ESSAYS ON THE DETERMINATION OF EMPLOYMENT AND WAGES

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Abstract

This collection of essays investigates the determination of employment and wages.

Chapter 1 explores how firms respond to changes in the demand for their output by comparing employers from which purchases were and were not made through the American Recovery and Reinvestment Act of 2009. This analysis finds that companies responded to these demand shocks by increasing both employment as well as wages during the Great Recession. Taken together, these equilibrium labor market outcomes provide evidence of widespread and substantial monopsony power among employers in the United States.

Chapter 2, which is co-authored with Alan B. Krueger, examines the extent to which economic rents are shared among different types of workers within a given company by utilizing the price of crude oil as an instrument for the productivity of petroleum extraction firms in the United States. This study demonstrates that the elasticity of wages with respect to exogenous shocks to productivity can be heterogeneous throughout a firm. Notably, we find that workers at the top of the earnings distribution tend to possess greater bargaining power over wages relative to their lower paid counterparts.

Chapter 3, which is co-authored with both Alan B. Krueger as well as Judd N. L. Cramer and was published in the Spring 2014 issue of the Brookings Papers on Economic Activity, considers the labor market outcomes of workers who become long-term unemployed in the United States. Our results suggest that unemployed workers’ attachment to the labor force generally declines as their duration of joblessness rises. Furthermore, we demonstrate that the nonparticipation of long-term unemployed workers is a critical component of the cyclical patterns in the labor market that have traditionally been observed over the course of the business cycle.
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To Annie, Mom, and Dad.
And in memory of Alan.
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1 The Labor Market Effects of Demand Shocks: Firm-Level Evidence from the Recovery Act

1.1 Introduction

This paper addresses a fundamental question in economics: How do firms respond to demand shocks? From the earliest days of the discipline, economists have recognized that producers will adjust the utilization of factor inputs in accordance with the demand for their output in order to maximize profits. However, the relative parsimony of such an explanation notwithstanding, economic theory and the existing literature suggest that there may be substantial heterogeneity in the manner by which firms will alter production in response to shifts in the demand for their output (e.g., Hamermesh 1993). Since factor inputs can be varied along both intensive and extensive margins, firms may adjust not only the number of workers they employ but also the quantity of hours worked by each employee as a consequence of fluctuations in product demand (Becker 1962; Oi 1962; Feldstein 1967; Rosen 1968). In addition, the dynamics of how employers react to changes in demand may be conditioned by factors such as the nature of adjustment costs (Bernanke 1986; Hamermesh 1989) and firms’ expectations of the magnitude and duration of these shocks (Crawford 1979; Topel 1982). Furthermore, an individual employer may face an even greater range of choices if labor markets are not perfectly competitive. Under a setting in which firms possess monopsony power to set wages, a profit-maximizing entity’s optimal response to a demand shock will entail the adjustment of the price as well as the quantity of labor inputs (e.g.,

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2As Smith (1776, Book I, Chapter VII) observes, “The whole quantity of industry annually employed in order to bring any commodity to market, naturally suits itself in this manner to the effectual demand. It naturally aims at bringing always that precise quantity thither which may be sufficient to supply, and no more than supply, that demand.”
Manning 2003). Therefore, this question occupies a central position in understanding how labor markets operate, and the multiplicity of potential outcomes emphasizes the importance of empirical research in this area.

To that end, this paper provides a novel perspective of how employers respond to changes in the demand for their output by leveraging two unique firm-level datasets. Specifically, I combine linked employer-employee payroll records from ADP, LLC with a comprehensive database of the contracts, grants, and loans that were awarded through the American Recovery and Reinvestment Act of 2009 (ARRA). In tandem, these data provide a direct connection between the purchases of goods and services and a firm’s corresponding utilization of labor inputs in order to produce the desired level of output. Moreover, these datasets reveal considerable variation across the timing and magnitude of changes in firm production. Thus, this study circumvents the data limitations that have traditionally constrained the existing literature. And as a consequence, I am able to discern individual firms’ reactions to clear changes in the demand for their output.

Accordingly, this study demonstrates that firms which experienced positive demand shocks through ARRA increased their utilization of labor inputs in order to fulfill these additional purchases. Using a matched difference-in-differences strategy, I estimate that employment was 3.5 log points higher at firms which received funds through ARRA contracts, grants, and loans relative to firms that did not receive Recovery Act funding. In addition, an event study framework reveals that firms adjusted employment rather quickly and in a fairly persistent manner following their initial attachment to the Recovery Act. And perhaps as a result of the prolonged nature of the projects that were typically funded by this particular component of the Recovery Act, I find that these adjustments to labor inputs occurred strictly along the extensive margin; on average, employees at these firms did not work longer hours in conjunction with the perceived increases in production.

Hamermesh (1993, Chapter I, Section III) and Manning (2003, Chapter 4, Section 3) stress the importance of not only employer-level data but also firm-specific demand shocks to the development of credible studies in this domain.
Furthermore, I uncover evidence of substantial wage-setting power by U.S. employers. Wages were 0.7 log point higher at firms that were responsible for providing this extra output than at a counterfactual sample of employers that were never connected to the Recovery Act. Notably, this increase in compensation was experienced by the incumbent workers of firms that expanded production in order to furnish these goods and services. Such an outcome is consistent with a monopsonistic model of labor markets in which firms face relatively inelastic labor supply. Indeed, the magnitudes of the observed changes in employment and wages imply that the elasticity of labor supply to an individual firm is roughly 4.8, which translates to workers being paid about 21 percent less than the marginal revenue product of labor.

It is worth noting that these empirical results are neither self-evident nor axiomatic a priori. As observed by Card et al. (2018), the prevailing view in labor economics has been that the market for workers is essentially competitive and that individual employers generally do not possess the power to set wages. Thus, according to the standard competitive model in which labor supply is assumed to be highly elastic, firms would not be expected to increase both employment and wages in response to positive changes in the demand for their output. Similarly, the macroeconomics literature on business cycles has historically developed along a continuum in which prices tend to be either instantaneously responsive (i.e., Say 1803) or completely rigid (i.e., Keynes 1936) in the face of demand shocks. Therefore, given that this paper analyzes the behavior of firms during the Great Recession, it is not readily apparent which of these polar cases should be most relevant to the adjustment of labor inputs and wages in this particular context. Likewise, one of the more common empirical approaches in the literature on imperfect competition has been to investigate relatively specialized labor markets in which there is likely to be scope for monopsonistic wage-setting (Staiger et al. 2010; Falch 2010; Ransom and Sims 2010; Matsudaira 2014). However, the Recovery Act was enacted with the express intent of stimulating the overall U.S. economy, and even the discretionary component of this legislation was designed to impact a wide swath of geographic
areas and industries. As a result, this study contends with an expansive cross section of labor markets that are not only characterized by disparate competitive structures but also located throughout the United States.

Consequently, this paper contributes to four distinct strands of the broader economics literature. First, this study serves as an unconventional entry in the labor demand literature (e.g., Hamermesh 2017). Second, it continues the resurgence of research on imperfect competition in labor markets by analyzing the connection between firm-specific performance shocks and wages (Kline et al. 2017; Garin and Silverio 2017). Third, this paper is related to an emerging literature on the importance of product demand in the determination of firm growth (Ferraz et al. 2015; Foster et al. 2016; Hebous and Zimmerman 2016; Pozzi and Schivardi 2016; Atkin et al. 2017). Lastly, I provide evidence regarding the efficacy of government purchases as countercyclical fiscal policy and add to a progression of studies that evaluate the Recovery Act (Chodorow-Reich et al. 2012; Wilson 2012; Conley and Dupor 2013; Dube et al. 2015; Garin 2016; Dupor and Mehkari 2016).

The paper is organized as follows. Section 1.2 provides institutional details pertaining to the design and implementation of the American Recovery and Reinvestment Act of 2009. Section 1.3 describes the primary data sources that form the basis of this study. Section 1.4 provides a theoretical framework for understanding the choices that an employer might face when confronted with a shock to the demand for its output, and Section 1.5 outlines the empirical strategy for identifying the responses of firms from which goods and services were purchased through the Recovery Act. In Section 1.6, I summarize the empirical results from this analysis. Finally, Section 1.7 discusses the implications of these findings within the specific contexts of monopsonistic labor markets and the multiplier effects of fiscal policy.
1.2 Institutional Details

The U.S. economy officially peaked in December 2007, and the unemployment rate gradually climbed as the financial crisis weighed on economic activity during the summer of 2008. This recession intensified following the bankruptcy of Lehman Brothers in September 2008, and the economy appeared to be in free fall as the U.S. presidential election took place that November. In the fourth quarter of 2008 alone, real gross domestic product plunged 8.4 percent at an annual rate, and employers shed nearly 2 million payroll jobs. Moreover, with the target federal funds rate having already been slashed to a range of 0.00 to 0.25 percent in December 2008, there was considerable uncertainty over the scope for further expansionary monetary policy.

Against this backdrop of economic distress, President-elect Obama and Democratic leaders in Congress publicly signaled their intentions to enact legislation in order to counteract the downturn following the 2008 election (Calmes and Zeleny 2008; Cowan 2008). Nevertheless, the precise details of this proposed fiscal stimulus remained in flux as both the Senate and the House of Representatives embarked upon the legislative process in early 2009 (Furman 2018). Even after the House of Representatives passed its version of an economic recovery plan on January 28, 2009, the prevailing sentiment was that the bill would undergo additional modifications once it was taken up by the Senate (Calmes 2009; Murray and Kane 2009). Indeed, on February 10, 2009, the Senate approved a bill that substantially altered the composition of tax and spending provisions in the economic stimulus package (Herszenhorn 2009a). As a result, these differences could only be resolved in conference, and a final compromise version of the legislation was ultimately ratified by both chambers of Congress on February 13, 2009 (Herszenhorn 2009b).

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8 Board of Governors of the Federal Reserve System. “Open Market Operations.”
On February 17, 2009, President Obama signed the American Recovery and Reinvestment Act into law. The Recovery Act served as the centerpiece of the U.S. federal government’s fiscal policy response to the Great Recession and committed $787 billion towards spurring economic activity (Congressional Budget Office 2009). In broad strokes, the total budgetary cost of ARRA was almost evenly split among the following three categories: tax relief; entitlements; and contracts, grants, and loans (Figure 1.1). The tax and entitlement provisions in the Recovery Act (e.g., the Making Work Pay tax credit for households, the increase in the exemption threshold for the alternative minimum tax, and the expansions in federal aid for state Medicaid and unemployment insurance programs) took effect almost immediately and were intended to provide over $500 billion in timely support to the U.S. economy during the depths of the Great Recession. In contrast, the discretionary component of ARRA authorized federal agencies to award $275 billion in contracts, grants, and loans to state and local governments, educational institutions, non-profit organizations, and private companies (Government Accountability Office 2009). As a result, this portion of the Recovery Act was intrinsically designed to “be more lagged but have larger cumulative countercyclical impacts and greater longer-run benefits” (Council of Economic Advisers 2014a).

These ARRA contracts, grants, and loans were subject to a number of unusual constraints that may have mitigated the potential endogeneity issues that often characterize the allocation of government funds. First, more than 200 separate federal agencies were responsible for implementing this component of the Recovery Act (Table 1.1). Second, from the outset, President Obama publicly vowed that any fiscal stimulus package would be free of so-called “earmarks” through which members of Congress could divert funds to favored projects, localities, or constituencies (Associated Press 2009). Such an approach contrasted

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*In its most recent assessment, the Congressional Budget Office determined that the Recovery Act will cost $836 billion through fiscal year 2019 (Congressional Budget Office 2015). Most of this increase in the budgetary impact of ARRA has been attributable to higher than projected outlays for income security programs such as unemployment compensation and the Supplemental Nutrition Assistance Program during the Great Recession (Congressional Budget Office 2010).*
sharply with not only the process by which Congress had routinely passed appropriations bills until that point (Congressional Research Service 2006) but also the manner in which the New Deal appears to have been crafted in response to the Great Depression (Fishback et al. 2003). Instead, the Recovery Act instructed federal agencies to rely primarily upon pre-existing funding formulas and merit-based criteria in order to award these contracts, grants, and loans (Orszag 2014). Additionally, under Section 1554 of the Recovery Act, even the recipients of these ARRA awards were obligated to adopt competitive procedures such as fixed-price contracts and sealed bids in the subsequent procurement of any goods and services (Government Publishing Office 2009). As a consequence, there is little documented evidence that discretionary ARRA funds were targeted to particular entities as a function of either political expediency or economic considerations (Boone et al. 2014). Finally, Congress and the Obama Administration included various safeguards in the Recovery Act that applied an unprecedented level of oversight to the disbursement of these contracts, grants, and loans (Government Accountability Office 2014). Specifically, the legislation authorized the formation of a Recovery Accountability and Transparency Board for the purposes of monitoring these outlays, and Section 1512 of the Recovery Act required all entities that received at least $25,000 in contracts, grants, and loans to provide comprehensive reports on the precise use of these funds.

1.3 Data

1.3.1 American Recovery and Reinvestment Act

On account of Section 1512 of the American Recovery and Reinvestment Act, the disbursement of every contract, grant, and loan was cataloged on the publicly accessible website, Recovery.gov. These records provide detailed information regarding the primary recipient of each award, the date on which an award was formally announced, the amount

10These data have been maintained by the U.S. General Services Administration since the decommissioning of Recovery.gov in 2015.
of funding that was allocated to each project, the federal agency that awarded these funds, and the objective of each proposed activity. In an unusual step, the Recovery Act also required the documentation of any subrecipients and vendors that subsequently acquired funding through each award. Therefore, these data include itemized transactions between the primary recipients and subrecipients that executed each ARRA award and the vendors from which additional goods and services were procured. Consequently, the Recovery Act offers an extraordinary view into the transmission of discretionary outlays from the federal government to the broader economy.

For example, on April 1, 2009, the Department of Education’s Office of Special Education and Rehabilitative Services awarded a grant of $759.2 million to the New York State Education Department. As described in the description for ARRA award number 21,251, this grant was intended to assist the State of New York in providing special education and related services to children with disabilities in accordance with Part B of the Individuals with Disabilities Education Act. The New York State Education Department subsequently designated 695 local school districts as subrecipients for the purposes of fulfilling this project. Furthermore, these school districts engaged in 16,810 separate transactions in order to procure $120.6 million in goods and services from almost 6,000 vendors through May 2011.

In all, the ARRA database lists 100,556 contracts, grants, and loans totaling $274.8 billion in funds that were awarded by federal agencies from February 2009 to March 2014. Nearly 200,000 primary recipients, subrecipients, and vendors participated in the 615,226 transactions that comprised these awards. Ultimately, the vendors that supplied additional goods and services accounted for 31 percent of the total funding from this component of the Recovery Act.

According to Section 200.330 of the Code of Federal Regulations, a subrecipient is defined as an entity that is designated by the primary recipient to operate a portion of a federal program and is responsible for making decisions with respect to a given award. In contrast, a vendor is defined as an entity that is strictly engaged in a procurement relationship with either a primary recipient or a subrecipient and solely provides goods and services that are ancillary to the operation of a federal program.
1.3.2 ADP

This paper also utilizes an anonymized dataset of linked employer-employee administrative records from ADP, LLC. ADP is one of the largest human resources companies in the world and serves more than 740,000 clients in over 140 countries.\textsuperscript{12} ADP processes paychecks for 1 out of every 6 workers in the United States alone. As a result, ADP maintains comprehensive payroll information for a sizable portion of the U.S. labor force.

These payroll records offer a number of distinct advantages even in relation to comparable administrative datasets in the United States. As demonstrated by Cajner et al. (2018), the overall ADP client base appears to be fairly representative of the broader U.S. economy in terms of employment size, industry composition, and geographic location. Hence, the ADP payroll data have been shown to be highly correlated with the U.S. Bureau of Labor Statistics Current Employment Statistics.\textsuperscript{13} Likewise, given its role in providing human resources services, ADP is capable of precisely measuring employment, earnings, and hours for a particular worker at remarkably high frequencies. As a result, these data afford a meaningful opportunity to perceive changes in not only the compensation but also the hours worked by each employee. In addition, owing to the breadth of its coverage, ADP can potentially follow individual workers as they transition from one employer to another over time.

For the purposes of this study, I analyze monthly payroll records for a subset of ADP clients beginning in May 2008. Relative to the overall ADP client base, these employers tend to be drawn from the upper end of the size distribution.\textsuperscript{14} Whenever possible, ADP provides a state and a 6-digit North American Industry Classification System (NAICS) industry code for each client. Moreover, with respect to workers, the ADP payroll data indicate whether an employee is paid on either an hourly or a salary basis.

\textsuperscript{12}ADP, LLC. “Corporate Overview.” August 2018.
\textsuperscript{13}ADP Research Institute. “National Employment Report Methodology.”
\textsuperscript{14}Although there are some exceptions, these clients predominantly employ at least 50 workers.
1.3.3 Data Universal Numbering System

The means by which I combine these two datasets is the Data Universal Numbering System (D-U-N-S) by Dun and Bradstreet, Inc. Dun and Bradstreet provides commercial data and analytics to companies around the world, and since 1963, it has assigned businesses a proprietary nine-digit identifier known as the D-U-N-S number. D-U-N-S numbers are granted to entities of all types including corporations, sole proprietorships, non-profit institutions, and government agencies. Additionally, the Data Universal Numbering System preserves the hierarchical structures within companies by linking the D-U-N-S numbers of parents and subsidiaries. Critically, the acquisition of a D-U-N-S number has long served as a prerequisite for conducting business with the U.S. federal government. Hence, the ARRA database typically identifies primary recipients, subrecipients, and vendors by name as well as D-U-N-S number. Moreover, ADP utilizes the Data Universal Numbering System for additional information regarding firm characteristics.

1.3.4 Combining ARRA and ADP Data

Therefore, I construct a sample of firms in the ADP payroll data which were directly connected to the American Recovery and Reinvestment Act. First, I manually assign D-U-N-S numbers to the primary recipients, subrecipients, and vendors that are listed in the ARRA database. I am able to either revise or supply the D-U-N-S numbers of entities in 128,338 of the 615,226 transactions that comprise this portion of the Recovery Act. Moreover, I leverage the connections between parents and subsidiaries in the Data Universal Numbering System in order to distinguish 100,788 separate firms that acquired funding through $248.4 billion in ARRA contracts, grants, and loans. Second, I apply a crosswalk of D-U-N-S numbers to a subset of ADP clients that are initially present in the payroll data from May 2008 to December 2008. And by consolidating the D-U-N-S numbers of parents and their subsidiaries, I am able to enumerate 60,726 distinct firms that processed payrolls through

\[15\] Dun and Bradstreet, Inc. “D-U-N-S Number Fact Sheet.”
ADP during this period. Finally, I combine these two datasets using the Data Universal Numbering System in order to identify 4,385 firms in the ADP payroll data that received $56.8 billion in funds through this component of the Recovery Act.

To put these values in context, there were nearly 5.1 million firms in the United States as of 2008. In other words, this subset of employers from the ADP payroll data constitutes approximately 1 percent of the universe of U.S. firms. Nevertheless, the sample that I construct includes 4.4 percent of the 100,788 entities for which a D-U-N-S number was available in the Recovery Act database. Likewise, this subset of ADP firms accounts for 22.9 percent of the $248.4 billion in ARRA contracts, grants, and loans that were awarded to entities which are also identifiable in the Data Universal Numbering System. Thus, this study considers a substantial share of the discretionary outlays that were allocated through the American Recovery and Reinvestment Act.

1.4 Theoretical Framework

Given the range of possible responses by firms from which goods and services were purchased through the Recovery Act, I develop a theoretical framework for discerning the particular forces that might influence the decisions of an employer which experiences a shift in the demand for its output. Specifically, I adopt the vacancy model of monopsony that is presented in Chapter 10 of Manning (2003) and serves as an alternative to the traditional formulation of labor demand.

Consider an employer that pays wage \( w \) and can create \( J \) jobs at cost \( c \) per position. Ultimately, this firm will employ \( N(w) \) workers in order to produce output that can be sold at price \( p \) per unit. Without loss of generality, it is assumed that \( N(w) \leq J \) so that the firm produces output of \( p \cdot N(w) \). At any point in time, there will be a stock of applicants \( A \) from whom additional workers could potentially be hired if \( N(w) < J \). Since individuals can enter and leave this applicant pool in response to changing circumstances, there will be

uncertainty around not only the value of $A$ but also the determination of employment $N(w)$. Thus, the firm will maximize profits in accordance with the following expression:

$$\pi = (p - w) \cdot E[N(w)] - c \cdot J$$

(1.1)

For the sake of tractability, the pool of potential workers $A$ is assumed to be normally distributed with both mean and variance of $N(w)$ (i.e., $A \sim N(N(w), N(w))$). Furthermore, the number of vacancies at this employer is defined as:

$$V = \frac{J - N(w)}{\sqrt{N(w)}}$$

(1.2)

Consequently, the expected level of employment can be re-written as:

$$E[N] = N(w) - \sqrt{N(w)} \cdot \left[ \phi(V) - V \cdot (1 - \Phi(V)) \right]$$

(1.3)

In other words, the firm’s decision in Equation 1.1 can now be expressed as:

$$\max_{V,w} \left\{ (p - w) \cdot \left[ N(w) - \sqrt{N(w)} \cdot \left[ \phi(V) - V \cdot (1 - \Phi(V)) \right] \right\} - c \cdot \left[ N(w) + \sqrt{N(w)} \cdot V \right] \right\}$$

(1.4)

where $\phi(V)$ is the probability density function of the standard normal distribution and $\Phi(V)$ represents the corresponding cumulative distribution function (i.e., the probability that an employer will have a vacant position). Therefore, the firm must choose number of vacancies $V$ and the wage $w$ such that profits will be maximized.

As shown in Appendix 1.A.1 and 1.A.2, the solution to the firm’s profit maximization problem from Equation 1.4 entails the following first order conditions for the number of vacancies $V$ and the wage $w$, respectively:

$$\Phi(V) = \frac{p - w - c}{p - w}$$

(1.5)

$$\left( p - w - c \right) \cdot \left\{ \frac{1}{2} + \frac{1}{2} \cdot \frac{N(w)}{E[N]} \right\} - \frac{1}{2} \cdot c \cdot \left\{ \frac{J - E[N]}{E[N]} \right\} = \frac{1}{\varepsilon_N}$$

(1.6)
where $\varepsilon_N$ represents the elasticity of labor supply with respect to the wage $w$:

$$\varepsilon_N = \frac{w \cdot N'(w)}{N(w)} \quad (1.7)$$

Intuitively, Equation 1.5 suggests that the likelihood of a vacant position at a particular employer rises (i.e., $\Phi(V) \uparrow$) as the marginal revenue product of labor increases relative to the wage per worker and the cost of creating a new job (i.e., $(p - w - c) \uparrow$). Equation 1.6 indicates that any disparity between the marginal revenue product of labor and the wage at a firm will reflect the elasticity of labor supply. For instance, if labor supply is perfectly elastic (i.e., $\varepsilon_N = \infty$), then this expression will effectively resemble the perfectly competitive outcome in which workers are paid the marginal revenue product of labor (i.e., $p = w$). Conversely, as the labor supply to an individual firm becomes less elastic (i.e., $\varepsilon_N \rightarrow 0$), the wage at that employer will progressively diverge from the marginal revenue product of labor (i.e., $(p - w) \uparrow$). In other words, firms will increasingly possess the power to set wages, and the labor market will be characterized by imperfect competition.

Within this theoretical framework, a positive demand shock from the Recovery Act can be represented as an increase in the marginal revenue product of labor (i.e., $p \uparrow$). Therefore, all else being equal, Equation 1.5 predicts that firms which received Recovery Act funds will be more likely to have vacant positions (i.e., $\Phi(V) \uparrow$) and subsequently increase employment (i.e., $N(w) \uparrow$) in comparison to their unaffected counterparts. In addition, Equation 1.6 implies that the effect of ARRA purchases on wages will ultimately depend on the elasticity of labor supply to a given employer. Notably, if labor markets are imperfectly competitive (i.e., $\varepsilon_N < \infty$), then this model indicates that a firm will respond to an increase in the marginal revenue product of labor (i.e., $p \uparrow$) by not only expanding employment but also raising the wage per worker (i.e., $w \uparrow$).
1.5 Empirical Strategy

1.5.1 Difference-in-Differences

This paper utilizes a difference-in-differences framework as the primary empirical strategy for identifying the impact of ARRA purchases on firms that directly participated in the Recovery Act. To be more precise, I estimate the following log-linear regression specification over a series of firm outcomes:

\[
\ln(Y_{j,t}) = \alpha_j + \gamma_{s,t} + \beta \cdot ARRA_j \cdot Post_{j,t} + \varepsilon_{j,t}
\]

where \(Y_{j,t}\) represents the measure of interest at firm \(j\) in calendar month \(t\), \(\alpha_j\) controls for firm fixed effects, \(\gamma_{s,t}\) accounts for calendar month effects that vary across each 6-digit NAICS industry \(s\), and standard errors \(\varepsilon_{j,t}\) are clustered at the firm level in order to allow for potential correlation within employers. The indicator variable \(ARRA_j\) designates whether or not firm \(j\) ever received ARRA funds, and \(Post_{j,t}\) corresponds to all periods starting from the initial month in which firm \(j\) became involved with the Recovery Act.\(^{17}\) Thus, the coefficient \(\beta\) provides a reduced form estimate of the causal effect of this component of the Recovery Act on each outcome of interest.

1.5.2 Event Study

The fundamental identifying assumption in the difference-in-differences framework is that the outcome under consideration would have exhibited similar trends at employers which were and were not affected by the Recovery Act in the absence of these ARRA contracts, grants, and loans. Consequently, I supplement the previous approach with an event study design of the following form:

\[
\ln(Y_{j,t}) = \alpha_j + \gamma_{s,t} + \sum_{k \in K} \beta_k \cdot ARRA_j \cdot I^k_{j,t} + \varepsilon_{j,t}
\]

\(^{17}\)As explained in Section 1.3, although the administrative payroll records from ADP are available beginning in May 2008, ARRA funds were first awarded starting in February 2009. Hence, I circumscribe the scope of this analysis to the 12 months preceding and the 48 months following the initial demand shock from the Recovery Act in order to maintain a sufficient number of observations per time period.
where $\mathcal{K} = \{-12, -11, \ldots, -2, 0, 1, \ldots, 47, 48\}$ represents the 12 periods preceding and the 48 periods following the initial month in which firm $j$ became involved with the Recovery Act. In contrast to Equation 1.8, this regression specification incorporates a sequence of indicator variables $I_{k,t}^j$ that define each calendar month $t$ in relation to the number of periods $k$ from which firm $j$ initially received ARRA funding. As a result, the coefficients of interest $\beta_k$ capture the dynamic effects of this component of the Recovery Act relative to the period immediately before the initial month of ARRA purchases, which has been normalized to zero. For instance, each of the coefficients corresponding to the periods prior to firm $j$ participating in the Recovery Act (i.e., $\beta_{-12}, \beta_{-11}, \ldots, \beta_{-3}, \beta_{-2}$) would have to be statistically indistinguishable from zero in order to validate the requisite assumption that a particular outcome would have trended along comparable paths at firms which did and did not acquire funds through the Recovery Act. Likewise, these regression coefficients provide an informative assessment of the speed with which an employer would respond to the demand shocks from the Recovery Act.

1.5.3 Dose-Response

As further support for the validity of the difference-in-differences approach, this study also implements a dose-response framework in order to distinguish between comparatively large and small changes in production that were generated by the Recovery Act. Specifically, I standardize the total value of ARRA purchases for each employer by its average monthly payroll as of 2008 and then contrast firms in relation to the median value of these normalized demand shocks through the following regression:

$$\ln(Y_{j,t}) = \alpha_j + \gamma_{s,t} + \beta_1 \cdot Q_j \cdot ARRA_j \cdot Post_{j,t} + \beta_2 \cdot (1 - Q_j) \cdot ARRA_j \cdot Post_{j,t} + \varepsilon_{j,t}$$ (1.10)

where $Q_j$ is an indicator variable that denotes whether or not firm $j$ received greater than the median proportional amount of ARRA funds. Consequently, the coefficients $\beta_1$ and $\beta_2$ reflect the differential impact of experiencing a relatively sizable shift in product demand as a result of the Recovery Act. It is worth emphasizing that this normalization does not
simply represent a bijection of each firm’s overall ARRA purchases. Indeed, the correlation between an employer’s total ARRA funding and the ratio of this amount to its total wage bill prior to the Recovery Act is only 0.45. Thus, this dose-response strategy attempts to exploit the heterogeneity in the relative magnitude of the increases in output that these firms ultimately faced.

1.5.4 Matching Procedure

I apply a matching procedure to the ADP clients in the administrative payroll records in order to identify a suitable group of counterfactual employers for the firms that obtained funding through the Recovery Act. Although the initial subset of firms was drawn from an ostensibly similar pool of relatively large ADP clients, Table 1.2 reveals considerable differences between employers that were and were not directly connected to the Recovery Act even before the enactment of this legislation. On average, companies that received ARRA funds not only employed more than three times as many workers as firms which were not directly connected to the Recovery Act but also paid wages that were 7 percent higher than at employers which never engaged in ARRA transactions.

Therefore, I utilize the coarsened exact matching algorithm that was developed by Iacus et al. (2012) as a means of constructing a sample of firms from the ADP payroll data which more closely resemble one another in terms of observable characteristics. In contrast to “equal percent bias reducing” procedures such as propensity score and Mahalanobis matching, coarsened exact matching relies upon a “monotonic imbalance bounding” method that allows the maximum amount of disparity between the treatment and control groups for each covariate to be determined ex ante. As a result, this approach explicitly establishes balance for each covariate without simultaneously worsening the imbalances along other dimensions of the data.

For the sake of transparency, I implement this coarsened exact matching procedure on the basis of a parsimonious set of firm characteristics. In particular, the algorithm matches
employers that were and were not involved with the Recovery Act exactly on 6-digit NAICS
industry code and then approximately on both employment size and monthly earnings per
worker as of 2008. Given that both covariates appear to reasonably follow normal distribu-
tions, the approximate bins for firm employment and earnings per worker are determined
using Sturges’s rule.18 Once the data have been appropriately grouped according to these
covariates, I further refine the sample by randomly pairing matched firms from the treatment
and control groups within each strata.

The resultant sample includes 2,999 firms that received a total of $19.3 billion in ARRA
funding as well as a corresponding number of employers which never participated in the
Recovery Act. In other words, the coarsened exact matching algorithm successfully assigns
a suitable counterpart to roughly two-thirds of the 4,385 ADP clients that were initially
identified as having been involved with the Recovery Act. The treated and counterfactual
firms in this matched sample now appear to be considerably more balanced not only with
respect to size and pay but also in terms of the composition of each employer’s workforce
(Table 1.2). On average, these firms employed nearly 230 workers who each earned roughly
$4,700 per month and of whom 57 percent were paid on an hourly basis as of 2008.

Furthermore, the matched sample appears to include a broad cross section of firms that
were directly connected to the Recovery Act (Table 1.3). Even though three-quarters of firms
that participated in the Recovery Act initially acquired funding shortly after the legislation
was enacted in 2009, more than 60 percent of employers continued to receive ARRA outlays at
some point over the subsequent four years. Likewise, nearly 60 percent of companies provided
goods and services through the Recovery Act in at least two separate months during this
period. Additionally, despite being somewhat concentrated in manufacturing, professional
services, and health care, every major industry is represented among this sample of employers
that engaged in ARRA transactions.

18Sturges (1926) proposes that \( n \) observations of normally distributed data be grouped into \( k \) bins where
\( k = \lceil 1 + \log_2(n) \rceil \).
1.6 Empirical Results

1.6.1 Firm Employment

First, I consider how firms that received Recovery Act funds responded in terms of total employment. As shown in Column 1 of Table 1.4, employment was 3.5 log points higher at firms from which goods and services were purchased through the Recovery Act relative to their unaffected counterparts. An event study framework provides support for the assumption that employment at firms which were and were not directly connected to the Recovery Act exhibited similar trends prior to the initial month of ARRA purchases (Figure 1.2.A). Furthermore, this methodological approach reveals that employers adjusted their utilization of labor inputs fairly quickly and in a persistent manner upon obtaining funds through an ARRA contract, grant, or loan. Notably, the dynamic pattern depicted in Figure 1.2.A suggests that firms hired additional workers within three months of becoming involved with the Recovery Act, that most of this adjustment occurred by the end of the first year, and that employment remained relatively higher during the subsequent 36 months. These results are compatible with previous studies that have found rather short lags in changes to the labor demand of employers (e.g., Hamermesh 1993, Chapter 7, Section II.B). In addition, the durability of this increase in employment is consistent with the fact that the majority of firms which participated in the Recovery Act engaged in multiple transactions across several periods (Table 1.3).

To further evaluate this causal interpretation, I examine various subsamples of the firms that faced increases in the demand for their output as a result of the Recovery Act. For instance, I contrast employers which were directly connected to the Recovery Act by the amount of funding that they ultimately received from these contracts, grants, and loans relative to their average monthly payroll in 2008 as detailed in Section 1.5.3. According to Figure 1.2.B, the observed increase in employment appears to be entirely attributable to firms that obtained more than the median proportional amount of Recovery Act funds in
this sample. Indeed, companies that acquired less than the median value of these normalized outlays demonstrated statistically insignificant changes in employment (Column 2 of Table 1.4). Similarly, I compare the employment responses of firms that principally served as vendors with those that were mainly primary recipients and subrecipients of ARRA awards.\textsuperscript{19} Reassuringly, I find that the increase in employment among vendors was statistically indistinguishable from the hiring of additional workers by primary recipients and subrecipients (Figure 1.2.C), which corroborates the efficacy of the numerous constraints that governed the disbursement of Recovery Act funds (Section 1.2).

1.6.2 Hours Per Worker

Second, I analyze the quantity of hours that were worked at firms which expanded production in response to the Recovery Act. Interestingly, employees did not appear to work a greater number of hours at firms that received ARRA funding relative to those that did not (Figure 1.3.A). If anything, the evidence suggests that employers which were directly involved with the Recovery Act may have substituted additional workers for longer hours in response to this increase in the demand for their goods and services (Column 1 of Table 1.5). In fact, Figure 1.3.B indicates that companies which exhibited comparatively stronger responses in terms of employment were also more likely to reduce the intensity with which their workers were utilized.

Although such an outcome would appear to contradict the conventional view that adjustments to employment tend to be rather costly, one possible interpretation of this result could be that ARRA contracts, grants, and loans were applied to projects with relatively lengthy durations. As previously noted in Section 1.2, this particular component of the Recovery Act was designed to produce longer-run economic effects. Thus, it may not have been either feasible or optimal for these companies to indefinitely extend the workweeks of their employees. Indeed, Hamermesh (1993, Chapter 6, Section IV) demonstrates that the

\textsuperscript{19}Since an employer could potentially be classified as all three roles depending on the Recovery Act award, I define vendors as firms that obtained at least half of their overall ARRA funds through the provision of goods and services to other primary recipients and subrecipients.
profit-maximizing response to a demand shock will entail changes to employment if the shift in production is lasting and persistent.

1.6.3 Earnings Per Worker

Next, I evaluate the impact of ARRA purchases on the monthly earnings of workers at firms that obtained funding through the Recovery Act. A difference-in-differences regression reveals that workers earned 0.5 log point more at companies that experienced these increases in product demand than at their unaffected counterparts (Column 1 of Table 1.6). Importantly, the corresponding event study estimates suggest that average earnings at firms which did and did not receive ARRA funds displayed comparable trends prior to initially becoming involved with the Recovery Act (Figure 1.4.A). Moreover, the dose-response framework implies that this effect on earnings was concentrated among employers which acquired relatively more funding through the Recovery Act (Figure 1.4.B). Therefore, I find that employees not only earned more but also worked an equivalent number of hours at businesses that participated in ARRA contracts, grants, and loans. In other words, the observed effect on earnings was not mechanically driven by a corresponding increase along the intensive margin of labor utilization.

1.6.4 Wage Per Worker

It can be inferred from the previous results for hours and earnings that workers were paid at a higher rate at employers that increased output as a consequence of the Recovery Act. Indeed, on average, wages were 0.7 log point greater at companies that provided goods and services through the Recovery Act (Column 1 of Table 1.7). An event study analysis reveals two important details regarding the dynamics of this effect on worker compensation (Figure 1.5.A). First, the coefficients prior to the initial month of ARRA purchases substantiate the critical assumption that wages had been trending along similar paths at firms that were and were not directly involved with the Recovery Act. Second, this difference in hourly compensation is not meaningfully apparent until after the first 12 months of becoming in-
involved with the Recovery Act, which is consistent with pervasive evidence that wages tend to be rather sticky in the short run. For instance, Grigsby et al. (2018) find that workers in the ADP payroll data are considerably more likely to experience changes in wages at 12-month intervals. Likewise, Barattieri et al. (2014) estimate an expected duration for wage contracts in the United States of roughly 4 to 5 quarters. Furthermore, a closer examination of the relative magnitudes of these demand shocks affirms the interpretation that this response was attributable to the Recovery Act. I find that the average hourly wage per worker was 1.3 log points higher at firms that received more than the median amount of standardized ARRA outlays, which more than accounts for the observed increase in employee pay (Figure 1.5.B).

Taken together, these results denote the presence of imperfect competition in U.S. labor markets. Since the coefficient in a log-linear regression specification can be interpreted as the percent change in the outcome of interest, the ratio of the estimated effects of the Recovery Act on employment and wages represents the elasticity of labor supply to an individual firm that was expressed in Equation 1.7. Thus, I derive an elasticity of labor supply of 4.8 from the difference-in-differences framework, which corresponds to workers at these firms being paid 21 percent less than the marginal revenue product of labor. In other words, this study finds meaningful evidence of monopsonistic wage-setting by employers in the United States.

1.6.5 Firm Payroll

Finally, I explore how firms reacted to ARRA purchases in terms of total payrolls. As depicted in Column 1 of Table 1.8, overall wage and salary payments were 3.9 log points higher at firms that received ARRA funds than at employers that were not directly connected to the Recovery Act. In addition, the event study design mitigates concerns that payrolls at firms that did and did not participate in the Recovery Act had been proceeding along divergent trends prior to the initial month of ARRA transactions (Figure 1.6.A). The magnitude of this response is compatible with the observed increases in both employment (Column 1 of Table 1.4) and earnings (Column 1 of Table 1.6). Likewise, in accordance with
the previous results, the dose-response empirical strategy indicates that the total wage bill was significantly higher for employers that experienced relatively larger increases in product demand (Figure 1.6.B).

1.7 Implications

1.7.1 Monopsonistic Labor Markets

In light of the ongoing debate regarding the competitiveness of labor markets, the empirical evidence in support of monopsonistic wage-setting by U.S. firms in this paper merits closer scrutiny. To start, it is worth noting that previous studies have found varying degrees of market power among employers. For instance, Depew and Sorensen (2013) cite a range of 1 to 10 for the elasticity of labor supply to an individual firm in their review of this literature. Thus, the elasticity of labor supply with respect to wages that I derive from the difference-in-differences framework lies comfortably within this range of estimates.

Moreover, one of the main features of the classic monopsony model is that the marginal cost of labor includes an additional component for the increase in wages which must be paid to the existing workers at a given firm. Therefore, evidence of wage gains by incumbent workers in response to the demand shocks from the Recovery Act would further corroborate such an interpretation of these empirical results. Consistent with the notion that employers possess the power to set wages, I find that incumbent workers were paid 0.7 log point more at firms which were directly connected to the Recovery Act (Figure 1.7). Notably, this increase in the average wage per existing worker matches the observed change in overall hourly compensation (Column 1 of Table 1.7). In addition, an event study framework demonstrates that the dynamic effects of these ARRA purchases essentially mirror the pattern from the corresponding analysis of all workers (Figure 1.5.A).

Lastly, although I find significant evidence of monopsonistic wage-setting by employers, there are reasons to suspect that even this result may understate the extent to which firms can set wages in the United States. In particular, a more careful inspection of the heterogeneity
in responses across both time and space suggests that the overall elasticity of labor supply may have been attenuated by the broader impact of the Great Recession.

For example, Figure 1.8 plots the coefficients from a series of difference-in-differences regressions that span the first four years after the initial demand shock from the Recovery Act in 12-month increments. As previously documented in Section 1.6.4, the effect of ARRA purchases on wages appeared to operate with a lag and did not materialize until after the first 12 months of becoming involved with the Recovery Act. Consequently, this delayed response in wages coupled with the relatively faster reaction in terms of employment (Section 1.6.1) accounts for the seemingly high, albeit extremely noisy, estimate of the elasticity of labor supply during the initial year of an ARRA project. One possible explanation for this peculiar outcome is that the majority of firms engaged in their first ARRA transactions in 2009 (Table 1.3) as the national unemployment rate surged 2.7 percentage points to a peak of 10.0 percent through October of that year.\footnote{U.S. Bureau of Labor Statistics. \textit{Unemployment Rate.} \textit{Current Population Survey.}} Hence, it is conceivable that a simultaneous increase in labor supply may have served as a countervailing force on wages and obscured the degree to which labor markets were imperfectly competitive during this period. Indeed, the implied elasticity of labor supply falls sharply following the first 12 months and reaches a low of 2.3 by the fourth year of experiencing these increases in product demand.

Likewise, I contrast firms that were directly connected to the Recovery Act by the relative severity of the Great Recession in the state in which each employer was primarily located. Specifically, this analysis incorporates data from the U.S. Bureau of Labor Statistics in order to differentiate companies according to the local economic conditions that they encountered as of 2009.\footnote{U.S. Bureau of Labor Statistics. \textit{Local Area Unemployment Statistics.}} Interestingly, even though employment appears to increase by a statistically similar magnitude at firms which acquired ARRA funds regardless of geographic location (Figure 1.9.A), I find that wages were considerably higher at employers which not only experienced positive demand shocks through the Recovery Act but also operated in

labor markets representing the bottom quartile of state unemployment rates (Figure 1.9.B). Thus, I derive an elasticity of labor supply with respect to wages of just 1.8 for firms that were located in states with comparatively lower levels of unemployment and about 6.5 for employers across the rest of the country (Table 1.9). To put these results in the appropriate context, an elasticity of labor supply to an individual firm of 1.8 corresponds to workers being paid roughly 56 percent less than the marginal revenue product of labor.

1.7.2 Multiplier Effects of Fiscal Policy

A unique aspect of this study is that it provides an unconventional opportunity to assess the efficacy of the contracts, grants, and loans that were awarded through the American Recovery and Reinvestment Act. Traditionally, the macroeconomics literature has relied upon structural general equilibrium models in order to evaluate the aggregate impact of changes in fiscal policies over time (e.g., Ramey 2011). More recently, a number of studies have leveraged variation in the allocation of funding across geographic areas as a means of estimating the multiplier effects of government outlays (e.g., Chodorow-Reich 2017). Although a comprehensive assessment along these lines is beyond the scope of this paper, I am able to measure an essential component of the overall multiplier effects of countercyclical fiscal policy by examining the direct responses of the firms that received ARRA funds.

A commonly used measure in the appraisal of fiscal policies is the cost per job-year. As explained in Section 1.6.1, firms that acquired ARRA funding increased employment by 3.5 log points relative to companies which were not directly connected to the Recovery Act during the 4 years after the initial month of ARRA purchases. Given that these companies employed an average of 231 workers during the 12 months prior to becoming involved with the Recovery Act, this result suggests that firms which engaged in ARRA transactions hired an additional 8 employees across the subsequent 4 years. In other words, employers from which goods and services were purchased through the Recovery Act gained about 32 job-

\footnote{The 13 lowest state unemployment rates during this period corresponded to Iowa, Kansas, Louisiana, Maryland, Montana, Nebraska, New Hampshire, North Dakota, Oklahoma, South Dakota, Vermont, Virginia, and Wyoming.}
years. On average, these firms acquired nearly $6.5 million in Recovery Act funds. Therefore, this analysis implies that the direct effects of the contracts, grants, and loans which were awarded through the Recovery Act generated increases in employment at a cost of $195,634 per job-year.

It is worth emphasizing that this analysis likely overstates the actual cost of the discretionary component of the Recovery Act. Clearly, this estimate of the cost per job-year excludes the indirect effects of the initial ARRA purchases on the employment of other firms that may have subsequently experienced increases in the demand for their own goods and services. Furthermore, any concerns regarding the potential “crowding out” of private sector activity are at least partly mitigated by the fact that ARRA funds were primarily allocated during the midst of the Great Recession. In addition, this study raises the possibility that monopsonistic labor markets may attenuate the intended effectiveness of countercyclical fiscal policy in terms of increasing employment. As noted in Section 1.7.1, employers that became involved with the Recovery Act raised the wages of incumbent workers by 0.7 log point relative to companies which never acquired ARRA funding. In other words, firms diverted some of the additional revenues from the Recovery Act toward the compensation of their existing workers, which virtually reduced their capacity to hire additional employees. Thus, these results demonstrate how monopsonistic wage-setting by employers can effectively dilute the impact of government outlays on the creation of new jobs.

1.8 Conclusion

This paper leverages the combination of administrative payroll records from ADP and a database of discretionary outlays for the American Recovery and Reinvestment Act in order to provide new evidence on not only the behavior of firms but also the competitiveness of labor markets in the United States. Utilizing a difference-in-differences empirical strategy on a matched sample of employers that were and were not directly involved with the Recovery Act, I find that firms primarily responded to these demand shocks by adjusting their
utilization of labor inputs along the extensive margin. Employment rose by 3.5 log points at firms that participated in the Recovery Act following the initial month of ARRA purchases, and this effect remained quite persistent over the subsequent 48 months. However, companies from which goods and services were purchased through the Recovery Act set relatively similar workweeks as firms that did not engage in ARRA transactions. Furthermore, largely driven by an increase in earnings per worker, average hourly wages were 0.7 log point higher at firms that acquired ARRA funds than at employers that never participated in the Recovery Act.

As a whole, these responses reveal the monopsonistic properties of U.S. labor markets. Specifically, the observed changes in employment and wages imply an elasticity of labor supply to an individual firm of 4.8, which is consistent with workers being paid 21 percent less than the marginal revenue product of labor. These effects are all the more notable for having been identified within the context of the Great Recession. Indeed, this study finds evidence to suggest that increases in labor supply which coincided with the economic downturn may have restrained worker compensation during this period. In other words, it seems plausible that even this calculation may represent an underestimate of the degree to which U.S. firms possess the power to set wages.

Finally, this paper demonstrates the importance of directly observing the behavior of employers both at a granular level as well as with a high degree of precision. And in particular, these results affirm the potential advantages of analyzing firm-specific performance shocks in terms of understanding how labor markets function. In light of the continued proliferation of administrative data sources, there may be considerable scope for credible research opportunities of a similar nature going forward.
1.9 Tables and Figures

Table 1.1: Federal Agencies by Total Amount of ARRA Awards

<table>
<thead>
<tr>
<th></th>
<th>Billions of Dollars</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. Office of Elementary &amp; Secondary Education</td>
<td>66.208</td>
</tr>
<tr>
<td>2. Department of Energy</td>
<td>38.537</td>
</tr>
<tr>
<td>3. Federal Highway Administration</td>
<td>27.978</td>
</tr>
<tr>
<td>4. Office of Special Education &amp; Rehabilitative Services</td>
<td>13.678</td>
</tr>
<tr>
<td>5. Department of Housing &amp; Urban Development</td>
<td>11.123</td>
</tr>
<tr>
<td>6. National Institutes of Health</td>
<td>10.181</td>
</tr>
<tr>
<td>7. Federal Transit Administration</td>
<td>9.721</td>
</tr>
<tr>
<td>8. Federal Railroad Administration</td>
<td>9.583</td>
</tr>
<tr>
<td>9. Environmental Protection Agency</td>
<td>7.437</td>
</tr>
<tr>
<td>10. Rural Utilities Service</td>
<td>6.477</td>
</tr>
<tr>
<td>11. Public Buildings Service</td>
<td>5.826</td>
</tr>
<tr>
<td>12. Administration for Children &amp; Families</td>
<td>5.161</td>
</tr>
<tr>
<td>14. National Telecommunication &amp; Information Administration</td>
<td>4.516</td>
</tr>
<tr>
<td>15. Department of Labor</td>
<td>4.451</td>
</tr>
<tr>
<td>16. Department of Justice</td>
<td>4.220</td>
</tr>
<tr>
<td>17. National Science Foundation</td>
<td>2.978</td>
</tr>
<tr>
<td>18. Department of Education</td>
<td>2.969</td>
</tr>
<tr>
<td>19. Department of Health &amp; Human Services</td>
<td>2.402</td>
</tr>
<tr>
<td>20. Health Resources &amp; Services Administration</td>
<td>2.262</td>
</tr>
<tr>
<td>21. Federal Financing Bank</td>
<td>1.960</td>
</tr>
<tr>
<td>22. Department of the Army</td>
<td>1.916</td>
</tr>
<tr>
<td>23. Department of Defense, Excluding Military</td>
<td>1.595</td>
</tr>
<tr>
<td>24. Department of Veterans Affairs</td>
<td>1.545</td>
</tr>
<tr>
<td>25. Department of the Air Force</td>
<td>1.539</td>
</tr>
<tr>
<td>26. Rural Housing Service</td>
<td>1.433</td>
</tr>
<tr>
<td>27. Federal Aviation Administration</td>
<td>1.390</td>
</tr>
<tr>
<td>28. Department of the Navy</td>
<td>1.263</td>
</tr>
<tr>
<td>29. Forest Service</td>
<td>1.164</td>
</tr>
<tr>
<td>30. National Aeronautics &amp; Space Administration</td>
<td>1.103</td>
</tr>
<tr>
<td>174 Additional Federal Agencies</td>
<td>19.393</td>
</tr>
<tr>
<td><strong>Total</strong></td>
<td><strong>274.705</strong></td>
</tr>
</tbody>
</table>

Source: U.S. General Services Administration.
Table 1.2: Summary Statistics

<table>
<thead>
<tr>
<th>Characteristics of Firms:</th>
<th>Full Sample</th>
<th>Matched Sample</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Firms</td>
<td>Firms</td>
</tr>
<tr>
<td></td>
<td>Without</td>
<td>With</td>
</tr>
<tr>
<td>Monthly Employment</td>
<td>174.1</td>
<td>553.7</td>
</tr>
<tr>
<td>Hourly (%)</td>
<td>65.4</td>
<td>60.7</td>
</tr>
<tr>
<td>Salaried (%)</td>
<td>31.7</td>
<td>36.5</td>
</tr>
<tr>
<td>Other (%)</td>
<td>2.9</td>
<td>2.8</td>
</tr>
<tr>
<td>Monthly Payroll (Thous. 2009$)</td>
<td>683.6</td>
<td>2,570.1</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Characteristics of Workers:</th>
<th>Full Sample</th>
<th>Matched Sample</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Firms</td>
<td>Firms</td>
</tr>
<tr>
<td></td>
<td>Without</td>
<td>With</td>
</tr>
<tr>
<td>Female (%)</td>
<td>46.2</td>
<td>42.5</td>
</tr>
<tr>
<td>Age (Years)</td>
<td>41.2</td>
<td>42.2</td>
</tr>
<tr>
<td>Monthly Earnings (2009$)</td>
<td>4,384.6</td>
<td>4,657.2</td>
</tr>
<tr>
<td>Monthly Hours</td>
<td>147.5</td>
<td>147.9</td>
</tr>
<tr>
<td>Wage (2009$ Per Hour)</td>
<td>30.7</td>
<td>32.9</td>
</tr>
</tbody>
</table>

| Number of Firms | 56,341 | 4,385 | 2,999 | 2,999 |

Note: Means are reported as of 2008. Earnings, hours, and wage have been winsorized at the 1\textsuperscript{st} and 99\textsuperscript{th} percentiles over all workers in each month.

Source: ADP; U.S. General Services Administration; author’s calculations.
Table 1.3: Firms With ARRA Purchases in Matched Sample

<table>
<thead>
<tr>
<th>Role in ARRA Awards (%)</th>
<th>Industry (%)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Recipients</td>
<td>Agriculture &amp; Mining</td>
</tr>
<tr>
<td>Vendors</td>
<td>Utilities</td>
</tr>
<tr>
<td>Year of Initial ARRA Purchase (%)</td>
<td>Construction</td>
</tr>
<tr>
<td>2009</td>
<td>Manufacturing</td>
</tr>
<tr>
<td>2010</td>
<td>Wholesale Trade</td>
</tr>
<tr>
<td>2011-2013</td>
<td>Retail Trade</td>
</tr>
<tr>
<td>Year of Final ARRA Purchase (%)</td>
<td>Transportation &amp; Warehousing</td>
</tr>
<tr>
<td>2009</td>
<td>Information</td>
</tr>
<tr>
<td>2010</td>
<td>Finance &amp; Real Estate</td>
</tr>
<tr>
<td>2011-2013</td>
<td>Professional Services</td>
</tr>
<tr>
<td>Months With ARRA Purchases (%)</td>
<td>Management of Companies</td>
</tr>
<tr>
<td>1 Month</td>
<td>Administrative &amp; Support</td>
</tr>
<tr>
<td>2 Months</td>
<td>Educational Services</td>
</tr>
<tr>
<td>3 Months</td>
<td>Health Care</td>
</tr>
<tr>
<td>4-12 Months</td>
<td>Arts &amp; Entertainment</td>
</tr>
<tr>
<td>13 or More Months</td>
<td>Accommodation &amp; Food</td>
</tr>
<tr>
<td>ARRA Purchases Per Firm (Mil. 2009$)</td>
<td>Other Services</td>
</tr>
<tr>
<td>Total ARRA Purchases (Bil. 2009$)</td>
<td>Public Administration</td>
</tr>
</tbody>
</table>

Number of Firms 2,999

Source: ADP; U.S. General Services Administration; author’s calculations.
Table 1.4: Effect of ARRA Purchases on Firm Employment

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Log Employment</td>
<td></td>
<td></td>
</tr>
<tr>
<td>ARRA × Post</td>
<td>0.0351 ***</td>
<td>0.0096</td>
</tr>
<tr>
<td></td>
<td>(0.0062)</td>
<td>(0.0076)</td>
</tr>
<tr>
<td>ARRA × Post × Bottom 50% of Purchases</td>
<td>0.0632 ***</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.0089)</td>
</tr>
<tr>
<td>ARRA × Post × Top 50% of Purchases</td>
<td></td>
<td></td>
</tr>
<tr>
<td>p-Value for Test of Equality</td>
<td>0.0000</td>
<td></td>
</tr>
<tr>
<td>Number of Firms</td>
<td>5,995</td>
<td>5,995</td>
</tr>
<tr>
<td>R-Squared</td>
<td>0.9597</td>
<td>0.9597</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>339,499</td>
<td>339,499</td>
</tr>
</tbody>
</table>

Levels of significance: *** = 0.01, ** = 0.05, * = 0.10
Note: Standard errors are clustered at the firm level and presented in parentheses. Difference-in-differences estimates reflect 12 months before and 48 months after initial ARRA purchase. Total ARRA purchases scaled by the firm’s average monthly payroll in 2008.
Source: ADP; U.S. General Services Administration; author’s calculations.
Table 1.5: Effect of ARRA Purchases on Hours Per Worker

<table>
<thead>
<tr>
<th></th>
<th>Log Hours Per Worker</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
</tr>
<tr>
<td>ARRA × Post</td>
<td>-0.0033</td>
</tr>
<tr>
<td></td>
<td>(0.0036)</td>
</tr>
<tr>
<td>ARRA × Post × Bottom 50% of Purchases</td>
<td>-0.0003</td>
</tr>
<tr>
<td>ARRA × Post × Top 50% of Purchases</td>
<td>-0.0066</td>
</tr>
<tr>
<td>p-Value for Test of Equality</td>
<td></td>
</tr>
<tr>
<td>Number of Firms</td>
<td>5,995</td>
</tr>
<tr>
<td>R-Squared</td>
<td>0.8152</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>339,499</td>
</tr>
</tbody>
</table>

Levels of significance: *** = 0.01, ** = 0.05, * = 0.10
Note: Standard errors are clustered at the firm level and presented in parentheses. Difference-in-differences estimates reflect 12 months before and 48 months after initial ARRA purchase. Total ARRA purchases scaled by the firm’s average monthly payroll in 2008.
Source: ADP; U.S. General Services Administration; author’s calculations.
Table 1.6: Effect of ARRA Purchases on Earnings Per Worker

<table>
<thead>
<tr>
<th></th>
<th>Log Earnings Per Worker</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
</tr>
<tr>
<td>ARRA × Post</td>
<td>0.0048 *</td>
</tr>
<tr>
<td></td>
<td>(0.0027)</td>
</tr>
<tr>
<td>ARRA × Post × Bottom 50% of Purchases</td>
<td>0.0030</td>
</tr>
<tr>
<td></td>
<td>(0.0032)</td>
</tr>
<tr>
<td>ARRA × Post × Top 50% of Purchases</td>
<td>0.0068 *</td>
</tr>
<tr>
<td></td>
<td>(0.0040)</td>
</tr>
<tr>
<td>p-Value for Test of Equality</td>
<td>0.4193</td>
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<td>Number of Firms</td>
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<tr>
<td>R-Squared</td>
<td>0.9115</td>
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<tr>
<td>Number of Observations</td>
<td>339,499</td>
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</tbody>
</table>

Levels of significance: *** = 0.01, ** = 0.05, * = 0.10

Note: Standard errors are clustered at the firm level and presented in parentheses. Difference-in-differences estimates reflect 12 months before and 48 months after initial ARRA purchase. Total ARRA purchases scaled by the firm’s average monthly payroll in 2008.

Source: ADP; U.S. General Services Administration; author’s calculations.
Table 1.7: Effect of ARRA Purchases on Wage Per Worker

<table>
<thead>
<tr>
<th></th>
<th>Log Wage Per Worker</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>ARRA × Post</td>
<td>0.0073 **</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.0036)</td>
<td></td>
</tr>
<tr>
<td>ARRA × Post × Bottom 50% of Purchases</td>
<td>0.0026</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.0046)</td>
<td></td>
</tr>
<tr>
<td>ARRA × Post × Top 50% of Purchases</td>
<td>0.0126 **</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.0051)</td>
<td></td>
</tr>
<tr>
<td>p-Value for Test of Equality</td>
<td>0.1178</td>
<td></td>
</tr>
<tr>
<td>Number of Firms</td>
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<td>5,995</td>
</tr>
<tr>
<td>R-Squared</td>
<td>0.9045</td>
<td>0.9045</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>339,499</td>
<td>339,499</td>
</tr>
</tbody>
</table>

Levels of significance: *** = 0.01, ** = 0.05, * = 0.10

Note: Standard errors are clustered at the firm level and presented in parentheses. Difference-in-differences estimates reflect 12 months before and 48 months after initial ARRA purchase. Total ARRA purchases scaled by the firm’s average monthly payroll in 2008.

Source: ADP; U.S. General Services Administration; author’s calculations.
Table 1.8: Effect of ARRA Purchases on Firm Payroll

<table>
<thead>
<tr>
<th></th>
<th>Log Payroll</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
</tr>
<tr>
<td>ARRA × Post</td>
<td>0.0387 ***</td>
</tr>
<tr>
<td></td>
<td>(0.0065)</td>
</tr>
<tr>
<td>ARRA × Post × Bottom 50%</td>
<td></td>
</tr>
<tr>
<td>of Purchases</td>
<td></td>
</tr>
<tr>
<td>ARRA × Post × Top 50%</td>
<td></td>
</tr>
<tr>
<td>of Purchases</td>
<td></td>
</tr>
<tr>
<td>p-Value for Test of Equality</td>
<td></td>
</tr>
<tr>
<td>Number of Firms</td>
<td>5,995</td>
</tr>
<tr>
<td>R-Squared</td>
<td>0.9501</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>339,499</td>
</tr>
</tbody>
</table>

Levels of significance: *** = 0.01, ** = 0.05, * = 0.10

Note: Standard errors are clustered at the firm level and presented in parentheses. Difference-in-differences estimates reflect 12 months before and 48 months after initial ARRA purchase. Total ARRA purchases scaled by the firm’s average monthly payroll in 2008.

Source: ADP; U.S. General Services Administration; author’s calculations.
Table 1.9: Effect of ARRA Purchases by State Unemployment

<table>
<thead>
<tr>
<th></th>
<th>Log Employment</th>
<th>Log Wage Per Worker</th>
</tr>
</thead>
<tbody>
<tr>
<td>ARRA × Post × Highest 3 Quartiles of State Unemployment</td>
<td>0.0340 ***</td>
<td>0.0052</td>
</tr>
<tr>
<td></td>
<td>(0.0064)</td>
<td>(0.0037)</td>
</tr>
<tr>
<td>ARRA × Post × Bottom Quartile of State Unemployment</td>
<td>0.0438 ***</td>
<td>0.0246 **</td>
</tr>
<tr>
<td></td>
<td>(0.0165)</td>
<td>(0.0107)</td>
</tr>
<tr>
<td>p-Value for Test of Equality</td>
<td>0.5668</td>
<td>0.0790</td>
</tr>
<tr>
<td>Number of Firms</td>
<td>5,995</td>
<td>5,995</td>
</tr>
<tr>
<td>R-Squared</td>
<td>0.9597</td>
<td>0.9045</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>339,499</td>
<td>339,499</td>
</tr>
</tbody>
</table>

Levels of significance: *** = 0.01, ** = 0.05, * = 0.10
Note: Standard errors are clustered at the firm level and presented in parentheses. Difference-in-differences estimates reflect 12 months before and 48 months after initial ARRA purchase. State unemployment rate of firm measured in 2009.
Figure 1.1: American Recovery and Reinvestment Act by Category
Billions of Dollars

Note: Regression includes firm fixed effects and industry-by-time effects. Dashed line reflects difference-in-differences estimate. Standard errors are clustered at the firm level. Bands reflect 95 percent confidence interval. Month prior to initial ARRA purchase set to zero.
Source: ADP; U.S. General Services Administration; author's calculations.
Figure 1.2.B: Event Study Estimates for Firm Employment by Total ARRA Purchases

Log Employment

Note: Total ARRA purchases are scaled by monthly payroll in 2008. Regression includes firm fixed effects and industry-by-time effects. Dashed lines reflect difference-in-differences estimates. Standard errors are clustered at the firm level. Bands reflect 95 percent confidence interval. Month prior to initial ARRA purchase set to zero.
Source: ADP; U.S. General Services Administration; author’s calculations.
Figure 1.2.C: Event Study Estimates for Firm Employment by ARRA Role

Log Employment

Note: Regression includes firm fixed effects and industry-by-time effects. Dashed lines reflect difference-in-differences estimates. Standard errors are clustered at the firm level. Bands reflect 95 percent confidence interval. Month prior to initial ARRA purchase set to zero.

Source: ADP; U.S. General Services Administration; author’s calculations.
Figure 1.3.A: Event Study Estimates for Hours Per Worker

Log Hours Per Worker (Total Per Month)

Note: Regression includes firm fixed effects and industry-by-time effects. Dashed line reflects difference-in-differences estimate. Standard errors are clustered at the firm level. Bands reflect 95 percent confidence interval. Month prior to initial ARRA purchase set to zero.
Source: ADP; U.S. General Services Administration; author’s calculations.
Figure 1.3.B: Event Study Estimates for Hours Per Worker by Total ARRA Purchases

Note: Total ARRA purchases are scaled by monthly payroll in 2008. Regression includes firm fixed effects and industry-by-time effects. Dashed lines reflect difference-in-differences estimates. Standard errors are clustered at the firm level. Bands reflect 95 percent confidence interval. Month prior to initial ARRA purchase set to zero.

Source: ADP; U.S. General Services Administration; author’s calculations.
Figure 1.4.A: Event Study Estimates for Earnings Per Worker

Log Earnings Per Worker (2009$ Per Month)

Months Since Initial ARRA Purchase

Note: Regression includes firm fixed effects and industry-by-time effects. Dashed line reflects difference-in-differences estimate. Standard errors are clustered at the firm level. Bands reflect 95 percent confidence interval. Month prior to initial ARRA purchase set to zero.

Source: ADP; U.S. General Services Administration; author’s calculations.
Figure 1.4.B: Event Study Estimates for Earnings Per Worker by Total ARRA Purchases

Log Earnings Per Worker (2009$ Per Month)

Note: Total ARRA purchases are scaled by monthly payroll in 2008. Regression includes firm fixed effects and industry-by-time effects. Dashed lines reflect difference-in-differences estimates. Standard errors are clustered at the firm level. Bands reflect 95 percent confidence interval. Month prior to initial ARRA purchase set to zero.

Source: ADP; U.S. General Services Administration; author's calculations.
Figure 1.5.A: Event Study Estimates for Wage Per Worker

Log Wage Per Worker (2009$ Per Hour)

Note: Regression includes firm fixed effects and industry-by-time effects. Dashed line reflects difference-in-differences estimate. Standard errors are clustered at the firm level. Bands reflect 95 percent confidence interval. Month prior to initial ARRA purchase set to zero.

Source: ADP; U.S. General Services Administration; author’s calculations.
Figure 1.5.B: Event Study Estimates for Wage Per Worker by Total ARRA Purchases

Note: Total ARRA purchases are scaled by monthly payroll in 2008. Regression includes firm fixed effects and industry-by-time effects. Dashed lines reflect difference-in-differences estimates. Standard errors are clustered at the firm level. Bands reflect 95 percent confidence interval. Month prior to initial ARRA purchase set to zero.
Source: ADP; U.S. General Services Administration; author’s calculations.
Figure 1.6.A: Event Study Estimates for Firm Payroll

Note: Regression includes firm fixed effects and industry-by-time effects. Dashed line reflects difference-in-differences estimate. Standard errors are clustered at the firm level. Bands reflect 95 percent confidence interval. Month prior to initial ARRA purchase set to zero.

Source: ADP; U.S. General Services Administration; author’s calculations.
Figure 1.6.B: Event Study Estimates for Firm Payroll by Total ARRA Purchases

Log Payroll (2009$ Per Month)

Note: Total ARRA purchases are scaled by monthly payroll in 2008. Regression includes firm fixed effects and industry-by-time effects. Dashed lines reflect difference-in-differences estimates. Standard errors are clustered at the firm level. Bands reflect 95 percent confidence interval. Month prior to initial ARRA purchase set to zero.

Source: ADP; U.S. General Services Administration; author’s calculations.
Figure 1.7: Event Study Estimates for Wage Per Incumbent Worker

Log Wage Per Incumbent Worker (2009$ Per Hour)

Note: Regression includes firm fixed effects and industry-by-time effects. Dashed line reflects difference-in-differences estimate. Standard errors are clustered at the firm level. Bands reflect 95 percent confidence interval. Month prior to initial ARRA purchase set to zero.

Source: ADP; U.S. General Services Administration; author’s calculations.
Figure 1.8: Elasticity of Labor Supply Per Year

Elasticity of Labor Supply

Years Since Initial ARRA Purchase

Note: Regressions of log employment and log wage per worker include firm fixed effects and industry-by-time effects. Bars reflect bootstrapped 95 percent confidence intervals. Confidence interval for observation one year since initial ARRA purchase exceeds range of y-axis.

Source: ADP; U.S. General Services Administration; author’s calculations.
Figure 1.9.A: Event Study Estimates of Firm Employment by State Unemployment

Note: State unemployment rate of firm measured in 2009. Regression includes firm fixed effects and industry-by-time effects. Dashed lines reflect difference-in-differences estimates. Standard errors are clustered at the firm level. Bands reflect 95 percent confidence interval. Month prior to initial ARRA purchase set to zero. Source: ADP; U.S. General Services Administration; author’s calculations.
Figure 1.9.B: Event Study Estimates of Wage Per Worker by State Unemployment

Log Wage Per Worker (2009$ Per Hour)

Note: State unemployment rate of firm measured in 2009. Regression includes firm fixed effects and industry-by-time effects. Dashed lines reflect difference-in-differences estimates. Standard errors are clustered at the firm level. Bands reflect 95 percent confidence interval. Month prior to initial ARRA purchase set to zero. Source: ADP; U.S. General Services Administration; author’s calculations.
1.A Appendix

1.A.1 First Order Condition With Respect to Vacancies

Recall the following expression for the profit maximization problem of a firm in Equation 1.4:

\[
\max_{V,w} \left\{ (p - w) \cdot \left[ N(w) - \sqrt{N(w)} \cdot \left[ \phi(V) - V \cdot (1 - \Phi(V)) \right] \right] - c \cdot \left[ N(w) + \sqrt{N(w)} \cdot V \right] \right\} \tag{1.A.1}
\]

The first order condition with respect to the number of vacancies \( V \) is as follows:

\[
-(p - w) \cdot \sqrt{N(w)} \cdot \left[ \phi'(V) - 1 + \Phi(V) + V \cdot \phi(V) \right] - c \cdot \sqrt{N(w)} = 0 \tag{1.A.2}
\]

Re-arranging terms in this expression:

\[
-(p - w) \cdot \sqrt{N(w)} \cdot \left[ \phi'(V) - 1 + \Phi(V) + V \cdot \phi(V) \right] = c \cdot \sqrt{N(w)} \tag{1.A.3}
\]

Dividing both sides of this expression by \( \sqrt{N(w)} \):

\[
-(p - w) \cdot \left[ \phi'(V) - 1 + \Phi(V) + V \cdot \phi(V) \right] = c \tag{1.A.4}
\]

Given that the derivative of the probability density function of the standard normal distribution is \( \phi'(V) = -V \cdot \phi(V) \):

\[
-(p - w) \cdot \left[ -1 + \Phi(V) - V \cdot \phi(V) + V \cdot \phi(V) \right] = c \tag{1.A.5}
\]

And after simplifying this expression:

\[
(p - w) - (p - w) \cdot \Phi(V) = c \tag{1.A.6}
\]

In other words:

\[
\Phi(V) = \frac{p - w - c}{p - w} \tag{1.A.7}
\]
1.A.2 First Order Condition With Respect to Wage

Recall the following expression for the profit maximization problem of a firm in Equation 1.4:

$$\max_{V,w} \left\{ (p - w) \cdot \left[ N(w) - \sqrt{N(w)} \cdot \left[ \phi(V) - V \cdot (1 - \Phi(V)) \right] \right] - \right.$$  
$$\left. c \cdot \left[ N(w) + \sqrt{N(w)} \cdot V \right] \right\}$$  \hspace{1cm} (1.A.8)

The first order condition with respect to the wage $w$ is as follows:

$$p \cdot N'(w) - \frac{1}{2} \cdot \frac{N''(w)}{\sqrt{N(w)}} \cdot \left[ \phi(V) - V \cdot (1 - \Phi(V)) \right] \cdot p -$$
$$N(w) - w \cdot N'(w) + \sqrt{N(w)} \cdot \left[ \phi(V) - V \cdot (1 - \Phi(V)) \right] +$$
$$\frac{1}{2} \cdot \frac{N'(w)}{\sqrt{N(w)}} \cdot \phi(V) - V \cdot (1 - \Phi(V)) \cdot w -$$
$$c \cdot N'(w) - \frac{1}{2} \cdot \frac{N'(w)}{\sqrt{N(w)}} \cdot c \cdot V = 0$$  \hspace{1cm} (1.A.9)

Re-arranging terms in this expression:

$$(p - w - c) \cdot N'(w) -$$
$$\frac{1}{2} \cdot \frac{N'(w)}{\sqrt{N(w)}} \cdot \left\{ (p - w) \cdot \left[ \phi(V) - V \cdot (1 - \Phi(V)) \right] + c \cdot V \right\} -$$
$$N(w) + \sqrt{N(w)} \cdot \left[ \phi(V) - V \cdot (1 - \Phi(V)) \right] = 0$$  \hspace{1cm} (1.A.10)

Recall that $\left[ \phi(V) - V \cdot (1 - \Phi(V)) \right] = \frac{N(w) - \mathbb{E}[N]}{\sqrt{N(w)}}$ in Equation 1.3:
\[
\begin{align*}
(p - w - c) \cdot N'(w) - & \quad \frac{1}{2} \cdot \frac{N'(w)}{\sqrt{N(w)}} \cdot \left\{ (p - w) \cdot \left[ \frac{N(w) - \mathbb{E}[N]}{\sqrt{N(w)}} \right] + c \cdot V \right\} - \\
& \quad N(w) + \sqrt{N(w)} \cdot \left[ \frac{N(w) - \mathbb{E}[N]}{\sqrt{N(w)}} \right] = 0
\end{align*}
\]

(1.A.11)

And after simplifying this expression:

\[
\begin{align*}
(p - w - c) \cdot N'(w) - & \quad \frac{1}{2} \cdot (p - w) \cdot N'(w) + \\
& \quad \frac{1}{2} \cdot (p - w) \cdot \left[ \frac{N'(w) \cdot \mathbb{E}[N]}{N(w)} \right] - \frac{1}{2} \cdot \frac{N'(w)}{\sqrt{N(w)}} \cdot c \cdot V - N(w) + N(w) = \mathbb{E}[N]
\end{align*}
\]

(1.A.12)

Since \( V = \frac{J - N(w)}{\sqrt{N(w)}} \) according to Equation 1.2:

\[
\begin{align*}
(p - w - c) \cdot N'(w) - & \quad \frac{1}{2} \cdot (p - w) \cdot N'(w) + \\
& \quad \frac{1}{2} \cdot (p - w) \cdot \left[ \frac{N'(w) \cdot \mathbb{E}[N]}{N(w)} \right] - \frac{1}{2} \cdot \frac{N'(w)}{\sqrt{N(w)}} \cdot c \cdot \left[ \frac{J - N(w)}{\sqrt{N(w)}} \right] = \mathbb{E}[N]
\end{align*}
\]

(1.A.13)

In other words:

\[
\begin{align*}
(p - w - c) \cdot N'(w) - & \quad \frac{1}{2} \cdot (p - w) \cdot N'(w) + \\
& \quad \frac{1}{2} \cdot (p - w) \cdot \left[ \frac{N'(w) \cdot \mathbb{E}[N]}{N(w)} \right] - \frac{1}{2} \cdot \frac{N'(w)}{N(w)} \cdot c \cdot J + \frac{1}{2} \cdot N'(w) \cdot c = \mathbb{E}[N]
\end{align*}
\]

(1.A.14)

Dividing both sides of this expression by \( N'(w) \):

\[
\begin{align*}
(p - w - c) - & \quad \frac{1}{2} \cdot (p - w - c) + \\
& \quad \frac{1}{2} \cdot (p - w) \cdot \frac{\mathbb{E}[N]}{N(w)} - \frac{1}{2} \cdot \frac{1}{N(w)} \cdot c \cdot J = \frac{\mathbb{E}[N]}{N'(w)}
\end{align*}
\]

(1.A.15)

Multiplying both sides of this expression by \( N(w) \):

\[
\begin{align*}
\frac{1}{2} \cdot (p - w - c) \cdot N(w) + \\
& \quad \frac{1}{2} \cdot (p - w) \cdot \mathbb{E}[N] - \frac{1}{2} \cdot c \cdot J = \frac{\mathbb{E}[N] \cdot N(w)}{N'(w)}
\end{align*}
\]

(1.A.16)
Dividing both sides of this expression by $\mathbb{E}[N]$:

\[
(p - w - c) \cdot \frac{1}{2} \cdot \frac{N(w)}{\mathbb{E}[N]} + \frac{1}{2} \cdot (p - w) - \frac{1}{2} \cdot \frac{1}{\mathbb{E}[N]} \cdot c \cdot J = \frac{N(w)}{N'(w)}
\] (1.A.17)

Adding $\frac{1}{2} \cdot c \cdot \frac{\mathbb{E}[N]}{\mathbb{E}[N]} - \frac{1}{2} \cdot c$ to the left-hand side of this expression:

\[
(p - w - c) \cdot \frac{1}{2} \cdot \frac{N(w)}{\mathbb{E}[N]} + \frac{1}{2} \cdot (p - w) - \frac{1}{2} \cdot \frac{1}{\mathbb{E}[N]} \cdot c \cdot J + \underbrace{\frac{1}{2} \cdot c \cdot \frac{\mathbb{E}[N]}{\mathbb{E}[N]} - \frac{1}{2} \cdot c}_{=0} = \frac{N(w)}{N'(w)}
\] (1.A.18)

Further simplifying this expression:

\[
(p - w - c) \cdot \left\{ \frac{1}{2} + \frac{1}{2} \cdot \frac{N(w)}{\mathbb{E}[N]} \right\} - \frac{1}{2} \cdot c \cdot \left\{ \frac{J - \mathbb{E}[N]}{\mathbb{E}[N]} \right\} = \frac{N(w)}{N'(w)}
\] (1.A.19)

Dividing both sides of this expression by $w$:

\[
\underbrace{(p - w - c) \cdot \left\{ \frac{1}{2} + \frac{1}{2} \cdot \frac{N(w)}{\mathbb{E}[N]} \right\} - \frac{1}{2} \cdot c \cdot \left\{ \frac{J - \mathbb{E}[N]}{\mathbb{E}[N]} \right\}}_{w} = \frac{N(w)}{w \cdot N'(w)}
\] (1.A.20)

Finally, substituting $\varepsilon_N = \frac{w \cdot N'(w)}{N(w)}$:

\[
\underbrace{(p - w - c) \cdot \left\{ \frac{1}{2} + \frac{1}{2} \cdot \frac{N(w)}{\mathbb{E}[N]} \right\} - \frac{1}{2} \cdot c \cdot \left\{ \frac{J - \mathbb{E}[N]}{\mathbb{E}[N]} \right\}}_{w} = \frac{1}{\varepsilon_N}
\] (1.A.21)
2 Rent Sharing Within Firms

2.1 Introduction

Researchers have long maintained an interest in measuring the extent to which economic rents are shared between firms and workers. These studies have tended to attract considerable attention for two reasons. First, the presence of rent sharing between employers and workers can be indicative of imperfect competition in labor markets (e.g., Manning 2011). Second, there is an extensive literature which suggests that disparities in productivity across firms as well as industries may contribute to wage inequality (Slichter 1950; Dickens and Katz 1987; Katz and Summers 1989; Abowd and Lemieux 1993; Van Reenen 1996; Abowd et al. 1999; Guiso et al. 2005; Card et al. 2013, 2014, 2016; Kline et al. 2017).

As succinctly cataloged by Card et al. (2018), the advent of linked employer-employee datasets has sparked a revival of research within this domain in recent years. Nevertheless, these microdata have not obviated the need for exogenous sources of variation in firm productivity for the purposes of producing credible empirical estimates. Without such productivity shocks, any positive correlation between firm rents and worker earnings could reflect multiple competing explanations rather than a clear violation of competitive behavior regarding the determination of wages.

We contribute to this literature by drawing upon an innovative study from Bertrand and Mullainathan (2001) that utilizes the price of crude oil as an instrument for the economic rents of petroleum firms in the United States. Despite their significance to the productivity of petroleum companies, oil prices are effectively determined by a variety of factors that are well outside the control of any particular firm. In essence, this critical component of firm productivity cannot be plausibly ascribed to the skills and actions of workers. Therefore,

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1This chapter is co-authored with Alan B. Krueger. We benefited from helpful comments by participants in various Princeton seminars as well as attendees of the 2018 Allied Social Science Associations Annual Meeting and A Conference in Honor of Joe Altonji’s 65th Birthday. This paper uses anonymized payroll records from ADP, LLC, which would not have been possible without the approval and assistance of Jan Siegmund, Ahu Yildirimaz, Sinem Buber Singh, and Mita Goldar. The authors are solely responsible for any errors as well as the views expressed herein.
Bertrand and Mullainathan exploit fluctuations in the price of crude petroleum in order to provide compelling evidence that the chief executive officers of oil corporations are routinely compensated for lucky outcomes over which they have no discernible effect.\footnote{Davis and Hausman (2018) subsequently update these results using more recent data and confirm that executives of publicly traded oil companies continue to be compensated for observable forms of luck.} Our paper expands the scope of their analysis by examining how exogenous shocks to firm rents affect the wages of different types of workers within the petroleum industry. Specifically, we leverage a unique dataset of anonymized administrative payroll records from ADP, LLC that allows us to evaluate the impact of changes in oil prices on the earnings of petroleum extraction workers in the United States.

Consistent with the existing literature, we find that firms tend to share a relatively limited portion of economic rents with their workers. On average, a 1 percent increase in the price of crude oil leads to a 0.05 percent gain in the earnings of workers at petroleum extraction companies in the United States. However, this result obscures the striking heterogeneity that we observe in the ADP payroll data regarding the manner in which rents are shared with workers. For instance, we distinguish significant asymmetry in the elasticity of rent sharing with respect to positive and negative changes in petroleum prices. The corresponding effect on wages appears to be roughly four times larger for increases in the price of crude oil than for decreases in this exogenous measure of firm productivity. In addition, we estimate that workers who earn more receive higher proportions of firm rents than their lower paid counterparts at the same company. Notably, the top earner at a U.S. petroleum extraction firm experiences an increase in earnings of 0.30 percent in response to a 1 percent rise in oil prices. In our view, these findings can be understood within the context of the bargaining model that was developed by de Menil (1971) and Svejnar (1986) in which higher paid workers represent insiders who possess comparatively greater abilities to negotiate over wages.

The paper is organized as follows. Section 2.2 provides an overview of the petroleum industry as well as a description of the production of crude oil. Then, Section 2.3 describes
the ADP payroll data that allow us to evaluate the earnings of various types of workers within a given firm. Section 2.4 presents a theoretical framework for understanding how workers bargain over firm rents, and Section 2.5 formulates our empirical approach to estimating the rent sharing parameters from such a model. The empirical results from this analysis are presented and discussed in Section 2.6.

2.2 Institutional Details

Petroleum serves as the largest source of energy not only in the United States but also around the world. In 2016, oil accounted for nearly a third of the total supply of primary energy and more than 40 percent of power consumption worldwide (International Energy Agency 2018). Accordingly, the aggregate oil industry reflects an extensive international network of firms that are highly responsive to global shifts in the supply of and demand for petroleum. As a result, the prices of both crude oil as well as petroleum products in the United States are largely determined by external factors. Moreover, it is effectively impossible for any single U.S. participant to exert unilateral control over this particular market.

In broad strokes, the petroleum industry comprises three major segments: upstream, midstream, and downstream.³ Upstream operations encompass a range of capital-intensive activities corresponding to the exploration of petroleum reservoirs and the extraction of crude oil. The midstream segment is responsible for the storage and transportation of unprocessed crude oil, which is ultimately purchased by downstream firms. Finally, downstream companies such as petroleum refineries engage in the physical transformation of crude oil into a variety of commercial products including gasoline, jet fuel, and heating oil.

Thus, at a fundamental level, the upstream segment reaps greater revenues in response to increases in the price of crude oil. Conversely, downstream firms tend to face higher input costs and tighter profit margins when oil prices rise. Furthermore, given the widespread

reliance on refined petroleum products as sources of energy, these price effects may also
translate into higher expenses across a spectrum of industries throughout the economy. For
instance, the profitability of air transportation companies can be quite sensitive to fluctua-
tions in the cost of jet fuel.

In accordance with the North American Industry Classification System (NAICS), this paper considers three separate types of U.S. companies that regard petroleum prices from
divergent perspectives. Our principal focus is NAICS code 211111, which includes “(1) the
exploration, development and/or the production of petroleum or natural gas from wells in
which the hydrocarbons will initially flow [...] or (2) the production of crude petroleum from
surface shales or tar sands or from reservoirs in which the hydrocarbons are semisolids.”⁴ We
contrast these upstream oil companies with NAICS code 324110, which represents petroleum
refineries.⁵ In addition, our analysis incorporates NAICS code 481, which comprises various
firms that “provide air transportation of passengers and/or cargo using aircraft, such as
airplanes and helicopters.”⁶

2.3 Data

2.3.1 U.S. Energy Information Administration

In practice, there are multiple benchmarks for the price of crude oil corresponding
to differences in grade and geographic origin. For instance, the main reference point in
the United States is West Texas Intermediate crude oil, which is produced domestically
and traded at Cushing, Oklahoma. Alternatively, Brent crude oil from the North Sea has
traditionally served as the primary index across global markets. Nevertheless, given the
relative homogeneity of crude oil, these prices tend to move in tandem over time.

For the purposes of this analysis, we rely upon the composite refiner acquisition cost
of crude oil, which has been calculated by the U.S. Energy Information Administration from

⁶U.S. Census Bureau. “2012 NAICS Definition: 481 Air Transportation.”
a monthly census of all petroleum refineries since January 1974.\textsuperscript{7} This index comprises a weighted average of the domestic and imported crude oil costs that are paid by U.S. refineries including all charges associated with the acquisition, transportation, and storage of petroleum.\textsuperscript{8} Consequently, the composite refiner acquisition cost represents a comprehensive measure of the various prices that are salient to firms throughout the petroleum industry (Figure 2.1).

\subsection*{2.3.2 ADP}

This paper utilizes an anonymized dataset of linked employer-employee payroll records from ADP, LLC in order to evaluate the distribution of rents that are generated by changes in the price of crude oil. ADP provides human resources services to more than 740,000 clients in over 140 countries and processes paychecks for 1 out of every 6 workers in the United States.\textsuperscript{9} As a function of the expansive scope of its business operations, ADP possesses a uniquely detailed and extremely timely perspective on the earnings of U.S. workers.

We analyze monthly payroll records for a subset of ADP clients in the United States from May 2008 to December 2018. Since ADP categorizes its clients within relatively narrow industries, we are able to isolate a group of employers that primarily engaged in the extraction of crude petroleum (NAICS code 211111) during this period. In addition, we construct a comparison sample of petroleum refineries (NAICS code 324110) and air transportation firms (NAICS code 481) given the potential sensitivity of these particular industries to fluctuations in the price of crude oil.

The ADP payroll data reveal considerable heterogeneity in the employment and earnings of workers across these three industries (Table 2.1). Notably, the upstream segment of the petroleum industry appears to be primarily dominated by men; fewer than a quarter of workers in petroleum extraction firms are female. This gender disparity in employment is not nearly as large at either petroleum refineries or air transportation firms. Moreover,

\footnotesize
\textsuperscript{7}U.S. Energy Information Administration. “Form EIA-14: Refiners’ Monthly Cost Report.”
\textsuperscript{8}U.S. Energy Information Administration. “Petroleum Marketing Explanatory Notes.”
\textsuperscript{9}ADP, LLC. “Corporate Overview.”
workers who participate in the exploration and production of crude oil are paid substantially higher than employees in many other U.S. industries. Since 2008, earnings of upstream petroleum employees have averaged more than $120,000 per year, which is roughly double the annual compensation per worker at oil refineries and air transportation companies during this period.

2.3.3 Standard and Poor’s

We also examine company filings from Standard and Poor’s Compustat database in order to quantify the effect of changes in petroleum prices on firm rents in the United States. As documented by Card et al. (2018), previous studies have developed a variety of empirical proxies for the theoretical concept of economic rents. Consistent with the existing literature, this paper considers two alternative measures of firm productivity. First, we approximate value added per worker as the difference between total revenues and cost of goods sold divided by firm employment. Second, following Kline et al. (2017), gross surplus per worker is calculated as the sum of earnings before income, taxes, depreciation, and amortization plus total payroll divided by the number of employees. Since data on labor costs are generally unavailable in Compustat, we rely upon the Quarterly Census of Employment and Wages in order to estimate each firm’s wage bill using average compensation per worker for the corresponding industry.10

Furthermore, this paper analyzes how rents are shared with the chief executive officers of U.S. petroleum firms using the ExecuComp supplement to the Compustat database. The ExecuComp data are compiled from the proxy statements that corporations file with the U.S. Securities and Exchange Commission prior to each annual meeting of shareholders.11 These proxy statements reveal both the amount as well as the forms of compensation that the executives of publicly traded companies receive in a given year. In light of recent changes to the reporting of executive pay, we adopt the conventions that were utilized by Davis and

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Hausman (2018) with respect to the valuation of non-equity incentives, deferred compensation, and perquisites.\textsuperscript{12} Moreover, in order to facilitate a more direct comparison to the ADP payroll data, our measurement of the total earnings for each chief executive officer explicitly excludes the value of stocks and options that were granted during the year.

2.4 Theoretical Framework

The context for our study is a modified version of the static bargaining model that is formally presented in Card et al. (2014). This model assumes that, in each period $t$, there are two types of workers, insiders $L_i^I$ and outsiders $L_i^O$, who possess varying degrees of bargaining power within a firm. Consistent with the theory that was established by Lindbeck and Snower (1988), insiders represent a privileged subset of the incumbent workers within each firm for whom the cost of replacement is quite high. In contrast, outsiders are defined as employees who can essentially be replaced at minimal cost and occupy a relatively disadvantaged position within a firm.

The firm bargains with both groups of workers over their particular wages $w_i^I$ and $w_i^O$ in each period $t$. If no agreement is reached, all parties will receive their outside options, which are assumed to be zero without loss of generality. However, if an agreement can be struck between an employer and its workers, the firm will earn quasi-rents $R_t(\theta_t)$ in which $\theta_t$ represents an exogenous shock to revenues in period $t$. As a consequence, the firm faces the following problem:

$$\max_{w_i^I, w_i^O} R_t(\theta_t) - w_i^I \cdot L_i^I - w_i^O \cdot L_i^O$$ (2.1)

Given this setup, the bargaining in each period $t$ maximizes the following generalized Nash objective function:

$$\left[ w_i^I \cdot L_i^I \right]^{\gamma_I} \left[ w_i^O \cdot L_i^O \right]^{\gamma_O} \left[ R_t(\theta_t) - w_i^I \cdot L_i^I - w_i^O \cdot L_i^O \right]^{1 - \gamma_I - \gamma_O}$$ (2.2)

where $\gamma^I$ and $\gamma^O$ symbolize the bargaining power of insiders and outsiders, respectively. Since insiders are characterized by their higher replacement costs in comparison to outsiders, $\gamma^I > \gamma^O$, and by construction, $\gamma^I + \gamma^O < 1$. Thus, the solution to this model is represented by the optimal wages $w^I_t$ and $w^O_t$ that split the gains from trade of the quasi-rents $R_t(\theta_t)$:

\[
\begin{align*}
  w^I_t &= \gamma^I \cdot \frac{R_t(\theta_t)}{L^I_t} \\
  w^O_t &= \gamma^O \cdot \frac{R_t(\theta_t)}{L^O_t}
\end{align*}
\] (2.3)

In other words, the wage for each type of worker will be determined by its comparative bargaining power over the total amount of firm rents. Clearly, this model can be further adapted in order to accommodate the presence of multiple groups of insiders within a single firm.

### 2.5 Empirical Strategy

#### 2.5.1 Firm Rents

Prior to quantifying the degree to which rents are shared with workers, we first validate the premise that the price of crude oil represents an exogenous source of variation in firm productivity. In theory, the relationship between oil prices and productivity can be established via the following expression:

\[
\ln \left( \frac{R_{j,t}}{L_{j,t}} \right) = \phi_j + \beta \cdot \ln (P_t) + t_j + \varepsilon_{j,t}
\] (2.5)

where $R_{j,t}$ reflects the quasi-rents of firm $j$ in year $t$, $L_{j,t}$ captures employment at firm $j$ in year $t$, $P_t$ is the price of crude oil in year $t$, $\phi_j$ accounts for firm fixed effects, and $t_j$ corresponds to distinct linear time trends for each firm $j$. Accordingly, the coefficient $\beta$ should provide a reduced form estimate of the elasticity of firm rents with respect to changes in oil prices.

Nevertheless, a practical concern with such an approach is that firm rents are not
strictly positive over time. Indeed, the Compustat data demonstrate that corporations periodically report negative measures of productivity, which are incapable of being expressed as logarithms by definition. Therefore, in light of this issue, we consider the following first differences transformation of the log-log model from Equation 2.5:

$$
\Delta \ln \left( \frac{R_{j,t}}{L_{j,t}} \right) = \phi_j + \beta \cdot \Delta \ln (P_t) + \epsilon_{j,t} \tag{2.6}
$$

where $\Delta$ symbolizes the first difference operator and $\phi_j$ reflects the fixed effects that remain from the firm-specific linear time trends $t_j$. This alternative specification allows us to approximate the first difference of the logarithm of a value with the percentage change in that particular measure, which can always be calculated for negative numbers.

### 2.5.2 Worker Earnings

We empirically estimate the rent sharing parameters in Equations 2.3 and 2.4 through the following regression:

$$
\ln (w_{i,j,t}) = \alpha_i + \beta \cdot \ln (P_t) + \mu_{i,t} + \mu_{i,t}^2 + \psi_{i,j,t} + \psi_{i,j,t}^2 + t_j + \epsilon_{i,j,t} \tag{2.7}
$$

where $w_{i,j,t}$ represents the average monthly earnings for worker $i$ at firm $j$ in year $t$, $P_t$ is the price of crude oil in year $t$, $\mu_{i,t}$ captures the age of worker $i$ in year $t$, $\psi_{i,j,t}$ reflects the tenure of worker $i$ with firm $j$ in year $t$, $\alpha_i$ accounts for worker fixed effects, and $t_j$ reflects firm-specific linear time trends. From the perspective of a given petroleum extraction company, changes in oil prices represent exogenous shocks to revenues. Therefore, we use the price of crude oil as an instrument for the economic rents that upstream petroleum firms generate each year and designate the coefficient of interest $\beta$ as a measure of the relative bargaining power of the different types of workers within these companies.

In addition, we assess the robustness of these empirical results using the following two approaches. First, we consider a first differences transformation of Equation 2.7:

$$
\Delta \ln (w_{i,j,t}) = \phi_j + \beta \cdot \Delta \ln (P_t) + \Delta \mu_{i,t}^2 + \Delta \psi_{i,j,t}^2 + \epsilon_{i,j,t} \tag{2.8}
$$
where $\Delta$ symbolizes the first difference operator and $\phi_j$ reflects the fixed effects that remain from the firm-specific linear time trends $t_j$. Since the first-differenced model represents an alternative method of accounting for unobserved heterogeneity that does not vary over time, it can offer additional support for the reliance upon individual worker fixed effects in our preferred specification (Wooldridge 2012, Chapter 14). Second, we examine the extent to which the elasticity of rent sharing may be asymmetric with respect to increases and decreases in the price of crude oil:

$$
\ln (w_{i,j,t}) = \alpha_i + \beta_1 \cdot 1\{\Delta \ln (P_t) > 0\} \cdot \ln (P_t) + \\
\beta_2 \cdot [1 - 1\{\Delta \ln (P_t) > 0\}] \cdot \ln (P_t) + \\
\mu_{i,t} + \mu_{i,t}^2 + \psi_{i,j,t} + \psi_{i,j,t}^2 + t_j + \varepsilon_{i,j,t}
$$

(2.9)

where $1\{\Delta \ln (P_t) > 0\}$ represents the indicator function for a positive change in oil prices in year $t$. In other words, the coefficient $\beta_1$ captures the elasticity of wages regarding positive shocks to firm rents while $\beta_2$ signifies the relationship between worker earnings and negative changes in the price of crude oil. And if the bargaining model in Section 2.4 were indeed accurate, we would expect workers to not only exploit increases in firm productivity but also insure themselves against downside risks to their compensation.

### 2.6 Empirical Results

#### 2.6.1 Firm Rents

As anticipated, the Compustat data reveal that the price of crude oil is a significant source of economic rents for the upstream segment of the U.S. petroleum industry. For instance, our approximation of Equation 2.6 indicates that a 1 percent rise in the composite refiner acquisition cost of crude oil corresponds to an increase of nearly 2.4 percent in value added per worker at petroleum extraction firms in the United States (Column 1 of Table 2.2.A). We obtain similar results with a comparable measure of firm rents such as gross surplus per worker (Column 1 of Table 2.2.B).
Nevertheless, we fail to identify a significant impact on the productivity of oil refineries and air transportation firms even though both industries generally regard petroleum as a primary input to production.\(^{13}\) Perhaps reflecting an ability to effectively pass higher costs through to customers, neither the value added (Column 2 of Table 2.2.A) nor the gross surplus (Column 2 of Table 2.2.B) of petroleum refineries and air transportation companies appears to be correlated with the price of crude oil. Therefore, the absence of additional rents within either industry suggests that these workers should not be able to derive higher earnings from changes in the price of crude oil.

### 2.6.2 All Workers

On average, petroleum extraction companies appear to capture most of the rents that are generated by changes in oil prices. Using the ADP payroll data, we calculate that a 1 percent rise in the price of crude oil leads to an increase of 0.05 percent in the earnings of workers from the upstream segment of the U.S. petroleum industry (Column 1 of Table 2.3.A). This coefficient is comparable to the range of rent sharing estimates that is cited in Card et al. (2018), which concludes that the elasticity of individual wages with respect to firm productivity is between 0.05 and 0.15. In other words, our results are reassuringly consistent with the recent literature.

Furthermore, the ADP data allow us to verify that workers at petroleum refineries and air transportation firms do not receive higher wages in response to changes in oil prices. Our estimation of Equation 2.7 suggests that the earnings of both downstream petroleum workers (Column 2 of Table 2.3.A) as well as air transportation employees (Column 3 of Table 2.3.A) are insensitive to the composite refiner acquisition cost of crude oil. This outcome is especially noteworthy since any such response in wages would be fundamentally incompatible with the weak correlation between petroleum prices and firm productivity that we inferred from the Compustat data for these particular U.S. industries (Column 2 of Tables 2.2.A and

\(^{13}\)Due to the limited coverage of petroleum refineries in the Compustat database, we combine these companies with air transportation firms for the purposes of this analysis.
2.2.B).

Critically, these results seem to be quite robust to alternative specifications of the relationship between the composite refiner acquisition cost of crude oil and wages. For instance, a first differences transformation of our preferred log-log model yields nearly identical estimates of the elasticity of rent sharing with respect to oil prices. To be precise, this analysis suggests that a 1 percent rise in the composite refiner acquisition cost of crude oil generates an increase of 0.06 percent in the earnings of upstream petroleum workers (Column 1 of Table 2.3.B). And once again, we fail to detect any impact from changes in oil prices on the earnings of employees at either petroleum refineries (Column 2 of Table 2.3.B) or air transportation companies (Column 3 of Table 2.3.B). Likewise, we find that the wages of upstream petroleum workers in the United States respond asymmetrically to increases and decreases in the cost of crude oil. As further support for our interpretation of the process by which wages are determined, the magnitude of the impact on earnings is nearly four times larger for positive changes in oil prices than for negative shocks to firm productivity (Column 1 of Table 2.3.C). Put differently, the asymmetry of this effect indicates that workers’ wages do not merely retrace their gains in tandem with the volatility in petroleum prices.

2.6.3 Top Earner

Nevertheless, the elasticity of rent sharing that we estimated across all petroleum extraction workers in Section 2.6.2 obscures significant heterogeneity across the earnings distribution of each upstream oil company. For instance, we find that the wages of the highest paid employee at a petroleum extraction firm are considerably more sensitive to changes in oil prices than the earnings of all other workers at that company. In particular, it appears that a 1 percent increase in the refiner acquisition cost of crude oil boosts the wages of incumbent top earners in the upstream segment of the U.S. petroleum industry by 0.30 percent (Column 1 of Table 2.4.A). In other words, the wages of the highest paid worker are more than six times more responsive to oil prices than the average earnings at these
companies. Consistent with our previous analysis, the coefficient for the price of crude oil does not appear to be statistically significant for top earners at either petroleum refineries (Column 2 of Table 2.4.A) or air transportation firms (Column 3 of Table 2.4.A).

As additional validation of these results, we estimate Equation 2.7 for the earnings of chief executive officers at publicly traded petroleum extraction corporations in the United States using the Execucomp supplement to the Compustat database. As shown in Column 1 of Table 2.4.B, the Execucomp data yield remarkably similar results to the ADP payroll records. We find that a 1 percent rise in oil prices translates to a 0.28 percent increase in the compensation of incumbent chief executive officers within the upstream segment of the petroleum industry. The magnitude of this coefficient is remarkably similar to the range of estimates that are presented in both Bertrand and Mullainathan (2001) as well as Davis and Hausman (2018) despite minor differences across all three studies regarding data samples, time periods, and regression specifications. As before, we are unable to identify a significant relationship between petroleum prices and the wages of chief executive officers at oil refineries and air transportation companies (Column 2 of Table 2.4.B).

2.6.4 Quintiles of Workers

We further document the heterogeneity in rent sharing within firms by quantifying the relationship between oil prices and wages across different subsets of incumbent workers. In a departure from the existing literature, our paper emphasizes the degree to which the bargaining power that employees possess can vary throughout a firm. Specifically, we leverage the broad scope and rich detail of the ADP payroll data in order to estimate a series of regressions over each quintile of the earnings distribution for a given employer.

Corresponding to our results from Section 2.6.3 for the top earners within the upstream segment of the U.S. petroleum industry, we find that incumbent workers in the highest quintile of earnings are able to exert substantially more bargaining power than their lower paid counterparts. Our analysis suggests that a 1 percent increase in the refiner acquisition
cost of crude oil raises the earnings of workers in the top quintile of the wage distribution at a petroleum extraction firm by 0.12 percent (Column 1 of Table 2.5.A). Again, we are unable to detect a statistically significant relationship between wages and petroleum prices among the top quintile of workers at either oil refineries (Column 2 of Table 2.5.A) or air transportation companies (Column 3 of Table 2.5.A).

In contrast, incumbent workers from the second (Column 1 of Table 2.5.B), middle (Column 1 of Table 2.5.C), and fourth (Column 1 of Table 2.5.D) quintiles seem to possess roughly similar abilities to bargain over rents at upstream petroleum firms in the United States. We calculate that a 1 percent rise in the price of crude oil increases wages for the middle three-fifths of the earnings distribution by 0.03 percent to 0.05 percent, which spans the average elasticity of rent sharing across all petroleum extraction workers that was presented in Section 2.6.2. As previously established, the earnings of employees in these three quintiles of the wage distribution at both petroleum refineries (Column 2 of Tables 2.5.B, 2.5.C, and 2.5.D) and air transportation firms (Column 3 of Tables 2.5.B, 2.5.C, and 2.5.D) appear to be wholly unresponsive to changes in oil prices.

Finally, the bottom quintile of incumbent workers within the upstream segment of the petroleum industry appears to be essentially excluded from this bargaining process. As indicated in Column 1 of Table 2.5.E, the ADP payroll data suggest that the earnings of the lowest paid workers at U.S. petroleum extraction companies are insensitive to fluctuations in the price of crude oil. In this regard, these workers appear to be indistinguishable from employees in the bottom quintile of wages at petroleum refineries (Column 2 of Table 2.5.E) and air transportation firms (Column 3 of Table 2.5.E).

2.6.5 Summary

To review, this study demonstrates that the elasticity of rent sharing is far from uniform within firms. As depicted in Figure 2.2, we find that the bargaining power of incumbent workers progressively diminishes from the top to the bottom of the earnings distribution...
at U.S. petroleum extraction companies. For context, the top earner of a firm within the upstream segment of the petroleum industry receives 0.30 percent of the economic rents that are generated by a 1 percent rise in the price of crude oil. Conversely, workers in the lowest quintile of earnings do not experience any such change in compensation. Moreover, given that the coefficients for the refiner acquisition cost of crude oil are statistically different across each quintile of the wage distribution, we conclude that there are meaningful disparities in the ability to bargain over economic rents within a given firm.

Nevertheless, the lack of a relationship between oil prices and wages among employees of either petroleum refineries (Figure 2.3) or air transportation firms (Figure 2.4) does not appear to reflect any underlying heterogeneity in rent sharing within these companies. Instead, incumbent workers from these industries are unaffected by volatility in the cost of crude oil regardless of their relative positions in the earnings distribution. These results are compatible with our analysis of the Compustat data in Section 2.6.1, which suggests that neither oil refineries nor air transportation firms in the United States generate additional economic rents from changes in petroleum prices.

2.7 Conclusion

Despite the fact that this paper narrowly focuses on a particular set of U.S. employers, these results still serve to enhance our understanding of the rent sharing literature. Critically, in contrast to previous studies that evaluate credibly rational shocks to firm productivity, our analysis demonstrates that workers may even be compensated for effectively arbitrary changes in economic rents over which they do not exert any control. Moreover, this investigation highlights the substantial differences that can exist regarding the allocation of firm rents across various types of workers at the same company.

Nevertheless, a clear explanation for the precise sources of these disparities in rent sharing within firms remains somewhat elusive. In the context of the chief executive officers of publicly traded companies, Bertrand and Mullainathan (2001) argue that poor corporate
governance can account for the responsiveness of worker earnings to obvious forms of luck. Perhaps the repercussions of insufficient oversight extend beyond merely corporate executives to a broader subset of employees at a firm. Alternatively, as noted by Kline (2008), the U.S. petroleum industry has traditionally been characterized by rather low rates of unionization. As a consequence, the absence of collective bargaining could at least partly justify this heterogeneity in worker outcomes. Thus, we believe that a closer examination of the organizational structures as well as internal dynamics of firms would be a productive avenue for future research.
Table 2.1: Summary Statistics

<table>
<thead>
<tr>
<th>Characteristics of Firms:</th>
<th>Petroleum Extraction</th>
<th>Petroleum Refineries</th>
<th>Air Transportation</th>
</tr>
</thead>
<tbody>
<tr>
<td>Monthly Employment</td>
<td>210</td>
<td>489</td>
<td>334</td>
</tr>
<tr>
<td>Hourly (%)</td>
<td>39.6</td>
<td>64.0</td>
<td>59.6</td>
</tr>
<tr>
<td>Salaried (%)</td>
<td>60.2</td>
<td>35.1</td>
<td>38.6</td>
</tr>
<tr>
<td>Other (%)</td>
<td>0.2</td>
<td>0.9</td>
<td>1.8</td>
</tr>
<tr>
<td>Monthly Payroll (Thous. of $)</td>
<td>2,600.8</td>
<td>3,889.0</td>
<td>2,181.0</td>
</tr>
<tr>
<td>Number of Firms</td>
<td>140</td>
<td>57</td>
<td>188</td>
</tr>
<tr>
<td>Characteristics of Workers:</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Female (%)</td>
<td>23.2</td>
<td>35.7</td>
<td>34.8</td>
</tr>
<tr>
<td>Age (Years)</td>
<td>39.3</td>
<td>38.7</td>
<td>39.6</td>
</tr>
<tr>
<td>Tenure (Years)</td>
<td>3.1</td>
<td>3.7</td>
<td>4.0</td>
</tr>
<tr>
<td>Monthly Earnings ($)</td>
<td>10,073.2</td>
<td>5,601.5</td>
<td>4,761.0</td>
</tr>
<tr>
<td>Number of Workers</td>
<td>74,968</td>
<td>72,927</td>
<td>120,528</td>
</tr>
</tbody>
</table>

Note: Data are available from May 2008 to December 2018.
Source: ADP; authors’ calculations.
<table>
<thead>
<tr>
<th>Percentage Change in Value Added Per Worker</th>
<th>Petroleum Refineries and Air Extraction Transportation</th>
</tr>
</thead>
<tbody>
<tr>
<td>Petroleum Refineries</td>
<td>Petroleum Refineries</td>
</tr>
<tr>
<td>Percentage Change in Refiner Acquisition Cost</td>
<td>2.35472 ** (0.94768)</td>
</tr>
<tr>
<td>Number of Firms</td>
<td>601</td>
</tr>
<tr>
<td>R-Squared</td>
<td>0.16472</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>6,630</td>
</tr>
</tbody>
</table>

Note: Value added per worker is defined as the difference between total revenues and cost of goods sold divided by firm employment. Standard errors are clustered at the firm level and presented in parentheses. All regressions include firm fixed effects. Data are available from 1975 to 2018. Source: Standard and Poor’s; U.S. Energy Information Administration; authors’ calculations.
Table 2.2.B: Effect of Oil Prices on Gross Surplus Per Worker

<table>
<thead>
<tr>
<th></th>
<th>Petroleum Extraction (1)</th>
<th>Petroleum Refineries and Air Transportation (2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Percentage Change in Refiner Acquisition Cost</td>
<td>3.65867 ** (1.77813)</td>
<td>0.03600 (0.33979)</td>
</tr>
<tr>
<td>Number of Firms</td>
<td>614</td>
<td>157</td>
</tr>
<tr>
<td>R-Squared</td>
<td>0.14894</td>
<td>0.14115</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>6,734</td>
<td>2,131</td>
</tr>
</tbody>
</table>

Note: Gross surplus per worker is defined as the sum of earnings before interest, taxes, depreciation, and amortization plus total payroll divided by firm employment. Labor costs are estimated using average compensation per worker for the corresponding industry of each firm. Standard errors are clustered at the firm level and presented in parentheses. All regressions include firm fixed effects. Data are available from 1976 to 2017.

Source: Standard and Poor’s; U.S. Bureau of Labor Statistics; U.S. Energy Information Administration; authors’ calculations.
Table 2.3.A: Effect of Oil Prices on Wages of All Workers

<table>
<thead>
<tr>
<th></th>
<th>Petroleum Extraction (1)</th>
<th>Petroleum Refineries (2)</th>
<th>Air Transportation (3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Log Refiner Acquisition Cost</td>
<td>0.04516 ***</td>
<td>0.00309</td>
<td>0.00206</td>
</tr>
<tr>
<td></td>
<td>(0.01622)</td>
<td>(0.01982)</td>
<td>(0.03498)</td>
</tr>
<tr>
<td>Age</td>
<td>0.14860 **</td>
<td>0.09952 ***</td>
<td>0.10868 ***</td>
</tr>
<tr>
<td></td>
<td>(0.05911)</td>
<td>(0.02023)</td>
<td>(0.03796)</td>
</tr>
<tr>
<td>Age Squared</td>
<td>-0.00040 ***</td>
<td>-0.00064 ***</td>
<td>-0.00044 ***</td>
</tr>
<tr>
<td></td>
<td>(0.00008)</td>
<td>(0.00012)</td>
<td>(0.00015)</td>
</tr>
<tr>
<td>Tenure</td>
<td>-0.22085</td>
<td>-0.06600</td>
<td>-0.00454</td>
</tr>
<tr>
<td></td>
<td>(0.18098)</td>
<td>(0.08805)</td>
<td>(0.04591)</td>
</tr>
<tr>
<td>Tenure Squared</td>
<td>-0.00047 **</td>
<td>-0.00064 ***</td>
<td>-0.00086 **</td>
</tr>
<tr>
<td></td>
<td>(0.00019)</td>
<td>(0.00020)</td>
<td>(0.00033)</td>
</tr>
<tr>
<td>Number of Firms</td>
<td>122</td>
<td>45</td>
<td>167</td>
</tr>
<tr>
<td>R-Squared</td>
<td>0.99897</td>
<td>0.99908</td>
<td>0.99885</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>212,295</td>
<td>157,648</td>
<td>360,345</td>
</tr>
</tbody>
</table>

Levels of significance: *** = 0.01, ** = 0.05, * = 0.10
Note: Standard errors are clustered at the firm level and presented in parentheses. All regressions include worker fixed effects as well as firm-specific linear time trends. Data are available from 2008 to 2018.
Source: ADP; U.S. Energy Information Administration; authors’ calculations.
Table 2.3.B: Effect of Oil Prices on Wages of All Workers

<table>
<thead>
<tr>
<th></th>
<th>Petroleum Extraction (1)</th>
<th>Petroleum Refineries (2)</th>
<th>Air Transportation (3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>First Difference of Log Refiner Acquisition Cost</td>
<td>0.06481 *** (0.01827)</td>
<td>-0.01463 (0.02713)</td>
<td>0.02437 (0.02832)</td>
</tr>
<tr>
<td>First Difference of Age Squared</td>
<td>-0.00020 ** (0.00008)</td>
<td>-0.00045 *** (0.00015)</td>
<td>-0.00043 ** (0.00017)</td>
</tr>
<tr>
<td>First Difference of Tenure Squared</td>
<td>-0.00042 (0.00036)</td>
<td>-0.00116 *** (0.00025)</td>
<td>-0.00133 ** (0.00063)</td>
</tr>
</tbody>
</table>

Number of Firms: 121 (1), 44 (2), 166 (3)
R-Squared: 0.02195 (1), 0.02209 (2), 0.02941 (3)
Number of Observations: 161,157 (1), 116,759 (2), 278,948 (3)

Levels of significance: *** = 0.01, ** = 0.05, * = 0.10
Note: Standard errors are clustered at the firm level and presented in parentheses. All regressions include firm fixed effects. Data are available from 2009 to 2018.
Source: ADP; U.S. Energy Information Administration; authors’ calculations.
Table 2.3.C: Asymmetric Effects of Oil Prices on Wages of All Workers

<table>
<thead>
<tr>
<th></th>
<th>Petroleum Extraction (1)</th>
<th>Petroleum Refineries (2)</th>
<th>Air Transportation (3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Log Refiner Acquisition Cost × Increase in Crude Oil Price</td>
<td>0.15516 **</td>
<td>0.05467 *</td>
<td>0.00284</td>
</tr>
<tr>
<td></td>
<td>(0.06021)</td>
<td>(0.03195)</td>
<td>(0.04119)</td>
</tr>
<tr>
<td>Log Refiner Acquisition Cost × Decrease in Crude Oil Price</td>
<td>0.04369 ***</td>
<td>0.00241</td>
<td>-0.01080</td>
</tr>
<tr>
<td></td>
<td>(0.01618)</td>
<td>(0.01869)</td>
<td>(0.03611)</td>
</tr>
<tr>
<td>Age</td>
<td>0.14160 **</td>
<td>0.09892 ***</td>
<td>0.05846 *</td>
</tr>
<tr>
<td></td>
<td>(0.05886)</td>
<td>(0.02043)</td>
<td>(0.03486)</td>
</tr>
<tr>
<td>Age Squared</td>
<td>-0.00040 ***</td>
<td>-0.00065 ***</td>
<td>-0.00044 ***</td>
</tr>
<tr>
<td></td>
<td>(0.00008)</td>
<td>(0.00012)</td>
<td>(0.00015)</td>
</tr>
<tr>
<td>Tenure</td>
<td>-0.21141</td>
<td>-0.07072</td>
<td>0.04074</td>
</tr>
<tr>
<td></td>
<td>(0.18137)</td>
<td>(0.08812)</td>
<td>(0.06173)</td>
</tr>
<tr>
<td>Tenure Squared</td>
<td>-0.00047 **</td>
<td>-0.00064 ***</td>
<td>-0.00086 **</td>
</tr>
<tr>
<td></td>
<td>(0.00019)</td>
<td>(0.00019)</td>
<td>(0.00033)</td>
</tr>
<tr>
<td>p-Value for Test of Equality</td>
<td>0.06395</td>
<td>0.11037</td>
<td>0.46776</td>
</tr>
<tr>
<td>Number of Firms</td>
<td>122</td>
<td>45</td>
<td>167</td>
</tr>
<tr>
<td>R-Squared</td>
<td>0.99897</td>
<td>0.99908</td>
<td>0.99885</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>212,295</td>
<td>157,648</td>
<td>360,345</td>
</tr>
</tbody>
</table>

Levels of significance: *** = 0.01, ** = 0.05, * = 0.10
Note: Standard errors are clustered at the firm level and presented in parentheses. All regressions include worker fixed effects as well as firm-specific linear time trends. Data are available from 2008 to 2018. Source: ADP; U.S. Energy Information Administration; authors’ calculations.
Table 2.4.A: Effect of Oil Prices on Wages of Top Earner

<table>
<thead>
<tr>
<th>Log Refiner Acquisition Cost</th>
<th>Petroleum Extraction (1)</th>
<th>Petroleum Refineries (2)</th>
<th>Air Transportation (3)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>0.30026 ***</td>
<td>0.07229</td>
<td>-0.10335</td>
</tr>
<tr>
<td></td>
<td>(0.10966)</td>
<td>(0.17785)</td>
<td>(0.09739)</td>
</tr>
<tr>
<td>Age</td>
<td>0.03514</td>
<td>-0.05406</td>
<td>-0.00181</td>
</tr>
<tr>
<td></td>
<td>(0.04250)</td>
<td>(0.06258)</td>
<td>(0.02642)</td>
</tr>
<tr>
<td>Age Squared</td>
<td>-0.00017</td>
<td>0.00053</td>
<td>0.00012</td>
</tr>
<tr>
<td></td>
<td>(0.00039)</td>
<td>(0.00058)</td>
<td>(0.00028)</td>
</tr>
<tr>
<td>Tenure</td>
<td>0.04436 **</td>
<td>0.02931</td>
<td>0.01410</td>
</tr>
<tr>
<td></td>
<td>(0.01843)</td>
<td>(0.02446)</td>
<td>(0.01136)</td>
</tr>
<tr>
<td>Tenure Squared</td>
<td>-0.00132 ***</td>
<td>-0.00069</td>
<td>-0.00020</td>
</tr>
<tr>
<td></td>
<td>(0.00047)</td>
<td>(0.00052)</td>
<td>(0.00033)</td>
</tr>
</tbody>
</table>

| Number of Firms              | 110                      | 43                       | 162                    |
| R-Squared                    | 0.99581                  | 0.99689                  | 0.99581                |
| Number of Observations       | 946                      | 330                      | 1,136                  |

Levels of significance: *** = 0.01, ** = 0.05, * = 0.10
Note: Standard errors are clustered at the firm level and presented in parentheses. All regressions include firm-specific linear time trends. Data are available from 2008 to 2018.
Source: ADP; U.S. Energy Information Administration; authors’ calculations.
Table 2.4.B: Effect of Oil Prices on Wages of Chief Executive Officer

<table>
<thead>
<tr>
<th></th>
<th>Log Annual Earnings</th>
<th>Petroleum Refineries and Air Transportation</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Petroleum Extraction (1)</td>
<td>Petroleum Refineries (2)</td>
</tr>
<tr>
<td>Log Refiner Acquisition Cost</td>
<td>0.28260 ***</td>
<td>0.10935</td>
</tr>
<tr>
<td></td>
<td>(0.05414)</td>
<td>(0.08804)</td>
</tr>
<tr>
<td>Age</td>
<td>0.45863 ***</td>
<td>0.50215 ***</td>
</tr>
<tr>
<td></td>
<td>(0.11005)</td>
<td>(0.13094)</td>
</tr>
<tr>
<td>Age Squared</td>
<td>-0.00334 ***</td>
<td>-0.00265 ***</td>
</tr>
<tr>
<td></td>
<td>(0.00079)</td>
<td>(0.00079)</td>
</tr>
<tr>
<td>Number of Firms</td>
<td>93</td>
<td>45</td>
</tr>
<tr>
<td>R-Squared</td>
<td>0.99924</td>
<td>0.99917</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>1,419</td>
<td>722</td>
</tr>
</tbody>
</table>

Levels of significance: *** = 0.01, ** = 0.05, * = 0.10

Note: Standard errors are clustered at the firm level and presented in parentheses. All regressions include worker fixed effects as well as firm-specific linear time trends. Data are available from 1992 to 2018.

Source: Standard and Poor’s; U.S. Energy Information Administration; authors’ calculations.
Table 2.5.A: Effect of Oil Prices on Wages of Top Quintile of Workers

<table>
<thead>
<tr>
<th>Log Refiner Acquisition Cost</th>
<th>Petroleum Extraction (1)</th>
<th>Petroleum Refineries (2)</th>
<th>Air Transportation (3)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>0.11766 ***</td>
<td>0.04122</td>
<td>-0.00316</td>
</tr>
<tr>
<td></td>
<td>(0.03802)</td>
<td>(0.03145)</td>
<td>(0.04552)</td>
</tr>
<tr>
<td>Age</td>
<td>-0.02316</td>
<td>0.01891</td>
<td>0.00251</td>
</tr>
<tr>
<td></td>
<td>(0.03285)</td>
<td>(0.05003)</td>
<td>(0.00217)</td>
</tr>
<tr>
<td>Age Squared</td>
<td>-0.00022 **</td>
<td>-0.00017 **</td>
<td>-0.00040 ***</td>
</tr>
<tr>
<td></td>
<td>(0.00011)</td>
<td>(0.00008)</td>
<td>(0.00015)</td>
</tr>
<tr>
<td>Tenure</td>
<td>0.02993</td>
<td>-0.00115</td>
<td>0.05411 ***</td>
</tr>
<tr>
<td></td>
<td>(0.06759)</td>
<td>(0.00425)</td>
<td>(0.02027)</td>
</tr>
<tr>
<td>Tenure Squared</td>
<td>-0.00034</td>
<td>-0.00022 **</td>
<td>-0.00037</td>
</tr>
<tr>
<td></td>
<td>(0.00024)</td>
<td>(0.00009)</td>
<td>(0.00027)</td>
</tr>
</tbody>
</table>

Number of Firms 109 43 161
R-Squared 0.99886 0.99947 0.99934
Number of Observations 46,842 37,538 81,717

Levels of significance: *** = 0.01, ** = 0.05, * = 0.10
Note: Standard errors are clustered at the firm level and presented in parentheses. All regressions include worker fixed effects as well as firm-specific linear time trends. Data are available from 2008 to 2018. Source: ADP; U.S. Energy Information Administration; authors’ calculations.
Table 2.5.B: Effect of Oil Prices on Wages of Second Quintile of Workers

<table>
<thead>
<tr>
<th></th>
<th>Petroleum Extraction (1)</th>
<th>Petroleum Refineries (2)</th>
<th>Air Transportation (3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Log Refiner Acquisition Cost</td>
<td>0.05273 ** (0.02056)</td>
<td>0.01746 (0.01316)</td>
<td>-0.00335 (0.03245)</td>
</tr>
<tr>
<td>Age</td>
<td>-0.01291 (0.01398)</td>
<td>0.16175 ** (0.06238)</td>
<td>0.03663 *** (0.01405)</td>
</tr>
<tr>
<td>Age Squared</td>
<td>-0.00012 (0.00011)</td>
<td>-0.00028 *** (0.00008)</td>
<td>-0.00026 * (0.00013)</td>
</tr>
<tr>
<td>Tenure</td>
<td>0.02740 (0.03451)</td>
<td>-0.18790 *** (0.05976)</td>
<td>0.03355 * (0.01962)</td>
</tr>
<tr>
<td>Tenure Squared</td>
<td>-0.00015 (0.00017)</td>
<td>-0.00010 (0.00011)</td>
<td>-0.00046 ** (0.00020)</td>
</tr>
<tr>
<td>Number of Firms</td>
<td>111</td>
<td>43</td>
<td>161</td>
</tr>
<tr>
<td>R-Squared</td>
<td>0.99941</td>
<td>0.99974</td>
<td>0.99948</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>52,965</td>
<td>41,736</td>
<td>89,161</td>
</tr>
</tbody>
</table>

Levels of significance: *** = 0.01, ** = 0.05, * = 0.10

Note: Standard errors are clustered at the firm level and presented in parentheses. All regressions include worker fixed effects as well as firm-specific linear time trends. Data are available from 2008 to 2018.

Source: ADP; U.S. Energy Information Administration; authors’ calculations.
### Table 2.5.C: Effect of Oil Prices on Wages of Middle Quintile of Workers

<table>
<thead>
<tr>
<th></th>
<th>Log Refiner Acquisition Cost</th>
<th>Age</th>
<th>Age Squared</th>
<th>Tenure</th>
<th>Tenure Squared</th>
<th>Number of Firms</th>
<th>R-Squared</th>
<th>Number of Observations</th>
</tr>
</thead>
<tbody>
<tr>
<td>Petroleum Extraction Refineries (1)</td>
<td>0.04700 **</td>
<td>-0.01435</td>
<td>-0.0005</td>
<td>-0.00100</td>
<td>-0.00004</td>
<td>110</td>
<td>0.99953</td>
<td>53,612</td>
</tr>
<tr>
<td>Petroleum Refineries (2)</td>
<td>0.00395</td>
<td>0.05187 ***</td>
<td>-0.00018 **</td>
<td>-0.01021 **</td>
<td>-0.00004</td>
<td>43</td>
<td>0.99969</td>
<td>41,655</td>
</tr>
<tr>
<td>Air Transportation (3)</td>
<td>0.00838</td>
<td>0.68430 ***</td>
<td>-0.00021 ***</td>
<td>-0.69494 ***</td>
<td>-0.00042 **</td>
<td>161</td>
<td>0.99949</td>
<td>90,916</td>
</tr>
</tbody>
</table>

Levels of significance: ** = 0.01, *** = 0.05, * = 0.10

Note: Standard errors are clustered at the firm level and presented in parentheses. All regressions include worker fixed effects as well as firm-specific linear time trends. Data are available from 2008 to 2018. Source: ADP; U.S. Energy Information Administration; authors’ calculations.
Table 2.5.D: Effect of Oil Prices on Wages of Fourth Quintile of Workers

<table>
<thead>
<tr>
<th></th>
<th>Petroleum Extraction (1)</th>
<th>Petroleum Refineries (2)</th>
<th>Air Transportation (3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Log Refiner Acquisition Cost</td>
<td>0.02732 *</td>
<td>0.01063</td>
<td>0.00524</td>
</tr>
<tr>
<td></td>
<td>(0.01565)</td>
<td>(0.01227)</td>
<td>(0.02612)</td>
</tr>
<tr>
<td>Age</td>
<td>0.12294 ***</td>
<td>0.11754 *</td>
<td>0.10371 ***</td>
</tr>
<tr>
<td></td>
<td>(0.03456)</td>
<td>(0.05971)</td>
<td>(0.03085)</td>
</tr>
<tr>
<td>Age Squared</td>
<td>-0.00011 **</td>
<td>-0.00029 ***</td>
<td>-0.00017 **</td>
</tr>
<tr>
<td></td>
<td>(0.00005)</td>
<td>(0.00011)</td>
<td>(0.00007)</td>
</tr>
<tr>
<td>Tenure</td>
<td>-0.14234</td>
<td>-0.05751</td>
<td>-0.06225 ***</td>
</tr>
<tr>
<td></td>
<td>(0.13311)</td>
<td>(0.06247)</td>
<td>(0.02298)</td>
</tr>
<tr>
<td>Tenure Squared</td>
<td>-0.00025</td>
<td>-0.00029 **</td>
<td>-0.00058 ***</td>
</tr>
<tr>
<td></td>
<td>(0.00021)</td>
<td>(0.00014)</td>
<td>(0.00020)</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
<th>Petroleum Extraction (1)</th>
<th>Petroleum Refineries (2)</th>
<th>Air Transportation (3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Number of Firms</td>
<td>110</td>
<td>43</td>
<td>160</td>
</tr>
<tr>
<td>R-Squared</td>
<td>0.99950</td>
<td>0.99949</td>
<td>0.99929</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>51,307</td>
<td>36,915</td>
<td>87,790</td>
</tr>
</tbody>
</table>

Levels of significance: *** = 0.01, ** = 0.05, * = 0.10
Note: Standard errors are clustered at the firm level and presented in parentheses. All regressions include worker fixed effects as well as firm-specific linear time trends. Data are available from 2008 to 2018. Source: ADP; U.S. Energy Information Administration; authors’ calculations.
Table 2.5.E: Effect of Oil Prices on Wages of Bottom Quintile of Workers

<table>
<thead>
<tr>
<th></th>
<th>Petroleum Extraction (1)</th>
<th>Petroleum Refineries (2)</th>
<th>Air Transportation (3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Log Refiner Acquisition Cost</td>
<td>-0.00562</td>
<td>-0.09002</td>
<td>0.01680</td>
</tr>
<tr>
<td></td>
<td>(0.02062)</td>
<td>(0.08929)</td>
<td>(0.04433)</td>
</tr>
<tr>
<td>Age</td>
<td>0.17136 ***</td>
<td>0.70535 ***</td>
<td>0.20183 ***</td>
</tr>
<tr>
<td></td>
<td>(0.05385)</td>
<td>(0.24964)</td>
<td>(0.03198)</td>
</tr>
<tr>
<td>Age Squared</td>
<td>-0.00019</td>
<td>-0.00096 ***</td>
<td>-0.00044 ***</td>
</tr>
<tr>
<td></td>
<td>(0.00013)</td>
<td>(0.00013)</td>
<td>(0.00015)</td>
</tr>
<tr>
<td>Tenure</td>
<td>-0.19705</td>
<td>-0.53195 *</td>
<td>-0.03171</td>
</tr>
<tr>
<td></td>
<td>(0.15597)</td>
<td>(0.28394)</td>
<td>(0.14528)</td>
</tr>
<tr>
<td>Tenure Squared</td>
<td>-0.00171 ***</td>
<td>-0.00131 ***</td>
<td>-0.00094</td>
</tr>
<tr>
<td></td>
<td>(0.00035)</td>
<td>(0.00029)</td>
<td>(0.00071)</td>
</tr>
<tr>
<td>Number of Firms</td>
<td>110</td>
<td>43</td>
<td>160</td>
</tr>
<tr>
<td>R-Squared</td>
<td>0.99877</td>
<td>0.99812</td>
<td>0.99790</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>42,169</td>
<td>28,218</td>
<td>70,773</td>
</tr>
</tbody>
</table>

Levels of significance: *** = 0.01, ** = 0.05, * = 0.10
Note: Standard errors are clustered at the firm level and presented in parentheses. All regressions include worker fixed effects as well as firm-specific linear time trends. Data are available from 2008 to 2018. Source: ADP; U.S. Energy Information Administration; authors’ calculations.
Figure 2.1: Price of Crude Oil

Note: Shading denotes recession.
Figure 2.2: Effects of Oil Prices on Wages at Petroleum Extraction Firms

Coefficient for Log Composite Refiner Acquisition Cost

Note: Bands reflect 95 percent confidence intervals that have been estimated from standard errors which are clustered at the firm level. All regressions include a quadratic in age, a quadratic in tenure, and firm-specific linear time trends. With the exception of top earner, each regression also accounts for worker fixed effects. Data are available from 2008 to 2018.

Source: ADP; U.S. Energy Information Administration; authors’ calculations.
Figure 2.3: Effects of Oil Prices on Wages at Petroleum Refineries

Coefficient for Log Composite Refiner Acquisition Cost

Note: Bands reflect 95 percent confidence intervals that have been estimated from standard errors which are clustered at the firm level. All regressions include a quadratic in age, a quadratic in tenure, and firm-specific linear time trends. With the exception of top earner, each regression also accounts for worker fixed effects. Data are available from 2008 to 2018.

Source: ADP; U.S. Energy Information Administration; authors’ calculations.
Figure 2.4: Effects of Oil Prices on Wages at Air Transportation Firms

Coefficient for Log Composite Refiner Acquisition Cost

<table>
<thead>
<tr>
<th></th>
<th>All Workers</th>
</tr>
</thead>
<tbody>
<tr>
<td>Top Earner</td>
<td></td>
</tr>
<tr>
<td>Top Quintile</td>
<td></td>
</tr>
<tr>
<td>Second Quintile</td>
<td></td>
</tr>
<tr>
<td>Middle Quintile</td>
<td></td>
</tr>
<tr>
<td>Fourth Quintile</td>
<td></td>
</tr>
<tr>
<td>Bottom Quintile</td>
<td></td>
</tr>
</tbody>
</table>

Note: Bands reflect 95 percent confidence intervals that have been estimated from standard errors which are clustered at the firm level. All regressions include a quadratic in age, a quadratic in tenure, and firm-specific linear time trends. With the exception of top earner, each regression also accounts for worker fixed effects. Data are available from 2008 to 2018.

Source: ADP; U.S. Energy Information Administration; authors’ calculations.
3 Are the Long-Term Unemployed on the Margins of the Labor Market?\textsuperscript{1}

3.1 Introduction

A number of observers have noted that in recent years conventional Phillips curve and Beveridge curve models have predicted greater price deflation, greater real wage declines, and fewer vacancies than actually occurred as a result of the high rate of unemployment experienced during the Great Recession and its aftermath. Several economists have provided possible explanations for the missed predictions of the Phillips curve, based on anchoring of inflation expectations (Bernanke 2007; 2010) or changes in the distribution of price increases and interactions in the Phillips curve (Ball and Mazumder 2011). Others have shown that price and wage Phillips curves are stable if the short-term unemployment rate is used instead of the total unemployment rate (Stock 2011; Gordon 2013; Watson 2014; Council of Economic Advisers 2014b), while others have shown that the Beveridge curve is stable if the short-term unemployment rate is used instead of the total unemployment rate (Ghayad and Dickens 2012).

This paper explores the extent to which the long-term unemployed, whose share of overall unemployment rose to an unprecedented level after the Great Recession, are on the margins of the labor force and are, therefore, possibly exerting little pressure on wage growth or inflation and slowing the process of matching unemployed workers to job vacancies. The hypothesis we seek to test is that the longer workers are unemployed the less they become tied to the job market, either because, on the supply side, they grow discouraged and search for a job less intensively (Krueger and Mueller 2011), or because, on the demand side, employers discriminate against them, based on the expectation (whether rational or irrational)

\textsuperscript{1}This chapter is co-authored with Alan B. Krueger and Judd N. L. Cramer and was published in the Spring 2014 issue of the Brookings Papers on Economic Activity. We thank Katharine Abraham, Peter Diamond, Henry Farber, Bo Honoré, Lawrence Katz, Scott Kostyshak, Christopher Nekarda, Richard Rogerson, David Romer, Robert Shimer, Carl Van Horn, Thomas Winberry, and Justin Wolfers for helpful comments, and Reid Stevens and Rebeccas Sachs for providing excellent research assistance. The authors are solely responsible for any errors as well as the views expressed herein.
that there is a productivity-related reason that accounts for their long jobless spell (Kroft et al. 2014; Ghayad 2013). Either of these explanations would imply that the long-term unemployed are on the margins of the labor market. Moreover, the demand-side and supply-side effects of long-term unemployment can be viewed as complementary explanations that reinforce one another rather than competing explanations, because statistical discrimination against the long-term unemployed could lead to discouragement, and the skill erosion that accompanies long-term unemployment could induce employers to discriminate against the long-term unemployed.

We assemble varied evidence to assess the hypothesis that the long-term unemployed are on the margins of the labor market. To preview our main findings, we tentatively conclude that the long-term unemployed are less connected to the economy than the short-term unemployed, and that many eventually withdraw from the labor force. The chance of holding a full-time, steady job for those who became unemployed in the 2008-2012 period was 19 percent at the start of their unemployment spell, 11 percent after 7 months without working, and only 6 percent after 2 years without working. Nearly half of those who were jobless for seven months or longer had withdrawn from the labor force within 15 months of becoming unemployed in 2012. Even at times when — or in regions where — the economy is relatively strong, the long-term unemployed face long odds of returning to steady, full-time employment. We also find that the long-term unemployed are about 60 percent as effective at matching to job openings as are the short-term unemployed. Furthermore, job finding rates are more sensitive to the state of the business cycle for the short-term unemployed than the long-term unemployed, suggesting that the long-term unemployed are more insulated from macroeconomic developments.

This paper is organized as follows. Section 3.2 provides a detailed profile of the long-term unemployed, and examines how their composition has varied over time. While some notable industries (such as construction) and demographic groups (such as African Ameri-

\[\text{Steady, full-time employment in this context means that someone who was unemployed in month } t \text{ was employed full-time for } 4 \text{ consecutive months starting in month } t + 12.\]
cans) are overrepresented among the long-term unemployed, the long-term unemployed are ubiquitous and spread throughout all corners of the economy. We find only modest changes in the composition of the unemployed over the business cycle, with those who are unemployed during recessions tending to have more highly rewarded characteristics than those who are unemployed during expansions.

Sections 3.3 and 3.4 examine the rates at which unemployed workers find employment or exit the labor force, by duration of unemployment. Using matched Current Population Survey (CPS) data we examine transition rates both over a month and over a year or more. Longer durations of unemployment are associated with lower rates of transition into employment, although there is debate as to whether the observed duration dependence is a result of heterogeneity among workers or changes in the employability of job seekers that take place during long jobless spells (Heckman and Singer 1984; Chan and Stevens 2001). From 2008 to 2012, 35.9 percent of those who were long-term unemployed (that is, unemployed for 7 months or longer) in a given month in the CPS were employed 15 months later, and 10.8 percent were employed steadily in a full-time position; the comparable figures for the short-term unemployed were 49.5 percent and 14.4 percent, respectively.

The long-term unemployed normally have a higher rate of labor force withdrawal than the short-term unemployed, although we document that following a recession the labor force withdrawal rates for all duration groups tend to collapse to a common, lower level. Nearly a fifth of the cyclical movement in the labor force attachment of the long-term unemployed appears to be related to shifts in the observed characteristics of the unemployed over the business cycle. The cyclical behavior of labor force withdrawal is also consistent with the expiration of extended unemployment insurance benefits leading the long-term unemployed to exit the labor force. The countercyclical pattern of labor force participation of the long-term unemployed suggests that a critical channel for the future path of long-term unemployment in the United States involves the evolution of labor force withdrawal rates by duration of unemployment.
Transition rates in the CPS have well-known measurement problems, because workers who are misclassified in one period and then correctly categorized in the following month will falsely appear as if they changed labor force status across these periods, even though they had not changed labor market status. Another limitation of the CPS data for our analysis is that the survey does not follow individuals who move to new locations. Therefore, we supplement the analysis with two additional data sets in Sections 3.5 and 3.6: the Survey of Income and Program Participation (SIPP) and Rutgers University’s Hildreth Center’s Work Trends Survey, a panel survey of workers who were unemployed between September 2008 and August 2009.\footnote{The CPS captures all ongoing spells of unemployment at a point in time, whereas the SIPP and Work Trends Survey capture new spells of unemployment occurring over a specified period of time, but under reasonable assumptions all three surveys should yield representative estimates of job finding rates by duration of unemployment.} Despite their design differences, it is reassuring that the SIPP and Work Trends Survey yield qualitatively similar results to the CPS. Even across varying intervals, definitions of short- and long-term unemployed, and job definitions, all three yield job finding rates that are about 20 to 40 percent lower for the long-term unemployed than for the short-term unemployed.

To further explore the effect of a stronger economy on the prospects of the unemployed, Section 3.7 compares trends in long-term and short-term unemployment in different regions of the United States. Our analysis indicates that long-term unemployment has remained elevated even in states where the total unemployment rate has fallen below its historical average. In addition, the long-term unemployed appear to be following a similar path in terms of job finding rates and labor force exit rates in both low-unemployment and high-unemployment states. By contrast, the short-term unemployed have exhibited higher job finding rates in low-unemployment states.

In Section 3.8 we explore whether the Beveridge curve can be caused to loop around a stable path following a sharp downturn by a process of labor force withdrawal rates collapsing and then gradually returning to their historical norm — with higher exit rates for the long-term unemployed — as well as a lower match efficiency for the long-term unem-
ployed. Specifically, we extend the calibration-type model of Kroft et al. (2014) to allow for varying labor force exit rates and differential match efficiency for the long-term unemployed to project the path of the Beveridge curve under a stable matching function. The results suggest that from 2002 to 2007 the long-term unemployed were about 60 percent as efficient in job matching as the short-term unemployed. Using the matching function estimated for the 2002-2007 period, the calibrated model does a reasonably good job of capturing the rise in unemployment and the outward shift of the Beveridge curve in the 2008-2013 period as well as the rise in the share of unemployed workers who are long-term unemployed.

Using the calibrated model to conduct a counterfactual simulation, we find that both the lower match efficiency of the long-term unemployed and their countercyclical pattern of labor force withdrawal have played an important role in the rising unemployment rate and the shift of the Beveridge curve since the Great Recession. Future projections predict a gradual return to the original Beveridge curve as the share of long-term unemployment declines due to labor force exits or transitions to employment. The gradual withdrawal from the labor force of many of the long-term unemployed is a potential source of hysteresis effects (Blanchard and Summers 1987; DeLong and Summers 2012).

Section 3.9 concludes the paper by briefly considering some of the policy implications of the hypothesis that many of the long-term unemployed are on the margins of the labor market. Perhaps most importantly, our findings suggest that a concerted effort will be required to raise the employment prospects of the long-term unemployed, especially since they are likely to withdraw from the job market at an increasing rate if they continue to follow the same path as in the previous recovery.

3.2 Profile of the Long-Term and Short-Term Unemployed

For background, this section provides a detailed portrait of the long-term unemployed in comparison to employed workers and short-term unemployed workers. We begin by reviewing

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4To be fair, Kroft et al. (2014) allow for differential match efficiency of the long-term unemployed through a multiplicative term, but they do not allow for a differential effect in the “meeting” function.
trends in the incidence of long-term unemployment, then summarize characteristics of the long-term unemployed, and then examine how a summary measure of the composition of the long-term unemployed (based on earnings prospects) has varied over time.

Figure 3.1 displays duration-specific unemployment rates in the United States based on published seasonally adjusted monthly data from the Bureau of Labor Statistics (BLS) from January 1948 through May 2014. The dark line indicates the long-term unemployment rate (defined as the number unemployed for 27 weeks or longer divided by the labor force), whereas the dotted line is the similarly defined unemployment rate for those unemployed for 14 weeks or less, and the dashed line is the rate for the intermediate group unemployed for 15 to 26 weeks. Notice that the long-term unemployment rate, which tends to rise during periods of recession and peak shortly afterwards, jumped to record heights during the Great Recession and peaked in early 2010 before starting to decline. Despite declining over the last 4 years, in 2013 it exceeded its previous annual peak before the Great Recession, reached in the aftermath of the deep 1981-1982 recession, and was well above its average in the last recovery. The two measures of short-term unemployment, however, have returned to close to the average rates they displayed during the last recovery. Thus, as an accounting matter, unemployment remains elevated because of the large number of people who have been unemployed for more than half a year.

For most of the six decades prior to the Great Recession, the share of the unemployed in the United States who were out of work for more than half a year oscillated between 10 and 20 percent during recoveries and recessions. The share of the unemployed who were long-term job seekers averaged over 40 percent in 2010, 2011, and 2012 — reaching as high as 45 percent — and as of this writing stood at 35 percent. The long-term unemployed, therefore, exert a more significant effect on unemployment dynamics today than they have in the past.

Table 3.1 reports the distribution of both employees and people on short-term or long-

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5See Abraham and Shimer (2002) for a careful analysis of why the duration of unemployment in the U.S. rose relative to the unemployment rate in the 1980s and 1990s.
term unemployment spells along several dimensions for the United States, using data from the 2013 CPS. For example, the table indicates that 34 percent of employed individuals are ages 16 to 34, 32 percent are ages 35 to 49, and 33 percent are ages 50 and older. Compared to their share of employment, young people are notably overrepresented among the short-term unemployed, while the middle-aged group is underrepresented. Compared to their share of the short-term unemployed, the oldest group is overrepresented among the long-term unemployed, although their share of the long-term unemployed almost matches their share of employment.

If the unemployed as a whole are compared to the employed, notably larger shares of the unemployed are younger, unmarried, and less well educated. For example, although about one-third of employed workers have earned a bachelor’s degree, less than 20 percent of the unemployed have done so. By contrast, nearly 20 percent of the unemployed lack a high school diploma, which is twice the rate for the employed. African Americans and Hispanics are also overrepresented among the unemployed. Not surprisingly, given the housing bubble earlier in the 2000s, a higher proportion of the unemployed previously worked in the construction industry than the share of workers currently employed as construction workers; nonetheless, only 11 percent of all unemployed workers were previously in the construction industry.

When compared to the short-term unemployed, a larger proportion of the long-term unemployed are over age 50 and are African American. Workers age 50 and over make up 30 percent of the long-term unemployed but just 20 percent of the short-term unemployed. African Americans constitute 23 percent of the long-term unemployed, compared with just 11 percent of the employed population and 17 percent of the short-term unemployed.

Along many other dimensions, however, the long-term unemployed appear broadly similar to the short-term unemployed. The educational achievement of the two groups is comparable, and both the industry distribution and occupational distribution are similar. The long-term unemployed are almost as likely to be unmarried as the short-term unem-
ployed. And differences across regions and between urban and rural areas (not shown in Table 3.1) are also small.

About two-thirds of both short-term and long-term unemployed workers last held jobs in two occupational categories, blue-collar jobs and sales and service jobs, whereas those occupations comprised just under half of all employed workers.

The similar industrial, occupational, and educational composition of short-term and long-term unemployed workers suggests that differential mismatch between workers and the types of jobs available does not account for much of the occurrence of long-term unemployment. From the limited information available in the CPS, the long-term unemployed seem to be qualified for about the same mix of jobs as the short-term unemployed. Thus, if there is a structural problem currently confronting the long-term unemployed that pushes them to the margins of the labor market, it presumably has come about because their skills, motivation to find a job, or self-esteem eroded during their long stretch of unemployment, or because employers treat the long-term unemployed differently, rather than because of different backgrounds possessed by the long-term unemployed at the start of their spells of unemployment. We return to this issue below.

### 3.2.1 Composition of the Unemployed over Time

Table 3.1 summarizes the characteristics of the unemployed in 2013, but there has been a large increase in the number of long-term unemployed and potential compositional shifts since the Great Recession. To create a summary measure of the characteristics of the unemployed that can be tracked over time we used the following procedure. We first estimated a wage regression using data from 2004-2006, which was a more or less “normal” period for the labor market, and then we combined the coefficients from this regression with the characteristics of either the short-term unemployed or long-term unemployed each year to track their earnings potential each year from 1995 to 2013. Specifically, the wage regression related the log hourly wage of workers to their education, experience, industry, occupation,
The estimated coefficients from this regression were then combined with the characteristics of those on short-term unemployment spells (26 weeks or less at the time of the survey) or on long-term unemployment spells (more than 26 weeks at the time of the survey) each year to derive a simple summary of the composition of the long-term unemployed with respect to their earnings prospects.\(^6\)

Figure 3.2 contains the results of this exercise. The long-term unemployed are predicted to have higher earnings than the short-term unemployed, in large part because they are older and have higher potential work experience. They are also predicted to earn about 10 percent less than the average employed person in 2013. There appears to be both a mild secular trend and a mild cyclical pattern in the composition of both the short-term and long-term unemployed, at least as far as their characteristics that predict earnings are concerned. The composition of the unemployed has gradually tilted towards those with characteristics associated with higher earnings, such as more education, since the mid-1990s.

The mix of the long-term unemployed with characteristics associated with higher earnings tends to rise during economic downturns. Predicted earnings of both the short-term and long-term unemployed rose by an average of 6 percent around the time of each of the two previous recessions. This pattern is consistent with Mueller’s (2012) finding that in recessions the pool of the unemployed tends to shift toward those with higher earnings in their previous jobs, because such workers are more likely to be displaced in recessions. The cyclical movements for the long-term unemployed slightly lag behind those for the short-term unemployed, probably because some of the short-term unemployed become long-term unemployed over time.

These findings complement Kroft et al.’s (2014, p.8) conclusion that when the per-

\(^6\)We use four categorical variables for highest level of educational attainment: less than high school, high school only, some college/associate degree, and bachelor’s degree or higher. We include linear and quadratic terms in potential experience. Also included in the regression are dummy variables for female, married, widowed/divorced, Hispanic ethnicity, white, black, 10 major industries, and 11 major occupations, as well as an indicator for new entrants (who lacked an industry and occupation).

\(^7\)We use CPS data from 1995 forward because we limit the sample to the period after the 1994 redesign of the CPS, which affected the share of long-term unemployed workers (Polivka and Miller 1994) and improved the ability to track individuals over time.
manently laid-off share is included, “compositional changes in the unemployed account for virtually none of the observed rise in long-term unemployment” during and after the Great Recession. In the section on transition rates below, we perform a similar exercise to examine changes in the composition of the unemployed with respect to their measured characteristics that predict job finding and labor force withdrawal.

3.3 Transition Rates: Current Population Survey Data

This section explores the labor market transitions of the unemployed over time. Specifically, we use longitudinally linked CPS data (see Nekarda 2009) to study how the long-term unemployed fare in later survey months. Because of limitations in the matched CPS data, such as the failure to track individuals who move to new locations, we supplement this analysis with the SIPP and Work Trends Survey in Sections 3.5 and 3.6.

We are most interested in documenting the cyclical pattern of transitions from unemployment to either employment or out-of-the-labor-force by duration of unemployment, and we focus on monthly transitions as well as those over a year or longer. As others have shown (Valletta 2011), the long-term unemployed have different labor market flows compared to short-term unemployed workers. We do not find any evidence that compositional changes over the business cycle account for cyclical swings in job finding rates or labor force withdrawal rates among the unemployed. The results suggest that a critical channel for the future path of long-term unemployment in the United States involves the evolution of labor force withdrawal rates by duration of unemployment over the business cycle. We will later embed different assumptions about movement from unemployed status to being out of the labor force into a calibration model along the lines of Kroft et al. (2014) to explore the role of labor force exits in the evolution of long-term unemployment and the Beveridge curve.

Figure 3.3 displays annual averages of monthly transition rates from unemployment to employment each year since 1994, based on BLS’s published transition rates for five duration-of-unemployment categories. Many researchers have documented that CPS data
can severely misstate gross labor market flows because of classification errors, which means one must use caution in interpreting the data. For instance, studies based on re-interview data from the CPS in which respondents are re-interviewed about one week after their initial interview have found that a person’s reported labor force status can be wildly inconsistent.\(^8\) If such classification errors are not perfectly serially correlated, then a nontrivial share of individuals may switch labor force status over time solely because of reporting errors in one month, which will tend to inflate transition rates computed from the CPS. Nonetheless, the CPS series conveys a signal about relative transition rates (both over time and across unemployment durations) and underlying movements in the official unemployment rate.

A few patterns are clear. First, the job finding rate is lower for those with a longer duration of unemployment, with the long-term unemployed finding jobs at less than half the rate of those who are very-short-term unemployed. Second, the cyclicality of job finding is clear in these data, with all rates declining during the recession of the early 2000s, and declining more dramatically during the Great Recession. Third, job finding rates for all groups remain well below their pre-Great Recession averages. Fourth, the job finding rate has risen for each group in the last 4 years, although it has barely increased for those unemployed longer than a year. In 2013, just under 10 percent of those who had been unemployed for more than one year transitioned into employment in the average month. This rate, though higher than in many European countries (Elsby et al. 2011), might overstate the prospects of the long-term unemployed due to classification errors and the fact that the long-term unemployed are particularly likely to take lower-paying, part-time jobs and temporary jobs; a point we revisit below.

The observed duration dependence in job finding rates could reflect worker heterogeneity (that is, that those with the most marketable skills tend to find jobs more quickly), or it could also be an effect of discouragement, skill erosion, and employer statistical discrimination against the long-term unemployed. Available evidence on the respective roles of

\(^8\)See Abowd and Zellner (1985) and Poterba and Summers (1986).
heterogeneity and duration dependence on unemployment hazard rates remains unsettled. On the one hand, econometric evidence that tries to model the distribution of unobserved heterogeneity tends to find that observed duration-dependent transition rates are not primarily a result of heterogeneous job searchers (Heckman and Singer 1984). Consistent with this interpretation is evidence showing that employers are less likely to call in workers for an interview if they have a jobless spell in their resumes (Kroft et al. 2014; Ghayad 2013), as well as evidence that the amount of time unemployed workers devote to searching for a job declines the longer they are unemployed (Krueger and Mueller 2011; Wanberg et al. 2012). On the other hand, studies have found nearly constant reemployment hazard functions if workers who are recalled to their previous job are removed from the sample (Katz 1986; Katz and Meyer 1990; Fujita and Moscarini 2013). We next examine whether the cyclical pattern of job finding rates is consistent with changes in observed worker heterogeneity and later turn to the issue of recall.

Figure 3.4, which uses the same scale for the $y$-axis as Figure 3.2, suggests that any effect of changing worker heterogeneity on the pattern of job finding rates over the business cycle for the long-term unemployed is very small. To construct this figure, we first estimated a logistic model in which the dependent variable was 1 if a worker who was unemployed in month $t$ was classified as employed in month $t + 1$, and 0 otherwise (that is, if the worker remained unemployed or exited the labor force). The explanatory variables were the same characteristics that were used to predict wages in Figure 3.2 (see Footnote 6). The model was estimated for the years 2004-2006. We then used the coefficients from this model to predict the job finding rate of the short-term (26 weeks or less) and long-term (27 weeks or longer) unemployed based on their characteristics each year.

The cyclical pattern suggests that there is a very slight shift in the characteristics of the long-term unemployed in recessionary periods toward those that are more favorable for finding a job, but the shift in the composition is modest, predicting a rise in the job finding rate of only about 1 to 2 percentage points. This is in contrast to the roughly 5-percentage-
point fall in the job finding rate for the long-term unemployed in the past two recessions. The shift in composition of the short-term unemployed is even smaller than that of the long-term unemployed.

Notice also that the predicted job finding rate for the long-term unemployed based on their characteristics is around 25 percent according to Figure 3.4. However, Figure 3.3 shows that the job-finding rate for the long-term unemployed is consistently well below that rate, even in periods of a relatively strong job market. (In 2004-2006, for example, the average monthly job finding rate for those unemployed longer than 26 weeks was 16 percent.) This overprediction is consistent with the view that the long-term unemployed face discrimination in the job market or become discouraged and search less intensively, or that they possess unobserved characteristics that lead to lower job finding prospects — or some combination of all three.

The short-term unemployed are predicted to have a slightly higher job-finding rate than the long-term unemployed. In contrast to the long-term unemployed, the predictions for the short-term unemployed are slightly below their actual average job finding rate (29 percent) in 2004-2006.

Figure 3.5 displays the monthly labor force withdrawal rates for the unemployed in each of the duration groups from 1994 to 2013. A few patterns are noteworthy. First, the long-term unemployed tend to have a higher rate of labor force exit than the short-term unemployed, perhaps partly reflecting their discouragement. Second, labor force exit rates tend to drop in a recession, especially for the long-term unemployed. Indeed, in the mild recession of the early 2000s, the labor force exit rate for the long-term unemployed fell by almost 10 percentage points to about the same level as the rate for recently unemployed

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9 This contrast does not appear to result from the labor force status of the long-term unemployed being more fluid from month to month than that of the short-term unemployed. Once they exit the labor force, the long-term and short-term unemployed have about the same probability of remaining out of the labor force. Over the period from 1994-2013, for example, we calculate that a worker who was long-term unemployed in his or her first survey month and then exited the labor force in the second survey month had a 64 percent chance of being out of the labor force in the third survey month. The corresponding figure for the short-term unemployed was very similar: 62 percent.
Likewise, in the deep recession in 2008–2009 the labor force withdrawal rate for the long-term unemployed again fell by around 10 percentage points, to virtually the same level as that of the short-term unemployed. Third, the labor force exit rate gradually rises for all duration groups after a recovery takes hold, and the rate rises more for the long-term unemployed. In other words, after labor force exit rates collapse in recessions to about the same level for all duration groups, they tend to move back in recoveries toward their historical norms, with a higher exit rate for the long-term unemployed.

Figure 3.6 suggests that a relatively small part of the cyclical pattern in the labor force exit rate for the long-term unemployed is due to compositional shifts and that for the short-term unemployed this is an even smaller factor. The figure shows the predicted unemployment-to-out-of-the-labor-force transition rates based on the same characteristics and approach used to construct Figure 3.4. Again, the scale is the same as in Figure 3.5. There is a cyclical pattern in the composition of the unemployed, with those more strongly attached to the labor force being more likely to be among the unemployed during a recession. Nevertheless, movements in composition would predict only about a 2-percentage-point decline in the labor force withdrawal rate for the long-term unemployed in a recession, in contrast with the roughly 10-percentage-point drop observed in the last two recessions. Over the 1994–2013 period, changes in the predicted labor force withdrawal rate of long-term unemployed workers due to shifts in their characteristics account for 18 percent of the total variation in the observed labor force withdrawal rate. This suggests that compositional shifts are responsible for part, but by no means all, of the time-series pattern of labor force attachment of the long-term unemployed shown in Figure 3.5.\footnote{This is based on calculating $1 - \frac{\Sigma(y_t - \hat{y}_t)^2}{\Sigma(y_t - \bar{y})^2}$, where $y_t$ is the observed labor force withdrawal rate in year $t$, $\hat{y}_t$ is the predicted rate from the logit equation combined with the characteristics of the long-term unemployed in year $t$, and $\bar{y}$ is the mean labor force withdrawal rate. Using the 1994–2013 data, this value equals 0.18.}

Labor force exits and their effect on the unemployment rate have often been neglected in past research, although recent work by Elsby et al. (2013) suggest that changes in the
participation margin account for 33 percent of the cyclical variation in the unemployment rate. We return to this issue in the calibration exercise at the end of the paper.

As mentioned earlier, during the most recent recession and similarly to the recession of the early 2000s, the rate of labor force withdrawal dropped for all durations of unemployment, but most markedly for the long-term unemployed, and only a small part of this drop was a result of compositional shifts. This phenomenon probably reflects, in part, the extension of unemployment insurance benefits, which require workers to search for a job and have been found to induce unemployed workers to stay in the labor force, thus elevating the measured unemployment rate (see Rothstein 2011; Farber and Valletta 2013). Many commenters have predicted that as these benefits are exhausted or scaled back, the withdrawal rate for the long-term unemployed may begin to rise toward its historical average. By 2013 it appeared that this process had begun to take place for those who had been unemployed for more than one year, but it was less apparent for those who had been unemployed between 27 and 52 weeks.

As shown in the simulations below, the movement of the labor force withdrawal rates of the long-term unemployed toward their historical averages has important implications for the unemployment rate and, relatedly, for the Beveridge curve. Nevertheless, barring an extraordinarily fast rebound in the labor force exit rates of the long-term unemployed relative to their short-term unemployed counterparts, it appears likely that the long-term unemployment rate will remain persistently high for a considerable time.

3.4 Longer-Term Transitions

To investigate more fully whether the long-term unemployed are on the margins of the labor market, we also look at transition rates for the long-term unemployed over longer periods of time using matched data from the CPS. In the CPS’s rotation group design, individuals are interviewed for 4 consecutive months, then dropped out of the survey for 8 months, and then interviewed again for 4 more months. This design makes it possible to
examine transitions over a 15-month interval. A monthly job finding rate of 10 to 15 percent for the long-term unemployed would exaggerate their connection to employment if random classification errors in labor force status inflate their transition rates or if the long-term unemployed tend to work in transitory jobs if they do become reemployed, as Stevens (1997) finds. One way to assess the importance of these issues, and obtain an alternative indicator of the extent to which the long-term unemployed are connected to the labor market, is to examine transition rates over a longer period of time.\footnote{Although classification errors will still bias job finding rates upward over a longer span of time, these errors only affect the first and last month of reported data and thus do not compound over the intervening months. By contrast, the impact of classification errors is magnified if monthly transition rates are used to compute longer-term transition rates, because the errors affect each monthly rate and thus compound over time.}

Indeed, the actual long-term transition rates are considerably lower than those implied by monthly data.\footnote{Assuming independence and a constant 0.10 probability of finding a job in any given month, the proportion of unemployed workers who gained employment within 15 months would be $1 - (1 - 0.10)^{15} = 0.79$.} As Figure 3.7.A illustrates, since the beginning of the Great Recession, 36 percent of those who were long-term unemployed in a given month were employed 15 months later; in comparison, 34 percent were not in the labor force and 30 percent were unemployed. Furthermore, of the 36 percent who were employed 15 months later, less than one-third had been employed full-time for 4 consecutive months. As a result, from 2008 to 2012, only 11 percent of those who were long-term unemployed in a given month returned to full-time, steady employment a year later.\footnote{Full-time, steady employment in this context means that someone who had been unemployed for 27 weeks or longer in month $t$ was employed full-time for 4 consecutive months starting in month $t + 12$. Throughout our analysis, we count the self-employed as employed.} If we include months of part-time employment, only 24 percent of long-term unemployed workers were reemployed for 4 consecutive months starting a year later.

If the experience of long-term unemployment weakens individuals’ labor market prospects, one would expect to see a difference in labor market outcomes between the short- and long-term unemployed. Figure 3.7.B also shows the comparable figures for the short-term unemployed. Although the difference between their job finding rates and those of the long-term unemployed is 13.6 percentage points, the difference between these two groups in finding
full-time, steady employment one year later is 3.6 percentage points. Still, with either measure, the CPS data indicate that over a 15-month span the job-finding hazard rate is about 25 percent lower for the long-term unemployed than the short-term unemployed. Although the short-term unemployed have struggled to find employment in this period, the long-term unemployed have faced even worse prospects.

Figure 3.8 provides a further disaggregated look at the transitions of the long-term unemployed, which highlights the transitory nature of their employment opportunities. In particular, the diagram divides the data by labor market status in the 2nd, 3rd, and 4th months in the sample. The first noteworthy observation from this figure is that only 22 percent of the long-term unemployed in month 1 report being employed for 1 month or more in months 2 through 4. This compares to 11 percent who report being out of the labor force in months 2 through 4, and 67 percent who report having been unemployed in at least 1 month between months 2 and 4 without ever moving to employment. Once the long-term unemployed leave the labor force for 3 straight months they are likely to stay out of the labor force, with only about 32 percent reentering; a slightly larger share move into employment than into unemployment, consistent with Barnichon and Figura (2013), who examined all nonparticipants who subsequently entered the labor force.

Those who did regain employment a year after exiting the labor force for 3 straight months were mostly employed intermittently or in part-time jobs. Only 2 percent of those who exited the labor force for 3 straight months were employed in full-time jobs for 4 consecutive months a year later.

Of the 22 percent who were employed in at least one of months 2 through 4 after having been long-term unemployed in the 1st survey month, 65 percent were also employed in month 16. But, even for this latter group, steady, full-time employment was not as prevalent as might be expected. Those who were jobless in months 2 through 4 displayed similar behavior to those who were initially long-term unemployed, as illustrated in Figure 3.7.A, although with slightly more movement into unemployment than into employment and not in the labor
force. All of these results underscore the fact that the long-term unemployed face difficulty regaining full-time, steady work over the longest period we can observe in CPS data. It appears that reemployment does not fully reset the clock for the long-term unemployed.

3.4.1 15-Month Transition Rates Over Time

Figure 3.9 shows the probability of moving to employment 15 months later for those who were classified as short-term unemployed (less than 27 weeks) or long-term unemployed (27 weeks or longer) in the initial survey month. (Time on the \(x\)-axis indicates the year of the initial survey.) Figure 3.10 provides the corresponding data for the likelihood of holding a full-time job for 4 consecutive months a year later by initial duration of unemployment, and Figure 3.11 contains the corresponding information for the likelihood of transitioning out of the labor force 16 months later. To lengthen the sample period, the figures begin with the 1982 CPS, although it is possible that the 1994 CPS redesign affected the data.

A few observations from these figures are noteworthy. First, over the entire 30-year period, the short-term unemployed are more likely to transition into employment (by either measure) than the long-term unemployed. As a percentage of long-term unemployed workers’ job finding rates, the differences are sizable, nearly 20 to 30 percent depending on the measure. However, in terms of levels, the differences in rates are not as great as one might expect if the long-term unemployed were distinctly more disconnected from the job market than the short-term unemployed (for example, in the last recovery 19 percent of the short-term unemployed were in steady, full-time jobs a year later as compared with 14 percent of the long-term unemployed).

Second, over the whole period, the long-term unemployed are, on average, 6 percentage points more likely to exit the labor force 15 months after the initial survey than are the short-term unemployed. The gap in the exit rate between the short-term and long-term unemployed narrows leading up to and coming out of a recession, and it grows during recoveries.
Lastly, we examine the cyclicality of the long-term transition rates. The figures indicate a pronounced drop in labor force exits around recessions for the long-term unemployed and a sharp drop in the job finding measures during recessions for the short-term unemployed. To summarize the cyclicality of the longer-term labor force transition rates by duration of unemployment, the top panel of Table 3.2 reports results of bivariate regressions, in which the dependent variable is the logarithm of the various 15-month transition rates for those who became unemployed in year \( t \) and the explanatory variable is the unemployment rate, averaged over years \( t \) and \( t + 1 \). The sample covers the period 1982 to 2013 (initial CPS from 1982 to 2012).

Consistent with the figures, the short-term unemployed display more of a procyclical relationship when it comes to job finding, especially for finding a steady, full-time job a year later. A 3-percentage-point drop in the unemployment rate, for example, is associated with a modest 20 percent increase in the transition rate into steady, full-time employment for the short-term unemployed. For the long-term unemployed, the coefficient on the unemployment rate in the regression for their transition to steady, full-time employment is statistically insignificant and small. Moreover, a \( t \)-test rejects the null hypothesis that the short-term and long-term unemployed have the same coefficient on the unemployment rate (two-tailed p-value = 0.04). Also, consistent with the visual impression from the figures, the labor force exit rate for the long-term unemployed is significantly and negatively related to the unemployment rate, while the relationship is insignificant and close to zero for the short-term unemployed.

One concern with the descriptive regressions in Table 3.2 is that the unemployment rate is partly determined by the flow rates that are the outcome variables of the models. To avoid the possibility of mechanical simultaneity bias, the lagged unemployment rate (that is, the year \( t - 1 \) unemployment rate) was used as an instrumental variable for the average.

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14 There is a long history of studying the cyclicality of the monthly job finding rate for the unemployed in CPS data. Important recent contributions are Elsby et al. (2009) and Shimer (2012). For the most part, that literature does not distinguish between short-term and long-term unemployed workers.

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unemployment rate in years $t$ and $t + 1$. The results, presented in the middle panel of Table 3.2, are qualitatively similar to those that used the average unemployment rate in the initial year and subsequent year, allaying concerns that simultaneity bias is responsible for the patterns in the upper panel.

Another possible concern is that the regression results are dominated by the period since the start of the Great Recession, when unemployment rose sharply. To address this issue, the bottom panel of Table 3.2 presents results where the sample is truncated in 2007. Again, the findings are similar. Indeed, the coefficient on the unemployment rate is positive but statistically insignificant in the regressions for both measures of employment for the initially long-term unemployed workers.

We performed some additional robustness checks of the results in Table 3.2. For example, in various models we included a time trend, included a dummy variable for the period before the CPS redesign, estimated the regressions with the transition rates in levels instead of logs, and limited the sample to the period after the CPS redesign but before the Great Recession. In all of these cases, we find results that are qualitatively similar to those in Table 3.2.

As a whole, this analysis of annual transition rates suggests that a stronger macroeconomy is only mildly associated with the likelihood of steady, full-time employment being regained by the long-term unemployed, and somewhat more strongly associated with similar employment being regained by the short-term unemployed.\(^{15}\) The long-term unemployed appear to be more insulated from the beneficial effects of a high-pressure labor market than the short-term unemployed. While a stronger economy appears to help the long-term unemployed to transition into some kind of employment, it does not seem to provide much help when it comes to finding steady, full-time employment.

\(^{15}\)This conclusion may seem at odds with Davis and von Wachter (2011), who find that earnings losses associated with job displacement are greater if workers are displaced during a period of high unemployment. However, it is unclear to what extent their findings are due to earnings for those who are reemployed versus the chance of becoming reemployed.
3.4.2 Meaning of “Not in the Labor Force” for the Long-Term Unemployed

As shown earlier, in recent years the long-term unemployed were slightly more likely to have left the labor force after 15 months than to have remained unemployed. We next explore the activities of the long-term unemployed when they report that they leave the labor force. This provides some purchase on whether those who exit the labor force are likely to seek employment if conditions improve and, therefore, to add to potential labor supply. It is also important to know whether those who leave the labor force are likely to be classified as “marginally attached,” since the marginally attached are more likely than other labor force nonparticipants to reenter the labor market (see Barnichon and Figura 2013; Krusell et al. 2011).

Using linked CPS data, we tabulated responses by those who had been long-term unemployed but then left the labor force to the following question: “(Do/Does) (name/you) currently want a job, either full or part time?” This question is critical for the BLS’s classification scheme. Someone who is out of the labor force but indicates that he or she wants a job is asked follow-up questions to determine his or her potential degree of discouragement. Conversely, someone who is out of the labor force but indicates that he or she does not want a job is precluded from being classified as “marginally attached” to the labor force.

Since the Great Recession, fully 73 percent of those who had been long-term unemployed in month 1 and then left the labor force by month 16 indicated that they did not want a job in month 16 of the survey. The share of nonparticipants who report that they do not want a job has trended upward over time (Barnichon and Figura 2013). Nevertheless, the large share of labor force exiters who say they no longer want a job suggests that they are exiting the labor force for an extended period of time. This is consistent with the view that many of the long-term unemployed were induced to search for a job and remained in

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16Currently, the official BLS measures of discouraged workers and marginally attached are relatively low. The U-5 measure of labor underutilization, which includes all marginally attached workers and has as its denominator the civilian labor force plus all persons marginally attached, stood at 8.1 percent in December 2013, just 1.4 percentage points above the headline unemployment rate.
the labor force longer than they otherwise desired in order to qualify for extended unemployment insurance benefits, and then left the labor force once benefits expired (Rothstein 2011; Farber and Valletta 2013).

This analysis included all respondents who classified themselves as “long-term unemployed” in their first month of the survey, so it is possible that a substantial portion of these workers may have already been effectively out of the labor force by the time they were surveyed in the CPS in month 16. To test whether or not this result essentially reflects a misclassification of “long-term unemployed” workers in month 1 of the survey, we also looked at those who reported being long-term unemployed every month during months 1 through 4 and months 13 through 15 and then left the labor force in month 16. Even with this restriction on the consistency of reported long-term unemployment, more than 40 percent of these long-term unemployed workers indicated that they did not want a job in the first month that they left the labor force.

A follow-up question for those who report they do not want a job is, “What best describes (name’s/your) situation at this time? For example, (are/is) (you/he/she) disabled, ill, in school, taking care of house or family, or something else?” Typically, those who leave the labor force because they no longer want a job report that they are either “taking care of house or family” or “in school” (Hotchkiss et al. 2012).

Since the Great Recession, those who had been long-term unemployed in the initial interview and then left the labor force by month 16 of the survey and reported that they no longer wanted a job indicated that they were currently “taking care of house or family” (51 percent), “in school” (23 percent), or engaged in “other” unspecified activities (20 percent). Other possible responses, including “disability” (4 percent), “illness” (2 percent), and “retirement” (1 percent), had modest response rates. In comparison to the long-term unemployed, those who had been unemployed for less than 27 weeks in month 1 and then left the labor force by month 16 and reported that they no longer wanted a job were (i) nearly twice as likely to report that they were currently in school (42 percent versus 23 percent),
and (ii) less likely to report that they were currently “taking care of house or family” (42 percent versus 51 percent).

The relatively low rate of long-term unemployed workers who withdraw from the labor force and report a disability suggests that the disability insurance program plays, at most, a minor role in incentivizing the long-term unemployed to withdraw from the labor force or in supporting them once they do withdraw from the labor force. This observation is also consistent with Mueller et al.’s (2013) conclusion that unemployment insurance exhaustions and disability insurance take-up are unrelated across states.

Lastly, it is worth noting that the behavior of (both short- and long-term) unemployed workers who exit the labor force suggests that those who exit the labor force are likely to do so for an extended period of time. For example, since the beginning of the Great Recession, of those who were long-term unemployed in month 1 and out of the labor force in month 2, 54 percent were out of the labor force and only 25 percent were employed in month 16. For the short-term unemployed, the comparable figures are 49 percent and 33 percent. While the difference in persistence of labor force nonparticipation may not be that great, the short-term unemployed are less likely than the long-term unemployed to exit the labor force in most years (see Figures 3.5 and 3.11).

3.5 Transition Rates: Survey of Income and Program Participation

The Survey of Income and Program Participation (SIPP) provides an alternative data set with which to examine longer-term transition rates by duration of unemployment. The SIPP has a number of strengths and weaknesses compared with the CPS. Two notable strengths are these: (i) the SIPP is specifically designed to be a longitudinal data set, and individuals who move to a new location are tracked in the survey; (ii) the sample consists of all those who enter a new spell of unemployment within the sample frame, so that unlike the CPS, it does not underrepresent short-term unemployment spells.\footnote{The CPS only includes ongoing spells of unemployment at the time of the survey reference week, which introduces length-biased sampling, and the CPS consequently underrepresents shorter spells of unemployment.} In terms of weaknesses,
the four-month retrospective interview in each wave of the SIPP has been found to cause “seam” effects that influence transition rates. In addition, the SIPP cannot be linked every year, lacks information on the duration of unemployment spells that were ongoing at the start of the survey, and has a relatively high attrition and nonresponse rate. Despite these limitations, the SIPP provides a way to assess the robustness of the transition rates estimated from the CPS.

We use data from the 1996, 2001, 2004, and 2008 SIPP panels. The panels vary in length from 3 years (the 2001 panel) to 5 years (the 2008 panel). We limit our sample to individuals who reported a new spell of unemployment each calendar year, and following Chetty (2008) we exclude the first sample month because new spells cannot be identified that month. The definition of a jobless spell that we employ in the SIPP requires that an individual be newly unemployed; subsequent months are counted in the spell if the individual is either unemployed or out of the labor force. This categorization differs from the CPS, which asks an unemployed worker to initially report his or her duration of unemployment (which in many cases likely includes months out of the labor force) and then increments the duration by an additional month each subsequent month the worker is without a job, available for work, and actively searching for work.

\[ \text{(Kiefer 1988)} \]

If the composition of the flow into unemployment is stable over time, however, the transition rates by duration of unemployment estimated from the CPS are unbiased. In the time period we examine, however, one could question whether the steady-state assumptions apply; hence, the SIPP likely provides a more representative summary of transition rates by duration of unemployment.

\[ \text{Each SIPP interview covers 4 months. The seam refers to adjacent months that cross from one interview to the next. The monthly transition rate is much higher in months that cross interviews than in months covered by the same interview. For example, in the 2008 panel, the one-month job finding rate was 23 percent between interviews and 9 to 12 percent for months covered by the same interview. The former is close to the CPS figure (which is known to be biased upward because of imperfectly serially correlated classification errors), and the latter could be biased downward because of the tendency for serially correlated classification errors within an interview.} \]

\[ \text{The SIPP panels comprise households (and descendant households) that are interviewed at 4-month intervals, called “waves.” Each SIPP panel consists of 9 to 15 waves. The sample in each wave consists of four rotation groups, each interviewed in a different month. For example, the first-wave interviews of the 2008 panel occurred from September to December 2008. The reference period for each interview is the preceding 4 months. We do not use data from pre-1996 panels because of significant changes introduced in the 1996 survey.} \]

\[ \text{Only the first spell of the year is included for individuals with multiple jobless spells in a calendar year. Across 14 calendar years, there were 24,194 short-term jobless spells (less than 7 months) and 17,272 long-term jobless spells (7 or more months) in our SIPP sample.} \]
There are some advantages to using the definition of jobless spells that we apply to the SIPP. For example, a worker who is unemployed for 6 months and then exits the labor force because he becomes discouraged is counted in the CPS as either short-term unemployed, if interviewed in the first 6 months, or as out of the labor force, if interviewed after 6 months, and excluded from our earlier results. Such an individual would be classified as long-term jobless in the SIPP. In any event, the SIPP provides an alternative to the CPS that can be used to probe the robustness of the CPS results, and our attempts to align the SIPP spell definition more closely with the CPS did not meaningfully alter our findings.

Figure 3.12 shows labor force transition rates for all newly unemployed workers 15 months after entering unemployment, and Figure 3.13 shows the corresponding results for the subset that became long-term jobless (those who were jobless for more than half a year). The contrast between the unconditional and conditional distributions provides an indication of the disparate outcomes between everyone who entered into unemployment and those who became long-term jobless. Paralleling the CPS analysis, the full-time steady employment category counts workers who were employed full-time for at least 4 consecutive months. The transition rates are reported according to the year that the jobless spell began. The figures display a pattern that is similar to the one drawn from the CPS. Among all those who became unemployed in 2012, 55 percent were employed 15 months later, and 31 percent of the long-term unemployed were employed. Twenty percent of all newly unemployed workers were in steady full-time positions a year after becoming unemployed in 2012, compared with 12 percent of the long-term jobless.

Nearly half (47 percent) of the long-term jobless were out of the labor force 15 months after entering unemployment in 2012, compared with 29 percent of all unemployed workers. Similar to the pattern in the CPS data, the labor force withdrawal rate for the long-term unemployed has risen over the course of the current recovery. Moreover, although the SIPP data are noisy, the labor force participation rate for the long-term jobless has been higher in expansionary years than in recessionary years, on average, while there is little difference
in the participation rate over the business cycle for the newly unemployed.

It is harder to discern cyclical patterns in the SIPP data than in the CPS because some years are missing data, but the job finding rate for the long-term jobless does not appear to be highly cyclical. For the long-term jobless, the average transition rate into either measure of employment was almost identical for those who lost their jobs in the Great Recession years (2008-2009) and those who lost their jobs in the last expansion (2004-2006). The job finding rates were higher in the late 1990s expansion, but they were about the same for the long-term jobless who entered unemployment during the 2001 recession as for their counterparts who became unemployed during 1996-1998. These data do not provide much support for the view that a stronger macroeconomy has coincided with improved job finding prospects for the long-term jobless.

We performed several checks on the data. First, we limited the sample to those below age 55 to avoid issues of early retirement. Second, we altered the definition of jobless spells by eliminating individuals who did not report searching for a job in at least one subsequent month after becoming unemployed. Third, we defined the duration of a jobless spell by the number of consecutive months an individual searched for a job. Fourth, we excluded individuals who reported that they were on layoff (either temporary or permanent), to preclude the possibility of recall to the workers’ previous jobs. In all of these cases, our findings were qualitatively similar to those reported in Figures 3.12 and 3.13.

3.5.1 Longer-Term Transition Rates for Continuous Joblessness

Thus far in looking at longer-term transitions, we have defined the long-term jobless as all of those who are out of work for more than 6 months. Although this is a common convention in the United States, it is arbitrary. Indeed, we find that there is a more or less continuous relationship between duration of joblessness and hazard rates into employment or out of the labor force.

Figure 3.14 shows the 15-month-ahead labor force status of workers with various lengths
of joblessness, for jobless spells that last from 1 to 36 months, based on the 2008 SIPP panel.
The panel began in May 2008 and ended in July 2013, and the sample consists of 18,000 new
jobless spells. (An individual could contribute more than one spell if that individual was
employed between spells.) Spells are included in the sample until they end in employment or
reach 36 months (or are censored). Labor force status is divided into four mutually exclusive
groups: full-time steady employed for 4 months in a row as of 15 months later (to match the
CPS definition), otherwise employed 15 months later (which includes those employed part-
time and others who have not been employed full-time for 4 consecutive months), unemployed
15 months later, and not in the labor force 15 months later.

The results indicate that the likelihood of holding a full-time job for 4 consecutive
months a year later (that is, in months 12-15 for someone in the first month of joblessness)
tends to decline with the duration of a jobless spell. For example, at the beginning of a
spell of unemployment, there was a 19 percent chance of holding a full-time, steady job a
year later. For those who were jobless for 6 to 18 months, that probability hovered around
10 percent, and for those who were jobless for 24 months the probability fell to about 6
percent. The probability fell even further, to 4 percent, for those who were jobless for 3
years. The finding that there was only a 19 percent chance of obtaining a full-time, steady
job a year after the start of a spell of unemployment reflects the fact that many unemployed
workers became long-term unemployed in this period, which was also evident in the CPS
comparisons.

The SIPP data also show a declining probability that a jobless worker will hold any
job 15 months down the road. A rising share of the jobless tend to leave the labor force as
the duration of joblessness increases, while the share unemployed 15 months in the future
tends to gradually decline after 6 months of joblessness. These results suggest that assigning
the status of long-term unemployed to all those who have been out of work for more than
6 months is an oversimplification that glosses over the poorer prospects of those who have
very long jobless spells. Nonetheless, the division does capture substantial differences in
the longer-term outcomes between the short- and long-term jobless. It is also a better approximation when it comes to the odds of finding steady, full-time work a year later, given the similarity in that transition rate for those out of work for 6 to 18 months, a group that represents a large share of the long-term unemployed.

3.6 Transition Rates: Work Trends Survey

A third data set that we use to examine longer-term transition rates is the Work Trends Survey (WTS), which was conducted by Knowledge Networks for Rutgers University’s Heldrich Center for Workforce Development (Zukin et al. 2011). The first wave of the WTS was conducted in August 2009. Respondents were asked whether they had been unemployed at any time in the previous 12 months as well as their labor force status at the time of the survey. A total of 1,202 unemployed workers were interviewed and then re-interviewed up to three times, with the final survey taking place in August 2011. The WTS allows for a comparison of ongoing spells and spells that were completed during the year. One weakness in the WTS survey, however, is that individuals who were unemployed in 2009 but out of the labor force as of August 2009 were not asked their duration of unemployment, so they are omitted from our analysis. The WTS data are also likely to suffer from other measurement problems, such as errors in respondents’ recall of their unemployment duration during the initial survey, but they nonetheless provide an alternative vantage point for viewing the 2-year labor force transition rates for those who were unemployed in the 12 months ending in August 2009.

Table 3.3 reports a tabulation of the WTS data. The combined sample of ongoing and completed spells indicates that 63 percent of those who were (or had been) short-term unemployed in the 12 months ending in August 2009 were employed in August 2011, compared with 47 percent of those who were (or had been) long-term unemployed in the 12 months ending 2009. The survey has information on full-time status as of August 2011, and the percentage-point gap is just as large: 38 percent versus 22 percent. The long-
term unemployed were a bit more likely to have left the labor force, but the labor force participation rate in the WTS data is considerably higher than in the CPS or SIPP, probably because those who had withdrawn from the labor force by August 2009 were necessarily excluded from the sample. In view of the fact that the transition rate is over a period of 2 years as opposed to 15 months, the job finding results are generally consistent with the CPS and SIPP data. Nevertheless, the stronger attachment to the labor force exhibited by both the short-term and long-term unemployed in the WTS data is surprising.

As one would expect, completed spells represent a larger share of all spells for the short-term unemployed (32 percent) than for the long-term unemployed (10 percent). The distribution of labor force status 2 years after the August 2009 survey is much more similar between the short-term and long-term unemployed for those who had completed their spell of unemployment as of the initial survey than it is for those who were in ongoing spells at that time. Comparing the ongoing spells with the combined sample, the gap in employment between the short-term and long-term unemployed is larger in the combined sample (using either measure of employment). There are many possible sources of these differences between the ongoing and combined sample. For example, the spells in the combined sample started a bit earlier in calendar time than the spells in the completed sample, and the composition of the newly unemployed could have changed over 2008-2009. In any case, the estimates point to a 20-25 percent lower job finding rate for the long-term unemployed than the short-term unemployed, and a 33-42 percent lower rate for full-time jobs. These rates encompass the range of estimates from the CPS and SIPP, despite the surveys’ varying designs, definitions of unemployment, and spans of time between surveys.

Lastly, the WTS data can be used to exclude individuals who expected to be recalled to their previous job in August 2009 (Table 3.4). Sixteen percent of the unemployed workers surveyed in 2009 reported that they had a “good chance” or “some chance” that they could return to their former employer. If we drop these individuals from the sample, the pattern of transition rates by duration of unemployment is notably similar. For example, in the com-
bined sample, 43 percent of the short-term unemployed were in full-time jobs 2 years later, compared with 23 percent of the long-term unemployed. This suggests that the possibility of recall does not account for the difference in reemployment rates between the short-term and long-term unemployed in this sample.

3.7 Regional Differences Within the United States

Some states are further along than others in recovering from the Great Recession. As of 2013, 11 states had unemployment rates that were below their average over the 25 years before the Great Recession (from 1982 to 2007). These states provide a possible indication of how the long-term unemployed could fare in a stronger economy. Our analysis suggests that long-term unemployment remains an unprecedented problem even in states that are currently experiencing low unemployment compared to their historical norm. The low-unemployment-rate states, many of which have benefited from a boom in energy production, are Alaska, Iowa, Louisiana, Montana, North Dakota, Oklahoma, Texas, Utah, Vermont, West Virginia, and Wyoming. The average unemployment rate in these 11 states in 2013 was 5.2 percent, compared with 7.2 percent in the rest of the country.

The shares of long-term unemployment in the two sets of states are shown in Figure 3.15, based on tabulations of the CPS each year from 1982 to 2013. The states with 2013 unemployment rates below their 25-year average also had relatively low unemployment and low long-term unemployment shortly before the Great Recession. Long-term unemployment grew dramatically in the low-unemployment-rate states during the Great Recession, reaching almost 35 percent of total unemployment in 2011, well above prerecession levels; for comparison, the previous high nationwide was 26 percent. The states with higher unemployment rates had a peak long-term-unemployment share of 45 percent, but the rise in long-term unemployment in these states started from a higher base. The long-term unemployment shares have fallen at about the same pace in both sets of states, down 6 percentage points from their peak in both groups. Even in states where the unemployment rate had fallen below
its historical average, however, in 2013 the share of long-term unemployed workers exceeded the previous nationwide peak.

To compare the longer-term prospects of the unemployed in both groups of states, we next examine long-term transition rates by duration of unemployment, using matched CPS data. Figures 3.16.A-F show the transition rates of the unemployed into: any employment 15 months later (Figures 3.16.A and 3.16.B); full-time, steady employment starting a year later (Figures 3.16.C and 3.16.D); and being out of the labor force 15 months later (Figures 3.16.E and 3.16.F). The figures do not show much evidence that the long-term unemployed are faring notably better in transitioning to employment in the low-unemployment states than they are in the high-unemployment states.

In the last year (2013), however, there has been an encouraging sign that the long-term unemployed are more likely to transition to full-time, steady positions in the low-unemployment states. It will be important to monitor whether this spike in transitions into steady employment continues. (It is also worth noting that despite this spike, 85 percent of the long-term unemployed in the low-unemployment states still had not managed to find steady, full-time employment a year after being initially surveyed.) The short-term unemployed, by contrast, have shown more consistent signs of improved job finding outcomes in the low-unemployment states since the start of the recovery. That pattern is consistent with the nationwide time-series evidence in Table 3.2, which shows a stronger response to economic conditions by the short-term unemployed than by the long-term unemployed.

Figures 3.16.E-F show that labor-force exit rates have been roughly comparable in both sets of states, especially for the short-term unemployed in the postrecession period. The labor force withdrawal rate for the long-term unemployed during the recovery has been slightly higher in the low-unemployment states than in the high-unemployment states, highlighting the risk that the long-term unemployment rate may return to normal because many of the long-term unemployed eventually exit the labor force. This issue is explored further in the calibration exercise in the next section.
3.8 Calibration Model

As is well known, after the Great Recession the relationship between vacancies and the unemployment rate, known as the Beveridge curve, shifted outward, with more vacancies than predicted given the high unemployment rate. Figure 3.17 shows the Beveridge curve using the total unemployment rate, and Figure 3.18 shows that this relationship is stable using the short-term unemployment rate. One possibility is that, after a severe shock, the Beveridge curve shifts out because of slow job growth, a rise in long-term unemployment, a reduction in overall match efficiency, and a decline in labor force exits, particularly among the long-term unemployed. The path of unemployment and vacancies could eventually loop back to the original Beveridge curve position because many of the long-term unemployed exit the labor force or (less likely) find a job, and the unemployment rate reflects a lower share of long-term unemployed workers after a time. Our goal in this section is to explore these hypotheses with a calibrated model of labor force flows and job matching in which the labor force withdrawal rate by duration eventually moves back to its historical norm.

Specifically, we extend the calibration model in Kroft et al. (2014). Kroft and colleagues estimate a search and matching model that is a simplification of Mortensen and Pissarides (1994) and Shimer (2005), with slight adjustments to help fit the data (for example to adjust for population growth and inconsistencies in flow data and reported durations). They focus on workers age 25 to 54 to avoid issues concerning the aging of the baby boom and increased school attendance. The authors assume that the unemployed \((U)\) and non-participants \((N)\) “meet” job openings according to a Cobb-Douglas “meeting function”:

\[
M(U + s \cdot N, V) = m_0 \cdot (U + s \cdot N)^{\alpha} \cdot V^{1-\alpha}
\]  

(3.1)

where \(M\) are meetings, \(s\) is the weight on the meeting efficiency of those not in the labor force relative to the unemployed, and \(V\) are vacancies. The authors assume that the share of

\(^{21}\)See Hobijn and Şahin (2012) for international evidence.
\(^{22}\)See Blanchard and Diamond (1989; 1994) for a prescient discussion of loops around the Beveridge curve and a model based on the assumption that employers rank job applicants based on their duration of unemployment.
meetings that go to the unemployed equals their share of search-intensity weighted nonemployment: \( \frac{U}{U + s \cdot N} \). The remaining meetings go to those not in the labor force, who are assumed to be hired if they obtain a meeting. The authors further assume that the probability that a meeting results in a hire for an unemployed individual is a declining function of his or her duration of unemployment, \( A(d) \), where \( d \) is duration. The authors then estimate \( \alpha \) and \( s \) to minimize the distance between the actual job finding rates for the unemployed and those not in the labor force between 2002 and 2007, and the predicted flows from the model:

- Probability of moving from unemployment to employment = \( A(d) \cdot m_0 \cdot x_t^{1-\alpha} \)
- Probability of moving from out of the labor force to employment = \( s \cdot m_0 \cdot x_t^{1-\alpha} \)

where \( x_t = \frac{V_t}{U_t + s \cdot N_t} \) and \( m_0 \) is a constant.

Using the predicted job finding rates for the unemployed by duration and those not in the labor force, as well as actual transition rates into non-employment and the actual path of vacancies, Kroft et al. (2014) are able to capture most of the rise in the share of long-term unemployed workers as a result of the slowdown in job vacancies that accompanied the Great Recession, as opposed to a change in the labor market performance of the long-term unemployed relative to the short-term unemployed. However, their model generates only a modest loop around the Beveridge curve that quickly returns to the original position.\(^{23}\)

We extend their model in two important respects. First, using data from 2002-2007, we estimate a job matching function, rather than a meeting function, of the form:

\[
J = (U_S + \delta \cdot U_L + \xi \cdot N)^\alpha \cdot V^{1-\alpha} \quad (3.2)
\]

where \( J \) is the number of jobs being filled by the nonemployed, \( U_S \) is the number of short-term

\(^{23}\)This observation is based on our replication of their model. In particular, when we replicated their model we also found a rise in the share of the long-term unemployed that mirrored the observed data. When we projected the Beveridge curve using their model, there was only a slight outward shift in the curve after the Great Recession that returned to the original position by late 2012, whereas the actual unemployment rate has not returned to the original curve as of this writing (June 2014). Intuitively, this finding resulted because their meeting function generated job growth, and a consequent drop in unemployment, that was stronger than observed in the recovery because it did not allow for a lower meeting rate of the long-term unemployed.
unemployed workers, $U_L$ is the number of long-term unemployed workers, $N$ is the number of nonparticipants, and $V$ is the number of vacancies. The parameters $\delta$ and $\xi$ reflect the potentially lower match efficiency of the long-term unemployed and nonparticipants. The short-term unemployed are defined as those with less than 27 weeks of unemployment, while the long-term unemployed are those with 27 weeks or more of unemployment. We estimate the parameters of the matching function by minimizing the distance between the system of equations for flows into employment for nonparticipants, short-term unemployed, and long-term unemployed using monthly CPS data.\(^{24}\) The estimated coefficients (with bootstrapped standard errors in parentheses) are: $\delta = 0.60 (.03)$, $\xi = 0.29 (.01)$, and $\alpha$ is 0.79 (.06).

Kroft et al. (2014) implicitly assumed that $\delta = 1$ in their meeting function, although they allowed for differential job finding rates for the short- and long-term unemployed because their meeting function is combined with $A(d)$ terms to generate job matches. Their estimate of $s$ was close to our estimate of $\xi$.

Kroft et al. (2014) imposed the same labor force withdrawal rate for the short-term and long-term unemployed. While this was plausible in the immediate aftermath of a recession, over time the labor force withdrawal rate tends to rise as the economy recovers, especially for the long-term unemployed (see Figure 3.5). Our second extension is that we allow for differential labor force withdrawal rates by duration of unemployment.

We follow Kroft et al. (2014) in letting the observed number of vacancies, labor force withdrawal rates, and transitions into unemployment evolve as they did from January 2008 forward. We also follow Kroft et al. (2014) and assign a duration of unemployment to those who initially transition from nonparticipant to unemployed, and those who transitioned from employed to unemployed, based on the observed distributions in that calendar year. For those who remain unemployed from one period to the next, we increment their duration of unemployment by 1 month.

\(^{24}\)To be more precise, the parameters were estimated using the labor force flows for those with more or less than 26 weeks of unemployment, and ensuring that the stocks of the number of unemployed workers implied by the transition rates match the actual CPS stocks of those with 6 and 7 months of unemployment.
Figure 3.19 uses our matching function estimated over the period 2002-2007 to project the Beveridge curve from 2008 to 2013. The projection seems to match the broader trends in the data reasonably well. The calibrated model predicts an outward shift in the Beveridge curve similar to what has been observed. In 2012 and 2013, the projection begins to move back toward the original Beveridge curve. The projection initially underpredicts the 5-percentage-point rise in the unemployment rate from January 2008 to October 2009 by one percentage point, and remains slightly to the left of the actual data. This under-prediction is consistent with Hall and Schulhofer-Wohl (2013), who find that there has been a steady downward drift in matching efficiency in the 2000s.\textsuperscript{25} As the labor force exit rate of both the long- and short-term unemployed began to rise, the projection began to move closer to the original Beveridge curve. As of December 2013, the model predicts that the unemployment rate would be 0.8 percentage point lower than the actual rate. As a whole, however, the calibrated model seems to capture the broad outlines of the shift of the Beveridge curve reasonably well. The root mean square error between the actual unemployment rate and projection based on the calibrated model is 0.9 percentage point.

We next use the calibrated model to investigate the impact on unemployment of the cyclical path of labor force withdrawals by duration of unemployment. As mentioned earlier, labor force exits collapsed for the long-term unemployed during the Great Recession and in the ensuing few years, before rising in the direction of their historical average. What impact did this pattern have on the unemployment rate and shift in the Beveridge curve? Figure 3.20 presents calibrated results where we impose the labor-force withdrawal rates for the long-term and short-term unemployed that occurred in 2006-2007, just before the recession, each year going forward. Had labor force withdrawal rates not collapsed and instead remained at their 2006-2007 levels, the figure indicates that the loop around the Beveridge curve would have been much more circumscribed and short-lived. The unemployment rate would have been underpredicted by 3 percentage points under this scenario. The root mean square error

\textsuperscript{25}Hall and Schulhofer-Wohl’s analysis focuses on eight different types of job seekers to control for heterogeneity.
in this counterfactual model rises to 1.6 percentage points, substantially worse than the fit when the actual path of labor force participation by duration of unemployment is used in the model.

To explore the role of the gradual increase in labor force withdrawal by the unemployed since the unemployment rate peaked in October 2009, we conducted another counterfactual exercise, in which we maintained the labor force withdrawal rates for the short-term and long-term unemployed at their October 2009 levels and projected the unemployment rate through December 2013 using the estimated matching function and all of the other realized flow data. Compared to the calibrated model (with the same matching function and realized flow variables, including actual labor force withdrawal rates) the unemployment rate was 1.3 percentage points higher in the counterfactual projection. Thus, the increase in labor force withdrawal rates from October 2009 through December 2013 appears to account for a little over one percentage point of the 3.2-percentage-point drop in the unemployment rate for prime-age workers. In this period, the labor force withdrawal rate for the short-term unemployed had returned to close to its historical average, while the rate for the long-term unemployed was still below it.

To probe what the calibration exercise implies going forward, we extended the projections through 2016 starting with the data observed for December 2013. Vacancies are treated as exogenous in the model; we assumed that vacancies grow at the same rate as they have over the last 2 years (31,000 per month). The labor force exit rates of the long- and short-term unemployed are assumed to linearly return from their December 2013 levels to their 2006 averages by 2016, an assumption that appears consistent with the 2002-2007 recovery and current trends (see Figure 3.5), as are the other flows to nonemployment. Under these assumptions, Figure 3.21 shows that by December 2016 the labor market is projected to almost return to the original Beveridge curve. This implies that the combination of rising labor force withdrawal rates and lower match efficiency for the long-term unemployed can account for a loop around the Beveridge curve.
Lastly, Figure 3.22 shows the share of prime-aged workers who are predicted by the model to be long-term unemployed each month since December 2007. The matching function does a relatively good job of capturing the rise in the share of long-term unemployment from 2009 to 2010.\textsuperscript{26} An extension of the calibrated model implies that as vacancies and matches rise, coupled with labor force withdrawal rates returning to their earlier, higher levels, long-term unemployment is expected to decline gradually, although by the end of 2016 its share is projected to remain well above prerecession levels.

We conclude from this exercise that the varying pattern of labor force withdrawal by unemployment duration is an important feature of the job market. Moreover, the fact that a similar pattern was observed in the past recovery suggests that labor force withdrawal of the long-term unemployed is historically an important (but unfortunate) mechanism by which the labor market returns to equilibrium.

3.9 Conclusion

The Great Recession and subsequent recovery have been distinguished by an exceptionally high rate of long-term unemployment. The extent to which the long-term unemployed actively search for a job and transition into employment or grow discouraged and exit the labor force will determine the degree of effective slack in the economy. Across a variety of data sets, over a horizon of 1 to 2 years the job finding rate among the long-term unemployed is about 20 to 40 percent below that of the short-term unemployed. Although the long-term unemployed have about a 1 in 10 chance of moving into employment in any given month, when they do return to work their new jobs are often transitory. After 15 months, the long-term unemployed are more than twice as likely to have withdrawn from the labor force as to have settled into steady, full-time employment. And when they do exit the labor force, the unemployed tend to say that they no longer want a job, suggesting that many labor force

\textsuperscript{26}The increases in the share of long-term unemployed at the beginning of 2011 and 2012 are due to a feature of the calibration model: at the beginning of the year, the distribution of unemployment spells for those who entered unemployment from employment or out of the labor force is updated to correspond to the actual distribution for such workers in that calendar year.
exits could be enduring. Furthermore, our calibration exercise suggests that the decline and gradual rise in labor force exits of the long-term unemployed over the business cycle plays an important role in the outward and then inward shift of the unemployment-vacancy relationship.

Past research has found many benefits of a high-pressure labor market, such as wage growth and upward career mobility, but many of those benefits do not appear to accrue to the long-term unemployed.\textsuperscript{27} Job finding rates are more sensitive to the state of the business cycle for the short-term unemployed than they are for the long-term unemployed, suggesting that the long-term unemployed are more insulated from macroeconomic developments than the short-term unemployed. Labor force exit rates are countercyclical for the long-term unemployed. Even in the roaring 1990s, a relatively small share of the long-term unemployed returned to full-time, stable employment; their problems were less prominent then because they accounted for a smaller share of the unemployed, however. Indeed, the main benefit of a stronger economy in regard to long-term unemployment may be that it reduces the likelihood that the short-term unemployed become long-term unemployed.

The portrait of the long-term unemployed in the United States that emerges here suggests that, to a considerable extent, they are an unlucky subset of the unemployed, and that their prospects decline the longer they remain unemployed, possibly because their skills atrophy. Their diverse and varied set of characteristics implies that a broad array of policies will be needed to substantially raise their job finding rate and stem their rising labor force withdrawal rate, since concentrating on any single occupation, industry, demographic group, or region is unlikely to materially improve the well-being of the long-term unemployed by itself. Understanding both the labor market hurdles and the personal hurdles faced by the long-term unemployed should be a priority for future research in order to craft solutions to reduce long-term unemployment.

Some may wish to draw macroeconomic policy implications from our findings. Only

\textsuperscript{27}See Okun (1973) and Katz and Krueger (1999).
time will tell if inflation and real wage growth are more dependent on the short-term unemployment rate than on the total unemployment rate. To us, the most important policy challenges involve designing effective interventions to prevent the long-term unemployed from receding into the margins of the labor market or withdrawing from the labor force altogether, and supporting those who have left the labor force to engage in productive activities. Overcoming the obstacles that prevent many of the long-term unemployed from finding gainful employment, even in good times, will likely require a concerted effort by policymakers, social organizations, communities, and families, in addition to appropriate monetary policy.
Table 3.1: Profile of the Employed, Short-Term Unemployed, and Long-Term Unemployed, 2013

<table>
<thead>
<tr>
<th></th>
<th>Employed</th>
<th>Short-Term Unemployed</th>
<th>Long-Term Unemployed</th>
</tr>
</thead>
<tbody>
<tr>
<td>Gender (%)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Male</td>
<td>53</td>
<td>55</td>
<td>56</td>
</tr>
<tr>
<td>Female</td>
<td>47</td>
<td>45</td>
<td>44</td>
</tr>
<tr>
<td>Age (%)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>16-34</td>
<td>34</td>
<td>57</td>
<td>42</td>
</tr>
<tr>
<td>35-49</td>
<td>32</td>
<td>23</td>
<td>28</td>
</tr>
<tr>
<td>50+</td>
<td>33</td>
<td>20</td>
<td>30</td>
</tr>
<tr>
<td>Marital Status (%)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Married</td>
<td>56</td>
<td>33</td>
<td>34</td>
</tr>
<tr>
<td>Widowed/Divorced/Separated</td>
<td>15</td>
<td>14</td>
<td>19</td>
</tr>
<tr>
<td>Never Married</td>
<td>29</td>
<td>53</td>
<td>47</td>
</tr>
<tr>
<td>Race (%)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>White, Non-Hispanic</td>
<td>66</td>
<td>55</td>
<td>51</td>
</tr>
<tr>
<td>African American</td>
<td>11</td>
<td>17</td>
<td>23</td>
</tr>
<tr>
<td>Hispanic</td>
<td>16</td>
<td>21</td>
<td>18</td>
</tr>
<tr>
<td>Asian/Pacific Islanders</td>
<td>6</td>
<td>4</td>
<td>5</td>
</tr>
<tr>
<td>Education (%)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Less Than High School</td>
<td>9</td>
<td>22</td>
<td>18</td>
</tr>
<tr>
<td>High School</td>
<td>27</td>
<td>33</td>
<td>36</td>
</tr>
<tr>
<td>Some College</td>
<td>19</td>
<td>21</td>
<td>20</td>
</tr>
<tr>
<td>Associate Degree</td>
<td>11</td>
<td>8</td>
<td>9</td>
</tr>
<tr>
<td>Bachelor's Degree or Higher</td>
<td>35</td>
<td>17</td>
<td>19</td>
</tr>
<tr>
<td>Industry (%)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Construction</td>
<td>6</td>
<td>12</td>
<td>10</td>
</tr>
<tr>
<td>Manufacturing</td>
<td>10</td>
<td>9</td>
<td>11</td>
</tr>
<tr>
<td>Wholesale and Retail Trade</td>
<td>14</td>
<td>14</td>
<td>16</td>
</tr>
<tr>
<td>Finance and Real Estate</td>
<td>7</td>
<td>4</td>
<td>5</td>
</tr>
<tr>
<td>Professional and Business Services</td>
<td>12</td>
<td>14</td>
<td>14</td>
</tr>
<tr>
<td>Education and Health Care</td>
<td>23</td>
<td>15</td>
<td>15</td>
</tr>
<tr>
<td>Leisure and Hospitality</td>
<td>9</td>
<td>16</td>
<td>12</td>
</tr>
<tr>
<td>All Other</td>
<td>19</td>
<td>16</td>
<td>16</td>
</tr>
<tr>
<td>Occupation (%)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Professional and Technical</td>
<td>22</td>
<td>12</td>
<td>11</td>
</tr>
<tr>
<td>Managerial and Financial</td>
<td>16</td>
<td>7</td>
<td>9</td>
</tr>
<tr>
<td>Administrative</td>
<td>12</td>
<td>12</td>
<td>15</td>
</tr>
<tr>
<td>Sales and Service</td>
<td>32</td>
<td>40</td>
<td>38</td>
</tr>
<tr>
<td>Blue Collar</td>
<td>17</td>
<td>29</td>
<td>27</td>
</tr>
</tbody>
</table>

Note: Industry and occupation refer to the previous job held by the unemployed, for those who held jobs.
Source: Authors’ calculations from the Current Population Survey.
Table 3.2: Cyclicality of Longer-Term Transitions: Regressions on Unemployment Rate

### Full Sample (N=30)

<table>
<thead>
<tr>
<th>Group</th>
<th>Transition Rate (Dependent Variable):</th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>U-U</td>
<td>U-O</td>
<td>U-E</td>
<td>U-FTSE</td>
</tr>
<tr>
<td>Short-Term Unemployed</td>
<td>0.106 ***</td>
<td>-0.007</td>
<td>-0.031 ***</td>
<td>-0.062 ***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.019)</td>
<td>(0.006)</td>
<td>(0.006)</td>
<td>(0.008)</td>
<td></td>
</tr>
<tr>
<td>Long-Term Unemployed</td>
<td>0.096 ***</td>
<td>-0.041 *</td>
<td>-0.026</td>
<td>-0.014</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.013)</td>
<td>(0.017)</td>
<td>(0.015)</td>
<td>(0.023)</td>
<td></td>
</tr>
</tbody>
</table>

### Instrument for Unemployment Rate with Previous Year's Unemployment Rate (N=30)

<table>
<thead>
<tr>
<th>Group</th>
<th>Transition Rate (Dependent Variable):</th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>U-U</td>
<td>U-O</td>
<td>U-E</td>
<td>U-FTSE</td>
</tr>
<tr>
<td>Short-Term Unemployed</td>
<td>0.070 **</td>
<td>-0.004</td>
<td>-0.017</td>
<td>-0.044 *</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.021)</td>
<td>(0.007)</td>
<td>(0.010)</td>
<td>(0.018)</td>
<td></td>
</tr>
<tr>
<td>Long-Term Unemployed</td>
<td>0.075 ***</td>
<td>-0.061 *</td>
<td>0.005</td>
<td>0.026</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.020)</td>
<td>(0.028)</td>
<td>(0.027)</td>
<td>(0.040)</td>
<td></td>
</tr>
</tbody>
</table>

### Pre-2008 Sample (N=25)

<table>
<thead>
<tr>
<th>Group</th>
<th>Transition Rate (Dependent Variable):</th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>U-U</td>
<td>U-O</td>
<td>U-E</td>
<td>U-FTSE</td>
</tr>
<tr>
<td>Short-Term Unemployed</td>
<td>0.089 ***</td>
<td>-0.007</td>
<td>-0.022 ***</td>
<td>-0.063 ***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.022)</td>
<td>(0.010)</td>
<td>(0.004)</td>
<td>(0.007)</td>
<td></td>
</tr>
<tr>
<td>Long-Term Unemployed</td>
<td>0.097 ***</td>
<td>-0.073 ***</td>
<td>0.001</td>
<td>0.031</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.017)</td>
<td>(0.009)</td>
<td>(0.010)</td>
<td>(0.016)</td>
<td></td>
</tr>
</tbody>
</table>

Statistical significance at the ***0.1 percent, **1 percent, and *5 percent levels.

Note: Table reports coefficient on unemployment rate from bivariate regression of log of transition rate on unemployment rate. Unemployment rate is the average of the rate in the year of the initial CPS year and the following year. Annual data for those who entered the survey from 1982 to 2012. No matched data are possible for those who entered the CPS in 1993 because of the CPS redesign. Newey-West standard errors (with 3 lags) shown in parentheses. FTSE is full-time, steady employment in months 13 to 16 of the survey. Source: Bureau of Labor Statistics; authors’ calculations using Current Population Survey Longitudinal Population Database (see Nekarda 2009).
Table 3.3: Long-Term Labor Force Transitions from Work Trends Survey

<table>
<thead>
<tr>
<th>Labor Force Status 2 Years Later</th>
<th>Sample:</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Ongoing</td>
<td>Completed</td>
<td></td>
</tr>
<tr>
<td>Employed</td>
<td>52.7 %</td>
<td>83.5 %</td>
<td>63.1 %</td>
</tr>
<tr>
<td>Employed Full-Time</td>
<td>23.7 %</td>
<td>67.3 %</td>
<td>38.4 %</td>
</tr>
<tr>
<td>Not in the Labor Force</td>
<td>15.6 %</td>
<td>8.1 %</td>
<td>13.1 %</td>
</tr>
<tr>
<td>Unemployed</td>
<td>31.8 %</td>
<td>8.4 %</td>
<td>23.9 %</td>
</tr>
<tr>
<td>N</td>
<td>175</td>
<td>82</td>
<td></td>
</tr>
</tbody>
</table>

Long-Term Unemployed

<table>
<thead>
<tr>
<th>Labor Force Status 2 Years Later</th>
<th>Sample:</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Ongoing</td>
<td>Completed</td>
<td></td>
</tr>
<tr>
<td>Employed</td>
<td>42.4 %</td>
<td>84.5 %</td>
<td>47.7 %</td>
</tr>
<tr>
<td>Employed Full-Time</td>
<td>15.8 %</td>
<td>67.4 %</td>
<td>22.3 %</td>
</tr>
<tr>
<td>Not in the Labor Force</td>
<td>15.8 %</td>
<td>3.3 %</td>
<td>14.2 %</td>
</tr>
<tr>
<td>Unemployed</td>
<td>41.8 %</td>
<td>12.2 %</td>
<td>38.1 %</td>
</tr>
<tr>
<td>N</td>
<td>284</td>
<td>30</td>
<td></td>
</tr>
</tbody>
</table>

Note: Sample of those who were unemployed at some point between August 2008 and August 2009. Results use sample weights from first wave. Long-term unemployed were either unemployed for more than six months in August 2009 or had been unemployed for more than six months in the previous year before gaining employment. Ongoing spells refer to those out of work at the time of the initial survey. Completed spells refer to those who were employed by the time of the initial survey.

Source: Heldrich Center’s Work Trends Survey, Roper Center; authors’ calculations.
Table 3.4: Long-Term Labor Force Transitions from Work Trends Survey, Excluding Recall

<table>
<thead>
<tr>
<th>Labor Force Status 2 Years Later</th>
<th>Sample:</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Ongoing</td>
<td>Completed</td>
</tr>
<tr>
<td>Employed</td>
<td>50.7 %</td>
<td>87.2 %</td>
</tr>
<tr>
<td>Employed Full-Time</td>
<td>27.3 %</td>
<td>71.4 %</td>
</tr>
<tr>
<td>Not in the Labor Force</td>
<td>15.6 %</td>
<td>3.4 %</td>
</tr>
<tr>
<td>Unemployed</td>
<td>33.7 %</td>
<td>9.4 %</td>
</tr>
<tr>
<td>N</td>
<td>142</td>
<td>54</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Labor Force Status 2 Years Later</th>
<th>Sample:</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Ongoing</td>
<td>Completed</td>
</tr>
<tr>
<td>Employed</td>
<td>41.2 %</td>
<td>88.1 %</td>
</tr>
<tr>
<td>Employed Full-Time</td>
<td>16.4 %</td>
<td>61.4 %</td>
</tr>
<tr>
<td>Not in the Labor Force</td>
<td>16.4 %</td>
<td>7.5 %</td>
</tr>
<tr>
<td>Unemployed</td>
<td>42.5 %</td>
<td>4.4 %</td>
</tr>
<tr>
<td>N</td>
<td>259</td>
<td>23</td>
</tr>
</tbody>
</table>

Note: Excludes workers who reported that there is “a good chance” or “some chance” they could work for their former employer. Sample of those who were unemployed at some point between August 2008 and August 2009. Results use sample weights from first wave. Long-term unemployed were either unemployed for more than six months in August 2009 or had been unemployed for more than six months in the previous year before gaining employment. Ongoing spells refer to those out of work at the time of the initial survey. Completed spells refer to those who were employed by the time of the initial survey. Source: Heldrich Center’s Work Trends Survey, Roper Center; authors’ calculations.
Figure 3.1: Unemployment Rates by Duration, 1948-2014

Percent of Civilian Labor Force (Seasonally Adjusted)

Note: Shading denotes recession.
Figure 3.2: Mean Predicted Wages for the Unemployed by Duration of Unemployment, 1994-2013

2013 Dollars Per Hour (Log Scale)

Note: Mean predicted hourly wages for the unemployed (annual averages) based on their characteristics. Shading denotes recession.

Figure 3.3: Monthly Probability of Transitioning from Unemployment to Employment by Duration of Unemployment, 1994-2013

Percent of Each Category of Unemployment Duration

Note: Dashed lines represent 1994-2007 averages. Shading denotes recession.
Figure 3.4: Mean Predicted Probability of Transitioning from Unemployment to Employment for the Unemployed Based on Their Characteristics, by Duration of Unemployment, 1994-2013

Note: Annual averages. Shading denotes recession. Predicted transition rate was derived by estimating a logistic model where the dependent variable was one if a worker who was unemployed in month $t$ was classified as employed in month $t + 1$, and zero otherwise (i.e., if the worker remained unemployed or exited the labor force). Explanatory variables were: education, experience, industry, occupation, race, new entrant status, gender, and marital status. The model was estimated for the years 2004-2006. The estimated coefficients from this model were then combined with the characteristics of the long-term unemployed (defined as those unemployed for longer than 26 weeks at the time of the survey) and short-term unemployed each year to predict the probability of transitioning to employment in the next month.

Figure 3.5: Monthly Probability of Transitioning from Unemployment to Out of the Labor Force, by Duration of Unemployment, 1994-2013

Percent of Each Category of Unemployment Duration

Note: Dashed lines represent 1994-2007 averages. Shading denotes recession.
Figure 3.6: Mean Predicted Probability of Transitioning from Unemployment to Out of the Labor Force for the Unemployed Based on Their Characteristics, by Duration of Unemployment, 1994-2013

Note: Annual averages. Shading denotes recession. Predicted transition rate was derived by estimating a logistic model where the dependent variable was 1 if a worker who was unemployed in month $t$ was classified as out of the labor force in month $t + 1$, and zero otherwise (i.e., if the worker remained unemployed or was classified as employed). Explanatory variables were education, experience, industry, occupation, race, new entrant status, gender, and marital status. The model was estimated for the years 2004-2006. The estimated coefficients from this model were then combined with the characteristics of the long-term unemployed (defined as those unemployed for longer than 26 weeks at the time of the survey) and the short-term unemployed each year to predict the probability of transitioning out of the labor force in the next month.

Figure 3.7.A: Longitudinal Transition Rates for the Long-Term Unemployed, 2008-2013

Note: Chart reflects the experience of those who were long-term unemployed in their first Current Population Survey interview (2008-2012) and their labor force status 15 months later (2009-2013).
Figure 3.7.B: Longitudinal Transition Rates for the Short-Term Unemployed, 2008-2013

Note: Chart reflects the experience of those who were short-term unemployed in their first Current Population Survey interview (2008-2012) and their labor force status 15 months later (2009-2013).
Figure 3.8: Longitudinal Transition Rates for the Long-Term Unemployed, 2008-2013

Figure 3.9: Probability of Transitioning from Unemployment to Employment After 15 Months, by Duration of Unemployment, 1982-2012

Percent of Total Labor Force Status Flows

Note: Dashed lines represent 1982-2007 averages. Shading denotes recession. Year on x-axis represents the survey entry year. There is no observation for 1993 due to survey redesign and CPS matching difficulties. Source: Authors’ calculations using Current Population Survey Longitudinal Population Database (see Nekarda 2009); National Bureau of Economic Research.
Figure 3.10: Probability of Transitioning from Unemployment to Full-Time, Steady Employment in Months 13-16, by Duration of Unemployment, 1982-2012

Note: Dashed lines represent 1982-2007 averages. Shading denotes recession. Year on x-axis represents the survey entry year. There is no observation for 1993 due to survey redesign and CPS matching difficulties. Source: Authors’ calculations using Current Population Survey Longitudinal Population Database (see Nekarda 2009); National Bureau of Economic Research.
Figure 3.11: Probability of Transitioning from Unemployment to Out of the Labor Force After 15 Months, by Duration of Unemployment, 1982-2012

Percent of Total Labor Force Status Flows

Note: Dashed lines represent 1982-2007 averages. Shading denotes recession. Year on x-axis represents the survey entry year. There is no observation for 1993 due to survey redesign and CPS matching difficulties.

Figure 3.12: Transition Rates for All Jobless Spells, 15 Months After Start of Spell, 1996-2013

Note: Data cover all workers who became unemployed in the calendar year. Shading denotes recession. Source: Census Bureau (Survey of Income and Program Participation); National Bureau of Economic Research; authors’ calculations.
Figure 3.13: Transition Rates for Long-Term Jobless Spells, 15 Months After Start of Spell, 1996-2013

Note: Data cover all workers who became unemployed in the calendar year. Once becoming unemployed, they were jobless, either unemployed or not in the labor force, for at least six consecutive months. Shading denotes recession.

Source: Census Bureau (Survey of Income and Program Participation); National Bureau of Economic Research; authors’ calculations.
Figure 3.14: 15-Month Ahead Transition Rates for All Jobless Spells from 2008 to 2012, by Jobless Spell Duration

Note: Data cover all workers who became unemployed from 2008 to 2012.
Source: Census Bureau (Survey of Income and Program Participation); authors’ calculations.
Figure 3.15: Long-Term Unemployed Share by Whether State is Above or Below Long-Run Unemployment Average, 1982-2013

Note: Shading denotes recession.
Figure 3.16.A: Transition to Employment After 15 Months for Short-Term Unemployed, by Status of 2013 State Unemployment Rate Above/Below Historical Average, 1994-2012

Note: Shading denotes recession. Year on x-axis represents the survey entry year. Short-term unemployed defined as unemployment duration up to 26 weeks.

Figure 3.16.B: Transition to Employment After 15 Months for Long-Term Unemployed, by Status of 2013 State Unemployment Rate Above/Below Historical Average, 1994-2012

Note: Shading denotes recession. Year on x-axis represents the survey entry year. Long-term unemployed defined as unemployment duration greater than 26 weeks.

Percent of Total Labor Force Status Flows

States Above 25-Year Average
States Below 25-Year Average

Figure 3.16.C: Transition to Full-Time, Steady Employment After 15 Months for Short-Term Unemployed, by Status of 2013 State Unemployment Rate Above/Below Historical Average, 1994-2012

Percent of Total Labor Force Status Flows

Note: Shading denotes recession. Year on x-axis represents the survey entry year. Short-term unemployed defined as unemployment duration up to 26 weeks.

Figure 3.16.D: Transition to Full-Time, Steady Employment After 15 Months for Long-Term Unemployed, by Status of 2013 State Unemployment Rate Above/Below Historical Average, 1994-2012

Percent of Total Labor Force Status Flows

States Above 25-Year Average
States Below 25-Year Average

Note: Shading denotes recession. Year on x-axis represents the survey entry year. Long-term unemployed defined as unemployment duration greater than 26 weeks.

Figure 3.16.E: Transition to Out of the Labor Force After 15 Months for Short-Term Unemployed, by Status of 2013 State Unemployment Rate Above/Below Historical Average, 1994-2012

Note: Shading denotes recession. Year on x-axis represents the survey entry year. Short-term unemployed defined as unemployment duration up to 26 weeks.
Figure 3.16.F: Transition to Out of the Labor Force After 15 Months for Long-Term Unemployed, by Status of 2013 State Unemployment Rate Above/Below Historical Average, 1994-2012

Percent of Total Labor Force Status Flows

Note: Shading denotes recession. Year on x-axis represents the survey entry year. Long-term unemployed defined as unemployment duration greater than 26 weeks.

Figure 3.17: Job Vacancy Rate vs. Unemployment Rate, 2000-2014

Job Vacancy Rate (Percent)

Unemployment Rate (Percent)

Dec-00
Dec-00 to Feb-01
Mar-01 to Nov-01
Apr-14
Jul-09 to Apr-14
Dec-01 to Nov-07
Dec-07 to Jun-09

Note: Job vacancy rate is defined as job openings as a percentage of the sum of job openings and total nonfarm payroll employment.
Figure 3.18: Job Vacancy Rate vs. Short-Term Unemployment Rate, 2000-2014

Job Vacancy Rate (Percent)

Unemployment Rate: 26 Weeks & Less (Percent)

Note: Job vacancy rate is defined as job openings as a percentage of the sum of job openings and total nonfarm payroll employment.

Figure 3.19: Calibrated Beveridge Curve, Actual vs. Simulated, 2000-2013

Job Vacancy Rate (Percent)

Unemployment Rate (Percent)

Note: Both vacancy and unemployment rates are percentages of the labor force ages 25 to 54.
Source: Bureau of Labor Statistics (Current Population Survey and Job Openings and Labor Turnover Survey); authors’ calculations.
Figure 3.20: Calibrated Beveridge Curve With 2006-2007 Average Labor Force Exit Rates, 2000-2013

Note: Both vacancy and unemployment rates are percentages of labor force ages 25 to 54.
Source: Bureau of Labor Statistics (Current Population Survey and Job Openings and Labor Turnover Survey); authors' calculations.
Figure 3.21: Forecast of Beveridge Curve if Labor Force Outflows Return to 2006 Level, 2000-2012 and 2013-2016 (Projected)

Note: Vacancy and unemployment rates are percentages of labor force ages 25 to 54.
Source: Bureau of Labor Statistics (Current Population Survey and Job Openings and Labor Turnover Survey); authors’ calculations.
Figure 3.22: Long-Term Share of Unemployment from Calibration, 2007-2012 and 2013-2016 (Projected)

Note: Long-term unemployment is defined as greater than 26 weeks.
Source: Bureau of Labor Statistics (Current Population Survey and Job Openings and Labor Turnover Survey); authors’ calculations.
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