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Joonkyu Choi, Veronika Penciakova, Felipe Saffie

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Political Connections, Allocation of Stimulus Spending, and the Jobs Multiplier*

Joonkyu Choi[†] Veronika Penciakova[‡] Felipe Saffie[§]

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Abstract

Using American Recovery and Reinvestment Act (ARRA) data, we show that firms lever their political connections to win stimulus grants and that public expenditure channeled through politically connected firms hinders job creation. We build a unique database that links information on campaign contributions, state legislative elections, firm characteristics, and ARRA grant allocation. Using exogenous variation in political connections based on ex-post close elections held before ARRA, we causally show that politically connected firms are 38 percent more likely to secure a grant. Based on an instrumental variable approach, we also establish that a one standard deviation increase in the share of politically connected ARRA spending lowers the number of jobs created per $1 million spent by 7.1 jobs. Therefore, the impact of fiscal stimulus is not only determined by how much is spent, but also by how the expenditure is allocated across recipients.

JEL Codes: D22, D72, E62, H57, P16
Keywords: Campaign Finance, State Grants, Public Expenditure Allocation, American Recovery and Reinvestment Act

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[†]Federal Reserve Board of Governors; joonkyu.choi@frb.gov

[‡]Federal Reserve Bank of Atlanta; veronika.penciakova@atl.frb.org

[§]University of Virginia, Darden School of Business ; saffieF@darden.virginia.edu

1 Introduction

During severe economic downturns, aggressive fiscal stimulus measures are implemented to stabilize the economy. Over the past decade, the world has suffered two of the worst economic downturns since the Great Depression. In the United States, both episodes—the Great Recession and the Global Pandemic—triggered fiscal packages in excess of 5 percent of GDP. The American Recovery and Reinvestment Act (ARRA) was enacted in the midst of the Great Recession, and over one-fourth of the funds were channeled directly to firms with the primary goal of saving and creating jobs. These stimulus funds were sizable and valuable to firms, with the average grant awarded exceeding $500,000. Recent literature has focused on identifying the employment effects of fiscal stimulus, namely the jobs multiplier—the number of jobs created per $1 million spent—while largely abstracting from whether the allocation of resources across recipients also affects job creation.

With hundreds of thousands of dollars on the line, firms may have incentive to exert political influence to secure resources, and such influence may subsequently impact the job creation effect of fiscal interventions. Are firms successful in influencing the allocation of stimulus spending? Does the allocation of these funds across firms matter for employment outcomes at the state level? This paper provides empirical answers to both questions. First, we find that firms' campaign contributions to state politicians before the enactment of ARRA have a positive and significant impact on the probability of winning ARRA grants. Second, controlling for state-level ARRA spending, we find that channeling a higher fraction of ARRA grants through politically connected firms lowers the state-level jobs multiplier. Therefore, it is not only the size of the fiscal stimulus that matters for employment, but also how this stimulus is allocated across firms.

ARRA is an ideal laboratory to study the effect of firms' political connections on the allocation of fiscal stimulus. In the years leading up to ARRA, private-sector businesses accounted for at least 28 percent of campaign contributions in state legislative elections, and some firms formed political connections to state politicians who would later be charged with disbursing ARRA resources. In the first two years of ARRA (2009–2010), over two-thirds of the funds were distributed to individual states through various grant programs, giving state governments near full discretion in allocating grants to firms.[1] In addition, ARRA featured a high degree of transparency that made information—unavailable for previous fiscal stimulus programs—accessible to the general public. The law required government agencies and business recipients to publicly disclose detailed records about grants and contracts.[2] Consequently, we are able to build a novel data set that combines micro data on government grants, firms' campaign contributions, election outcomes, and firm characteristics. Using this new data set, we first exploit variation in political connections across firms within states to study the link between political connections and the allocation of fiscal stimulus. We then use cross-state variation in the share of stimulus funds channeled through politically connected firms to evaluate the impact of political connections on the state-level jobs multiplier.

To identify the causal effect of firms' political connections on the allocation of grants, we exploit ex-post close elections as a source of random variation. A key assumption is that winning by a small margin is almost random for the top two candidates (Lee, 2008; Akey, 2015). Using this random variation allows us to overcome the endogeneity of unobserved factors driving both firms' decision to support politicians and the probability of winning ARRA grants. We focus on a group of firms that made campaign contributions to politicians running for office in close elections, and compare the ARRA grant outcomes of firms that supported more election winners (treated) to those of firms that supported fewer or no winners (control). Because the decision to support more candidates in close elections could potentially be correlated with the probability of winning ARRA grants, it is important to compare firms that supported the same number of candidates in close elections. We therefore match treated firms to their counterparts on the number of candidates supported in close elections, as well as state and industry.

We find that firms that contribute to winning candidates are 38 percent more likely to secure an ARRA grant and receive 10 percent larger amount of funds. We use institutional features of ARRA to design placebo tests that provide further support for our identification strategy. Our results are also robust to using various alternative empirical specifications. While the matching procedure helps us to achieve a balanced

[1] See Conley and Dupor (2013) and Leduc and Wilson (2017).

[2] Recipients were required to report on awards, vendors, spending, and project status. As a result, we can identify the ultimate vendors associated with state grants. All this information is recorded in recipient report data that was made publicly available in Recovery.gov, a now-defunct website. We are grateful to Bill Dupor for making these data available on his personal webpage.

sample and enhance precision of our estimates (Iacus et al., 2012), we show that our results are robust to using an unmatched sample. Our results are also robust to using an alternative threshold of vote shares in defining close elections and to changing the empirical specification to a regression discontinuity design.

To investigate whether grant allocation across firms matters from an aggregate and policy perspective, we broaden the scope of our analysis from firms to states. Because close elections account for only a small share of economic activities and ARRA spending, we modify our empirical strategy. Specifically, we adapt the empirical framework used in the jobs multiplier literature by introducing the fraction of ARRA spending disbursed to politically connected firms as a new explanatory variable. We use the framework to identify the effect of allocating a larger share of resources to politically connected firms on the state-level jobs multiplier.

We must account for two sources of endogeneity in order to make a causal interpretation of our estimates. First, we address endogeneity arising from the correlation between local needs and the size of stimulus resources by instrumenting for ARRA funding with predicted Department of Transportation (DOT) spending based on pre-existing allocation formulas (Wilson (2012)). Second, we address the potential correlation between firms' ability and willingness to exert political influence and local economic conditions by introducing a new instrumental variable. We instrument for the opportunity firms have to form political connections with an indicator of whether the state prohibited corporate campaign contributions in 2002. Our identifying assumption is that, conditional on states' economic and political environment at the onset of the recession, both instruments are unlikely to be correlated with unobserved factors that affected states' speed of recovery.

We find that the enactment of ARRA created or saved on average 27.2 jobs per million dollars spent, but raising the share of the spending channeled through politically connected firms by one standard deviation lowers this multiplier by 7.1 jobs. Our results hold after accounting for states' industrial composition and firm age and size distribution, the geography of the housing bust, and anticipation effects, as well as several alternative measures of political environment and union influence. We also show that employment growth in states with a high share of politically connected spending is slower during the first three years of recovery.

In a nutshell, the allocative distortion caused by political connections is sizable. Although only 6 percent of grant recipients contribute to campaign finance during state legislative elections, they account for 21 percent of total ARRA grants.[3] We show, for the first time, that the allocation of fiscal stimulus has a persistent impact on the local jobs multiplier. Thus, when analyzing fiscal stimulus policy, it is important to take into consideration the political process by which funds are allocated to firms.

Related Literature This paper bridges the literatures studying firms' political activities and the employment effect of fiscal stimulus.

Firms exert political influence over governmental decisions through a variety of channels. For instance, firms can employ current or former politicians (Bunkanwanicha and Wiwattanakantang, 2008; Akcigit et al., 2018), use lobbying (Kerr et al., 2014; Kang, 2016; Hassan et al., 2019) or campaign contributions (Faccio, 2004; Claessens et al., 2008; Cooper et al., 2010; Akey, 2015) to affect the design and implementation of public policy in their favor. The literature has documented that politically connected firms can increase their value through various channels, including tax benefits (Arayavechkit et al., 2018), less regulation (Fisman and Wang, 2015), more favorable terms for government loans (Khwaja and Mian, 2005), and government bailouts (Faccio et al., 2006).

More closely related to our analysis, the literature has studied how firms lever their political connections to capture government spending. There is more evidence for developing countries than for advanced economies.[4] In the context of the United States, Duchin and Sosyura (2012) find that politically connected firms were more likely to receive Troubled Asset Relief Program (TARP) funds and that these firms subsequently had lower investment efficiency. Related to our work, Goldman et al. (2013) find that firms with a board of directors connected to the winning party in the 1994 federal elections received significantly more procurement contracts in the subsequent years. Brogaard et al. (2020) use sudden deaths and resignations of politicians to document that connected firms are able to initially bid lower prices and favorably renegotiate terms of procurement contracts.

At a more aggregate level, the literature has found mixed evidence on the importance of politics for the disbursement of stimulus funds during the Great Recession.

[3]On average, across states, less than one percent of all firms contribute to state legislative elections.

[4]For developing countries see, for example, the studies for Brazil (Colonnelli and Prem, 2017), Czech Republic (Titl and Geys, 2019), India (Lehne et al., 2018), Lithuania (Baltrunaite, 2017), and Russia (Mironov and Zhuravskaya, 2016). For advanced economies see, for example, studies for Denmark (Amore and Bennedsen, 2013) and South Korea (Schoenherr, 2018).

Leduc and Wilson (2017) find that states with more political contributions from the public works sector to the governor and state legislator spent a higher fraction of the ARRA highway funds they received from the Federal Highway Administration. Boone et al. (2014) document that Congressional districts represented by members in positions of influence did not receive more ARRA funds, and that funds were not directed to swing districts where the money might help secure an electoral advantage.

The Great Recession also revitalized the literature on the employment effect of fiscal stimulus. Most empirical studies exploit geographic variation in fiscal spending to estimate the aggregate effects of policy. Chodorow-Reich et al. (2012) focus on the state budget relief provided by Medicaid grants and Wilson (2012), Conley and Dupor (2013), and Leduc and Wilson (2013) use the state allocation of highway expenditure. Meanwhile, Dube et al. (2018) focus on within-state, cross-county variation in ARRA expenditure, and Mian and Sufi (2012) exploit cross-city variation in ex-ante exposure to the 2009 "Cash for Clunkers" program. The literature often draws on institutional features of ARRA for identification purposes. Barrot and Nanda (2020) study how the increase in the celerity of government payments contributed to job creation during ARRA, and Dupor and Mehkari (2016) use formulaic ARRA spending by federal agencies as an instrument to separate the effects of the stimulus on wages and employment.[5]

We make three contributions to the literature. First, while prior studies of corporate political activities in the United States have focused on federal-level campaign contributions and lobbying of large, publicly listed companies, we provide evidence on the political engagement and influence at the sub-national level of all firms, including small and privately-held firms. Second, we causally establish that political connections of firms to state politicians has an impact on the allocation of stimulus grants.[6] Third, we causally show that disbursing a higher share of stimulus to

[5]Beyond the analysis of ARRA, Nakamura and Steinsson (2014) and Dupor and Guerrero (2017) exploit the geographic variation on military expenditure, and Ramey and Zubairy (2018) use quarterly time series data to perform local projection regressions and study the cyclical properties of fiscal multipliers. Internationally, Acconcia et al. (2014) estimate the fiscal multiplier using a quasi-experiment arising from provincial spending cuts in Italy following the expulsion of mafia-connected city council members. A more comprehensive review of the recent fiscal & employment multiplier literature can be found in Chodorow-Reich (2019).

[6]In this regard, our work is complementary to Boone et al. (2014), who find that the U.S. congressional representation did not have an impact on the regional allocation of ARRA spending. Our work identifies the level of connection (firm-state politician) and allocation (firm-state) at which political factors are indeed important.

politically connected firms lowers the jobs multiplier.

The remainder of this paper is structured as follows. Section 2 describes the institutional features of the American Recovery and Reinvestment Act and the data sources used in our analysis. Section 3 studies how campaign contributions to state politicians determine the allocation of ARRA grants. Section 4 studies whether the distribution of ARRA resources across firms affects the state-level jobs multiplier. Section 5 concludes.

2 Institutional Context and Data

2.1 The American Recovery and Reinvestment Act

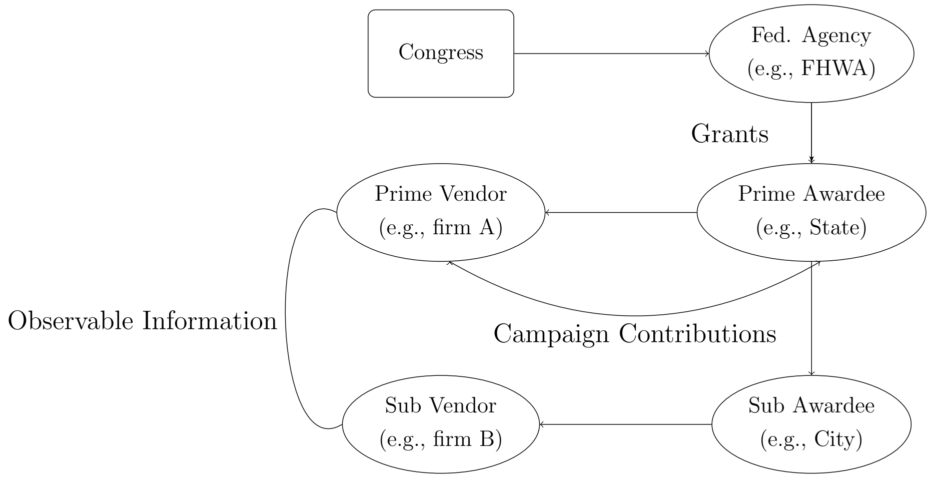
ARRA was an economic stimulus package that was designed to invigorate a rapidly declining economy during the Great Recession. The bill was enacted into law in February 2009, and at roughly $800 billion, it was, at the time, one of the largest fiscal stimulus packages in United States history. The primary objective of ARRA was to create and save jobs.[7] Stimulus funds were distributed in various forms including unemployment benefit extensions (Hagedorn et al., 2013; Chodorow-Reich et al., 2019), fiscal aid to state governments (Chodorow-Reich et al., 2012), and procurement contracts and grants awarded to private-sector businesses.

The focus of this study is ARRA grants awarded to firms. Federal grant spending is often channeled through subnational governments, and such intermediation creates room for influence to be exerted over local politicians in the allocation process. For example, consider ARRA highway infrastructure investment projects. The Federal Highway Administration (FHWA) first appropriates ARRA funds to states, mostly through preexisting highway grant programs. State governments, the prime grant awardees, then submit the selection of projects and the private businesses that will perform the task—referred to as prime vendors—to the FHWA for approval. When necessary, the projects involve participation of local governments (e.g., county or city) as sub grant awardees, who then channel the funds to firms, or sub vendors. Because it was critical to rapidly disburse funds, virtually all ARRA highway projects were approved by the FHWA, and thus states had near full discretion in selecting prime

[7] Other objectives were to provide temporary relief to individuals in economic hardship and invest in public infrastructure, education, health, and renewable energy.

vendors (Leduc and Wilson, 2017). Figure 1 summarizes the fund distribution process.

Figure 1: Allocation of Grants and Contracts during ARRA



Two features of the distribution process are worth highlighting. First, state officials directly influence the allocation of ARRA grants to firms in their states via selection of prime vendors. Therefore, political connections between businesses and state legislators formed through campaign contributions in earlier elections could affect the distribution of funds. Second, the institutional design provides opportunities for placebo tests. Campaign contributions to state-level politicians in a state should only help a firm win grants as a prime vendor (not as a sub vendor) in that particular state and not in any other state.

A key attribute of ARRA is its transparency. Section 1512(c) of the Recovery Act established a stringent reporting requirement that applied to all ARRA funding recipients. In particular, grant recipients were required to report numerous elements of their awards on a regular basis including the dollar amount, place of performance, project status, and most importantly, the vendors associated with the project. The last element is typically not available in other federal grant data sets. Because we observe the identity of the vendors, we can obtain information about their characteristics and political activities by linking the ARRA grant data with other data sets.

2.2 Data Sources

We obtain information on firm characteristics from the National Establishment Time Series (NETS). NETS is a longitudinal data set of millions of businesses in the United States that contains establishment-level information including number of employees, location, industry, and business ownership structure. NETS is maintained by Walls & Associates, and its data source is the Dun and Bradstreet's (D&B) Marketing Information file. It is known that with appropriate trimming of micro enterprises, NETS becomes a representative sample of businesses with paid employees in the United States, and its cross-sectional distributions are consistent with those of official government data sets (Barnatchez et al., 2017). We use NETS to measure firm characteristics such as size, industry, and headquarter location.

Our data on ARRA grants comes from the Recovery Act Recipient Report. ARRA required that recipients of contracts and grants report detailed information about their awards, including the list of prime and sub awardees, awarding agency, awarded amount, place of performance, and vendors. The recipient report data provides the D&B identifier of grant awardees and name and zip code of vendors that perform the tasks. We first merge the recipient report data and NETS based on the D&B identifiers. Records that remain unmatched are then linked using probabilistic name and location matching.

To measure political connections of firms to state legislators, we use campaign finance contribution data from the National Institute of Money in Politics (NIMP). NIMP is a nonprofit organization that compiles public records on campaign finance at the federal and state level. We use probabilistic name and address matching to construct firm-level information on the amount of campaign contributions made by firms to politicians running for office in state legislative elections.[8] Because most ARRA grants were awarded in 2009 and 2010, we focus on standard elections for state legislative positions held between 2006 and 2008, with terms lasting until at least 2010. Terms for state legislators vary by state, with most lasting between two and four years. In our sample, there are about 5,000 elections in 2006 and 2008 and 500 elections in 2007. We obtain outcomes of these elections from the State Legislative Election Results Database compiled by Klarner et al. (2013).

[8] Appendix A.1 provides additional details on the matching procedures involved in constructing our data set.

2.3 Firms in State Politics and Stimulus Spending

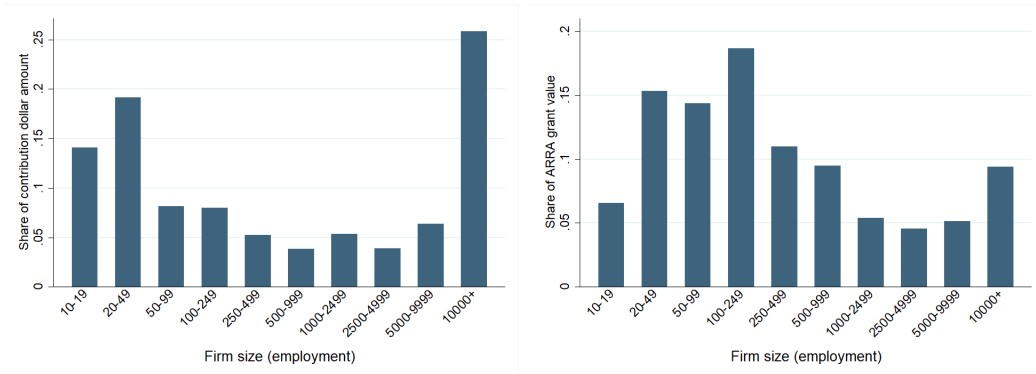
Our resulting data set reveals three facts pertinent to our analysis of how firms exert political influence over the allocation of fiscal stimulus spending.

First, private-sector businesses account for at least 16 percent of all state campaign contributions and 28 percent of their dollar amount. The remaining contributions are made by individuals, unions, and associations. The large share of firm campaign contributions may seem counter intuitive, as firms are perceived to primarily engage in political activities through business associations. However, business associations speak for industries and coalitions, not individual businesses. They are therefore more useful in influencing regulatory change than in helping firms secure government grants. By linking campaign finance data with NETS for the first time, we are able to document the political engagement by firms that enables them to create connections to local politicians.

Second, small- and medium-sized enterprises actively engage in local elections via campaign contributions. The left panel of Figure 2 depicts the dollar share of campaign contributions by firm size groups, measured by firm employment. Firms with 500 or fewer employees account for 55 percent of total firm contributions, and those with fewer than 50 employees account for 33 percent. This finding is in contrast to the conventional belief that corporate political activities are mostly done by large firms. While this is true in the case of federal-level lobbying, which is associated with large fixed costs and entry barriers (Kerr et al., 2014), campaign contributions to local politicians appear to be much more accessible to small businesses. Our data therefore highlights both the importance of state-level political engagement by SMEs, and the advantage of using a nationally representative data set, such as NETS, over data that contains only publicly listed firms (e.g., Compustat).

Third, businesses, and SMEs in particular, play an important role in fiscal stimulus. The recipient report data reveal that 26 percent of total ARRA obligations made in the first two years of fiscal stimulus were channeled to firms via contracts and grants. Grant-winning firms were awarded, on average, 1.8 grants, and the average size of each grant was over $500,000. As the right panel of Figure 2 documents, these grants were channeled primarily to SMEs. In fact, 66 percent of ARRA grant spending to prime vendors went to firms with 500 or fewer employees, with 22 percent channeled to firms with fewer than 50 employees. The remainder of this paper investigates the connection between this political engagement and fiscal stimulus, as well as its aggregate implications for the jobs multiplier.

Figure 2: State campaign contribution & grant shares by firm size



Notes: Left figure plots the dollar share of campaign finance contributions, and right figure plots the dollar share of ARRA grants awarded by firm size group. Firm size is measured by number of employees in 2008 and ARRA grant awards are measured by dollar amount obligated to firms as prime vendors. Following Barnatchez et al. (2017), we exclude firms with less than 10 employees from calculation as this group is over-represented in NETS.

3 Political Connections and Grant Allocation

3.1 Identification Strategy

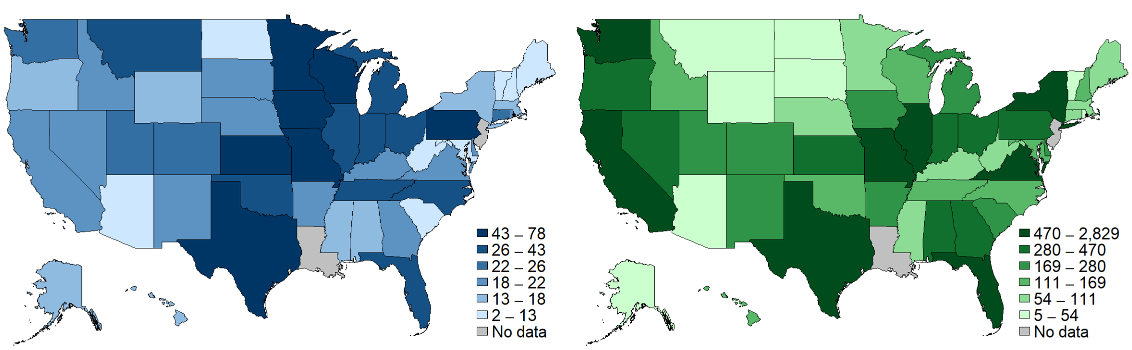
In this section, we empirically investigate the effect of firms' political connections to state legislators—as measured by campaign contributions in state legislative elections—on ARRA grant allocation. Without an appropriate identification strategy, comparing grant outcomes of firms with strong political connections to those of firms with weak or no connections would be subject to endogeneity bias. For example, unobserved firm characteristics (e.g., access to insider political information) could be simultaneously driving firms' decision to make donations to politicians, ability to predict the winners, and attainment of government grants.

An ideal empirical approach to studying the effect of political connections on grant allocation would be to take a group of firms connected to politicians running for office, randomly assign election victories, and observe how grants are allocated to firms after the election. To mimic the ideal experiment, we analyze the grant outcomes of firms that contribute to candidates running for office in close elections. Our identifying assumption is that the outcome of a close election is difficult to predict and largely determined by random factors uncorrelated with grant outcomes. Lee (2008) shows that when candidates cannot manipulate the election outcome, the event of winning by a small margin (i.e., a vote share close to the 50 percent threshold) is virtually random for the top two candidates. We follow the literature in defining a close election as one won by a 5 percent or smaller margin of victory, where the margin of victory is defined as the vote share of the election winner minus that of the second-place candidate (Lee, 2008; Akey, 2015; Do et al., 2015).[9]

3.2 Descriptive Statistics

We focus on a subsample of elections with legislative terms lasting until at least 2010. Our close election sample encompasses 629 elections across 48 states during the 2006, 2007, and 2008 election cycles.[10] Figure 3 shows the number of candidates in these elections and the number of firms that supported them. There is ample variation across states in the number of candidates, and close elections are not concentrated in swing states or a specific region. The correlation between the number of candidates in close elections and the number of firms supporting those candidates is small (0.26) because the latter is also a function of the economic size of each state.

Figure 3: Number of candidates (L) and firms (R) associated with close elections



Source: NETS, ICPSR State Legislative Election Returns Databse, Authors' own calculation.

Notes: The figures plot the distribution of candidates (left) and the firms supporting the candidates (right) who were running for office in close elections during the 2006, 2007, and 2008 election cycles.

[9] This definition implies that the winner receives 52.5 percent or less of the total vote in a close election with two candidates.

[10] Figure A.1 in Appendix A.2 depicts the distribution of the margins of victory in our data. The empirical density function is decreasing in the margin of victory, which is consistent with what has been previously documented in other election settings (Akey, 2015; Akcigit et al., 2018). Table A.1 in Appendix A.2 reports the distribution of the number of candidates firms support in close elections in each state.

3.3 Empirical Specification

We build a treatment-control framework to estimate the effect of gaining political connections on grant outcomes. Because a firm can secure political connections to more than one legislator in a state, firm political connections vary at the firm-state-politician level.[11] Meanwhile, the outcome variable of interest that indicates whether a firm receives an ARRA grant in a state is defined at the firm-state level. Therefore, we aggregate firm political connections to the firm-state level. Specifically, we construct $Frac(Win)_{i,s}$ as the number of close election winners supported by firm i in state s, divided by the number of close election candidates supported by firm i in state s. That is,

$$Frac(Win)_{i,s} = \frac{\sum_j (Supported_{i,s,j} \times Win_{s,j})}{\sum_j Supported_{i,s,j}}$$

where $Supported_{i,s,j}$ takes a value of one if firm i donated to candidate j's campaign in a close election in state s and zero otherwise. $Win_{s,j}$ takes the value of one if candidate j won the close election in state s and zero otherwise. Then, we define a treatment dummy, $Treat_{is}$, that takes a value of one if $Frac(Win)_{is}$ is greater than or equal to 0.5 and zero otherwise. Our objective is to compare the grant outcomes of firms that randomly gained large political connections in state s with those of less lucky firms in the same state. For example, if a firm supported one candidate in a close election, $Treat_{is}$ is 1 if that candidate won the election and zero otherwise. If the firm supported two candidates in close elections, $Treat_{is}$ is 1 if one or both of the candidates won their election and zero if neither did.

For an appropriate comparison between treated and control groups, we compare firms in the same industry that made campaign contributions to the same number of candidates in the same state. Imagine if we were to compare a firm that supported 20 candidates with one that supported only 2. We would expect that on average the firm supporting 20 candidates would gain more connections, and that unobserved

[11] On average, there were 13 close elections in a state and the average firm made campaign contributions to politicians in 2.4 close elections. Note that only in 3.7 percent of firm-election pairs, firms hedge the election outcome risk by supporting both top candidates in the same election. Low degrees of hedging are also found in other election settings (see Akcigit et al. (2018)). We drop these cases from our analysis, but results are robust to keeping them in the sample. Table A.1 in Appendix A.2 shows the full distribution of the number of politicians a firm supports in close elections in a state.

factors which drove the firm to support more candidates may be correlated with subsequent grant outcomes. We also need to compare a firm with strong connections in state A to a firm with weak connections in state A, not in state B. Because the amount of ARRA spending received and the level of engagement in political activities systematically differ across industries, it is also appropriate to account for the industry of the firms. Accordingly, for every treated firm-state observation ($Treat_{is}$ equal to one), we find non-treated firm-state pairs ($Treat_{is}$ equal to zero) that match on state s, the number of candidates the firm supported in close elections in state s, and the industry of the firm. We use one-to-many matching with replacement and matching weights constructed based on Iacus et al. (2012). In Table A.2 in Appendix B, we show that treated and control groups in the matched sample are not statistically different in unmatched characteristics.

We compare treated and control firms by running the following regression:

$$Y_{i,s} = \beta_0 + \beta_1 Treat_{i,s} + \gamma' X_{i,s} + \epsilon_{i,s} \tag{1}$$

$Y_{i,s}$ is an indicator variable that takes the value of one if firm i receives a grant in state s and zero otherwise. $Treat_{i,s}$ is the treatment dummy defined above and $X_{i,s}$ is a vector of control variables. Under our identifying assumption, $Treat_{i,s}$ is uncorrelated with the error term. Nonetheless, we control for several key firm characteristics that could potentially be correlated with the firms' ability to win government grants and predict election winners. Controlling for these characteristics enhances the precision of estimates and reduces potential endogeneity bias, if any exists.

We control for firm size, as measured by the number of employees. Barnatchez et al. (2017) conduct an extensive analysis of the properties of the employment distribution in NETS and suggest using employment as a categorical variable rather than a continuous one. We follow their suggestion.[12] We also control for an indicator variable $Young_{i,s}$ that takes the value of 1 if firm i is 10 or younger and zero otherwise. Both firm age and size are measured as of 2008. Firms headquartered in a state may be more likely to receive grants from that state and potentially have a better understanding of its political climate. Thus, we control for an indicator $Instate_{i,s}$ that

[12]Specifically, we define firm employment categories as the following: less than 4, 5-9, 10-19, 20-49, 50-99, 100-249, 250-499, 500-999, 1000-2499, 2500-4999, 5000-9999, and 10000 or more. We focus on businesses that hire employees since we later analyze the job creation effect of stimulus grants. Following Neumark et al. (2005), we exclude firms with one employee.

takes the value of 1 if firm i is headquartered in state s and zero otherwise. We also control for the total number of candidates firm i supported in state s, $TotalCand_{i,s}$, including but not limited to those in close elections. This variable captures the overall engagement of firm i in politics in state s and is measured in logs. Finally, we control for the number of candidates firm i supported in close elections in state s, denoted as $NumCandCE_{i,s}$, and include industry by state fixed effects.

3.4 Results

Table 1 shows that gaining political connections has a positive and statistically significant effect on the probability of winning the grant.[13] We introduce the control variables sequentially moving from Column (1) to (3). Column (3) is our main specification and shows that a stronger political connection increases the chances of winning a grant by 0.69 percentage points. The final column represents the main specification estimated on a sample in which a close election is defined based on 3 percent margin of victory.[14] While standard errors are larger than their counterparts in Column (3) due to a smaller sample size, we find that the baseline results are robust to using a tighter margin of victory in defining a close election.

To interpret the estimated effect in Column (3), it is important to note that grant allocation is heavily concentrated to a small share of firms.[15] Among the control group, the mean probability of winning a grant is 1.8 percent, implying that the estimated marginal treatment effect is a 38 percent increase in the probability of winning a grant.

[13] We estimate the regression equations after multiplying $Y_{i,s}$ by 100 for ease of interpretation.

[14] Equivalently, the winner has won with 51.5 percent or less vote share.

[15] The mean probability of winning a grant in our sample is 2.1 percent. Cox et al. (2020) documents a similar evidence for a high concentration of federal procurement contracts to a small fraction of firms.

Table 1: Treatment Effect on Winning a Grant

	(1) Win	(2) Win	(3) Win	(4) Win
Treat	0.634***	0.678***	0.686***	0.760**
	(0.083)	(0.128)	(0.079)	(0.331)
Young			-0.593*	-0.843*
			(0.337)	(0.467)
Instate			1.515***	2.198***
			(0.410)	(0.677)
TotalCand			0.142	0.196
			(0.143)	(0.231)
NAICS4 X State FE	No	Yes	Yes	Yes
NumCandCE FE	No	Yes	Yes	Yes
Emp Category FE	No	No	Yes	Yes
Victory Margin	5%	5%	5%	3%
Obs.	6187	6143	6143	4034
R-sq	0.00	0.27	0.30	0.35

Notes: Unit of analysis is firm $\times$ state. $Treat$ indicates whether 50% or more of candidates a firm supported in close elections won the election in a state, $Young$ indicates whether the firm is 10 years old or younger, $Instate$ indicates the state in which a firm is headquartered, and $TotalCand$ is the log number of candidates a firm supported in a state. Win indicates whether a firm received at least one grant from a given state as a prime vendor. The mean probability of winning a grant is 2.1 percent. We include 4-digit NAICS, state, # of candidates supported in close elections, and employment category FE. ***, **, and * indicate significance at the 1%, 5%, and 10% significance levels, respectively. SEs are clustered at the state and industry level.

In Table 2, we investigate whether stronger political connections have an effect on winning larger or more grants. Val and Num are the total dollar value and the total number of grants a firm receives in a given state, respectively. These variables are highly positively skewed and it is common to use their log values in regressions. However, because the log is not defined at zero, log transformation results in a conditional-on-positive selection bias even when the treatment is random (Angrist and Pischke, 2008, p.94-102). Therefore, we present the results applying an inverse hyperbolic sine transformation on the dependent variable, which we denote as IHS (column 1 and 2), as well as a $\log(1 + x)$ transformation (column 3 and 4).[16] We estimate that grant dollars received and the number of grants awarded increase significantly by nearly 10

[16] The IHS of x is defined as $\text{IHS}(x) = \ln(x+\sqrt{1+x^2})$. $\text{IHS}(x)$ is approximately equal to $\ln(x)$ shifted by a constant for $x > 0$, while it is well-defined at zero ($\text{IHS}(0) = 0$). Therefore, regression coefficients under the IHS transformation can be interpreted in the same way as in log transformation, and one can include zeros in outcome values and thus avoid conditional-on-positive selection bias. For more details on the IHS transformation, see Burbidge et al. (1988) and Pence (2006).

percentage points and 1 percentage point, respectively. To understand the economic magnitude of these estimates, Appendix A.3 reports the average windfall of a dollar contributed during a close election. This calculation combines the probability of the candidate winning, the effect of contributions on the expected number of contracts, and its effect on the average size of those contracts. Note that this is an unexpected windfall at the moment of contributing, as the firms do not anticipate ARRA. The economic magnitude is indeed relevant, as every dollar contributed in a close election generates, on average, $2.46 in grants.[17]

Table 2: Treatment Effect on the Value and Number of Grants

	(1) IHS(Val)	(2) IHS(Num)	(3) Log(1+Val)	(4) Log(1+Num)
Treat	0.099***	0.013**	0.095***	0.010**
	(0.025)	(0.005)	(0.024)	(0.004)
Young	-0.065	-0.012**	-0.061	-0.010**
	(0.042)	(0.005)	(0.040)	(0.004)
Instate	0.187***	0.028***	0.177***	0.023***
	(0.064)	(0.010)	(0.061)	(0.008)
TotalCand	0.034	0.006**	0.032	0.005**
	(0.022)	(0.003)	(0.021)	(0.002)
NAICS4 X State FE	Yes	Yes	Yes	Yes
NumCandCE FE	Yes	Yes	Yes	Yes
Emp Category FE	Yes	Yes	Yes	Yes
Obs.	6143	6143	6143	6143
R-sq	0.31	0.36	0.32	0.36

Notes: Unit of analysis is firm × state. *Treat* indicates whether 50% or more of candidates a firm supported in close elections won the election in a state, *Young* indicates whether the firm is 10 years old or younger, *Instate* indicates the state in which a firm is headquartered, and *TotalCand* is the log number of candidates a firm supported in a state. Val and Num are the value and number of grants a firm received from a state, and IHS and LN stand for the inverse hyperbolic sine and log transformations, respectively. We include 4-digit NAICS, state, # of candidates supported in close elections, and employment category FE. ***, **, and * indicate significance at the 1%, 5%, and 10% significance levels, respectively. SEs are clustered at the state and industry level.

3.5 Placebo Tests

To validate our identification strategy, we conduct placebo tests and verify whether we obtain insignificant coefficients from regressions where we expect to find no treat-

[17] Note that this can equivalently be thought of as the firm gaining, on average, $2.46 in revenue for every dollar contributed in a close election.

ment effect. The results are presented in Table 3. In the first column, we ask whether being connected to legislators in a given state is predictive of receiving grants in *other* states. In principle, state legislators can only exert influence over grant allocation in their own states and consistent with this argument, we do not find a statistically significant treatment effect in other states. In the second column, we test whether being treated in a given state has a significant impact on receiving grants in the same state as a sub vendor. As discussed earlier, sub vendors are chosen by local governments (e.g., cities or counties) and thus state legislators are likely to play a limited role, if any, in the allocation of grants to sub vendors. Consistent with this argument, we find that being connected to state legislators does not have a statistically significant impact on sub vendor grant allocation.

Table 3: Grant outcomes as sub vendors in treated states and prime vendors in other states

	(1) Grant PV Other	(2) Grant SV
Treat	-0.023	0.122
	(0.525)	(0.339)
Young	-0.533	-0.302
	(0.330)	(0.278)
Instate	-3.950***	2.059***
	(1.114)	(0.749)
TotalCand	0.100	-0.091
	(0.214)	(0.179)
NAICS4 X State FE	Yes	Yes
NumCandCE FE	Yes	Yes
Emp Category FE	Yes	Yes
Obs.	6143	6143
R-sq	0.55	0.29

Notes: Unit of analysis is firm × state. *Treat* indicates whether 50% or more of candidates a firm supported in close elections won the election in a state, *Young* indicates whether the firm is 10 years old or younger, *Instate* indicates the state in which a firm is headquartered, and *TotalCand* is the log number of candidates a firm supported in a state. Grant PV Other indicates that a firm won a grant from any state other than the focal state, and Grant SV indicates that a firm won a grant from a given state as a sub vendor. We include 4-digit NAICS, state, # of candidates supported in close elections, and employment category FE. ***, **, and * indicate significance at the 1%, 5%, and 10% significance levels, respectively. SEs are clustered at the state and industry level.

3.6 Robustness Analysis

Having established the results, we further explore whether our findings are robust to several alternative specifications. First, we estimate a set of regression discontinuity (RD) design models. Specifically, we use the following specification:

$$Y_{i,s} = \beta_0 + f(MarginVictory_{j,s}) + \beta_1 Win_{j,s} + Win_{j,s} \times g(MarginVictory_{j,s}) + \epsilon_{i,s} \quad (2)$$

where β_1 is the coefficient of interest, $Y_{i,s}$ indicates whether firm i has received an ARRA grant in state s, $Win_{j,s}$ is an indicator that takes the value of one if candidate j has won the election and zero otherwise, $MarginVictory_{j,s}$ is the difference in vote share that candidate j has received relative to his opponent, and f and g are polynomial functions. As is standard in the regression discontinuity literature, we use the local linear and quadratic functions for f and g.[18] As shown in Table A.1, about a third of firm-state (i, s) pairs in our sample support more than one candidate j in state s, and the outcome variable in this regression is defined at a broader level than the treatment. Nonetheless, we find it useful to verify whether the estimated marginal effect at the mean is consistent with our main findings.

The first and second columns of Table 4 use a 5 percent margin of victory, the third and fourth columns use a 3 percent margin of victory, and the fifth and sixth columns use the mean squared error optimal bandwidth suggested by Imbens and Kalyanaraman (2012) (denoted as IK). We find positive and statistically significant treatment effects in the RD specifications. The results imply that being connected to an election winner leads to a 35 to 42 percent increase in the probability of winning an ARRA grant, in line with the marginal effect of 38 percent estimated in our main specification.[19]

[18] See, for example, Akey (2015) and Gelman and Imbens (2019).

[19] Figure A.2 in the online appendix visualizes the RD effects reported in Table 4.

Table 4: Regression Discontinuity Design

	(1)	(2)	(3)	(4)	(5)	(6)
Win	1.850***	2.137***	2.122***	2.032**	1.956***	2.137***
	(0.547)	(0.754)	(0.681)	(0.967)	(0.620)	(0.754)
Obs.	23770	23770	14420	14420	17895	23770
Functional form	first order	second order	first order	second order	first order	second order
Bandwidth	5%	5%	3%	3%	IK	IK
Marginal effect	35.1%	41.8%	41.5%	39.4%	37.4%	41.8%

Notes: This table presents results from regression discontinuity design regressions. Win is an indicator whether a firm has won an ARRA grant as a prime vendor in a given state and RD Estimate is the estimated treatment effect. ***, **, and * indicate significance at the 1%, 5%, and 10% significance levels, respectively. Standard errors are clustered at the state level.

We also conduct additional robustness checks, the results of which are reported in Appendix A.4. First, we estimate the regressions on an unmatched sample to see whether our results are driven by the matching procedure. Table A.4 confirms that our main results also hold in an unmatched sample. Additionally, in Table A.5 we show that our results are quantitatively robust to clustering the standard errors at the state level (columns 1 to 3)) and at the industry level (columns 4 to 6).

Summarizing, we use close elections as a source of random variation to causally show that politically connected firms are more likely than non-politically connected firms to win ARRA grants. While we use close elections for identification purposes, the implications of our analysis are broader—politically connected firms affect the allocation of stimulus spending. To study the aggregate implications of our firm-level findings, we evaluate whether the influence of politically connected firms over the distribution of stimulus spending impacted how effectively ARRA achieved its key objective of supporting local employment. To do so, we transition from a firm-level to state-level analysis, which necessitates a different empirical strategy because only a small subset of firms are involved in close elections.[20]

[20] Only 15 percent of politically active firms participated in close state legislative elections in the 2006-2008 election cycles. Politically active firms account for 30.4 percent of ARRA spending, and firms participating in close elections account for 13.8 percent of ARRA spending

4 Allocation of ARRA and State Employment

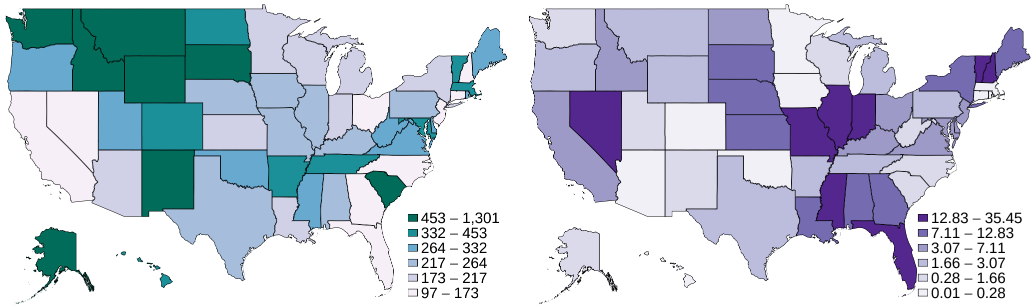
Our empirical approach in this section exploits geographic variation in stimulus spending and the share of that spending channeled through politically connected firms to identify the effects of both factors on local labor market outcomes. Given the importance of states in allocating ARRA grants, our analysis is conducted at the state level. The existing empirical literature uses variation across states in ARRA spending per capita, depicted in the left panel of Figure 4, to determine whether states that received more resources per capita saved and created more jobs.[21] Put simply, two states like Illinois and Pennsylvania, which each channeled between \$220 and \$230 of ARRA stimulus per capita to firms, are expected to save a similar number of jobs in the canonical employment multiplier literature.[22] This approach implicitly assumes that the distribution of stimulus spending across firms within states has no impact on local employment outcomes. We relax this assumption, and use variation in the fraction of ARRA allocated to politically connected firms, depicted in the right panel of Figure 4, to determine whether the jobs multiplier differed in states with a higher fraction of politically connected spending.[23] In particular, we examine whether the fact that Pennsylvania channeled less than 2.1 percent of ARRA spending through politically connected firms, while Illinois channeled 22.7 percent, mattered for the local jobs multiplier.

[21] Recent studies also analyzing ARRA include, Chodorow-Reich et al. (2012), Chodorow-Reich (2019), Conley and Dupor (2013), Dube et al. (2018), Dupor and Mehkari (2016), Dupor and McCrory (2018), Feyrer and Sacerdote (2011), and Wilson (2012)

[22] We define ARRA spending as the resources allocated to firms via grants and contracts. Specifically, it is the total local amount reported in the recipient reports to prime and sub vendors of grants, and to prime- and sub-awardees of contracts.

[23] We define politically connected spending as prime vendor ARRA grants allocated to firms that supported at least one winning candidate in state elections held between 2006 and 2008.

Figure 4: Distribution of ARRA spending per capita across states (L) and Distribution of ARRA spending through politically connected firms (R)



Notes: Left figure shows the distribution of ARRA spending through grants to prime and sub vendors and contracts to prime- and sub-awardees between 2009 and 2010. Right figure shows the distribution of ARRA grant spending channeled through prime vendors that supported at least one winning candidate in state elections held in 2006-2008 as a fraction of total ARRA spending channeled through firms.

4.1 Empirical Model

We adapt the cross-state instrumental variable regression used in the literature (Wilson, 2012; Conley and Dupor, 2013; Chodorow-Reich, 2019) by introducing an additional endogenous variable that measures the fraction of ARRA spending channeled through politically connected firms.:

$$G_{s,T} = \alpha + \beta_1 A^{pc}_{s,T} + \beta_2 S_{s,T} + \mathbf{X_{s,0}}\Gamma + \varepsilon_{s,T} \tag{3}$$

$$V^{j}_{s,T} = \delta + \mathbf{X_{s,0}}\Theta + \mathbf{Z_{s,0}}\Phi + \nu_{s,T} \tag{4}$$

$G_{s,T} = (E_{s,T} - E_{s,0})/P_{s,0}$ is the change in employment in state s between an initial period ($t = 0$) and an end period ($t = T$), scaled by population. $A^{pc}_{s,T}$ denotes the total ARRA grant and contract spending per capita distributed between $t = 0$ and $t = T$ to firms. $S_{s,T}$ is the share of total ARRA spending per capita given to prime vendor grant awardees that supported at least one winning candidate in state elections held between 2006 and 2008. $\mathbf{X_{s,0}}$ is a set of control variables, all of which are pre-determined in the initial period. $V^{j}_{s,T}$ denotes either $A^{pc}_{s,T}$ or $S_{s,T}$. Our vector of excluded instrument for both total ARRA spending per capita ($A^{pc}_{s,T}$) and share allocated to politically connected firms ($S_{s,T}$) is denoted by $\mathbf{Z_{i,0}}$.

To measure the impact of fiscal policy, the literature estimates the marginal effect of ARRA spending on employment, commonly referred to as the jobs multiplier. When ARRA only creates jobs through the amount of spending, the jobs multiplier is simply β_1 and is interpreted as the number of jobs saved per additional \$1 million spent. In our framework, employment is affected not just by the additional spending, but also by how that spending is allocated across firms. Specifically, the jobs multiplier can be derived by taking the partial derivative of Equation 3 with respect to $A^{pc}_{s,T}$, while accounting for the fact that $S_{i,T} = A^{pc,c}_{i,T}/A^{pc}_{i,T}$, where $A^{pc,c}_{i,T}$ denotes politically connected ARRA spending per capita:

$$\frac{\partial G_{s,T}}{\partial A^{pc}_{s,T}} = \left(\beta_1 - \beta_2 \frac{A^{pc,c}_{s,T}}{(A^{pc}_{s,T})^2}\right) + \left(\frac{\beta_2}{A^{pc}_{s,T}}\right) \frac{\partial A^{pc,c}_{s,T}}{\partial A^{pc}_{s,T}} = \beta_1 + \beta_2 \left(\frac{\frac{\partial A^{pc,c}_{s,T}}{\partial A^{pc}_{s,T}} - S_{s,T}}{A^{pc}_{s,T}}\right) \quad (5)$$

Equation 5 allows for the allocation of the marginal ARRA spending $\left(\frac{\partial A^{pc,c}_{s,T}}{\partial A^{pc}_{s,T}}\right)$ to differ from the existing allocation ($S_{s,T}$). Note that if the allocation remains the same $\left(\frac{\partial A^{pc,c}_{s,T}}{\partial A^{pc}_{s,T}} = S_{s,T}\right)$, the jobs multiplier is simply equal to β_1. However, this framework allows us to calculate the marginal effect of ARRA spending when the allocation of these resources vary. In particular, if $\left(\frac{\partial A^{pc,c}_{s,T}}{\partial A^{pc}_{s,T}} > S_{s,T}\right)$, and $\beta_2 < 0$, then an extra \$1 million will create less than β_1 jobs due to differences in job creation by politically connected and non-politically connected firms.

4.2 ARRA Spending and Instrumental Variables

Using Recovery Act Recipient Reports data, we first calculate the amount of ARRA stimulus disbursed to firms within a state by December 2010. Specifically, we sum the amount allocated to four types of recipients—grant prime vendors, grant sub vendors, contract prime vendors, and contract sub vendors, which adds up to \$71 billion. The total ARRA spending allocated to firms is about 26 percent of total ARRA spending paid out during this period.[24]

Our analysis introduces a second endogenous variable that measures the share of ARRA stimulus disbursed to politically connected firms ($S_{s,T}$). We calculate $S_{s,T}$

[24]Much of the remaining ARRA resources were allocated at the federal level or assistance at the state-level through programs such as the Medicaid reimbursement process, which alone amounted to \$88.5 billion.

as the sum of the amount allocated to grant prime vendors who supported at least one winning candidate during the state legislative elections held in 2006 through 2008 divided by total ARRA stimulus disbursed to firms within a state. We focus on political connections formed during elections held in 2006 through 2008, which determined the state officials who were in office when ARRA funds were disbursed to firms in 2009 and 2010.

There are three key sources of endogeneity. The first two pertain to the endogeneity of total ARRA spending and have been noted in the previous literature (Chodorow-Reich et al., 2012; Dupor and Mehkari, 2016; Wilson, 2012). First, ARRA was in part allocated based on how severely states were impacted by the crisis. Second, states played a role in soliciting funds from the federal government, and those states who may have been successful in doing so may also be better managed, and better managed states may simply have better economic performance. Additionally, there is one key source of endogeneity of political connectedness. By measuring firms' political connections based on campaign contributions in state elections between 2006 and 2008, we ensure that the actual formation of political connections is not determined by current economic conditions. However, we know from the previous section that politically connected firms are more effective in soliciting funds from the state government. Therefore, our OLS results could be biased if the severity of current economic conditions impacted the degree to which firms were able to exert their political influence to obtain ARRA funds. We construct two instruments to address these endogeneity concerns.[25]

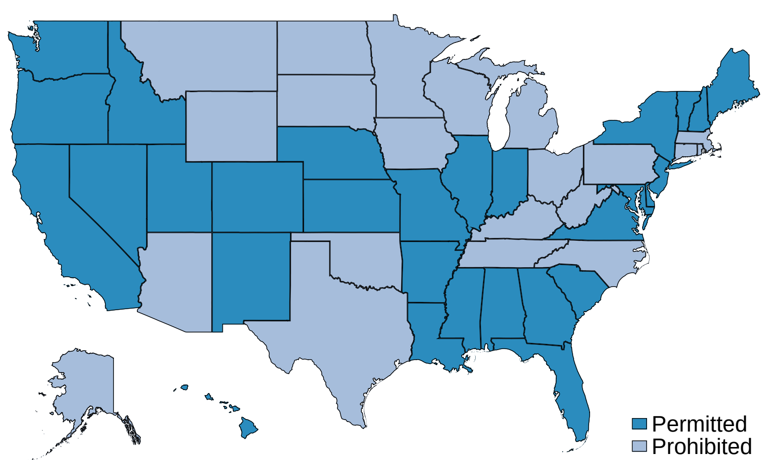
The first instrument, used by Wilson (2012), Conley and Dupor (2013) and Chodorow-Reich (2019), addresses the endogeneity of $A_{s,T}^{pc}$ by taking advantage of the fact that a large fraction of Department of Transportation (DOT) ARRA spending was allocated to states based on pre-recession formulas. We follow Wilson (2012) and construct the instrument as the predicted amount of DOT spending based on a linear combination of the state's lane miles of federal-aid highways, estimated vehicle miles traveled on these highways, estimated payments into the federal highway trust fund, and Federal Highway Administration obligation limits. The first three factors are measured in 2006 and the last in 2008. In our data, DOT funding accounts for

[25]Our IV strategy can also address potential bias that arises from measurement errors in the explanatory variables. In particular, politically connected spending may contain measurement errors that stem from opacity in campaign contribution information.

31 percent of all spending (35 percent on average across states), and 76 percent of grants to prime vendors (78 percent on average across states). Although the DOT instrument is derived from DOT spending, as in previous studies, the instrument is highly correlated with per capita spending allocated to firms (the correlation is 0.73).

The second instrument addresses the endogeneity of $S_{s,T}$ by capturing the potential of firms to attain political connections. In particular, we introduce an indicator denoting whether a state prohibits or permits corporate campaign contributions in state elections, as of 2002. The indicator is based on information from the Federal Elections Commission's (FEC) Campaign Finance Law 2002 publication. Figure 5 shows that 21 states across the country prohibit corporate campaign contributions in state elections.[26] For example, while Pennsylvania prohibits them, Illinois permits them. The idea is that the formation of political connections via campaign contributions is less likely if the state prohibits them. Because we measure corporate campaign contribution restrictions in 2002, it is unlikely to be associated with either the state's economic conditions during our analysis period or the firm's ability to exert influence due to (or in spite of) these economic conditions.

Figure 5: Corporate campaign contribution limits



Notes: The figure depicts whether states permit or prohibit political contributions by corporations.

[26]Note that the campaign contribution restrictions we capture with our instrument pertain specifically to corporations. Campaign contributions by unincorporated businesses (e.g., partnerships and sole proprietorships) are treated differently.

4.3 Dependent and Control Variables

In the baseline analysis, the initial period coincides with the passage of the ARRA stimulus bill in February 2009. The end period is December 2010, by which point nearly two-thirds of ARRA stimulus had been disbursed. Our dependent variable measures the change in the employment between the beginning and end periods, scaled by 2009 working age population. Employment data are obtained from the Bureau of Labor Statistics' (BLS) Current Employment Statistics (CES) data on total statewide, non-farm, seasonally adjusted employment, and working age population data is obtained from the United States Census Bureau.

As in Wilson (2012), we introduce five control variables to our baseline specification because they are potentially correlated with employment growth, ARRA spending, and our instruments. All control variables are measured before the initial period. The first two variables account for states' initial employment situation. In particular, we control for the employment-to-population ratio in February 2009 and lagged employment growth, measured as the log difference of the employment-to-population ratios between December 2007 and February 2009. The third variable measures the change in house prices during the housing boom. It is measured as the log difference in the house price index, published by the Federal Housing Financing Agency (FHFA), between the fourth quarter of 2003 and the fourth quarter of 2007. This variable accounts for the fact that the run-up in house prices is correlated with the depth of the subsequent crisis and may also be correlated with formula factors used in the construction of one of our instruments.

The last two controls in Wilson (2012) are needed to account for two sources of ARRA stimulus not channeled through firms. ARRA provided fiscal stimulus to states using a formula that explicitly factors in the change in average personal income per capita. We measure this as the change between 2005 and 2006 in the three-year trailing average of personal income per capita. ARRA also provided tax relief to state residents via a payroll tax cut and an increase in the income threshold for the Alternative Minimum Tax (AMT). States' estimated tax relief is measured as the sum of the state share of people eligible for the payroll tax cut multiplied by the total national cost of the payroll tax cut and the state share of AMT payments in 2007 multiplied by the total national cost of the AMT adjustment. To account for region-specific employment trends, we deviate from Wilson (2012) and also control for Census Division fixed effects.[27]

We introduce three additional control variables to account for potential omitted factors correlated with both political influence and state level employment growth. First, the prevalence of labor unions in a state may affect the degree of its labor market flexibility. At the same time, labor unions may exert their political influence to shape campaign finance laws. Therefore, we control for the fraction of employees in each state that are union members in 2008. Second, the degree of political corruption in a state could be related to its campaign finance regulations, and also correlated with the speed at which the state can recover from recessions. We therefore control for state corruption, measured as the average annual number of federal, state, and local officials convicted of corruption-related crimes per 100,000 persons between 1976 and 2002 (Glaeser and Raven, 2006). Specifically, we construct a discrete variable that differentiates between states that are above versus below the median of corruption convictions per capita.[28]

4.4 Baseline Results

Our baseline 2SLS and corresponding OLS results are reported in Table 5. The first two columns report the OLS (column 1) and IV (column 2) baseline results. The last two columns we drop the union representation and corruption index controls.[29]

Our baseline IV results in Column (4) show that while ARRA saves jobs, increasing the share of politically connected spending lowers employment growth and the jobs multiplier. In fact, increasing the share of politically connected spending by one standard deviation above the mean ($\sigma_s = 8.7$ percentage points) lowers employment growth by 30 percent (-0.7 percent versus -0.9 percent growth), holding constant ARRA spending per capita ($\bar{A}_t^{pc}$) and all other controls at their cross-sectional mean. To understand the impact of a one standard deviation increase in the share of politically connected spending on the jobs multiplier, we make use of Equation 5. When the marginal million in ARRA spending is allocated according to the cross sectional mean $\left(\frac{\partial A_{s,T}^{pc,c}}{\partial A_{s,T}^{pc}} = \bar{S}_T\right)$ the jobs multiplier indicates that 27.2 jobs are saved for every additional \$1 million in ARRA spent. If instead, we allow the allocation of that same

[27]See Table A.8 in Appendix A.4 for results that exclude these Census Division fixed effects.

[28]Summary statistics for the variables used in our baseline analysis are shown in Table A.6 in Appendix A.5.

[29]The results of our first stage regressions are reported in Table A.7 in the Appendix

marginal million dollar to be biased towards connected firms $\left(\frac{\partial A^{pc,c}_{s,T}}{\partial A^{pc}_{s,T}} = \bar{S}_T + \sigma_s\right)$, the job multipliers decreases to 20.1 jobs. We therefore find that increasing the share of politically connected spending by one standard deviation above the mean reduces the jobs saved per $1 million in ARRA spent by 7.1 jobs, or by 26 percent.

In Columns (3) and (4), we show that excluding the corruption dummy and union membership has little impact on the coefficients for ARRA spending and fraction of politically connected spending, which provides support for the exogeneity of our campaign contribution limit IV. Further, the second to last row of the table reports the first-stage F-statistic. We check for possible weak instrument bias by comparing the first-stage F-statistic with critical values obtained by Stock and Yogo (2005). The F-statistics in Columns (2) and (4) fall between the 10 percent and 15 percent significance level critical values.

Our results show that fiscal stimulus helps save jobs, but the distribution of resources across firms matters for the jobs multiplier. Allocating stimulus to politically connected firms distorts the job-creation effects of stimulus spending.

4.5 Robustness Analysis

Our identification strategy relies on instrumenting the spatial distribution of ARRA spending and the degree of firms' political connections. Omitted factors that are correlated with the instruments and also with the outcome variable could challenge our identification. Table A.9 in the Appendix explores omitted factors that the existing literature has explored as potentially being correlated with state employment growth and our DOT instrument. We show that accounting for state industrial composition, change in house prices during the housing bust, and anticipation of the passage of ARRA stimulus do not qualitatively or quantitatively affect our results.

Table 6 shows that introducing alternative proxies for labor market flexibility and political environment and accounting for the firm age and size distribution of states also does not qualitatively or quantitatively alter our baseline results. In our baseline specification, we account for the possible correlation between employment growth, campaign contribution limits, and union membership. In Columns (2) and (3), we consider two related measures of union influence—fraction of workers represented by unions in 2008 (column 2) and an indicator of whether the state has passed right to work legislation (column 3).

Table 5: Baseline: Second stage results

Dependent variable: change in emp-pop ratio, Feb 09 - Dec 10

	(1)	(2)	(3)	(4)
	OLS	IV	OLS	IV
ARRA spending	10.30*	27.17**	10.53**	24.56**
	(5.766)	(11.03)	(4.801)	(11.19)
Frac. connected spending	-0.00753	-0.0252**	-0.00621	-0.0220*
	(0.00879)	(0.0128)	(0.00855)	(0.0113)
Emp growth (07-09)	0.0956*	0.00360	0.0926*	0.00436
	(0.0489)	(0.0664)	(0.0519)	(0.0594)
Emp pc (09)	0.0379	0.0268	0.0396	0.0333
	(0.0514)	(0.0383)	(0.0506)	(0.0394)
Change in PI moving avg	-3.924	-2.395	-3.769	-2.444
	(3.227)	(2.200)	(3.239)	(2.216)
HPI growth (03-07)	0.00823	0.0107	0.00991	0.0116
	(0.0121)	(0.0110)	(0.0136)	(0.0119)
Tax benefits (mn pc)	-1.749	-0.364	-6.707	-5.285
	(8.896)	(10.56)	(8.568)	(8.719)
Corruption (dummy)	0.000647	-0.000692		
	(0.00232)	(0.00149)		
Union membership (08)	-0.0315	-0.0373		
	(0.0257)	(0.0260)		
Constant	-0.0145	-0.0162	-0.0169	-0.0209
	(0.0201)	(0.0199)	(0.0189)	(0.0200)
Division FE	Yes	Yes	Yes	Yes
F-stat	.	5.420	.	6.096
Obs.	50	50	50	50
R-sq	0.53	0.39	0.51	0.40

Notes: The dependent variable is the Δ in employment between Feb. 2009 and Dec. 2010 relative to working age pop. in 2009. The variables of interest are ARRA spending p.c. and the share allocated through politically connected firms. The IVs are anticipated DOT spending and an indicator of whether a state permits corporate campaign contributions. Our controls include division fixed effects, prior employment growth, initial employment p.c., house price growth between 2003 and 2007, change in personal income before the crisis, expected tax benefits p.c., corruption dummy, and union membership. ***, **, and * indicate sig. at the 1%, 5%, and 10% sig. levels. The F-stat test statistic is reported. Robust SEs.

Our baseline also accounts for the correlation between employment growth, contribution limits, and political environment by controlling for state corruption. Columns (4) and (5) consider two alternative ways of measuring political environment. Column (4) accounts for state governments' administrative capacity by using managerial capacity scores from Maxwell School's Government Performance Project. State governments with lower administrative capacity may engage in more intense political relationships with firms and may have been harder hit during the Great Recession. Column (5) accounts for whether the state legislature is controlled by the same party as the governorship. Split governments may affect willingness and ability to regulate campaign contributions and to manage the economic recovery.

Finally, Columns (6) and (7) account for the possible correlation between employment growth, contribution limits, and firm characteristics. Because older and larger firms may have more resources to be politically active, they may advocate for looser campaign finance laws. At the same time, older and larger firms may experience a different pace of recovery. Consequently, we control for the average fraction of firms that are aged ten or older (column 6) and average fraction of firms that have more than 500 employees (column 7). Notably, across all alternative specifications, our baseline results hold, both qualitatively and quantitatively.

4.6 Differential Evolution of Employment Growth

In this section, we examine whether the share of politically connected spending has a persistent effect on employment growth. We first estimate our baseline IV specification redefining the dependent variable as the change in employment between February 2009 and each month from March 2009 until December 2012.[30] We then evaluate the predicted employment growth at each point in time for the bottom and top deciles of the share of politically connected spending, evaluating all other variables at their means. In addition to predicted employment growth, we also report the 90 percent and 68 percent confidence intervals.[31] Figure 6 shows that in December 2010, employment growth of the bottom decile is 0.42 percentage points higher than the top decile. By December 2012, employment growth of the bottom decile is 1.14 percentage

[30] Wilson (2012) implements a similar approach to estimate the jobs multiplier at alternative end dates.

[31] The dynamic fiscal multiplier literature typically uses one standard deviation bands (e.g., Blanchard and Perotti (2002). In any case, our results are typically significant at 10 percent.

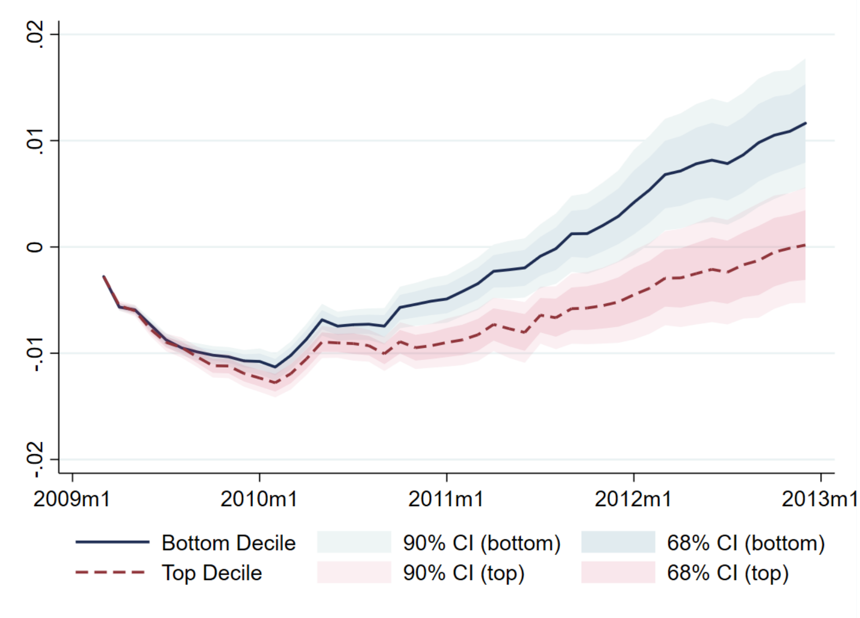
Table 6: Robustness for Fraction Connected ARRA Spending
Dependent variable: change in emp-pop ratio, Feb 09 - Dec 10

	(1) Baseline	(2) Union rep	(3) RTW	(4) Admin cap	(5) State gov	(6) Firm age	(7) Firm size
ARRA spending	27.17**	26.78**	24.54**	31.30**	26.56**	27.02**	23.41***
	(11.03)	(10.86)	(10.14)	(13.06)	(11.98)	(10.75)	(8.849)
Frac. connected spending	-0.0252**	-0.0244**	-0.0233*	-0.0282*	-0.0259**	-0.0235*	-0.0309*
	(0.0128)	(0.0124)	(0.0126)	(0.0155)	(0.0130)	(0.0130)	(0.0161)
Full controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Division FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional FE	No	No	No	Yes	No	No	No
F-stat	5.420	5.416	5.661	4.563	6.117	6.393	6.675
Obs.	50	50	50	50	50	50	50
R-sq	0.39	0.39	0.40	0.36	0.39	0.40	0.41

Notes: The dependent variable is the Δ in employment between Feb. 2009 and Dec. 2010 relative to working age pop. in 2009. The variables of interest are ARRA spending p.c. and the share allocated through politically connected firms. The IVs are anticipated DOT spending and an indicator of whether a state permits corporate campaign contributions. Our standard controls include division fixed effects, prior employment growth, initial employment p.c., house price growth between 2003 and 2007, change in personal income before the crisis, expected tax benefits p.c., corruption dummy, and union membership. New controls are union representation (column 2), right to work state dummy (column 3), Administrative capacity discrete variable (column 4), divided state government dummy (column 5), Avg. firm age (column 6), and Avg. firm size (column 7). ***, **, and * indicate sig. at the 1%, 5%, and 10% sig. levels. Robust SEs.

points higher than the top decile.

Figure 6: Evolution of Employment Growth



Notes: The figure depicts the estimated employment growth of states in the top decline versus bottom decile of share of politically connected ARRA spending.

5 Conclusion

Billions of ARRA stimulus funds were directly allocated by state and local officials to firms. With the average awarded grant worth more than half a million dollars, stimulus funds were valuable to firms. These conditions created incentives for businesses to exert political influence over the distribution of ARRA funds. Using a novel database constructed by matching nationally representative firm-level data with data on campaign contributions, state election outcomes, and ARRA grant allocation, we first show that firms connected to state legislators are 38 percent more likely to win an ARRA grant.

We then evaluate whether firms' political influence over the distribution of stimulus impacted the local jobs multiplier during the Great Recession. Our state-level analysis shows that increasing the share of ARRA spending allocated to politically connected firms by one standard deviation lowers employment growth by 30 percent and the jobs multiplier by 26 percent, or 7.1 jobs. We also find evidence that the detrimental effect of politically connected spending on local employment is persistent.

Our findings are consistent with the evidence that politically connected firms often renegotiate their contract terms to delay product delivery or produce fewer goods and services for a given amount of spending (Brogaard et al., 2020). A consequence may be that firms hire fewer new workers and thus create fewer jobs. Because the allocation of fiscal stimulus impacts the effectiveness of policy, our results suggest that more attention is needed when allocating these resources, especially when firms can exert political influence on the distribution.

Reexamining the impact of fiscal stimulus has become particularly relevant in light of recent events. Practically every country in the world is seeking to save and create jobs in the midst of a worldwide recession triggered by a global pandemic. In response, G-20 countries have enacted stimulus packages in excess of 5 percent of GDP, with many countries directing stimulus funds to firms as a key policy tool. Using ARRA as a laboratory, we show that it is important to take into account the political process by which funds are allocated to firms when analyzing the employment effect of fiscal stimulus. Therefore, the discussion of fiscal policy cannot be centered solely around the size of the stimulus but must also take into account the processes by which these funds are allocated to firms.

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A Appendix

A.1 Data Construction: Merging Details

This paper combines firm-level data from the National Establishment Time Series (NETS) with grant/contract-level data from the Recovery Act Recipient Report, and state campaign contribution-level data from the National Institute of Money in Politics (NIMP). To link these three sources, we first link NETS with the Recovery Recipient Report data, and then separately, NETS with NIMP data. The two merges (NETS-Recovery Recipient Report and NETS-NIMP) proceed in three steps—Preparation, Merging, and Deduplication.

Preparation: the first set of steps are implemented to harmonize the key matching variables across the three data sets with the goal of improving match quality.

1. For NETS we create a data set that is unique in firm ID, establishment ID, name, city, state, and zip code of the establishment. Firms that own multiple establishments, especially those with subsidiaries, have several distinct business name and location pairs in NETS. We use the ID of the headquarter of each firm as its firm ID. For the Recipient Report data, we also create a data set that extracts firm ID, name, city, state, and zip code. Recipient Report data and NETS share the same business identifier structure maintained by Dun and Bradstreet, the Dunsnumber, which helps us in merging the two data sets. Note that not all firms in the Recipient Report data report their Dunsnumbers. For the NIMP data, we first drop contributions made by individuals, the party, and non-contributions, and subsequently extract contributor (firm) name, city, state, and zip code.

2. For each data source, we implement the same set of cleaning steps for firm name, city, state and zip code:

    - Names are standardized to improve match quality. This procedure involves capitalization, elimination of special characters, standardization of company type (e.g., COMPANY changed to CO), and standardization of common words (e.g., variations of the word PRODUCT to PROD). The first and longest words of the name are saved as separate variables to be used later in merging.

- Zip codes are verified to contain only numbers and standardized to be 5 digits.
- State codes are capitalized and verified against a list of United States states. If a state code is missing but zip codes is available, it is added using a crosswalk between zip codes and states.
- City names are capitalized. If a city name is missing but a zip code is available, it is added using a crosswalk between zip codes and cities.

Merging: We link NETS with Recipient Report and NIMP data separately, but the procedure is the same. Note that matches resulting from each step described below are excluded from subsequent steps.

1. When available, the first match pass is based on the Dunsnumber. This step is only possible when matching NETS to the Recipient Report data.
2. The second match pass links records where the company name matches exactly.
3. The third match pass links records that match exactly on the 5-digit zip code, exactly on the longest or first word of the company name, and have similar full company names based on the Levenshtein distance and Jaro-Winkler score.
4. The fourth match pass links records that match exactly on the city name, exactly on the longest or first word of the company name, have similar full company names, and are located in the same state.
5. The fifth match pass links records that match exactly on the state code, exactly on the longest or first word of the company name, and have similar full company names.
6. The sixth, and final, match pass links records that have exactly the same longest or first word of the company name, and have similar full company names.

Deduplication: As a consequence of the probabilistic nature of the merging, a single Recipient Report or NIMP record can be linked to multiple NETS records. The aim of the final step is to disambiguate multiple matches so that each firm in the Recipient Report or NIMP data is linked to only one firm in NETS.

1. Records that match on Dunsnumber are always given preference. All remaining matched records receives a composite score that is calculated as the simple sum of the full company name Jaro-Winkler score, the city Jaro-Winkler score, an indicator of whether the records list the same state, and a discrete variable with value of 1 if the records have the same 5-digit zip code, a value of 0.5 if the records have the same 3-digit zip code, and a 0 otherwise. For each firm in the Recipient Report and NIMP data, we keep the NETS match with the highest composite score.

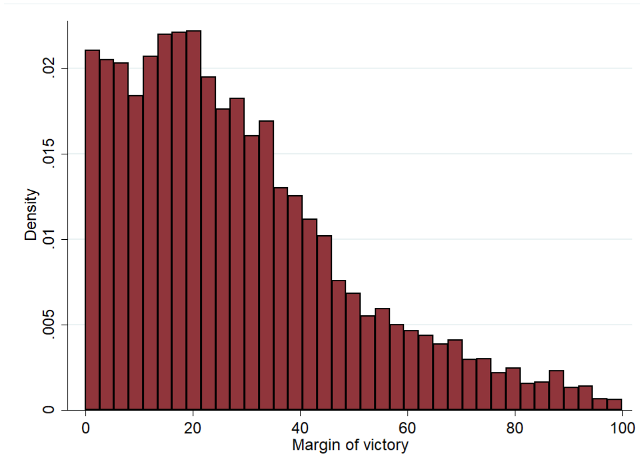
2. We break ties (i.e., the composite score is the same for multiple records) randomly.

Merging Evaluation: Our merging procedure identifies 55 percent of prime vendors from the Recipient Report data in NETS. These firms account for 64 percent of records and 85 percent of the grant dollar value. Our merging procedure identifies nearly 60 percent of contributors from NIMP in NETS. These firms account for 63 percent of contribution records and 65 percent of their value. It is worth noting that while we drop individual, party, and non-contribution records from NIMP data before matching, non-business entities remain in our data. For example, 30 percent of the unmatched records are associated with labor unions, business associations, and political committees. The presence of these entities helps explain the lower match rate between NETS and NIMPS than NETS and Recovery Report data.

A.2 Grant Allocation: Regression Analysis

Figure A.1 shows the full distribution of the margin of victory in the state legislative elections held between 2006 and 2008. Table A.1 reports the distribution of the number of candidates firms support in close elections in each state. The mean and median margins of victory are 28.5 percent and 24 percent, respectively, and the elections won by a 5 percent or lower margin of victory constitute 10 percent of the elections.

Figure A.1: Margin of Victory in State Legislative Elections (2006-2008)



Source: ICPSR State Legislative Election Returns Databse

Notes: This histogram presents the margin of victory for state legislative elections of which terms lasted at least until 2010. These elections occurred during the 2006, 2007, and 2008 election cycles. Margin of victory is defined as the vote share of the winner minus that received by the second place candidate. We exclude elections with only one candidate in this histogram.

Table A.1: Number of Candidates a Firm Supports in Close Elections in a State

	Frequency	Percent
1	10494	66.8
2	1736	11.1
3	897	5.7
4	553	3.5
5	399	2.5
6+	1630	10.4
Total	15709	100.0

Table A.2 shows the differences in the unmatched variables between the treated and control units. Specifically, we regress the variable of interest on the treat dummy on a sample matched on state, the number of candidates supported in close elections, and industry, taking into account the matching weights. The results show that there are no statistically significant differences in the unmatched characteristics between these two groups.

Table A.2: Balance of Characteristics

	(1) Ln(Emp)	(2) Young	(3) Paydex	(4) Total Candidate	(5) Instate
Treat	-0.044	0.017	-0.068	0.181	0.002
	(0.059)	(0.012)	(0.248)	(0.279)	(0.010)
Obs.	6187	6187	4587	6187	6187

A.3 Ex-post Dollar Value of Contributions

As shown in Table A.1, about two-thirds of the firm-state pairs in our sample consist of firms that support a politician only in one close election in a state. In this sample, our $Treat_{i,s}$ dummy is a simple indicator that takes the value of 1 if the candidate firm i supported in state s has won the election and zero otherwise. To understand the monetary value of political connections in the context of ARRA, we utilize this specific subsample to measure the realized rate of return, in terms of ARRA grant value, of a dollar donated to a politician. Specifically, we define a variable

$$CamountW_{i,s} = \$amount_{i,s} \times Treat_{i,s}$$

where $\$amount_{i,s}$ is the dollar amount that firm i has donated to the candidate in state s running for office in a close election. Because both grant dollar value and $\$amount_{i,s}$ are highly positively skewed, we apply the IHS transformation on both variables. Having IHS on both sides of the equation allows us to interpret the coefficient as the percent return on a percent increase in dollars contributed to the winner. We run the following regression:

$$\text{IHS}(Val)_{i,s} = \beta_0 + \beta_1 \text{IHS}(CamountW)_{i,s} + \gamma' X_{i,s} + \epsilon_{i,s} \tag{6}$$

where $\text{IHS}(Val)_{i,s}$ is the grant dollar amount that firm i receives from state s as a prime vendor. The estimated coefficient in Table A.3 implies that a 1 percent increase in campaign contributions to an election winner results in a 0.019 percent increase in grant value. In our sample, this effect translates into an average unexpected windfall of \$2.13 for every dollar donated to a candidate in a close election, or a 213 percent average rate of return.[32] In our setting, the effect of campaign contribution on receiving a grant is small, but the average unexpected return is quite substantial once we take grant values into account.[33] Because this return is unexpected by the firm at the moment of contributing, it serves as a lower bound for the monetary return of corporate political engagement.

Table A.3: Rate of Return Regression

	(1) IHS(Val)
IHS(CamountW)	0.019***
	(0.003)
Young	-0.048
	(0.041)
Instate	0.216***
	(0.060)
TotalCand	0.026
	(0.021)
NAICS4 X State FE	
NumCandCE FE	
Emp Category FE	
Obs.	5690
R-sq	0.32

Notes: Unit of analysis is firm × state. $Treat$ indicates whether 50% or more of candidates a firm supported in close elections won the election in a state, $Young$ indicates whether the firm is 10 years old or younger, $Instate$ indicates the state in which a firm is headquartered, and $TotalCand$ is the log number of candidates a firm supported in a state. IHS(Val) is inverse hyperbolic sine of the value of grants a firm received from a state. We include 4-digit NAICS, state, # of candidates supported in close elections, and employment category FE. ***, **, and * indicate significance at the 1%, 5%, and 10% significance levels, respectively. SEs are clustered at the state and industry level.

[32] In the sample, the mean of $CamountW$ is \$366 and that of Val is \$82,086, where both are calculated including zeros. Therefore, a \$3.66 (= \$366 × 0.01) increase in campaign contributions to a close election winner leads to a \$15.6 ($\$82,086 \times 0.019 \times 0.01$) increase in grant value. In other words, a firm receives \$4.26 ($= \frac{15.6}{3.66}$) in grants for every dollar donated to a close election winner. Because the chance of a politician winning in a close election is approximately 50 percent, the average unexpected windfall is \$2.13 (= \$4.26 × 0.5) for every dollar donated to a candidate in a close election.

[33] This finding is quantitatively consistent with Kang (2016), who finds that in the context of lobbying activities in the energy sector, the effect of lobbying expenditures on a policy's enactment probability is small but the average returns from lobbying expenditures are over 130 percent.

A.4 Grant Allocation: Robustness Analysis

Unmatched Sample: Table A.4 evaluates whether the main result and the placebo tests are sensitive to the matching procedure. The outcome variables are indicator variables taking a value of one if a firm wins a grant as a prime vendor in a given state (column 1), if a firm wins a grant as a prime vendor in any other state (column 2), and if a firm wins a grant as a sub vendor in a given state (column 3) and zero otherwise. The results are consistent with those from the matched sample shown in Table 1 and Table 3.

Table A.4: Robustness: Unmatched Sample

	(1) Grant PV	(2) Grant PV Other	(3) Grant SV
Treat	0.485***	-0.322	0.103
	(0.144)	(0.447)	(0.314)
Young	-0.343	-0.691	-0.163
	(0.255)	(0.589)	(0.323)
Instate	2.327***	-4.907***	2.386***
	(0.739)	(1.228)	(0.669)
TotalCand	0.137	0.234	0.218
	(0.149)	(0.263)	(0.242)
NAICS4 X State FE	Yes	Yes	Yes
NumCandCE FE	Yes	Yes	Yes
Emp Category FE	Yes	Yes	Yes
Obs.	9965	9965	9965
R-sq	0.36	0.59	0.38

Notes: The unit of analysis of this regression is firm by state. $Treat$ is a dummy indicating whether 50% or more of candidates that a firm supported in close elections won the election in a given state, $Young$ is a dummy indicating whether the firm 10 years old or younger in 2008, $Instate$ is a dummy indicating whether a firm is headquartered in a given state, and $TotalCand$ is the log total number of candidates a firm supported in the elections in a given state. Grant PV is an indicator whether a firm received a grant as a prime vendor in a given state, Grant PV Other is an indicator whether a firm received a grant in any other states and Grant SV is an indicator whether a firm received a grant as a sub vendor in a given state. We control for four-digit NAICS by state fixed effect, and fixed effects for the number of candidates a firm supported in close elections and its size category measured by the number of employees. ***, **, and * indicate significance at the 1%, 5%, and 10% significance levels, respectively. Standard errors are clustered at the state and industry level.

Alternative Standard Error Clustering: Table A.5 presents the main result and placebo tests with alternative levels of standard error clustering. Columns 1 to 3 show the results when the standard errors are clustered at the state level, and

Columns 4 to 6 show the results when the standard errors are clustered at the industry (four-digit NAICS) level. Table A.5 shows that the results in Table 1 and Table 3 are robust to these alternative ways of standard error clustering.

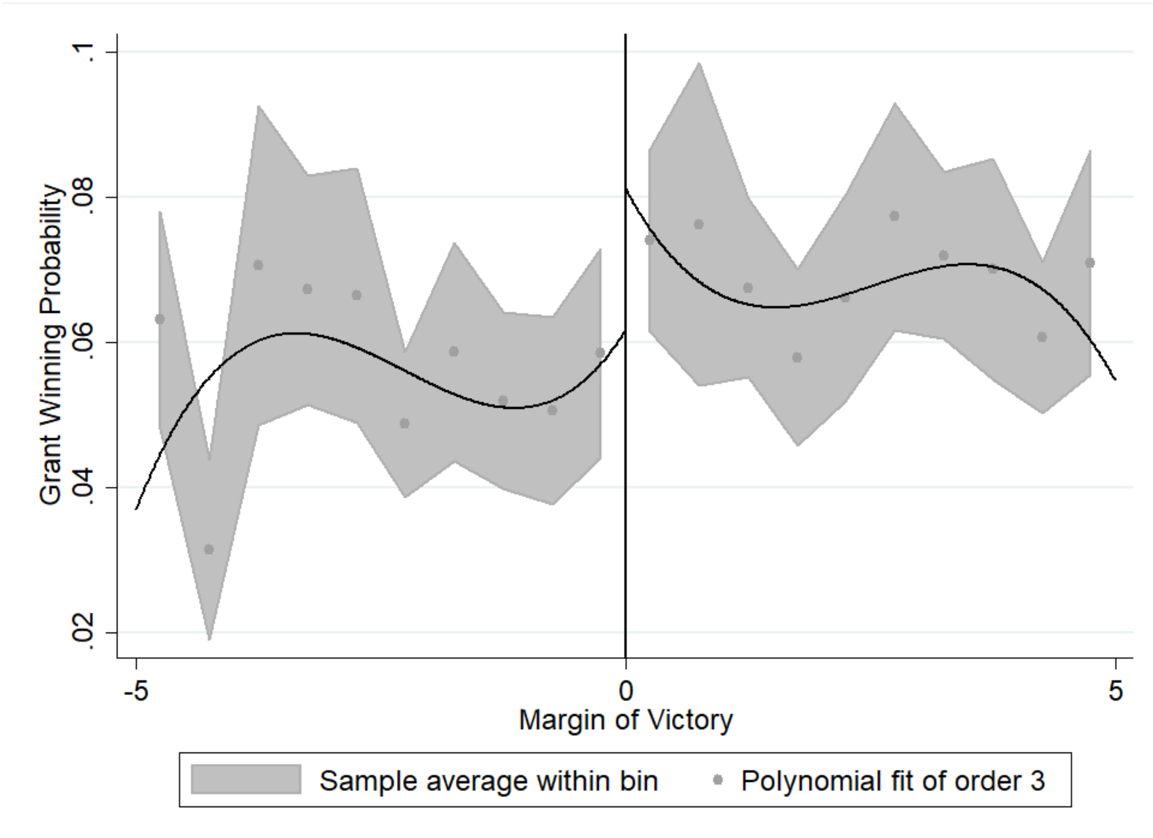
Table A.5: Robustness: Alternative Standard Error Clustering

	(1) Grant PV	(2) Grant PV Other	(3) Grant SV	(4) Grant PV	(5) Grant PV Other	(6) Grant SV
Treat	0.686***	-0.023	0.122	0.686***	-0.023	0.122
	(0.208)	(0.525)	(0.339)	(0.255)	(0.372)	(0.373)
Young	-0.593**	-0.533	-0.302	-0.593	-0.533*	-0.302
	(0.277)	(0.330)	(0.278)	(0.397)	(0.304)	(0.456)
Instate	1.515***	-3.950***	2.059***	1.515***	-3.950***	2.059***
	(0.476)	(1.114)	(0.749)	(0.452)	(0.813)	(0.570)
TotalCand	0.142	0.100	-0.091	0.142	0.100	-0.091
	(0.161)	(0.214)	(0.179)	(0.187)	(0.360)	(0.200)
NAICS4 X State FE	Yes	Yes	Yes	Yes	Yes	Yes
NumCandCE FE	Yes	Yes	Yes	Yes	Yes	Yes
Emp Category FE	Yes	Yes	Yes	Yes	Yes	Yes
Clustering	6143	6143	6143	6143	6143	6143
Obs.	State	State	State	NAICS4	NAICS4	NAICS4
R-sq	0.30	0.55	0.29	0.30	0.55	0.29

Notes: The unit of analysis of this regression is firm by state. $Treat$ is a dummy indicating whether 50% or more of candidates that a firm supported in close elections won the election in a given state, $Young$ is a dummy indicating whether the firm 10 years old or younger in 2008, $Instate$ is a dummy indicating whether a firm is headquartered in a given state, and $TotalCand$ is the log total number of candidates a firm supported in the elections in a given state. Grant PV is an indicator whether a firm received a grant as a prime vendor in a given state, Grant PV Other is an indicator whether a firm received a grant in any other states and Grant SV is an indicator whether a firm received a grant as a sub vendor in a given state. We control for four-digit NAICS by state fixed effect, and fixed effects for the number of candidates a firm supported in close elections and its size category measured by the number of employees. ***, **, and * indicate significance at the 1%, 5%, and 10% significance levels, respectively. Standard errors are clustered at the state and industry level.

Visual Representation of the RD effects: Figure A.2 visualizes the treatment effects estimated from a regression discontinuity design regression, which are reported in Table 4. Each dot represents the average grant winning probability of a 0.25 percent-sized bin, and shaded areas show 95 percent-confidence intervals around the average probabilities. Solid lines are predicted probabilities from local cubic polynomial regressions.

Figure A.2: RD plot



Notes: This figure visualizes the RD effect on being connected to a close election winner on the probability of winning an ARRA grant. The x-axis represents the difference of vote share between the top two candidates, and the y-axis represents the probability of winning an ARRA grant. The lines are predicted probabilities from local cubic polynomial regressions on samples within 5% vote share difference. The dots represent the average grant winning probability in 0.5%-sized bins, with 95%-confidence intervals in shaded areas.

A.5 Allocation of ARRA and State Employment

Summary Statistics: Table A.6 reports the summary statistics for all the variables used in our baseline analysis.

Table A.6: Summary Statistics

	Mean	SD	Min	Max	N
A) Dependent variable					
Emp growth pc (Feb 09 - Dec 10)	-0.007	0.007	-0.025	0.027	50
B) Explanatory variables					
ARRA spending (ths. pc)	0.311	0.197	0.097	1.301	50
Frac. connected spending	0.068	0.087	0.000	0.355	50
Emp growth (07-09)	-0.051	0.024	-0.119	-0.006	50
Emp pc (09)	0.447	0.040	0.377	0.551	50
HPI growth (03-07)	0.218	0.118	-0.113	0.422	50
Change in PI moving avg	0.001	0.001	-0.000	0.003	50
Tax benefits (ths pc)	0.566	0.110	0.435	0.919	50
Union membership (08)	11.412	5.771	3.500	24.900	50
Corruption (dummy)	0.500	0.505	0.000	1.000	50
C) Instrumental variables					
DOT IV (ths. pc)	0.164	0.072	0.114	0.460	50
Corp contrib (dummy)	0.580	0.499	0.000	1.000	50

First-stage results: Table A.7 reports the first stage regression results for our baseline specification.

Table A.7: First stage results

	(1) ARRA spending	(2) Frac connected ARRA
DOT IV (ths pc)	1.664***	0.503**
	(0.544)	(0.191)
Corp contrib (dummy)	0.012	0.143***
	(0.032)	(0.024)
Emp growth (07-09)	2.865*	-0.463
	(1.527)	(0.661)
Emp pc (09)	-0.712	-0.135
	(0.846)	(0.378)
Change in PI moving avg	-87.940*	54.656**
	(46.033)	(23.238)
HPI growth (03-07)	-0.094	-0.074
	(0.332)	(0.131)
Tax benefits (mn pc)	71.849	-334.377**
	(332.413)	(162.517)
Corruption (dummy)	-0.001	-0.027
	(0.043)	(0.022)
Union membership (08)	-0.001	0.001
	(0.006)	(0.003)
Constant	0.445	0.201
	(0.399)	(0.169)
Division FE	Yes	Yes
Obs.	50	50
R-sq	0.74	0.69

Notes: The dependent variable in column (1) is p.c. ARRA spending to firms; in column (2) is fraction of ARRA spending through politically connected firms. The IVs are anticipated DOT spending and an indicator of whether a state permits corporate campaign contributions. Our controls include division fixed effects, prior employment growth, initial employment p.c., house price growth between 2003 and 2007, change in personal income before the crisis, expected tax benefits p.c., corruption dummy (above versus below the median in corruption), and union membership in 2008. ***, **, and * indicate sig. at the 1%, 5%, and 10% sig. levels. Robust SEs.

Variations of the Baseline Regression: Table A.8 reports the second stage regression results using only controls included in Wilson (2012) (columns 1 and 2), including also division fixed effects (columns 3 and 4), and adding the corruption dummy and union membership (columns 5 and 6). The regression in column 6 represents our baselne specification.

Table A.8: Baseline: Second stage results

Dependent variable: change in emp-pop ratio, Feb 09 - Dec 10

	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV
ARRA spending	6.538	14.77**	10.53**	24.56**	10.30*	27.17**
	(4.492)	(6.261)	(4.801)	(11.19)	(5.766)	(11.03)
Frac. connected spending	-0.00609	-0.0261*	-0.00621	-0.0220*	-0.00753	-0.0252**
	(0.00765)	(0.0138)	(0.00855)	(0.0113)	(0.00879)	(0.0128)
Emp growth (07-09)	0.145***	0.118***	0.0926*	0.00436	0.0956*	0.00360
	(0.0412)	(0.0323)	(0.0519)	(0.0594)	(0.0489)	(0.0664)
Emp pc (09)	0.00775	0.00853	0.0396	0.0333	0.0379	0.0268
	(0.0416)	(0.0383)	(0.0506)	(0.0394)	(0.0514)	(0.0383)
Change in PI moving avg	-4.645*	-4.402**	-3.769	-2.444	-3.924	-2.395
	(2.488)	(2.162)	(3.239)	(2.216)	(3.227)	(2.200)
HPI growth (03-07)	0.0109	0.00212	0.00991	0.0116	0.00823	0.0107
	(0.0117)	(0.00855)	(0.0136)	(0.0119)	(0.0121)	(0.0110)
Tax benefits (mn pc)	-2.118	1.893	-6.707	-5.285	-1.749	-0.364
	(8.596)	(6.367)	(8.568)	(8.719)	(8.896)	(10.56)
Corruption (dummy)					0.000647	-0.000692
					(0.00232)	(0.00149)
Union membership (08)					-0.000315	-0.000373
					(0.000257)	(0.000260)
Constant	-0.00159	-0.00508	-0.0169	-0.0209	-0.0145	-0.0162
	(0.0135)	(0.0150)	(0.0189)	(0.0200)	(0.0201)	(0.0199)
Division FE	No	No	Yes	Yes	Yes	Yes
F-stat	.	8.356	.	6.096	.	5.420
Obs.	50	50	50	50	50	50
R-sq	0.46	0.33	0.51	0.40	0.53	0.39

Notes: The dependent variable is the Δ in employment between Feb. 2009 and Dec. 2010 relative to working age pop. in 2009. The variables of interest are ARRA spending p.c. and the share allocated through politically connected firms. The IVs are anticipated DOT spending and an indicator of whether a state permits corporate campaign contributions. Our controls include division fixed effects (columns 3-6), prior employment growth, initial employment p.c., house price growth between 2003 and 2007, change in personal income before the crisis, expected tax benefits p.c., corruption dummy, and union membership. ***, **, and * indicate sig. at the 1%, 5%, and 10% sig. levels. The F-stat test statistic is reported. Robust SEs.

Table A.9: Robustness for ARRA Spending

Dependent variable: change in emp-pop ratio

	(1)	(2)	(3)	(4)	(5)
	Baseline	Manu share	Bartik	Housing bust	Anticipation
ARRA spending	27.17**	26.77**	29.68***	30.51**	25.56**
	(11.03)	(10.81)	(10.87)	(13.33)	(11.32)
Frac. connected spending	-0.0252**	-0.0321**	-0.0199*	-0.0291*	-0.0228*
	(0.0128)	(0.0154)	(0.0104)	(0.0153)	(0.0131)
Emp growth	0.00360	-0.000825	0.0186	0.0575	0.0840
	(0.0664)	(0.0695)	(0.0566)	(0.0563)	(0.0883)
Emp pc	0.0268	0.0145	-0.0256	0.0459	0.0135
	(0.0383)	(0.0333)	(0.0231)	(0.0447)	(0.0395)
Change in PI moving avg	-2.395	-3.036	-1.078	-3.355	-3.745
	(2.200)	(2.375)	(1.536)	(2.387)	(2.389)
HPI growth (03-07)	0.0107	0.0102	0.00670	0.00914	0.0171
	(0.0110)	(0.0104)	(0.00910)	(0.00943)	(0.0108)
Tax benefits (mn pc)	-0.364	-3.084	-10.41	-4.638	4.451
	(10.56)	(10.98)	(11.43)	(9.968)	(9.978)
Corruption (dummy)	-0.000692	-0.00256*	-0.00453***	-0.000914	0.000160
	(0.00149)	(0.00153)	(0.00147)	(0.00153)	(0.00150)
Union membership (08)	-0.000373	-0.000610	-0.000659**	-0.000555	-0.000339
	(0.000260)	(0.000377)	(0.000320)	(0.000357)	(0.000270)
Manufacturing share		-0.0671			
		(0.0523)			
Exp. emp change (Bartik)			31.13**		
			(12.80)		
HPI growth (07-09)				-0.0304	
				(0.0220)	
Constant	-0.0162	0.00276	-0.0129	-0.0190	-0.0169
	(0.0199)	(0.0186)	(0.0160)	(0.0203)	(0.0206)
Division FE	Yes	Yes	Yes	Yes	Yes
F-stat	5.420	5.340	5.112	4.311	5.243
Obs.	50	50	50	50	50
R-sq	0.39	0.39	0.50	0.39	0.56

Notes: The dependent variable is the Δ in employment between Feb. 2009 (Dec. 2008 in column 5) and Dec. 2010 relative to working age pop. in 2009 (2008 in column 5). The variables of interest are ARRA spending p.c. and the share allocated through politically connected firms. The IVs are anticipated DOT spending and an indicator of whether a state permits corporate campaign contributions. Our standard controls include division fixed effects, prior employment growth, initial employment p.c., house price growth between 2003 and 2007, change in personal income before the crisis, expected tax benefits p.c., corruption dummy, and union membership. New controls include manufacturing share of employment (column 2), expected change in employment based on state industrial composition (column 3), and change in house prices during the housing bust (column 3). ***, **, and * indicate sig. at the 1%, 5%, and 10% sig. levels. Robust SEs.