The Labor Market Effects of Demand Shocks: 
Firm-Level Evidence from the Recovery Act*

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Abstract

How do firms respond to demand shocks? I approach this fundamental question from a novel perspective by leveraging two firm-level datasets that provide a uniquely detailed opportunity to examine how employers react to changes in the demand for their output. Specifically, this paper combines linked employer-employee administrative records for a subset of U.S. firms from ADP, LLC with a comprehensive database of transactions from the American Recovery and Reinvestment Act (ARRA), which appropriated $275 billion in purchases of goods and services during the Great Recession. Utilizing a matched difference-in-differences strategy as well as exploiting heterogeneity in both the timing and the magnitude of these purchases, I compare firms that received ARRA funds to a counterfactual sample of employers that were not directly connected to the Recovery Act. I find that companies which experienced these demand shocks responded by increasing both employment and wages relative to their counterparts. Furthermore, the magnitudes of these changes suggest that the labor supply to an individual firm is relatively inelastic, even in a deep recession, and provide evidence of monopsonistic wage-setting in U.S. labor markets.

JEL Classification: D22, E62, H32, J23, J31, J42
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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.\footnote{As 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.”} 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., 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 administrative 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.

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

\[^2\] 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.
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 2 provides institutional details pertaining to the design and implementation of the American Recovery and Reinvestment Act of 2009. Section 3 describes the primary data sources that form the basis of this study. Section 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 5 outlines the empirical strategy for identifying the responses of firms from which goods and services were purchased through the Recovery Act. In Section 6, I summarize the empirical results from this analysis. Finally, Section 7 discusses the implications of these findings within the particular contexts of monopsonistic labor markets and the multiplier effects of fiscal policy.

2 Institutional Details

The U.S. economy peaked in December 2007, and the unemployment rate gradually climbed as the financial crisis weighed on economic activity through the summer of 2008. The 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

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percent at an annual rate,\(^5\) and employers shed nearly 2 million payroll jobs.\(^6\) Moreover, with the target federal funds rate having already been slashed to a range of 0.00 to 0.25 percent in December 2008,\(^7\) 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).

On February 17, 2009, President Obama signed the American Recovery and Reinvestment Act (ARRA) 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).\(^8\) 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). The tax and entitlement

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\(^5\)U.S. Bureau of Economic Analysis. “Table 1.1.1.” *National Income and Product Accounts.*


\(^7\)Board of Governors of the Federal Reserve System. “Open Market Operations.”

\(^8\)In its most recent assessment, Congressional Budget Office (2015) determined that the Recovery Act will cost $836 billion through fiscal year 2019. Most of this increase in the budgetary impact of ARRA was 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).
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 2014).

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). 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 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.

3 Data

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 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

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9 These data have been maintained by the U.S. General Services Administration since the decommissioning of Recovery.gov in 2015.

10 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.
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 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.

3.2 ADP

This paper also utilizes 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.¹¹ 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

¹¹ADP, LLC. “Corporate Overview.” August 2018.
ADP payroll data have been shown to be highly correlated with the U.S. Bureau of Labor Statistics Current Employment Statistics. 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. In general, the payroll data uphold the anonymity of each client and worker. However, 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.

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


\[13\] Although there are some exceptions, these clients predominantly employ at least 50 workers.

\[14\] Dun and Bradstreet, Inc. “D-U-N-S Number Fact Sheet.”
as D-U-N-S number. Moreover, ADP utilizes the Data Universal Numbering System for additional information regarding firm characteristics.

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 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

\footnote{U.S. Bureau of Labor Statistics. "Distribution of Private Sector Firms by Size Class." Quarterly Census of Employment and Wages.}
the American Recovery and Reinvestment Act.

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 expected profits in accordance with the following expression:

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

(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:

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

(2)

Consequently, the firm’s decision in Equation 1 can now be expressed as:

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

(3)

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 the wage $W$ and the number of vacancies $V$ such that expected profits are maximized.

The solution to the firm’s profit maximization problem in Equation 3 entails the following first order conditions for the wage $w$ and the number of vacancies $V$, respectively:

$$
\Phi(V) = \frac{p - w - c}{p - w} \tag{4}
$$

$$
(p - w - c) \cdot \left\{ \frac{1}{2} + \frac{1}{2} \cdot \frac{N(w)}{\mathbb{E}[N]} \right\} - \frac{1}{2} \cdot c \cdot \left\{ \frac{J - \mathbb{E}[N]}{\mathbb{E}[N]} \right\} = \frac{1}{\varepsilon_N} \tag{5}
$$

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

$$
\varepsilon_N = \frac{w \cdot N'(w)}{N(w)} \tag{6}
$$

Intuitively, Equation 4 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 5 indicates that any disparity between the marginal revenue product 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 (i.e., $p = w$). Conversely, as the labor supply to an individual firm becomes less elastic (i.e., $\varepsilon_N \to 0$), the wage at that employer will 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 4 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., $N(w) \uparrow$) than their unaffected counterparts. In addition, Equation 5 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 (i.e., \( p \uparrow \)) by not only expanding employment but also raising the wage per worker (i.e., \( w \uparrow \)).

5 Empirical Strategy

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.\(^{16}\) 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.

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,\(^{16}\)

\(^{16}\)As explained in Section 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.
grants, and loans. Consequently, I supplement the previous approach with an event study design of the following form:

$$\ln(Y_{j,t}) = \alpha_j + \gamma_{s,t} + \sum_{k \in \mathcal{K}} \beta_k \cdot ARRA_j \cdot I_{j,t}^k + \varepsilon_{j,t} \quad (8)$$

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 7, this regression specification incorporates a sequence of indicator variables $I_{j,t}^k$ 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.

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:

$$\ln(Y_{j,t}) = \alpha_j + \gamma_{s,t} + \beta_1 \cdot Q_j \cdot ARRA_j \cdot Post_{j,t} + \beta_2 \cdot (1 - Q_j) \cdot ARRA_j \cdot Post_{j,t} + \varepsilon_{j,t} \quad (9)$$
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.

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 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 Ia-
cus 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 match-
ing, 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 distributions, the approximate bins for firm employment and earnings per worker are determined using Sturges’s rule.\textsuperscript{17} 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 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 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 two or more months, and about

\textsuperscript{17}Sturges (1926) proposes that \( n \) observations of normally distributed data be grouped into \( k \) bins where \( k = \lceil 1 + \log_2(n) \rceil \).
30 percent of firms experienced ARRA purchases in at least 13 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.

6 Empirical Results

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 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 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 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 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 5.3. According to Figure 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 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.\footnote{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.} 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 2.C), which corroborates the efficacy of the numerous constraints that governed the disbursement of Recovery Act funds (Section 2).

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 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 5). In fact, Figure 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 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 profit-maximizing response to a demand shock will entail changes to employment if the shift in production is lasting and persistent.

6.3 Earnings Per Worker

Next, I evaluate the impact of ARRA purchases on the 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 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 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 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.

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 7). An event study analysis reveals two important details regarding the dynamics of this effect on worker compensation (Figure 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 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 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 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 6. 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. In other words, this study finds meaningful evidence of monopsonistic wage-setting by employers in the United States.

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 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 6.A). The magnitude of
this response is compatible with the observed increases in both employment (Table 3) and
earnings (Table 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 6.B).

7 Implications

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 7). Notably, this increase
in the average wage per existing worker rivals the observed change in overall employee pay
(Table 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 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 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 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 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 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. “Unemployment Rate.” 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. Interestingly, even though employment appears to increase by a statistically similar magnitude at firms which acquired ARRA funds regardless of geographic location (Figure 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 cor-
responding to the bottom quartile of state unemployment rates (Figure 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 9). To put these results in the proper 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.

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 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-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 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.

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 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 wages 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.
References


9 Exhibits

Table 1

Federal Agencies by Total Amount of ARRA Contracts, Grants, and Loans

<table>
<thead>
<tr>
<th>Agency</th>
<th>Billions of Dollars</th>
</tr>
</thead>
<tbody>
<tr>
<td>Office of Elementary &amp; Secondary Education</td>
<td>66.208</td>
</tr>
<tr>
<td>Department of Energy</td>
<td>38.537</td>
</tr>
<tr>
<td>Federal Highway Administration</td>
<td>27.978</td>
</tr>
<tr>
<td>Office of Special Education &amp; Rehabilitative Services</td>
<td>13.678</td>
</tr>
<tr>
<td>Department of Housing &amp; Urban Development</td>
<td>11.123</td>
</tr>
<tr>
<td>National Institutes of Health</td>
<td>10.181</td>
</tr>
<tr>
<td>Federal Transit Administration</td>
<td>9.721</td>
</tr>
<tr>
<td>Federal Railroad Administration</td>
<td>9.583</td>
</tr>
<tr>
<td>Environmental Protection Agency</td>
<td>7.437</td>
</tr>
<tr>
<td>Rural Utilities Service</td>
<td>6.477</td>
</tr>
<tr>
<td>Public Buildings Service</td>
<td>5.826</td>
</tr>
<tr>
<td>Administration for Children &amp; Families</td>
<td>5.161</td>
</tr>
<tr>
<td>U.S. Army Corps of Engineers, Civil Program Financing</td>
<td>4.700</td>
</tr>
<tr>
<td>National Telecommunication &amp; Information Administration</td>
<td>4.516</td>
</tr>
<tr>
<td>Department of Labor</td>
<td>4.451</td>
</tr>
<tr>
<td>Department of Justice</td>
<td>4.220</td>
</tr>
<tr>
<td>National Science Foundation</td>
<td>2.978</td>
</tr>
<tr>
<td>Department of Education</td>
<td>2.969</td>
</tr>
<tr>
<td>Department of Health &amp; Human Services</td>
<td>2.402</td>
</tr>
<tr>
<td>Health Resources &amp; Services Administration</td>
<td>2.262</td>
</tr>
<tr>
<td>Federal Financing Bank</td>
<td>1.960</td>
</tr>
<tr>
<td>Department of the Army</td>
<td>1.916</td>
</tr>
<tr>
<td>Department of Defense, Excluding Military</td>
<td>1.595</td>
</tr>
<tr>
<td>Department of Veterans Affairs</td>
<td>1.545</td>
</tr>
<tr>
<td>Department of the Air Force</td>
<td>1.539</td>
</tr>
<tr>
<td>Rural Housing Service</td>
<td>1.433</td>
</tr>
<tr>
<td>Federal Aviation Administration</td>
<td>1.390</td>
</tr>
<tr>
<td>Department of the Navy</td>
<td>1.263</td>
</tr>
<tr>
<td>Forest Service</td>
<td>1.164</td>
</tr>
<tr>
<td>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>Total</td>
<td>274.705</td>
</tr>
</tbody>
</table>

Source: U.S. General Services Administration.
Table 2

<table>
<thead>
<tr>
<th>Summary Statistics</th>
<th>Full Sample</th>
<th>Matched Sample</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Firms Without ARRA Purchases</td>
<td>Firms With ARRA Purchases</td>
</tr>
<tr>
<td>Monthly Payroll (Thous. 2009$)</td>
<td>683.6</td>
<td>2,570.1</td>
</tr>
<tr>
<td>Employment</td>
<td>174.1</td>
<td>553.7</td>
</tr>
<tr>
<td>Hourly (%)</td>
<td>65.4</td>
<td>60.7</td>
</tr>
<tr>
<td>Salaried (%)</td>
<td>31.7</td>
<td>36.5</td>
</tr>
<tr>
<td>Other (%)</td>
<td>2.9</td>
<td>2.8</td>
</tr>
<tr>
<td>Female Employment (%)</td>
<td>46.2</td>
<td>42.5</td>
</tr>
<tr>
<td>Per Worker:</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Age (Years)</td>
<td>41.2</td>
<td>42.2</td>
</tr>
<tr>
<td>Monthly Earnings (2009$)</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>
<tr>
<td>Number of Firms</td>
<td>56,341</td>
<td>4,385</td>
</tr>
</tbody>
</table>

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

Source: ADP; U.S. General Services Administration; author's calculations.
<table>
<thead>
<tr>
<th>Role in ARRA Awards (%)</th>
<th>Industry (%)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Recipients</td>
<td>Agriculture &amp; Mining 0.2</td>
</tr>
<tr>
<td>Vendors</td>
<td>Utilities 0.3</td>
</tr>
<tr>
<td>Year of Initial ARRA Purchase (%)</td>
<td>Construction 6.4</td>
</tr>
<tr>
<td>2009</td>
<td>Manufacturing 10.8</td>
</tr>
<tr>
<td>2010</td>
<td>Wholesale Trade 7.3</td>
</tr>
<tr>
<td>2011-2013</td>
<td>Retail Trade 1.7</td>
</tr>
<tr>
<td>Year of Final ARRA Purchase (%)</td>
<td>Transportation &amp; Warehousing 1.1</td>
</tr>
<tr>
<td>2009</td>
<td>Information 2.9</td>
</tr>
<tr>
<td>2010</td>
<td>Finance &amp; Real Estate 2.9</td>
</tr>
<tr>
<td>2011-2013</td>
<td>Professional Services 20.9</td>
</tr>
<tr>
<td>Months With ARRA Purchases (%)</td>
<td>Management of Companies 0.2</td>
</tr>
<tr>
<td>1 Month</td>
<td>Administrative &amp; Support 3.0</td>
</tr>
<tr>
<td>2-6 Months</td>
<td>Educational Services 7.4</td>
</tr>
<tr>
<td>7-12 Months</td>
<td>Health Care 21.2</td>
</tr>
<tr>
<td>13-24 Months</td>
<td>Arts &amp; Entertainment 1.2</td>
</tr>
<tr>
<td>25 or More Months</td>
<td>Accommodation &amp; Food 0.4</td>
</tr>
<tr>
<td>ARRA Purchases Per Firm (Mil. 2009$)</td>
<td>Other Services 8.1</td>
</tr>
<tr>
<td>Total ARRA Purchases (Bil. 2009$)</td>
<td>Public Administration 4.1</td>
</tr>
</tbody>
</table>

Number of Firms 2,999

Source: ADP; U.S. General Services Administration; author's calculations.
Table 4

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

### Effect of ARRA Purchases

<table>
<thead>
<tr>
<th></th>
<th>Log Hours Per Worker</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>ARRA x Post</td>
<td>-0.0033</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.0036)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>ARRA x Post x Bottom 50% of Purchases</td>
<td>-0.0003</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.0046)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>ARRA x Post x Top 50% of Purchases</td>
<td>-0.0066</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.0051)</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

|                      |                      | p-Value for Test of Equality | 0.3234 |
|                      |                      | R-Squared                   | 0.8152 |
| Number of Observations | 339,499             |                              | 339,499 |
| Number of Firms       | 5,995                |                              | 5,995 |

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 6

<table>
<thead>
<tr>
<th>Effect of ARRA Purchases</th>
<th>Log Earnings Per Worker</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
</tr>
<tr>
<td>ARRA x Post</td>
<td>0.0048 *</td>
</tr>
<tr>
<td></td>
<td>(0.0027)</td>
</tr>
<tr>
<td>ARRA x Post x Bottom 50%</td>
<td></td>
</tr>
<tr>
<td>of Purchases</td>
<td></td>
</tr>
<tr>
<td>ARRA x Post x Top 50%</td>
<td></td>
</tr>
<tr>
<td>of Purchases</td>
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<tr>
<td>p-Value for Test of Equality</td>
<td>0.4193</td>
</tr>
<tr>
<td>R-Squared</td>
<td>0.9115</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>339,499</td>
</tr>
<tr>
<td>Number of Firms</td>
<td>5,995</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>
<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 x Post</td>
<td>0.0073 **</td>
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</tr>
<tr>
<td></td>
<td>(0.0036)</td>
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<tr>
<td>ARRA x Post x Bottom 50% of Purchases</td>
<td>0.0026</td>
<td>0.0046</td>
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<tr>
<td>ARRA x Post x Top 50% of Purchases</td>
<td>0.0126 **</td>
<td>0.0051</td>
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p-Value for Test of Equality 0.1178
R-Squared 0.9045
Number of Observations 339,499
Number of Firms 5,995

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 8

<table>
<thead>
<tr>
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<th>Log Payroll</th>
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<tbody>
<tr>
<td></td>
<td>(1)</td>
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<tr>
<td>ARRA x Post</td>
<td>0.0387 ***</td>
</tr>
<tr>
<td></td>
<td>(0.0065)</td>
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<tr>
<td>ARRA x Post x Bottom 50%</td>
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<tr>
<td>of Purchases</td>
<td></td>
</tr>
<tr>
<td>ARRA x Post x Top 50%</td>
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<td>of Purchases</td>
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<td>p-Value for Test of Equality</td>
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<tr>
<td>R-Squared</td>
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<td>Number of Observations</td>
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<tr>
<td>Number of Firms</td>
<td>5,995</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 9

<table>
<thead>
<tr>
<th>Effect of ARRA Purchases</th>
<th>Log Employment</th>
<th>Log Wage Per Worker</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>ARRA x Post x Highest 3 Quartiles of State Unemployment</td>
<td>0.0340 ***</td>
<td>0.0052</td>
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<tr>
<td></td>
<td>(0.0064)</td>
<td>(0.0037)</td>
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<tr>
<td>ARRA x Post x Bottom Quartile of State Unemployment</td>
<td>0.0438 ***</td>
<td>0.0246 **</td>
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<tr>
<td></td>
<td>(0.0165)</td>
<td>(0.0107)</td>
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<td>p-Value for Test of Equality</td>
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<tr>
<td>R-Squared</td>
<td>0.9597</td>
<td>0.9045</td>
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<tr>
<td>Number of Observations</td>
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<td>339,499</td>
</tr>
<tr>
<td>Number of Firms</td>
<td>5,995</td>
<td>5,995</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

American Recovery and Reinvestment Act by Category

Billions of Dollars

Figure 2.A

Event Study Estimates

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% confidence interval. Month prior to initial ARRA purchase set to zero.

Source: ADP; U.S. General Services Administration; author's calculations.
Figure 2.B

Event Study Estimates 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% confidence interval. Month prior to initial ARRA purchase set to zero.

Source: ADP; U.S. General Services Administration; author's calculations.
Figure 2.C

Event Study Estimates by ARRA Role

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% confidence interval. Month prior to initial ARRA purchase set to zero.

Source: ADP; U.S. General Services Administration; author's calculations.
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% confidence interval. Month prior to initial ARRA purchase set to zero.

Source: ADP; U.S. General Services Administration; author's calculations.
Figure 3.B

Event Study Estimates by Total ARRA Purchases

Log Hours Per Worker (Total 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% confidence interval. Month prior to initial ARRA purchase set to zero.

Source: ADP; U.S. General Services Administration; author’s calculations.
Figure 4.A

Event Study Estimates

Log Earnings Per Worker (2009$ 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% confidence interval. Month prior to initial ARRA purchase set to zero.

Source: ADP; U.S. General Services Administration; author's calculations.
Figure 4.B

Event Study Estimates by Total ARRA Purchases

Log Earnings Per Worker (2009$ Per Month)

Top 50% of ARRA Purchases

Bottom 50% of 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% confidence interval. Month prior to initial ARRA purchase set to zero.
Source: ADP; U.S. General Services Administration; author’s calculations.
Figure 5.A

Event Study Estimates

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% confidence interval. Month prior to initial ARRA purchase set to zero.

Source: ADP; U.S. General Services Administration; author's calculations.
Figure 5.B

**Event Study Estimates by Total ARRA Purchases**

Log Wage Per Worker (2009$ Per Hour)

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% confidence interval. Month prior to initial ARRA purchase set to zero.

Source: ADP; U.S. General Services Administration; author’s calculations.
Figure 6.A

Event Study Estimates

Log Payroll (2009$ 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% confidence interval. Month prior to initial ARRA purchase set to zero.

Source: ADP; U.S. General Services Administration; author's calculations.
Figure 6.B

Event Study Estimates 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% confidence interval. Month prior to initial ARRA purchase set to zero.

Source: ADP; U.S. General Services Administration; author’s calculations.
Figure 7

Event Study Estimates

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% confidence interval. Month prior to initial ARRA purchase set to zero.

Source: ADP; U.S. General Services Administration; author's calculations.
Figure 8

Difference-in-Differences Estimates

Elasticity of Labor Supply

Note: Regressions of log employment and log wage per worker include firm fixed effects and industry-by-time effects. Bars reflect bootstrapped 95% 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 9.A

Event Study Estimates by State Unemployment

Log Employment

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% confidence intervals. Month prior to initial ARRA purchase set to zero.
Figure 9.B

Event Study Estimates 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% confidence intervals. Month prior to initial ARRA purchase set to zero.