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

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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 public expenditure channeled through politically connected firms hinders job creation. We build a unique database that links campaign contributions and state legislative election outcomes to 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 64 percent more likely to secure a grant. Based on an instrumental variable approach, we also establish that state-level employment creation associated with grants channeled through politically connected firms is nil. 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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# 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, while largely abstracting from whether the allocation of resources across recipients affects the size of the employment multiplier.

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 affect the job creation patterns of fiscal interventions. Are firms successful in influencing the allocation of stimulus spending? Does the allocation of these funds across firms affect the size of the local employment multiplier, measured by the number of jobs created or saved per million dollars spent at the state level? This paper provides empirical answers to both questions. 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, and that only the share of ARRA grants allocated to non-politically connected firms contributes to state-level job creation. Therefore, it is not only the size of the fiscal stimulus that matters for employment growth, 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 grant awards to firms.[1] In addition, ARRA featured a high degree of

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

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 exploit variation in political connections across firms within states to study the link between political connections and the allocation of fiscal stimulus and cross-state variation in the share of stimulus funds channeled through politically connected firms to evaluate the impact of political connections on the employment 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 take 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). On average, firms that support a larger number of candidates (for separate offices) in close elections create connection with more state legislators, and the decision to do so is potentially correlated with the probability of winning ARRA grants. Therefore, we further focus on firms that supported the same number of candidates in close elections. We do so by matching treated firms to their counterparts on the number of candidates supported in close elections, state, and industry, and also use these variables as controls in our regressions.

We find that firms that contribute to winning candidates are 64 percent more likely to secure an ARRA grant and receive 10 percent larger grants. A series of placebo tests provide further support for our identification strategy. We find that stronger connections to legislators in a given state only have a causal effect on the allocation of grants in that state and not in other states. Our results are also robust

[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.

to using various alternative empirical specifications. While the matching procedure helps us to achieve a balanced 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 regression discontinuity design analysis.

Our results show that, at the firm-level, political connection causally affect the allocation of stimulus spending. From a macroeconomic and policy perspective, how grants are allocated is only of interest if it affects the main target of the policy – job creation. To tackle this issue, we extend the job multiplier literature to separately identify the employment effect of expenditure channeled through politically connected versus non-politically connected firms by exploiting cross-state variation in both ARRA funding and the fraction of that funding channeled through politically connected firms. To account for endogeneity arising from the correlation between local needs and the size of stimulus resources, we follow Wilson (2012) and instrument for ARRA funding to firms with predicted Department of Transportation (DOT) spending based on pre-existing allocation formulas. Our interest in separately identifying the effect of politically-connected and non-connected spending requires us to introduce additional instruments addressing the potential correlation between firms' ability and willingness to exert political influence and local economic conditions. To this end, we capture the opportunity firms have to form political connections with two new instrumental variables – whether the state prohibits corporate campaign contributions and the number of state legislature seats per capita. These instruments are unlikely to be associated with states' economic conditions during the Great Recession because restrictions on corporate campaign contributions are measured in 2002 and the size of the state legislature is pre-determined.

In line with previous estimates, the total state-level employment multiplier for ARRA funds is 15 jobs per million dollars.[3] When we allow for a differential multiplier for the resources channeled through politically connected firms, we find that non-politically connected firms create 13 jobs per million dollars, while the employment multiplier associated with grants to politically connected firms is not significantly different from zero. Our results are robust to alternative controls, alternative timing,

[3]For a comprehensive review of this literature and estimates of the employment multiplier, see Chodorow-Reich (2019). Our estimates are broadly in line with those reported in Chodorow-Reich et al. (2012); Dupor and Mehkari (2016); Wilson (2012).

and potential confounding factors linked to labor market flexibility and state industry composition. Moreover, the differential employment effect seems to be driven by connected firms fulfilling their fiscal commitments inefficiently by incurring delivery delays and extra costs that may decrease the need for or prevent them from hiring more workers.

The allocative distortion caused by political connections is sizable. Although only 6 percent of grant recipients contribute during local elections, they account for 21 percent of total ARRA grants.[4] Moreover, based on our multiplier estimation, the same million dollars allocated to the average state in the top quartile of funds channeled through politically connected firms would have saved 23 percent more jobs if this state had allocated a similar share of funds to non-politically connected firms as the average state in the bottom quartile. In a nutshell, we show, for the first time, that the allocation of fiscal stimulus has an impact on the employment multiplier. Thus, when analyzing fiscal stimulus policy, it is important to take into consideration the political process that allocates the funds 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. 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

[4] On average, across states, less than one percent of all firms contribute to local elections.

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. (2018) 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.

These prior studies have primarily focused on federal-level campaign contributions and lobbying activities of large, publicly listed companies. We widen the scope of the existing literature by studying the importance of the relationship between state governments and small- and medium-sized enterprises (SME). By merging, for the first time, firm characteristics from a nationally representative data set with state-level campaign finance and ARRA grant data, we are able to document that SMEs account for 55 percent of total corporate state campaign contributions and receive 72 percent of the more than $125 billion in ARRA grants awarded to firms by state authorities. In fact, ours is the first study to causally establish that political connections of SMEs to state politicians has an impact on the allocation of stimulus grants. We also provide a new perspective by linking the politically influenced allocation of government expenditure to local employment growth.

The Great Recession revitalized the literature on the employment effect of fiscal stimulus. Most empirical studies exploit geographic variation in fiscal spending to estimate the macroeconomic 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), Leduc and Wilson (2013), and Leduc and Wilson (2017) use the state allocation of highway expenditure.[5] 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. **?** 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.[6]

[5]Leduc and Wilson (2017) find that states with more political contributions from the public works sector to the governor and state legislators tended to spend more of the ARRA highway funds received from the Federal Highway Administration. We establish firm-level causal evidence that is consistent with their cross-state conditional correlation evidence using a comprehensive data on grants that is inclusive of highway projects.

[6]Beyond the analysis of ARRA, Nakamura and Steinsson (2014) and Dupor and Guerrero (2017)

Although prior studies recognize that firms are crucial for understanding the effects of fiscal stimulus, this literature has not studied how government spending is allocated across firms and whether this allocation can impact the macroeconomic effects of the policy. We provide novel evidence that allocation affects the strength of the employment multiplier. We show that while a $1 million allocated to non-politically connected firms creates 13 jobs, the same amount allocated to politically connected firms creates none.

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 employment 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.

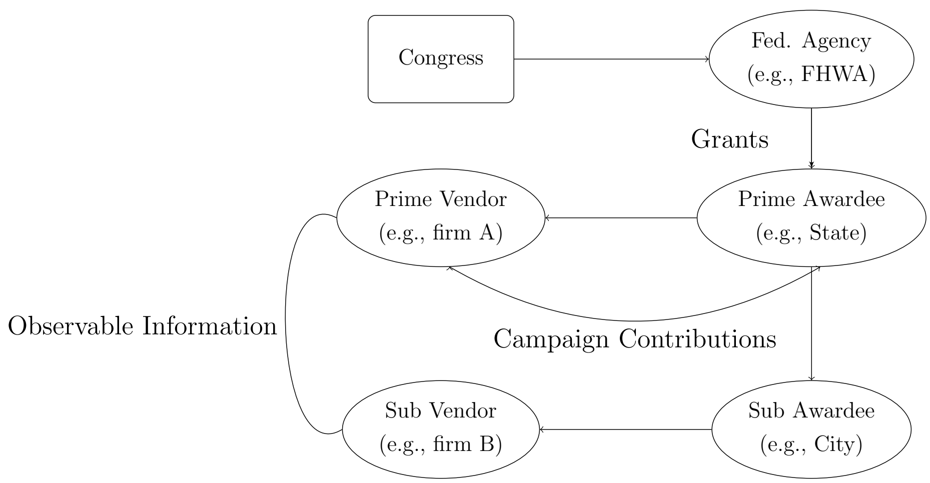
The focus of this study is ARRA grants awarded to firms. Federal grant spending is often channeled through subnational governments, and such intermediation creates

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).

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

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 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 local 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. The Recovery Act states that "every taxpayer dollar spent on our economic recovery must be subject to unprecedented levels of transparency and accountability." In accordance with this objective, Section 1512(c) of the Recovery Act established a high standard 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). The 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. The 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, the NETS becomes a representative sample of the United States businesses with paid employees, and its cross-sectional distributions are consistent with those of official government data sets (Barnatchez et al., 2017). We use the 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, award 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 merge the recipient report data and NETS based first 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 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).

## 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 counterintuitive, 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.

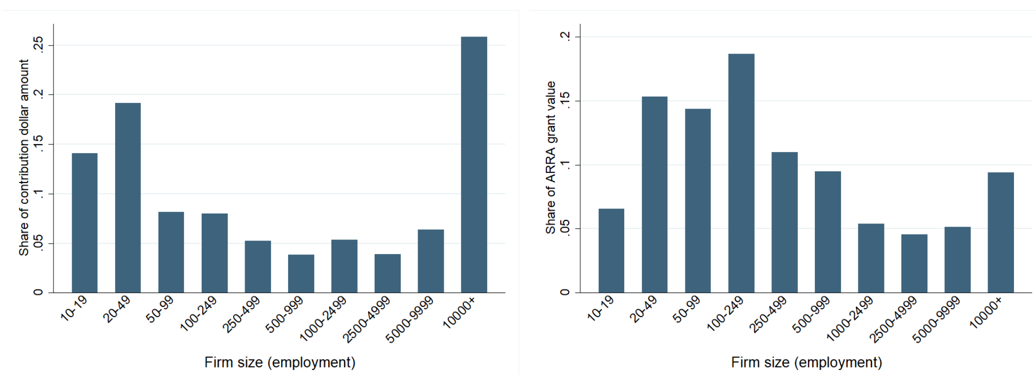
Second, small- and medium-sized enterprises actively engage in local elections via campaign contributions. The left panel of Figure 2 depicts the dollar share of campaign contributions by firm size groups, measured by firm employment. Firms with 500 or fewer employees account for 55 percent of total 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 a large fixed cost 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 advantage of using a nationally representative data set,

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

such as NETS, over data that contains only publicly listed firms (e.g., Compustat), and the importance of state-level political engagement by SMEs.

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. These facts highlight the active engagement of SMEs in local politics and the importance of ARRA grants as a source of revenue for these firms. The remainder of this paper investigates the connection between this political engagement and fiscal stimulus, as well as its aggregate implications for job creation.

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 to examine political influence over ARRA grant allocation in 2009 through 2010. Consistent with the existing literature, the empirical density function is decreasing in the margin of victory (Akey, 2015; Akcigit et al., 2018). 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.[10]

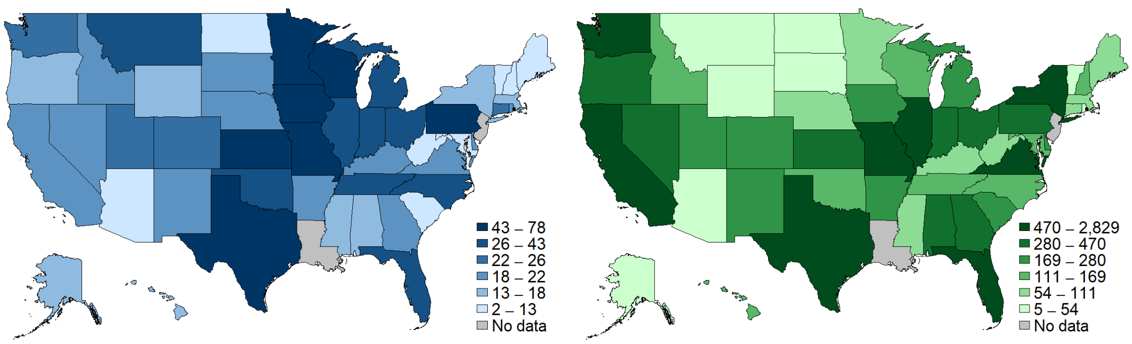
Our close election sample encompasses 629 elections across 48 states during the 2006, 2007, and 2008 election cycles. Figure 3 shows the number of candidates in these elections and the firms that supported them. There is ample variation across

[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] For the distribution of the margins of victory in our data, see Figure B.1 in Online Appendix B.2.

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.

## 3.3 Empirical Specification

The outcome variable of interest, defined at the firm-state level, indicates whether a firm receives an ARRA grant in the state. In contrast, firm political connections vary at the firm-state-politician level – a firm can secure political connection to many state legislators if it supports multiple politicians in elections. Indeed, on average, there were 13 close elections in a state and the average firm made campaign contributions to politicians in 2.4 close elections.[11]

For ease of interpretation, we build a treatment-control framework to estimate the effect of gaining political connections on grant outcomes. First, 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,

[11] Only in 3.7 percent of firm-election pairs do firms hedge the election outcome risk by supporting both top candidates in the same election. Lowe levels 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 B.1 in Online Appendix B.2 shows the full distribution of the number of politicians a firm supports in close elections in a state.

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

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 need to compare firms 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 factors that drove it 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. 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 exactly on the number of candidates the firm supported in close elections in the same state. We also match firm industry to balance industry composition. We use one-to-many matching with replacement and matching weights constructed based on Iacus et al. (2012).

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 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 significant positive effect on the probability of winning a grant. Column (3) reports the result with the full set of controls. When evaluated at the mean, treated firms are 64 percent more likely to win a grant. We introduce the control variables sequentially moving from Column (1) to (3). If our identifying assumption is invalid–that is, if the $Treat$ indicator is correlated with the error term–the estimated coefficient on $Treat$ will change as we add control variables. Across the specifications reported in Table 1, the estimated effect is quantitatively robust to the sequential inclusion of control variables.

[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.

Table 1: Treatment Effect on Winning a Grant

| | (1)<br>Win | (2)<br>Win | (3)<br>Win |
|---|---|---|---|
| Treat | 0.006*** | 0.007*** | 0.007*** |
| | (0.000) | (0.000) | (0.001) |
| Young | | | -0.006* |
| | | | (0.003) |
| Instate | | | 0.015*** |
| | | | (0.004) |
| TotalCand | | | 0.001 |
| | | | (0.001) |
| NAICS4 X State FE | No | Yes | Yes |
| NumCandCE FE | No | Yes | Yes |
| Emp Category FE | No | No | Yes |
| Obs. | 6187 | 6143 | 6143 |
| R-sq | 0.00 | 0.27 | 0.30 |

**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. Win indicates whether a firm received at least one grant from a given state as a prime 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.

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, the log is not defined at zero. Thus, it 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).[13] We estimate that grant dollars received and the number of grants awarded increase significantly by nearly 10 percentage points and 1 percentage point, respectively. To understand the eco-

[13]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 transforamtion, 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).

nomic magnitude of the effect of political connections during ARRA, Appendix A.1 estimates 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 as, at the moment of contributing, the firms do not anticipate ARRA. The economic magnitude is indeed relevant, as every dollar contributed in a close election generates in expectation \$2.46 in grants.[14]

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** |
| | (0.005) | (0.005) | (0.005) | (0.004) |
| Young | -0.065 | -0.012* | -0.061 | -0.010 |
| | (0.052) | (0.007) | (0.050) | (0.006) |
| Instate | 0.187*** | 0.028** | 0.177*** | 0.023** |
| | (0.048) | (0.012) | (0.045) | (0.010) |
| TotalCand | 0.034* | 0.006* | 0.032* | 0.005* |
| | (0.019) | (0.003) | (0.018) | (0.003) |
| 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 treatment effect. The results are presented in Table 3. In the first column, we ask whether

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

being connected to legislators in a given state is predictive of receiving grants in *other* states. In principle, state legislators cannot exert influence over grant allocation in other states. 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)<br>Grant PV Other | (2)<br>Grant SV |
|---|---|---|
| Treat | -0.000 | 0.001 |
| | (0.005) | (0.004) |
| Young | -0.005 | -0.003 |
| | (0.003) | (0.004) |
| Instate | -0.039*** | 0.021** |
| | (0.011) | (0.008) |
| TotalCand | 0.001 | -0.001 |
| | (0.003) | (0.001) |
| 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 $\beta_1$ 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 $g$.[15] As shown in Table B.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 $\beta_1$ and its implied 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 a positive and statistically significant treatment effect in the RD specification. When evaluated at the mean, being connected to an election winner results in a 35 to 39 percent increase in the probability of winning an ARRA grant.[16] Combining this result with those from the main regression, we conclude that political connections to state legislators increase the probability of winning a grant by 35 to 64 percent, where the degree of the effect depends on the specific measurement of political connection.

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

[16] Figure B.2 in the online appendix visualizes the RD effects reported in Table 4.

Table 4: Regression Discontinuity Design

| | (1) Win | (2) Win | (3) Win | (4) Win | (5) Win | (6) Win |
|---|---|---|---|---|---|---|
| RD Estimate | 0.019*** | 0.021*** | 0.021*** | 0.020*** | 0.020*** | 0.021*** |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) |
| Obs. | 223188 | 223188 | 223188 | 223188 | 223188 | 223188 |
| Functional form | first order | second order | first order | second order | first order | second order |
| Bandwidth | 5% | 5% | 3% | 3% | IK | IK |
| Marginal Effect at Mean | 34.5% | 38.2% | 38.9% | 37.0% | 37.0% | 38.2% |

**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 Online Appendix B.3. First, we estimate the regressions on an unmatched sample to see whether our results are driven by the matching procedure. Table B.2, confirms that our main results are robust to not matching. Second, in Table B.3 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)). Lastly, in Table B.4 we redefine a close election using a threshold of 3 percent margin of victory and confirm that our results are robust to this tighter threshold, albeit with a smaller sample size.

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 macroeconomic 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.[17] In particular, in the next section we extend the local emplolyment multiplier literature (Chodorow-Reich et al.

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

(2012), Wilson (2012), etc.) by studying the differential employment effect of ARRA channeled through politically connected firms.

# 4 Allocation of ARRA and State Employment

Having shown that political connections affect the allocation of ARRA grant spending, we turn to whether this allocation has implications for employment outcomes. The foundation of our empirical approach is the well-established use of geographic variation in stimulus spending to identify the effect of this spending on local labor market outcomes.[18] Given the importance of states in allocating ARRA grants, our analysis is conducted at the state level. As shown on the left map in figure 4, there is significant variation across states in ARRA spending per capita allocated to firms via contracts and grants in 2009 and 2010.[19] The existing empirical literature exploits this variation to determine whether regions that received more resources per capita saved and created more jobs. 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.

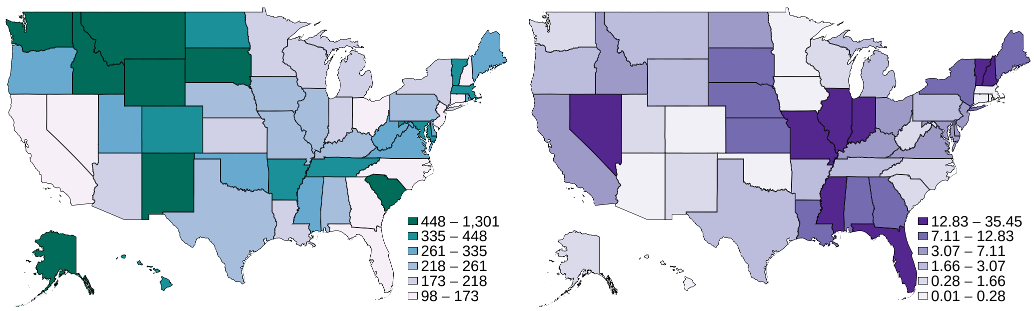
This approach implicitly assumes that the distribution of stimulus spending across firms within states has no impact on the size of the local jobs multiplier. The firm level analysis in section 3 establishes that politically connected firms are more likely to win ARRA grants, and now, we are interested in whether ARRA spending channeled through these firms has a differential impact on local employment growth than ARRA spending channeled through non-politically connected firms. To identify this effect, we first need sufficient geographic variation in the fraction of ARRA grants awarded to politically connected firms. The right map in figure 4 shows the fraction of ARRA spending to firms that was allocated through prime vendors of grants that supported at least one winning candidate in state elections held between 2006 and 2008. In the remainder of this section, we exploit the fact that two states like Pennsylvania and

[18]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)

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

Illinois, which received a similar amount of ARRA stimulus per capita, differed in their disbursement of the spending across firms. 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 local employment growth between 2009 and 2010.

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

To this end, we estimate the following standard, cross-state instrumental variables regression (Wilson, 2012), which is modified only by splitting ARRA stimulus into spending channeled through politically connected and non-politically connected firms:

$$(E_{s,T} - E_{s,0}) = \alpha + \beta_1 A_{s,T}^{connected} + \beta_2 A_{s,T}^{non-connected} + \mathbf{X_{s,0}}\Gamma + \varepsilon_{s,T} \tag{3}$$

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

$(E_{s,T} - E_{s,0})$ is the change in employment in state $s$ between an initial period $(t = 0)$ and an end period $(t = T)$, scaled by population. $A^{connnected}$ denotes the total per capita ARRA grant spending allocated between $t = 0$ and $t = T$ to prime vendors that contributed to at least one winning candidate in state elections held between 2006 and 2008. $A^{non-connected}$ is the total per capita ARRA grant and contract spending during our analysis period allocated to firms, net of $A^{connected}$. $\mathbf{X_{s,0}}$ is a set of control variables, all of which are pre-determined in the initial period. Our vector of excluded instrument for both connected and non-connected spending ($j \,\epsilon\, \{connected, non-connected\}$) is denoted by $\mathbf{Z_{i,0}}$.

## 4.2 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 adjustment employment, and working age population data is obtained from the United States Census Bureau.

Our empirical framework closely follows Wilson (2012), and we therefore include the same five control variables in our baseline specification. All control variables are measured before the initial period and are included because they are potentially correlated with employment growth, ARRA spending, and our instruments. 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.

As Wilson (2012) notes, the last two controls are needed to account for two sources of ARRA stimulus not channeled through firm. ARRA provided fiscal stimulus to states using a formula that explicitly factors in the change in average personal income per capita. More specifically, 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.

## 4.3 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 ($A_{s,T}^{Total}$). Specifically, we sum the local amount allocated to four types of recipients – grant prime vendors, grant sub vendors, contract prime vendors, and contract sub vendors, which amounts to $71 billion. The total ARRA spending allocated to firms is about 26 percent of total ARRA spending paid out during this period.[20]

Our analysis involves separating $A_{s,T}^{Total}$ into the spending allocated to politically connected and non-politically connected firms. Because our firm-level analysis confirms that political connections to state legislatures only help firms in obtaining prime vendor grants, we calculate $A_{s,T}^{Connected}$ as the sum of the local amount allocated to grant prime vendors who supported at least one winning candidate during the state legislature elections held in 2006 through 2008. These elections, held in the years before the enactment of ARRA, determined the state officials that were in office when ARRA funds were disbursed to firms in 2009 and 2010. The amount of money allocated to non-politically connected firms is simply the difference between $A_{s,T}^{Total}$ and $A_{s,T}^{Connected}$.

Both $A_{s,T}^{Non-connected}$ and $A_{s,T}^{Connected}$ are likely endogenous because they may be correlated with current economic conditions. To understand the specific potential sources of endogeneity, it is useful to decompose these two variables as follows:

$$A_{s,T}^{Non-connected} = (1 - FC_{s,T}) \times A_{s,T}^{Total} \tag{5}$$

$$A_{s,T}^{Connected} = FC_{s,T} \times A_{s,T}^{Total} \tag{6}$$

where $FC_{s,T}$ denotes the fraction of total ARRA spending allocated to politically

[20]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.

connected firms. We should therefore be concerned about the endogeneity of overall ARRA stimulus spending ($A^{Total}_{s,T}$) and the degree of political connectedness ($FC_{s,T}$).

As previous literature has noted (Chodorow-Reich et al., 2012; Dupor and Mehkari, 2016; Wilson, 2012), there are two key sources of endogeneity in total ARRA spending. 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 are able to exert their political influence to obtain ARRA money. We construct three instruments to address these endogeneity concerns.

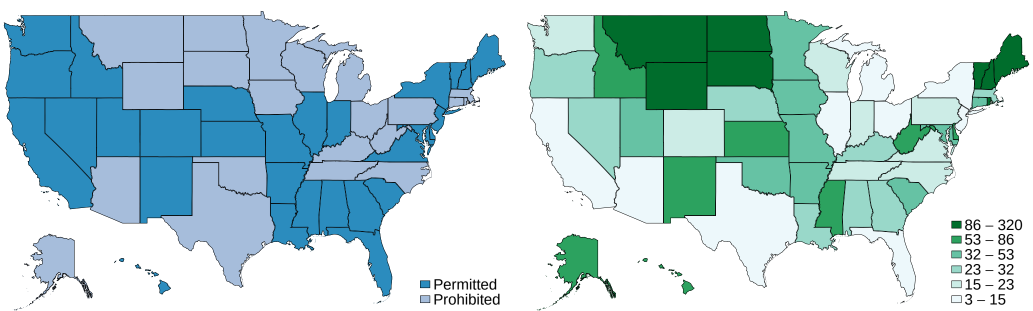
The first instrument, used by Wilson (2012) and Conley and Dupor (2013), takes advantage of the fact that a large fraction of Department of Transportation (DOT) ARRA spending was allocated to states based on pre-recession formulas. We follow Wilson (2012) and construct the instrument as the predicted 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 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 and third instruments capture the potential of firms to attain political connections. The first of these is 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. The left map in figure 5 shows that 21 states across the country prohibit corporate campaign contributions in state elections. For example, while Pennsylvania prohibits them, Illinois permits them.[21] The second of these instruments is the number of state legislative seats per capita. The data on the number of seats in each state legislature is obtained from the National Conference of State Legislatures. The right map in figure 5 depicts the distribution of the measure across states, highlighting the fact that there is substantial variation in this ratio across the country.

Both of these factors are relevant for the likelihood that a firm can form a political connection. The formation of political connections via campaign contributions is less likely if the state prohibits them and potentially more likely if there are more state officials to contribute to. Because we measure corporate campaign contribution restrictions in 2002 and the size of the state legislature is pre-determined, it is unlikely that either is associated with the state's economic conditions during our analysis period.[22]

Figure 5: Corporate campaign contribution limits & number of seats in state legislatures




**Notes:** The left figure depicts whether states permit or prohibit political contributions by corporations. The right figure depicts the per capita number of seats in the state legislature, where population is measured in 2009.

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

[22] Summary statistics for the variables used in our baseline analysis are shown in Table 14 in Appendix A.2.

### 4.4 Baseline Results

The results of our first stage regressions are reported in Table 5. The first column shows the results for total ARRA spending allocated to firms. Consistent with the prior literature, the DOT instrument is statistically significant and positively associated with ARRA spending. Columns (2) and (3) decompose this total into the amount allocated to non-connected recipients (2) and politically-connected prime grant recipients (3). In these two regressions, we also instrument for political connectedness. The DOT IV is only predictive of non-connected ARRA spending, while our political connections instruments are only positively and significantly correlated with ARRA spending to politically connected firms. Moreover, none of the additional controls are significantly associated with the three outcome measures.

Our baseline 2SLS and corresponding OLS results are reported in Table 6. The dependent variable in all regressions is the change in employment between February 2009 and December 2010, scaled by 2009 working age population. The four regressions also control for prior employment growth between December 2007 and February 2009, initial employment per capita in February 2009, house price growth between 2003 and 2007, change in real personal income per capita between 2005 and 2006, and estimated tax benefits per capita.

Across the four regressions, only prior employment growth and change in per capita personal income are significantly correlated with employment growth. The positive relationship between growth at the onset of the recession and growth in 2009 through 2010 may reflect the persistence of economic conditions during the crisis. The negative relationship between the change in per capita income and employment growth reflect that faster growing states before the crisis tended to be harder hit by the recession.

Turning to the effect of stimulus spending on growth, the first two columns confirm the existing literature's finding that ARRA spending saved jobs. The IV estimate in column (2) indicates that 15.4 jobs were created or saved per $1 million in ARRA funds received by states. This estimate is within the range of estimates found in the existing literature (Chodorow-Reich et al., 2012; Dupor and Mehkari, 2016; Wilson, 2012). The IV coefficient (15.4 jobs) is higher than the OLS coefficient (7.2 jobs), suggesting that ARRA was directed towards harder hit states. In columns (3) and (4), we separately identify the effect on employment growth of ARRA stimulus allocated to politically connected versus non-politically connected firms. In both our OLS (column 3) and IV (column 4) regressions, we find that only ARRA to non-politically connected firms saved jobs. Our IV estimate shows that every \$1 million in ARRA disbursed through non-politically connected firms created or saved 13.3 jobs, while the money disbursed through politically-connected firms created no jobs. The OLS coefficient for non-connected ARRA spending is biased downwards, likely because money was directed to states in need. Meanwhile, the OLS coefficient for connected ARRA spending is biased upwards, potentially because connected firms were less successful in exerting their influence over the allocation of stimulus funds in harder hit states.

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 statistic in column (2) is substantially higher than the 10 percent significance level critical value they list, while the F statistic in column (4) falls between the 10 percent and 15 percent significance level critical values. In the latter case, our coefficients are biased towards the OLS estimates reported in column (3). Because we use three variables to instrument for two endogenous variables in column (4), we can also implement the Hansen (1982) $J$-test of overidentifying restrictions. The p-value of the test is reported in the last row of the table, and is above the 10 percent significance level threshold, which provides support for the exogeneity of our excluded instruments.

Our results show that fiscal stimulus helps save jobs, but the distribution of resources across firms matters for the size of the multiplier. Allocating stimulus to politically connected firms distorts the job-creation effects of stimulus spending. This finding is consistent with existing micro-evidence highlighting inefficiencies in investment and contracting of politically connected firms (Duchin and Sosyura, 2012; Brogaard et al., 2018). In fact, according to our estimates, 21 percent more jobs were created or saved in a state like Pennsylvania, in which only 2.1 percent of ARRA spending went to politically connected firms, than in a state like Illinois, in which 22.7 percent of ARRA spending went to these firms.

Table 5: First stage results

| | (1) ARRA spending | (2) ARRA non-conn PV grants | (3) ARRA conn |
|---|---|---|---|
| DOT IV (ths pc) | 2.024*** | 2.450*** | 0.034 |
| | (0.583) | (0.606) | (0.043) |
| Corp contrib (dummy) | | 0.042 | 0.022*** |
| | | (0.042) | (0.005) |
| State leg seats (ths pc) | | -0.889** | 0.155** |
| | | (0.397) | (0.073) |
| Emp growth (07-09) | 1.071 | 1.417 | 0.006 |
| | (0.877) | (0.901) | (0.109) |
| Emp pc (09) | -1.025 | -0.800 | -0.012 |
| | (0.638) | (0.531) | (0.057) |
| HPI growth (03-07) | 0.368 | 0.323 | -0.031 |
| | (0.239) | (0.259) | (0.027) |
| Change in PI moving avg | -68.807 | -67.241 | -5.764 |
| | (44.857) | (45.597) | (4.161) |
| Tax benefits (ths pc) | -0.105 | -0.139 | -0.004 |
| | (0.166) | (0.175) | (0.018) |
| Constant | 0.527** | 0.412* | 0.009 |
| | (0.243) | (0.213) | (0.028) |
| Obs. | 50.000 | 50.000 | 50.000 |
| R-sq | 0.631 | 0.653 | 0.535 |

**Notes:** The dependent variable in column 1 is p.c. ARRA spending to firms; in column 2 is p.c. ARRA spending net of the amount to politically connected firms; and in column 3 is the p.c. grant spending to politically connected grant prime vendors. The IVs are anticipated DOT spending, an indicator of whether a state permits corporate campaign contributions, and the number of state legislative seats p.c.. Our controls include prior employment growth, initial employment p.c., house price growth between 2003 and 2007, change in personal income before the crisis, and expected tax benefits p.c.. ***, **, and * indicate sig. at the 1%, 5%, and 10% sig. levels. Robust SEs.

Table 6: Second stage results

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

| | (1) OLS | (2) IV | (3) OLS | (4) IV |
|---|---|---|---|---|
| ARRA spending | 7.243* | 15.406*** | | |
| | (4.061) | (5.788) | | |
| ARRA to non-connected | | | 6.889* | 13.261*** |
| | | | (4.081) | (4.189) |
| ARRA grants to connected PV | | | -22.537 | -29.823 |
| | | | (28.532) | (31.505) |
| Emp growth (07-09) | 0.149*** | 0.124*** | 0.153*** | 0.135*** |
| | (0.041) | (0.033) | (0.043) | (0.032) |
| Emp pc (09) | 0.002 | -0.000 | 0.004 | 0.003 |
| | (0.041) | (0.038) | (0.042) | (0.038) |
| HPI growth (03-07) | 0.012 | 0.006 | 0.013 | 0.008 |
| | (0.011) | (0.008) | (0.011) | (0.008) |
| Change in PI moving avg | -5.522** | -5.049** | -5.855** | -5.632** |
| | (2.535) | (2.275) | (2.649) | (2.236) |
| Tax benefits (mn pc) | 2.732 | 6.313 | 2.606 | 5.415 |
| | (7.689) | (6.085) | (7.790) | (6.308) |
| Constant | -0.002 | -0.006 | -0.002 | -0.005 |
| | (0.014) | (0.016) | (0.015) | (0.015) |
| Obs. | 50 | 50 | 50 | 50 |
| R-sq | 0.445 | 0.408 | 0.452 | 0.427 |
| DOT IV | No | Yes | No | Yes |
| Corp contrib limit IV | No | No | No | Yes |
| Leg. seats IV | No | No | No | Yes |
| Cragg-Donald Wald F stat | | 38.752 | | 10.689 |
| Hansen J stat p-val | | | | 0.161 |

**Notes:** The dependent variable is the $\Delta$ in employment between Feb. 2009 and Dec. 2010 relative to working age pop. in 2009. The variables of interest are ARRA spending p.c. (columns 1 and 2), and the amount allocated through politically connected and non-politically connected firms (columns 3 and 4). The IVs are anticipated DOT spending, an indicator of whether a state permits corporate campaign contributions, and the number of state legislative seats p.c.. Our controls include prior employment growth, initial employment p.c., house price growth between 2003 and 2007, change in personal income before the crisis, and expected tax benefits p.c.. ***, **, and * indicate sig. at the 1%, 5%, and 10% sig. levels. The F-stat and Sargan overidentification test statistics are included. Robust SEs.

## 4.5 Robustness Analysis

Having established in our baseline specification that only ARRA grants allocated to non-politically connected firms are associated with positive and significant state-level employment growth, we turn to an exploration of the robustness of this result.

**Alternative controls**. Numerous studies explore the employment effect of ARRA and use different sets of control variables. Table 7 presents a third set of robustness results that evaluate the sensitivity of our baseline to alternative control variables considered in the literature.

Table 7: Robustness: alternative controls

| | Non-connected | Connected |
|---|---|---|
| Baseline | 13.26*** | -29.82 |
| | (4.19) | (31.50) |
| Manu share | 12.77*** | -33.30 |
| | (4.22) | (30.93) |
| Ind composition | 12.16*** | -36.04 |
| | (3.98) | (30.37) |
| $\Delta$ HPI (07Q4-09Q1) | 13.25*** | -29.72 |
| | (4.41) | (31.03) |
| Census Regions | 22.28*** | -23.97 |
| | (7.26) | (32.03) |

**Notes:** The dependent variable is the $\Delta$ in employment between Feb. 2009 and Dec. 2010 relative to working age pop. in 2009. All instruments are included. All regressions control for prior employment growth, initial employment p.c., hour price growth between 2003 and 2007, change in personal income p.c. before the crisis, and expected tax benefits p.c.. ***, **, and * indicate significance at the 1%, 5%, and 10% sig. levels. Robust SEs.

In the second row, following Chodorow-Reich et al. (2012) and Dupor and Mehkari (2016), we account for the possible effect of the secular decline in manufacturing by controlling for state-level manufacturing intensity. In the third row, we control for the predicted change in employment between February 2008 and December 2010 in the manner of Bartik (1991). By doing so, we account for the potential correlation between state industry composition, changes in employment, and components of the DOT spending formulas used in the construction of our instrument (Dube et al., 2018; Wilson, 2012). In the fourth row, recognizing that the spatial distribution of the housing bust is not perfectly correlated with the spatial distribution of the housing boom, we add a control for the change in the house price index between 2007:Q4 and

2009:Q1 (Chodorow-Reich et al., 2012; Chodorow-Reich, 2019). In the fifth row, we include Census region fixed effects to account for region-specific employment trends. In each of these alternative specifications, the positive and significant coefficient on non-connected ARRA spending remains, as does the negative and insignificant coefficient on politically connected ARRA spending. Moreover, the size of both coefficients remains stable across most specifications.[23]

**Timing**. Table 8 presents a fourth set of robustness analysis.

Table 8: Robustness: timing

| | Non-connected | Connected |
|---|---|---|
| Baseline | 13.26*** | -29.82 |
| | (4.19) | (31.51) |
| Int – Jan 2009 | 17.76*** | -36.59 |
| | (5.28) | (37.58) |
| Int – Dec 2008 | 9.70** | 1.82 |
| | (3.91) | (30.31) |

**Notes:** The dependent variable is the Δ in employment between the initial period and Dec. 2010 relative to working age pop. in 2009. All instruments are included. All regressions control for prior employment growth, initial employment p.c., hour price growth between 2003 and 2007, change in personal income p.c. before the crisis, and expected tax benefits p.c.. ***, **, and * indicate significance at the 1%, 5%, and 10% sig. levels. Robust SEs.

We test the sensitivity of our baseline results to the initial (February 2009) period. This period was chosen because it coincides with the enactment of the ARRA stimulus bill. As Ramey (2011) notes, however, agents may anticipate the fiscal shock and begin reacting before the shock has been realized. In rows two and three, we consider two alternative initial periods: January 2009 and December 2008, respectively. In both regressions, our baseline results hold.[24]

Overall, we find that our results are robust to alternative controls and timing assumptions.[25] The next subsection explores potential confounding factors that could jointly explain the regional political footprint and employment outcomes.

[23]The one exception is that the coefficient on non-connected spending rises to 21 when Census region fixed effects are included. See full regression results in Table B.7 in the Online Appendix.

[24]See table B.8 in the Online Appendix for the full regression table results.

[25]In Online Appendix B.3, we also confirm that our results are robust to alternative composition of instruments and controlling for non-firm ARRA spending.

## 4.6 Potentially Confounding Factors

Our identification strategy relies on instrumenting the spatial distribution of ARRA spending and the degree of corporate political connections. A strong first stage and favorable Hansen and Cragg-Donals results validate the use of our strategy. Therefore, only omitted factors that are correlated with the instruments and also with the outcome variable could challenge our identification. This subsection explores two potential confounding factors that could drive both, political influence and employment responses at the state level.

**Labor market flexibility**. One potential concern is that states with less flexible labor markets could have more politically connected firms that try to avoid regulations. At the same time, these regulations could make job creation more difficult. Table 9 explores three measures of labor flexibility across states.

Table 9: Potentially confounding factor: labor market flexibility

| | Non-connected | Connected |
|---|---|---|
| Baseline | 13.26*** | -29.82 |
| | (4.19) | (31.50) |
| Union membership | 17.17*** | -40.99 |
| | (5.17) | (31.35) |
| Union representation | 17.52*** | -37.82 |
| | (5.21) | (30.55) |
| Right to work states | 15.03*** | -21.04 |
| | (4.54) | (32.66) |

**Notes:** The dependent variable is the $\Delta$ in employment between Feb. 2009 and Dec. 2010 relative to working age pop. in 2009. All instruments are included. All regressions control for prior employment growth, initial employment p.c., hour price growth between 2003 and 2007, change in personal income p.c. before the crisis, and expected tax benefits p.c.. ***, **, and * indicate significance at the 1%, 5%, and 10% sig. levels. Robust SEs.

The second row controls for union membership. On average, union membership is 11.4 percent across states (median is 10.6 percent). The correlation with our dependent variable is -0.032 while the strongest absolute value correlation with an instrument is 0.11 (DOT IV). The third row controls for the fraction of employees represented by a union in the state. On average, 12.9 percent of employees are represented by a union (the median is 12.8 percent). The correlation with our dependent variable is -0.021 while the strongest absolute value correlation with an instrument is 0.14 (DOT IV). The fourth row controls for an indicator of whether a state has right to work regulations. About 44 percent of states have right to work laws. The correlation with our dependent variable is -0.049 while the strongest absolute value correlation with an instrument is 0.09 (number of seats to population ratio). Given the low correlation of these alternative factors there is little concern about labor markets driving our main result. In general, Table 9 shows that after controlling for the flexibility of the labor market we see a larger employment effect of non-connected spending.[26] Two controls, union membership and union representation, are negative and significant, although of low economic magnitude. Therefore, there seems to be an independent effect of labor flexibility on stimulus, with lower employment growth in less flexible states.

**Industry composition**. A potential confounding factor could come from heterogeneity in job creation across industries. For example, if industries in which politically connected firms operate tend to have particularly low employment multipliers, we would be misinterpreting the effect of fundamental differences across industries as the effect of political connection. To alleviate this concern we perform a within-industry analysis. Table 10 summarizes the results.

Table 10: Potentially confounding factor: industry composition

| | Non-connected | Connected |
|---|---|---|
| Baseline | 13.26*** | -29.82 |
| | (4.19) | (31.50) |
| Const share | 13.81*** | -26.28 |
| | (4.59) | (32.75) |
| Const ARRA | 8.28** | -24.53 |
| | (3.74) | (31.99) |

**Notes:** The dependent variable is the $\Delta$ in emp. (row 1) and construction emp. (rows 2-3) between Feb. 2009 and Dec. 2010 relative to working age pop. in 2009. All instruments are included. All regressions control for prior emp. growth, initial emp. p.c., hour price growth between 2003 and 2007, change in personal income p.c. before the crisis, and expected tax benefits p.c.. Employment-related controls reflect total or construction emp, and total or construction grants, as appropriate. ***, **, and * indicate sig. at the 1%, 5%, and 10% sig. levels. Robust SEs.

Because most of ARRA funds are concentrated in construction – it accounts for

[26] Table B.9 in the Online Appendix shows the full regression table results.

46.8 percent of non-connected ARRA resources and 64.9 percent of connected ARRA resources – and because our instrument relies on the formulaic DOT expenditure, the construction sector is an ideal laboratory.[27] The second row in Table 10 controls for the share of construction employment. Our results are robust to state variation in the importance of the construction sector. The third row performs the within-construction-sector analysis by using only construction employment to construct the dependent variable, ARRA-construction spending, and employment-related controls.[28] The main result holds within construction, as only ARRA construction funds to non-politically connected firms create or save construction jobs.

## 4.7 Potential Channels

Having ruled out that non-political factors determine the lack of job creation by connected firms, we explore potential causes underlying this regularity. In accordance with the literature, we consider two potential differences between connected and non-politically connected firms that can explain the differential employment effect. On the one hand, the literature has documented that politically active firms are larger, more productive, and more capital intensive (Kerr et al., 2014; Arayavechkit et al., 2018), therefore if non-politically connected firms are younger and smaller, their marginal employment creation could be larger (Haltiwanger et al., 2013). The NETS database allows us to sort firms according to age and size to study these differences. On the other hand, politically connected firms have been shown to charge higher markups (Gutierrez and Philippon, 2018), incur delivery delays and cost over-runs (Schoenherr, 2018), and renegotiate the terms of contracts ex-post to receive more funds for the same amount of work (Brogaard et al., 2018). These mechanisms imply that politically connected firms potentially deliver a lower quality product, produce less, or over a longer time span, and avoid hiring new workers. Unfortunately, ARRA grants data do not include measures of performance and thus we cannot directly measure the firm efficiency in performing grant-related jobs.

[27]The great recession was especially damaging for the construction sector due to the collapse of the housing market. Therefore, employment growth is negatively correlated with the prerecession employment share of construction (-0.18). Moreover, the correlation between construction employment share and our instruments is the highest for DOT IV (0.27). See figure B.3 in the Online Appendix for a complete description of the industrial composition of spending allocated to connected and non-politically connected firms.

[28]When connected and non-connected construction funds are combined the overall emp. multiplier on construction sector emp. is 9.17. See the full output in Table B.10 in the Online Appendix.

As a limited but useful alternative, we use the USAspending.gov database to see how firms that win ARRA grants in our sample performed on federal procurement contracts that they were awarded before ARRA (2006 through 2008). Specifically, these data allow us to measure delivery delays–constructed as the difference between the promised completion date and the initial delivery date–relative to the initial number of days to delivery; and cost overruns–constructed as adjustments in costs unrrelated to changes in deliverables–relative to initial contract value.[29]

**Life-cycle characteristics**. In the data, connected firms are older, considerably larger, and exhibit less financial risk.[30] To evaluate if these are important factors explaining the different employment creation between connected and non-politically connected firms, we build state-level weighted averages (by firm-level ARRA funds) of age, size, and financial health for connected and non-connected ARRA recipients. Therefore, if non-politically connected firms create jobs because of being younger, smaller, and more financially constrained we should see that the employment multiplier of non-politically connected firms decreases when controlling for business demographics of ARRA recipients at the state level and that life-cycle variables are significant. Table 11 shows the results of this experiment.

The results on Table 11 show the effect of controlling for life-cycle characteristics on the main coefficients of interest. Size and financial health affect the connected multiplier in the direction predicted by theory as some of the employment creation by connected firms might be due to their smaller size or higher marginal value of resources because of their financial constraints. Nevertheless, Table B.11 in Online Appendix B.6 shows that none of the life-cycle characteristics are significant and they do not increase the explanatory power of the regression. Therefore, there are no clear signs of life-cycle differences being a strong factor behind the job creation patterns of non-politically connected firms.

[29] 58 percent of connected firms and 42 percent of non-politically connected firms in our data had prior procurement contracts. We use this sample to measure the relative efficiency of connected versus non-politically connected firms. The main assumption is that firms exhibit similar efficiency in grants and contracts. Table 15 in Appendix A.2 compares the life-cycle and inefficiency measures between connected and non-politically connected firms.

[30] We use the PAYDEX score, which measures a business's past payment performance. A higher score means lower financial health.

Table 11: Robustness: life-cycle characteristics

| | Non-connected | Connected |
|---|---|---|
| Baseline | 13.26*** | -29.82 |
| | (4.19) | (31.50) |
| Age $\geq$ 10 (sep) | 14.79*** | -48.54 |
| | (5.44) | (56.69) |
| Emp $\geq$ 250 (sep) | 9.53*** | -6.80 |
| | (2.94) | (61.57) |
| Credit score (sep) | 12.61*** | -38.21 |
| | (4.16) | (32.27) |

**Notes:** The dependent variable is the $\Delta$ in employment between Feb. 2009 and Dec. 2010 relative to working age pop. in 2009. All instruments are included. All regressions control for prior employment growth, initial employment p.c., hour price growth between 2003 and 2007, change in personal income p.c. before the crisis, and expected tax benefits p.c.. At the state level, life-cycle characteristic variables are measured as the weighted average (by ARRA money received) for connected and non-politically connected firms, separately. ***, **, and * indicate significance at the 1%, 5%, and 10% sig. levels, respectively. Robust SEs.

**Contract inefficiency**. Table 15 shows that connected firms incur larger costs overruns and longer delays. Therefore, non-politically connected firms might create jobs by hiring more workers in order to not default on the deadline or the terms of the contract. If inefficiency is a main driver of our baseline result, we should see a lower multiplier for non-politically connected firms after controlling for these characteristics and we should also see that these characteristics are significant. Table 12 shows the results of this experiment.

The results in Table 12 show the effect of controlling for contract inefficiency patterns on the main coefficients of interest.[31] Cost overruns do not seem to be an important factor as they decrease the precision of the baseline estimates, and they are not significant. In contrast, and in line with our conjecture, delivery delays decrease the point estimate of the non-connected multiplier. Moreover, delivery delays increase the precision of the baseline estimates, are significant themselves and improve the overall fit of the empirical model. Therefore, non-politically connected firms seem to create more jobs by incurring in fewer delays, pointing to efficiency concerns in the political allocation of grants.

[31] Table B.12 in the Online Appendix shows all of the relevant coefficients.

Table 12: Robustness: contract inefficiency

| | Non-connected | Connected |
|---|---|---|
| Baseline | 13.26*** | -29.82 |
| | (4.19) | (31.50) |
| Cost overrun (sep) | 19.08** | -18.07 |
| | (7.54) | (33.37) |
| Delivery delay (sep) | 11.26*** | -10.19 |
| | (3.43) | (32.36) |

**Notes:** The dependent variable is the Δ in employment between Feb. 2009 and Dec. 2010 relative to working age pop. in 2009. All instruments are included. All regressions control for prior employment growth, initial employment p.c., hour price growth between 2003 and 2007, change in personal income p.c. before the crisis, and expected tax benefits p.c.. At the state level, contract inefficiency variables are measured as the weighted average (by ARRA money received) for connected and non-politically connected firms, separately. ***, **, and * indicate significance at the 1%, 5%, and 10% sig. levels, respectively. Robust SEs.

# 5 Conclusion

Over $30 billion of ARRA stimulus funds were directly allocated by state and local officials to firms.[32] With the average awarded grant valued at half a million dollars, stimulus funds were valuable to firms. These conditions created incentives for businesses to exert political influence over the distribution of these funds. Using a novel database constructed by matching nationally representative firm-level data with data on campaign contributions, state election outcomes, and ARRA grant allocations, we show that firms connected to state legislators are 64 percent more likely to win an ARRA grant.

We evaluate the economic implications of firms' political influence over the distribution of ARRA by studying whether it impacted the ability of ARRA to achieve its key objective of creating and protecting jobs. Our state-level local employment multiplier analysis shows that ARRA grants channeled through politically connected firms did not support local job creation. In light of our estimates, states that allocated 10 percent or more of funds through politically connected firms could have increased

[32]$34.1 billion was channeled to firms as prime vendors and sub vendors between 2009 and 2010. These funds were allocated to firms directly by state and sub-state authorities, respectively. Of the $25.7 billion was specifically channeled to firms as prime vendors and was therefore allocated by state authorities.

jobs creation by more than 20 percent by reducing this share to below 1 percent. The data suggest that delivery delays by politically connected firms can help explain the weak employment outcomes.

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 Ex-post Dollar Value of Contributions

As shown in Table B.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:

$$\text{IHS}(Val)_{i,s} = \beta_0 + \beta_1 \text{IHS}(CamountW)_{i,s} + \gamma' X_{i,s} + \epsilon_{i,s} \tag{7}$$

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 13 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.[33] In our setting, the effect of campaign contribution on receiving a grant is small, but the average unexpected return is quite substantial

[33] 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 × 0.019 × 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.

once we take grant values into account.[34] 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 13: Rate of Return Regression

| | (1)<br>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 $\times$ 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. 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.

[34] 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.2 Allocation of ARRA and State Employment

Table 14 reports the summary statistics for all the variables used in our baseline analysis.

Table 14: Summary Statistics

| | Mean | SD | Min | Max | N |
|---|---|---|---|---|---|
| *A) Dependent variable* | | | | | |
| Emp growth pc (Feb 09 - Dec 10) | -0.0068 | 0.0070 | -0.0254 | 0.0275 | 50 |
| ARRA spending (ths pc) | 0.3119 | 0.1980 | 0.0982 | 1.3015 | 50 |
| ARRA to non-conn (ths pc) | 0.2949 | 0.1991 | 0.0892 | 1.2959 | 50 |
| ARRA grants to conn PV (ths pc) | 0.0170 | 0.0205 | 0.0000 | 0.1051 | 50 |
| Emp growth (07-09) | -0.0502 | 0.0250 | -0.1164 | -0.0014 | 50 |
| Emp pc (09) | 0.4479 | 0.0416 | 0.3777 | 0.5666 | 50 |
| HPI growth (03-07) | 0.2179 | 0.1185 | -0.1130 | 0.4218 | 50 |
| Change in PI moving avg | 0.0008 | 0.0005 | -0.0002 | 0.0025 | 50 |
| Tax benefits (ths pc) | 0.5671 | 0.1101 | 0.4358 | 0.9237 | 50 |
| *C) Instrumental variables* | | | | | |
| DOT IV (ths pc) | 0.1646 | 0.0730 | 0.1143 | 0.4620 | 50 |
| Corp contrib (dummy) | 0.5800 | 0.4986 | 0.0000 | 1.0000 | 50 |
| State leg seats (ths pc) | 0.0582 | 0.0679 | 0.0032 | 0.3201 | 50 |

Table 15: Connected versus non-politically connected firms

| Variable | Political Connections | Mean | Median |
|---|---|---|---|
| *Life-cycle characteristics* | | | |
| Age $\geq$ 10 (%) | Connected | 83.7 | 100.0 |
| | Non-connected | 64.6 | 100.0 |
| Employment $\geq$ 250 (%) | Connected | 46.2 | 0.0 |
| | Non-connected | 14.2 | 0.0 |
| Credit score | Connected | 72.7 | 74.0 |
| | Non-connected | 73.2 | 75.5 |
| *Contract inefficiency* | | | |
| Avg funding only action | Connected | 1.34 | 0.15 |
| relative to initial value | Non-connected | 0.58 | 0.00 |
| Delivery delays relative to | Connected | 4.67 | 2.10 |
| initial number of days | Non-connected | 3.35 | 1.30 |

# B Online Appendix

## B.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.

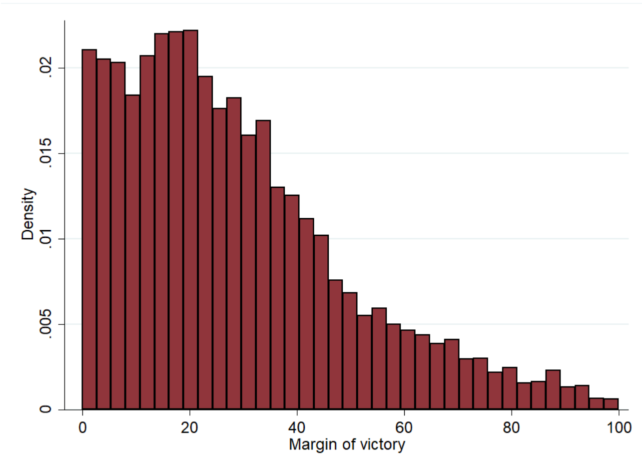
2. We break ties (i.e., the composite score is the same for multiple records) randomly.

**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.

## B.2 Grant Allocation: Regression Analysis

Figure B.1 shows the full distribution of the margin of victory in the state legislative elections held between 2006 and 2008. Table B.1 reports the distribution of the number of candidates firms support in close elections in each state.

Figure B.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 B.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 |

## B.3 Grant Allocation: Robustness Analysis

**Unmatched Sample**: Table B.2 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 B.2: Robustness: Unmatched Sample

| | (1)<br>Grant PV | (2)<br>Grant PV Other | (3)<br>Grant SV |
|---|---|---|---|
| Treat | 0.005*** | -0.003 | 0.001 |
| | (0.001) | (0.004) | (0.003) |
| Young | -0.003 | -0.007 | -0.002 |
| | (0.003) | (0.006) | (0.003) |
| Instate | 0.023*** | -0.049*** | 0.024*** |
| | (0.007) | (0.012) | (0.007) |
| TotalCand | 0.001 | 0.002 | 0.002 |
| | (0.001) | (0.003) | (0.002) |
| 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 B.3 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 B.3 shows that the results in Table 1 and Table 3 are robust to these alternative ways of standard error clustering.

Table B.3: 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 | 0.007*** | -0.000 | 0.001 | 0.007*** | -0.000 | 0.001 |
| | (0.002) | (0.005) | (0.003) | (0.003) | (0.004) | (0.004) |
| Young | -0.006** | -0.005 | -0.003 | -0.006 | -0.005* | -0.003 |
| | (0.003) | (0.003) | (0.003) | (0.004) | (0.003) | (0.005) |
| Instate | 0.015*** | -0.039*** | 0.021*** | 0.015*** | -0.039*** | 0.021*** |
| | (0.005) | (0.011) | (0.007) | (0.005) | (0.008) | (0.006) |
| TotalCand | 0.001 | 0.001 | -0.001 | 0.001 | 0.001 | -0.001 |
| | (0.002) | (0.002) | (0.002) | (0.002) | (0.004) | (0.002) |
| 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.

**3 percent Margin of Victory**: Table B.4 shows the main result and robustness checks using a sample with an alternative definition of close elections. Specifically, an election is defined as a close election if the winner has won with a 3 percent or lower margin of victory, or equivalently, with 51.5 percent or less vote share. While standard errors are larger than their counterparts from the main sample due to a smaller sample size, we find that the baseline results are robust to using a tighter margin of victory for the definition of a close election.

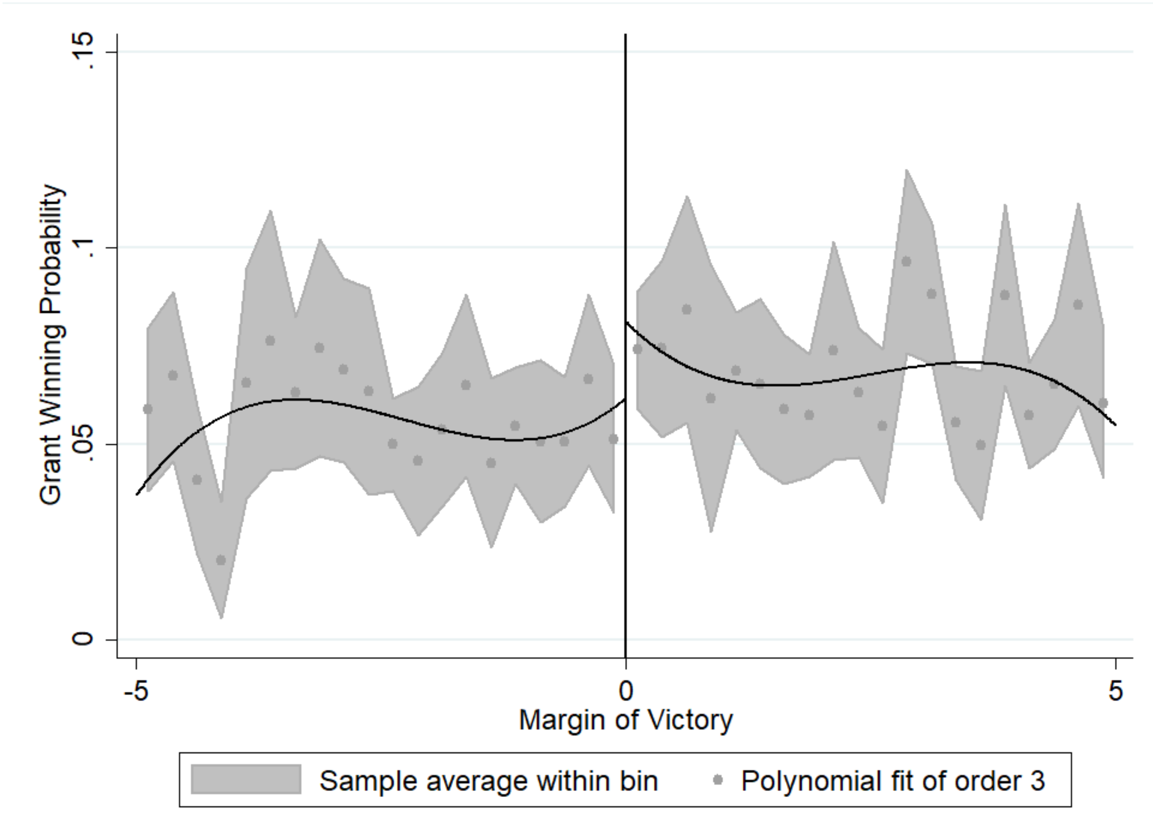
Table B.4: Robustness: 3 percent Margin of Victory

| | (1) Grant PV | (2) Grant PV Other | (3) Grant SV |
|---|---|---|---|
| Treat | 0.008** | 0.008 | -0.003 |
| | (0.003) | (0.011) | (0.004) |
| Young | -0.008* | -0.011 | -0.007** |
| | (0.005) | (0.009) | (0.003) |
| Instate | 0.022*** | -0.053*** | 0.025*** |
| | (0.007) | (0.019) | (0.008) |
| TotalCand | 0.002 | 0.001 | 0.001 |
| | (0.002) | (0.004) | (0.002) |
| NAICS4 X State FE | Yes | Yes | Yes |
| NumCandCE FE | Yes | Yes | Yes |
| Emp Category FE | Yes | Yes | Yes |
| Obs. | 4034 | 4034 | 4034 |
| R-sq | 0.35 | 0.54 | 0.34 |

**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 B.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 B.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 0.25%-sized bins, with 95%-confidence intervals in shaded areas.

## B.4 Employment Multiplier: Robustness Analysis

**Composition of instruments**: Table B.5 evaluates the sensitivity of baseline results to the composition of instruments. Column 1 is the baseline; Column 2 only includes the campaign contribution indicator; Column 3 only includes the number of state legislature seats per capita; and Column 4 includes both IVs and their interaction. Our baseline results qualitatively hold across the three alternative specifications.

Table B.5: Robustness: composition of instruments

| | (1)<br>Baseline | (2)<br>Only corp contrib | (3)<br>Only leg seats | (4)<br>Interaction |
|---|---|---|---|---|
| ARRA to non-connected | 13.261*** | 16.865** | 15.156*** | 10.456*** |
| | (4.189) | (7.519) | (5.189) | (2.749) |
| ARRA grants to connected PV | -29.823 | -80.489 | 31.882 | -46.876 |
| | (31.505) | (57.292) | (52.050) | (33.818) |
| Emp growth (07-09) | 0.135*** | 0.130*** | 0.123*** | 0.145*** |
| | (0.032) | (0.033) | (0.034) | (0.037) |
| Emp pc (09) | 0.003 | 0.006 | -0.001 | 0.005 |
| | (0.038) | (0.040) | (0.037) | (0.039) |
| HPI growth (03-07) | 0.008 | 0.007 | 0.005 | 0.011 |
| | (0.008) | (0.009) | (0.007) | (0.010) |
| Change in PI moving avg | -5.632** | -6.000** | -4.886** | -5.945** |
| | (2.236) | (2.463) | (2.121) | (2.421) |
| Tax benefits (mn pc) | 5.415 | 7.049 | 6.187 | 4.199 |
| | (6.308) | (6.683) | (6.133) | (6.884) |
| Constant | -0.005 | -0.007 | -0.006 | -0.004 |
| | (0.015) | (0.017) | (0.016) | (0.014) |
| Obs. | 50 | 50 | 50 | 50 |
| R-sq | 0.427 | 0.350 | 0.402 | 0.437 |
| DOT IV | Yes | Yes | Yes | Yes |
| Corp contrib limit IV | Yes | Yes | No | Yes |
| Leg. seats IV | Yes | No | Yes | Yes |
| Interaction | No | No | No | Yes |
| Cragg-Donald Wald F stat | 10.689 | 6.708 | 8.157 | 7.834 |
| Hansen J stat p-val | 0.161 | | . | 0.375 |

**Notes:** The dependent variable is the $\Delta$ in emp. between February 2009 and December 2010 relative to the state working age pop. in 2009. Anticipated DOT spending is always included as an IV. Additional IVs are an indicator of whether a state permits corporate campaign contributions (columns 1, 2, and 4), the number of state legislative seats p.c (columns 1, 3, and 4), and their interaction (column 4). Controls are prior emp. growth, initial emp. p.c., house price growth between 2003 and 2007, change in personal income before the crisis, and expected tax benefits p.c.. ***, **, and * indicate significance at the 1%, 5%, and 10% significance levels. Robust SEs.

**ARRA spending controls**: Table B.6 compares the baseline results (column 1) to alternative specifications controlling for ARRA funds channeled through non-business recipients. In column 2 the remaining ARRA funds are included as a control, while in column 3 they are instrumented for using the Health and Human Services instrument. Controlling for non-business ARRA spending does not alter our baseline results.

Table B.6: Robustness: ARRA spending controls

| | (1) Baseline | (2) Remaining ARRA | (3) IV remaining ARRA |
|---|---|---|---|
| ARRA to non-connected | 13.261*** | 17.111*** | 14.817*** |
| | (4.189) | (5.882) | (4.381) |
| ARRA grants to connected PV | -29.823 | -23.376 | -29.367 |
| | (31.505) | (36.665) | (33.001) |
| Emp growth (07-09) | 0.135*** | 0.135*** | 0.137*** |
| | (0.032) | (0.036) | (0.034) |
| Emp pc (09) | 0.003 | 0.004 | 0.004 |
| | (0.038) | (0.035) | (0.036) |
| HPI growth (03-07) | 0.008 | 0.011 | 0.010 |
| | (0.008) | (0.009) | (0.008) |
| Change in PI moving avg | -5.632** | -5.036** | -5.372** |
| | (2.236) | (1.957) | (2.216) |
| Tax benefits (mn pc) | 5.415 | -2.779 | 0.918 |
| | (6.308) | (10.293) | (7.396) |
| Remaining ARRA spending | | 10.105 | 5.299 |
| | | (7.509) | (5.425) |
| Constant | -0.005 | -0.010 | -0.008 |
| | (0.015) | (0.017) | (0.015) |
| Obs. | 50 | 50 | 50 |
| R-sq | 0.427 | 0.457 | 0.457 |
| DOT IV | Yes | Yes | Yes |
| Corp contrib limit IV | Yes | Yes | Yes |
| Leg. seats IV | Yes | Yes | Yes |
| HHS IV | No | No | Yes |
| Cragg-Donald Wald F stat | 10.689 | 14.748 | 4.279 |
| Hansen J stat p-val | 0.161 | 0.376 | 0.275 |

**Notes:** The dependent variable is the $\Delta$ in emp. between February 2009 and December 2010 relative to the state working age pop. All regressions include the three instruments. Baseline controls are prior emp. growth, initial emp. p.c., house price growth between 2003 and 2007, change in personal income before the crisis, and expected tax benefits p.c.. Column 2 controls for non-business ARRA spending and column 2 instruments for this spending using the HHS instrument. ***, **, and * indicate significance at the 1%, 5%, and 10% significance levels. Robust SEs.

**Alternative controls**: Table B.7 introduces additional control variables – share of manufacturing employment (2), predicted change in employment based on the industrial composition of the state before the Great Recession (3), change in the house price index during the housing boom (4), and Census region fixed effects (5). The inclusion of additional controls has little effect on the coefficients of interest.

Table B.7: Robustness: alternative controls

| | (1)<br>Baseline | (2)<br>Manu share | (3)<br>Ind composition | (4)<br>$\Delta$ HPI (07Q4-09Q1) | (5)<br>Census Regions |
|---|---|---|---|---|---|
| ARRA to non-connected | 13.261*** | 12.768*** | 12.156*** | 13.246*** | 22.282*** |
| | (4.189) | (4.224) | (3.980) | (4.413) | (7.263) |
| ARRA grants to connected PV | -29.823 | -33.300 | -36.038 | -29.721 | -23.967 |
| | (31.505) | (30.927) | (30.367) | (31.026) | (32.027) |
| Emp growth (07-09) | 0.135*** | 0.136*** | 0.136*** | 0.137*** | 0.077* |
| | (0.032) | (0.035) | (0.037) | (0.042) | (0.044) |
| Emp pc (09) | 0.003 | 0.003 | -0.009 | 0.003 | 0.028 |
| | (0.038) | (0.038) | (0.031) | (0.039) | (0.041) |
| HPI growth (03-07) | 0.008 | 0.008 | 0.006 | 0.008 | 0.011 |
| | (0.008) | (0.007) | (0.007) | (0.008) | (0.009) |
| Change in PI moving avg | -5.632** | -5.933** | -5.724*** | -5.654** | -5.083** |
| | (2.236) | (2.744) | (2.031) | (2.405) | (2.458) |
| Tax benefits (mn pc) | 5.415 | 5.292 | -0.915 | 5.281 | |
| | (6.308) | (6.460) | (9.389) | (7.178) | |
| Manu share | | -0.008 | | | |
| | | (0.029) | | | |
| Ind composition | | | 9.230 | | |
| | | | (7.989) | | |
| $\Delta$ HPI (07Q4-09Q1) | | | | -0.001 | -0.011 |
| | | | | (0.013) | (0.014) |
| Constant | -0.005 | -0.004 | -0.003 | -0.005 | -0.018 |
| | (0.015) | (0.014) | (0.014) | (0.015) | (0.020) |
| Obs. | 50 | 50 | 50 | 50 | 50 |
| R-sq | 0.427 | 0.430 | 0.453 | 0.427 | 0.464 |
| DOT IV | Yes | Yes | Yes | Yes | Yes |
| Corp contrib limit IV | Yes | Yes | Yes | Yes | Yes |
| Leg. seats IV | Yes | Yes | Yes | Yes | Yes |
| Region FE | No | No | No | No | No |
| Cragg-Donald Wald F stat | 10.689 | 10.049 | 10.532 | 10.338 | 10.361 |
| Hansen J stat p-val | 0.161 | 0.170 | 0.223 | 0.166 | 0.353 |

**Notes:** The dependent variable is the $\Delta$ in emp. between February 2009 and December 2010 relative to the state working age pop. All regressions include the three instruments. Baseline controls are prior emp. growth, initial emp. p.c., house price growth between 2003 and 2007, change in personal income before the crisis, and expected tax benefits p.c.. Additional controls are included in columns (2) through (5). ***, **, and * indicate significance at the 1%, 5%, and 10% significance levels. Robust SEs.

**Timing**: Table B.8 shows that our baseline results hold if we consider anticipation effects. In particular, results are qualitatively similar if we change the initial period from February 2009 to either January 2009 or December 2008.

Table B.8: Robustness: timing

| | (1) Baseline | (2) Int – Jan 2009 | (3) Int – Dec 2008 |
|---|---|---|---|
| ARRA to non-connected | 13.261*** | 17.760*** | 9.701** |
| | (4.189) | (5.283) | (3.914) |
| ARRA grants to connected PV | -29.823 | -36.585 | 1.816 |
| | (31.505) | (37.579) | (30.307) |
| Early emp growth | 0.135*** | 0.164*** | 0.288*** |
| | (0.032) | (0.039) | (0.050) |
| Initial emp pc | 0.003 | -0.004 | -0.020 |
| | (0.038) | (0.042) | (0.038) |
| HPI growth (03-07) | 0.008 | 0.005 | 0.017** |
| | (0.008) | (0.009) | (0.009) |
| Change in PI moving avg | -5.632** | -6.215** | -7.316** |
| | (2.236) | (2.646) | (2.979) |
| Tax benefits (mn pc) | 5.415 | 8.909 | 13.353** |
| | (6.308) | (6.917) | (6.337) |
| Constant | -0.005 | -0.006 | -0.004 |
| | (0.015) | (0.018) | (0.016) |
| Obs. | 50 | 50 | 50 |
| R-sq | 0.427 | 0.419 | 0.556 |
| DOT IV | Yes | Yes | Yes |
| Corp contrib limit IV | Yes | Yes | Yes |
| Leg. seats IV | Yes | Yes | Yes |
| Cragg-Donald Wald F stat | 10.689 | 10.810 | 10.162 |
| Hansen J stat p-val | 0.161 | 0.194 | 0.225 |

**Notes:** The dependent variables are the change in employment between Feb. 2009 (column 1)/ Jan. 2009 (column 2)/ Dec. 2009 (column 3) and Dec. 2010 relative to the state working age pop. in 2009. All regressions include the three instruments. Controls are prior emp. growth, initial emp. p.c., house price growth between 2003 and 2007, change in personal income before the crisis, and expected tax benefits p.c.. Additional controls are included in columns (2) through (5). ***, **, and * indicate significance at the 1%, 5%, and 10% significance levels. Robust SEs.

## B.5 Employment Multiplier: Confounding Factors

**Labor market flexibility**: Table B.9 highlights that our baseline results are robust to controlling for labor market flexibility, as measured by union membership (2), union representation of employees (3), or right to work laws (4).

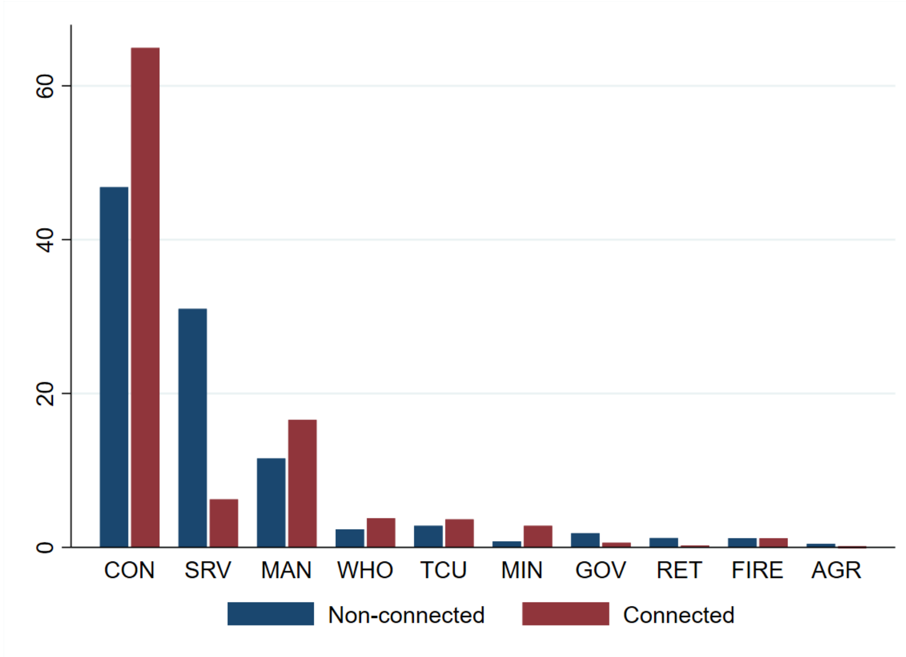
Table B.9: Potentially confounding factor: labor market flexibility

| | (1) Baseline | (2) Union membership | (3) Union representation | (4) Right to work states |
|---|---|---|---|---|
| ARRA to non-connected | 13.261*** | 17.169*** | 17.520*** | 15.029*** |
| | (4.189) | (5.170) | (5.207) | (4.540) |
| ARRA grants to connected PV | -29.823 | -40.988 | -37.821 | -21.035 |
| | (31.505) | (31.348) | (30.550) | (32.659) |
| Emp growth (07-09) | 0.135*** | 0.129*** | 0.131*** | 0.144*** |
| | (0.032) | (0.029) | (0.029) | (0.034) |
| Emp pc (09) | 0.003 | -0.001 | -0.001 | -0.007 |
| | (0.038) | (0.036) | (0.036) | (0.031) |
| HPI growth (03-07) | 0.008 | 0.007 | 0.007 | 0.008 |
| | (0.008) | (0.008) | (0.008) | (0.007) |
| Change in PI moving avg | -5.632** | -5.881** | -5.943** | -6.654*** |
| | (2.236) | (2.329) | (2.358) | (2.518) |
| Tax benefits (mn pc) | 5.415 | 15.553** | 16.372*** | 16.370* |
| | (6.308) | (6.063) | (6.332) | (8.973) |
| Union membership | | -0.000* | | |
| | | (0.000) | | |
| Union representation | | | -0.000** | |
| | | | (0.000) | |
| Right to work (dummy) | | | | 0.004 |
| | | | | (0.002) |
| Constant | -0.005 | -0.007 | -0.006 | -0.008 |
| | (0.015) | (0.015) | (0.015) | (0.016) |
| Obs. | 50 | 50 | 50 | 50 |
| R-sq | 0.427 | 0.423 | 0.430 | 0.453 |
| DOT IV | Yes | Yes | Yes | Yes |
| Corp contrib limit IV | Yes | Yes | Yes | Yes |
| Leg. seats IV | Yes | Yes | Yes | Yes |
| Cragg-Donald Wald F stat | 10.689 | 8.739 | 8.921 | 10.510 |
| Hansen J stat p-val | 0.161 | 0.213 | 0.210 | 0.132 |

**Notes:** The dependent variable is the change in emp. between Feb. 2009 and Dec. 2010 relative to the state working age pop. in 2009. The variables of interest are the amount allocated through politically connected and non-politically connected firms. All regressions include the three instruments. Controls are prior emp. growth, initial emp. p.c., house price growth between 2003 and 2007, change in personal income before the crisis, and expected tax benefits p.c.. Column (2) controls for the % of emp. that are union members. Column (3) controls for the % of emp. represented by a union. Column (4) indicates the state has passed right to work laws. ***, **, and * indicate significance at the 1%, 5%, and 10% significance levels. Robust SEs.

**Industry composition**: Figure B.3 plots the distribution of ARRA funds across sectors. The largest share of both connected and non-connected spending is channeled through the construction sector. For this reason, in Table B.10, we confirm that our baseline results (column 1) are robust to controlling for the share of construction employment in the state (column 2) and to repeating our baseline empirical specification within the construction sector (column 3 and 4).

Figure B.3: Industry composition of connected and non-connected ARRA spending




**Notes:** Depicts the fraction of connected (red) and non-connected (blue) ARRA spending by sector. CON is construction, SRV is services, MAN is manufacturing, WHO is wholesale trade, TCU is transportation, communications and public utilities, MIN is mining, GOV is public sector, RET is retail trade, FIRE is finance, insurance, and real estate, and AGR is agriculture.

Table B.10: Alternative Channel: Industry Composition

| | (1) Baseline | (2) Control Const Share | (3) Const ARRA (comb.) | (4) Const ARRA (sep.) |
|---|---|---|---|---|
| Non-conn (overall) | 13.261*** | 13.812*** | | |
| | (4.189) | (4.590) | | |
| Conn (overall) | -29.823 | -26.279 | | |
| | (31.505) | (32.749) | | |
| ARRA (const) | | | 9.626** | |
| | | | (4.034) | |
| Non-conn (const) | | | | 8.280** |
| | | | | (3.742) |
| Conn (const) | | | | -24.527 |
| | | | | (31.993) |
| Construction share | | -0.062 | | |
| | | (0.088) | | |
| Prior growth (specific) | 0.135*** | 0.125*** | 0.012*** | 0.013*** |
| | (0.032) | (0.034) | (0.004) | (0.004) |
| Initial emp pc (specific) | 0.003 | 0.010 | -0.240** | -0.215** |
| | (0.038) | (0.040) | (0.099) | (0.104) |
| HPI growth (03-07) | 0.008 | 0.011 | 0.004 | 0.005 |
| | (0.008) | (0.008) | (0.003) | (0.003) |
| Change in PI moving avg | -5.632** | -4.962* | -0.875 | -1.085 |
| | (2.236) | (2.589) | (0.816) | (0.922) |
| Tax benefits (mn pc) | 5.415 | 2.502 | 3.867* | 3.737 |
| | (6.308) | (6.319) | (2.266) | (2.322) |
| Constant | -0.005 | -0.005 | 0.001 | 0.001 |
| | (0.015) | (0.017) | (0.002) | (0.002) |
| Obs. | 50 | 48 | 48 | 48 |
| R-sq | 0.427 | 0.426 | 0.577 | 0.559 |
| DOT IV | Yes | Yes | Yes | Yes |
| Corp contrib limit IV | Yes | Yes | Yes | Yes |
| Leg. seats IV | Yes | Yes | Yes | Yes |
| Cragg-Donald Wald F stat | 10.689 | 10.714 | 93.352 | 6.142 |
| Hansen J stat p-val | 0.161 | 0.216 | . | 0.052 |

**Notes:** The dependent variable is the $\Delta$ in total (1, 2) or construction emp. (3, 4) between Feb. 2009 and Dec. 2010 relative to the state working age pop. in 2009. All instruments are included. All regressions control for prior emp. growth, initial emp. p.c., hour price growth between 2003 and 2007, change in personal income p.c. before the crisis, and expected tax benefits p.c.. Employment-related controls reflect total or construction emp, and total or construction grants, as appropriate. ***, **, and * indicate sig. at the 1%, 5%, and 10% sig. levels. Robust SEs.

## B.6 Employment Multiplier: Potential Channels

**Life-cycle characteristics**: Table B.11 shows that controlling for life-cycle characteristics of firms receiving ARRA grants has little effect on baseline results. Controls for average firm age (2), firm size (3), or credit score (4) are never significant and have limited effect on the coefficients of interest or the explanatory power of the model.

**Contract inefficiency**: Table B.12 evaluates whether inefficiency, measured by cost overruns (2) and delivery delays (3) help explain our baseline results. We see that delivery delays lower the point estimate of non-connected spending, increase the precision of our estimates, are significant, and improve the fit of our model.

Table B.11: Robustness: life-cycle characteristics

| | (1) Baseline | (2) Age (sep) | (3) Size (sep) | (4) Credit score (sep) |
|---|---|---|---|---|
| Non-conn (overall) | 13.261*** | 14.792*** | 9.531*** | 12.606*** |
| | (4.189) | (5.444) | (2.941) | (4.165) |
| Conn (overall) | -29.823 | -48.535 | -6.802 | -38.206 |
| | (31.505) | (56.693) | (61.567) | (32.272) |
| Emp growth (07-09) | 0.135*** | 0.131*** | 0.128*** | 0.138*** |
| | (0.032) | (0.037) | (0.030) | (0.033) |
| Emp pc (09) | 0.003 | 0.001 | -0.007 | 0.001 |
| | (0.038) | (0.040) | (0.037) | (0.036) |
| HPI growth (03-07) | 0.008 | 0.009 | 0.009 | 0.010 |
| | (0.008) | (0.008) | (0.008) | (0.009) |
| Change in PI moving avg | -5.632** | -5.533** | -6.124** | -5.715** |
| | (2.236) | (2.418) | (2.506) | (2.285) |
| Tax benefits (mn pc) | 5.415 | 5.731 | 7.922 | 5.988 |
| | (6.308) | (6.313) | (6.541) | (6.373) |
| Age $\geq$ 10 (NC) | | 0.272 | | |
| | | (0.662) | | |
| Age $\geq$ 10 (C) | | -0.084 | | |
| | | (0.353) | | |
| Emp $\geq$ 250 (NC) | | | -1.049 | |
| | | | (1.076) | |
| Emp $\geq$ 250 (C) | | | 0.352 | |
| | | | (0.300) | |
| Credit score (NC) | | | | 0.287 |
| | | | | (0.398) |
| Credit score (C) | | | | 0.056 |
| | | | | (0.061) |
| Constant | -0.005 | -0.006 | -0.001 | -0.029 |
| | (0.015) | (0.016) | (0.013) | (0.036) |
| Obs. | 50 | 50 | 50 | 50 |
| R-sq | 0.427 | 0.406 | 0.482 | 0.435 |
| DOT IV | Yes | Yes | Yes | Yes |
| Corp contrib limit IV | Yes | Yes | Yes | Yes |
| Leg. seats IV | Yes | Yes | Yes | Yes |
| Cragg-Donald Wald F stat | 10.689 | 5.753 | 4.888 | 11.770 |
| Hansen J stat p-val | 0.161 | 0.217 | 0.316 | 0.171 |

**Notes:** The dep variable is the $\Delta$ in emp between Feb 2009 and Dec 2010 relative to the 2009 working age pop. All instruments are included. All regressions control for prior emp. growth, initial emp. p.c., hour price growth between 2003 and 2007, change in personal income p.c. before the crisis, and expected tax benefits p.c.. Col (2) controls for firm age, col (3) for firm size, and col (4) for the credit score of connected (C) and non-connected (NC) firms, separately. ***, **, and * indicate sig. at the 1%, 5%, and 10% sig. levels. Robust SEs.

Table B.12: Robustness: contract inefficiency

| | (1) Baseline | (2) Cost overrun (sep) | (3) Delivery delay (sep) |
|---|---|---|---|
| Non-conn (overall) | 13.261*** | 19.079** | 11.262*** |
| | (4.189) | (7.536) | (3.426) |
| Conn (overall) | -29.823 | -18.067 | -10.188 |
| | (31.505) | (33.373) | (32.359) |
| Emp growth (07-09) | 0.135*** | 0.119*** | 0.112*** |
| | (0.032) | (0.036) | (0.038) |
| Emp pc (09) | 0.003 | 0.000 | 0.005 |
| | (0.038) | (0.037) | (0.036) |
| HPI growth (03-07) | 0.008 | 0.002 | 0.003 |
| | (0.008) | (0.009) | (0.009) |
| Change in PI moving avg | -5.632** | -4.932** | -5.100** |
| | (2.236) | (2.365) | (2.344) |
| Tax benefits (mn pc) | 5.415 | 6.192 | 5.679 |
| | (6.308) | (6.674) | (6.537) |
| Cost overrun (NC) | | 0.000 | |
| | | (0.000) | |
| Cost overrun (C) | | -0.000 | |
| | | (0.000) | |
| Delivery delay (NC) | | | -0.076** |
| | | | (0.032) |
| Delivery delay (C) | | | -0.046*** |
| | | | (0.017) |
| Constant | -0.005 | -0.006 | -0.003 |
| | (0.015) | (0.016) | (0.014) |
| Obs. | 50 | 50 | 50 |
| R-sq | 0.427 | 0.409 | 0.503 |
| DOT IV | Yes | Yes | Yes |
| Corp contrib limit IV | Yes | Yes | Yes |
| Leg. seats IV | Yes | Yes | Yes |
| Cragg-Donald Wald F stat | 10.689 | 11.449 | 8.942 |
| Hansen J stat p-val | 0.161 | 0.226 | 0.156 |

**Notes:** The dep variable is the $\Delta$ in emp between Feb 2009 and Dec 2010 relative to the 2009 working age pop. All instruments are included. All regressions control for prior emp. growth, initial emp. p.c., hour price growth between 2003 and 2007, change in personal income p.c. before the crisis, and expected tax benefits p.c.. Col (2) controls for the avg cost overrun and col (3) the avg delivery delay of connected (C) and non-connected (NC) firms, separately. ***, **, and * indicate sig. at the 1%, 5%, and 10% sig. levels. Robust SEs.