rigidity is therefore \( P(\Delta w_t = 0) = R \times P(\Delta \omega_t = 0) \), where \( R = P(\Delta u_t = 0 | \Delta \omega_t = 0) \) is probability of accurately reporting true wage, conditional on rigid wages (Card and Hyslop, 1996).
receiving zero wage changes, and, more importantly, fewer very small wage cuts than are observed in most labor force surveys and administrative data sets (Wilson, 1999; Altonji and Devereux, 2000; Fehr and Goette, 2005). Moreover, and not surprisingly, studies correcting for measurement error consistently find more evidence of downward rigidity, as reviewed in Dickens et al. (2007).

Akerlof, Dickens, and Perry (1996) rely on validation studies conducted by Bound and Krueger (1991) which provide estimates of the ratio of the variance of the error component to the variance of reported wages and dismiss apparent flexibility as merely reflecting measurement error, and maintain that nominal rigidity is important. They argue that measurement error is responsible for virtually all negative wage changes in individual survey datasets and the true distribution is characterized by asymmetry entailing almost total censoring at zero. If a normal measurement error is added to such a censored distribution, many negative values would be generated. In order to assess the impact of reporting errors on observed wage distribution and get direct evidence on wage changes for individuals, they conducted a telephone interview of District of Columbia residents and asked workers who had not changed employers in the last 12 months whether their base pay had changed and, if so, whether it had increased or decreased. Their survey results show that only 2.7% of respondents experienced a decline in nominal wages and a massive 45% had rigid wages. To check whether the PSID-generated data could have arisen from a population that resembles their survey, making appropriate allowance for reporting error in the PSID, they "dirty" their data by adding an error which is normally distributed with standard deviation 0.167 (which is their estimate of the standard deviation of the difference between employee- and employer-reported wages from the 1977 CPS validation study) and unit
autocorrelation to 55.8% of the distribution, and assume that 44.2% report their wages or salaries exactly right. The dirtied histogram shows a much fatter left tail and even more instances of negative wage change than the PSID, implying that an error-free distribution of wage changes from the PSID would show an even smaller proportion of wage cuts than their own survey.

Altonji and Devereux (2000) relies mainly on functional-form and distributional assumptions in order to identify measurement error. They use both firm level personnel files and household survey data and conclude that nominal wage cuts are rare once one adjusts for measurement error.

Gottschalk (2005) using the Survey of Income and Program Participation (SIPP), finds that the distribution of changes in reported wages, before adjusting for measurement error, is similar to that found in the PSID. Approximately 17% of workers report a decline in nominal wages while working for the same employer. He assumes that the measurement error-free base wage trajectory is a step function and identifies true wage changes as structural breaks in the reported base wage series. After applying this identification approach and correcting for measurement error, he arrives at results which are much closer to those found in the firm-specific studies. His estimates from a nationally representative data set indicate that between 4% and 5% of hourly workers actually experience a cut in their nominal wages over a year period while working for the same employer. He concludes that the balance reflects measurement error.

Dickens et al. (2007) report the results of the International Wage Flexibility Project, which examines individual earnings in 31 different datasets from 16 countries. They remove autocorrelation in wage changes based on the assumption that measurement error
introduces negative autocorrelation in wage changes and find that in many countries wage cuts are rare so that wage change distributions are typically asymmetric.

Barattieri, Basu, and Gottschalk (2014) using micro data on wage changes (SIPP 1996 Panel) examine the contribution of measurement errors to the reported frequency. They deal with measurement error by keeping only wage changes that can be viewed as a time-series structural shift for the individual’s wage series (the same identification approach as Gottschalk, 2005) and they report that the procedure removes 63% of wage changes for hourly-paid workers (see also Bils et al., 2014).

In this paper, we employ data drawn from the 1996, 2001, 2004 and 2008 panels of the SIPP, which cover the years 1996-2013, to assess the effectiveness of dependent interviewing at reducing bias in the estimates of the extent of downward nominal wage rigidity in the US economy. In the 2004 and 2008 panels of the SIPP, dependent interviewing was used much more extensively than in the past. By focusing on changes rather than levels of wages and using responses from prior interviews to query apparent inconsistencies over time this questioning method reduces the incidence of reporting and measurement errors. Our wage change distributions derived from SIPP 2004 and 2008 panels exhibit remarkably larger zero spikes and asymmetries vis-à-vis those derived from 1996 and 2001 panels when dependent interviewing was not used. Such contrasting difference in results between these two sets of SIPP panels can only be attributed to the adoption of dependent interviewing and its concomitant lessening of incidence of reporting and measurement error, a fact that has been confirmed by our autocorrelation analysis of log nominal wage changes of job stayers in adjacent years across different panels.
This paper proceeds as follows: Section 2 describes our data in detail. Section 3 examines the plots of the distribution of individual log wage changes and their characteristics in terms of magnitudes of zero spikes and asymmetries. Section 4 presents simple estimates of the extent of downward nominal wage rigidity, analyzes the correlations between log nominal hourly wage changes of job stayers across panels and offers evidence for measurement error in the data. We end this section by describing and presenting implementation results of the IWFP measurement error correction method. Section 5 concludes and summarizes the findings of this study.

2. Data

Our analysis uses data drawn from the 1996, 2001, 2004 and 2008 panels of the Survey of Income and Program Participation (SIPP), which cover the years 1996-2013. The SIPP is a nationally representative survey of the non-institutionalized, civilian population conducted by the U.S. Census Bureau since 1983. Within each three- to five-year panel, households are interviewed every four months, a period the Census Bureau refers to as a wave. The SIPP also tracks individuals as they move (Neumark and Kawaguchi, 2004)\textsuperscript{47}. A

\textsuperscript{46} The 2008 panel has been extended as long as possible to provide an overlap with the reference period for data coming from the new design being used in the 2014 panel. There is full sample from the 2008 panel through wave 15, providing full (4 rotation groups) data through May 2013. Because of the Federal government shutdown in October the second rotation group in wave 16 did not have data collected, meaning that one-quarter of the sample is missing for the reference months from June, July, August, September of 2013. For June-August we have 3/4 of the sample, and from September-October we have 1/2 sample, and the final reference months data in November is available for the last rotation group which was interviewed in December. This means 2008 panel is the longest SIPP panel covering about 5 years.

\textsuperscript{47} The SIPP has an important advantage compared to another large survey used for this kind of analysis, namely the Outgoing Rotation Group data from linked Current Population Surveys. Unlike the CPS, where an individual is interviewed for four consecutive months, not interviewed for the next eight months, and then interviewed for another four months before being dropped from the sample, the SIPP panels, which we use, follows each individual for up to 60 months, thus creating the proper panel data essential for our analysis. In fact, the CPS is even less suitable than this summary indicates, because the sampling unit is the household and not the individual. An individual leaving the housing unit is not followed; instead, new residents become survey members (Barattieri et al. (2014)).
large number of individuals are interviewed in order to collect detailed data regarding the source and amount of their income, a variety of demographic characteristics, and their eligibility for different federal programs. Each individual is followed for a period ranging from 36 to 60 months, with interviews taking place every four months on a rotating basis.

In 2004 the U.S. Census Bureau implemented new interview procedures for the SIPP in an attempt to significantly reduce reporting errors for a wide variety of characteristics including individual level wages. The primary tool for achieving this was a more extensive and more focused use of dependent interviewing methods, in which “substantive answers from previous interviews are fed forward and used to tailor the wording and routing of questions in the next interview” (Moore et al., 2009; Jäckle, 2009).

The reasons for using SIPP for our analysis have been twofold. First, because the recall period of four months is relatively short, data from the SIPP are thought to be less prone to respondent recall errors than other federal surveys that collect retrospective income data from as long as a full year prior to the interview. Second, in the 2004 and 2008 panels of the SIPP, dependent interviewing (DI) are used much more extensively than in the past. This questioning method reduces the incidence of reporting, and hence, of measurement errors in our data.49

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48 According to Jäckle (2009) “dependent interviewing refers to a method of designing questions in panel surveys, where substantive answers from previous interviews are fed forward and used to improve data quality, in terms of longitudinal consistency and item non-response, and survey processes, in terms of efficiency of data collection and respondent burden. This differs from traditional independent interviewing, where respondents are typically asked the same questions about their situation at different points in time, without reference to previous answers.”

49 Additionally, in the 2004 and 2008 wave 2+ instruments, all of the labor force earnings screens also contain a feature which essentially allows respondents to self-select a dependent amount question. The “L” response option can be entered if the respondent says something like “whatever I said during the last interview” (see Bates and Okon (2003) and Moore et al. (2009)).
As it was noted, the Census Bureau collects SIPP data in four staggered rotation groups, with one rotation group beginning each month. Therefore, estimating year-to-year values requires that the data be aligned by calendar month. The years used in here include 1996-1999, 2001-2003, 2004-2007 and 2009-2013.

The other notion that drives our choice of data is the need to create a `like-for-like' pay measure. Thus we restricted our samples only to those who have not changed jobs and all data in this paper refer to “stayers”. The SIPP asks respondents whether they are paid by the hour and their corresponding hourly pay rate in each month. We use this hourly pay rate whenever it applies. The SIPP also reports total monthly earnings per job, whether the job lasted the entire month and the number of hours worked per week. As a step aimed at minimizing measurement error we focus on the smaller sample of people who directly reported their hourly wage to the SIPP interviewer (because they get paid by the hour).\(^5\)

3. The Distribution of Individual Wage Changes

To measure the extent of downward nominal wage rigidity, the first essential step is to examine the distribution of wage changes. We would expect that the distribution of wage changes to be essentially symmetric through the point of zero wage change in the absence of rigidity. Downward nominal wage rigidity would cause a distinct shortage of nominal wage cuts, with a corresponding pile-up of observations at zero wage change or possibly slightly

\(^{5}\) We then further our analysis by constructing and looking at a larger sample in which for persons who report an hourly wage, regular hourly pay rate is used and for those who do not report an hourly wage, monthly earnings is converted to hourly pay using weeks-per-month and hours-per-week divisors. When computing monthly earnings of those workers that are not paid by the hour, we assume that workers do not alter their earnings response based on the length of a month and use smooth 4.3 weeks per months. SIPP records starting date and end date of each job that does not last the entire month. We use this information to calculate hourly wages for those months. The histograms of the distribution of log nominal wage changes of Job stayers for both smaller and bigger samples are included in the Appendix.
above. This shortage of negative wage-change observations implies a particular type of right-skewness, or asymmetry, in the distribution (Lebow et al. 2003). As with other empirical studies documenting the extent of downward nominal wage rigidities in the U.S., in this section we examine the plots of the distribution of individual log wage changes and their characteristics in terms of magnitudes of zero spikes and asymmetries.

We begin our analysis by presenting a series of histograms representing the distributions of annual changes in nominal log hourly wage rates for the SIPP samples described above. Figure 1 in the Appendix contains the histograms of changes in nominal log hourly wage rates for the SIPP panels 1996, 2001, 2004, 2008. For scale reasons we have censored the log nominal wage changes at .65 and -.35. Therefore, the masses at the upper and lower extremes represent the cumulative fractions in the respective tails of the distribution. By way of illustration, for panel 2008 which covers 15 waves, each individual is traced at most for 5 consecutive years, i.e. each person has at most five wage reports.

As the histograms in Figure 1 show, actual wage changes exhibit discontinuity with a spike at zero. This is true in all the panels and in all the distributions of annual changes in nominal log hourly wage rates for the SIPP samples. If one assumes that the notional wage distribution – is continuous, unimodal, and symmetric then it appears that nearly all the observations at zero represent wage freezes that would have been wage declines.

When we compare the histograms for 1996 and 2001 panels with those for 2004 and 2008 panels, two prominent features stand out. First, the zero-spike was substantially higher in 2004 and 2008 panels relative to 1996 and 2001 panels. Second, and more importantly, the proportion of workers receiving wage cuts was considerably lower in 2004 and 2008 panels vis-à-vis 1996 and 2001 panels. Such contrasting difference in results between these
two sets of SIPP panels can be attributed to the adoption of dependent interviewing as the change is abrupt and discontinuous with the start of the 2004 panel. We will see later that the changes in the observed distributions results from a reduction of frequency of reporting error.$^{51}$

Another important observation, hinted at in Figure 1, is that the prevalence of zero wage changes and other characteristics of wage distributions vary over the business cycle. First, in 2006-2007 about 42 percent of workers reported zero wage change; in 2012-2013 the share had risen to about 60 percent, suggesting that during the recent recession and subsequent jobless recovery, the increase in the proportion of workers subject to downward nominal wage rigidity has been distinctly large. Second, the proportion of workers getting a wage increase declined over the period and was noticeably lower in 2012-2013 (35 percent) relative to 2006-2007 (about 50 percent). Third, and strangely enough, the proportion of workers receiving wage cuts is slightly higher in 2006-2007 (7.5%) than in 2012-2013 (4.7%).$^{52}$

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$^{51}$ Bils et al. (2014) report similar results in their recent paper: “There are considerable differences in frequencies of wage changes across the SIPP panels. The 1996 and 2001 panels show even higher frequencies than do 1990 to 1993. Our estimates interpret this as reflecting slightly higher measurement error for these panels and modestly more flexible wages. The Calvo parameters are calculated at 0.38 and 0.33 for the 1996 and 2001 panels. The last two panels, 2004 and 2008, show much lower rates of wage changes. Our calculations explain the drop in frequencies primarily by a fall in the measurement error rate.” (emphasis added)

$^{52}$ As a further check on our results, we performed this exercise with the SIPP augmented sample of log nominal wage changes of job stayers (combined hourly paid and monthly earnings data) and we arrived at similar patterns in frequencies of wage changes across SIPP panels, albeit with relatively somewhat smaller zero spikes and asymmetries which we interpret as a consequence of dirtying data. Figures 1 - 2 in Appendix 1 summarize the distribution of log of wage changes for both samples.
4. Measuring Rigidity and Correcting for Measurement Errors: Methods and Estimates

4.1. Measuring Rigidity

We adopt the procedure used in the International Wage Flexibility Project to estimates the extent of downward nominal wage rigidity \( (n) \).\textsuperscript{53} To measure nominal wage rigidity in a particular year, we can construct a simple metric that provide answer to the following question:

- What fraction of workers who would normally receive a wage cut in the absence of downward rigidity will instead receive a nominal wage freeze?

To answer this question, we proceed as follows. Suppose a fraction \( n \) of workers is protected from wage cuts by downward nominal rigidity. The fraction of workers whose wages do not change will equal the fraction of workers in the left tail of the notional distribution below zero times \( n \). This means that we can estimate the fraction \( n \) as the ratio of the number of workers with a wage freeze to the total at risk of a cut (that is, those receiving no wage change plus those receiving a cut).

Thus, for downward nominal wage rigidity, our measure is straightforward. We assume that everyone who had a nominal wage freeze would have had a nominal wage cut in the absence of downward nominal rigidity and construct

\[
n = \frac{f_n}{(f_n + c_n)}
\]

Where \( f_n \) is the fraction of workers with nominal wage freezes and \( c_n \) is the fraction with nominal wage cuts.

\textsuperscript{53} Our discussion in this section is based on Dickens and Goette (2005) and Dickens et al. (2007). They provide a detailed description of the procedure.
Such a measure of nominal rigidity does not show the actual percentage of workers experiencing nominal rigidity; rather it attempts to capture what share of workers relative to the group that might otherwise have experienced declining nominal wages, experiences wage rigidity instead (see Dickens et al., 2007).

Table 1. summarizes this information across different SIPP panels. Our results for the SIPP panels show an increasing trend of zero-spike, the measure of downward nominal wage rigidity. The main advantage of this approach is to show significant differences on the estimates of simple measures of downward nominal wage rigidity across the panels before and after the introduction of dependent interviewing in the survey design.

It is our contention that the large change in the measure of downward nominal wage rigidity at the break between the 2001 and 2004 panel can be attributed to a very large reduction in measurement error with the introduction of dependent interviewing.

The presence of errors in the reporting, recording, or calculating of the wage level in any year—assuming that errors are not correlated from one year to the next—would cause large positive wage changes to be followed by small or negative wage changes in the next year, while small positive changes, or any size negative changes, would be followed by large positive changes. All else equal, the more errors present in a particular dataset, the more negative will be the auto-covariance of wage changes (Dickens et al., 2007).

54 The assumption that all autocorrelation in changes in log wages is due to measurement error is suggested by the findings of Abowd and Card (1989) who show that the best characterization of the stochastic process generating individual wages in US panel data is an ARIMA(0,1,1) – a process which is MA1 in first differences. Measurement error with no serial correlation added to a random walk will generate this sort of process. Since all the covariance in wage changes is due to the MA1 process, the assumption that the measurement error is the only source of serial correlation in wage changes is tantamount to assuming that any observed wage change that goes away within a year was an error (see Dickens and Goette, 2005).

55 If wages are measured with error that is uncorrelated from year to year, the error will produce a negative covariance between wage changes in adjacent years. The magnitude of this covariance will depend on the frequency and the variance of the errors (see Dickens and Goette, 2005).
Table 2 contains covariances and correlations between log nominal hourly wage changes of job stayers for 1996, 2001, 2004 and 2008 SIPP panels. The covariances are presented below the diagonal of the table, while the correlation coefficients are presented above the diagonal. There are considerable differences in frequencies of wage changes across the SIPP panels. Looking first at the diagonal elements of Table 2, the cross-sectional variation in percentage changes in nominal hourly wage of job stayers (the standard deviation of the change in the logarithm of nominal hourly wage) varies between a minimum of 7.0 percent in 2011-2012 (2008 Panel) and a maximum of 22.8 percent in 2001-2002 (2001 Panel). The variances of changes in log nominal hourly wage of job stayers also vary across different panels. The last two panels, 2004 and 2008, show much smaller cross-sectional dispersion in wage changes. Turning to the first-order autocovariances, which are displayed directly below the diagonal, it is clear that consecutive changes in individual log nominal hourly wage are strongly negatively correlated in 1996 and 2001 panels. The average first-order correlation coefficients of changes in the logarithms of nominal hourly wages for these two panels are respectively -0.30 and -0.27. This average coefficient is much smaller for 2004 panel (-0.11) and becomes almost negligible for 2008 panel (-0.09). Overall, our analysis supports the hypothesis that with the more extensive use of dependent interviewing in 2004 and 2008 SIPP panels the incidence of measurement error has been drastically declined and hence our results more accurately reflect the extent of downward nominal rigidity in the US economy.

In the following section we clean the data using IWFP protocol so that we could have a consistent comparison of measure of downward nominal rigidity before and after the survey redesign.
4.2. IWFP Measurement Error Correction: Implementation and Results

The evidence presented in the prior sections regarding the strong negative correlation of consecutive changes in individual log of nominal hourly wages for 1996 and 2001 panels as compared to 2004 and 2008 panels along with significant differences on the estimates of simple measures of downward nominal wage rigidity across the panels suggest that the observed measure of earning in earlier panels of SIPP is distorted by reporting and recording error. In order to make meaningful comparison of the extent of wage rigidity across panels, we need a way to transform the observed wage change distribution into an estimate of the true distribution without errors. The Error in observed wage changes has to be taken into account in the calculation of measures for nominal and real wage rigidities, because measurement error in wage levels creates spurious variance in wage changes, which is a sign of wage flexibility and seriously impedes the assessment of wage rigidity.

We apply the International Wage Flexibility Project (IWFP) method as formulated by Dickens and Goette (2007b). IWFP protocol has been widely used to assess the degree of wage rigidity based on micro-level data. This method has two stages: The first step is a correction for measurement error. It extracts the estimated distributions of true wage changes from observed wage changes. Hence, the true wage change distribution is an estimate of error-free presentation of observed wage changes. The second main stage is the estimation of wage rigidities. It involves comparing true wage changes with the notional ones. The notional wage change distribution is the counterfactual situation in which there would be no wage rigidities that hinder the adjustment of individuals’ wages.

IWFP assumes that process generating observed wage change is:
\[ w_{it} = w_{it-1} + e_{it} \]  \hspace{1cm} (1)

\[ w_{it}^o = w_{it} + \eta'_{it} \quad \text{where} \quad \eta'_{it} = \begin{cases} 0 & \text{if } \mu_{it} > 0 \text{ or } \tau_i > 0 \\ \eta_{it} & \text{otherwise} \end{cases} \]  \hspace{1cm} (2)

where

\( w_{it} \) is the natural log of the true wage for person \( i \) at time \( t \).

\( w_{it}^o \) is the natural log of the wage for that person at time \( t \) observed in the data.

\( \eta_{it} \) is a random variable assumed to be an \( i.i.d. \) two-sided Weibull with mean zero and parameters \( b_t \) and \( \alpha \) and \( \mu_{it} \) is assumed to be an \( i.i.d. \) random variable with a uniform distribution over the interval \([-c, 1-c]\).

\( c \) is the parameter of the error distribution that is equal to the probability that someone who is prone to error makes the error.

\( \tau_i \) is assumed to be an \( i.i.d. \) random variable with a uniform distribution over the interval \([p-1, p]\).

\( p \) is parameter of the error distribution that determines the probability that someone is not prone to reporting error.

According to equation (1), the (log) true wage \( w_{it} \) of an individual \( i \) at time \( t \) follows a random walk which implies zero autocorrelation of wage changes (\( e_{it} \) is \( i.i.d. \) process).

According to equation (2), The (log) observed wage \( w_{it}^o \) consists of the true wage and an error \( \eta_{it}' \).

It is assumed that a fraction of the population \( p \) never reports errors, while the remaining individuals report errors with probability \( c \).

\[ 56 \text{ The Weibull distribution has support on interval } (0, \infty]. \text{ The two-sided Weibull has support on the entire real number line excluding } 0. \text{ The density is given by } \frac{d}{dx} = ba|x|^{a-1} e^{-b|x|^{a}}. \]
The estimator assumes that the log of wage change is discrete variable and could only take values from -0.245 to 0.495 in steps of 0.01 or zero. The model assumes wages are measured with errors that are uncorrelated in adjacent years, therefore, a mistakenly reported large wage increase in one year will likely be revised in the next year so the changes in log of wages will be negatively correlated. The magnitude of negative covariance could be determined by knowing the frequency and the variance of errors. The estimation procedure starts with an initial guess on the parameters of error distribution and corrects the histograms of observed wage changes. The correction is based on the following relationship between true wage changes and the observed ones: \( f^o = Tf^t \) where \( f^o \) is a vector of observed frequencies in each cell of the wage change histogram, \( f^t \) is a similar vector for the true frequencies and \( c \) is a transition matrix whose columns are the percentage of observations in each cell of the true distribution that will end up in each cell of the observed distribution owing to measurement errors in wages. Inverting \( T \) and multiply both sides of equation by the inverse, gives \( T^{-1}f^o = f^t \) Hence, if the transition matrix \( T \) is known, the true wage change distribution can be recovered from the observed distribution. We will optimize our initial guess about frequency and curvature of error distribution using an iterative process to minimize a quadratic distance measure of the difference between the actual and predicted fraction of people switching from low to high or high to low as a function of remaining parameters. It can be shown that the \( T \) matrix is identified by finding the following elements:

- \( p \) the probability of not being prone to error

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57 This assumption is supported by the findings of Abowd and Card (1989) that the stochastic process generating individual wages in the US is MA1 in first difference. Bound and Kueger’s (1999) estimates of extent of measurement error in the US survey data also suggest that error could be sufficient to explain all the negative correlation between observed wage changes.
We have used Powell’s optimization method to estimate above parameters.

To measure nominal and real wage rigidities, the generalized method of moments is used to fit the expected histogram of wage changes to the error-corrected (true) wage-change histograms. The method essentially uses the fraction of observations in each cell of the wage change histogram as the moments. The model assumes that, in the absence of rigidity, log wage changes have a symmetric two-sided Weibull\(^{58}\) distribution, which is referred to as the notional wage change distribution. The estimation is based on a grid search for asymmetries in the wage change distribution around the expected inflation rate and zero respectively for DRWR and DNWR. The technical derivations of estimation technique are available in Dickens and Goette (2007b).

In applying the error correction and estimation procedure we have focused on the longest panels before and after the introduction of dependent interviewing to SIPP survey design. More specifically, for 1996 panel, following the IWFP approach, we estimated the proportion of the population that is prone to make errors \((1 - p)\) to be 67%. The probability that someone who is likely to make a mistake makes one \((c)\) is estimated 47%. Therefore, the error rate in 1996 panel is estimated to be 32%. In the process of applying the IWFP protocol to our SIPP 2008 Panel data, the error correction part was skipped because, as expected, the autocovariances turned out to be not negative enough. This may safely be

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\(^{58}\) Briefly, examination of the true wage change distributions in the International Wage Flexibility Project indicate that first, wage change distributions are more peaked and have fatter tails than the normal distribution. Second, the upper half of the distribution (above median), which is presumably not affected by wage rigidities, is well approximated by a Weibull distribution.
interpreted as being the result of error free wage distributions and hence indistinguishability of empirical and true distributions. The procedure described above yields estimates of the extent of downward nominal wage rigidity (n) and of downward real wage rigidity (r) simultaneously. Even though we have estimated the extent of DNWR and DRWR in the SIPP using this protocol, we did not find any evidence for the existence of DRWR in the data.

Figure 3. Presents the observed wage change distribution (empirical distribution) for different years in 1996 panels as compared to estimated true distributions after applying the error correction techniques the frequency of wage freezes increases on average by 11.5%. The frequency of wage cuts also declined by 8.8% on average. It is evident that measurement error in 1996 panel causes negative wage changes which is a sign of wage flexibility, therefore, simple measures of DNWR are much smaller in the empirical distribution.

Table 3 and 4 present the main result of estimation of different metrics of extent and impact of downward nominal wage rigidity using Mixed Method of Moments approach.

Table 3 summarizes the result of error correction process on estimating the simple metrics of extent and impact of downward nominal rigidity for different years of 1996 panels. The simple measure of DNWR is calculated both for the empirical distribution and also estimated true distribution of wage changes in 1996. The magnitude of these estimates using true distribution are much closer to those for empirical distribution in 2008 panel. Wage sweep-up measures are presented in the last column of table 3. This metric is also introduced by Dickens and Goette (2006) to measure the impact of the rigidity on average nominal wage change in a particular year. The sweep-up measures by how much (in percentage terms) is the average wage change higher because of the existence of nominal rigidity. Wage sweep up
estimates are also higher and more comparable with 2008 panel estimates after applying the error correction method.\textsuperscript{59}

\textbf{5. Conclusion}

In this paper, our contention has been that errors in the reporting of wage levels would greatly exaggerate the actual frequency of wage cuts, understate the true rigidity and reduce the extent and economic significance of downward wage rigidity. To test the validity of our contention and examine the extent of downward nominal wage rigidity in the US economy, we have employed data drawn from the 1996, 2001, 2004 and 2008 panels of the SIPP, which cover the years 1996-2013. The reasons for using SIPP for our analysis have been twofold. First, data from the SIPP are thought to be less prone to respondent recall errors than other federal surveys that collect retrospective income data from as long as a full year prior to the interview. Secondly, in the 2004 and 2008 panels of the SIPP, dependent interviewing was used much more extensively than in the past. This questioning method by focusing on changes rather than levels of wages and using responses from prior interviews to query apparent inconsistencies over time reduces the incidence of reporting and measurement errors. We focused our analysis on the sample of hourly paid job stayers which presumably is the least error-prone. The distribution of Individual wage changes

\textsuperscript{59}The sluggish pace of nominal wage growth in recent years along with the noticeable rise in the average wage sweep-up may reflect the phenomenon of "pent-up wage deflation". The average wage sweep-up can be interpreted as the increase in average labor costs due to downward wage rigidity. If firms are sensitive to unit labor costs, then a higher average wage sweep-up should be associated with lower employment or higher unemployment, as predicted by the model of Akerlof \textit{et al.} \textbf{(1996)}. Daly and Hobijn (2014) present a graph which plots the 12-month moving average of the fraction of workers in the CPS data reporting zero nominal wage changes along with the unemployment rate and show that there is always a non-trivial fraction of workers receiving zero wage changes in the U.S. economy. This fraction increases around business cycle downturns although with a lag relative to the unemployment rate. They state that these two patterns: (i) the spike at zero wage changes lags the spike in the unemployment rate, and (ii) the prevalence of zero wage changes stays high well after the unemployment rate has begun to come down are two features of the data that Phillips \textbf{(1958)} argued could produce curvature in the wage Phillips curve.
displayed several key features of the wage adjustment process. First, there was always a significant spike at zero in the wage change distribution across all panels. Secondly, the zero-spike was substantially higher in 2004 and 2008 panels vis-à-vis 1996 and 2001 panels. Thirdly, and more importantly, the fraction of workers getting a wage decrease declined quite noticeably in 2004 and 2008 panels as compared to 1996 and 2001 panels, which in itself tends to suggest that reporting errors greatly exaggerate the apparent frequency of nominal wage cuts. Further support for our contention was found by analyzing covariances and correlations of log nominal hourly wage changes of job stayers in adjacent years. Given the magnitudes of negative first-order correlation coefficients and their drastic decline of their absolute values across 2004 and 2008 panels it is likely that, with the more extensive use of dependent interviewing in 2004 and 2008 SIPP panels, the incidence of measurement error has been remarkably reduced and hence later panels results more accurately reflect the extent of downward nominal rigidity in the US economy. We have examined this conjecture by applying the IWFP error correction protocol to SIPP Panels. Our technique yields no correction to the empirical wage change distribution of 2008 panel, but makes substantial correction to that of 1996 panel. After the correction, the distinctive characteristics of the base wage distribution, which are lost in the observed wage change distribution, are largely recovered. Using this correction makes our comparisons across data sets much more meaningful and reliable. This may, of course, be interpreted as being the result of less polluted with error wage distributions and hence indistinguishability of empirical and true distributions in 2008 panel.
References


Table 1. Characteristics of Year-to-Year Wage Change Distributions of Hourly Paid Job Stayers

<table>
<thead>
<tr>
<th>Year-to-Year</th>
<th>% All Hourly Paid Job Stayers with</th>
<th>Measure of DNWR n</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Nominal Cut</td>
<td>Rigid Wage</td>
</tr>
<tr>
<td>1996 Panel</td>
<td></td>
<td></td>
</tr>
<tr>
<td>1996-1997</td>
<td>17.6</td>
<td>15.3</td>
</tr>
<tr>
<td>1997-1998</td>
<td>13.4</td>
<td>17.4</td>
</tr>
<tr>
<td>1998-1999</td>
<td>14.1</td>
<td>20.5</td>
</tr>
<tr>
<td>2001 Panel</td>
<td></td>
<td></td>
</tr>
<tr>
<td>2001-2002</td>
<td>21.3</td>
<td>17.8</td>
</tr>
<tr>
<td>2002-2003</td>
<td>20.3</td>
<td>19.6</td>
</tr>
<tr>
<td>2004 Panel</td>
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<td></td>
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<tr>
<td>2004-2005</td>
<td>10.9</td>
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<tr>
<td>2005-2006</td>
<td>8.3</td>
<td>39.6</td>
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<td>2006-2007</td>
<td>7.5</td>
<td>42.9</td>
</tr>
<tr>
<td>2009-2010</td>
<td>6.7</td>
<td>55.4</td>
</tr>
<tr>
<td>2008 Panel</td>
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<td></td>
</tr>
<tr>
<td>2010-2011</td>
<td>5.2</td>
<td>59.5</td>
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<td>2010-2011</td>
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<td>58.9</td>
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<tr>
<td>2012-2013</td>
<td>4.7</td>
<td>60.9</td>
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<td>------------</td>
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<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
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<td>1. Δ log w</td>
<td>0.042504</td>
<td>-0.2600</td>
</tr>
<tr>
<td>2. Δ log w</td>
<td>-0.008359</td>
<td>0.024326</td>
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<tr>
<td>3. Δ log w</td>
<td>-0.000221</td>
<td>-0.008239</td>
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<tr>
<td>4. Δ log w</td>
<td></td>
<td></td>
</tr>
<tr>
<td>5. Δ log w</td>
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<tr>
<td>6. Δ log w</td>
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<td>7. Δ log w</td>
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<td>9. Δ log w</td>
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<td>10. Δ log w</td>
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<td>11. Δ log w</td>
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</tr>
<tr>
<td>12. Δ log w</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

* Covariances below diagonal and correlations above diagonal.
### Table 3. Characteristics of Year-to-Year Wage Change Distributions of Hourly Paid Job Stayers

**1996 and 2008 SIPP Panels**

<table>
<thead>
<tr>
<th>Year</th>
<th>% All Hourly Paid Job Stayers with Nominal Cut</th>
<th>Simple n (a) (Measure of DNWR)</th>
<th>Sweep-up n (b)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>1996 Panel</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1996-1997</td>
<td>17.6</td>
<td>46.5</td>
<td>1.3</td>
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<td>1997-1998</td>
<td>13.4</td>
<td>56.4</td>
<td>1.4</td>
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<td>14.1</td>
<td>59.2</td>
<td>1.8</td>
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<tr>
<td><strong>1996 Panel</strong></td>
<td></td>
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<td>1996-1997</td>
<td>8.8</td>
<td>75.3</td>
<td>2.9</td>
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<td>1997-1998</td>
<td>4.8</td>
<td>85.3</td>
<td>2.9</td>
</tr>
<tr>
<td>1998-1999</td>
<td>5.1</td>
<td>86.6</td>
<td>4.2</td>
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<td><strong>2008 Panel</strong></td>
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<tr>
<td>2009-2010</td>
<td>6.7</td>
<td>89.2</td>
<td>4.5</td>
</tr>
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<td>2010-2011</td>
<td>5.2</td>
<td>92.0</td>
<td>4.7</td>
</tr>
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<td>2011-2012</td>
<td>5.0</td>
<td>92.2</td>
<td>4.9</td>
</tr>
<tr>
<td>2012-2013</td>
<td>4.7</td>
<td>92.8</td>
<td>5.4</td>
</tr>
</tbody>
</table>

\(a\) The ratio of the number of workers with a wage freeze to the total at risk of a cut, expressed in percentages.

\(b\) The sweep-up n measures by how much (in percentage terms) is the average wage change higher because of the existence of nominal rigidity. Here we calculate the n sweep up defined as \(n \times (-E(dw|dw<0))\), since \(E(dw|dw<0)\) refers to the notional distribution and we are using the true (or observed) distribution to calculate it, then \(E(dw|dw<0)=\text{sum(mass of cell for cells below zero } \times \text{midpoint of cell) } \times (\text{spike+tail)/tail} \). The estimates for the amount of sweep-up are calculated using the IWFP protocol (Dickens et al. (2006)).

\(c\) In the process of applying the IWFP protocol to our SIPP 2008 Panel data, the error correction part was skipped because, as expected, the autocovariances turned out to be not negative enough. This may safely be interpreted as being the result of error free wage distributions and hence indistinguishability of empirical and true distributions.
Figure 3. Empirical vs. Estimated True distribution of Log Nominal Wage Changes - Panel 1996:

Figure 1. Log of Wage Change Distribution (1997)

Figure 2. Log of Wage Change Distribution (1998)

Figure 3. Log of Wage Change Distribution (1999)
Empirical Distribution of Log Nominal Wage Changes - Panel 2008:

![Graphs showing wage change distributions for 2008, 2010, and 2011.]
Empirical Distribution of Log Nominal Wage Changes - Panel 2008:
Table 4. Estimates of Nominal Wage Rigidity and Wage Sweep-up (Mixed Method of Moments)*

<table>
<thead>
<tr>
<th>Nominal Wage Rigidity</th>
<th>Sweep-up-n&lt;sup&gt;1&lt;/sup&gt;</th>
<th>p&lt;sup&gt;2&lt;/sup&gt;</th>
<th>c&lt;sup&gt;3&lt;/sup&gt;</th>
<th>Error Rate&lt;sup&gt;4&lt;/sup&gt;</th>
</tr>
</thead>
<tbody>
<tr>
<td>1996 Panel</td>
<td></td>
<td></td>
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<tr>
<td>1996-1997</td>
<td>89.0</td>
<td>2.3</td>
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<td>32.4</td>
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<td>1997-1998</td>
<td>84.1</td>
<td>1.5</td>
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<td>47.5</td>
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<td>1998-1999</td>
<td>96.7</td>
<td>2.1</td>
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<td>32.1</td>
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<td>2008 Panel</td>
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<td>2009-2010</td>
<td>82.9</td>
<td>2.4</td>
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<td>2010-2011</td>
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<tr>
<td>2011-2012</td>
<td>83.8</td>
<td>1.8</td>
<td></td>
<td></td>
</tr>
<tr>
<td>2012-2013</td>
<td>90.2</td>
<td>2.6</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

*All figures are in percentage terms.
1. Sweep-up-n is based on notional dw measured as \(-n \times (E(dw|dw<=0)) \times P(dw<=0)\). In other words, it is the magnitude of sweep-up due to nominal rigidity computed as \(-n \times (average notional wage change for those with notional wage changes less than or equal to zero) \times (fraction with notional wage changes less than or equal to zero)\).
2. The probability that a person is not likely to make a mistake.
3. The probability that someone who is likely to make a mistake makes one.
4. The error rate is calculated from the equation \((1-p)c\), where p is an estimate of the probability of making no errors and c is an estimate for making an error if prone to error.
Appendix 1:

Figure 1. Histogram of the Distribution of Log Nominal Wage Changes
SIPP Sample of Hourly Paid Job Stayers

SIPP 1996 Panel

1997

1998

1999
SIPP 2001 Panel

2002

SIPP 2004 Panel

2005

2006

2007
SIPP 2008 Panel

2010

2011

2012

2013
Figure 2. Histogram of the Distribution of Log Nominal Wage Changes
SIPP Augmented Sample of Monthly Earnings and Hourly Paid Job Stayers

Panel 1996
Panel 2008

Rigidity N= .7360156774520874

Rigidity N= .7741935253143311

Rigidity N= .7098146677017212

Rigidity N= .7515348792076111